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William H. Melody, Liora Salter, and Paul Heyer, eds., *Culture, Communication and Dependency: The Tradition of H.A. Innis* (Norwood, N.J.: Ablex 1981).

THIS USEFUL, WELL-EDITED, and carefully organized collection of essays grew out of a conference on the legacy of Harold Innis held at Simon Fraser University in 1978. It is interesting to observe that the proceedings of an interdisciplinary symposium on the work of the man often reputed to be Canada's most outstanding social scientist, appears as one of a series of books on communication and the information sciences, edited out of the University of California and published by a New Jersey publishing house. This points towards an increasing international interest in the ideas and conceptualizations of Innis at precisely the time when Canadian social science is turning ever more towards parish paradigms of branch plant origin, and when the Canadian publishing industry, the Chrysler of modern communications media, is limping along on the basis of government largesse while losing what little entrepreneurial drive towards intellectual innovation it ever possessed. That Innis himself would likely have had much of insight to say about such a situation serves to reinforce his current relevance, despite the best efforts of the University of Toronto to marginalize him, creating a cult figure without analytical substance for public relations purposes, while continuing in its social science output to spew forth an ideological and methodological orthodoxy that Innis despised.

The contributions to the volume come from a variety of disciplines, and a variety of vantage points. They include veteran pioneers in the social sciences, some of whom were (roughly) of Innis' own generation — Donald Creighton (history), S.D. Clark (sociology), Horace Gray (American institutional economics). They include second generation Innisians like M.H. Watkins. They include a variety of scholars in related fields with a strong emphasis on modern communications studies. And they include social scientists currently researching contemporary economic and social issues with respect to the Canadian North, studies that *may* form, in one sense, logical extensions of the type of material Innis himself covered in a more Southern context and in a historically earlier period.

The unifying theme of the collection, as pointed out by the editors, is that there is no real distinction between the early Innis (of the CPR, fur trade, cod fisheries, etc.) and the later Innis (of the works on the history of communications media). Rather, the later studies were simply efforts to prove in a more universal fashion, the essential elements *implicit* in the analytical framework put to work in the earlier, more properly staple-oriented studies. One could say that the main concern of Innis was the mode of circulation — of information, of capital, of commodities. And that fundamental concern, illuminated so well in this useful volume, provides an opportunity to look back over past debates about Canadian social, political, and economic issues, and perhaps reassess their lessons. In particular, the volume helps to correct three factors influencing recent Canadian intellectual history that needlessly undermined part of the general credibility of Innis-type studies.

One of these was the legacy of the 1960s when communications studies, which to Innis were a foundation-stone for a more general and critical political economy, became trivialized in the hands of some who professed to be followers of Innis, and thus degenerated into another exercise in Madison Avenue Razzmatazz. The serious analyses of communications, including their role as instruments of imperialist domination, both economic and cultural, are one of the main virtues of this collection. A second factor was the overzealousness of efforts in the late 1960s and early 1970s to turn Innis into a

nationalist cult figure. He *was* a strong nationalist, but a critical and qualified one; and, more importantly, the type of analytical approaches he pioneered are applicable to current and past problems of political economy, regardless of the personal political predilections of the applicant. Thus to make the objective statement that Canada is a dependent and peripheral part of the world capitalist system, whose social structure bears some similarities to other societies emerging from political colonialism, does not *in and of itself* constitute a moral endorsement of the position that Canada is involved in a sort of national liberation struggle; nor does it even involve conceding political legitimacy to the existence of the CBC or the Canada Council. A third factor is the revival of Marxism in Canada in the 1970s which led all too often to either a categorical rejection of Innis (and his union of staple theory with imperial communication systems as the core of his political economy), or misguided efforts to reconcile Innis and Marx. (This is distinct from the view that the two are sometimes complementary, though in other ways very different, modes of analysis of the type of problems that *both* recognized as important, while mainstream social science ignored them.) On all three of these issues, the volume has something to contribute — that the contributions will often be controversial is all to the good, since Innis' studies, to be fruitful, must be ongoing and critical, and never frozen into a body of dogma in the way that mainstream economics has become.

There are some major weaknesses in the volume in terms of gaps in the material covered and perhaps in terms of choices of material to be included. While communications are well explored, including many aspects normally neglected, the traditional deficiencies of the staple theory remain largely uncorrected here. One of these concerns the very meaning of, and limits to, the concept of staple. Is staple-theory merely another name for a sort of commodity-fetishism practised by a subset of Canadian social scientists, as some critics of the approach have alleged? Is the staple-theory really just a variant of theory of export-led growth? Perhaps the answers to these frequently heard charges can be found by recalling the original connotation of staple in the history of commerce, when it referred not to a primary product; but to any commodity in relation to a given port-of-trade specializing in that particular commodity, *and* the accompanying set of regulations and institutions concerning trade in that commodity. Commodity trade was embedded firmly in a set of political institutions. In the original concept of a staple, and by reviving this much-neglected facet of Innis, one can rescue him from being subsumed as just a special case of modern export-led growth theory, as well as pointing to parallels in his analytical model to those of economic-anthropologists of the Karl Polanyi school.

Another largely neglected facet of the Innis paradigm (though there is a useful starting point in this volume in Irene Spry's paper) is the debt question. Always in the background, and sometimes in the foreground of Innis' political economy, were the overhead costs associated with debt service. Debt functioned as a means of organizing the productive process *within* the staple extracting hinterland, *and* as a means of guaranteeing the international division of labour between hinterland and metropoles. Here Innis' role as an anticipator of the Latin American structuralists becomes an interesting venue for analysis. If Innis had written *The Economic History of Brazil* and Furtado *The Cod Fisheries*, there likely would have been very little difference in the outcome, apart from the fact that *The Cod Fisheries* would have become comprehensible without the enormous effort on the part of the reader it now requires.

A third insufficiently explored theme is the nature of, and the role of the State. The State is vital to Innis' political econ-

omy as the instrument that creates the infrastructure that actually permits the exploitation of the staple to begin. Examination of Innis *vis à vis* the state, might also have yielded interesting insight into Innis the political man — including throwing more light on his far from flattering views of the old League for Social Reconstruction and of the work of Gustavus Myers.

These considerations point to the conclusion that (contrary to Robin Neill's interesting contribution to the collection) for Innis the staple theory was inextricably a part of an institutionalized, *political* process of economic development, and that the theory, with its implicit emphasis on the growth of aggregate stocks of factors of production, rather than per capita income, and its focus on infrastructure and debt, examined the creation of political borders out of geographic dimensions. That throws into question the legitimacy of various regionally-oriented studies of natural resource based growth, including those in the volume, as genuinely representing an application of the Innisian tradition, whatever their intrinsic value (which in fact is considerable).

Orthodox economics assumes that social overheads are already in place, that the price system and national capital markets operate to all intents and purposes perfectly, and that information can, as a useful approximation to reality, be treated as a free good. Innis on the contrary examined a process where the state was engaged in a vigorous program of infrastructural spending, where the spectre of an enormous debt load overhung all political and economic decisions, in which the price system as an allocative device was in its infancy and therefore fraught with imperfections, and where control of information flows (and the infrastructure for transportation and communication) was the key to power, economic and political. Today's world is one typified by swollen military bureaucracies and massive energy megaprojects, escalating and increasingly precarious debt burdens, a price system rigidified beyond repair by the exercise of monopoly power, and with control of information technology, the key economic and political battleground. The continuing relevance of Innis, and the irrelevance of his major competitor in the paradigms of economic orthodoxy, is obvious.

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Roger Hall and Gordon Dodds, *Canada: A History in Photographs* (Edmonton: Hurtig 1981).

I SHOULD BEGIN BY ADMITTING that I am an old-photo junkie. I can happily spend hours pouring over these remarkable artifacts of high-tech consumer capitalism, exploring every nook and cranny for clues to bygone customs and experiences or pondering the subtle nuances of a face in the camera's eye. Nothing else can give me such a surge of the immediacy of the past — a frozen moment, preserved with scientific precision, for my rapturous absorption.

In recent years I have noticed that my addiction is shared by growing numbers of social historians, who have not only dug into dusty vaults and attics to find rare old photographs, but who have also been compiling them for mass distribution. The Visual History Series sponsored by the National Museum of Man and the National Film Board has spawned much of this new activity, and the new series of short monographs on Canadian cities is similarly making available great gobs of photographic imagery from Canada's past. More specialized collections have presented prairie women, plains Indians, Canadian soldiers, Canadian ethnic groups, Hamilton workers, and Toronto from almost every conceivable angle. The new enthusiasm even brought about the republication of Ralph Greenhill's pioneering *Early Photography in Canada* in a lavish, expanded form.

The book of historical photographs has, in fact, increasingly become the social historian's medium for reaching beyond an academic audience (and, by no accident of course, into the lucrative market for coffee-table bestsellers). It is a medium of communication which differs from the older "illustrated history" in that the photos are intended to tell most of the story themselves, with a little help from introductory text and explanatory captions. With the publication of Roger Hall and Gordon Dodds' ambitious *Canada: A History in Photographs*, the genre has found its fullest expression, and assessing the stunning visual cavalcade which these two archivist-historians have assembled can help us to grasp the significance of this new phenomenon of history-through-photographs.

Hall and Dodds have compiled a history steeped in the past decades' new social history. The themes emphasized in the pictures and the accompanying text are generally day-to-day lives of Canadians who never saw the inside of a parliamentary chamber (though the post-1960 photos include an unexplained efflorescence of political portraits). Some of the snippets of everyday life which they present are gems. Everyone will have his or her fancy tickled by particular photos, but I liked Winnipeg's 1913 Santa Claus and several of the shots of store interiors, especially a drug-store magazine rack in Vancouver in 1945. Some of the casual, unposed pictures of groups gathered on boat or train platforms or pictures of working people on the job similarly gave me fascinating glimpses of the routines of Canadian society half a century ago. The dramatic pictures of events like the 1938 occupation of the Vancouver Post Office by unemployed workers, moreover, help to bring alive these moments of violent confrontation in Canadian history, right down to the blood oozing from a cracked skull. The regional representation is admirably broad, without the common reliance on big-city glamour, and the compilers have unearthed many hitherto unpublished photos, avoiding the reprinting of well-known images from previous illustrated histories. The book's handsome design suffers only from some unfortunate reductions of quite detailed shots to miniscule size on the printed page, especially in the nineteenth-century section. There is no doubt that historical enthusiasts, professional and amateur, will come away from the book with some new insights into Canada's past.

Questions arise, however, about the overall impact of this "photographic record of Canada." What kind of total history of the past 100 years emerges from this series of images, which the compilers promise will "present a story along a chain of time?" Is it really possible to present "a history in photographs?" What criteria for preparing a visual record of historical processes lie buried in Hall and Dodds' ambiguous "blend of selectivity and serendipity?" The answers to these questions are never centrally addressed in the book, but, since this kind of publication reaches a much wider readership than more specialized scholarly works, Canadian historians need to be alert to the implicit content of photographic history. In this case the responsibility is particularly pressing, since the compilers present their work as a contribution to the great search for a national identity.

Historical photographs are slippery objects to handle as sources. Unlike other representations of the past, from scholarly essays to paintings, where an interpretation of events is plainly evident, a photo appears to be the past itself, unmediated by any interpreter. It carries the ideological patina of scientific precision — it can be used to "prove conclusively" all manner of occurrences in natural and human experience. And, thanks to new archival efforts, we now have hundreds of thousands of photographs to "document our past."

Camera work, however, was never as objective and scientific as all that. There

are significant biases inherent in photography which must be borne in mind when applying it to historical reconstruction. Hall and Dodds admit that "the camera... does lie;" but they might have gone a good deal further in assessing the limitations of camera work — both the technical possibilities and the inclinations of photographers and their clientele. Technically, of course, photography was a cumbersome, complicated process until nearly the turn of the century, and the excitement and action of the later photojournalism was normally impossible. Much of the nineteenth-century visual record that Hall and Dodds present, therefore, has a sedate, prim, almost lifeless quality. The jacket blurb even promises a Confederation-era portrait of "a quiet Victorian land of crinoline skirts, rural villages, and farmers' markets...." Could the dabbler in Canadian history who picked up this book appreciate the turbulence of much of late 19th-century Canadian society — the Orange-Green riots, the election violence, the tumultuous strikes, the charivaris, and so on? It appears that this part of the photographic record may be reinforcing some old, inaccurate stereotypes.

The real problem is that from the very beginning photographers did not focus their lenses on everything around them. They selected subject matter according to their aesthetic, moral, or commercial tastes and then carefully composed the picture, choosing particular angles of vision or even orchestrating the poses of their subjects. Hall and Dodds mercifully save us from many examples of the most popular of the early stylized photos — the portraits. There are nonetheless numerous posed photographs here, some with unmistakable propagandistic intent (like wartime photography).

The selectiveness in the choice of subject matter is probably the most striking distortion in the photographic record. The routine and mundane were generally ignored. From the start novelty was prized. By the turn of the century, as the uses to which photography were being put were multiplying, the emphasis in documentary camera work continued to be on the colourful and bizarre, the spectacular and heroic, the extraordinary rather than the pedestrian. Hall and Dodds' decision to reject "imagery which was too static or sombre" reinforces that tendency in the photographic record of the Canadian past. Their book consciously eschews most of the great state occasions that were invariably photographed (though it does catch a few royal visits and diplomatic conferences), and, refreshingly, the "great events" it highlights are generally incidents of social protest, like the 1885 Northwest rebellion, turn-of-the-century Doukhobor marches, unemployed protests of the 1930s, and demonstrations of the 1960s. In the eye of the social historian these flashpoints deserve our attention. The same cannot be said, however, for the undue prominence given to the exotic and comical — from a militiaman embracing his horse, to "Suicide" Ted Elder jumping over a parked car. The fact that Canadians wanted to record these oddities on film is meat for the social historian's maw, but how they contribute to a visual overview of our past is less clear.

Moreover, photographers' fascination with the exceptional and these compilers' concern to bypass the monotonous reduces the attention which the apparently drab lives of millions of working people deserve in any social history of Canada. This book does contain many fine vignettes from working-class life, but two spheres seem poorly represented — the home and the job. Pictures of women's domestic routines are, of course, rare, but the artifact of the amateur shutterbug, the Brownie snapshot, which was often used around the home, finds little space here. (A fine shot of two Manitoba maids preparing their mistress' hair is a rare exception in the book.) The early twentieth-century social reformers who used their cameras to expose the living conditions of

slum dwellers are similarly ignored. Too "sombre" or just overused in previous publications? In either case, the deletion leaves pre-World War I Canada looking a trifle more elegant and genteel than it was.

Perhaps the most underutilized branch of camera work in this book is industrial photography. There are some excellent photos of lumberjacks and fisherfolk, but are three shots of the interior of factories sufficient to convey the range of experience of so many thousands of Canadian workers? And where are the miners who figured so prominently in resource communities across the country? It would be unfair to say that the Canadian working class was slighted in this book — there are many superb views of their collective efforts for dignity and justice, and many pictures of work and play. And there is a noticeable change of mood in the coverage of the 1930s, where real suffering and bitter struggles leap off the page. But it would be fair to identify a shying away from some of the ugliness of the working-class experience, especially in the early years.

In fact, despite its claims to being the great democratic art, photography is a class-based medium. The working class generally used cameras only for the typically reviled home snapshots. When they appear in a photographer's frame, therefore, they usually reflect any of a number of middle-class visions: the quaint artisan, the pathetic or horrifying slum-dweller, the heroic proletarian, and so on. So many pictures of workers in this book — and of European immigrants and farmers as well — appear as violations of, or intrusions on, their world by outsiders. Unemployed men in the 1930s, for example, look sheepish and humiliated by the sensationalist photographer whose flashbulb exploded before them.

Ultimately we must ask where the compilers of this book see the photograph fitting as a historical tool in comparison with the manuscript, map, newspaper, or government report. On the whole, Hall and Dodds seem less interested in examining what was recorded on film for new insights into the social history of Canada. Instead, it seems that in most cases they have chosen photographic material which will represent visually what social historians have been setting down on paper in recent years. That is, the photograph becomes a stand-in for the printed word — presumably communicating directly with a TV-dazed population which is highly attuned to visual images. If that is to be their primary purpose, we must be aware of the ambiguities in the photographic image, especially the emotional impact of old photos: in most cases, they encourage a nostalgic view of the past — one shared by older forms of antiquarian history. The appearance of so many new books of historical photographs — and even my self-proclaimed addiction to them — is part of a generalized resurgence of nostalgia in contemporary popular culture. We would be falling down as interpreters of the past if we allowed popularized social history to radiate the past with sentimentality. In this regard Hall and Dodds have shown far greater care in their compilation of images than most (Toronto's Michael Filey is a leading offender), but they cannot escape completely from the trap that old photos lay for us. The many novelty pictures, of course, encourage a gentle smile, as do the oddities of dress or interior decor. But even visions of suffering or pain easily take on a cast of striking beauty, which abstracts the haunting face in the camera's eye from a harsh social context.

In the end, a balanced judgment of this "history in photographs" must admit that the whole is less than the sum of its parts. Photographs are valuable tools for rediscovering aspects of the past, and, when used judiciously, can no doubt help many more Canadians to understand their history. Hall and Dodds are probably right that "readers will discover in such a visual display many more thoughts and connec-

tions about the history of Canada than they had previously thought possible." It is less clear that photographs can stand by themselves, in a loosely connected series, as an accurate chronicle of the development of Canadian society. They are too biased to the exceptional, leave too much out, and deliver too potent a dose of sentimentality. I am grateful to Hall and Dodds for their efforts in compiling this impressive photographic collection (and thereby feeding my habit), but would-be imitators should beware the tricks of the camera's deceptive eye.

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S.E.D. Shortt, ed. *Medicine in Canadian Society: Historical Perspectives* (Montreal: McGill-Queen's University Press 1981).

HEALTH AND DISEASE are fundamental aspects of human life, yet Canadian historians, by and large, have neglected to investigate either perceptions of physical and mental health or the impact of contagious diseases on Canadian life. In an effort to arouse interest in the field, Professor S.E.D. Shortt has collected a sampling of previously published papers on various themes in the history of medicine. As the title of the anthology indicates, the emphasis has been placed on the social rather than the biographical approach which has so long dominated the efforts of both amateurs and professionals. In an inspired and inspiring article, "Antiquarians and Amateurs: Reflections on the Writing of Medical History in Canada," Shortt himself challenges Canadian social historians to unite with medical personnel to uncover the evolution of the medical profession and the dynamics of the doctor-patient relationship. Unfortunately, as the contents of the collection reveal, this hope is yet to be fulfilled.

Ranging from the provocative to the pedestrian, the essays in this volume cover Canada geographically from the Maritimes to British Columbia and the Arctic, and chronologically from the early contact period to the failure of the King government to implement a national health insurance scheme in 1945. The major strength of the collection rests in the variety of themes it presents. These include descriptions of the health status of Canada's native peoples; discussions of the origins of medical relief, asylums, and modern psychiatry; analyses of Canadian attitudes towards sexuality and birth control; evaluations of the successes and failures of the public health reform movement; and a commentary on the medical profession's variable views on health insurance. By far the largest number of articles pertain to the complex theme of professional development in terms of changing scientific views, the formation of licensing systems, the conservative influence of medical education on women, and the quantitative aspects of general practice in a single Canadian city. While such breadth is testimony to the many topics that have been investigated, scholars in medicine and history will be disappointed to find only hints of the richness of the field. Indeed, Robert Fortuine's paper on historical accounts of Eskimo health and Relief MacKay's essay on the origins of medical relief in Nova Scotia offer little more than factual material on unknown areas of Canadian social history. In contrast, Neil Sutherland's innovative work on the implementation of public health programmes for school children, Terry Copp's scathing critique of the development of preventive services in Montreal between 1870 and 1930, and Janice McGinnis's informative discussion of the 1918 influenza epidemic as a catalyst in the formation of a federal department of health, all represent well-researched and effectively argued viewpoints on a crucial aspect of Canadian life. Sound as they are on an individual basis, not one of these articles could be considered the definitive interpretation of either municipal, provincial or federal

health activities in the early twentieth century or of the reform groups which lobbied so strenuously for specific public health programmes. Clearly, the content of *Medicine in Canadian Society* is a pioneering effort, chosen to whet rather than to satisfy professional interest in medical history.

As well, the format of the study could prove to be a stumbling block to the uninitiated, lacking as it does any introductions to the individual articles or clearcut thematic groupings. Judith W. Leavitt and Ronald L. Numbers have demonstrated in their anthology, *Sickness and Health in America* (Madison: University of Wisconsin Press 1978), how thematic units presented with a brief overview of their subject matter and timespan enable the reader to comprehend both setting and context fully and thus to integrate medical history with national social history. In the Canadian context, Shortt ought to have realized that merely juxtaposing articles like Michael Bliss's witty discussion of pre-Freudian sexual theories with Angus McLaren's commentary on birth control and abortion was not sufficient and that his readers required explicit thematic linkages in order to understand "the broad significance of the healing arts for Canadian history." Thus, his historiographical article is a useful overview of the field but it fails to supply the sustained analytical guidance which is essential in this type of work.

The limitations of Shortt's chronological approach to medical history are strikingly apparent in his choice of articles to illustrate aspects of professional development. Seven of the nineteen essays in the collection can be considered to represent this critical issue yet the reader is left to draw parallels between Geoffrey Bilson's analysis of Canadian doctors' attitudes towards cholera and Charles Roland's comments about the slowness with which antisepsis was adopted by Canadian surgeons. Without a firm grasp of the various disease causation theories which flourished during the nineteenth century and an understanding of changing patterns in practice and therapeutics, students reading either paper would not benefit except in a superficial sense. Similarly, Barbara Tunis' article on medical licensing controversies in Lower Canada and Hilda Neatby's essay on the growth of a formal structure for medical practitioners in the Northwest Territories illustrate specific aspects of the growing pains which the profession endured. The geographical and chronological differences between the two articles, however, far outweigh the similarities in content. Moreover, Shortt makes no effort to bridge the gap with a paper which focuses on the changes which occurred in Ontario although Joseph Kett's interesting comparison of Canadian and American medical institutions offers some much-needed information about medicine in Upper Canada prior to 1870. The two most interesting essays on the professional development theme are Veronica Strong-Boag's feminist assessment of the limitations of women's medical education and Margaret Andrews' evocation of the changes in general practice which occurred in Vancouver between 1886 and 1920. Like the other articles in the anthology, Strong-Boag's and Andrews' work would have had a far greater impact on the reader if they had not stood as isolated examples of current scholarship. In his haste to bring medical history to the attention of Canadian historians, Shortt has sacrificed coherence and continuity for piecemeal exposition.

In spite of these flaws, *Medicine in Canadian Society* is still a significant contribution. The individual essays provide a valid starting point for further research and the bibliography appended to the collection offers a brief introduction to other writing on medical history. Although Shortt fell victim to some of the hazards which anthology editors face, his pioneering effort is the rallying point from which

(as he puts it) "to create a solid nexus between medical and social history."

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T.A. Crowley, ed., *1st, 2nd, 3rd Annual Agricultural History of Ontario Seminar Proceedings 1976, 1977, 1978* (Guelph: University of Guelph, Office of Continuing Education, 1977, 1978, 1979).

IN THE LAST FEW YEARS there has been an attempt to stimulate an interest in Canadian rural and agricultural history and the major impetus in this venture has come from the University of Guelph and some individuals at Queen's University. Since 1976 annual meetings have been held at Guelph and the proceedings have been published under the editorship of Terry Crowley. A wide range of specialists — historians, geographers, landscape architects, archivists, civil servants, businessmen, librarians, and agricultural educators — have participated. These individuals have provided papers on topics that discuss rural and agricultural life in its multiple dimensions. This included the family, education, technological changes, communications, landscape, business, and the archival sources available at the Ontario Archives and at the University of Guelph. Many of the papers are complemented with diagrams, documents or photographs. Like most collections, the 19 papers vary considerably as to quality and interest.

The first selection of essays for 1976 has a heavy emphasis on agricultural development through such devices as energy use, the livestock business, horticultural legacy, and the different field crops. The first by D. Cochrane, the curator of Agriculture at Upper Canada Village, provides a brief enlightening perspective on agricultural development in Upper Canada to 1867, noting the shift in importance from lumber to agriculture and the effect this had on eastern Upper Canada. Remarks are also made on the relative importance of livestock and poultry. R.W. Irwin, an agricultural engineer, outlines the move from oxen to horses to machinery and the effect that mechanization had on man and animal power. The livestock business evolution towards the registered livestock herds and the role of provincial government support for exhibitions is narrated by G.E. Raithby, professor of Animal and Poultry Science. The Director of the Horticultural Institute of Ontario, T.A. Archibald, relates the significance of how various types of horticulture progressed and the people who led in this venture with the support of various levels of government and the Ontario Agricultural College promoted its commercial and environmental development. A brief history of field crops in Ontario is provided by B.E. Twamley, professor of Crop Science, who describes the expansion and importance of different crops at different times. Unfortunately there is no conclusion. D. Gagan's investigation of wills, of family size, and of how land was passed on within families and the shift in age of child-bearing females is useful information on an important aspect of rural existence. Popular agricultural education, which is defined as non-formal education by F. Partridge, a former librarian, devotes most of the article to the introduction of agricultural topics to formal education at elementary, secondary, university, and college levels. Ignored is the fact that education at the upper level was for élites, a minority; the topic is fascinating, the results disappointing. Nonetheless as a whole the first proceedings augured well for its successors.

The second Annual Proceedings of 1977 emphasizes the geographic aspect of rural living. The land survey system is tersely described, and complemented with diagrams of different surveys of the eighteenth to the twentieth centuries by J.E. Hietala, a civil servant. This is followed by two geographers C.F.J. Whebell and F. Dahms. The former assesses the modal-

ities of growth and discusses how and why growth occurs from the centre to hinterland and the reverse, as well as the role of competition. The latter in discussing "declining villages" concludes that they have become in fact "dispersed cities," since there is a re-growth of from 50 to 100 per cent since 1940. Whether this would be valid if compared with the rate of growth of urban centres such as Toronto, Hamilton, Kitchener-Waterloo is questionable. J.J. Talman offers an interesting comment on the reading habits in 1917 of Ontario farmers. Most emphasis is placed on the *Family Herald and Weekly Star* from Montreal as it appears to be the most significant. The other journals are compared with it. With a circulation of 191,374 for all the journals read, he concludes that farmers had a wide range of choices. For some reason the *Grain Growers Guide* of Manitoba, an important production that circulated in Ontario, is omitted from the list. N. Ball, an archivist, forcefully defends the activities, the value and worth of bush farmers, who were pragmatic. The argumentation is solidly presented. Ontario Archival sources for agricultural history are surveyed by A. Ross of the Ontario Achives, indicating sources that still can be exploited with success.

The third Annual Proceedings was printed for the first time in 1978. A similar format to the first two is followed but this time accenting aspects of agriculture as business. Ontario's agricultural implements industry from 1850 to 1891 is discussed by A. Skeoch, an historian. He argues that the two most significant factors in this development were American technological changes and the mergers that produced a Massey-Harris but which was still dependent on American patents for its financial health. V.B.W. Flynn, a businessman, outlines the development of the independent telephone systems in rural Ontario and how they worked in concert with Bell. Also stressed is the importance of the telephone as a means of communication and in the everyday life of rural society. The cheese industry, in the 60 years after 1864, is studied by E. Haslett, a civil servant. The growth and decline of this important industry is well documented and solidly argued; it concludes that cost was the major factor in the decline of this industry. The rural landscape is not forgotten as O. Scott, landscape architect and C. O'Neal, architectural conservationist, consider the changing rural landscape in southern Ontario, and Ontario barns respectively. O'Neal's article contains diagrams of the types of barns and concludes with a plea for conserving these barns. Scott considers the evolution of the rural landscape from road to roadscape and concludes that the rapid changes appear to be leading to a return to the bleak roadscape of the mid-19th century and documents this with supportive photographs. Agricultural history sources at the University of Guelph are listed by librarian J. Moldenhauer. The holdings are impressive; the presentation, however, suggests why researchers might be reluctant to confront librarians.

Each of the Proceedings contains a neat, comprehensive introduction by the editor T.A. Crowley who has done a commendable job in tying together these diverse articles. There is little doubt that the information is useful as it offers additional material on Ontario's changing society and more work such as this should be conducted. Many specialists of Ontario history, caught up in their own interests, might miss much of this research. That would be unfortunate. It is to be hoped that the conferences and publication of the proceedings will continue.

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Carleton University

Jennifer S.H. Brown, *Strangers in Blood. Fur Trade Company Families in Indian Country* (Vancouver: University of British Columbia Press 1980).

Boire, manger, coucher ensemble,
C'est le marriage, ça me semble.
(medieval proverb)

STALINISTS AND STRELNIKOV aside, there can be few of us for whom E.M. Forster's gentle dictum that "the true history of the human race is the story of human affections" does not find a responsive chord. It is, I believe, one of the weaknesses of the last decade of Canadian historical writing that our race to professionalism, to "structure et conjuncture" and to writing for an increasingly elite and more narrowly defined audience, has tended to diminish our concern for the broader public responsibilities of the historian. In this context, Jennifer Brown's *Strangers in Blood*, the study of family life in the fur trade, is both an unusual and welcome addition to the literature. For the relationship between native and European societies is not only a universal theme of Canadian history and a continuing political concern but one whose human impact is clear in the anguish of the nineteenth-century trade.

How could I spend my days in the civilised world and leave my beloved children in the wilderness? The thought has in it the bitterness of death. How could I tear them from a mother's love, and leave her to mourn over their absence, to the day of her death? (107)

Yet the expansion of the profession, in particular the contemporary diversity of subject material, has also made Canadian history more ready to adapt the questions and methods of other disciplines. This is particularly evident in the fur trade literature where geographers, historians, anthropologists, archaeologists, and others have all made their contribution. Jennifer Brown's interest is in the classic anthropological themes of family and kinship. Using both Hudson's Bay Company and North West Company material she has applied her professional grasp of anthropological theory to the context of fur trade family in the eighteenth and nineteenth centuries. Although not quite a "coming of age in Moose Factory" her study is an effective welding of history and anthropology the two disciplines of man and mankind — a rapprochement which could illuminate many corners of Canadian history and one which R.H. Tawney urged nearly 50 years ago in his inaugural lectures at LSE.

The liaisons, and eventual marriages, between European fur traders and native women were based not only upon lust but on the recognition by both European trading companies of the economic value of native female labour and the political significance of the dynastic alliances the marriages represented. They were based too on native practice and were held together by consent rather than by any external legal bond. Affection between the parties was important but as in native societies of the time the relationship was to be seen in a broader community context. The Moose Factory register of a marriage between James Harper and Charlet Turner recorded by Charlet's father, reads not only as a marriage agreement but as an arrangement between the father and new son-in-law. "James Harper I this day consent to be your father in law and by the blessings of the ald mite god join you to my beloved Daughter Charlet." (76) Within the context of the ordered discipline of the HBC the descendants of these unions began to form an "expanding web of marital and kinship links." At the larger posts such as York Factory, this was to develop into a unique social structure. Although initially the HMC had tried to discourage country marriages, it eventually found them useful enough to revert instead to a strategy of control, establishing contracts and rules of behaviour and morality.

The North West Company was guided by similar economic and political motives. But by contrast its traders were more mobile and inherited a pattern of Indian-White relations from the French trade. Although some Nor-westers did form stable relationships the economic instability of the pattern of NWC trade

inevitably made many of their sexual relationships short-term. The authority and discipline characteristic of the HBC did not exist among the loosely organized partnerships of the Montreal traders. Although some of the officers might have desired, for purposes of racial harmony, to restrict the "trade in women" which grew up in several places, unlike the HBC counterparts, they were powerless to enforce such policies.

At the union of the two companies in 1821, the practice of marriage *à la façon du pays* came under the critical eye of Sir George Simpson. Although he had indulged himself with several "bits of Brown" before his marriage to his cousin, his concern for the economic burden of the increasingly large families of resident traders and his distrust of the political influence of some of the Indian and mixed blood wives led to a general discouragement of further intermarriage in the north west. As communications with the "civilized" world expanded, the attitudes of mid-Victorian England became more influential. Late marriage by upwardly mobile men, a stigma attached to illegitimacy and a more subordinate position for women were some of the effects to be felt in fur trade society.

The significance of the new economic and social order of 1821 is also to be found in changed attitudes to children. Previously, the long-term nature of most HBC relationships had meant a strong parental concern for the future of the children, with many of the boys being sent to England for education and good fur trade husbands being sought for the girls. The decades after 1821, however, were conspicuous for "the rise of racial categorization and discrimination and for the economic and sexual marginalization of the native-born sons and daughters in the new order." (205) Racism became, according to Brown, the pretext for economic exploitation, and the source of much bitterness for fur trade parents. Like Charles McKenzie they accused the Company of having "stamped the Cain mark upon all born in this country; neither education nor abilities serve them." (208) Such company policies in large part determined the political and racial identities which emerged in Western Canada and which to this day have had significant social consequences.

Historians might have wished for more context in this book and more consideration of the European attitudes to marriage and family in the period. Samuel Pyeatt Menefee's recent *Wives for Sale*, for example, examines the relative informality of marriage in Scotland in the mid-eighteenth century. In the Orkneys (the source of most HBC non-native labour) couples who had wed through a hole in the stone of Odin in the Ring of Brognar could divorce by leaving the nearby church through separate doors after the service — a system similar to the "custom of the county" and comparable with the Indian pattern. Wife-selling, vividly described in the *Mayor of Casterbridge*, was known in Britain from the eleventh to the twentieth century and of course had its counterpart in the "turning off" of fur trade practice.

The study, as the author acknowledges, focuses on the male upper class of fur trade society and should be read in conjunction with Sylvia Van Kirk's *Many Tender Ties* for some appreciation of the attitudes of the women and children involved. The anthropologist's concern for the genealogical and structural detail of kinship has also left little space for conclusions about the broad impact of industrial capitalism on the major issues of race, class, and family.

Yet this is an important book which significantly enhances fur trade studies. A Swiss proverb suggests enigmatically that "marriage is a covered dish." Jennifer Brown has lifted that lid and in so doing has brought intellectual light and warmth

to two centuries of western Canadian history.

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Arthur Silver, *The French Canadian Idea of Confederation: 1864-1900* (Toronto: University of Toronto Press, 1982).

CE GENRE D'ÉTUDE se faisait attendre. Depuis une douzaine d'années, nous connaissions bien, peut-être trop bien, l'attitude des Canadiens français face à chaque question qui vint troubler les rapports entre les deux nations à la suite de la confédération. Ce qui manquait et ce qui devenait de plus en plus indispensable, c'était une vue d'ensemble qui tracerait l'évolution, à travers cette succession de litiges ethniques, de la conception que les Canadiens français se faisaient de la confédération. Dans une monographie bien écrite et fortement documentée, Silver comble cette lacune au grand soulagement surtout des enseignants et des étudiants de cours généraux d'histoire canadienne et québécoise qui se sentaient de plus en plus débordés par la matière à parcourir.

L'auteur affirme que l'optique des Canadiens français à l'égard de la confédération changea entre 1864 et 1900. Mais au lieu d'y voir un refoulement sur soi presque pathologique des Québécois, conception qui prévaut dans l'historiographie traditionnelle canadienne anglaise, Silver y discerne plutôt une ouverture progressive de ceux-ci sur le Canada.

S'appuyant sur une analyse minutieuse et approfondie de la presse québécoise, l'historien affirme qu'au début la confédération était aux yeux des Canadiens français beaucoup plus une séparation qu'une union. Tous leurs efforts tendaient donc à soutenir la 'souveraineté' de leur état. Deux facteurs vinrent cependant bouleverser leur vision. D'abord les privilèges que la minorité anglo-protestante arracha au Québec diluèrent sensiblement le caractère français de leur province. Ensuite, la répression que subissaient les minorités francophones au Canada menèrent les Québécois littéralement à découvrir l'existence de ces groupes, à s'impliquer de plus en plus dans leurs luttes et à la limite à s'identifier à eux ainsi qu'au grand ensemble canadien. Silver prend soin de préciser que ce nouveau sentiment d'appartenance était beaucoup plus psychique que physique; c'est à dire que les Québécois n'éprouvrent aucun désir de quitter en masse leur sol natal pour aller s'établir à travers le Canada. Il n'en reste pas moins qu'à la fin de cette époque les Canadiens français réclamaient le droit de vivre en français partout au Canada. Et l'historien de conclure: ". . . beyond the still special home of Quebec, all Canada should yet be a country for French Canadians." (243)

L'auteur admet que les sentiments qu'il cherche à identifier sont ambigus. Toutefois nous irions plus loin: la thèse même que Silver présente est ambigue tant bien dans la méthodologie qui la soustend que dans son élaboration. Sur le plan méthodologique d'abord, on est en droit de se demander ce que les journaux d'alors (et d'aujourd'hui d'ailleurs) peuvent révéler de l'âme d'un peuple. À ce sujet l'historien lui-même reconnaît que la presse était intimement liés aux intérêts des partis; qu'elle reflétait le point de vue d'une certaine élite politique et professionnelle. Il affirme néanmoins qu'on peut y répérer certains grands thèmes qui se répétaient à travers les années indépendamment de l'allégeance particulière du journal. Ces thèmes duraient, poursuit-il, parce qu'ils devaient trouver un écho dans le peuple.

On veut bien. Mais ici se pose le problème: comment dissocier le grand thème du contenu qu'en donne la presse partisane? Car il faut bien admettre qu'avant de traduire les aspirations profonde d'un peuple, les journaux devaient justifier l'élite politique, qu'elle soit au pouvoir ou

en opposition. Quel sens en effet l'expression 'autonomie' pouvait-elle avoir quand la presse s'en servait pour couvrir les gestes d'un Cartier, d'un Langevin ou d'un Laurier qui allaient carrément à l'encontre de ce principe? Au lieu d'y voir un signe de la persistance de certaines idées-clef comme le fait l'auteur, nous y voyons plutôt un exemple lamentable de la manipulation des mots. En dernier ressort, malgré les affirmations de Silver, son approche tend à suggérer que non seulement les grand thèmes mais la façon dont la presse les traitaient au jour le jour étaient un reflet fidèle de l'opinion publique.

Toujours sur le plan méthodologique, l'auteur veut bien constater les limites de la presse en tant que témoignage intégral d'un État d'esprit. On doit en effet le féliciter de son emploi de romans, de récits, de brochures qui viennent enrichir sa documentation. Il n'en reste pas moins que Silver a parfois tendance à raisonner comme si à un moment donné, la presse dit tout ce qu'il y a à dire à propos d'une question particulière. Cette presse ne formait toutefois pas un bloc monolithique. Elle changeait selon le contexte journalistique ou politique. Mais à l'exception du chapitre d'introduction, l'historien ne paraît pas tenir davantage compte de ces variations. L'existence d'un journalisme indépendant influait beaucoup, croyons-nous, sur la discussion et l'élaboration de certains grands thèmes de l'époque. Ce journalisme avait la vie dure, n'étant pas choyé comme ses homologues partisans. Par moments il disparaissait tout bonnement. Ne faudrait-il pas dès lors lui donner plus de poids? Silver semble pourtant en minimiser l'importance. Ainsi il affirme (89) que seuls quelques journaux ultramontains (extrémistes par surcroit!) *et libéraux défendaient la thèse de* l'inconstitutionnalité de la loi créant des écoles non-confessionnelles au Nouveau-Brunswick. Plutôt que de voir là un signe révélateur d'un état d'esprit, l'auteur préfère s'en remettre aux feuilles partisanes pour sonder l'opinion publique.

Aussi le contexte politique peut-il parfois expliquer certains silences au lacunes de la presse. Le fait que les journaux ne parlaient pas des droits des minorités lors de l'élaboration de l'Acte de l'Amérique du Nord britannique n'est pas en soi une indication que les Canadiens français s'en désintéressaient. Hector-Louis Langevin avait laissé entendre à l'archevêché de Québec, de manière détournée il est vrai, que la constitution protégerait les minorités catholiques des provinces maritimes. S'il se crut obligé d'en parler, c'est qu'intérêt il y avait au Québec pour cette question. Si par contre les journaux ne l'abordèrent pas c'est que le clergé prit Langevin à sa parole jusqu'à ce que la crise éclate au Nouveau-Brunswick. Et fait à souligner, ce fut le journal indépendant, *Le Nouveau Monde*, qui le premier invoqua l'inconstitutionnalité de cette loi. Si à peine quatre ans après la confédération, on pouvait prétendre que les droits des minorités étaient inscrits dans la constitution, c'est dire qu'il y avait des attentes en 1867 qui n'ont pas trouvé expression dans la presse. Ici l'on touche à la thèse même du livre que l'on trouve peu concluante. C'est la notion même d'une évolution dans la mentalité des Canadiens français à l'égard de la confédération que nous voulons mettre en cause. Pourquoi tout d'abord Silver termine-t-il son étude vaguement en 1900? Cette date qui correspond plus ou moins avec l'avènement au pouvoir de Laurier et l'emprise de Bourassa sur le Québec marquerait-t-elle le cheminement d'un processus? Si l'image que les Canadiens français se faisaient de la confédération était le fruit de crises ethniques successives qui secouaient le Canada, ne serait-il pas plus logique de voir 1917 comme l'aboutissement d'une évolution et Groulx comme son expression intellectuelle?

En écrivant ceci, nous ne cherchons pas à soulever des polémiques constitutionnelles, mais bien à saisir la dynamique

de cette période. Celle-ci n'est pas à nos yeux une progression linéaire du genre "Du Québec libre au Canada français." Elle est plutôt une manifestation continuelle des deux pendants de l'âme canadienne française, la québécoise et la canadienne, voire la nord-américaine, qui transcende les formes constitutionnelles. Même avant la naissance du régime confédératif, il existait une double attente dans l'esprit de certains Canadiens français (il reste à déterminer si ceux-ci étaient des marginaux ou les interprètes des aspirations populaires) à savoir, une plus large autonomie pour le Québec et le droit des catholiques, et à plus forte raison des francophones catholiques, à vivre leur culture où qu'ils soient en Amérique du Nord. Comment sinon expliquer le ferme appui que les Canadiens français accordèrent à leurs coreligionnaires du Canada-Ouest dans les années 1850? L'article 93 de l'AANB était le symbole même de cette double attente. Il est donc faux de dire comme le fait Silver (60) que les Canadiens français en bloc ne voulaient pas de garanties constitutionnelles pour les minorités. Par contre ils rejetaient avec véhémence les propositions de Galt qui cherchaient à soustraire les anglo-protestants de leur condition de minoritaires en leur accordant des privilèges exorbitants.

Quoiqu'il en soit les carences de l'article 93 ne tardèrent pas à se faire jour. La presse indépendante, et notamment l'ultramontaine, fit alors campagne en faveur des minorités. Ce n'est qu'après coup que les journaux d'opposition, tantôt nationaux tantôt conservateurs, emboîtèrent le pas pour des raisons on ne peut plus électorales. Si par conséquent la presse mit du temps à exprimer la notion de droits minoritaires garantis par la constitution, c'est que celle-ci était massivement conservatrice dans les années qui suivirent la confédération, fait que Silver lui-même admet sans en tirer les conclusions qui s'imposent. Déjà à l'époque de la confédération donc, le concept de droits minoritaires protégés par la constitution s'exprimait. Que cette idée ait eu plus ou moins d'importance dans les années successives dépendait du contexte politique.

Ceci dit, il n'est pas surprenant que les questions de droits minoritaires devinrent 'québécoises.' Car certains avaient déjà compris que le régime confédératif consacrait un équilibre de forces: le pouvoir politique des deux nations était intimement lié au sort de leurs minorités respectives. Cette conception est implicite, mais non moins claire, dans les écrits de Mgr. Bourget entre autres. Si les minorités catholiques et francophones étaient bafouées le pouvoir même des Canadiens français en serait amoindri. Ce fait explique aussi les différentes réactions des Canadiens français face à ces questions, leur appui n'étant que moral pour les minorités franco-américaines, alors qu'il était carrément politique pour leurs confrères canadiens. Parler comme le fait Silver d'ingérence du Québec dans les affaires de ses provinces-soeurs, ingérence qui serait inspirée par la doctrine impérialiste qui se propageait alors dans le monde, c'est obscurcir toute cette question.

Bref, le livre de Silver répond à un réel besoin de faire la synthèse d'une époque importante. Il s'appuie sur une documentation impressionnante. Cependant l'approche de l'auteur trahit, malgré ses assertions, la suffisance que manifeste trop souvent l'histoire politique au Canada. L'historien soulève un débat intéressant. Le sujet n'est pas pour autant épuisé.

Roberto Perin
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A.A. den Otter, *Civilizing the West: The Galts and the Development of Western Canada* (Edmonton: The University of Alberta Press 1982).

THIS BOOK IS A STUDY in contrasts. Derived largely from a Ph.D. thesis completed several years ago at the University of Alberta, it attempts to serve two mas-

ters and ends leaving a reader wishing one or the other had been treated more fully. One study revolves round the tangled tale of capital formation on the frontier; the other offers an analysis of community formation with some attention to the emergence of working-class consciousness.

The arrival of Alexander Tilloch Galt and his son Elliott to the southeastern corner of Alberta set the stage for one of the larger private land and resource grabs in early western Canada. The techniques of business interaction with government to gain privileged access to resources and public subsidization of the necessary over-heads (railroads in this instance) to make them profitable was hardly a new field for Galt, whose previous experience in central Canada equipped him for his new field of endeavour. The author clearly outlines the open relationship between the wielders of power on the frontier and the distant civil servants and politicians in Ottawa who bent every regulation to meet demands for government largesse.

The elder Galt takes centre stage for much of the first half of this book, which exposes his quest for fresh income and his disenchantment with politics. The latter led him westward, where his Ottawa and London connections provided the enabling and financial prerequisites for recuperation of his fortune. Large coal deposits in southeastern Alberta and the determination of the federal government to push prairie development propelled Galt to the forefront and built a place for his reclusive son Elliott as well. Den Otter untangles the establishment of the Galts' pre-eminence with a great deal of finesse, outlining the web of political manoeuvring necessary for the construction of branch railroads and their attendant grants of land: the base for the fortunes of the North Western Coal and Navigation Company Limited (later reorganized as the Alberta Railroad & Navigation Company), their instrument for extracting wealth from the under-exploited resources of that corner of the Northwest.

The early years of the company's activities were fraught with failure, mostly a consequence of the slow pace of prairie settlement and the uncertainty of coal mining in a territory that had few urban places and primitive transportation facilities. Expansion of western settlement and railroads after 1885 consolidated coal's place in the prairie economy and prompted diversification into land development and irrigation projects. Coal mining and the town of Lethbridge would become less central to NWC&N Co.'s preoccupations as time went on, but remained its most important single component until well after the turn of the century. The success of the Galts and their very able manager C.A. Magrath was largely a function of their capacity to influence politicians to share their vision of western expansion. No matter who occupied the seat of government in Ottawa, they were successful.

A curious feature, mentioned only in passing by den Otter but clearly revealed in a summary of the stockholders of the company, was the extent of British domination. For all its Canadian content and orientation, the NWC&N Co. was close to 90 per cent British controlled, serving primarily as a funnel for the British speculators anxious to reap the rewards available on Canada's resource frontiers. The constant traipsing back and forth to London to convince reluctant board members of the necessity for more investment was a continuous concern; almost as important as the many trips to Ottawa to importune sometimes skeptical officials of the necessity for more subventions or amendments to the terms of the various leases held by the Company.

The early cornerstone of the Galt enterprise was the coal deposited not far from the surface at Coal Banks (Lethbridge) near the juncture of the Oldman and Belly Rivers. Galt was attracted to the potential of the deposits and gradu-

ally built up a primitive mining camp to ship the coal via the prairie river system to markets in the United States and western Canada. Much of the early history of the community hinged on the ability of the NWC&N Co. to transport coal and its struggles with the CPR to ensure a market. The subsequent arrival of a railroad branch-line solidified early gains and stabilized the community. Prairie boosterism characterized the formative period of Lethbridge's expansion, with a local elite quickly arriving to service the requirements of the amalgam of miners, labourers, and managers brought together as a community of sorts by the Galts. The normal sort of urban tub-thumping ensued as Lethbridge attempted to assert its hegemony over that southeastern corner of Alberta, but most of it came to very little.

The operation of the mines and their relationship to that larger community, which make up much of the second half of the book, is less powerfully developed than the analysis of the structural foundations of the NWC&N Co. Unquestionably a difficult theme, den Otter manages a fair amount of discussion but the scene shifts too frequently from community development to labour relations to provide firm focus. At the same time, his analysis of the formation of the work force repeats established myths without much sustained analysis. The "foreign element" may indeed have been a decisive factor in defining the miners' outlook, but the perspective is always from those who viewed the miners from a distance, as managers, coal consumers, or the town's elite. The most interesting chapter for labour historians deals with the 1906 strike, an abject failure from many perspectives. Yet the discussion is strangely wooden, given the extended nature of the conflict and the centrality of the issues raised by the supposed threat to prairie farmers during the long cold winter. The spectacle of Mackenzie King shuffling back and forth between the miners' spokesmen and the managers, ever conscious of the political implications of any development, is one that would pervade Canadian labour relations for some time after.

Underneath the welter of detail, the nature of capital formation in Western Canada and its implications for the emergence of a prairie working class emerges clearly, while established notions regarding collusion between politicians and entrepreneurs are reinforced. The Galts and their successors were fully integrated with the political establishment and directly tuned to the moods of potential investors in Montreal, Toronto, and London. Their success in coordinating the alienation of prairie territories and resources in the interest of eastern Canadian and British capitalists was part and parcel of one of the great adventures for Canadian entrepreneurship. That workers had any chance at all against such an array of power and sentiment would surprise us; any success, no matter how fleeting, was a considerable accomplishment and a positive contribution to the western labour tradition. The formation of unions and their integration into the community is strongly told here.

The "Civilization of the West" meant many things to many people. For den Otter, it was primarily the transference of capitalist perspectives and the inevitable conflicts that such impositions provoked. His conclusion, like so much prairie writing, seems to lament the intrusion of its more volatile side. This is an important chapter in Canadian labour history from any number of perspectives. It deserves a wider readership than a regional audience is likely to give it.

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Carleton University

Peter McNally, ed., "Special Issue on Library History," *Canadian Library Journal*, 38, 6 (December 1981).

(This issue is a collection of short papers on mechanics' institutes, semi-public

libraries, and Carnegie libraries, presented to the Canadian Library Association's Library History group at a Hamilton conference in June, 1981. Individual copies can be obtained at $4.50 from the CLA, 151 Sparks St., Ottawa K1P 5E3.)

McNALLY HAS EDITED an ambitious theme issue; the essays deal with communities from the Maritimes to Saskatchewan and one paper reaches back to the seventeenth century. Of the 11 contributors only one, Yvan Lamonde, is an historian. This explains the lack of acquaintance with previous work done on libraries by historians. The papers would have been stronger and more interesting if comparisons had been made with previous work.

More than a handful of accounts on Canadian mechanics' institutes have been published starting with Eleanor Shaw's study of the London mechanics' institute in 1941. There is published material on mechanics' institutes in Napanee, Nova Scotia, Halifax, Goderich, Drummond County, Quebec, British Columbia, and Kingston as well as Foster Vernon's unpublished thesis on Ontario institutes, William Judd's edited version of the London mechanics' institute minute book, and George Bobinski's book on Carnegie libraries. There is unpublished work on the Brockville, Paris, and Woodstock mechanics' institutes. Only two papers mention any of this work. A number of authors point to the British influence on libraries in Canada but there is only a cursory treatment of the large literature on British mechanics' institutes.

The authors do not stray much beyond outlines of the history of their subjects. There is little information about who used the libraries. Sources such as Andrew Carnegie's endowment fund correspondence are ignored. These letters reveal much about who patronized book collections throughout Canada. In many communities numerous workingmen borrowed books from public libraries.

These criticisms can't shade the excellent material found in this collection. David Hayne provides a survey of libraries in Quebec. He points to an explosion of circulating libraries prior to Confederation. This raises questions of who was reading the books and why. Further work could say something about literacy and self-education in this period. For example the Montreal Library/Bibliothèque de Montréal had 8,000 books in 1831. He describes the Institut Canadien de Montréal (1844-1880s) as the first workers' university. However the question of who used this library is not resolved. There were 350 members in 1845.

Hayne has found church libraries from the 1840s. Were there analogous libraries in Ontario? Also many parishes had libraries, indeed by 1860 half of Quebec's parishes had some kind of library. Montreal's Fraser Institute was another semi-public library. In 1901 it had an impressive 38,000 books. Hugh Fraser, a Montreal businessman and philanthropist, founded it with a donation. This anticipated Carnegie's endowments. Other communities had similar benefactors. In 1905 Senator Henry Corby gave Belleville a public library.

Yvan Lamonde's study of Montreal libraries is the best piece in the collection. His work also points to a growing reading public. During the period 1851-1890, 109 libraries were formed, 39 of these during the 1850s. The diversity of these libraries is intriguing. Sailors, fraternal societies, railwaymen, church groups, parishes, athletic associations, and workers' groups all established book collections. When the Montreal Amateur Athletic Association was founded in 1881 one of its first acts was to provide a reading room and library.

Lamonde shows that reading rooms were popular because they held dozens of newspapers and magazines from Canada, the United States, and Britain. However he notes that the library was competing with a culture that emphasized non-print activities during leisure time such as

sports and shows. Yet questions about the relationship of the whole community and the libraries are not probed. Did most people read? Was late-nineteenth-century Montreal a print-oriented society? Lamonde thinks so, he gauges the growth of print culture against the rapid increase in the number of libraries.

At one point Lamonde moves on to a larger question. He contends that the time (1850s) the government gave money to libraries was the transition point onward to public libraries. Yet for decades after mid-century most libraries in small town Quebec and Ontario were housed in rented halls. In Ontario even after legislation in 1895 created public libraries this continued to be the case. It was only after Andrew Carnegie endowed 1000s of libraries in North America that public libraries emerged as they are known today. By constructing impressive libraries, membership and book circulation increased rapidly.

Nora Robins' discussion of the Montreal mechanics' institute clearly would be much better if it made comparisons with other studies. She is off base in tying the institute to the fight for an enlarged electorate and a democratic form of government. The early mechanics' institute had only a few hundred members and several hundred books. A number of factors accounted for this institute's slow growth — lecture topics were dull and at least until the 1850s members found few books of interest. In 1859 a report to the institute's board showed that the average member belonged only for 2½ years. Even in 1870 only 9,400 books circulated. This meant a small membership and a poor selection of books. The directors' attempts to censor books could not have helped the situation.

The two papers on mechanics' institutes are disappointing. Both pay no attention to recent historiography. John Wiseman's account gives a good summary of legislation that altered Ontario mechanics' institutes and eventually transformed them into public libraries. He seems unaware that the fantastic growth of institutes in the 1880s illustrates a fast growing reading public.

Jim Blanchard's essay is caught up in semantics. He argues that because the mechanics' institutes were not controlled and exclusively used by workingmen they were failures as "mechanics' " institutes. He can't see that thousands of people were reading hundreds of books and among these people were numerous working-class people. A comparison with other studies would have shown this to be the case. The mechanics' institutes were used by the whole community.

Although Blanchard argues that workingmen were not readers there is much evidence to the contrary. If we look at other examples it is plain that they did read. The Gananoque Carriage Company had a library for its workers. Orange lodges throughout Ontario organized reading rooms and libraries for their members. The Kingston and Pembroke Railway had a reading room and library for its workers and the Grand Trunk Railway operated libraries for its men in Belleville, Stratford, and other centres. There was a London workingmen's library during the 1890s. The Knights of Labor had libraries in towns such as Belleville, Cornwall, and Hamilton. The YMCA provided libraries in many communities. The Brockville YMCA library had more than 500 books. Brockville's volunteer firemen had a library, the Catholic Mutual Benevolent Association had a library of over 700 books, and the Brockville Young Men's Liberal Club operated a library. The Brockville mechanics' institute had many working-class members. Yet this town had a population of only 8,400 during the 1880s.

All of these essays skirt the question of censorship because at no time do they give an account of what was being read in libraries. This is a glaring oversight because numerous catalogues from mechanics' institutes survive. Examples

include those from Brockville, Toronto, Grimsby, Elmsly, Hamilton, St. Catharines, Niagara, and Napanee. Close attention to these sources would detail what was read and reveal the ideas that circulated in the book collections. Also the case of Brockville indicates that library boards in Ontario restricted or censored the books available.

The issue closes with three short discussions on Maritime, Winnipeg and Wolsely, Saskatchewan libraries. There were only nine public libraries throughout Maritime Canada at the end of World War I. This was because these provinces lagged behind Ontario in establishing provincial legislation to support public libraries. Does this mean people did not have access to books? I suspect there were numerous semi-public book collections that could be found in rented rooms above the main streets of Maritime towns and cities.

Carol Budnick's essay on Winnipeg notes the CPR workers operated a library from 1884-1912 and the Knights of Labor organized a library in 1885. Her discussion of the Carnegie library would have benefitted from the Carnegie endowment correspondence. There was a great deal of opposition to Carnegie endowments in many communities and Winnipeg was no exception. Budnick uncovered a poem that speaks to this opposition entitled "Carnegie's Library."

> There's a scent from the books of dead men's bones,
> And a splatter of blood over all:
> There's a rough ragged hole in each leaf you turn,
> Like a wound from a rifle-man's ball.

Judy Thompson's homage to the Wolsely library is the last essay. Wolsely was founded in 1882 by settlers from Ontario. It was always a small town but by the early 1890s the community had established a mechanics' institute with the ever-popular newspaper reading room. One more instance of the thirst of nineteenth-century people for books.

Despite its flaws there is much of interest in this collection. The authors have added to what is known about the reading public in nineteenth-century Canada and we can look forward to the CLA publishing more work in this area.

Dale Chisamore
Brockville, Ont.

D.J. Hall, *Clifford Sifton, Vol. I: The Young Napoleon 1861 - 1900* (Vancouver: University of British Columbia Press 1981).

CLIFFORD SIFTON IS A FINE WORK in the venerable tradition of Canadian political biography. Organized as a chronological narrative, the volume tells the story of Sifton's rise to political prominence to 1900, first as a Manitoba politician and then as one of Laurier's most important cabinet ministers. Hall also presents much good and useful information on various matters related to the politics of late-nineteenth-century Canada such as the Manitoba schools question, Canadian-American relations, and the control of the *Manitoba Free Press*. Hall's research has been thorough and careful; his writing style clear; and his assessments largely convincing. UBC Press is to be commended for producing a well-bound, effectively but not lavishly illustrated book almost entirely free from typographical errors. The musings which follow should not be construed as undermining in any substantial way the reviewer's opinion that Hall has made a very useful contribution to Canadian historiography.

A political biography of a "near great" tends, of course, to be history from the top down. For example, the author provides valuable information about Sifton's efforts to encourage immigration but gives little material about the migrants themselves. He discusses the administrative problems encountered during the Klondike gold rush but readers wishing to learn about its social history will have to

consult other sources. A biographer must necessarily focus on the actions and perspectives of his/her subject. This is a valid and valuable approach and Hall performs his chosen task well. However, one cannot but wish that this skilled historian had selected a rather broader topic. As Lucien Febvre said to Fernand Braudel: "*Philip II and the Mediterranean*, a good subject. But why not *The Mediterranean and Philip II*? A subject far greater still?"

As for the biographer's orientation to his subject, the book's sub-title is illuminating. Although one does not discover the origins of the term "The Young Napoleon" until the end of the volume, by that time one is not surprised to discover that it was not a friendly nickname but one applied to Sifton by opposition spokesmen supposedly because of his genius at planning, strategy, and tactics. "Young Napoleon" also conjures up such characteristics as ambition, egotism, ruthlessness, authoritarianism, and pragmatism. All these emerge in Hall's portrait of Sifton. Certainly a biographer ought to be critical not eulogistic; however, one begins to think of Sifton in terms that Henry Fonda applied to Walter, that big, mean, ugly, crafty fish, in *On Golden Pond*. One does feel at times that Hall's defence of Sifton's activities is too weak, that his pointing out of Sifton's inconsistencies too obvious, that his "political advantage" explanation of Sifton's actions too simple. To make Sifton out as all sweetness and light, honesty and principle, would be absurd, but one feels rather uneasy that about the best one can say about Sifton is that most of his associates and enemies were just as bad or worse. Undoubtedly Hall has been hampered in presenting a potentially more admirable side to Sifton by the culling of all personal (and most business) correspondence from the Sifton Papers. Sifton emerges rather like a stock character — a wealthy, bigoted, dishonest or at least duplicitous and deceptive, narrow-minded, hard-hearted individual with the "hide of a rhinoceros." (53) One wishes for a more extended discussion of what made Sifton tick — his political philosophy, his social concepts, his view of the world, and so forth — and how his character and perceptions reflected or differed from the space and time in which he lived. One desires, in other words, to be able to understand Sifton more effectively on his own terms and in his own environment.

In his economic conceptions Sifton was typical of many Canadian liberals then and now. Growth or development was the unquestioned deity. The most appropriate form of worship was free enterprise, but there were times when the state needed to bow down with gifts of railway subsidies or immigration promotions which were designed not only to promote the God of Progress but also to strengthen the ability of free enterprise to be the primary means of worship. Sifton was a wealthy lawyer/businessman who not only respected the interests of capital and considered that the state should interfere with those interests as little as possible, but also believed that the business approach should be adopted by government. Thus, for example, Sifton viewed the translation and printing of the legislative debates for the Franco-Manitobans as a waste of money and, therefore, as something to be eliminated. As Minister of the Interior he concluded that automatic annual increases for civil servants under his jurisdiction were not conducive to efficiency and they were therefore withheld. Such business efficiency was frequently as counter-productive in government as in industry. Moreover, it meant translating the businessman's *raison d'être* "how can I gain the most wealth" into the politician's "how can I gain the most political power." It was not an approach conducive to principles or consistency.

There is a postscript to the Sifton story. A short Canadian Press item appeared in October 1982, to the effect that Graeme Sifton, 25, would lose his job as an advertising salesman for the *Win-*

nipeg Free Press, the paper that Clifford, his great grandfather, had once owned. Graeme was reported to have said that the family's fortune did not alleviate the pain of being laid off since "You still have to live today." Ah well, it was just a necessary business decision, right Clifford?

William M. Baker
University of Lethbridge

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

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

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

Professor Morton's discussion of the Great War and the post-war years is first rate. While Canada did not experience "total war" in the European sense, the Great War eventually touched nearly every facet of life in Canada as demonstrated in the expanded role of the state during and after the war particularly in the area of social welfare responsibility. The war was not a unifying experience either, argues Professor Morton, but rather it accentuated the divisions and tensions within Canadian society and politics. For the inter-war years, the author traces the usual themes of Canadian isolationism and defence cutbacks, but he also looks closely at the problem of the veteran and the failure of re-establishment as a means of illustrating the unsettling influence of war. All of these concerns were swept away in September 1939 with the outbreak of World War II. The long truce was over.

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

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

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

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

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

Glenn T. Wright
Public Archives of Canada

Jim Tester, *The Shaping of Sudbury: A Labour View* (Sudbury: Local 598, Mine-Mill Union 1980).

I WISH I HAD BEEN AT THE MEETING of the Sudbury and District Historical Society on 18 April 1979 when Jim Tester spoke on "The Shaping of Sudbury." Those fortunate enough to attend must have been enthralled as they listened to this labour activist with five decades' experience recount many facets of Northern Ontario's mining history from a point-of-view based on his own experience and study. Reading his address, however, is "the next-best-thing-to-being-there" as every page of this little pamphlet resounds with Jim's voice impressing upon its audience the decisive role miners and their successive unions have had in the development or "shaping" of Sudbury.

In the course of conducting labour history interviews, I have often been struck by the vast differences in reactions of workers with seemingly similar backgrounds to their class experiences, first as they lived them, then as they later reflected upon them. Jim Tester, for example, is one of a vanishing breed of remarkable labour leaders who, born into the working class, became dynamic and articulate spokesmen for their class through organizational struggles and non-academic education. What unique combination of childhood and adult influences gave them the courage, the skill, and the vision necessary to become such superb representatives of their class? *The Shaping of Sudbury* may not entirely answer the question of how certain workers acquire the right blend of class consciousness and leadership abilities to push them into the forefront of the struggle; it does provide, however, a concrete expression of the creative capacities this special kind of person possesses.

Tester himself touches on this problem, though somewhat lightly. Although in his view Scots are "opinionated" and are "rarely neutral," he does not attribute his own strong social conscience to his origins in Scotland for his family included "ultra-conservatives" as well as "militant radicals." "It is more a matter of commitment," he believes, "of how ones sees his own life in relation to others and society generally." Additional insight concerning the "calibre and devotion" of people like Tester has recently come from Taimi Davis of Toronto, widow of Jim Davis, another outstanding labour activist and intellect:

> Just what made them the kind of people they were or are? The critical situations called for special kinds of people and it all moulded their characters, quite different from so many labour leaders of today. In the thirties, there were tough jobs to be done and one way or another they got done — and no one had money — that speaks a volume.

Because this little pamphlet so aptly illuminates the unique qualities of this *genre* of individual, it is more than "a short history of Sudbury from a labour point-of-view" as advertised. As a first-hand account of Tester's experiences and thought, it may also be counted as a primary document in the history of Sudbury and, indeed, of the history of mining in Northern Ontario.

Unlike many short outlines of local and labour history, Tester places his subject — the story of the Mine, Mill and

Smelter Workers Union and its impact on the community of Sudbury — within a much broader context. For Tester that context is the history of mining and miners' struggles in Northern Ontario and the enormous influence these struggles had far beyond the bounds of the communities in which these struggles took place.

Did you know, for example, that before Mine-Mill, metal workers traditionally worked 84 hours in one week and that every other Sunday a 24-hour shift was required to effect the shift change? Or that until 1918, the workday for miners was twelve hours, but that in that year, the Conservative Government of Ontario brought in a Mining Act which limited the hours of work in mines to eight hours in one day, this largely the result of political agitation by Cobalt's Local 146 of Mine-Mill.

Or did you know that in 1914 this same local (then in the Western Federation of Miners, Mine-Mill's predecessor) helped bring about Ontario's Workmen's Compensation Act, then the most enlightened legislation of its kind anywhere, and that the Act's stipulation making employers pay all monetary costs in compensation cases has provided a powerful incentive for safety education and safety consciousness? (Or that the issue is still alive today as many employers are mounting political pressure to have this stipulation changed?)

Many readers of this journal perhaps already are aware of these facts; it is unlikely, however, that all members of his audience were, since the activities of labour, even in their own communities, have not until recently come within the purview of most historical societies. Perhaps this is one reason for Tester's address being so convincing, as it is neither a "harangue" as it could have been, nor a simple recitation of the "facts." Intermingled with its narrative of events rather is a careful explanation of the benefits of unionism, not only to union members, but to society generally. All the more effective an approach in Sudbury, which had just come out of a lengthy, and, to many members of the middle class, an incomprehensible strike.

In a period when, according to Gallup Poll figures, public estimation of unionism is not high, Tester's address provides a powerful antidote, for it reminds us all what unions are really about, in addition to wage increases and shorter hours. Especially the kind of unionism represented by the often maligned Mine-Mill and its predecessor, the Western Federation of Miners, whose constitutions have since before 1900 always included an anti-discrimination clause: "Any member who refuses to work with, or discriminates against another member because of race, creed, colour, sex or national origins, shall be subject to trial and discipline. . . ." What a contrast with many American Federation of Labor affiliates who openly practised discrimination against their own members even after World War II!

For Mine-Mill not only did the enforcement of its anti-discrimination clause mean a "conscious educational process" for its membership, but this process was also extended to the nickel companies beginning with the first collective agreements signed at Sudbury. According to Tester, Sudbury is "probably the freest of racial and ethnic tensions" of any city in Canada, all the result of rules established in the work-place. For him, this achievement alone has made the historic struggles of his union worthwhile.

Equally important as the banning of discrimination by management and union members alike, though related to it, is another incalculable benefit of unionism — the pride it gives working people in themselves. With union contracts spelling out such rights as orderly job postings and grievance procedures, employees were no longer subject to "fear and favouritism," to the "whim and caprice" of supervisors. Somewhere in this pride, and in the consciousness of the struggles and traditions

which have made this pride justifiable, undoubtedly lie the reasons behind, and outcome of, Sudbury's recent labour struggles.

These triumphs of Mine-Mill Tester skillfully interweaves into a short history which encompasses not only early mining conditions in Sudbury, but also something of the origins of the Western Federation of Miners, the rise and fall of Local 146 at Cobalt, the defeat of the miners' union at Kirkland Lake in the early 1940s. But central to it all is the story of how Mine-Mill came to Sudbury, of the attempt by Inco and Falconbridge to smash the union through the "Nickel Rash," their company union; of the violent attack on Mine-Mill's office and its organizers, of the union's successful counterattack through a leaflet, reprinted with the pamphlet, and, finally, of the certification of Local 598 at Inco and Falconbridge in 1944. In addition, appended is the transcript of the Question-and-Answer period which offers forceful and thoughtful responses on such wide-ranging topics as contract language and the use of union members, rather than lawyers, as contract negotiators; the IWW and the OBU; and the question of labour militancy, past and present.

In his introductory remarks, Tester lodges a legitimate complaint against the kind of traditional history which overlooks working people and the interaction between classes in favour of illustrious leaders being the determining factor in the course of events. As examples he points to two works on Sudbury, *For Years to Come* by John F. Thompson (past-president of Inco) and *Sudbury Basin* by D.M. LeBourdais (commissioned by Falconbridge,) both of which, not surprisingly ignore the role of Mine-Mill. He might also have included J.E. Havel's *Politics in Sudbury* which mentions neither Inco nor Falconbridge, and only cursorily the press of the Mine-Mill and Steelworkers Unions, as factors influencing the electoral process in that community. Fortunately, these omissions will be corrected by the book Jim Tester is now preparing for publication on the history of Mine-Mill in Sudbury. Given the quality of this splendid 38-page booklet, all those interested in the history of both labour and Northern Ontario can await its appearance with much anticipation. It is to be hoped, however, that Jim makes no omissions of his own. While the historical society admittedly was not the place to introduce the subject, Tester's book, if it is to tell the whole story, will have to include the controversial and conflicting roles played by the political parties of the left, not only in the shaping of Sudbury, but in the shaping of Mine-Mill itself.

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Robert Chanteloup, ed. "Labour in Atlantic Canada," *Social Science Monograph Series*, IV (Saint John: University of New Brunswick at Saint John 1981).

UNTIL RECENTLY, SOCIAL SCIENCE research on Atlantic Canada has been as "underdeveloped" as the region's economy. While within the last decade a growing number of social historians and sociologists have focused their concerns on the area (for example, Brym & Sacouman, eds. *Underdevelopment and Social Movements in Atlantic Canada*), a paucity of information about the region still remains a problem. If for this reason alone, a social science monograph on "Labour in Atlantic Canada" should be received enthusiastically. Unfortunately, this enthusiasm quickly fades as one reads through the eleven articles which make up this collection.

Any collection of articles by various authors runs the risk of being fragmentary or appearing disorganized; thus, it is an editor's responsibility to ensure against this through editorial remarks, synopses, and the organization of materials. Such a task is especially critical when the subject matter is as general and all-encompassing

as that of "labour" and is approached from a multiplicity of perspectives and disciplines. To produce an intellectually coherent work, the editor must explain the theoretical and methodological scope of the problem, provide a rationale for selecting the specific contributions, and indicate the significance of the work in relation to existing research in the field. Robert Chanteloup's effort is seriously deficient on all these counts.

Chanteloup informs us in his too brief and sketchy "Preface" (totalling three pages) that while the editorial board gave some thought to focusing on specific dimensions of labour they "came to the inescapable conclusion that virtually no lines could be drawn. Labour is just too fundamental and too pervasive. As this volume shows, casting a broad net produced an equally varied harvest." (4) One can hardly disagree with the argument that "labour" is complex and multi-faceted; Chanteloup's conclusion, however, from this is highly dubious. As social scientists we, of course, can never study the "totality" of anything. Instead, we focus our research in certain directions and work within the theoretical perspectives we find most relevant, all the while recognizing the limitations of our approach. However, a "laissez-faire" attitude seldom, if ever, results in good scholarship. We insist that our students explain the parameters of their research; surely we have a right to expect the same from our colleagues. Furthermore, we must keep in mind that while a "varied harvest" is colourful, it is not necessarily beneficial nor fruitful.

The monograph's most serious shortcoming is its lack of focus. The purpose of this monograph, the editor tells us, was to "draw together . . . some of the labour research being carried on throughout the region . . . and stimulate interest in . . . and awareness of . . . a topic which has so much crucial significance for Atlantic Canada." (6) Except in some vague and superficial way, the monograph does not fulfill this objective. There appears to be no set of theoretical assumptions, no particular methodological guidelines, no rationale for the specific topics included and no framework linking the diverse contributions. Chanteloup's introduction is both uninformative and at times misleading. For example, he states that Donnelly's study of Campobello Island provides support for Marx's thesis that labour is the most fundamental human activity. While Donnelly may accept this premise, his research and the data he presents do not warrant such a conclusion.

More importantly, some of the basic questions one needs to ask about labour in the region are never, with few exceptions, asked. Issues, like the shifting patterns of employment opportunities, the degree of paid and non-paid labour, the wider social structure in which labour exists, and the significance for labour of the region's socio-economic context and historical development are not even considered. Given the vast body of theoretical and empirical literature in Canadian political economy and the generally accepted thesis that social, economic, political, and historical factors have a significant influence on labour, I find it puzzling as to why these factors were neglected.

To ignore the political economy of an "underdeveloped" region like Atlantic Canada when studying the "crucial significance" of labour is to assume either that the conditions facing labour are the same everywhere (which begs the question of why focus on the region), or that labour is "autonomous;" that it is not affected by other socio-structural factors (which leads one to ask why bother studying it). Both assumptions are, of course, incorrect. Existing research has already demonstrated that the structural constraints placed on labour in the periphery differ from those faced by labour elsewhere. Failure to recognize this is reminiscent of the 1960s, when many American-trained sociologists argued that understanding Canadian society meant nothing more than looking at suicide

rates, crime statistics, and voting patterns in Toronto rather than San Francisco. Fortunately, most have now given up this ideological fallacy.

The shortcomings created through this lack of focus and the absence of theory are further compounded by the uneven quality of the articles themselves. We never learn the editor's rationale for selecting these particular articles written by historians, sociologists, political scientists, economists, people in management and administration, and government types. The contributions differ in terms of their methodological rigour, research objective, length, style, and content. Since frequently it is unclear what the central point of an article is, or how it relates to the general concern of labour in Atlantic Canada, it is difficult to evaluate its merits.

Some articles are purely descriptive or journalistic; others are more analytical. Some address social policy concerns; others focus on more structural problems. Some are scholarly and informative; others are highly polemical. One article is 26 pages in length, another is five. Furthermore, Chanteloup neglects to organize these contributions under any headings; nor does he give any clues of how they relate to one another. The topics covered include such diverse interests as Elizabeth Gilbert's "The Social Worker in the Work Place: A Possible Alternative to Grievance Arbitration" (a proposal for using social workers as mediators in employer-employee conflict), Hugh Lautard and Donald Loree's "Occupational Differentiation between British and French in the Atlantic Provinces, 1951-1971" (which calls into question Porter's vertical mosaic thesis), Fred Donnelly's "Occupational and Household Structures of a New Brunswick Fishing Settlement: Campobello Island, 1851" (a demographic study of 132 households), Robert Garland's "The Misfortunes of the Labour-NDP Alliance in Atlantic Canada" (a highly polemical work which has more affinity with "back-room" politics than academic scholarship, and James Overton's "The Myth of the Reluctant Worker: Attitudes to Work and Unemployment in Newfoundland" (a political economy analysis of the structure of unemployment and its effects on workers' attitudes). While interdisciplinary research should be encouraged and applauded, it must occur within some framework. Otherwise the end product is highly unsatisfactory.

Despite these weaknesses, however, there are some articles which merit attention. James Overton's contribution (the most analytical piece) is a good structural analysis of unemployment, which situates the present conditions of Newfoundland workers and their value systems within the broader social context of uneven capitalist development. In this informative work, the author examines unemployment not as a consequence of individualistic attitudes towards work, as suggested by the Economic Council of Canada, but rather as shaped by the structure of labour in the periphery, where numbers of people frequently serve as a reserve army of labour. Hazleton's study of the failure of the labour movement in Prince Edward Island is primarily a descriptive account of two strikes on the Island. Hazleton does suggest a relationship between the structure of the Island's agrarian economy and the attitudes of individualism, both of which mitigated against any sympathy for union organizers.

Other contributions in the monograph, for example, Kell Antoft's study of the Nova Scotia Joint Labour-Management Study Committee and Richard Apostle and Donald Clairmont's findings on the marginal work world provide some interesting data; being devoid of a theoretical context, however, these articles do little to further our grasp of Atlantic Canada. Similarly, Hugh Lautard and Donald Loree's work and Peter Toner's analysis of "The Loyal Orange Lodge" are interesting in their own right; yet in neither case is it clear how they increase our awareness of labour in the region.

There is without doubt useful information in at least some of the articles. Students of the Atlantic region and Atlantic Canadian society will find a few of the articles both interesting and informative. Overall, however, this monograph lacks academic merit. The editor's failure to provide a rationale for the selection of articles, the absence of a theoretical framework, and editorial comments which would assist the reader and impart a context for analysis, plus the uneven quality of the contributions are all serious deficiencies of this volume. Also, interestingly enough, that while a broad net was cast, the harvest reaped did not include any research pertaining to women's unpaid work or women's contribution to labour in the region. As a result of these shortcomings, we never get any holistic understanding of the structure or significance of labour in Atlantic Canada.

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Gérard Parizeau, *La vie studieuse et obstinée de Denis-Benjamin Viger (1774-1861)* (Montreal: Fides 1980).

IN THE LAST CENTURY, the amateur historian was an important figure on the cultural stage. Even as Ranke, research, PhDs, and footnotes came to rule in academic circles, the writing of history remained for many members of polite society an after-dinner avocation like breeding roses or collecting butterflies. At its best, amateur history could be very good, more generous and adventurous in its approach to the past than the sterile and specialized works of many scrupulously "scientific" professionals. On the other hand, it could be quite bad: parochial in scope, prolix in presentation, lax in distinguishing between original and derivative points. Most often these works were strongly marked by the moral or patriotic designs of the amateur author who had more difficulty than his professional counterpart in suppressing the impulse to sermonize blatantly.

This nineteenth-century tradition is by no means dead; Gérard Parizeau, an insurance executive, prolific amateur historian, and member of the Royal Society of Canada, has managed to keep it very much alive in recent years. His subject in this work is Denis-Benjamin Viger, a key figure in Lower Canadian political life of the first half of the nineteenth century. The cousin of both Bishop Lartigue and Louis-Joseph Papineau, Viger was a close associate of the latter and a respected leader of the *patriote* movement. Although not active in the insurrections of 1837-38, he was nevertheless imprisoned under the regime of martial law. Stubbornly opposing the union of the Canadas, Viger stood aloof from the emerging Baldwin-Lafontaine party. In 1843 he shocked the proponents of Responsible Government by accepting the offer of the "despotic" Governor Metcalfe to form a ministry. Viger remained active in politics until the 1850s, always championing the national rights of French Canada and giving vent to his increasingly conservative political and social views. If you wish to find out more about D-B Viger, I would recommend the *Dictionary of Canadian Biography* article by André Lefort and Fernand Ouellet, which is vastly superior in every respect to Gérard Parizeau's biography. The latter is of interest only as a historiographic curiosity, the worst sort of out-dated amateur history.

There is an antique odour to this biography with its indifference to current historiographic problems and its interest in the moral qualities of its central character. These qualities are of course mostly positive — Viger was intelligent, tenacious, peaceful, courteous, amiable.... If he launches a lawsuit over a contested inheritance involving his wife, he does so because of the legal principle at stake, not in order to acquire a piece of property. If he accumulates the second largest real estate empire in the city of Montreal, it is

not to make money for himself, but to give generous donations of land to the church. Parizeau's writing style is ridiculous. He refers once to the Public Archives of Canada as "la vieille dame de la rue Wellington" and is so pleased with the phrase that he repeats it four times at various points in the text. The organization of this work is chaotic. One gets the impression that a saboteur must have broken into the author's study and secretly shuffled his file cards so that no order — topical, chronological, or alphabetical — could ever emerge when they were transcribed. The text bounces from digression to repetition to irrelevant quotation as aimlessly as the marble in a pin-ball machine.

In the end, Gérard Parizeau has virtually nothing to tell his readers about Denis-Benjamin Viger and he is only able to fill up the 300 pages required for a respectable volume by the most barefaced padding. For example, documents readily accessible in published collections are quoted *in extenso* in three and four page chunks. Even descriptions of the author's working conditions serve to fill a few lines in introducing a paraphrase of one of Viger's political pamphlets: "Jugeons-en par ce résumé que nous en avons fait un jour à Nice, en écoutant d'une oreille distraite la guitare d'Yepes, tout en jetant un coup d'oeil admiratif sur les plus belles tulipes qui soient." (160) It is disappointing to find this sort of drivel published by Fides, a firm that has printed some first-rate historical works in the past.

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M.-A. Bluteau, J.P. Charland, M. Thivierge et N. Thivierge, *Les cordonniers, artisans du cuir* (Montréal: Boréal Express/Musée national de l'homme 1980).

LE JOUR MÊME où j'ai commencé la lecture de l'ouvrage collectif de Bluteau, Charland et Thivierge sur *les cordonniers, artisans du cuir*, le quotidien *La Presse*, mon employeur, faisait la manchette de son cahier d'économie et finance, avec le titre suivant: "2,000 mises-à-pied dans la chaussure." La même semaine, deux jours plus tard, le magazine *Perspectives*, publié dans plusieurs quotidiens du Québec, publiait un article titré: "Un Cordonnier? Mais c'est une perle rare!"

Il s'agit peut-être plus que de coincidences: quand l'on fait de l'histoire sociale aujourd'hui, à plus forte raison quand l'on se penche sur les travailleurs, il est bien difficile de ne pas déboucher sur le présent, et peut-être même sur le futur. Les puristes et les tenants de l'histoire traditionnelle, axée sur l'analyse du passé, s'en scandaliseront peut-être, mais de plus en plus de nouveaux historiens, surtout ceux qui font de l'histoire sociale, ne craignent pas de s'engager dans le contemporain, même s'ils deviennent alors peut-être davantage sociologues qu'historiens. Ils se trouvent donc à porter jugement sur le présent et certains, même, comme nos quatre historiens, se font même futurologues ou moralistes et y vont de leurs bons conseils.

C'est peut-être le fait que j'ai commencé la lecture de leur ouvrage au moment même où *La Presse* attirait l'attention de ses lecteurs sur le marasme de l'industrie canadienne de la chaussure, qui fait que j'ai trouvé tout à fait normal que les auteurs qui, sous la direction de Jean Hamelin, se sont attelés à décrire l'évolution du métier de cordonnier de la Nouvelle-France à nos jours, y soient allés de leurs conseils pour redresser, si possible, cette industrie, plongée actuellement dans le marasme, ce qui est peut-être un peu risqué pour des historiens, qui ne sont pas des économistes, mais pas complètement, selon moi, hors de propos.

Ceci dit, le travail des jeunes historiens, Bluteau, Charland et les deux Thivierge en est d'abord un d'historique. Une histoire d'autant plus d'actualité que les

récentes célébrations du 60ième anniversaire de la CSN et le lancement de l'ouvrage de Jacques Rouillard sur la question qui nous a rappelé le rôle fort important des ouvriers de la chaussure dans la naissance de la Confédération des travailleurs catholiques du Canada, ancêtre de la CSN. La CTCC, en effet, doit en bonne partie sa naissance au fait qu'à l'aube du 20ième siècle, le clergé québécois, "afin de protéger le monde ouvrier des doctrines jugées subversives, comme le socialisme européen ou le matérialisme américain (Gompérisme)," amorce un processus de noyautage des unions nationales qui regroupent les cordonniers de la vieille capitale. Les cordonniers, avec les Chevaliers de Saint-Crépin et d'autres organismes plus anciens dont on ne sait que peu de choses et qui remontent à 1835, furent parmi les premiers ouvriers à se regrouper pour protéger leurs intérêts au Québec.

Les cordonniers furent d'ailleurs parmi les premiers grévistes de longue durée au Québec. Une grève dura neuf longues semaines à Québec dès 1869. Malheureusement, de dire les auteurs, on ne connaît pas les termes de l'entente qui mis fin au conflit. Les grévistes furent parmi les premiers au Québec, semble-t-il, à bénéficier d'un fonds de grève. La grande loge des Chevaliers de Saint-Crépin leur fournit $3,472.78, somme considérable pour l'époque.

Les quatre auteurs de l'étude *Les cordonniers, artisans du cuir* ne sont guère tendres pour le genre de syndicalisme pratiqué par les cordonniers. "Depuis 1900, écrivent-ils, le clergé québécois a réussi à récupérer complètement le mouvement syndical de la chaussure, à lui faire brandir le drapeau de la respectabilité et de la morale chrétienne. Il ne semble pas que les aumôniers syndicaux ou même l'archevêché aient voulu livrer les ouvriers pieds et poings liés au patronat, mais ils se sont substitués aux élites ouvrières, ont donné aux travailleurs des buts et une pratique qui leur était étrangère. Et dans cette mesure, ils ont participé à un processus d'aliénation des travailleurs en les empêchant de développer eux-mêmes une vision du monde et une politique qui leur seraient propres. Si économiquement, l'ouvrier a perdu le contrôle de son travail, idéologiquement, il a perdu son autonomie, sa spontaniété, sa liberté."

Comme le font remarquer les auteurs dans leur avant-propos, les temps ont bien changé et la manchette de *La Presse* d'octobre dernier, annonçant 2,000 mises-à-pied dans la chaussure, n'a pas eu l'impact qu'elle aurait eu un demi-siècle plus tôt. "On oublie facilement de nos jours," écrivent-ils, "qu'il y a à peine deux générations, Québec était un grand centre de manufactures de chaussures."

Comme la plupart des ouvrages historiques publiés par Boréal Express, l'ouvrage sur les cordonniers et les artisans du cuir est abondamment illustré. En fait, on retrouve pour ainsi dire une photo ou gravure par page, grâce ici à la collaboration du Musée national de l'homme, co-éditeur de l'ouvrage, et du Centre d'études sur la langue, les arts et les traditions populaires (CELAT), de l'Université Laval, en plus des Archives nationales du Québec. Cette utilisation massive de l'image rend, selon moi, la lecture de ces ouvrages plus agréable et on devrait ainsi réussir à intéresser à l'histoire davantage de gens. La connaissance de nos origines a ainsi plus de chances de dépasser le cercle des seuls initiés.

Comme tout ouvrage, celui des auteurs de l'étude sur les cordonniers, artisans du cuir, ne vide pas toute la question. On n'a toujours pas trouvé les archives de la *"Quebec Journeyman Shoemakers,"* de 1835, certainement un des premiers organismes de défense des travailleurs au pays. On ne sait toujours pas si cette société a vécu longtemps, quels en étaient les membres et quelles luttes elle a mené. On n'en sait pas plus sur la *"Société bienveillante des cordonniers de Québec,"* fondée en 1867.

À noter que les manufacturiers de chaussures de Québec, eux, figurent parmi les pionniers du "syndicalisme patronal" et de l'actuel Conseil du patronat. Groupés en association dès 1889, les manufacturiers de Québec purent organiser une stratégie commune face aux organisations ouvrières.

De même, un peu plus loin, les auteurs avouent que les documents à leur disposition sont à peu près muets sur l'origine, l'âge, l'instruction, le niveau de vie et la situation du logement des travailleurs de la chaussure de Québec à la fin du 19ième siècle. Plusieurs années de recherches seront requises pour répondre à ces questions, écrivent les auteurs.

Qu'il nous soit permis de souhaiter que ces recherches soient entreprises au plus tôt. Selon nous, à la lecture du livre des Bluteau, Charland et des deux Thivierge, l'enjeu en vaut la peine.

Pierre J.G. Vennat,
chroniqueur syndical à
La Presse, Montréal

Paul Larocque, *Pêche et Coopération au Québec* (Montréal: Éditions du Jour 1978).

À TRAVERS LE VOLUME *Pêche et coopération au Québec* Paul Larocque apparait nourir une double ambition: d'abord celle d'enrichir la documentation historique concernant le développement plus récent des pêcheries gaspésiennes et plus spécifiquement celui qui s'est operé sous l'égide de la coopération. C'est un travail bien fait dont le modèle devrait mettre un terme au style d'études dont nous avons dû nous contenter jusqu'à maintenant dans le domaine: rapports de fonctionnaires distillés à même d'autres rapports de fonctionnaires. Ce livre est aussi celui d'un practicien engagé qui scrute le développement de la Coop. Pêcheurs Unis du Québec à la lumière de l'idéal coopératif "de prise en charge collective débouchant sur l'expérimentation socio-politique;" on y voit naître puis progressivement s'émousser l'idéal associationiste sous la pression invincible de l'entreprise privée menacée dans ses fondements même par le projet, du clergé gaspésien globalement d'accord avec les objectifs, mais opiniâtrement déterminé à en demeurer le maître d'oeuvre, du gouvernement provincial apôtre de la rationnalité qui y fait triompher le règne du savoir faire scientifique.

Deux concepts vont servir à situer Pêcheurs Unis du Québec et en quelque sorte à donner le sense de son évolution: ceux de mouvement et de développement. Peut on parler de PUQ d'abord comme un *mouvement social* caractérisé par "le non-conformisme, le rejet de l'ordre établi, la mobilisation et les luttes" puis s'éloignant progressivement de ce rôle par l'institutionnalisation et la transformation en un groupe d'intérêt avec direction administrative, préoccupé de gestion, d'efficacité? . . . Second concept, Pêcheur Unis s'est-elle occupée de *développement*, cette conjugaison harmonieuse "d'une utilisation optimale des ressources humaines et matérielles avec les attentes exprimées par les membres d'une société," ou bien, en tant que producteur a-t-elle poursuivi des objectifs de croissance "d'abord centrés sur la rentabilité des investissements individuels?"

Evidemment, pour l'auteur, ces concepts ne constituent que des points de référence: Pêcheurs-Unis n'a jamais rien eu d'un mouvement révolutionnaire, porteur d'un projet de société nouvelle. On constate plutôt que les années ont fortement érodées l'idéal associationiste animant les propagateurs des premières heures de sorte qu'on doit aujourd'hui en parler comme d'une institution. De la même façon, la volonté de rédemption des économiquement plus démunis et l'embryon de critique du libéralisme ébauché au cours des années 40 va progressivement se résorber entre les mains d'administrateurs "compétents et plus conscients des réalités économiques ambiantes." S'agit-il alors, d'excommu-

nier Pêcheurs Unis des rangs bienheureux de la coopération? Non, mais c'est avec "une pointe de nostalgie" que l'analyste cherche vainement une autre solution viable au développement des pêcheries québécoises.

Dans quelle mesure le mode actuel d'exploitation des pêcheries québécoises peut-il être qualifié d'échec? Pêcheurs Unis a la main haute sur plus de 75% des activités du secteur. En contexte d'économie libérale, la coopération n'est pas bienvenue. Même qu'elle ne pourra prendre racine que dans des secteurs où l'entreprise privée ne trouve pas son compte. On retrouve l'entreprise privée dans les pêcheries du Québec au moment où l'individualisme et l'ignorance des pêcheurs vont lui laisser une large marge d'exploitation. Puis elle suivivra longtemps parce que grassement entretenue par des pouvoirs publics qui l'instituent, dès lors, agence déguisée de distribution d'aide sociale, tout en organisant l'horaire des envois de fonds pour un maximum d'effet électoral. Il y a quelques années à peine, l'état de développement de PUQ a finalement décidé le gouvernement à laisser aller les multinationales du poisson engouffrer des fonds public ailleurs.

La pratique coopérative est un mode de propriété et de gestion de l'entreprise fort exigeante sur le plan de la participation des sociétaires en dehors des heures normales de travail: il y a tout un mode de vie et de répartition traditionnelle des tâches sociales qui vient faire obstacle à cette implication du grand nombre. L'horaire bien particulier du travail de pêcheur ferait même de celui-ci un "délégateur naturel de pouvoir." Il ne sera vraiment disponible qu'en dehors de la saison de pêche.

Et cette saison de pêche, dans quel contexte se déroule-t-elle? Voici quelque-uns des thèmes qui nourissent la réflexion des travailleurs du secteur: saisonnalité des pêches, tempêtes, perissabilité des captures, bris de bateaux, sous utilisation des usines, approvisionnement irrégulier des usines, horaire irrégulier des travailleurs de la transformation (roulement de main d'oeuvre, absentéisme, ralentissement, etc. . . .), second transformation négligeable, faiblesse du marché intérieur, soumission aux volontés du marché américain, pollution et toxicité, négligeable connaissances biologique, double palier gouvernemental de juridiction, etc. . . . Chaque jour déverse sa manne de problèmes et continuellement les fonctionnaires sillonnent la côte dans un frou-frou de paternalisme ardent, vérifiant l'état des bâtisses, des quais, des bateaux, des agrès, rendant jugement sur l'éligibilité à une subvention à la capture de poisson, à la réfrigération, à l'achat de glace, à l'achat d'agrès, de bateaux, au transport du poisson vers les usines, etc. . . .

La bande de jeunes techniciens qui en 1964 a deferlé sur la Gaspésie, dans le cadre du BAEQ, avait une puissance accumulée suffisante pour sonner le glas définitif du développement autonomes des pêcheries, n'eut été de la détermination qu'a démontré PUQ à suivre sa propre voie et à demeurer en charge de ses affaires. À la lecture de *Pêche et Coopération,* on voit se manifester de façon persistante cette résistance aux attaques des accapareurs: c'est là une des grandeurs de Pêcheurs Unis et aussi ce qui laisse croire que cette histoire n'est pas terminée. . . .

Ceci étant dit, reste encore a poser les premiers "jalons" de gestes concrets qui pourraient mener de la situation décrite par Paul Larocque à ce grand espoir du développement régional communautaire dans une Gaspésie à qui tout et tous crient que les choses "importantes et modernes" se produisent ailleurs, là où sont imprimés les journaux qu'on y lit, réalisés les émissions de télévision qu'on y regarde, là d'ou viennent les fonctionnaires qui y grenouillent. . . .

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Louise Clermont-Laliberté, *Dix ans de pratiques syndicales, la CEQ 1970-1980* (Sainte-Foy: Centrale des enseignants du Québec, Service des communications 1981).

PARFOIS, POUR BIEN comprendre un texte il convient de s'attarder à la fonction qu'on a voulu lui voir remplir. Cela s'impose tout à fait quand il s'agit de l'ouvrage de madame Clermont-Laliberté. Mais d'abord, nous voulons présenter l'auteure. Celle-ci a entretenu de forts longues relations avec la Centrale des enseignants du Québec, en y travaillant bien sûr, mais aussi en ayant rédigé sur elle une thèse de maîtrise en sciences politiques (*Fractions, tendances et rapports imaginaires: la CEQ, 1965-1973*. Thèse de maîtrise, Université Laval, 1977) et une étude pour le compte de Léon Dion (*Culture politique de la CEQ: 1965-1973*, Laboratoire de sciences administratives et politiques, Université Laval, 1975, 405 p.). Voilà pour l'auteure! L'oeuvre maintenant: elle résulte d'une commande de la centrale, désireuse de se procurer un document susceptible d'aider à faire un bilan de ses pratiques syndicales, afin que les militants puissent s'en inspirer lors de son congrès d'orientation. Ce congrès, confesse elle-même madame Clermont-Laliberté, est venu de "la crainte de perdre définitivement le leadership dans l'expression des grandes orientations de l'organisme syndical." Le fossé idéologique se creusant entre les dirigeants et les membres, il s'agissait d'éviter de perdre l'adhésion active de ces derniers. Pour ce faire, la stratégie de la centrale ne s'est pas limitée à ce retour sur le passé, bien au contraire. On a mis en branle l'opération "Proposition d'école," étalée sur deux années et qui aboutit à la prise d'une série de résolutions au congrès d'orientation. Dans ce cas, on a mené une vaste consultation auprès des membres afin de trouver une "plate-forme" de revendications capable d'obtenir l'assentiment de la majorité, et sur laquelle mener à lutte. Cette rupture d'avec les campagnes menées tambour battant, en clamant des slogans qui disaient l'urgence de changer le système, ce changement dans la stratégie n'est pas étranger au travail de légitimation après-coup entrepris par madame Clermont-Laliberté.

L'ouvrage s'attache à l'évolution de la centrale sur quatre grands axes: le rapport à l'employeur, le rapport à l'État, le rapport au mouvement syndical et au mouvement ouvrier. La première partie du texte se rapporte à ceux-ci, la seconde sur la défense des intérêts économico-corporatifs des travailleurs de l'enseignement, et la dernière sur les propositions de changement social. De fait, c'est à l'établissement du cadre d'analyse que l'autrice se livre pendant la première partie, alors que par la suite elle s'intéresse au vécu de la centrale. Elle s'attache d'abord à l'ambiguité du syndicalisme, qui vise bien sûr à limiter l'exploitation et la domination que subissent les travailleurs, mais qui assure aussi la gestion de la lutte des classes. La convention collective vient en effet institutionnaliser, et de ce fait circonscrire l'action syndicale. La reconnaissance syndicale et l'obligation de négocier de bonne foi imposent les cadres du syndicalisme d'affaire. Malgré cela, madame Clermont-Laliberté s'oppose à ceux qui prétendent que le syndicat est une créature patronale, ce en quoi nous l'appuyons.

Dans le cas des travailleurs des services publics, le rapport à l'employeur ne se fait pas avec le propriétaire des moyens de production désireux d'accumuler la plus-value, mais plutôt avec les politiciens et leurs gérants, responsables du maintien de l'ordre capitaliste. Chez les associations d'enseignants, pendant les années soixante le corporatisme s'est mesuré au syndicalisme. C'est l'époque des premières négociations collectives, et de la première convention provinciale, celle de 1966. C'est la centrale qui doit assurer, dès ce moment, les négociations pour toute la province. Mais malgré le succès

de l'idée syndicale, peut-on parler dans le cas des enseignants, d'appartenance à la classe ouvrière? Madame Clermont-Laliberté tente de répondre par l'affirmative. C'est à grand renforts de références à Gramsci que l'autrice démontre que les enseignants, réunis en syndicats, sont la fraction intellectuelle de la classe ouvrière, qui lui permet de dépasser une action strictement réformiste, alors que l'État, au service de la classe dominante, tente de gérer les relations de travail de telle manière que l'ordre établi ne soit pas menacé. Mais cette démonstration demeure un peu trop rondement menée, et on ne peut s'empêcher de remarquer que l'auteure ne fait aucune référence directe à l'oeuvre de Gramsci, et ne semble s'appuyer que sur une lecture de seconde main, soit l'analyse qu'en a faite J.-M. Piotte (*La pensée politique de Gramsci*. Paris, Éditions Anthropos, 1970).

Une fois établi que les enseignants représentent la fraction "intellectuelle" de la classe ouvrière, madame Clermont-Laliberté s'attache aux relations de la centrale avec le mouvement syndical, puis avec le mouvement ouvrier. Dans le premier cas, celle-ci a d'abord rompu avec les réunions tripartites (syndicats-employeurs-État) destinées à bien gérer les relations de travail, pour se rapprocher des autres associations de travailleurs, comme le démontrent les fronts communs successifs. L'auteure ne craint pas d'affirmer que "la CEQ des années soixante-dix peut-être étudiée sous l'angle d'intellectuel collectif qui s'est lié à la classe ouvrière dans un mouvement de prise de conscience du rôle créateur du prolétariat pour définir une nouvelle civilisation." La centrale intervient donc pour que la classe ouvrière puisse dépasser le trade-unionisme, ce qu'elle ne pourrait faire seule s'il faut en croire Lénine; et c'est en l'absence d'un parti révolutionnaire qu'elle accepte de jouer ce rôle d'avant-gardre. Comme on le voit les enseignants n'assument pas leur rôle de quidage qu'en classe. . . .

La seconde partie de l'ouvrage, qui s'intéresse à la défense des intérêts économico-corporatifs des enseignants, vient faire le détail de la solidarité qui s'est tissé entre la centrale et les autres organismes voués à la défense des intérêts des travailleurs. C'est la genèse des fronts communs, des efforts pour orienter vers de nouvelles priorités les dépenses publiques, pour la réduction des écarts salariaux entre les diverses catégories de travailleurs, etc. Malheureusement l'auteure donne peu de chiffres, une multitude de renseignements nous font défaut. Faut-il les signaler au risque d'être qualifié d'intellectuel au service de la classe dominante? Pourquoi ne pas faire état des salaires, des conditions de travail des enseignants, et de les comparer à ceux des autres catégories de travailleurs, à la capacité de payer de l'État. Cela permettrait pourtant d'appréhender de manière moins théorique l'appartenance des enseignants à la classe ouvrière, dont ils seraient l'expression consciente. L'auteure semble tenir à conserver la confusion entre la défense d'intérêts purement corporatifs et ce rôle d'avant-garde . . . dont il est tellement question dans la troisième partie du volume.

Les chapitres concernant les propositions de changement social s'ouvrent de manière prévisible, tout à fait conforme aux écrits de Gramsci: à la nécessité de conquête politique et économique s'ajoute celle de la conquête idéologique, de l'hégémonie à exercer dans ce domaine par la classe ouvrière. Il s'agit pour celle-ci de s'opposer aux appareils idéologiques de l'État bourgeois. Se présentant, on l'a vu, comme la fraction consciente du prolétariat, la centrale s'attribue une fonction de guidage à ce propos. Notamment à partir de 1972, car après la présentation de l'étude *L'école au service de la classe dominante* au congrès de cette année-là, elle consacre une part importante de ses ressources à son mandat d'éducation, dans le but de faire passer la classe ouvrière du syndicalisme d'affaire à un

syndicalisme de combat. Bien sûr, la lutte des travailleurs de l'enseignement s'effectue surtout au niveau de l'école, principal appareil idéologique de la bourgeoisie, comme nous l'a appris Louis Althusser.

Naît donc en 1972 la Commission d'enquête sur le rôle de l'école et de l'enseignement (CÉREE) en société capitaliste. Aux dires d'Yvon Charbonneau, *nouveau président de la centrale*, la commission est à la fois un instrument d'animation, de formation, de recherche militante et de conscientisation. Il faut détrôner l'idéologie d'égalisation des chances par l'école et mettre en évidence son rôle de reproduction des inégalités. Il faut contrer l'idéologie dominante au sein même de l'institution scolaire en y opposant une pédagogie de conscientisation, capable de faire comprendre leurs intérêts de classe aux étudiants, en majorité fils de travailleurs. Les travaux de la CÉREE aboutissent à la publication, en 1974, du texte *École et luttes de classes au Quebec*, et en 1975 de *Pour une journée d'école au service de la classe ouvrière "Manuel du 1er mai."* Toutes ces publications visent à la fois la formation des étudiants, des autres travailleurs et des enseignants eux-mêmes, que leur séjour à l'école normale ou à l'université n'a pas initié à une pareille analyse du rôle de l'école.

L'auteure passe rapidement sur la réception réservée à ces documents. Ils ne semblent pas avoir soulevé l'ire des membres, sans doute parce que leur parution coïncidait régulièrement avec des négociations difficiles avec l'État-patron. Cela ne signifie pourtant pas que tous ont adhéré à leur contenu. Madame Clermont-Laliberté paraît tout à fait désireuse de masquer cet aspect de la réalité, et c'est dans *Proposition d'école: défis d'aujourd'hui et propositions de demand* (CEQ, juin 1980) qu'il fait aller chercher ce qui me semble être la meilleure analyse de la question. On y confesse en effet que les attaques lancées contre la "*Manuel du 1er mai*" ont rencontré des oreilles attentives chez les enseignants eux-mêmes. On va jusqu'à écrire que des membres "sont devenus très critiques face aux orientations prises par la Centrale en 1971-1972; ils ont alors tenté de réduire le rôle de direction de la Centrale pour insister au plus haut point sur son rôle de représentation des opinions et de la pensée des membres." De cette situation est née l'idée d'un congrès d'orientation s'appuyant sur *l'opération "Proposition d'école" qui ne* vient pas, comme l'affirme madame Clermont-Laliberté, du désir de la CEQ de ne pas mettre tous ses oeufs dans le même panier.

Plutôt que de continuer à lancer ses analyses sur le rôle de l'école capitaliste, la Centrale a essayé, entre 1978 et 1980, de réaliser un consensus sur un ensemble de revendications, et cela avec un nouveau président pour le moins plus discret que monsieur Charbonneau sur les orientations politiques à donner au mouvement. Il est dommage que l'auteur aît préféré ne pas approfondir cette question de l'adhésion des membres aux orientations de la Centrale. Et c'est avec scepticisme que nous devons recevoir ses explications sur le changement d'attitude de la CEQ. En effet le désir de ne pas s'opposer au parti québécois n'explique pas seul l'adoucissement du ton, mais je serais prêt à reconnaitre avec elle qu'il était plus stratégique de recourir à cette argument. Et de fait, les propositions d'école, en faisant alterner des revendications propre à satisfaire les intérêts économico-corporatifs des enseignants et d'autres s'attaquant au rôle de l'école capitaliste pourront mieux séduire que les études structuro-marxistes antérieures.

Le dernier chapitre de madame Clermont-Laliberté s'attache aux moyens propres à favoriser le changement social. À ce sujet, l'auteure remet aux dirigeants des organisations ouvrières un rôle directeur, et affirme pour la CEQ: "Le Congrès assume très explicitement cette fonction de direction idéologique et politique qui lui revient de droit." Elle y fait le détail des institutions, de la structure céquiste,

en précise les rôles, les responsabilités. Toutes sont les plus propres à conduire au but fixé: changer la société. La conclusion se termine pourtant par un recul . . . stratégique sans doute. Renier sa fonction hégémonique, se rabattre sur les luttes menées pour les intérêts matériels des membres: "En somme, dit-on au dernier paragraphe de l'ouvrage, c'est à partir des luttes pour la défense de leurs intérêts économico-corporatifs que la CEQ a tenté de jouer un rôle de chercheur et d'éducateur. . . ." Une vieille tactique céquiste que de faire des deux terrains d'action de la centrale un "package deal."

Le travail de madame Clermont-Laliberté n'est certe pas sans intérêt. Cette étude fait partie d'une batterie de textes que la CEQ à lancé pour exercer son hégémonisme idéologique et culturel, que ce soit auprès de ses membres ou des autres travailleurs. Mais quand on veut y voir une étude scientifique, l'auteure a omis de se poser un certain nombre de questions, sans doute parce qu'elles n'auraient aidé en rien le congrès d'orientation, ou peut-être l'orientation qu'on voulait lui donner. . . . Alors pourquoi se surprendre d'y trouver la légitimation des actions prises? S'attendrait-on à voir un fonctionnaire s'attaquer aux politiques de son ministère? Aussi il me semble qu'il faudrait considérer ce texte comme une source essentielle à la compréhension de la Centrale, et non comme une analyse des pratiques et des positions de celle-ci.

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Elliot Feldman and Neil Nevitte, eds. *The Future of North America* (Center for International Affairs, Harvard University, Cambridge, Mass, and the Institute for Research on Public Policy, Montreal, Quebec, 1979).

THIS COLLECTION of 14 articles and comments is the work of nearly 30 contributors on the question of the nation status of Quebec and Canada and their relationship to the United States. Themes covered in the book touch on Canadian and Quebec nationalism, federal-provincial relations, Canadian regionalism, Quebec separation and confederation. United States-Canadian tensions in the fields of international affairs and communications are also discussed. The topics of energy, resources, communications, bureaucracy, and foreign investment are considered. Not all the papers in this book were written by academics. Many are the work of politicians who have held, or presently hold, important posts in the government of Quebec or Canada. Contributors include Mordecai Richler, René Lévesque, Walter Gordon, Frank W. Peers, Allan E. Gotlieb, Richard Simeon, Ian McDougall, Gordon W. Stead, Peter Gourevitch, Gordon F. Gibson, Robert Bourassa, Flora MacDonald, and John Roberts. Most of the comments on the papers were by Americans.

The title of the book is deceptive. Few of the articles address the future of North America directly. The book is rather a comprehensive, careful statement about the controversies which punctuated the 1970s and promised to dominate the 1980s. Inevitably historical events have assured that the *Future of North America* was out of date before it went to the publisher. Some articles are more affected by the course of history than others. For example, those proposing specific constitutional modifications are of little interest in the wake of Trudeau's constitutional repatriation. Robert Bourassa (298) deserves the prize as seer for having predicted in 1978, when this book went to press, not only the outcome of the Quebec referendum, but the results of the subsequent provincial and federal elections!

The *Future of North America* is carefully put together, well-written, and broad-ranging. Some articles can even be characterized as popular and entertaining (Mordicai Richler's piece). Elliot

Feldman's introductory essay is an efficient summary of the articles in the book collapsed into a brief 20 pages. The hurried reader might not wish to read further. The concluding piece by Nevitte on "Nationalism, States and Nations" is equally important. It rigorously relates the other articles to his own reflections on the question of nationalism.

Another original point of the book concerns its organization. We have here not only the articles, most of which were written as papers for presentation at Harvard University's 1977-8 Seminar on Canadian-United States Relations (Center for International Affairs) but the comments (some too brief) of those individuals invited to criticize papers at the various seminars. A post-conference exchange of views between paper givers and "expert commentators" allowed the various perspectives to crystalize. Therefore opposing points of view are clearly and coherently set forth.

The editors and the majority of the article do not advocate the point of view of the present Canadian Federal Government, but their criticisms are muted and polite. There is only one socialist contributor to the volume, Micahel Parenti. His remarks are limited to reminding Robert Bourassa of the potential evils of encouraging foreign investment in Quebec. One must look elsewhere for a comprehensive left analysis of these questions. Nevertheless the book clearly shows that there was no monolithic consensus on these issues within the Canadian or American establishment mainstream during the pre-constitutional repatriation period. The Quebec point of view is well presented in the collection. Its importance in North America is not underestimated.

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S.L. Sutherland, *Patterns of Belief and Action: Measurement of Student Political Activism* (Toronto: University of Toronto Press 1981).

CAN A SURVEY OF STUDENT opinion, begun in 1968 and published 13 years later, find security and relevance in the contemporary world? A generous and optimistic publisher evidently thought so. A bleary eyed and critical reviewer has some doubts.

This study of 1200 University of Alberta students (959 of whom completed questionnaires) was one of many similar projects (mostly American) undertaken at the height of the student movement. Academics who were frightened, or opportunistic, or (more rarely) committed to the movement itself, believed that if activism were to continue, society should know what made the radicals tick. Personally, I always found such studies offensive since they appeared to treat radicals like diseased freaks who could only be understood if examined under a microscope. Ironically, at the very time many of these projects were completed, student activism was fizzling out, rendering irrelevant the apocalyptic scenarios of both those that feared and favoured the movement's success. In the meantime the psychologists and sociologists discovered that student activists generally came from liberal or permissive families in which the parents were well educated and where traditional religion was relatively unimportant. Radical students were found also to be high achievers and more likely than not to be studying in the arts.

Much of the content of the present study can be summarized by saying that it reinforces — a decade later — the above findings. University of Alberta student radicals were typical of their counterparts elsewhere. The author also discusses whether activist students had happy childhoods and witnessed "warm" vs "combative" parental relationships. The causal significance of these observations is entirely lost on me.

Since the author is concerned with the whole phenomenon of activism, she examines "conservative" activists as well. These students came from father-

dominated, church-going, and charity-giving families. But since this group constituted only 1.8 per cent of the total surveyed (or 17 people), it is unclear how extensively one can generalize from this tiny collection. The 78 per cent of responding students who did not fit the radical or conservative categories as defined by the author were not analyzed further.

The difficulty with most surveys of student activism, including this one, is that they proved almost useless in forecasting or explaining student behaviour in the 1970s. Presumably, the social, environmental, and family traits of the North American student population did not change in the last decade, but the student movement faded away. This suggests that the movement was far less a sociological or psychological phenomenon than researchers contended. Discovering the common elements of the activists' backgrounds is not the same as saying that these elements caused the student movement. If they did, then why did their influence not persist? This book, which had the benefit of perspective, does not address the broader currents of political, economic, and cultural change which enveloped the sociological realities. For that reason the survey and findings seem even more extraneous for being published so long after the events they describe.

The author must have known that her mostly unoriginal findings could not have justified the publication of this book. At best an article would have sufficed. The alleged significance of the study lies in its methodology: its analysis of the relationship between attitudes and activities through the discipline of social psychology. Here, some pertinent and sensible observations are made.

Previous studies on the question of what inclines people to "be mobilized to a cause" either assumed that behaviour could be predicted from an individual's attitudes, or that behaviour should be examined to the exclusion of attitudes. Sutherland contends that "views and deeds create one another reciprocally" and that *both* constitute an individual's ideology, though ideas formed in the context of action are more significant and "truer" than those formed in the absence of action. This perspective might be related to the potential tension between the social historian who is concerned with what people do and the intellectual historian who focusses on what people think. A full picture of the past demands combining both of these dimensions and carefully evaluating the relative significance of each.

Of course the testing ground for the author's thesis was the phenomenon of student activism, and it is unfortunate that so little new was learned. She concludes: "It seems, in short, to be true that those students with some background benefits [family socialization] are likely to become active in politics. It seems also to be true that identification of an action component in the subject's past allows the researcher to predict coherent and thorough going sets of viewpoints which are compatible with the 'seed' views. In short it is plausible to argue that activity in concert with others to influence the conduct of public affairs leads to improvement in coherence of the belief system." Translation: If one comes from an activist background, and one's university experience reinforces this conditioning, one is likely both to remain politically active and sustain coherent political beliefs. I knew that.

One final complaint: while the author at times shows some literary flair (of which the above passage is not an example), the study is full of dense, disciplinary jargon. The reader comes away "formatted," "operationalized," "randomized," "trichomotized," "incremented," and finally "disvalued."

All of this adds up to a less than gripping intellectual experience. But why this book took more than a decade finding its

way to print — now that would be an interesting story.

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Allan Moscovitch and Glenn Drover, eds., *Inequality: Essays on the Political Economy of Social Welfare* (Toronto: University of Toronto Press 1981).

IT'S A SORRY TALE that this book has to tell. It's also a familiar one, both in the inequalities of income, services, and rights illustrated from Canadian data, and in (what the editors aim to stress) the causes to which the inequalities are ascribed. The contributors, manifesting vigorous Marxist convictions current in Canadian schools of social work, find these causes in the economic processes of capitalism. These processes create, or at least perpetuate, a class structure; and those who have a privileged position in the class structure will, to keep the structure and the processes going, undo any serious efforts to diminish its inequalities. Survey articles by Drover and Moscovitch, by Drover, by Moscovitch, and by Bert Young, in which the government-subsidized circulars of the National Council of Welfare are drawn upon as assiduously for detailed facts as Marx drew upon the reports of Her Majesty's inspectors of factories, organize plausible support for the postulates about causation, without penetrating very deep into the operation of the causes themselves.

Has the book anything new to say? The tendency, especially manifest in the epilogue by Stanley Ryerson, to expand the subject so that no feature of capitalism is left uncondemned, is not new. The addition to basic Marxist theory of the Trotskyist-Leninist theory of imperialism is not new either, though, rendered as a theory of the development of a central region at the expense of the hinterland, it is tolerably well applied by Nils Kuusisto and Rick Williams to the Atlantic Provinces, and intelligently deployed in its classical global setting by Philip Ehrensaft and Warwick Armstrong to describe the rise and fall of "dominion capitalism." The latter article has something new and arresting to suggest: it argues that Argentina and Uruguay were in substance as much dominions of the British Empire as Canada, Australia, and New Zealand; and implies that the horrible fate overtaking Argentina and Uruguay when their comfortable roles of suppliers to Britain dwindled away may presage the fate of the English-speaking dominions. Canadians may take some comfort from the disanalogies also noted, among them the absence in Canada of an overweening class of large "pastoral capitalists" entrenched in the armed forces.

An article by Dorothy Smith, "Women's Inequality and the Family," advances furthest in theoretical innovation, and has some penetrating things to say about how middle-class wives, displaced from former roles in family-businesses, now concentrate upon the task of preparing their children to seize through education the advantages that await them in perpetuating the class-structure. Smith aims at saying equally penetrating things about the changes that the rise to dominance of large corporate enterprises has imposed on farm-wives and working-class wives, too. Unfortunately, this commendable program founders in utter confusion. She is inclined to picture wives of all sorts as having fallen, from often being in earlier generations at least *de facto* equal partners with their husbands, into complete subordination in the home, in spite of broad new opportunities for work outside. "Now an individual man appropriates as his the work done by his wife or other women of his family." (169) Setting aside the pop feminist cant of that statement, which is not made more scientific by being expressed in Marxist phrasing, one might ask, were not women always subject to the possibility of exploitation,

escaping it in some numbers to some degrees according to variations in their relations with their husbands? How have the variations changed? Are the proportions of exploitation greater or less? Smith has nothing like statistical evidence to answer these questions. Her examples from earlier generations of farm life point both ways; and the recent Murdoch case, which she cites just to make a point of Mrs. Murdoch's substantial partnership in operations, belies her claim that such partnerships are a thing of the past.

Is it cant, or just lack of theoretical energy, that leads all too many of the contributors to treat the liberal state as if it could do nothing right, or even effective? Here, failing relative tests of achievement (for example, raising the level of investment in Atlantic Canada to the Canadian average), it gets no credit for successes on absolute tests (keeping regional disparities "manageable"). A number of the contributors — most notably Howard Buchbinder, in an article on the social services — leave undistinguished what the politicians of the liberal state have consciously aimed at from what functions their policies, successful or unsuccessful with respect to those aims, may serve in perpetuating the system.

Unless this distinction is carried through, however, and unless the politicians are granted ultimate aims that considerably transcend the narrow liberalism that Patrick Kerans attacks in his contribution, the past behaviour of politicians in Canada and elsewhere in "legitimizing" the system is unintelligible. What does legitimation amount to, if it does not involve meeting in part needs that the market is perceived not to meet? Moreover, the paradox of the present crisis, in which the federal government appears determined to abandon successful measures of legitimation to an extent that will jeopardize the political stability of the system, will not be appreciated. Nor will the challenge that the paradox poses to Marxist theory. It might be unreasonable to ask that the authors of the present collection take up the challenge; they have on the whole aimed simply at putting Canadian inequalities in the perspective of Marxist theory as received. One might have asked, notwithstanding, that they not have done something — a little — to block advances in theory with the confusions noted.

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Keith G. Banting, *The Welfare State and Canadian Federalism* (Montreal: McGill-Queen's University Press 1982).

IF, IN REVIEWING THIS BOOK, one were to follow current fashion and to analyze the "text" for its "silences," one would be advised to wear protective ear plugs to avoid being deafened by them. This problem however, exists not primarily in the book itself, but is a direct expression of the theoretical poverty of mainstream political science. The deafening silence is that of one hand clapping — of the attempt to describe "objectively" and to understand political institutions and practices independent of serious analysis of the socio-economic and ideological context.

Having said that, I am quick to add that *The Welfare State and Canadian Federalism* is a very competent and articulate work of descriptive analysis which will be useful to those of us teaching courses on the Canadian State and the political economy of social welfare. It provides excellent statistical and historical information on the creation, structure, and operation of the income security system. While it focuses on only this one major element of the welfare state, and leaves health, education, and other social services largely to one side, the treatment of its chosen subject matter is comprehensive, scholarly, and often interesting. Unfortunately, its attempt to theorize the welfare state in Canada is undermined by

its narrowness of focus and its subservience to the conceptual discipline of orthodox political science.

The problem begins in the construction of the question which Banting is to address in the book:

> To what extent is the role of the Canadian State moulded by the structure through which it operates? To what extent are the decisions taken each day by political leaders shaped by the institutions within which they work: our federal system, our particular version of parliamentary government, our electoral system? (1)

And further:

> *The question addressed here . . . is:* To what extent have Canadian efforts in this major dimension of public policy have been shaped by *the institution within which we conduct our* collective political life? (3)

Presumably, this issue is a hot item in *the halls of poli-sci departments, but to* the outsider it may evoke a lack of comprehension as to what is being addressed and why. *It all might depend, of course,* on how the author defines the state as distinct from the institutions or structures, or on what is meant by the "shaping" of state activities by the institutional forms. Surely the structure of the state itself is "shaped" by (or is at least determined "in the last instance" by) other factors and gives them institutional form, whether they be the "common will," the social contract, relations of class struggle and dominance, or structures of ideological hegemony. The actual institutions and practices of the state and of political elites would then seem to mediate between the context in which basic social goals and directions are determined, and the social and economic base in which they are realized.

But in harmony with the main themes of the orthodox political science discourse, the state itself, its institutional forms, and the constellations of political *agents are taken as given or as independent* variables. One of Banting's working assumptions is that "The Canadian structure of government ensures that the *interests of governments, as governments,* assume an unusual importance in decision making." (43)

On the empirical level we know what he is getting at here, and it has a journalistic validity that we can easily accept in day-to-day terms. But as theory or (heaven forbid) "science," such an assertion simply silences most of the really interesting and productive debates. What determines the structure (and, therefore, practices and interests) of the state? Why, the structure (and, therefore, practices and interests) of the state, of course.

Such bracketing of essential questions is accompanied by the basic euphemisms and ideological categories of the discourse. *The welfare state is the natural* outgrowth of a "modern industrial society." More financial assistance to an ever-*larger population of dependent and socio*-economically marginal people is "progressive," an expansion of "social rights" and of "social justice." Opposition to expanded welfare programs is "conservative." The transfer of economic surpluses from "have" to "have not" regions via income security programs is a form of "redistribution of wealth" which offsets "regional disparities." Policy in the social welfare field is made by political power figures and their key advisors subject to pressures from the plurality of competing "elite groups" (business, labour, the professions, and special interest groups.)

There are few if any, references in this discussion to the concrete experience of the social problems that income security programs are meant to solve or control. This is the dialogue between the ivory tower and the Peace Tower about what "we" are to do with all the ants down there. There is no discussion of why *expanding regional inequalities, and* unemployment are so basic to our "modern industrial society" in general, and to *the Canadian political economy in particular.* There is no suggestion that income security programs cannot grow indefinitely in line with demand, or that there

is any contradiction between welfare state expansion and private accumulation itself. (Some problems are anticipated with regard to pensions, but these are not considered to be fundamental.) The basic stability of economic structures and of social relations over the long term is assumed. The rich and diversified Marxist literature on the state that addressed many of those issues effectively is dismissed in a few sentences as "deterministic."

The problem with this book, then, is that, given its starting point and analytical perspective, it can only conclude with the observations with which it should have started. Do we, in 1982, need another book which discovers that:

> [Institutional patterns] are not the only or even the most important determinant of policy. The nature of the Canadian economy, the nation's social and demographic profile, its cultural and political configuration, all leave their imprint on the Canadian welfare state. . . . The social role of the Canadian state does not depend solely, or even centrally, on the particular structures through which it operates. (179)

While here a break with orthodoxy appears possible such that economic structures, ideology, and class, gender and ethnic/racial relations might become important elements in the theorization of the Canadian state, Banting in the end, remains true to his discipline:

> To some extent, political science does suffer from what has been labelled "the curse of expertise in independent variables." The corrective for such a disciplinary "mobilization of bias" lies in comparative studies of public policy. Focusing firmly on public policy as the phenomenon to be explained, treating it explicitly as the product of a variety of factors, and examining it in a comparative context — both across time and across political units -- can help to place the role of political institutions in perspective.

This position may indeed be considered heretical within the priesthood, but to a naive outsider it seems cautious to say the least. If *The Welfare State and Canadian Federalism* is an example of this new departure, then those in power in both government and academia can rest assured that the lid is still firmly in place on their whole sordid mess.

Rick Williams
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David P. Ross, *The Working Poor, Wage Earners And The Failure Of Income Security Policies* (Toronto: James Lorimer & Co. 1981).

DAVID ROSS attempts, quite creditably, to shatter some of the widely held myths about poor people in Canada. One of the major objectives of his book is to show that most of the people defined as poor are in fact wage earners. They work hard to earn their poverty and represent some 60 per cent of the total poor population of Canada. Although their incomes fall below the poverty line, the wage earning poor do not qualify for support within the present income security system.

The first part of the book describes this problem. The second part reviews the development of direct income security programmes from Confederation through the 1970s. In the third part Dr. Ross presents his proposals in the context of the 1980s. He formulates a policy proposal which would, in his view, be a first major step towards insuring the inclusion of the wage earning (working) poor within the income security system. This is accomplished by posing the objective of an income supplementation policy: to enable all household units to have incomes no lower than the recognized poverty levels.

In order to achieve this objective Ross proposes what he envisions as a "first step" in a comprehensive guaranteed income security scheme. This would be a revision and extension of the current child tax credit arrangement. This is a federal government programme involving refundable credits against federal taxes payable. Under the present (1978) scheme there is a credit of $200 for every child under eighteen. This payment is reduced by five

cents for each dollar of other family income beyond $18,000. "This meant that a family with one child and an income level above $22,000 would cease to receive any benefit, and a family with three children and an income level above $30,000 would receive no benefit."

Ross proposes increasing present benefit levels which he sees as "too modest" and adjusting the frequency of payments to more than once a year. He argues that his proposal can be financed by reducing the tax credit recovery threshold and increasing funding. Increased funding could be achieved either by putting more money into the programme or reallocating a portion of social programme benefits that presently go to high income families. As well, the Family Allowance Programme could be terminated or the tax schedule could be revised to recover more money from upper income families. In addition, Unemployment Insurance could have a time limit "beyond which the unemployed are encouraged to transfer to a combined tax credit/training/job creation scheme or, if this fails, social assistance." Ross would also lower the minimum wage thereby increasing low wage employment. These wages could then be supplemented via the tax credit scheme. Ross figures that such an "expanded tax credit approach" can be financed without new funds, given these adjustments to other programmes and combined with reducing the recovery threshold. The tax credit proposal would, according to Ross, establish an "incremental base" for ultimately achieving a comprehensive guaranteed income programme which would provide an answer to the present plight of the working poor.

There are significant contradictions which emerge from a consideration of Ross' proposals. Low minimum wage levels tend to reinforce the low wage labour market within the competitive sector. Reducing or maintaining existing minimum wage levels would subsidize the proliferation of low wage employment via the income supplementation scheme and, ultimately, via the tax system. Contraction of Unemployment Insurance coverage and its integration into the refundable tax credit system would further extend the notion that the state bears the responsibility for unemployment. It would also support the argument that lengthy Unemployment Insurance benefit periods provide a disincentive to work (a position which Ross appears to share). Thus, the impact of this more "enlightened" social policy approach which purports to lay the basis for some sort of guaranteed income system, is to reinforce state support of those sectors of the labour market which now produce the working poor, the very group whose needs David Ross wishes to address.

This contradiction is not surprising if one considers the role of the capitalist state in supporting the accumulation process as against the needs of the working poor who are victimized by that process. Ross does not address this contradiction.

The reader is left with the assumption that the right income supplementation programme will satisfy the needs of the working poor. Ross locates the arena for change within the welfare system, not within the competitive sector of the labour market. When wages are insufficient to provide for the reproduction of labour, reform pressures turn to the state to make up the difference. However, the present economic crisis diminishes the possibility for welfare reform as the material base for such reform erodes. As Ian Gough has argued: "If capitalism more and more engenders a welfare state, it is also proving difficult for capitalism to cope with the problems of financing the requisite expenditures."

The ideological nature of Ross' treatment of the problem limits and directs his view and the resultant proposals. The issues of social welfare policy need to be seen within the context of Canadian capitalism. Otherwise we will continue to

debate programmatic solutions instead of coming to the heart of the matter.

H. Buchbinder
York University

Roy T. Bowles, *Social Impact Assessment in Small Communities* (Scarborough: Butterworths 1981).

THE LAST TEN OR FIFTEEN years have seen the flowering of a new management science, that of impact assessment. Environmental, economic, and social impact assessment are now required for most major development projects which take place in the advanced industrial nations, especially those which take place on government land or which involve public financing. Impact assessment has quickly generated its own body of academic theorists and practitioners (in some cases it seems almost to have taken over universities), it has helped make "consultation" a growth industry and it even has its occasional critic. Our lives are increasingly affected by it, albeit in ways which are not all that obvious. Until fairly recently environmental and economic impact assessment received most attention as did the technique of cost-benefit analysis which was commonly used in such assessments. In the last few years, however, greater attention has been given to what is called social impact assessment (SIA) particularly in connection with major energy development projects. Given this, it is important to have an understanding of what social impact assessment is, why it is being used, and what its drawbacks and limitations are. Thus a book which aims to provide an "integrative review" of literature dealing with SIA might be timely and welcome. And a book focusing especially on "small communities" could prove valuable to those concerned with the problems generated by large-scale industrial developments in rural areas, the Canadian North, and other "hinterland" regions.

Unfortunately Roy T. Bowles' book proves to be of very little value when it comes to providing answers to the questions about SIA outlined above. The book has many drawbacks. It is badly written, poorly integrated, and in places incoherent. Many of the key terms and concepts lack adequate definition, a great many important issues are simply not dealt with in anything but the most superficial manner (public participation in planning, for example) and significant bodies of literature are totally ignored even though they are essential to any understanding of SIA. Bowles creates an elaborate jargonistic facade, presumably to give SIA the kind of scientific authenticity which will make it attractive to those who are impressed by such things, that makes the book difficult to read. This is a problem, but a far greater one is that behind the facade created by Bowles there appears to be very little of substance being stated. In spite of this I think it is important to make some critical comments about what is said in the book for two reasons: 1) regardless of its limitations the book is likely to be used in teaching and by those involved in SIA because it is geared towards a specific set of needs and because there is a lack of adequate alternative material, and 2) there are conclusions drawn and prescriptions made by Bowles which I think are very dangerous from the point of view of organized labour. These prescriptions may be all the more dangerous for being made as part of a scheme for apparently humanizing capitalism and making it more responsive to the needs of small communities and what are called traditional societies.

In his book Bowles is concerned with "the processes by which large externally-controlled resource industries affect social life and social well-being in small communities in Canada's northern hinterland;" he sees small communities as those where "daily life is not in direct contact with industrial and urban patterns." Arguing that too often development projects are simply assumed to provide benefits

(mainly economic) to such communities and that often communities may "gain little and lose much" from development, he makes a call for more systematic and far reaching investigation of the impacts of such projects on community life by means of social impact assessment.

Early in his book Bowles makes clear his commitment to "small communities," "traditional lifestyles," "local economies," and "hinterland areas." He is one of an increasingly influential body of academics who have taken this stand in opposition to the orthodox modernization theory of the post-War period. Both modernization theorists and those like Bowles work with a dualist model of society and the economy. The *traditional* sector of society is distinct and separate from the *modern* sector. The former is characterized by small scale production using simple technology, the latter by capital intensive production on a large scale. The ideal type of traditional society is the little community, distinct, homogeneous, self-contained, and self-sufficient. Non-market values, mutual aid, and community spirit are supposedly important in traditional societies, while values and behaviour generally are distinctly different from those claimed as typical of urban industrial society. It is this idealized "defined community with more-or-less stable patterns of social behaviour, social relationships, and way of life" which Bowles *assumes* in his work and takes as a model of the socially vital and economically viable community. This is important for it is such vitality and viability which, according to Bowles, affects a community's capacity to mediate and control the impacts of large scale development.

Within the framework of modernization theory and practice, the existence of traditional society was regarded as one of the barriers to development and to the improvement of living standards which supposedly went hand in hand with industrialization. A great deal of attention was focused by social scientists on removing obstacles to modernization, in particular, to changing traditional values. By the 1960s, however, an increasing body of academics has become critical of modernization theory and practice. Detailed and sensitive accounts of the impact of such schemes on people in many areas were written which pointed out that the schemes were less beneficial than anticipated. In this context it was often argued that the cost of modernization outweighed the benefits and that such a path to development was unwise. Thus the academic orthodoxy of the time was challenged and a need for judgement, commitment and even action on the part of those academics concerned with "traditional cultures" and the "rural way of life" was recognized. A kind of populist social science emerged, somewhat ruralist and romantic in orientation but committed to protecting and advancing the interests of the people. Like these academics Bowels lays his values on the line when he states that "community vitality is an important renewable resource whose development can contribute substantially to the quality of life." Like them he is anxious to find means to save small communities. And like the advocates of "alternative" and "small is beautiful" models of development he too looks for a middle road to development (between "tradition" and "modernity"). Recognizing and accepting the inevitability of large scale development schemes (he does not question their rationale), he is concerned with trying to lessen their harmful effects by adapting them to local circumstances. It is in this context that he advocates the use of social impact assessment. Not only can this technique provide a framework for predicting the effects of development schemes, he argues, but it can also make project planning more disciplined as well as give it a "humanistic orientation." If, for example, community residents were given an "informed and authentic role in planning" within the framework of SIA, then this will allow them to "protect that

which is valuable about their way of life" and at the same time "give them access to benefits" from the projects. Here we have the radical appeal of SIA for people such as Bowles. But what exactly is social impact assessment? Unfortunately Bowles does not provide us with a very clear answer to this question. Some of his comments are simply inane, as when he informs us that SIA "is an application of social science methodology to assist in social planning," as if the social sciences were a unified body of thought with one methodology. Other comments suggest important lines of argument but these are never taken up and finally all the reader is left with is a vague impression of what SIA is *in theory*. And even the chapter devoted to "case studies" does not really clarify the situation.

To understand SIA we need to place its development in historical context. It needs to be appreciated as one technique of state management among a number of other techniques. Its use needs to be appreciated in terms of the limitations of economic impact analysis and cost benefit analysis. Cost benefit analysis became a popular tool with which planners could evaluate the desirability of development projects in the post World War II period. It offered a way of evaluating the projects in terms other than those of simple commercial profitability. By including in the evaluation of projects costs and benefits (jobs or pollution, for example) normally excluded from consideration by commercial interests, some way of deciding on the desirability of the project for society as a whole could, in theory, be arrived at. The technique could thus be used to help planners decide which development projects to undertake. Since its first use, cost benefit analysis has come under some criticism. The best of this criticism questions its assumptions and how it is used in practice. It bases the specific critique of cost benefit analysis on a wider critique of welfare economics from which CBA is derived. Much of the criticism of CBA, however, accepted as correct the basic idea that the "costs" and "benefits" of a particular scheme could be counted in monetary terms and weighed against each other to provide a rational and objective way of making what might otherwise be politically awkward and difficult decisions. This criticism was more concerned with the techniques by which specific monetary values were assigned to what have become known as "intangibles" and with the problems of measuring these intangibles generally. What dollar value do you assume for human life in the study of airport location? How do you assign a value to environmental destruction? As a result of political struggles over development projects the intangibles in cost benefit studies began to assume a greater importance. It was even recognized that some of them could not be assigned monetary values and they were often lumped together under the phrase "quality of life." For the critics of modernization theory CBA was a two-edged sword. It could be (and very occasionally was) used to place limits on the actions of private or public corporations in the interests of "the nation." But, as Bowles points out, "economic cost-benefit analysis, when applied to rural communities, misses much of the important data." As such it is a technique which reflects planning's urban industrial bias. The situation can be corrected, however, by including previously excluded data in CBA and by the use of a technique such as social impact assessment. SIA can be used to draw attention to some of the wider social issues involved in development and it can provide a way of getting concerns about "quality of life" considered in project planning. Of course it assumes some form of public participation in the planning process by which people can articulate their concerns. While in theory development projects could be stopped if the social costs of the project were shown to be too high, in practice the use of SIA at best seems to mean minor adjustments in their organization.

Bowles is clearly promoting SIA as a way of "humanizing" planning and replacing "judgement" with "analysis." The neutral state armed with this technique will thus be able to stand above any particular set of interests and assume "social responsibility." In this way the politics of development will vanish or at least will take its proper place within a carefully constructed scientific exercise of *project planning.* A cynic might argue that what we have with SIA is simply another form of social engineering, a means by which the implementation of development projects can be prevented from disruption and people pacified through participation. This image of SIA is reinforced by language used by Bowles. In spite of the claims for humanism, real people do not appear in this study at all. And people's lives and struggles are discussed (where they are discussed at all) in the ethereal language of "localized social units," "life circumstances," "reality situations," "constructive life contexts," "exogenous variables," and "impacted units." This pseudo-scientific language may help create the impression of objectivity and rationality in decision-making but in reality it is the language of control and manipulation.

In fact, the whole of Bowles's book has an intangible and ethereal quality to it. From the unrealistic assumptions about communities and their idealized nature to the final proposals for modifying the operations of corporations involved in development projects there is an all-pervasive air of unreality about the book. If only there was a detailed discussion of the forms of citizen participation in SIA. If only there was a discussion of the problems associated with such participation where it has been structured by the state. If only there was a critical theory of the state introduced. If only a real community with all its conflicts, divisions, and problems could be identified. If only there was some theoretical or empirical discussion of subsistence production. If only there was some broader historical appreciation of the variety of forms which capitalist development has taken. The list could go on for several pages but the point has, I think, been made. Bowles works in a world of abstractions.

Given that the social impacts of a particular development project can be forecast and given that the needs of all the people in the community can be articulated through some kind of participation in the planning of the project (and that the needs are not contradictory and conflicting), Bowles' argument still rests on the unproven assumption that it will be possible to change the *modus operandi* of those responsible for the projects. He argues that such projects need not be disruptive of established patterns of life in small communities if "governments and corporations are willing to modify *their traditional modes* of operation to be compatible with local desires and objectives" (I would add not only *willing* but also *able*). In particular, Bowles suggests that "traditional local economic activities," that is, subsistence production and commodity production, can be integrated with wage labour if "employers and planners relax *their own traditional* conceptions of the schedule and rhythm of wage labour." But more than this he suggests a "fundamental rethinking of and reorganization of the traditions, institutional structures, and priorities which guide the execution of resource projects." This will, of course, mean changing the "logic" of such projects including the commitment to "quick returns on capital, rigid time frames for project completion, and centralized criteria for judging efficiency." It also might mean the relaxation of "management-union agreeements and hiring exclusively through central union halls." In a more recent contribution by Bowles with the title "Preserving the contribution of traditional local economies" (1980), he outlines in more detail his plan for replacing full-time regular work with part-time and temporary wage labour so

that projects can fit in with traditional economies. But he also makes it clear that such a proposal requires that "the traditional way of calculating profit for an extractive industry" be changed. Then "if the accounting system is 'modernized' to take into account the cost of lost production in the local economy it may be more profitable to close down the operation for a short period to permit workers, for example, to participate in the harvest of fish during a seasonal run." Now much of this can be dismissed as wishful thinking. It would be nice if capital was not simply concerned with profit rates. But there may just be enough sense in his arguments to attract support in some quarters. In fact, what needs to be pointed out is that Bowles' notion of capitalism and of corporations is just as much an ahistorical abstraction as is his ideal typical small community with its traditional economy. It might surprise Bowles to find out that the new flexible capitalism which he is suggesting is not so new. In fact, historically and world wide there has been considerable variation in the specific nature of the wage labour-capital relationship, often exactly along the lines that Bowles is suggesting. In Newfoundland at the turn of the century both the mining and pulp and paper industries were integrated with the seasonal fisheries. From the point of view of capital the advantages of this kind of setup were many. In some cases specific operations could only be carried on at certain times of the year. A full-time workforce did not have to be maintained and so labour costs could be kept down. This setup meant also that unionization was difficult and this further led to wages being depressed. Nor have these kinds of arrangements ceased to exist. A part of the fishing industry is still reliant on seasonal wage labour and can only continue to operate because the workforce can support itself by other means during the off-season.

The labour reserve system in South Africa is another example of capital using an explicit strategy of temporary employment for workers in order to cut labour costs and undermine labour organization to press for improved conditions. Workers leave their families on the reserves to support themselves, while they temporarily work in the mines for low wages. In such a case a degree of subsistence production or the production of commodities for local or non-local sale allows companies to pay wages which do not cover the full costs of the worker maintaining his labour power. Similarly, in parts of the United States (in particular Appalachia) coal mining companies developed a clear policy of encouraging subsistence production so they could keep wages down in the 1930s. Similar comments could also be made concerning the importance of production for use (subsistence) amongst workers in many other locations. In Britain, for example, subsistence production has been and continues to be important for urban workers, as does the "occupational pluralism" which is supposedly so characteristic of rural hinterland areas. The dual nature of subsistence production should be clearly recognized. The cultivation of allotments has been an important element in workers' survival in uncertain economic circumstances. It has been encouraged by the state as a means of getting people to grow their own poor rate. But it has also been a means by which wages could be kept down.

What this brief discourse does is place the discussion of the specifics of Bowles' "traditional society" (in this case subsistence production) on terrain which is very different from that provided in his book. From this it is clear that "subsistence" cannot be treated in an ahistorical fashion as something outside capitalism and the ongoing struggle between capital and labour. Nor can it be considered as something which is simply good in and of itself. A similar analysis applied to Bowles' suggestions for modifying the *modus operandi* of large scale capitalism might lead us to the conclusion that his

strategy is one which would clearly lead to the undermining of the power of organized labour in its dealings with capital. It is no accident that with the current economic crisis there has been a resurgence of what is called the "traditional" sector in many developed nations, in particular Italy. This is part of efforts by capital to undermine the power of organized workers and so cut labour costs as well as aid in the restructuring of their operations. Sub-contracting to small firms with unorganized workers, the use of part-time labour, and the removal of parts of their operations outside the factory altogether — there has been a major expansion of homework — as well as the flight of capital to parts of the world where labour lacks power, these are all evidence of capital's attempts to resolve its crisis of profitability at the expense of working people. In this context we should seriously question the kinds of suggestions made by Bowles for preserving "traditional economies."

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John Swaigen, *How to Fight for What's Right: The Citizen's Guide to Public Interest Law* (Toronto: James Lorimer 1981).

In a contest for a human order, laws must be changed and disputed of course. Particular laws may and will be broken, as a matter of conscience, as has been done in the past. There must be inflexible opposition to attempts to trench upon basic constitutional rights; and if they do trench, then we must disobey. And equally, if in such a contest the democracy were to be victorious, the victors would honour these rights and rules in their turn. (E.P. Thompson, 1979)

IT IS ABOUT SUCH contests that John Swaigen's *How to Fight for What's Right: The Citizen's Guide to Public Interest Law* is directed. The book is very much about "rights and rules;" how far the former extend and how we should best employ the latter during contests which challenge them.

Swaigen describes the book's three goals as, "how to get into court to further your case; how to anticipate and deal with some of the problems that arise during public interest cases before courts and tribunals; and how to keep out of trouble and keep out of court." These goals are rarely if ever lost sight of, and continually reappear for discussion throughout the book.

Swaigen's focus is narrow but all the same intriguing. He admits that his purpose was to map out "legal implications and tactics" rather than to comment on the morality of our legal system. This is troubling at times. There are certain instances when this tactical theme could have been embellished with greater political insight into the nature and quality of justice dispensed in our legal system; especially given Swaigen's experience in the area of public interest law. This, however, is a small point. The book makes no pretence to be theoretical and the goals which are established are nicely achieved.

This is not a legal text, nor was it intended to be. It is useful and appealing to lawyers, but Swaigen is not writing here for fellow practitioners. Rather he advises on "what to do till the lawyer arrives." When should public interest groups consult a lawyer and what role should members play in the formation of legal strategy? Swaigen remains constantly alert to the dangers occasioned by the over-legalization of disputes, as well as the degree of manipulation that can arise with the involvement of lawyers. Similarly, he inveighs against public interest groups which divest themselves of control over their struggles by pursuing instead the solitary goal of judicial remedies.

In this respect, Swaigen's message is reminiscent of Staughton Lynd's, *Labor Law for the Rank and Filer* (Miles and Weir, Ltd., Singlejack Books, San Pedro, California, 1978). There Lynd advised:

. . . whenever a problem can be solved without

the help of a lawyer, do it. Besides being expensive, the law takes a long time. And it is written and administered by individuals who for the most part do not understand or sympathize with the experience of working people.

The tone of the books is similar. People need not be intimidated by either lawyers or legal problems. Frequently, people and groups have the skills and resources necessary to at least analyze their problem and devise a strategy for its solution. Swaigen, like Lynd, succeeds in sifting out the "legalese" and making a complex system more intelligible. Both would presumably agree that deciding to pursue legal remedies is a tactical decision in itself. Such a course should not be adopted voluntarily before one is clear as to what liabilities it may entail and what options it may preclude.

Swaigen's discussion of specific legal issues and the pitfalls surrounding them is concise and useful. His chapter on picketing and demonstrations, for example, not only capsulizes a very difficult area of law, but in addition offers a useful guideline for conduct, necessary precautions, and potential liability. Any labour lawyer who has had to counsel striking unionized workers on the do's and don'ts of picketing would be well advised to have a copy of the book handy.

Swaigen is at his best when tracing the links between seemingly obscure areas of public interest law and the ongoing struggle for social change. His final chapter deals with the frequent necessity for public interest groups to have "charitable status" under the provisions of the *Income Tax Act*. The designation exempts such groups from having their already meagre financial resources eroded even further by taxation. Which organizations qualify as "charitable" is far from clear. It certainly applies to non-profit groups which are primarily educational in nature. But what about groups which are directed to "political education and dialogue?" Lest anyone doubt the significance of this tangle of legal semantics, Swaigen indicates that the Manitoba Foundation for Canadian Studies has recently lost its designation of charitable status. This group publishes *Canadian Dimension*, and without the tax break which accrued from its former designation, an important forum for left thought may be lost. In this discussion, as elsewhere, Swaigen not only leads us into the morass of regulatory decision making but provides us with the basic understanding and encouragement to challenge such decisions.

How to Fight for What's Right is well researched and carefully written. The author has taken pains to include a list of supplementary sources for those with at least a modest legal training as well as a glossary for the uninitiated. The book is the second in a series published under the direction of the Canadian Environmental Law Research Foundation and their future efforts are eagerly awaited.

Keith Johnston
Regina

Ben Swankey, *Native Land Claims: For a Just Settlement* (Toronto: Progress Books 1980).

THE ACCEPTANCE of native claims by the federal government is an admission that it frequently mismanaged the Indian assets that it took over to protect and that native lands should not be usurped without adequate compensation. But, is it in the government's best interest to satisfy native claims? In 1975, the Commissioner of Indian Claims observed:

> While native people look to the Government as a trustee, there is no avoiding the fact that their claims are against this same trustee and often centre on the very nature of the Government's trust responsibility. Moreover, whatever Indians gain through negotiations other Canadians lose.

The government could continue to ignore, as it has for generations, the agitation of various native peoples and the evidence which legitimizes their grievances; however, big business and industry are also

pressuring the government to open lands still occupied by the original people to commercial exploitation. At least eight important comprehensive claims have been launched by the native people of these frontier areas. Whether the settlement of those claims based on aboriginal title, or of the 250 specific claims based on lawful obligations, will be perceived by the native people affected as just and therefore final depends on many factors.

In this pamphlet, Ben Swankey maintains that there cannot be a just settlement as long as the government exists to satisfy the interests of business and industry. By disregarding considerations of race and culture, Swankey manages to perceive the issues solely in terms of social class. The subject of native claims is used as a vehicle for expressing the Communist Party of Canada's views about capitalist society and for prescribing the means for bringing about social justice.

Negotiators for both native organizations and the government would likely agree with Swankey that the delays in the claims process are contrived to "whittle down" the demands of the claimants, and that cash or piecemeal settlements will not solve the social and economic problems of native people. That they would agree native people "have as big a stake in the struggle to defeat the rule of the big corporations as have the vast majority of the people in Canada" is, of course, less likely. Having established, without discussion, that "colonialists, corporations and governments" are the source of all racial discrimination and oppression, Swankey explains that justice and equality will be possible only when political power has been won by an alliance of people opposed to the big corporations — the corporations "who covet the land of the Native peoples and who benefit from discriminatory practices against them." In the meantime, the Communist Party advocates preferential treatment for Canada's native peoples and the return of sufficient amounts of suitable land to enable them to enjoy an economically viable existence in accordance with their traditions.

Sandwiched between proclamations of the Communist Party is a potentially useful itemization of the major claims and the groups prosecuting them. Though a number of years have passed since Swankey compiled this list, it still serves as an abbreviated guide to current claims as only a few relatively minor claims have been settled and the rest are still outstanding. Noticeably out of date, however, is the meagre coverage of the James Bay Agreement, the only comprehensive settlement to date. The *James Bay and Northern Quebec Implementation Review*, released by the Department of Indian and Northern Affairs in February 1982, made it clear that the 1975 James Bay Agreement is not a suitable "model" for resolving the other aboriginal claims as Swankey and others had hoped.

Why the recent historical background to native claims appears near the end of *Native Land Claims* is unclear but, given that it is a political pamphlet rather than a book, it is probably unfair to criticize it for organizational weakness or for the way in which the arguments are, or are not, developed. A fitting conclusion to the effort is the third appendix, a Pollyanna's account of the wonderful gains made by the indigenous peoples of the Soviet Union after 1917.

Although he calls for Canada's native people to join the working class in militant struggle, Swankey points out that Ottawa is cultivating a native middle class with other interests. In *National Identity or Cultural Genocide? A Reply to Ottawa's New Indian Policy*, a similar pamphlet written by Swankey in 1970, he asserted "there is a solid base for unity of white and Indian workers in defence of their needs as a class." Nothing perceptible has been built on that base over the last decade, and Swankey now seems less certain that labour and native people are natural allies.

Native peoples still perceive themselves as separate and distinct nations. Unemployment is the predominant feature of Indian life, and non-Indian workers have never been particularly welcoming to Indians either as neighbours or as co-workers. Racial prejudice, aggravated by ignorance about native people and their history, permeates every level of Canadian society; moreover, status Indians have been systematically prevented from interacting with the larger society by more than a century of separate legislation and social services.

Although Swankey fails to recognize the impediments to a unification of native and working-class interests, *Native Land Claims* fills a need in a world in which information about native peoples, their experiences, and their current struggles is grievously lacking. It introduces the subject of native claims and several important related issues. One only wishes that Swankey had been able to go beyond wishful thinking to suggest how to arrive at "a just settlement." While the redistribution of power to the working class may, in theory, be the best way to arrive at social justice, native people are obliged to press for recognition of their rights and claims within the context of the present social, political, and economy reality.

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R. Kenneth Carty and W. Peter Ward, eds., *Entering the Eighties: Canada in Crisis* (Toronto: Oxford University Press 1980).

THIS IS A COLLECTION of papers presented to a noon-hour lecture series, "Canada at Midlife Crisis," at the University of British Columbia early in 1980. The authors were assigned topics, and the editors appear to have discerned in the various responses answers to the questions which, they claim, form the essence of the Canadian experience: "Who are we? What are we doing together? How shall we go about our common business?" There is a certain amount of question begging here, of course — common business indeed — but what may be rather more offensive to some readers, as it was to me, is the double implication that somehow the seminar participants were being more essentially Canadian as they dipped into their brown baggies at the lunchtime lectures than they are at other times, and that we must all willy-nilly share in the midlife crisis which, it seems to be purported, gnaws at the vitals of the various distinguished academics who appear in these pages.

But editor-bashing is poor sport. If Canada's chief problem in entering the 1980s is seen to be a crisis of confidence and identity, of a lack of common ideals to whisk us romantically into a glowing future, that is hardly the fault of Messrs. Carty and Ward. Liberal idealism enters on the ground floor in most of these essays. It is complemented almost throughout by its equally liberal counterpart, a repudiation of the interventionist state. It may simply be a measure of how out of touch I am with the prevailing currents in contemporary Canadian thought, but I was astounded at the mid-Victorian aura that clings to much of the argument in this volume, albeit it is frequently expressed in a contemporary idiom.

Professor Mallory believes that changes in rules coupled with a renewed commitment by members of parliament to the historic functions of that institution will elevate the nation's politics. Professor Brown wishes to remind us of the importance of personality in national leadership. Professor Smith holds that the existing political forum will never get the constitution-making task done properly, and must be replaced by a special body. Character and process are at issue, not material reality. Professor Smiley urges that government stop meddling with issues of language and culture. Professor Nelles wishes to see the federal government abandon any pretensions to jurisdiction

over energy. Professor Bliss would like governments to abstain from all sorts of intervention in the economy, but recognizes that they won't. Two contributors speak directly about Quebec. Professor Cook reads the riddle of the "French speaking sphinx": what Quebec wants is *la survivance*. It has achieved it, thus far, by voting for Liberal federalism in national elections and against it in provincial ones. Professor Balthazar insists that the *québécois* constitute a distinct people, but a mosaic one that must learn to celebrate multiculturalism.

As befits such a roster of academic luminaries, the pieces are for the most part highly articulate and craftily constructed. I enjoyed most the ones with which I most heartily disagreed, those by Nelles, by Smiley, and, to a lesser degree (of enjoyment, not disagreement), by Bliss, for these authors are consummate rhetoricians. I could not help feeling, though, that these papers were all the products of a rather elaborate parlour game, perhaps an attempt to recreate by way of pastiche the lost proceedings of the Montreal Literary and Scientific Society's 1880 general meeting.

All this, I am sure, will be seen as quite unfair by the contributors to this volume and by some part at least of their readership. But I must confess to a great gulf of empathy between their apparent worldview and my own. Canada *has* entered the 1980s in crisis — we agree on this at least — but are its dimensions really to be described in terms of process and appearance, of character and values? Is salvation really to be sought in the refashioning of institutions, the embrace of unifying ideals, the abandonment of social life to a renewed possessive individualism? I would describe the crisis rather differently. Its crucial phenomena are economic stagnation and decline, the prospect of a new technological assault on employment, deteriorating living standards compounded by governments' retreat from social welfare objectives, and a new bout of interimperialist rivalry that may yet precipitate a holocaust. That, at any rate, is how it looks to me, and I simply don't know what Viv Nelles dressing up as James Mavor, or Don Smiley as Goldwin Smith, has to do with any of it.

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David Crane, ed., *Beyond the Monetarists: Post Keynesian Alternatives to Rampant Inflation, Low Growth and High Unemployment*, Canadian Institute for Economic Policy Series (Toronto: James Lorimer 1981).

NUMEROUS TIMES RECENTLY I have heard economists, watching the relentless slide of western economies under the alchemist-like ministrations of the monetarists, ask the very pertinent questions, "When will the monetarists finally admit failure?" Some, I expect, will never admit failure regardless of the outcome. Already a host of apologetics have come forward to explain the failures in individual countries: monetary control was not stringent enough; the political will not sufficient to cut back government expenditure enough; the central banks were controlling the wrong monetary aggregate; or whatever. I have no doubt that given time and a sufficient intensity of application, (perhaps even a return to the nineteenth-century gold standard) the monetarist prescription will eventually bring inflation down, but along with it will come the whole economy, a cure worse than the disease.

In the face of the sorry record of such policies, why is it that these economists continue to hold sway among governments and central bankers? Certainly there is no lack of critics, both within the academic community and among the general public, including even the business sector. At least part of the answer lies in the inability of the critics to achieve any degree of consensus, not so much on the causes of stagflation — indeed there is a

large degree of agreement on the origins among post-Keynesians — but rather on the solutions to the problems.

It was with this disarray in mind that the Canadian Institute for Economic Policy sponsored a conference in December 1980 to consider alternatives to monetarist policies. The present volume is a compilation of the conference proceedings.

The post-Keynesian consensus centres on the argument that stagflation — low growth, inflation, and rising unemployment — originates on the supply side of the market, in the "supply shocks" such as the OPEC initiated energy price increases, and in the market power of the aligopolistic sector and its organized workers to pass on costs and raise prices even in an economy characterized by insufficient demand. This is a problem Keynes never really addressed. His concern, understandable in the context of the inter-war period, was only with the demand side.

Crane suggests in his introduction that the consensus goes farther than the analysis of causes and includes general agreement on a policy response encompassing four elements: a sharing of the costs and benefits of remedial policies, incomes policies, an industrial strategy to resume growth, and a general commitment to an equitable distribution of output. (6-7) But if this collection is indicative of the state of post-Keynesian thinking, consensus ends with this general statement of goals and methods. Contention arises as soon as any attempt is made to give substance to the concepts of incomes policies and industrial strategies.

Perhaps most contentious in view of the subsequent debate was Professor Weintraub's proposal for a tax-based incomes policy, the purpose of which would be to control wages effectively by penalizing those corporations which did not hold down their employees' incomes. Aside from Professor Weldon's criticism that such a programme is "unworkable in practice, unfounded in economic theory, not available as a costless experiment — indeed it would be very expensive — and is socially damaging" (78), it is firmly imbedded in the unproven, and I would argue untrue, assumption that negotiated wage increases are the *cause* of inflation rather than the *result* of inflation. This suggests that, whatever the superficial consensus claimed by the post-Keynesians, there is still lacking a basic theoretical agreement on the causes and mechanisms of inflation.

Similarly on the question of an industrial strategy, proposals ranged from Professor Cripps' argument for British withdrawal from the Common Market, the imposition of import controls, and economic planning, to William Diebold's emphasis on tripartism in the United States and John Sheperd's advocacy of regional specialization in the context of repatriating the Canadian economy. What appears to be lacking in all three is any theoretical base that would give coherence to the numerous proposals. Indeed, in the following debate, one of the central points of the supposed consensus, the concern with equality, is deemed to be incompatible with an industrial strategy when Professor Crookell suggests that innovation requires a more unequal society, or as he puts it, "The 'Just Society' was not designed to be innovative." (126)

There is, however, a lot worthwhile in *Beyond the Monetarists*, particularly the criticism of the monetarist position from theoretical, empirical, and equity perspectives. I would particularly recommend the paper by Simon Fraser's John McCallum whose use of monetarist methods to demonstrate the complete failure of monetarist prediction makes one wonder how the Chicago School can possibly retain any credibility. The experience of Canada, the United States, and Britain under monetarist policies, also recounted in the volume, is depressing in more ways than one. But perhaps the last word is said by Galbraith in his opening paper. He is in biting good form in his counterattack to

the monetarist assault on the Keynesian consensus that, since World War II and until recently, has pervaded the western economies. Nevertheless, the point is that there *was* a Keynesian consensus. As this book demonstrates, there is not yet a post-Keynesian consensus which is why the monetarist position, which in its simplistic sweep threatens the very basis of the liberal-capitalist world, has been accepted. In this regard, the most striking image in the book is that raised by British speaker Wynne Godley in comparing monetarist solutions to Sir Walter Raleigh's final remark as he faced the executioner. After inspecting the axe and feeling its sharp edges, he noted: "This is sharp medicine, but it is a physician that will cure all my diseases." (41)

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J.M. Sosin, *English America and the Restoration Monarchy of Charles II. Transatlantic Politics, Commerce, and Kinship* (Lincoln: University of Nebraska Press 1980).

THIS, THE FIRST OF three projected volumes on the history of the English colonies in America under the later Stuarts, is a very ambitious undertaking. An older generation of "imperial" historians attempted synoptic accounts of colonial America but their viewpoint was almost exclusively that of Whitehall and the problems of imperial administration. General accounts are, of course, still written but these are usually at the introductory or textbook level. For almost two decades now original scholarship has concentrated on intellectual history and, even more so, on detailed studies of the social history of local communities, first in New England and more recently in the Chesapeake colonies. These studies have unravelled the matrix of daily life and labour and the patterns of thought and consciousness in early American society. What has not yet emerged clearly is the relationship of these findings to the larger political history of the American colonies. Sosin here offers a syncretic work which attempts to integrate local forces with imperial concerns. His central thesis is that commercial, familial, or religious connections across the Atlantic bound local and imperial concerns together and directed the course of events.

These connections and their impact on both sides of the Atlantic are examined in considerable and sometimes opaque detail for all the mainland colonies during the reign of Charles II. Here, as in his earlier works on the Revolutionary period, Sosin shows his mastery of archival sources in England and America. The reader is never very far from the primary material or the voices of the participants in the events being studied. Also, the author tries to keep as close as possible to the actual chronology. He generally deals with all the colonies simultaneously in a set of parallel chapters before advancing the story for any one region. While these methods have the advantage of showing the interplay between imperial concerns and colonial ambitions as they evolved, they do make for difficult reading. Sosin's analysis does not rely much on the statistical evidence which has formed the basis of the demographic studies of recent years, but these works have not yet fully explained the exercise of power and government for the colonies.

Throughout most of the late seventeenth century, Sosin argues, conflicting land claims, Indian threats, natural disasters, and a depressed economy produced an instability for the colonists which could only be countered by "personal connections or family and religious ties [which] formed the nexus of the English transatlantic trading world." The result was that the colonists were inevitably part of the imperial economy even without the coercion of the Navigation Act. The complaints of the colonists about the costs of English policy should not be taken too

seriously since intrinsic conditions determined the opportunities for prosperity. Within this framework shifting transatlantic political alliances rather than any firm imperial direction or execution of policy shaped colonial history. Indeed, despite the desires of professional colonial administrators and occasional shows of "rational, purposeful rule" there really was no overarching Caroline imperial policy except for the advancement of religious toleration — an extension of domestic policy — and the encouragement of representative assemblies in the colonies in order to make the collection of revenues easier. Even these goals were pursued with inconsistency and timidity throughout the period examined.

Sosin challenges the recent claim of Stephen Webb that during the Restoration period there was a royal policy in favour of an absolutist and militaristic garrison type of government for the colonies. The money and means were simply not available for such a policy. To pursue even limited goals the imperial authorities had to depend on unreliable favourites or on connections with competing alliances of friends, families, religious interests, or commercial partners on both sides of the Atlantic. Thus, for example, Sosin shows how pusillanimous royal policy was for New England, and especially for Massachusetts during Charles' reign. Money, rather than power, determined Whitehall's course. Even the modest aims of religious toleration and compliance with the trade laws could only be accomplished by working through factions in the colonies, factions which could exploit this dependency to their own ends.

Proprietors fared little better than the Crown in their colonies. Often they had little influence over their officials, and struggles among the settlers over access to land or liability to taxes produced intermittent chaos and sometimes armed unrest. The Crown's interest in these colonies was brought about by divisions and quarrels among the colonists rather than as a result of the forthright assertion of a coherent imperial policy.

Clearly, for Sosin, internal struggles within each colony were closely bound up with the direction of imperial policy and administration. In looking at these struggles he comes nearest to confronting the internal social history of the colonies. Allegiance or opposition to proprietors, royal governors, local magistrates, or various factions within elected assemblies was determined by whether the interests of those authorities co-incided with one side or another in a colonial dispute. Behind quarrels over taxation, land, defence policy and, therefore, political power, Sosin discovers divisions between established and recent settlers, competing commercial groups, or religious and ethnically based factions. Thus, the "rebellion" in Virginia in 1676 was caused neither by Governor Berkeley's cupidity or tyranny nor by the desire of the leader of the rebellion, the young planter Nathaniel Bacon, to establish popular democratic reforms. It arose from bad administration at the county level, a failure of communication by the Governor's council, especially about defence policy, general economic distress in the colony, and, characteristically, the opportunism of the newly-arrived upper ranks whom Bacon represented. In Massachusetts in the early 1680s factional differences often took the form of the mercantile establishment against the upholders of religious tradition. Sosin does not admit the presence of a recognizable social dimension. The "moderates," the party generally backed by the merchants which was prepared to come to terms with the wishes of the English government, were often linked together economically through land speculation and socially through marriage. They generally had superior social standing and resented the fact that the zealous Puritan party was also the popular party which appealed to the small towns, controlled the General Court

and attacked the urban mercantile elite. Similarly, the fact that freed servants found access to land increasingly difficult in the middle colonies is admitted to having contributed to social divisions there. And even Bacon's rebellion, although not caused by social discontent in any conventional sense, did reveal, Sosin says, the presence of real class animosity and resentment in both directions.

Sosin's focus is always on how the divisions within the colonies fitted the nexus of transatlantic connections which, according to this account, shaped colonial history. His work is a corrective to those local studies which have analyzed social and generational tension or examined the rise and decline of shaping social ideas without fitting them into the larger picture. But the relationship of the findings of those studies to the divisions and connections which Sosin writes about does not emerge clearly. There are two more volumes to follow so a history integrating all these considerations may yet emerge.

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Michael Stephen Hindus, *Prison and Plantation: Crime, Justice and Authority in Massachusetts and South Carolina, 1767-1878* (Chapel Hill: University of North Carolina Press 1980).

USING A WIDE VARIETY of court records, contemporary manuscripts, and treatises, Dr. Hindus has written an original, comparative study of crime, justice, and authority in two of the most different states of ante-bellum America. The book focuses on the relationship between law, economy, and society in the urban and expanding industrial society of Massachusetts and in the rural slaveholding society of South Carolina, and makes a qualitative and quantitative analysis of their legal cultures. The formal structures of authority in Massachusetts revealed how the rule of law was used to order the affairs of society, and the state's use of the criminal sanction was also reflected in other areas of the legal system. Thus law was an important element in regulating personal conduct, and the courts were centralized and efficient. In South Carolina, however, the law was minimal, and hence did not reach many areas of life. The criminal sanction was wielded privately by the planter aristocracy using extra-judicial means, and institutions both central and local — including the courts — were ineffective and incompetent.

A striking feature of Dr. Hindus' work is the similarities he draws from the two different legal cultures, similarities which allow him to conclude that sharp structural contrasts often obscure important similarities. The two states shared a class orientation, delays and harassments in the courts, inequities in sentencing and pardoning, and miserable conditions in the gaols. The judicial system in each state upheld the power of dominant groups in society, pitting industrialists against workers in Massachusetts and planters against slaves in South Carolina. Deference was used constantly in the administration of criminal justice, enforcing the social and economic systems. Thus the prison in Massachusetts was modeled after the factory, and in South Carolina after the plantation. Personal violence increased in both states, and the rates per capita of homicide and suicide were higher after 1870 than before 1860. In the end, it is the "purpose" of law and authority, and not the structure or institutions, which should provide the point of comparison. Social, racial, and political antagonisms will fuel the sense of threat and danger that creates a prison or plantation regardless of its institutional or anti-institutional form.

On the institutional level, however, the Massachusetts system became the prototype of modern American criminal justice. But its values incorporated those shared interests of both states: the promi-

nence of race and class. Planters retained their hegemony without building legal and political structures, and industrialists retained theirs by the creation of such structures. Honorary officials continued down into the twentieth century together with other ante-bellum features of American society such as no gun controls, untrained police, anarchy in the streets, delays in the courts, and squalor in the gaols. The landscape is not an attractive one, but Dr. Hindus marshalls his facts to demonstrate its contours.

The book has several attractive supplementary features. The tables on patterns of crime in rural and urban counties, criminal prosecutions, county and municipal conviction rates, sentences for personal, property and morals offences, sentences by race, status and sex, and different forms of punishment form a lucrative mine of information for the social historian. The annotation is excellent, and the bibliography comprises a wide range of periodical literature, theses, and unpublished papers. The index is workable, but the subject references are very narrowly interpreted and may mislead the unwary reader. Dr. Hindus will be criticized for his biases, and for his interpretations, but this book will stand as a model of legal and social history long after its detractors are forgotten.

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Claudia L. Bushman, *"A Good Poor Man's Wife" — Being a Chronicle of Harriet Hanson Robinson and Her Family in Nineteenth Century New England* (Hanover, N.H.: University Press of New England 1981).

ON 10 OCTOBER 1864, Harriet Hanson Robinson recorded in her diary with considerable pride that it was her housekeeping talents which enabled her family to live comfortably within their very limited means. As a result, she judged herself to be "a good poor man's wife." (106) Harriet, however, was far more than a devoted wife, mother, and housekeeper. She was a noted Massachusetts suffragist, women's club organizer, and author, whose most important work, *Loom and Spindle*, has become a classic in the study of nineteenth-century working women. Moreover, her life (1825-1911) spanned eight decades of important transformations in the American nation, and in the lives of its women.

Fortunately for the historian, Harriet Hanson Robinson was a dedicated diarist for most of her life. Her extensive diaries (27 daily diaries) permit us not only to enter the private world of the Hanson and Robinson families, but also to acquaint ourselves with the issues and personalities of her time. As a "second-echelon social reformer" (xi) married to William Stevens Robinson, journalist, political activist, and author of the widely read "Warrington" letters, Harriet recorded her "salty comments on contemporary social and political issues — abolition, suffrage, labour reform, Massachusetts politics — in which she and her relatives were involved." (xiv) Claudia Bushman has therefore performed an important service in bringing this remarkable collection of family diaries, scrapbooks, and correspondence to public attention.

The history of Harriet Hanson and her family over several generations is particularly valuable for documenting the life cycle of American women in the nineteenth and early twentieth centuries. More specifically, it reveals the extent to which middle class women's lives were burdened by on-going concerns (financial, religious, social) and disrupted by sudden crises (loss of husband and/or child). Although William Robinson left behind many personal papers, this volume is drawn primarily from the writings of the female members of the family, and it is therefore essentially a tale of women's lives. It is Bushman's contention that "sundered by early male death and financial failure . . . the Robinsons were matriarchal." (xv) By the time one comes to

the end of this saga of four generations of the family, one is inclined to agree that the women in each generation formed the linch-pin of the family.

When Harriet was only seven years old, she accompanied her recently widowed mother to the thriving textile center of Lowell, Massachusetts. Here Mrs. Hanson found employ as a boarding house keeper for one of the textile corporations, and thus managed, in spite of tremendous odds to keep her family together. Her lifelong commitment to her family, and her penchant for independent thinking, especially in religious matters, were formative influences on the young Harriet. By age ten, the latter was busy working in the mills as a doffer in order that she might also contribute to the family's sustenance.

Chapters II and III of this volume are devoted to a discussion of Harriet's work and education in Lowell. These are clearly the best chapters in the book for the author supplements the material contained in the family documents with many useful references to other secondary studies. Unfortunately, this effort to place Harriet and her family within the larger context is not sustained; several of the subsequent chapters are based almost exclusively on the family papers.

The early chapters of the book also presage a disturbing feature of this book. As the story of Harriet Hanson unfolds, the author becomes increasingly critical of her, and the empathy of the author for subject which is so essential to good biography rapidly dissipates. This is surprising for as the author states in her acknowledgements, "... Harriet Robinson's character has value for me in working out my own destiny, and my life has been bound by hers for ten years." (ix) One therefore expects to encounter a very sympathetic treatment of this personage. Yet, chapter by chapter, the author painstakingly assembles "evidence" to demonstrate that Harriet was an egocentric, vindictive, power hungry, and somewhat superficial woman driven by a desire "to write herself a place in history." (1, 27) When Harriet published a collection of her deceased husband's writings at her own expense, the motive attributed to her is a desire for power: "She hoped to build a place for herself on her dead husband's career." (138) She is described as using various "devices" to enhance her husband's importance. (140) In the preceding pages, however, Professor Bushman has included several quotations indicating Harriet's passionate devotion to her husband. Is it not possible, then, that it was this love and loyalty which led the middle-aged widow to go into debt for over a thousand dollars, leave her family for two months, and personally hawk copies of the book throughout the state?

Harriet's own writings are also treated harshly. *Loom and Spindle* merits more than four pages of analysis. (183-8) Harriet's glowing account of the early days in the Lowell mills is explained by her desire "to show that female operatives were as good as everybody else — that they were equal to and indistinguishable from others.... Harriet divided and defined society so that she, as a former poor working girl, could be at the top." (185) As a result, she is taken to task for suggesting that some workers were drawn from impoverished "better" classes and is ridiculed for her statement that "There were others who seemed to have mysterious antecedents ... and strange and distinguished looking men and women sometimes came to call on them." (185) Professor Bushman, however, fails to point out that the majority of female operatives came from rural New England families. In fact, some of her principal observations coincide remarkably well with those of Thomas Dublin in his recent study *Women at Work*.

The author also rebukes Harriet for idealizing the mill girls who contributed to the *Lowell Offering*, and for exaggerating their literary achievements, by pointing out that "few of those involved con-

tinued to write; only four went on to produce books in their maturity." (187) It is interesting to compare this assessment with that of Jane Wilkins Pultz in her introduction to the 1976 edition of *Loom and Spindle*. Pultz makes the following evaluation of these same young women:

> Many left the mills to teach, or to start their own schools, several went as missionaries to the Indians of the American West, one studied law and became a well-known sculptor, and at least two founded libraries in their New Hampshire towns. Since many of Harriet's friends were contributors to the *Lowell Offering*, it is not surprising that they authored at least thirty-one books, four of them were editors of newspapers or magazines, and they published so many articles and periodicals, ranging from Sunday School papers to the Atlantic Monthly, that it is impossible to estimate their number.

Harriet's feminism is also rendered suspect, for the author contends that her personal relations were more important than her suffrage objectives. Much is made of her personal grudges while some of the truly fascinating facets of her feminism, such as her interpretation of the Trinity ("God the father, God the mother, who is the holy spirit of productive love, and the son. Three in one."[99]) are quickly passed over. It is interesting that the title of this book, "A good poor man's wife" provides no clue to the various public roles that the dynamic Harriet Hanson Robinson played throughout her long life.

While the author's interpretation of the character and significance of Harriet Hanson Robinson is open to debate, she is to be commended for effectively displaying the rich tapestry of domestic relations of a most interesting nineteenth-century American family.

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Linda K. Kerber, *Women of the Republic: Intellect and Ideology in Revolutionary America* (Chapel Hill: University of North Carolina 1980).

DESPITE ITS COMMITMENT to explore rarely considered aspects of the American Revolution, this book has a conventional organization of topics. There are four subtopics — the political ideology of the imperial crisis, the dislocations specific to the war, the deliberate and unintended social changes following on independence and republican government, and the development of American nationalism. Kerber describes the role of women, particularly their intellectual responses, in these contexts.

For women, as for most men, the revolution increased their degree of politicization, but women's formal participation remained nearly nil, while that for men became central to their culture. This diversion was not intellectually inevitable. Political thought in the Enlightenment was often so abstractly oriented to natural rights as not necessarily to preclude women from them. Social commentary noted that the condition of women varied from society to society, and therefore the usual subordination was not a matter of natural law. Little thought, however, was given to the bridge between hypothetical equality and actual subservience: women were seldom a separate focus of political thought. Shrewd and wide-ranging as Kerber's discussion of early modern political thought may be, it has an anachronistic quality. Her analysis shows that intellectual rigour required that the political status of women be an important topic; her chief empirical finding is that it was not. Yet the nominal focus of her book is the status of women in revolutionary ideology: "If American women were to count themselves as the daughters of Liberty, they would have to invent their own ideology." (32)

Most of the book shows how women took part in the revolution, in peculiarly feminine ways, without such an invention. Despite the etymological dissociation, women were patriots (on both sides). For example, they took part in boycotts of British goods by ostentatious self-denial

of luxuries, by increased domestic manufactures, and by demonstrations against retailers. As Kerber appreciates, there was little new in such activities, which were consistent with women's roles in the domestic economy. During the war itself women played traditional roles in unusual circumstances: the wives of loyalists were exiled regardless of their own views; soldiers' wives served as mess cooks and nurses; and landladies became jailors. These services had such low levels of institutionalization that experience in them did not forge bonds of allegiance for women the way military service did for men.

Much of the evidence presented in these discussions is intrinsically interesting, but it usually has an anecdotal quality. This anecdotal treatment depends in part on the isolation of women. Their petitions, for instance, were usually individual ones, not by groups. (After the war, however, women's organizations flourished.) Since the structure of Kerber's interpretation alternates between reference to traditional behaviour and observations on lapses in accommodation between ideology and participation, there is a lack of a positive analysis of social change taking place for particular groups at particular times and places. It is the Revolution, not women, which implicitly argues for the importance of the book's subject. Yet the social and intellectual history of women may have a periodization quite inconsistent with that of national history. The most important change in the status of women during this period was their achievement of literacy equivalent to men's (at least in the North) in the half-century after 1790. Why this change from traditional behaviour occurred is still unclear. It was certainly compatible with republicanism, since the role of motherhood could be identified with the socialization of citizens. Whether or not republicanism necessitated it is another question.

The chapters dealing with the legal history of the revolution avoid anachronism more than most, since the issues of women's status were made explicit at the time. Common family property rights, especially in dower, complicated the confiscation of loyalist property: should loyalists' wives retain rights to dower if they were themselves supporters of the revolution? To encourage wives of loyalists to stay rather than to go into exile, Massachusetts guaranteed them dower rights. Women who accompanied their husbands, however, were ironically burdened by the American application of the common law: their property interests were subsumed, and in these cases penalized, with their husbands'. Hence a body of law intended to protect women, because of their legal inabilities when married, became a means to rationalize their deprivation in the name of conjugal loyalty.

This specific denial of dower rights for loyalist women was part of a more general pattern: "The erosion of dower rights was the most important legal development directly affecting the women of the early Republic." (147) Wives' property rights became less protected against suits for their husbands' debts, and the sale of land without renunciation of dower was facilitated. Such changes in the legal status of women accorded with men's interests, and thus divorce law changed little from the colonial period. This lapse occasions one of Kerber's most anachronistic comments: "The rhetoric of revolution, which emphasized the right to separate from dictatorial masters, implicitly offered an ideological validation of divorce, though few in power recognized it." (184)

Despite the title, there is little consideration, after the introduction, of the sexual roles specific to republics. Yet in the most deeply republican area in the world, Switzerland, wives in some cantons were denied independent property rights and political participation into the middle of the twentieth century. Kerber thinks that the denial of equality to women represented a compromise of the revolution by

traditional values. That surely occurred, but the new values affirmed by the late eighteenth-century democratic revolutions, equality and individualism, reinforced the traditional subordination of women. After all, the ideal citizen was the yeoman, and the equality was fraternal.

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Louis J. Kern, *An Ordered Love: Sex roles and sexuality in Victorian Utopias — the Shakers, the Mormons, and the Oneida Community* (Chapel Hill: The University of North Carolina Press 1981).

IN THIS STUDY OF THREE nineteenth-century American utopias, Louis Kern seeks to demonstrate that in Shaker, Mormon, and Oneidan communities "questions of sexuality and sexual behaviour were an integral part of life . . . intimately related to their theological and ideological foundations" and thus to challenge the assumption "that economic and political affairs are *preeminently* important in the historical process," a premise he finds particularly prevalent and limiting in earlier studies of Victorian communitarian experiments. The book begins with a discussion of the mixed hysteria, indignation, and fear which characterized popular reactions to the utopians. Kern describes these as a psychological projection, a transfer onto the innovators at the margin of the guilt about sexual pleasure and non-reproductive sexuality, the forbidden desires and the clandestine practices of the mainstream. The remaining chapters deal in turn with the celibacy of the Shakers, the polygamy of the Mormons, and the pantagamy of the Oneidan community by providing "capsule psychobiographies, grounded in Freudian psychological assumptions" of the movements' leaders, and then analyzing the theological and ideological assumptions of the new sexual gospels, suggesting in the studies of particular sexual practices the ways in which the nineteenth-century sectarian utopias "provided a primitive form of proto-psychoanalysis, an institutional way for society to deal with individual psychological problems."

Several useful strands appear in this web of arguments. There is a thoughtful analysis of the relationships between post-millenarian theological propositions and the utopians' attempts to begin reform with the smallest possible social units, individuals and conjugal pairs, to reorder the disorder created by industrialization first through challenges to the power relationships of intimate and spiritual life. Each account of the remaking of sex roles, so as to usurp erstwhile female power for the male elders of the sect, accentuates the dimensions of the threat posed by women's growing social, moral, and religious authority in nineteenth-century America.

And yet the author has over-reached his mark in his attempt to right the balance among economic, cultural, and psychological elements in the study of communitarian movements, a laudable aim even if many readers will not have found other studies equally unsatisfactory in this respect. The demands of intimate life are presented as implausibly all determining. It is not possible to credit an analysis of Mormon widows which emphasizes their post-menopausal condition to the exclusion of welfare considerations, or a discussion of the intervals between marriages under polygamy which weighs the strengths of man's affections and sexual appetites over the life cycle but does not similarly monitor his changing wealth. Although he rejects the applicability of "modern standards" to the study of sectarian utopias of the Victorian era, Kern's Freudian presuppositions loom large, as does his retrospective interest in the tenability of more recent communitarian experiments. Without the anchors of careful chronology or of distinct economic and political context, the concerns of the 1960s seem to over-ride the priorities of a century before.

While coping with these frustrations, readers will find especially taxing the necessity to keep the complete *OED* and

standard reference works on utopians close to hand in order to disentangle a flamboyantly arcane diction and the frequent allusive references to events crucial to the analysis.

Joy Parr
Queen's University

David J. Jeremy, *Transatlantic Industrial Revolution: The Diffusion of Textile Technologies between Britain and America, 1790-1830s* (Cambridge, Mass.: MIT 1981)

TECHNOLOGICAL INNOVATION, while not as celebrated as the moment of invention, is equally important to industrial transformation and in this book David Jeremy does an excellent job in exploring the complex factors which influenced American manufacturers in the adoption and adaptation of British textile technology. Drawing from an impressive array of transatlantic historical sources — patent office records, British and American immigration records, local factory records, private manuscript collections, and early technical journals — Jeremy raises several interesting questions about the social and economic forces shaping technological diffusion, especially about the role of skilled workers in this process.

Jeremy examines technological transfer through four basic stages from the accumulation of the necessary workers, machinery and capital, the establishment of pilot projects, the widespread use of the new technology, and finally the adaptation of that technology to take advantage of local resources. After demonstrating the failure of legal restrictions to stop the flow of machinery, plans, and skilled workers from Britain to America, Jeremy details the transformation of cotton spinning, power loom weaving, calico printing, and woollen manufacturing through each of these stages. Successful transformation depended on a wide range of factors from the greater strength of American cotton fibres which allowed for the early introduction of power looms to the American manufacturers' ability to defend their small markets against more powerful British competitors.

The key link in this process of diffusion and innovation, however, was the skilled artisan. Samuel Slater and his lesser known comtemporaries provided the craft skills which enabled American capitalists to understand and operate the new British technology. Their experience in America demonstrated how the breakdown of traditional craft secretiveness and the increasing use of patent laws alienated craft skills from the workers. As Jeremy's history of the Boston Manufacturing Company illustrates, once this knowledge had been "institutionalized" within the company, technological adaptation focused on the elimination of skilled labour and the substitution of highly paid male workers with cheap female labour. The success of the Boston Manufacturing Company's Waltham system was demonstrated in the Lowell mills.

Jeremy also draws some necessary distinctions between the individual and aggregate effects of the immigration of British textile workers on the United States. For the most part, emigrant textile workers were fleeing the influence of the new British technology and only a small percentage, usually older men with years of experience in factory production, were responsible for technological diffusion. By the 1820s, British emigrants could only fill about half of the jobs created by the expanding cotton industry. Native American workers, particularly machinists, also contributed to the impressive innovations of those years. While Jeremy touches upon the conflicts between British workers and American manufacturers, the reaction of native workers to those developments is neglected. Given the growing interest in transatlantic cultural ties of American workers, an examination of technology's influence on such ties would have been enlightening. As well, Jeremy's claim that the Waltham system permitted a more humane treatment of

American textile workers demands some demonstration.

Still, it would be unfair to dwell too much on these limitations which lie outside the main concerns of the book when Jeremy pursues his main themes with diligence. This is an important book even for those whose main interests lie outside the history of technology or the textile industry in the United States because it sheds light upon the social and economic role of technology in the lives of early nineteenth-century capitalists and workers. With illustrations of textile machinery and a glossary of technical terms, Jeremy does his best to explain to technological illiterates like myself, who do not immediately appreciate the importance of such innovations as Horrock's variable batten speed motion or the Schofield slubbing billy, the significance of innovation.

Peter DeLottinville
Public Archives of Canada

Thomas C. Cochran, *Frontiers of Change: Early Industrialism in America* (New York: Oxford University Press 1981).

THIS RELATIVELY SHORT ESSAY by one of the deans of American economic and business history explores the causes and implications of the rapid industrialization of the United States between 1785 and 1855. Impressively well-grounded in the researches of modern American economic historians, the book is nevertheless "old" rather than "new" economic history in basic approach. It offers no formal models, no graphs, no long columns of figures, no explicit testing of hypotheses. This is because, to Cochran, the movement of such observable economic variables as prices and output represents only a part of the development story; these variables and relevant institutional changes, to which he gives considerable attention, are both results of deeper and more fundamental workings of cultural values.

The book's hero is the often anonymous American master mechanic, the builder of tools, machines, and mills, whose practical skills created, improved, and adapted the basic technology of the period to cheapen and increase production across a broad range of industries. Such artisans, far from resisting change in defence of a traditional way of life and work, played entrepreneurial roles in technological and economic development. Indeed, what impresses Cochran is the pervasiveness in America from the eighteenth century onward of the spirit of innovation, as revealed in the readiness to take risks, to fail, and to adapt (e.g. by trying new techniques or by moving on) that such Americans showed.

At the centre of the book's view of industrialization are metal-working and machine-making, and it is very well informed not only on the complex parts played by iron and coal in America, but also on the role of such other key industrial materials in the United States as wood and leather. These and related industries developed particularly in what was, to Cochran, the key region in America's economic success, the mid-Atlantic region from New York to Philadelphia and Wilmington. Mechanized textile manufacturing, notably the famous mills of Boston's hinterland, he characterizes as a "by-product" of the more central changes embodied in machine-making as such. The dominant place of the great New England mills in our usual images of American industrialization he attributes to the very uniqueness of these mills and to their proximity to a major port through which many travellers and writers journeyed (and, a modern observer could have added, to their proximity to the modern Boston area academic research complex).

Despite this argument that one region was more vital than the others, Cochran, following Diane Lindstrom in particular, views the development of the eastern United States as essentially an intra-regional process, in which each of the east's four major ports and its adjacent

hinterland constituted, initially, a relatively separate region. After about 1808, he does not think foreign trade was a leading sector in American development, nor was foreign competition really the major problem for aspirant industrialists that tariff advocates thought it was. Rather, American manufacturing rose largely by supplanting household production in rural America and through the expansion of urban markets themselves. Within each city's nearby hinterland and then increasingly farther afield, new urban demand permitted farmers to increase their incomes and to specialize, which, in a circular and cumulative process, permitted them in turn to buy more urban goods. As such development occurred, given the proximity of the major centres, American growth was further enhanced by the opportunities available for inter-regional trade and specialization.

The book is informed by a sense of international context, although it is by no means a systematic comparative treatment. Cochran cannot resist asserting early American leadership in many areas of mass production, something he thinks is too little recognized, but he nevertheless strives to avoid models that see industrialization as a race, in which one country led and others followed at various distances. Specifically, while recognizing the variety of international economic interchanges of his era (including the movement of skilled British artisans), he contends that models that assert that the industrial revolution in other countries than Britain resulted from the diffusion of British technology and capital cannot account for the actual processes of change either in the United States or in northwestern Europe. Thus, he argues that the industrialization of the United States was a largely "self-sufficient" process partly paralleling and partly diverging from both British and European patterns. Where there were variations, these were determined by culture, geography, and resources.

To anyone who still holds to the once common view of a period of economic take-off, in the American case after 1840 and led by railroads, Cochran's stress on pre-1825 developments will be especially striking. By no means does he underestimate the roles of railroads in the American economy, though he sees them as "not dramatic," but, he says, the truly "revolutionary" phase of American industrialization, if there was one at all, was over by 1825. If we have failed to see how far the United States had progressed by 1825, it is, he thinks, because the immense task of settling the frontier was still in its infancy and because of the sheer scale of American agriculture, which may have led us to neglect the extensive industrial growth already underway in the east.

There is much sound common sense, and much to debate, in these reflections of a very accomplished historian. In making his case for the importance of the 1785-1825 period, he paints a picture of American agriculture and of pre-1785 America generally as in some sense traditional or backward that seems too schematic and too little attentive to the dynamics of the eighteenth-century colonial economies. Nor does he resolve very satisfactorily the question of why, across western European society, a culture favouring innovation developed unevenly. For example, it is not clear whether the apparently divergent culture of the American south was cause or consequence of the divergence of its economic patterns from the northeast. In fact, geography and resources may often be sufficient explanation for variations, and culture may not need such emphasis.

While Cochran is in many ways right to reject the leader and followers model of the industrialization of this era, it is also clear that time does matter, that is that some discoveries do not need to be made more than once and that the economic world into which later industrializers emerged was not one where the processes of an earlier time could be duplicated. To

one class of reader to whom the book is addressed, the student of modern economic development, there is really only discouragement in the message offered, because it is not at all clear how one would set out to create the culture that is apparently required for self-sustaining industrialization to take hold; and without that culture, the book's message is that economic development policies are doomed to fail. Finally, the book asserts more than it analyzes in detail the nature and ubiquitousness of the particular artisan culture on which Cochran places such emphasis. Even so, his book serves its purpose well as a brief and stimulating survey of some central issues in American economic history.

Douglas McCalla,
Trent University

Anita Clair Fellman and Michael Fellman, *Making Sense of Self: Medical Advice Literature in Late Nineteenth-Century America* (Philadelphia: University of Pennsylvania Press 1981).

MAKING SENSE OF SELF focuses on American medical advice literature of the 1870s and 1880s. In what is a very short book (141 pages of actual text) the Fellmans attempt to analyze what the advice revealed about the intellectual and emotional state of the people who wrote it and possibly the people who read it. Unfortunately, they sidestep the issue of whether or not anyone followed the advice and in doing so eliminate an important anchorage for what is a sophisticated study of often contradictory and ambivalent advice.

Although concern for self as represented by interest in the health of the body and the mind was not new, the Fellmans argue it was given added potency in the late nineteenth century. The white collar and new managerial positions emerging divorced the worker from what was produced creating an ambivalence and anxiety in many about the validity of what they did. While an interesting theory, it is difficult to substantiate. What is not is the anxiety felt which seemed to focus on physical and mental well-being. Americans believed in a healthier or more natural past which had been lost and which they wanted to regain. In effect they wanted to return to Eden. To do this they needed help. People could no longer trust their own instincts for in a modern industrial society even they seemed to have lost their moorings. Needless to say, dependence on experts was an easy solution. Someone in authority would tell you what to do and how to behave and all you had to do was obey and physical and mental salvation would be yours. The religious analogies abound, although the Fellmans insist on downplaying them in favour of economic ones.

Americans were uncomfortable with, self conscious about, but fascinated by their own bodies. They believed the body, a closed system with finite energy, had to be cared for and felt it was the responsibility of the experts to indicate how this was to be done. However, the Fellmans note two problems in the medical advice given. First, advice which on the surface seems innocuous enough could have hidden repercussions. For example, the suggestion that one should not force the body to do two things at once when directed at women resulted in the prohibition of intellectual training during the years of puberty. Second, the belief that through reason "nature would be revealed and humans could learn the rules for right living which civilization has obscured" raised questions difficult if not impossible to answer. (29) For instance, what was natural in an industrial society? What Americans wanted and what the advice literature was hard pressed to give them was the advantages of modern industrial life with none of its disadvantages in the form of unhealthy bodies and tired minds.

The best the experts could offer was a precarious health based on moderation or

as the Fellmans term it "self-repressed expression." But what was it specifically that had to be repressed? Sexuality was the answer for some. Of particular concern was masturbation for it was feared that masturbation would force men and women to go against what was seen as the public norm. It supposedly made men isolated and withdrawn and made women victims to any profligate. In their discussion of sexuality, the Fellmans are careful to note that the Victorians did not believe in the sexual passivity of women. Nevertheless, as their study indicates, the double standard did exist. The medical literature stressed that unlike men, women were at the mercy of their bodies; they suffered from ill health as a result of what could be called biological determinism. This limited their ability to act both physically and mentally.

The latter was particularly vital. Mental work more than physical was the route to success in these decades and as a result physicians began to worry about brain fatigue which in extreme cases could lead to insanity. Because neurophysiologists believed in somatic or physical causes of insanity, little chance for cure existed since at this time physical treatment of the brain was not possible. The Fellmans argue that as alienists no longer believed they could bring about recovery, the cure rate of asylums declined. The declining rates of recovery, however, were probably linked more to the increasing crowded conditions of the asylums and the resultant lack of attention paid to patients. Despite the belief in somatic causes of insanity, treatment did not alter nor the belief in underlying moral causes of insanity. It simply made both a bit more complex. In the final analysis it was still moral causes, causes which people themselves could control or thought they could, which resulted in insanity.

Illness of the body or of the mind could be prevented but it was not going to be easy. The proper advice had to be followed and this often necessitated going against what appeared to be the natural needs or cravings of the body. To help in this, will power was exerted. In this way the individual had control over his/her own destiny. If will failed, then there was no-one to blame but oneself, certainly not the experts or their advice. Such was the insidious nature of the philosophy underlying the advice literature. As a result, many Americans had a feeling of malaise about themselves that they could not escape. By focusing on this physical-psychological malaise, the Fellmans have introduced a new aspect to an already complex period and one which future historians will have to recognize. To understand the past, the study of the political, economic, and social context is not enough. The physical and the psychological nature of its inhabitants must be included as well.

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R.A. Burchell, *The San Francisco Irish 1848-1880* (Berkeley: University of California Press 1980).

WHEN CALIFORNIA BECAME PART of the United States in 1848, San Francisco was but a tiny settlement. By 1870, it was the nation's tenth largest city. Of its considerable foreign-born population, the Irish were the most numerous and by 1880, one third of all residents could be counted as members of the Irish community. Other Irish Americans have been the subjects of many studies; until now the San Francisco Irish have not. With the publication of this important work, a prominent void in Irish American historiography has been at least partially filled. R.A. Burchell of the University of Manchester has provided an informative, interesting, and imaginative interpretation of San Francisco's early Irish community, which was in many ways unique.

The uniqueness of the Irish San Franciscan experience derived more from the location and newness of the city than from the Irish themselves, although the fact that

few of the early Irish residents came directly to San Francisco from Ireland was an important factor. Unlike the older cities of the East, where Irish Catholic immigrants by the 1000s encountered negative reactions in grand and disturbing doses when their religion was diagnosed as a prime cause of perceived social and economic threats to established residents, San Francisco was a new community, in a new state, in a new region of the country. There at the beginning, the Irish constituted a major element in San Francisco's early urban development and as the city grew with amazing speed, they developed with it. This "massive growth of the city," Burchell argues convincingly, "gave opportunity for the Irish to build into its central structure, which took form and character from their efforts."

The Irish presence in San Francisco did not produce the degree of conflict which arose elsewhere when, as Oscar Handlin found in Boston, "the old social order and the values upon which it rested were endangered." San Francisco, says Burchell, had no such old social order and thus provided a "relatively friendly environment" which "gave the Irish the chances they took." By contrast with other Irish American communities, the San Francisco Irish were "comparatively successful and fortunate."

Burchell examines in great detail both the Irish involvement with and contribution to the expansion of the city and concerns himself as well with the broader community's influences on the Irish. A somewhat expanded treatment of the immigrant's pre-Californian background would have been useful, but Burchell quite adequately treats their arrival and settlement and their changing residential and occupational patterns. This treatment together with his examination of Irish family and community life, their political and non-political associations form the bases for the author's analysis of the accommodation and acculturation — Americanization, if you will — of the Irish as he attempts by comparative methods to fit them into the patterns of the larger urban community. For the most part, he does this quite well.

Much of what the Irish achieved in San Francisco's early decades is credited to their persistence but, as Burchell takes care to explain, that persistence was at work in a generally favourable environment. He neither glosses over nor exaggerates the role of prejudice, whether against or within the Irish community, but maintains that it was a less important factor there than elsewhere because cosmopolitanism became an early characteristic of the city. Burchell argues that San Francisco, unlike Handlin's Boston, developed no "confirmed definition of racial particularism" which, as far as the Irish were concerned, marked them as "a different group, Celtic by origin, as distinguished from the 'true' Americans, who were Anglo-Saxon, of course." In a modification of the impact of a beneficial cosmopolitan San Francisco, the author does present a clear and sound examination of the importance of the substantial Chinese presence which, while it seemed to pose an economic threat helped to make the Irish appear far less threatening to Anglo-Americans.

In the preparation of this study, Burchell made good use of a variety of sources, especially census records, city directories, and the contemporary press. The results of his research are woven together in well-planned, generally well-executed writing ending with cogent, well-founded conclusions. The work is rich in detail (perhaps overwhelmingly so in places) about many aspects of Irish life in San Francisco; yet there are gaps. One wonders, for example, why the author, having compiled and presented so much data and analysis of the working-class Irish, failed to include anything on their contribution to trade unionism. There is also no discussion of Irish language survival or revival in San Francisco. Surely a goodly number of the early immigrants,

whether they arrived via Australia or the Eastern States, had their origins in the Gaelic-speaking West of Ireland and no amount of Americanization could cause, in such a short period of time, the complete demise of their use of "the language that the stranger does not know." (In fact, a Philo-Celtic Society to revive Irish was founded in San Francisco in 1879.) While one might wish these and other gaps filled, the work nevertheless represents a valuable addition to American ethnic history.

A few other things, however, detract from the book's overall quality. Most serious is Burchell's briefly-noted and exceedingly thinly based claim that the San Francisco Irish were beginning in the 1860s to think themselves threatened (whether socially, economically, or politically is not made clear) by blacks (136), a claim left unexplained and undocumented in a work in which most documentation seems complete. Perhaps only a New Yorker would be concerned that Chanango County becomes Shenango County (217, n.169), but everyone who contends on a regular basis with undergraduate writing should see the redundancy of a "legacy... from the past." (12) Finally, for all its merit, the book too often fails to convey a sense that it is about real people.

To conclude, this reviewer got the decided impression that at least some of the author's ability to see the uniqueness of the Irish experience in early San Francisco is derived as much from Mr. Burchell's love of San Francisco today as it is from his appreciation of the San Francisco of the past. It is hoped that he will continue his research on the San Francisco Irish.

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William M. De Marco, *Ethnics and Enclaves: Boston's Italian North End* (Ann Arbor: UMI Research Press 1981).

WILLIAM DE MARCO's *Ethnics and Enclaves*, which is part of UMI Research Press's series, "Studies in American History and Culture," is not really a book but rather an extended essay. After accounting for the index, bibliography, appendices, maps, tables, and photographs, the original 156 pages are trimmed to 57 pages, a mere 47 of which deal with original research. The volume is a study of Boston's Italian North End to 1930 and is less concerned with a general chronological history than with distinguishing the various Italian immigrant subcultural groups which De Marco (acknowledging Amos H. Hawley) labels "enclaves."

De Marco, however, does not explain clearly his theory of enclaves. He proposes that although most scholars of Italian American urban settlement examine ethnic communities of Italians *qua* Italians, they should instead recognize the differing enclaves of Old-World regional, provincial, and village groups within the homogeneous Little Italy. In one instance he writes about the co-*regionalists* from Abruzzi, Campania, or Sicily (37); in another chapter the enclave involves co-*regionalists* but also subsumes groups of Old World *townspeople* from Taurasi, Chiusano, San Domenico, and Mirabella Eclano. (23) Elsewhere we learn that an Italian attended only one of the two Italian National Parishes in the North End depending on his/her *provincial* or *hometown* origin in Italy. In other words, De Marco never states explicitly whether the primary contacts of an enclave are based on regional, provincial, or more local Old-World loyalties.

The issue becomes even more confusing when the enclaves are defined as (Oscar Handlin's) " 'functional equivalents' of the village and family network." (xvii) Certainly, enclaves of people from the same village could maintain to a great extent their hometown's kin and family networks. On the other hand, migrants from a common region (which contains scores of villages) could never duplicate the close-knit ties of their respective Old-World villages, in the North American urban setting but at best could create new community networks.

A second major shortcoming of this study is its failure to convey to the reader the true value of enclave theory. One senses that the author has taken on a greater task than he can handle, and he does not recognize the import of his findings. As a result Dr. De Marco inflates the originality of his work but never places it in its deserving context.

The idea of studying immigrant groups in terms of regional, provincial, or village enclaves was not discovered by De Marco. Rudolph Vecoli, in his 1963 doctoral dissertation on Chicago's Italian community to 1915, discussed settlement patterns, work, and voluntary associations almost exclusively in the context of hometown and regional enclaves. In recent years numerous theses have been and are being written from the perspectives of regional, if not more local enclaves, rather than from the perspectives only of national groups, that is, Norwegians, Italians, Slovaks, or Greeks.

The volume is divided into five chapters, four of which examine the North End's settlement patterns, church attendance, occupations, and intra-marriage rates. Chapter 2, on "Enclaves and New Villages," asserts that Italian subcultural neighbourhoods developed in the North End, and indeed the reader must take those assertions on faith. Except for a minor foray into the early Genoese community in the Ferry Court area and on North Bennett Street, De Marco devotes four short paragraphs to describing the enclaves of immigrants from Abruzzi, Naples, and Campania in the turn-of-the-century North End.

The chapter on marriage patterns reveals interesting data on intra-marriage between co-provincials or co-regionalists. In a sample of 1,582 marriages, taken from the records of St. Leonard's and Sacred Heart Italian National Catholic Parishes every ten years between 1899 and 1929, intra-marriage rates between co-provincials never dropped below 76.3 per cent (39, St. Leonard's Parish, 1919) and reached 88.4 per cent in St. Leonard's parish in 1929. Intra-marriage rates between co-regionalists reached 90 per cent at Sacred Heart Church in the same year. Over 60 per cent of the weddings took place between individuals of the same hometowns. (40) Dr. De Marco confusingly concludes that "This variation of villages, towns, and cities represented in the respective Sacramental Files can be attributed to a retention of provincial ties just as the enclaves had demonstrated." (40) Does a 60 per cent hometown intra-marriage rate not suggest more localized ties?

What is more disappointing is the context in which the chapter on marriage patterns is placed. De Marco argues in his introduction and conclusion to that chapter that the *1910 Dillingham Commission* (*United States Senate Report of the Immigration Commission*, volumes 26 and 27 [*Immigrants in Cities*]), "failed adequately [sic] to identify and analyze" (35 and 44) the Italian enclaves of the North End. It seems silly for him to quibble with a commission which carried out its research over 70 years ago. Effectively, De Marco states that the value of his findings lies in his discovery of facts which a commission overlooked almost three generations back. No doubt he offers more than that. (Incidentally, De Marco chides the Dillingham Commission for defining Rome as a province of southern Italy; Rome is a city but in fact *is* situated in the province of Rome in southern Italy.)

The chapter on occupations is based on random samplings of names and occupations from the *Boston City Directory*, 1899, 1909, 1919, and 1929 but ends up as a grab-bag of details on work and enterprise with no interpretive framework. He presents no solid evidence to demonstrate how subcultural enclaves influenced work patterns (which, indeed, was probably the case). Rather than examine the family economy, De Marco simply asserts that wives in the North End were full-time homemakers, following their

traditional value systems of the "*via vecchia*" (the "old way" or "old road"). In keeping with the enclave theory, would it not have been more appropriate for De Marco to have differentiated regional mores from a generic Italian "via vecchia?"

The volume is handsomely assembled and maps and tables are well integrated with the text. Some of the photographs are well-selected; however, at least 15 individual and group portraits could have been excluded. The editing of the text is rather slipshod and the prose stilted.

Ethnics and Enclaves begins with fresh insights into the underlying identities within a supposedly homogeneous ethnic group. Unfortunately, the ideas are asserted and repeated but not developed or presented in a larger framework.

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James Borchert, *Alley Life in Washington: Family, Community, Religion, and Folklife in the City, 1850-1970* (Urbana: University of Illinois 1980).

REJECTING THE PREVAILING "urban impact" thesis which maintains that urban migration breaks down primary groups and destroys folk societies, James Borchert convincingly contends that during the period 1850-1970 the black migrant alley residents of Washington, D.C. "used their primary groups and folk experiences to create strategies which enabled them to survive the often harsh and difficult urban experience." (xii) Through the judicious and discerning use of a wide variety of printed, manuscript, and photographic documents, he succeeds in the difficult task of reconstructing the social life of a group which has not left a written record.

The first chapter of his work provides the basic historical outline of the "rise and fall" of Washington's alleys. Although there is very little documentation available, it is probable that the inhabitation of these alleys commenced during the antebellum period when affluent householders subdivided the rear of their properties and built small wooden frame houses for unskilled white labourers. A series of migrations by freedmen during and after the Civil War offered monetary incentives for landowners and builders to expand this low cost housing. By the latter 1870s large entrepreneurial developers took over the task of alley house construction and erected numerous series of two-storey row brick buildings. The demography of these alleys was transformed at this time from majority white to predominantly black segregated populations. The peak of alley inhabitation was reached in 1897 when nearly 19,000 dwellers, 93 per cent black, were reported. Borchert cites three causes for the attrition of alley populations and housing in the twentieth century. First, revolutions in transportation have allowed the concentrated pedestrian city population to disperse. Secondly, many inner city properties have been taken over by business interests to meet spatial demands for such things as warehouses, loading zones and garages. And thirdly, the public efforts of progressive reformers such as Jacob Riis and two First Ladies, Ellen Wilson and Eleanor Roosevelt, led to legislation that either condemned or restored alley houses. It is ironic to note that the rejuvenation of older alley dwellings today has been a part of "gentrification" by which certain remaining alley districts have become expensive and fashionable residential areas for Washington's elite.

The social organization of alley families is examined in the second chapter. Rather than adopting simple conclusions on the basis of census statistics alone, Borchert seeks "central tendencies" as revealed in many case studies and photographs. His analysis here stresses the historical continuity of extended kinship networks among black families in the move from country to city. He is highly critical of historical studies of black

families which make value judgements on the criteria of mainstream family models and maintains that the variations perceived by such comparisons for the black family represent "different" orders rather than disorder and hopelessness. For example, he argues that the central role of women in alley families did not reflect matriarchal disorder so much as patterns of sexual egalitarianism "developed in the slave family." (71)

A strong case is made in the third chapter for the viability of alley communities. The physical isolation of the alley block encouraged face-to-face contact, social interaction, the development of ethical codes and sanctions, as well as various forms of mutual assistance. One form of protective association involved "guile and manipulation" whereby alley residents tricked "outsiders" into believing that alleys were dangerous places "thus maintaining their own community, family and 'turf' intact." (135) Again Borchert emphasizes historical continuity by attributing the antecedents of such practices to "slave efforts to manipulate the overseer." (135)

An entire chapter on childhood might well have been entitled "childlore" because of the extensive use that the author makes of children's folklore. Citing such well-known Afro-American folk games as the "dozens" as well as the texts of many rhymes and songs Borchert shows that the informal socialization processes of the traditional oral culture within the alley community were much better tailored to the needs of that environment than the formal education of the outside. These "traditional forms . . . maintained key integrating experiences." (165)

Survival strategies are a central concern of the fifth chapter, "Work and the Fruits of Labor." Here the author clearly demonstrates that an alley wife's financial earnings from laundering or domestic service were so meagre that even when combined with her husband's wages as an occasional day labourer they were insufficient to meet the everyday monetary obligations of rent, medical expenses, and the high prices charged by white alley store owners for subsistence products. As a result, traditional, collective, extra-legal, and illegal survival tactics were integral to alley living. These strategies included: general resourcefulness in the use of the environment through such techniques as gathering edible wild foods, firewood, and useful "junk"; skillful consumer practices such as the discriminating purchase of second-hand goods; the use of folk medicine and home remedies; the manufacture and sale of illegal liquor; gambling in the numbers racket and craps; and random, "necessary" theft. Borchert asserts that while activities involving crime and violence "may have been anathema to the mainstream culture, they represented acceptable and positive institutions within the alley." (195)

The book's chapter on religion and folklife affirms the unity of black rural and alley cultures. Alley religion continued to be conducted by folk preachers who like their congregations blurred the sacred and the secular while accentuating the physical and emotional dimensions of religious experience. These expressions were especially apparent in singing and in communal rituals such as funerals and calendrical celebrations. In terms of content the adaptations of folk narratives to the urban environment meant a shift to "more realistic and less supernatural." (207) However, the didactic functions of such narratives (e.g., for child rearing) remained. In general, folklore "continued to play an important role in socialization and social control." (208)

Borchert concludes his work by urging more scholarly enquiries into the alley experiences of folk migrants in major cities. He maintains that the larger significance of such studies for an understanding of Afro-American cultures is that alleys represent transitional "proto-ghettoes" — an avenue by which today's massive ghettoes "can be traced back to the slave community." (237)

Perhaps the most illuminating section of the appendices is entitled "Photographs and the Study of the Past." This is an insightful exemplification of the kinds of information which the historian can glean from such primary documents. The author unequivocally concludes that "photographic sources can stand on an equal footing with printed ones." (298)

In general this book is a fine Afro-American study that may serve as a model for future research into the history of urban minority working-class groups. However, two major flaws of organization and methodology must be mentioned. Firstly, although the initial chapter determines a chronology for alley housing, the rest of the work is largely a synchronic presentation and that is difficult to justify given a 120 year scope. Is is realistic to discuss briefly the religion and folklife of Washington's alleys, 1850-1970, as though it was an unchanging phenomenon? When speaking of *the* alley community can one cite statistics culled in 1880 and then leap to case studies carried out in the 1930s? Perhaps. But even with innumerable qualifications concerning the limitations of one's sources this approach would appear to fulfill a prophecy about cultural continuity even if that were not the case.

A second and unpardonable flaw of this study is that while alley life is most certainly a living memory for many Washington residents, Borchert has made no attempt to conduct oral history interviews because he got "side-tracked" by the rest of his documentation. His rationalizations about the limitations of such oral sources do not hold water, especially in light of the large number of biased accounts by non-residents that he has so thoughtfully interpreted. Moreover, as he himself admits, excellent examples of Afro-American folk histories based on oral history interviews exist, such as William Montell's *Sage of Coe Ridge* and Gladys Fry's *Night Riders in Black Folk History*. Folk histories from oral documents can be successfully written and one hopes that Borchert's work will be augmented by an enterprising field worker in the near future.

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I.K. Sundiata, *Black Scandal: America and the Liberian Labor Crisis, 1929-1936* (Philadelphia: Institute for the Study of Human Issues 1980).

THE ABOLITION OF THE trans-Atlantic slave trade did not end the export of labour from Africa. Instead, a steady stream of labour was sold under various guises including labour contracts and indentures. Labour from Liberia was regularly shipped to the Spanish plantation island of Fernando Po, notorious for its harsh conditions. In 1929, an international scandal implicated several high ranking Americo-Liberians in the continued traffic in coerced labour.

Liberia was colonized by black Americans encouraged to emigrate to Africa by groups such as the American Colonization Society. Membership in these societies, especially widespread in the American south, included blacks, abolitionists, and slave-holders. Similar to the British experiment in Sierra Leone some 50 years earlier, the Liberian venture involved a half-hearted attempt to rid the United States of unwanted free blacks. By 1900, however, only slightly over 15,000 blacks from the U.S. and the West Indies had emigrated.

These early settlers founded a precarious community, composed of several coastal enclaves. Most of the settlers were not attracted to farming. Many engaged in what little business was possible, and some even sought to reproduce in Africa the labour intensive plantations of the ante-bellum South. In general, the Americo-Liberians constituted a settler elite, living apart from the idigenous African inhabitants. Yet for these emigrant Afro-Americans to engage in enslaving

fellow Africans was certainly scandalous, although not unexpected in light of the ideology of the settler class.

The 1929 scandal was fostered, in part, by the Firestone Rubber Company and the Hoover administration. Since World War I, Firestone had been searching for a secure source of rubber. Liberia offered such prospects and in 1926 Firestone was granted a huge concession in return for the promise of capital investment and a direct loan to the Liberian government. Moreover, Firestone called for the eventual recruitment of over 350,000 workers, thus providing the commercial backwater of Liberia a considerable tax base. Despite Firestone's rather successful strategy to distance itself from the scandal, Firestone continued to rely upon labour recruited through local chiefs (notorious for abuses of power in return for monetary rewards) until 1962.

Sundiata's study of the 1929 labour scandal deals primarly with the domestic politics and international implications of the affair. This is a well documented and researched study. Chapter 5 on the Afro-American response to the scandal is particularly revealing of the disunity in the American black community on the heels of the Garvey movement. Despite the title and the critical role of labour in the events and precedents of the scandal, there is little in this book to interest those in labour or working-class history.

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Mark Thomas Connelly, *The Response to Prostitution in the Progressive Era* (Chapel Hill: University of North Carolina Press 1980).

ONE OF THE GREAT PROBLEMS of doing labour history is the difficulty of getting at what work, and life supported by that work, involved for the workers themselves. The greatest stumbling block is the scarcity of sources. Even after decades of compulsory education, literacy is still largely a possession of the middle classes. Historians have little documentary evidence that comes straight from the worker's mouth and must depend upon that channelled through the pens of usually well-intentioned but inevitably biased bourgeois reformers. Historians who would chart the course of women's work since the Industrial Revolution find their task further obstructed by the peculiar vision society has had towards labour performed by females — that it is somehow divorced from the rest of the economy and that the workers involved have basically different goals in mind than do their male counterparts. Predominantly female professions have often played upon this attitude to gain a foothold and have therefore clouded the issue even more. Is it surprising then that prostitution, an ancient field of labour still not seriously challenged by male competition, should be one of the least understood? Even today, prostitutes in most countries are not regarded as workers, as part of the labour force, as payers of taxes and contributors to unemployment insurance and pension plans. If the personal history of most workers is obscure, theirs is simply beyond the pale.

It is the difficulty of getting at the place of these workers in American urban industrial society between 1890 and 1918 that accounts for both the strengths and weaknesses of Mark Thomas Connelly's *The Response to Prostitution in the Progressive Era.* You will notice his careful title. A glance at Connelly's extensive list of primary sources shows why this should be so: chiefly comprised of contemporary journalism and a few public and private reports, the citations are overwhelmingly concerned with the prostitute as a social evil rather than as a social constituent. On the whole very little reliable information about the everyday workings of prostitution is to be found in this type of source. For those unfamiliar with the genre, a glance at the easily accessible Coles reprint of Ernest A. Bell's *War on the*

White Slave Trade (1909) should prove convincing. In an effort to eschew all (possibly instructive) pornographic detail, authors confined themselves to worldly-wise head-shaking. The product is often vastly entertaining but hardly informative. Rather than try to patch together something from these sources, Connelly has opted to write a book about the sources of information themselves. Although his insistence that others have elsewhere handled the history of prostitution adequately (6) is an overstatement of fact (as the almost total lack of any works on prostitution *per se* in his list of secondary sources shows), he should not be faulted for his choice. The history of how society perceived prostitution is a legitimate endeavour in its own right.

Connelly concentrates his study on what he terms antiprostitution. Such a focus, he maintains, allows him to discuss a variety of themes important to the United States society changing so inexorably between the Gay Nineties and the Great War. Therefore, although he may not be able to put his finger on prostitution's place in that society, he certainly paints a graphic picture of the part it was thought to play. In his own words:

> antiprostitution became a psychological clearinghouse for an extraordinary range of troubling issues: the appearance of independent and mobile young women in the cities, widespread clandestine sexual immorality, a conspiracy to flood the nation with alien prostitutes, the catastrophic physical and social consequences of the unchecked contagion of venereal disease, the depersonalization and commercialization of sex in the metropolises, an alien-controlled conspiracy to debauch American girls into lives of "white slavery", and, by 1918, the ability of the nation to wage war for democracy, decency, and civilization. (7)

Connelly constructs his discussion carefully, devoting an introductory chapter to an overview and then one each to the themes of women's emancipation, immigration, public health, urbanization and reformism, and ending with the loss of innocence made complete by the war.

This is an interesting and well-written book. The themes it develops are of importance to the social history not just of the United States but of other urbanizing societies. I have two last comments. One is that I wish Connelly had declared his colors on one issue. Did white slavery (the forcible recruitment and holding of prostitutes) exist or not? He explains its mythological basis: the concept that women were inately too pure to take up sex for either enjoyment or myth. But showing that something might be a myth is not the same as saying it is. I had hoped that, after plowing through as much material as he has, he could give a more definite answer. The other comment is that the work is written from an impeccably feminist perspective, one with which Connelly is clearly at ease. For those of us who support the synthesis of "women's" and "men's" history, this is indeed encouraging.

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Ronald Steel, *Walter Lippmann and the American Century* (Toronto: Atlantic Monthly Press 1980).

THORSTEIN VEBLEN WROTE ONE of his most interesting pieces on the subject of the intellectual preeminence of the Jews in nineteenth-century Europe. In his discussion he gave considerable weight to the effect of being in, rather than of, a given society. And that sense of being in but not really belonging to one's society gave bite to the Norski Veblen's lethal analyses of "his" America. A somewhat similar emphasis occurs in Glen Seretan's recent biography of Daniel De Leon where De Leon is explained, in part, by the myth of the wandering Jew.

Lippmann, like De Leon, was something of a wanderer. While he did not go to the same lengths to hide his European-Jewish lineage, he certainly spared no effort in wrapping himself in the garb of *homo Americanus*. Yet, like Veblen and De Leon, Lippmann remained at one

remove from American society. This gave to Lippmann's writing what most people accept as "detachment." But that Olympian pose in Lippmann was, more often than not, assumed for the purpose of advancing his own acceptability, influence, and, he hoped, power. Unlike the others, he had no passionate concern to achieve any basic changes in America. Because of this he was widely believed to be objective, reliable.

Ronald Steel's immensely interesting biography tangles with the psychological problem, but only as a very minor theme in an essentially political story. One can sympathize with the author because Lippmann's political-journalistic journeying is, of itself, more than enough to fill one large volume. As it is, Steel has had to eliminate a great deal of material, particularly correspondence, which will undoubtedly be worked over by successive theses and books. What he has achieved in this study is an excellent framework within which others will be tempted to work.

Steel's Lippmann appears, much of the time, as a sophisticated version of Upton Sinclair's Lanny Budd — a kind of steely-eyed observer-participant-commentator on all of the great events of his time. Yet Steel accepts the conventional view that Lippmann did, in fact, have real influence, and the informing purpose of his biography seems to be to assess and judge the "positions" taken by Lippmann at the peak points of that influence. Judged thus by a present-day progressive Lippmann was 'right' at the beginning (as a socialist blending into a progressive), and at the end (as an outspoken opponent of Mr. Johnson's War). What transpired in between, during the long years from about 1922 to 1965, is largely reprehensible.

At the beginning, the precocious youngster from Harvard proclaimed himself a socialist and his quickness of intellect caught the admiring attention of such eminents as Graham Wallas, Santayana, and William James. By 1914 he was with the *New Republic* crowd, hob-nobbing with the likes of Harold Laski, Mabel Dodge Luhan, Learned Hand, and Felix Frankfurter. His hero was then (and was to remain) Theodore Roosevelt. By 1917-19 he was with Colonel House and Woodrow Wilson, helping to draft the Fourteen Points and participating in the ill-starred American activities in Paris. In 1922 he wrote his best book, *Public Opinion*, expressing the view that most people live by stereotypes, and began his famous column "Today and Tomorrow" which appeared for the first nine years of its very long life in the *New York World*. In 1931 Lippmann moved himself and his column to the *New York Herald-Tribune*. By this time his name was a buzz-word across middle class America. Equally important to Lippmann, as the biography makes clear, was his mouth-watering salary of 25,000 depression-time dollars plus large (unspecified) revenue from syndication and generous vacation-travel time. Largely because it was widely assumed that his column *was* an opinion-former, he now found open the doors of the most prominent political leaders in Europe and America; he could have had a chair of government at Harvard or been president at Chapel Hill.

Somewhat disappointingly, Steel does not really probe the question of Lippmann's influence. One might well argue, on the basis of the ample evidence in his book, that Lippmann, more often than not, was read, received, or approved because he was telling his audience what it wanted to hear. Like a journalistic version of Adlai Stevenson, he couched in elegant language the conventional wisdom and made his readers feel superior as they found their views, or instincts confirmed and fleshed out by one who was so evidently "on the inside." And to the whole performance Lippmann added the needed touch of glamour with his professionally shampooed hair, personally tailored suits, and photos with the Lamonts at the cap-

tain's table on those marvellous Atlantic liners.

Is this too harsh? Well, maybe; but remember that Lippmann's brief flirtation with socialism came at the peak point of the Socialist party's electoral strength; his progressivism moved easily from the New Nationalism to the New Freedom; he was a "crusader" for the League and as quickly deplored the results of the 1919 "settlement;" in the 1920s he expressed *realpolitick* and as things fell apart he moved not quite as fast as did FDR, but after the fall of France proclaimed that isolationists had been duped about the causes of American intervention in 1917 by "a swarm of innocent but ignorant historians." Even in the few cases where direct influence can be demonstrated, as when Lippmann helped Dwight Morrow arrange a compromise in 1927 concerning Mexican appropriation of the Mexican oil industry, the result was thoroughly agreeable to American bankers and oilmen. And beyond matters of high policy the list of "lapses" is depressingly long. He waffled painfully on the Sacco-Vanzetti case (according to Steel because he "cared about his influence as a public person"), he kept well "clear" of race relations, writing practically nothing about anti-semitism, supporting immigration restrictions, and even supporting the assault upon Japanese Americans on the west coast with the words "nobody's constitutional rights include the right to do business on a battlefield." And he wrote very little about Joe McCarthy.

On the other hand, Lippmann ticked off John Kennedy for failing to use normal diplomacy during the Cuban missile crisis and he was fairly early with his condemnation of the intervention in Vietnam. Perhaps only a cynic would conclude that the imperial Lippmann, even in these cases, intuited what most of his syndicated subjects really wanted to hear.

In any event, the question that Steel leaves unanswered — whether or not Lippmann really did exert an almost unprecedented influence — seems to me more important than whether or not Lippmann was "right" or "wrong" on any given issue. His books, including *A Preface to Morals*, never got quite beyond the level of penetration achieved by, say, H.G. Wells. Lippmann knew this, but never got down to the book which he promised himself he would write, and which his undoubted intellect might well have produced. Perhaps had he been able to shed the last remnants of the wanderer's sense of not quite belonging, things would have been different and one would not be tempted to the conclusion that neither the shape, nor the policies, nor the opinions of America would have been different without him.

What would now be most interesting would be publication of a healthy selection from Lippmann's huge correspondence, particularly of letters written to him, so that we might discern more clearly where power really did reside in those sixty-odd fateful years.

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David J. Rothman, *Conscience and Convenience: The Asylum and its Alternatives in Progressive America* (Boston: Little, Brown and Co. 1980).

THE DEBATE ON THE nature of the "asylum," particularly so-called social welfare institutions, has been a heated one over the past decade. The polar extremes are at present marked on the one hand by some who continue to cling at their peril to the Whig notion of progress, that institutions like hospitals, prisons, and schools have shown steady improvement over time and, while flawed, nevertheless continue to serve the needs of society as a whole. On the other hand there are are the social control advocates of various shades who strongly question the efficacy of such institutions, seeing them more as manifestations of social conflict. Michel Foucault, the most pervasive yet problematic interpreter of the historical devel-

opment of institutionalization in modern society indeed argues that these are symbolic representations of a more fundamental aspect of society, namely, the division of power made possible by both social and psychological disciplinary structures upon which all authority rests and is legitimated and perpetuated. From Foucault's point of view not only have these institutions been a failure historically but a failure almost of necessity.

One early advocate of the social control argument was David Rothman who still stands near the centre of this debate. An extension in many respects of his earlier work, *The Discovery of the Asylum*, this present work in conjunction with the latter has already been heralded by Charles Rosenburg, the noted American social historian, as "one of the most influential and widely read historical studies of the past generation." Contrastingly, Gerald Grob, a vociferous critic of Rothman's latest effort, describes it as "poor history" because he "has bent, misinterpreted, or ignored many facts in order to shape his mosaic" thus producing "a perfect example of research dictated by personal convictions." Of these, Grob might be taken somewhat less seriously for he has consistently tried to dismiss the social control argument. In fact Grob has often fallen victim to his own set of ideological blinkers which he refuses to acknowledge and which themselves do not always readily correspond to historical "fact." Regardless, what is clear is that Rothman has once more challengingly stirred the historical pot pourri.

The Jacksonian Era resulted in a significant transformation in the type and use of the asylum. So too did the years 1900 - 20. According to Rothman, "the Progressive Era marked a major divide in attitudes and practices toward the deviant, creating new ideas and procedures to combat crime, delinquency, and mental illness." (43) Following an overview of the Jacksonian legacy, he examines innovations in criminal justice, juvenile justice, and mental health which transpired early in this century. These include the widespread use of probation, parole, the juvenile court system, social workers, psychometric testing, training schools, and preventative mental health care in the United States.

Each of the above shared the same fundamental progressive design. Reformers almost unanimously denounced the blatant shortcomings of the asylum as a whole and its failure to reform and rehabilitate inmates. To this extent they were anti-institutional in their attitudes. Instead of repudiating institutionalization, however, reformers proposed to make asylums more efficient and extend social service precepts beyond their confines. This was done through the probation system, the teaching profession, and the Mental Hygiene movement among others. Simultaneously, better programs for inmates, the professionalization of staff, increased funding, and the rationalization of existing institutions were endorsed.

Their purpose was to address the problems of deviants more efficiently and directly. Hitherto inmates had been treated as a class with little regard for individual needs which has greatly contributed to the asylum's previous failure. Here Rothman notes a significant departure. Now deviancy increasingly acquired connotations of sickness of the individual thus combining environmental with biological concepts.

Despite these efforts and new views well-intentioned, progressive reform floundered. The asylums of this era which inherited these changes were little better in 1950 than in the previous century. Why? Herein lies Rothman's main thesis. He states that there always existed an ongoing tension between conscience and convenience. Obvious institutional failure gave rise to humanitarian concern. The latter, however, was usually overridden by larger concerns of economy and social control. Political and judicial decision-makers, further influenced by individual

and public attitudes and a general ignorance of these institutions and their inmates, greatly impeded any chance at asylum reform or any real analysis of their utility. The victim of chronic underfunding and poorly-trained staff, the institutions were left unsuccessfully to straddle theory and praxis. The final section of Rothman's book illustrates this well. Based on a diary kept by prison administrators at a penitentiary in Norfolk, Mass. from 1932 - 33, he shows convincingly how the dreams of reformers turned into nightmares. Moreover, no alternative to the asylum system was forthcoming because it remained the most convenient mode of social control.

While a solidly researched, insightful, and even provocative work, this book is not without its shortcomings. As in *The Discovery of the Asylum,* Rothman once again tends to lump all progressive reformers together without giving much information as to class origins, education, or motivation. Nor do we learn very much about inmate populations let alone the socio-economic developments which may have spurred attitudinal changes. Reformers and inmates alike are too objectified in this analysis. A lingering question is, whether this information would substantially alter Rothman's main thesis.

The intellectual context is also largely ignored by him. Rothman, for example, fails to discuss the influence of social darwinist thought, more precisely neo-Lemarckianism. Judging by contemporary medical journals, these ideas helped develop social attitudes on public health and mental hygiene, particularly as they related to deviancy, thus legitimating many social welfare practices. While repeatedly noting the influence of medicine and psychiatry in the Progressive Era, Rothman makes little effort to explore how recent findings in neurology and eugenics might have affected public responses to the asylum.

Moreover, one cannot limit these reforms to the asylum alone. They are indicative of a change in *mentalité* that was expressed in several ways through the public schools and women's organizations as well as traditional social welfare institutions. Of interest in this regard to the labour historian is the attempt by mental hygienists after World War II to attribute growing working-class militancy to psychological distress. This contributed to the emergence of industrial psychology which was meant to help minimize the ills of the workplace and diffuse discord. The asylum was but a major participant in this general thrust, a point which Rothman ignores. Lastly, Rothman makes virtually no use of recent secondary literature in the field, such studies as Richard Fox on California asylums and John Conley on prisons in the Progressive Era. This unnecessarily diminishes the book's usefulness.

In sum, *Conscience and Convenience* is a seminal work despite these flaws. It is applicable to the Canadian as well as American scene for many of the Progressive ideals filtered north. For this reason alone, Rothman deserves a careful read by Canadian historians in the field. Perhaps, more importantly this book adds not only to our understanding of the past but the present too — a considerable accomplishment.

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Margaret Brenman-Gibson, *Clifford Odets: American Playwright, The Years from 1906-1940* (New York: Atheneum 1981).

EVEN BEFORE BEGINNING TO READ *Clifford Odets,* I was confronted by two daunting elements. The first was the blurb from the publisher, which includes extravagant praise from many of the major figures of the American stage, Arthur Miller, Harold Clurman, Anne Bancroft, Lee Strasberg, Elia Kazan, and a number of others. One effect of such comments is

to set the book up for a fall. Can any book be that good?

The second factor was the book's physical size. Although it covers only the first 34 years of Odet's life, it takes 750 large pages of close type to do it. Six hundred and thirteen pages of biography, with normal footnotes, are followed by a variety of indices and other reference material which includes 57 pages of special notes and comments. Thus my expectations of quality created by the blurb were followed by expectations of quantity. In all those pages should nothing be left out.

And in certain areas this is the case. Our main interest in Odets today is as a symbol of the 1930s. He was an actual member of the Communist party for only a very brief period but he was a consistent member of that vague fraternity called "the left." His most famous play, the one-act, "Waiting for Lefty," was an immediate hit in 1935 and went on to be played in union halls, at political benefits, and at any theatre associated with progressive causes. (This extended throughout the world. In Canada it had so much popularity that it became a constant feature of the Dominion Drama Festival, hardly a radical organization.)

By the end of his life, however, Odets was in Hollywood, working for television. This completed many years of a love-hate relationship with the movies, the result of which was some good screenplays but also a number which have done little for Odets' reputation. I'm sure that many who revere Odets for "Lefty" and for his other plays of that period, such as *Awake and Sing*, would not be happy to learn that his last screenplay produced was *Wild in the Country*, starring Elvis Presley.

Brenman-Gibson provides detail after detail of the life of a man who could write both. Her training is as a psychoanalyst but her special study has been the psychology of drama and the dramatist. Also, through her husband, William Gibson, a student of Odets, she has become a close friend of her subject. Thus she possesses the psychological training, knowledge of the creative process, and had personal contact with her subject. The preface to the biography, however, shows that her primary concern is defined by the first. She states that she had been disappointed with psychoanalytical biographies in their "Freudian-fundamentalist approach." Mainstream literary biographies seemed less procrustean and limiting but far more bland and limited. In her work on Odets she hoped to combine the two and to create something which would be neither.

She certainly has not been constrained by any concern for brevity. She often quotes at great length from diaries or letters. Where someone else would be satisfied with a sentence she goes on for pages. The result is to reveal nuances of character which a shorter account might miss, but this also leads to a substantial amount of repetition. Not that Brenman-Gibson is afraid of this either. She has an irritating habit of saying, "That evening in his diary Odets spoke of 'X'." Then in a long quotation she includes the passage. Then a few pages later she recalls how Odets said "X". One begins to feel that either she has no memory or she thinks her readers do not.

Still, the analytic depths she reaches are amazing. I think every biographer should read this book before writing his/her own, no matter who is the subject, although I certainly would not claim that her detailed psychoanalysis of her character would suit everyone. Like many of her calling, she seems to be obsessed with sexual innuendo and with relationships between parents and children.

But for Odets this seems unusually appropriate. It is rare for a biographer to find reams of autobiographical notes left by a subject. And in Odets' case they often seem to fit perfectly into rather stereotypical Freudian notions. For example, in his diaries and notes Odets always recorded instances of masturbation with a black-circled capital letter M. He was

constantly concerned with the size of his penis.

Yet he saved his major worries for his father. There can have been few individuals who had such perfect examples of the long-suffering and early-dying mother and the authoritarian, insufferable, seemingly immortal, and yet essentially incompetent father. From young middle-age, at the onset of Clifford's success, his father depended on his son to finance a series of ridiculous business ventures and an often absurd lifestyle. He was still alive long after his son's death and able to tell Brenman-Gibson that his son and the whole family would have been nothing without him. And he, of course, could have been much more without them.

Thus it seems suitable when Brenman-Gibson provides interpretations of Odets' various works which search for oppressive father figures and destroyed mothers, and young men who reflect Odets' felt contortions between a sadistic machismo and a submission, even masochism. In the special "notes and comments," in which Brenman-Gibson allows her psychoanalytical insights full rein, there are some particularly fascinating suggestions about how these concerns shaped Odets' creativity.

But the great moment of "Waiting for Lefty" is when the union realizes that rather than waiting for Lefty, the great leader, who is now dead, they must "Strike! Strike! Strike!" The success of the play partly reflects the psyche of the writer but still more the psyche of the contemporary theatre and of the age, in a recognition that mass action must supersede devotion to an individual hero.

The blurbs purport that Brenman-Gibson captures this, but I see no suggestion that this is the case. Her references to the larger theatrical context are limited. With the exception of other figures associated with Odets in the Theater Guild or the Group Theatre, she considers only Eugene O'Neill. A few others are mentioned, but there is little suggestion that something larger is happening out there, particularly in Europe. Even the Group's various attempts at penetrating the Russian connection are not elucidated except superficially as politics.

And that is all the politics are throughout the work, superficial. It has always surprised me that there seems such a constant link between psychoanalysis and the left. A number of important figures in the one have flirted with the other and some have attempted to reconcile the two. But it is more than socialist rhetoric to note that whereas there is an obvious social component to psychoanalysis, the tendency is for analysis to descend to an obsession with bourgeois individualism, a tendency which leads to the anti-social introversion in literature which has been labelled psychologism.

And that seems to be what we have here. There can have been few biographers who elucidate a character the way Brenman-Gibson has. And she does it without restraint. She has no fear of offending Odets' descendants or his memoir. She recounts everything, no matter how sordid it might seem. She is similarly free in her analysis. Highly intelligent and perceptive, she never lets a moment of humility prevent her from suggesting an interpretation in which she believes, no matter how unusual. The only possible complaint might be that she can be frightening in her scientific certainty about the vagaries of the human mind she is examining.

But for a book of this length there is a very large gap. A similar figure in a different age would have written different plays, no matter how similar his relationship with his mother and father. Odets himself would have been a very different writer had the optimism of the radical thinkers of the 1930s been met by something other than the cynicism of World War II. Brenman-Gibson, with her often condescending and always superficial accounts of socialist movements, would not be able to see what this means. I per-

sonally can posit a situation in which Odets' masturbation and troubles with his father may have continued but in which the promise of "Lefty" would have ended with something other than Elvis Presley.

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Karin Becker Ohrn, *Dorothea Lange and the Documentary Tradition* (Baton Rouge: Louisiana State University Press 1980).

DOROTHEA LANGE'S BEST-KNOWN photograph shows a seated woman looking out of careworn eyes into the dim and worrisome prospects offered by the middle distance. On each side of her, with faces hidden in her shoulders from the camera, is a child, and, on her lap, a sleeping infant. They are all in rags. The mother has the thin drawn gaunt look of one who has had too many years of not quite enough beans and fatback. The text informs us that she is 31.

The lines of this photograph pull one into it immediately: the woman's right elbow rests on her knee at the bottom of the frame slightly left of centre; from there her straight-boned forearm leads one's eye up past a prominent collar-bone and Adam's apple by way of a long thin heart-breakingly feminine hand to the centre of the composition: the face of a suffering mother. Her fingertips ever so lightly touch the corner of her long full turned-down mouth, beneath which three moles disfigure lower lip and chin. Two long deep creases ascend from this unsmiling mouth to the wings of a thin straight nose. (Had she been pug-nosed would this photograph have its present power?) The eyes underneath broad black eyebrows are deep-set and in a perpetual squint, as if against too strong and persistent and hostile a sun. High cheekbones, wide brow — in a frown, again perpetual — lacklustre black hair, prominent ears complete the picture of one whose face has become an icon of the Depression, the madonna of the Dust Bowl. Some suggest she looks like an American Indian, some like Abraham Lincoln. The lesson is the same: hard times, stoicism, history as betrayal.

What gives this photograph its special status in American art? Properly to discuss this question would require our comparing it with Grant Wood's "American Gothic," John Steinbeck's *The Grapes of Wrath* and John Ford's movie of the novel, Walker Evans' photography and James Agee's text for *Let Us Now Praise Famous Men*, even, perhaps, Woody Guthrie's songs. One would have to consider the enduring strength of Jeffersonian ideals: the photos of the migration (which Lange and Paul Schuster Taylor called an "American Exodus" in their 1939 book on the Dust Bowl) of the 1930s out of the plains to the West Coast arose from its impact on *family* farms. It was not seen in this art as a mass movement, or as the displacement of a labour force, or as an enclosure movement maiming or destroying villages. Rather the artists concentrated on the Ma Joads. Many of the photographs which Lange and others took for the Farm Security Administration centred on the family around the stove or on the bed in the tarpaper shacks whence the migrants came or in the tents and Hoovervilles where they landed.

But no such discussion is to be found in Ohrn's book on Dorothea Lange. Instead she has written a rather sketchy biography drawing deeply on the memories of Lange's contemporaries — her husband Taylor and many of her friends are still alive — and following their lead in uncritically applauding Lange and assessing her work as the natural outpouring of a great talent and a wonderful human being. I do not deny that she was those things. It is simply that I miss historical analysis and aesthetic valuation. Ohrn does not give the reader confidence that she has looked very hard at Lange's pictures. During the 1930s Lange photographed migrant farm workers; in World

War II, Japanese in American concentration camps and workers in cities; after the War, she turned, in a manner confirming Betty Friedan, to her family, then to the Family of Man in the manner of Edward Steichen. In her last years she began, as she turned increasingly toward the homely, the everyday, and the immediately surrounding, to talk a kind of California-ese:

> The family cabin, north of San Francisco represents something to me that is . . . elemental. The cabin is a protection from the wind and weather, one board thick. And it shakes when the ocean is high and beats on the rocks. The cabin shakes and you feel every wave. And the children respond to that. They get elemental, too. (198)

Ohrn echoes this note, speaking of Lange's desire to "experience her subjects' experience . . . on a personal level," of Lange's "personal involvement" in her work and her "intense feelings about the power of visual imagery." (233) There is much talk about "relationships." But there is no attempt to explain the shifts and changes in sensibility which these hackneyed terms point to.

Is it my imagination, or are standards of scholarly publishing and editing slipping? Here is a handsomely bound and printed book which reads like a dissertation (which it was in an earlier version) whose supervisor took a sabbatical that year. The narrative flows not: tell me from reading pp. 1-3 where Lange lived during her girlhood in New York. The prose suffers from illogicalities, infelicities, and solecisms. On page 89 the word "situation" occurs three times, to mean successively "circumstances," "plight," and "scene." On the same page workers receive "below-subsistence wages" without, apparently, dying, and we encounter this pearl: "The pear growers provided no housing and paid four cents a box for the fruit they picked." Later on p. 166, Lange publishes a book posthumously. These things matter, because they divert the reader's attention away from the subject. It would be as though Lange were to print a picture of a fruit-picker out of focus.

The deficiencies of this book do not stop with points of style. The photographs have been very badly reproduced. "Migrant Mother" is so dark as to be nearly unintelligible. To see what it is supposed to look like I would suggest turning to Beaumont Newhall's *History of Photography*, where it has been reprinted sharply, with all the lines of that damaged life visible in the face as Lange intended.

Ohrn's argument also flattens contours. She has a tendency to take her informants' word for everything; she is silent about the problematic character of photography as an art; and she almost resolutely avoids questions about Lange's thinking in relation to American Left-liberal politics in an intense time, from the 1920s to the 1960s. Let me illustrate this harsh view by citing the book's treatment of its secondary theme, "the documentary tradition." What is this? Here is an early remark:

> Once photography began to be accepted as an art form in its own right, more photographers felt free to explore the unique ability of their medium to combine aesthetic considerations with a true-to-life record of the world around them. It was this combination, if not the Photo-Secession movement per se, that created a seed-bed for the development of what was to be known two decades later as documentary photography. (6-7)

Photographers, that is, took pretty pictures of what there is. (Let us leave aside the question of that "unique ability": consider Dürer, Ingres, Roman portrait busts, the marvelous cast leopards in the Chinese exhibition that toured North America in 1978.) Later on, in a chapter called "Documentary Expression in the Thirties" (26ff), Ohrn invokes the following as feeding into the "documentary": mural paintings for banks and courthouses, plays by the Federal Theater Project, folklore collecting, *Life* magazine, *Middletown*, university programmes in American Studies, Pare Lorentz and com-

pany, radio sopa operas, journalists such as Edmund Wilson and John Spivak (who "used various techniques of three-dimensional reporting to vivdly describe events" — whatever that means), and, of course, our photographers — Lange, Abbot, Riis, Genthe, Hine, Evans, Bourke-White, many others. The upshot? A "widespread movement to integrate art, literature, journalism and social science into documentary forms having social significance as their primary purpose." (35) We are not told what "social significance" is; surely there was a whole bouquet of different social significances. What it seems to have come down to in Lange's case was that the Depression, the Dust Bowl, and the creation, out of an idealized American Yeomanry, of the "okie" as type and symbol, were not supposed to have happened in the U.S.A.

As Lange turned away from the political to the domestic and the universal, one might think that her place in the "documentary tradition" would have changed. But no, she's still there. Ohrn sums up:

> Because of the juxtaposition of her own use of the camera as a tool to document relationships and the growth of the documentary approach, her work was characterized by 'that quest for understanding, that burning desire to help people to know one another's problems, that drive for defining in pictures the truth' which is at the heart of the documentary tradition. (233. Ohrn here quotes from Beaumont Newhall's preface to Lange's *American Country Women*.)

From start to finish, then, if Lange took it, it was a document. The "documentary," in Ohrn's hands, is not a very illuminating or precise category.

I miss a sense of reality as artifact. Even photographers construct their worlds by focussing, framing, choosing to emphasize foreground or background, isolating details, and printing in various degrees of contrast. Ohrn knows this (see for example her comparisons of Lange's photographs with those of Ansel Adams, 122ff.), but fails to draw out its implications. If photographers "make" pictures rather than "take" them, as Lange said (see 233), then the principles governing their construction should, in a work like this, be drawn out fully. To classify Lange's (or anyone's) photographs as documents in a "documentary tradition" which believes itself merely to be opening its eyes to the simple truth, will not do anymore. Ever since it got round that Robert Flaherty taught the Aran Islanders how to catch a shark for his "documentary" *Man of Aran* viewers of this kind of art have been aware, if not suspicious, of the ambiguities of that "tradition." Photographs and photographers *idealize;* one wants to know the sources, for a photographer like Lange, of the idealizations, even if she did not state them, or know them, herself.

Here are some questions then for Ohrn to think about should she continue to study Lange's work, as I hope she does. First, whence the strong triangularity of Lange's compositions? So many of her pictures, including "Migrant Mother," arrest the eye at the centre of the frame with an element that forms the apex of a triangle the base of which is the bottom edge of the picture. Are we seeing the traditional pyramid of vision of Renaissance painting? Did Lange look at painting and if so what did she admire? Secondly, did she ever consider working in colour and if not why not? Thirdly, what was her attitude toward authority, especially male authority? Consider the photo at p. 126 and the one (also reproduced on the book's dust jacket) at p. 39, the first showing a soldier guarding the Japanese on their way to the camps in 1942, the second showing a policeman keeping order during the general strike in San Francisco in 1934. The two figures appear a head taller than those in the crowd behind them; both are cool, aloof and self-assured; both look possessed of legitimate command. Or is there an irony here? Ohrn tells us that Lange considered the sequestering of the Japanese unjust, but says nothing about the strike in San

Francisco. Fourthly, to what extent can Lange's way of seeing be assimilated to Leo Marx's argument in *The Machine in the Garden*, the railroad in that book being replaced in Lange's lenses by tractors and automobiles? The okies themselves spoke of being "tractored out," that is, driven from their farms by a more efficient agriculture they could not raise the capital to compete with (see Lange and Taylor's *An American Exodus*). Leo Marx's discussion of the "Middle Landscape," a clouded pastoral vision of small family farms forever threatened by big technology, could perhaps light the way into the controlling ideas behind Lange's camera. Fifthly, an observation I owe to Dr. Rosemary Ommer, why are there no faces in Lange's American photographs after about 1956? She shot portraits all her life, and continued to do so around the world in her late work, but in her work done at home in her last decade the human face seems to have hidden itself away. Or is this a function of Ohrn's selection? Lastly, although Ohrn is at pains to portray Lange as she wished to be portrayed, as an artist mistrustful of Art, as a photographer for whom technique counted little, and as an unselfconscious maker of pictures framed to get you to look at the world rather than at photography, she includes a late picture (1950, 207) blown up from a snap of Pakistani children, showing the upper part of one child's face, just nose, brow, and very round wide eyes. In the middle of each staring black pupil you can see, if you look closely, a tiny reflection of Dorothea Lange's head and hands and camera. Is not the whole problem of photography, as the twentieth century has come to define it, present in those faint mirrorings?

One final remark. Ohrn's book, for all its shortcomings, is a wonderfully affectionate treatment of a subject difficult I think not to warm to. One would like to have known Dorothea Lange, the creator of some of the most authoritative and enduring photographs in the history of the art. Her kindness and humanity shine through these pages as through her photographs. I do not think Karin Becker Ohrn has said the last word about Lange, but in her sympathy and restraint (see for example her gentle treatment of the unlikeable Ansel Adams) she is emotionally worthy of her subject.

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Elyce J. Rotella, *From Home to Office: U.S. Women at Work, 1870-1930* (Ann Arbor: UMI Research Press 1981).

OF ALL THE TOPICS WITHIN THE AREA of women's studies, perhaps none has received as much scholarly attention as the changing economic role of women. Two of the most remarkable labour force trends during the last century have been the rising participation rates of women, and their concentration into a restricted range of occupations. These trends have registered their greatest impact on clerical occupations. Once a male domain, the clerical sector now employs more women than any other major job category. *From Home to Office* provides a systematic, statistically-grounded analysis of these changes in female work patterns between 1870 and 1930 in the United States. What distinguishes this book from other recent contributions to women's history is its approach. Rotella presents a "new economic history" of the formation of America's modern female labour market, using econometric techniques to test economic theories of the supply and demand for female labour. Her analysis and conclusions deserve the attention of scholars interested in changing the nature of women's work.

The first third of the book outlines the basic trends in female work patterns during the six decades around the turn of the century. This sets the stage for an in-depth look at clerical occupations in the remaining chapters. Good quality time-series data on female employment and earnings

for the total labour force, a sample of urban centres, and the state of Ohio form the basis for the analysis. Rotella begins by documenting the rapid growth of the female labour force. A largely urban phenomenon, this growth resulted in the concentration of female workers in a small number of "women's jobs." Rotella then attempts to identify the factors which influenced the female labour supply in a sample of major cities in the years 1890 and 1930. Regression analyses provide an adequate test of the orthodox "household choice model" of female labour force participation. The rapid influx of single women into paying jobs signals for Rotella a breakdown in the traditional institution of the "family economy." Indeed, by 1930 young, single women were exercising considerable independence regarding work decisions. In ever increasing numbers they moved from home to office.

At this point Rotella narrows her focus to clerical occupations. The clerical sector was at the very centre of transformations in women's work roles. In fact, clerical jobs absorbed almost one-quarter of all additional female non-agricultural workers in the United States between 1870 and 1930. This new army of clerks tended to be younger and more likely native born, single, and better educated than other employed females. This helped redefine the image of the working woman. But what is even more noteworthy about clerical work is that unlike other female job ghettos, it is the rare instance of women actually "taking over" an established male occupation.

This poses a major question for Rotella: which supply and demand factors accounted for the "pace and pattern" of growth in female clerical employment? Her statistical analysis yields no conclusive answer. This is not surprising, however, considering the complex interaction among numerous factors on both sides of the supply-demand equation. What the data do seem to indicate is that clerical *growth* was related to the long-term increase in female labour force participation, while the *timing* of the growth reflected short-term fluctuations in demand.

But even more crucial to explaining the contemporary nature of clerical work is the question of why these jobs had become "feminized" by 1930. Rotella attempts to provide the first comprehensive answer to this little-researched question. Several standard explanations, drawn from labour economics, are rejected in favour of a new interpretation based on human capital theory. According to Rotella, the most powerful factor influencing the demand for female clerical labour was mechanization, especially the wide adoption of the typewriter between the 1880s and 1910. Why, though, did employers not continue to recruit men into the new tasks created in the wake of advancing office mechanization and rationalization? Because the male-female differential in clerical salaries remained fairly constant, argues Rotella, the major inducement for employers to shift to female labour was the relative productivity gains resulting from mechanization.

Certainly the convergence between the diffusion of new office technology and spurts in female clerical recruitment lend credence to this hypothesis. But Rotella goes further, resting her case on Gary Becker's distinction between firm-specific and firm-general skills. Essentially this refers to the deskilling of clerical tasks through new technologies and work rationalization measures. As a result, it was no longer necessary for workers to develop skills geared to a specific firm's requirements. Rather, more basic general skills acquired through public educational institutions tended to boost worker productivity across all firms. It was largely these technology-induced productivity gains — complemented by lower female salaries and employer stereotypes regarding women's suitability for menial

office tasks — which underlay clerical feminization.

The study ends by comparing the determinants of women's participation in the clerical labour market in a sample of cities for the years 1890 and 1930. The human capital model, despite the insights it furnished regarding the role of mechanization, is unable to account for why women took up office employment at these two points in time. A major tenet of the model is that workers seek to maximize the return on their "human capital," such as education and previous work experience. Yet, surprisingly, the relatively higher salaries which women could earn in clerical jobs did not figure prominently in their work decisions. Women's educational levels and the local availability of jobs were more important in this respect.

This new economic history of United States women's transition from home to office presents a welter of quantitative evidence to buttress its arguments. The data contained in more than 70 tables and figures are actually quite accessible, mainly because Rotella concisely summarizes their highlights in the text. Scholars with a more statistical bent will undoubtedly find the time-series data on female employment and earnings a useful resource. At the same time, a number of problems are evident in the type of data utilized, the analytic techniques applied to them, and the theories underpinning this analysis.

Certainly Rotella has provided us with what is probably the most thorough quantitative picture of the major supply and demand factors which shaped the late nineteenth- to early twentieth-century female labour force, especially in the clerical sector. But there are other influential factors, mentioned briefly by Rotella, which are not included in the econometric models. For example, this type of economic analysis cannot easily incorporate societal sex role stereotypes or employer preferences for female employees. Similarly, Rotella argues that mechanization and the broader process of work rationalization through scientific management and other strategies to reorganize production best account for clerical feminization. Yet this connection between mechanization and rationalization within the office requires supporting evidence. Furthermore, neither variable is adequately measured to determine its relative contribution to the process of clerical feminization. On a theoretical level, Rotella confines herself to main-stream labour economics. But considering the difficulties human capital theory faces in explaining the growth and changing characteristics of the clerical labour force, the thoughtful reader is forced to consider alternatives. One useful alternative to human capital theory and other models of supply and demand is the labour market segmentation approach. Indeed, Rotella's documentation of how a female job ghetto emerged in the office easily lends itself to a segmentation perspective. And her emphasis on deskilling can find strong corroboration in recent neo-marxist research on the labour process. These theoretical implications, although unexplored by Rotella, offer fertile ground for future discussion and research.

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Graham D. Taylor, *The New Deal and American Tribalism: The Administration of the Indian Reorganization Act, 1934-45* (Lincoln: University of Nebraska Press 1980).

IN 1887, THE U.S. CONGRESS passed the General Allotment Act (popularly known as the Dawes Act). In brief, the Dawes Act broke up tribal lands into 160 acre portions, turned them over to Indian allottees, and sold surplus lands to the government. This legislation was accompanied by a Bureau of Indian Affairs (BIA) policy of educating Indians for life in the larger society — in practice this meant education

designed to bring about "the disintegration of the tribes." From the vantage point of the late twentieth century it would be ahistorical to condemn these measures as cynical attempts to defraud Indians of their land and to destroy native cultures. Many nineteenth-century friends of the Indian regarded assimilation as the only alternative to extinction — an understandable position given the frightening rate of Indian poulation decline during that period.

Of course, these programmes did not work. Indians who had no tradition of farming often found it expedient to sell their allotments for needed cash. In addition, much of the allotted land was in arid regions of the west where there was little possibility that small, 160 acre farms (white or Indian) could survive. The primary result of the Dawes Act was the loss of almost two-thirds of Indian lands. The attempt to turn Indians into white men through education was a similar, and related, failure. Sending Indians to boarding schools prepared them for little but a life of dissatisfaction and despair. Without a viable land base, Indian peoples in the early twentieth century fell prey to increasing poverty, disease, and alcoholism.

Graham D. Taylor's book analyzes the efforts of the Roosevelt administration to correct the inequities of the Dawes Act. The central figure in his account is John Collier, head of the BIA from 1933 to 1945. Collier was a reformer whose main instrument as a New Deal bureaucrat was the Indian Reorganization Act (IRA) of 1934. That act, although somewhat modified by Congress, was largely Collier's work. Its major provisions repealed the Dawes Act, set aside $2 million for the purchase of additional lands, created a $10 million credit fund for loans to tribal corporations, and provided for the creation of tribal governments on Indian reservations. Within two years, Indians were required to vote on the question of inclusion under the act. Those who voted in the negative were to remain directly under the supervision of the BIA, while those who accepted the act were to frame constitutions and create tribal councils with wide powers of self-government. The tribes were also encouraged to set up tribal corporations with the authority to manage their own resources and to buy back previously allotted land. (In practice the BIA sharply limited the independence of both the tribal councils and corporations.)

This ambitious scheme was founded primarily upon Collier's belief that if the tribal governments which once shaped Indian lives were resurrected, Indians could build a new way of life based upon the foundations of the old. The IRA was well meant, but as Taylor explains it rested on a fundamental misunderstanding of Indian culture. The majority of Indians to whom the IRA applied had never had a tribal government in the sense imagined by Collier and his associates. For most of these Indians, primary allegiance lay, not to their linguistic group, but to their village or band. In most cases, as Taylor points out, tribes were "basically artificial units of Indian political and social life."

The implementation of the IRA exacerbated old divisions and created new ones. Tribal councils and corporations were frequently dominated by mixed-blood or "progressive" Indians who had the appropriate political skills. Indeed, Collier and his colleagues seemed blind to the fact that the whole process of holding referenda, writing constitutions, and electing delegates was completely alien to many Indians. Ultimately, the majority of these tribal governments could not command the full support of their own people. That lack of support also meant that the economic goals of the IRA could not be met. The essential difficulty here was that the BIA could not persuade Indians to subordinate local interests and loyalties in order to create land units large enough to be economically successful.

Although Congressional hostility and the onset of war played a part, Taylor is

right in assigning primary responsibility for the failure of the IRA to Collier and his staff. Although the BIA was less paternalistic under Collier's stewardship than formerly, its supervision still tended to limit Indian initiative. At bottom, however, the IRA failed because its framers were ignorant of Indian culture. Although aboriginal political systems were not considered especially interesting by American anthropologists of the 1920s and 1930s, there were those who could have provided the needed advice — not the least of which were the Indians themselves. Indeed, despite Collier's genuine concern for Indians, his failure to consult them before the framing of the IRA is especially striking. Taylor does point out, however, that in this matter Collier and other reformers felt pressed for time; none of them in 1934 could predict how long the Roosevelt administration would last.

In this particular affair, and indeed, throughout the book, Taylor's sense of fairness is evident. This is no easy condemnation of naive do-gooders, but a sensitive treatment of an extraordinarily complex problem. Thoroughly based on a variety of government and personal collections, this work is a model of scholarship. The book also implicitly suggests the need for a subsequent intellectual history which would explain how white America came to have its erroneous ideas about the nature of Indian governments. Perhaps Professor Taylor is the scholar for this task; he has done an excellent job of explaining the consequences of those ideas.

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Edwin R. Bayley, *Joe McCarthy and the Press* (Madison: University of Wisconsin Press 1981).

EDWIN BAYLEY WAS A YOUNG political reporter for the *Milwaukee Journal* during the heyday of Wisconsin's Senator Joe McCarthy. Now the dean of the Graduate School of Journalism at Berkeley, Bayley recalls those days as "the most dismal and most exciting" of his life. Because he was troubled by the frequent charge that the press "made" McCarthy, Bayley began an extraordinarily intensive search through 129 newspapers to discover what exactly the press had done with McCarthy's anti-Communist crusade. The result is a remarkably good book which not only illuminates the complex and disturbing relationship which existed between the press and McCarthy but also illuminates to historians the continuing usefulness of long hours in newspaper morgues.

Bayley's book presents a view of McCarthy which undermines the argument that he was a simple but malevolent populist who found a cause that won him acclaim that his skills could never have acquired. In fact, McCarthy built cleverly upon the disintegrating legacy of Lafollette progressivism, including the strong midwestern isolationist streak. For McCarthy the real enemy was not the Soviet Union but rather the "traitors" within. He manipulated midwestern fears of Washington, of the establishment, and even of the military. In his early years he did so with an unerring sense of the moment. But however much McCarthy drew on Wisconsin's political traditions and on the anti-Communist paranoia of post-war America, he was in many ways unique, and this uniqueness explains much of the problem the press faced in dealing with McCarthy.

Bayley looks at several specific questions in his evaluation of McCarthy and the press: the reaction to the February 1950 speech in which McCarthy made his charge that there were 205 Communists in the State Department; the wire service treatment of McCarthy's career; the role of the Wisconsin press in the 1952 Senatorial election campaign; the personal relationship of McCarthy with reporters and publishers; and television's handling of McCarthy and McCarthyism. In Bayley's

view, the wire services and television come off worst in his examination. The wire services fretted about stories hostile to McCarthy because they feared offending conservative editors. McCarthy's outrageous charges passed over the wires quickly. Their substance was rarely examined, and the denials came too late. Associated Press above all, was "playing the consensus game." It could "move no faster or further than the middle group of newspaper clients."

Television has been given much credit for destroying McCarthy. The credit is unwarranted, however. Edward R. Murrow's *See It Now* broadcast seems almost defensive in retrospect. It also came late. By 1954 when Murrow spoke out against McCarthy, the Senator was a much easier target than he had been two years earlier. At that time the television networks were excessively cautious, because they feared government regulation, because blacklisting had already affected so many in the media, and because television did not enjoy First Amendment protection. Still, Bayley's account does bear out television's influence in those early days. The Army-McCarthy hearings punctured McCarthy's bombast. "Have you no sense of decency, sir?" was heard by millions and apparently believed. Most of those who heard it no longer believed in Joe McCarthy.

Bayley's study of the 1952 election also proves that where the anti-McCarthy papers were read — and the *Chicago Tribune* was not — McCarthy did poorly. Bayley concludes that the press did matter, but during McCarthy's rise the press failed to carry out its basic duty which is "to furnish the information." One of the legacies of the sorry McCarthy age was the move to interpretive reporting. It has become the obligation of a reporter to tell what is true.

Joe McCarthy and the Press is a fine book written with great clarity and admirable subtlety. It meets Ed Bayley's standards: It not only furnishes information; it tells what is true.

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E. Digby Baltzell, *Puritan Boston and Quaker Philadelphia: Two Protestant Ethics and the Spirit of Class Authority and Leadership* (New York: The Free Press 1979).

THIS THIRD BOOK by sociologist E. Digby Baltzell on America's upper class is at once a powerful, compelling, frustrating, and even somewhat frightening portrayal. Its power derives from his cogent and felicitous writing style, along with the intriguing patterns of evidence he marshalls for his case. Yet, this same evidence is frustratingly circular and incomplete on several levels, and Baltzell's own values and political ideology will probably disturb any reader with even a passing faith in participatory democracy or an egalitarian society. For Baltzell, a member of Philadelphia's upper class, is an unabashed elitist who has few kind words for what he calls "antinomianism" and "anti-authoritarism" in America.

Baltzell's essential thesis is that despite surface similarities, Boston and Philadelphia have produced two very different upper class lifestyles and leadership patterns. Whereas the Philadelphia upper class has traditionally been highly privatized, pursuing the solitary quest for wealth and eschewing a leadership role in politics, society and the arts, Boston's First Families have produced generations of men and women who have made major contributions in these areas. He traces the root cause of these differences to the concepts of class authority in the two cities, which in turn flow directly (but not solely) from their differing religious traditions. Whereas Philadelphia's Quakerism emphasized anti-authoritarism, perfectionism, and equality, leading to a spirit of "egalitarian individualism." Boston's

Puritan origins stressed hierarchy, authority, and social cohesion. Furthermore, the two contrasting styles of leadership in Boston and Philadelphia stand as allegories for America's past and future. If Boston's Puritan-based concepts dominated America at least down to World War I, increasingly the traditions of egalitarism and anti-authoritarism which found their fullest flower in Philadelphia's Quaker past, have come to dominate the American scene. And Baltzell does not like this, not one little bit. As he says, "it is the proper function of an upper class in any healthy society to wield authority... through the respect it commands throughout society...."

Baltzell's general analysis and arguments concerning Boston and Philadelphia, if not his value judgements, are cogent and compelling, and I suspect that in most respects he is correct about the differences between the two cities. Yet, much of the success of his argument depends upon his writing ability and erudition — his glibness — rather than upon convincing and objective evidence. To my mind, his evidence and arguments are highly selective and subjective, and much of it simply does not stand up to rigorous analysis. I am most bothered by the fact that he refuses to recognize a certain circularity in one of his main arguments, along with the way in which he selects his evidence in such a way as to either sanitize the record in Boston as opposed to Philadelphia, or in the way his case studies do not always link up in the way he suggests.

One of Baltzell's key arguments for the dominance of Boston in national affairs is in chapter three, where he uses the number of biographies in the *Dictionary of American Biography*, along with the average number of lines of listees, to show Boston's superiority. He assumes that the DAB is an "objective" barometer of historical significance, yet throughout the book he recognizes that Bostonians early gained prominence in the historical profession, that they often practiced a strong sense of "Boston chauvinism," and that the Philadelphia biographies in the DAB were poorly written and incomplete. Yet, he continues to maintain that the DAB is a wholly objective measure of historical significance. It may well be the best that we have, but I hardly think that its objectivity can be taken for granted. He runs no test for bias in this source, he does not question whether some kind of regional or ethnic bias might have determined, at least to some extent, the persons who were listed and the kind of biographies they had written about them.

Similarly, his evidence for Boston's upper classes is often "sanitized" so that their performance appears better than their cohorts in Philadelphia. One example of this is where Baltzell dwells at length on the corrupt machinations of P.A.B. Widener and W.L. Elkins in the latter city, yet dismisses the larger scale corruption of the Ames brothers in Credit Mobilier as being comparatively trivial and unimportant. Also, his analyses often do not "link-up" in the ways in which he suggests. In discussing Progressive Reform in Boston and Philadelphia, he engages in an in-depth analysis of that movement in Philadelphia, showing it to be narrowly concerned with business efficiency. Yet he never actually looks at the comparable movement in Boston, just assuming it was dominated by the First Families, and that it was of a more broadly reformist nature. Perhaps it was, but the evidence is never presented. In the same vein, religious controversies and splits among the upper classes in Philadelphia become "schisms," whereas in Boston the rise of Unitarism and Episcopalianism are treated as unitary expressions of the same religious orthodoxy and class authority. Perhaps they are, but the evidence is not convincing.

Finally, Baltzell's own beliefs in class authority and its benefits are not only disconcerting, but also emphasize the idea that what is most important is that the

upper classes lead, and the rest of us follow, without ever really examining whether the results of this are actually beneficial. What is at issue for Baltzell is not so much the results of leadership, but simply the importance of having class authority and leadership in society. This is graphically displayed by his comments about protests during the Vietnam War: "In the more recent past, when the Protestant establishment had become suspect and democratic elitism was firmly in the saddle, atrocity stories about Americans in Vietnam were the rule, and American boosterism was replaced by mistrust and a growing instinct for disparagement among more and more Americans...." To paraphrase W.C. Fields, all things considered, I'd rather be in Philadelphia.

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William Graebner, *A History of Retirement: The Meaning and Function of an American Institution, 1885-1978* (New Haven: Yale University Press 1980).

FOR SEVERAL DECADES the sociology of aging and the elderly has been adrift in a sea of data searching for an important question to ask. Now William Graebner has provided that question and has also taken us a long way towards answering it. The question which Graebner poses in *A History of Retirement* is remarkably simple: how can we account for the historically unique social category of elders which appeared with the advent of modern capitalism? His answer, as I shall argue below, makes his work of significance not only to the small world of social gerontology or the historians of "old age" but also to all those with an interest in the manner in which the evolution of modern capitalism reorganized both the structure of the labour force and the structure of the worker's life cycle.

The debate which for several decades has preoccupied sociologists and, more recently historians, can be broadly characterized as an attempt to determine the relationship between modernization and the status of the aged. In its first phase the debate was characterized by a rather simple "before" and "after" model, as Laslett has called it. "Before" the elderly had power, prestige, and resources; "after" they had little or any of these. Social historians were successfully able to demolish such a simplistic analysis but, once the bloodletting was complete, had precious little to put in its place. To do this the question had to be reformulated and Graebner has demonstrated the key to this reformulation. Very simply, until the universalization of the retirement principle — the labour practice of superannuating workers on the basis of chronological age in advance of physiological decline — "old age", in its modern sense, simply did not exist. Until well into the twentieth century old people did many things — they became sick, disabled, and unemployed, they were grandparents and widows — but, except for the wealthy, they did not *retire*, that is withdraw from productive activity in advance of physiological decline. Only in the twentieth century did such a practice become common, and indeed only since World War II has it become virtually universal. The consequence of this transformation of the economic life cycle was the creation of a new social category of retired elders still by and large fit for production but who do not take part in production. The question, then, is why did this transformation of old age in the twentieth century take place?

The proximate cause of the spread of the retirement principle was the establishment and growth of an alternative system of income provision for older workers. The central component of this system in the United States was, of course, the 1935 Social Security Act which marked the birth of the American welfare state. Far from being simply a piece of welfare legislation, however, Graebner argues that it was primarily intended as a piece of retirement legislation, a labour policy

designed to clear the market of older workers and thus "solve" the unemployment problem brought on by the Depression. This part of Graebner's argument is not novel. Labour economists have long since been aware of the labour market functions of public policy, although curiously it is this rather orthodox (but never so well documented) argument which has led some historians to label Graebner's analysis as "revisionist."

The novel component of Graebner's study is his analysis of the remote causes which led to the creation of this welfare state for the elderly. This he locates in the emergence of modern corporate capitalism which transformed the American economy at the turn of the century and the subsequent process of rationalization which it engendered in the capitalist enterprise. One side of this process, as Braverman pointed out, was the rationalization of the labour process. But an equally important component of Taylorism and the efficiency movement in general, was the growing effort to rationalize the labour force — the labour inputs to be injected into that process. The reconstitution of the labour process, as Taylor argued, required a reconstituted labour force — one selected on "scientific" principles. Only those workers most "fit" for each task were to be selected and, as Graebner's ample documentation demonstrates, the corporate rationalizers saw little room for the older worker in the new labour force which this selection process was intended to create. Graebner locates this new corporate rationality within the context of prior labour struggles for higher wages and a shortened workday as well as changing technological conditions which made the older, and slower, worker increasingly redundant.

Graebner documents the gradual process which finally led to the eventual "triumph of retirement" after World War II in a series of chapters which cover the civil service, teachers, industrial workers, the 1935 "retirement legislation" (the Social Security Act), and the post-war institutionalization of mandatory retirement as the normal conclusion of the economic life cycle. He closes with a discussion of the reconsideration of retirement in the 1970s and current efforts in the United States to deinstitutionalize retirement, once again in the name of efficiency.

As with Braverman's analysis there is much here that can be criticized both in terms of what is said and what is left unsaid. The theoretical assumptions and implications of Graebner's work are left largely implicit in the plethora of historical detail and, when teased out of the analysis, will be found by many to be questionable. The scope of the analysis — from the nineteenth century to the present — also means many more detailed case histories of particular occupations, industries, and firms are required to support, modify or reject Graebner's thesis. But like Braverman, Graebner's analysis is significant for the question it poses. Just as Braverman focused our attention on how capitalism reconstituted the labour process (*how* we work), Graebner has drawn our attention to the equally important issue of how capitalism has reconstituted the labour force (*who* works).

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E. Richard Brown, *Rockefeller Medicine Men, Medicine and Capitalism in America* (Berkeley: University of California Press 1979).

E. RICHARD BROWN SETS out at once to identify key trends in the consolidation of the modern medical profession and to offer a sharp critique of those trends as a battering ram of corporate hegemony. If the second task turns out to be more helpful as inspiration than execution, that fact does not in the end damage the overall usefulness of the book. By its absorption in some of the most important and pervasive changes in twentieth-century Ameri-

can life — the rise of the expert, professional self-rule, the cult of science and technology, the influence of private wealth through foundations on social priorities and public policy — *Rockefeller Medicine Men* not only brings the history of medicine out of its self-delimited "quiet zone" but more generally challenges our "whiggish" understanding of institutional development.

Brown neatly identifies the transitions in a profession which from 1870 onwards enjoyed the best century of any American occupational group. From "merely scattered members of the lower professional stratum," at odds with themselves over their theories and treatment of disease (which commonly ranged from ineffective to lethal), physicians by 1930 had upgraded themselves into a firmly-organized, highly educated profession, with respectable incomes for all and substantial wealth for a core of "elite practitioners." While the full flowering of the profession in terms of both income and public esteem awaited the post-World War II era, Brown shows convincingly that the structural building blocks were in place by 1920. Initial efforts symbolized by the formation of the American Medical Association (AMA) in 1847 were aimed at control of entry into the field of physicians, continuously overpopulated by nineteenth-century proprietary medical school "mills." Licensure, an earlier practice snuffed out by Jacksonian anti-monopolist, free-market fervor which was re-instituted beginning in the 1870s on a state by state basis, firmly entrenched the "regular physicians" over dissident sects like the herbalists and eclectics. Then, spurred mainly by developments in Germany, the mainline physicians (made up primarily of heroic allopathic interventionists and less interventionist homeopaths) were drawn increasingly to the results of basic medical research. Research science became the "effective ideology of professionalization" for physicians, who as late as the 1880s numbered members who preferred a good ear to the chest over a stethoscope. Roughly coincident with the regularization of physicians occurred the concentration of their workshops. The development of surgery as a specialized skill sped the construction of hospitals; indeed, "most of the hospitals now in existence were founded between 1880 and 1920," and by 1929 "seven out of ten physicians had some kind of hospital affiliation."

Research-minded "scientific medicine" triumphed in bold strokes over its more informal, rule-of-thumb predecessors. Increasingly, only graduates of those schools with research and clinical facilities could pass more stringent licensing requirements; in addition, the very entry into such schools now demanded previous liberal arts and especially basic science training. Working students who had previously attended evening classes at "sundown institutions" were now out of luck; as one elite reformer put it in 1886: "It does not pay to give a $5000 education to a $5 boy." The famous Flexner Report of 1910 further pushed basic reforms of the medical education system. Older, independent, self-financing schools were effectively closed down or subordinated to university governance, while associated hospital physicians became full-time salaried faculty. The centralized research hospital also gave birth to a proliferation of medical sub-specialties. To complete today's system it remained only to replicate across the country the model in place at Johns Hopkins and in modified form at Harvard at the beginning of the century and to find ways for masses of the American citizenry to pay for care in such a system. Physicians who had effectively manipulated the early stages of medical modernization by the mid-twentieth century were being elbowed aside by hospital and insurance establishments. Government as well, with millions of dollars in aid to the medical system beginning with the Hill-Burton Act in 1946, would help to define the priorities of the future.

Brown's most original and problematic contribution to this history is his emphasis upon the continuous corporate direction of scientific medicine. His research in the Rockefeller and Carnegie foundation libraries does indeed demonstrate the critical role played in the medical saga by the policy advisors to the biggest philanthropies in the country. From Brown's study the interwoven careers of four men especially seem to have influenced the design of the modern health care system: Frederick T. Gates, the Baptist minister who became the prime architect of the Rockefeller philanthropies; William H. Welch, eminent, German-trained bacteriologist, first dean of Johns Hopkins Medical School and chief advisor to the Rockefellers on medical projects; Simon Flexner, executive director of the Rockefeller Institute for Medical Research; and Simon's brother, Abraham Flexner, a professional educator whose influence at both the Carnegie and Rockefeller Foundations helped to reshape American medical education along the Hopkins model. Together, these individuals, through their university, research institute, and philanthropic connections to Big Money evolved a sophisticated, well-rationalized vision of medicine as the modern bulwark of social stability and progress. Applying the same principles to the medical field which enlightened capitalists of the National Civic Federation applied to industry, Gates and his friends saw that they would have to put a rein on the destructive, competitive quality of unregulated medicine in order to ensure the long-term health of the (capitalist) social system.

Here begins the problem that plagues the entire book. Aside from some juicy quotations by Gates embracing imperialism, social control, and disdain for the mob (what else should we expect from Rockefeller's lieutenant?), the main link Brown finds between "medicine" and "capitalism" is the cult of science itself. Health, as approached by modern medical science, became "an engineering job, merely the application of technology to nature," while ignoring the role of society and environment as forces affecting disease. "Disease" [not poverty or exploitation], proclaimed Gates, "is the supreme ill of human life" and "the main source of almost all other human ills." In Brown's view the hold of "such narrowly technical approaches is due to their usefulness to powerful classes and interest groups." His own evidence, however, suggests that the historical interaction of doctors, emergent health administrators, and corporate sponsors cannot be reduced to an adaptation to "the needs of the dominant class." Who, in the end, was using whom? There is little insight to be gained from Brown's comment about the career of William Welch, "If Welch had not been born a white male into a prosperous class, he would never have had the material support he needed." By such materialist yardsticks, it was surely the new health administrative elite as well as the bulk of physicians themselves who were the big winners in the rise of modern medicine; the Rockefellers, after all, had already made their money. Nor, unfortunately, can the narrow technical values reflected in medical science be laid entirely at the doorstep of the corporate moguls. Twentieth-century Progressives and even many socialists matched them catheter for catheter and tractor for tractor. Such widespread instrumentalist and compartmentalized thinking must surely have deeper social and cultural roots.

But once one has invoked the malevolent genie of corporate hegemony, almost everything that happens appears tainted from above. What is worse, exit from such a closed chamber is impossible. Even the prospect of universal access to modern medicine therefore, is hardly comforting to the author: "A national health service would not necessarily end medicine's role of legitimizing corporate capitalist society. It would, if anything, enable these ideological functions to compete less with

the need of the marketplace." Except for some nostalgic references to more holistic assumptions of the nineteenth century, there seems to be little in our health care system worth preserving. While concluding with the hope that we will "make a health care system that effectively serves the health needs of the majority classes," Brown forgets the rank and file metaphysician's counsel that "you can't beat something with nothing."

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Tony Platt and Paul Takagi, eds., *Punishment and Penal Discipline: Essays on the Prison and the Prisoners' Movement* (Berkeley: Crime and Social Justice Associates 1980).

WE CAN IDENTIFY FIVE TYPES of discourses that have arisen in association with the United States prisoners' movement. To write in terms of "discourses" may seem to be a rarefied way of avoiding argument with actual positions put forward within the movement, yet inasmuch as that movement has been deeply heterogeneous — containing a huge range of human experience with a corresponding variety of expressions — and inasmuch as it, unlike the international labour movement, lacks historically-established traditions of analysis, international centers of opinion, or active and well-used means of communication, some distancing, such as a description of "discourses" can provide, may be forgiven, at least as an introduction to this hidden and terrible subject. Most of the 16 essays reprinted in this volume originally appeared in the California journal, *Crime and Social Justice*, a journal that has done more than most in providing a means where existing "discourses" have been criticized and new ones created.

The first and most available discourse is that of social science. It finds institutional support within academic communities where increasing numbers of "criminal justice" and criminology departments are being created. It also has a secure and venerable place within the discipline of sociology. At its best it is capable of useful empirical analysis. In this volume the essays by Ivan Jankovic on the labour market, social class, and imprisonment are exemplary in their objectivity and willingness to analyze positions that are unpopular within the "criminal justice establishment." Generally, however, the discourse of social science (that now must include economics and psychiatry) has served to obscure important issues. At its worst it has been frankly apologetic and self-serving. Speiglman's analysis of "prison psychiatrists" and Pfohl's on the social scientists who "predict" violence for prison officials clearly show how the pretence at impartiality and scientific independence has been eroded as growing numbers of social scientists have become dependent upon the hierarchy of guards and "correctional personnel" not only for their "data" but for their income. Another of the characteristics of this discourse has been its ignorance of history. The origin and evolution of particular institutions and practices such as the jail, the penitentiary, "good time," and parole have been left uninvestigated, and the relationship between these and other historical movements such as war, employment, the trade union movement, racism, the centralization of the state, the composition of the working class, and the mode of production have been left unanalyzed.

A second discourse has been provided by precisely such an historical interest. The pioneers of this were Georg Rüsche and Otto Kirchheimer whose *Punishment and Social Structure* (1939) remained an unrecognized, minor classic until rediscovered by those who in the 1960s had become intellectually emaciated on the stale diet of orthodox criminology. Rüsche and Kirchheimer sought with considerable success to analyze the history of punishment by referring its main forms to

the dominant relations of production of a given period. Thus, in the history of Europe they identified several periods — medieval, renaissance, mercantilism, and industrial capitalism — and found systems of punishment (fines, mutilation, hard labour, disciplinary imprisonment) that corresponded to them. Besides a scholarly appreciation of their work, the present volume adds an interesting essay by Shank on the work of J. Thorsten Sellin's similar historical analysis of the origin of the workhouse and the house of correction. Takagi's history of the Walnut Street Jail and Herman and Julia Schwendinger's work have both gained much from the tradition started by Rüsche and Sellin. The importance of such an historical approach has become obvious as a generation of reformers and militants have been forced to succumb to the recent repressive phase within the prisons that followed the creativity of thinking and practices that darkly bloomed in the 1960s and 1970s. As the problem of change has been experienced in our own life-time, interest in historical periodization has grown. Perhaps the most important single result of these investigations, a result parallelled by those historians of crime and punishment in England and France, is that punishment can be best understood, not merely as a reaction to "crime," but much more broadly, as part of a larger system of repression whose continuum might include the factory, the school, the barracks, and the hospital. The most intoxicating example of this approach was provided in 1977 with the publication of Michel Foucault's *Discipline and Punish: The Birth of the Prison*.

A third discourse has arisen since the 1960s, the discourse of Marxism. While this has its affinities with the second discourse (Rüsche and Kirchheimer were both associated with the "Frankfurt School" and Foucault's own relationship to Marxism has by no means been hostile) there are some important differences. The main one of these is that Marxism provides a tradition of revolutionary class analysis that is as much interested in the revolutionary transformation of the present to a classless future as it is in the historical analysis of the past. Indeed, it is in the tension between the two interests that the possibility of its greatest creativity exists. Even if it might justly be said that the separation between historical analysis and revolutionary rhetoric was a major limitation of the prisoners' movement in the late 1960s and early 1970s, at the same time this discourse was distinguished from the others in that it made possible a dialogue with the most powerful ideology within U.S. prisons, an ideology that was wedded to action, that linked the movement within the walls to the world-wide movements of popular liberation, and that could overcome the racial divisions that were systematically and viciously promoted by prison authorities. On the other hand, some academic marxisms arose during the same period that belaboured so arduously at the treadmill of scientificity that they gave an appearance of active movement without actually getting anywhere and produced nothing except, like those forced to turn Victorian penological wheels, self-exhaustion and the pushing of air. Wherever criminologists working within a Marxist framework avoided "the poverty of theory" and succeeded in making effective links with the movement inside, they were crushed, as were the leaders of the Berkeley School of Criminology. But not silenced, as the current volume testifies.

Unlike some other journals dedicated to prison reform or abolition such as *The Outlaw* (San Quentin), *The Prisoners' Digest International* (largely the Federal system), or *NEPA News* (New England prisons), *Crime and Social Justice* found many of its readers among professional and critical criminologists. Consequently, its pages often contained articles of a very high standard that could easily meet the peer review requirements of an academic discipline. Dario Melossi's articles on

Marx and punishment, the work of the editors, Tony Platt and Paul Takagi, and the fecund, spirited scholarship of the Schwendingers were major interventions as much among scholars and thinkers in the international debates within Marxism as among the evolving discussion among prisoners themselves. The journal also published a certain amount of writing from inside prisons. On the whole these were generally interviews, poetry, or pleas of advocacy in various cases; they were not an organic part of the excursive and analytical pieces whose criticisms of the dominant social science discourse were often cogent, courageous, and scholarly, but which nevertheless still were distanced from the thinking inside.

Both the historical discourse and the academic Marxist discourse suffered from this, because in the former the self-activity or autonomous struggle of those who suffered in various penal regimes was not understood, while in the latter the revolutionary initiative of the Marxist subject, the oppressed proletariat, never became a primary cause within the dialectic of change. It is because of these failings, that those who have followed the debates in labour and social history in the last few years have so much to offer. For it is precisely within that discussion where one can find among historians and those interested in Marxist theory an emphasis upon how working people make history. In England there is *History Workshop*, in the U.S. there is the *Radical History Review*, in Italy *Primo Maggio*, in Canada this journal.

In the prisons of the United States there have been two other discourses that have not as yet met in serious dialogue with any of the previous three, a religious and a legal discourse. The latter of course has been most visible to those who have followed the history of the Nation of Islam. Its importance to Afro-American prisoners was absolutely decisive in the 1950s and 1960s because the pride, collective consciousness, and disciplined behaviour it created among the devout proved that among the most oppressed the individualizing, demoralizing psychology of defeat could be overcome. The Pallas and Barber article "From Riot to Revolution" rightly stresses the contribution of those who follow the call to Allah. The Martin Luther or Thomas Müntzer of this movement was Malcolm X. Although the most influential, the Nation of Islam was not the only prisoner-based religious movement. Partly inspired by the Muslim's example, the Church of the New Song ("And they sung a new song, saying, Thou art worthy to take the book, and to open the seals thereof; for thou was slain, and hast redeemed us to God by thy blood out of every kindred, and tongue, and people, and nation." Revelation 5:9) arose in some southern prisons and quickly spread, among white prisoners especially, to the Federal system and to New England. An antinomian, democratically-organized religion, it developed a theology of "Eclatarianism" that may have some affinities with some strands of Québecois millenarianism and that certainly has similarities with the revolutionary apocalyptic thinking of the English Civil War. In an historic decision in Federal District Court (northern Georgia) in 1972 it gained recognition by the authorities and thus protection under the First Amendment of the U.S. Constitution. Both Islam and the Church of the New Song overcame the loneliness of the prisoner's condition; they achieved a space within the penitentiaries for collective gatherings not controlled by the guards; they provided a sense of hope in those institutions incompletely described by Norman Mailer as "infernal machines of destruction." They accomplished this by a transcendent, spiritual discourse that has been a living presence through at least a millennium of human history.

Neither of these religious movements could have survived without the legal victories that each had won. Indeed the discourse of the law is the universal language

of American prisoners. Hateful though it is to many, it is one that all share. Despite its obscurities, inconsistencies, and delays, it still necessarily remains the discursive terrain over which the contending forces within American prisons must fight for freedom. The Muslims gained important Supreme Court victories: the right to hold religious services, to possess the Koran, to receive religious literature in the mail, to be visited by ministers, to follow their own dietary code, and to wear religious symbols. Though weaker in numbers and organization than the Muslims, the Church of the New Song sought an even more important precedent, namely, the right to make individual legal counselling an integral part of their "free exercise seminars" as they called their religious services ("Congress shall make no law respecting an establishment of religion, or prohibiting the *free exercise* thereof"). Thus, the individualist tradition of the "jail-house lawyer" that was responsible for so many victories in attaining civil rights, and the collectivist tradition toward freedom for inside religious congregations, was almost joined, only to be struck down in U.S. District Court.

The thicket of litigation has discouraged many who have not the patience or the necessity of enduring its thousand cuts and scratches. But, as Bob Martin points out in his very interesting analysis of the history of the National Prisoners' Rights Association in Massachusetts, those strategies that sought to outflank the law by concentrating on "rehabilitation" policies or on a public campaign for the abolition of prisons have found themselves mired in a chemical waste dump of chemotherapy or susceptible to policies of segregation and fragmentation of the prison leadership. The accumulation of a corpus of case law limiting the discretionary tyranny of prison officials has never been wholly the product of "bourgeois law." "The laws of kings," wrote Gerrard Winstanley, the religious communist of the English Revolution, "have been always made against such actions as the common people were most inclinable to, on purpose to ensnare them into their sessions and courts; that the lawyers and clergy, who were the King's supporters, might get money thereby and live in fullness by other men's labours." Yet, as a result of such struggles of which Winstanley was a spokesman, the jury, the writ of *habeas corpus*, and the expression of the law in the vernacular, were abiding victories. Civil rights, like the contract rights of factory workers, have been the result of mass struggle, small but precious trails through the law's thicket of thorns. Just as labour history has penetrated the relative superficiality of an institutional approach to labour's organizations to discover the social history of creativity in community and factory, so it is hoped that one of the new directions of contemporary criminology will be the investigation, analysis, and documentation of the prisoners' efforts and creativity in the production of their own rights. In this process the pioneering traditions (we need refer no longer to "discourses") of critical criminology, historical analysis, and Marxism which the Berkeley School has done so much to revive will be essential. Moreover, as the "criminalization" of militants within the labour movement advances, ever-widening sectors of the U.S. working class will find the debates of theory and action within the prisoners' movement relevant to our future safety. This is why *Punishment and Penal Discipline* deserves a readership beyond the professional circle of "criminal justice" specialists on the one hand, and such prisoners who are still allowed to receive it in the mails, on the other.

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Patricia G. Zelman, *Women, Work, and National Policy: The Kennedy-Johnson Years, Studies in American History and Culture, No. 33* (Ann Arbor, Michigan: UMI Research Press 1982).

SEVERAL YEARS BEFORE the feminist movement in the United States had a clearly defined outlook and was organized to influence public policy, equal opportunity for women had been enunciated as national policy and its legal bases had been established. Or, at least this is the perspective presented by Patricia Zelman in *Woman, Work, and National Policy*. Zelman contrasts the difficulties surrounding the attempts of blacks to get legislative action and public acceptance with the relative ease with which women's equality slipped into the American way of life. In particular, equal employment opportunity for women became national policy on the basis of the initiative of a handful of women who had personal influence with Kennedy and Johnson and were able to successfully hitch the women's issue to the coattails of the civil rights movement.

According to Zelman, women's groups had relatively little impact in the initial stages of establishing women's equality as national policy in the 1960s because of an ideological split which had occurred soon after the suffrage issue had been resolved. The split centred on the issues of legal equality versus special protection for women workers. Women's organizations which had worked together on suffrage were deeply divided over whether the subsequent focus of activity should be the immediate and practical concerns of working women who needed protection from exploitative labour conditions or the more long-range goal of the complete elimination of all discriminatory laws against women. These were not compatible objectives, for protection necessitated arguing that women needed to be considered in a different light from men, while absolute legal equality necessitated a perspective which saw danger in any type of unequal treatment. The protectionist stance was held in particular by trade unions, but was supported also by other groups like the League of Women Voters, YWCA, WCTU, the National Council of Catholic Women, and the National Council of Jewish Women, who felt this approach most supported the needs of working-class women. This reform position was strongly and consistently supported by the Women's Bureau in the Department of Labor, which according to Zelman was most influential in establishing national policy on women. Special protection for women was considered a short-sighted policy by the National Woman's Party, who felt that achieving total legal equality would benefit all women, regardless of economic class. In 1923 it brought forward the Equal Rights Amendment to the Constitution, proposing that "Men and women shall have equal rights throughout the U.S. and every place subject to its jurisdiction." The Women's Bureau and many women's organizations saw the vagueness of the wording of ERA as particularly threatening not only to protective legislation for working women, but also to laws on inheritance, divorce, child custody, rape, and property ownership, which treated women unequally, but favourably; so they consistently opposed it. But most significant was the fact that there was not one clear united voice on what the thrust of public policy toward women should be.

Nevertheless, equal employment policy for women did become national policy. It began during John F. Kennedy's presidency and was expanded by Johnson. Kennedy was no feminist, and was frequently criticized by women in his party (most notably Eleanor Roosevelt) for his failure to appoint any women to top posts. But thanks to the efforts of one woman, Esther Peterson, who was Director of the Women's Bureau, Assistant Secretary of Labour, and not incidentally a close friend of Kennedy, the President's Commission on the Status of Women (PCSW) was established. It was a prestigious Commission headed by Eleanor Roosevelt, but essentially it was a conservative one. Its basic interest was in widening women's sphere, not in changing it, and it refused

to endorse ERA. But it did recommend an equal pay bill which, as the Equal Pay Act of 1963, became the first federal equal pay legislation for women.

The final report of the PCSW came out only six weeks before Kennedy's assassination, so little action had been taken on most of its recommendations. For Johnson, this was an opportunity not to be missed. In his attempts to both stress new initiatives and the continuity of the Kennedy administration, the women's issue was ideal. Also, he could surpass Kennedy by moving swiftly in an area where Kennedy had dragged his feet. Two months after becoming President, Johnson pledged to end "stag government;" he promised to appoint 50 women to top policy-making jobs in the government within a month. Once again, this initiative on the part of a president is seen as the result of one woman, in this case Anna Rosenberg Hoffman, a long-time party worker, having the ear of the man in charge. While Johnson did not make good his pledge, this was not entirely his fault, according to Zelman: first of all there was the difficulty of finding 50 top posts which were vacant, but even more detrimental to this attempt to bless women's employment issues was the fact that women were not prepared to respond as a political group. Because there was no public pressure on Johnson to do more — even from organized women — he drifted away from the issue and concentrated on the attention-getting programs of the War on Poverty and the Great Society. But in spite of Johnson's flash-in-the-pan support of women's rights, he still emerges as the hero of the hour. It was his initiatives which raised the policy discussion of equal rights to a new level; encouraged a small, influential group of women within government to act on women's issues; and began the process which eventually politicized women's issues.

The "fluke" linking of sex with race issues in the Civil Rights Act of 1964 is further indication to Zelman that women's rights had little to do with public pressure. The story of Judge Smith's tongue-in-cheek move to include women in the Civil Rights Bill is now fairly well known. By including women, the ridiculousness of the whole concept of equality for what Smith felt were obviously inferior groups, would be evident to all. But should that ploy fail, the added advantage of having women included in the bill would be the weakening of any enforcement measure since any money spent would not be focused on race equality. The fact that the sex clause was included in the bill, despite what was perceived by many supporters of civil rights for blacks as a threat to the entire bill's passage, is perceived by Zelman as the result of the powerful speeches in the House of Representatives of one woman in particular, Martha Griffiths.

The fact that Johnson never committed himself to the sex clause in the bill, according to the author, "testifies to the low level of public interest."(70) Even the War on Poverty, which deliberately downplayed poverty among women, does not smudge Johnson's record on women in this book. In fact because women were treated rather badly in the War on Poverty program, the first stirrings of a feminist political consciousness arises. This feminist outlook became clarified as the difficulties of implementing the sex clause in the Civil Rights Act became apparent. And, it is the recognition of the necessity, by Washington "insiders," that the time was ripe for group political pressure that Betty Friedman eventually was convinced to establish NOW. By this time, as a result of government action, the old split between the various women's groups had been bridged: the linking of sex with race had put an emphasis on equality, rather than protection. So the way was paved for a national organization, inspired by government, to monitor and attack the actions of government.

Zelman's analysis is an interesting one, but limited. Essentially the author

views the political process as "insiders" in government often do: they see themselves as initiators of political action and view the interests and pressures of specific groups as being short-sighted when they do not follow up on these pathbreaking initiatives. In this case women's groups are rather slow off the mark and Johnson's efforts failed for this reason. Yet this was not a total tragedy, for his first steps to interest the public in women's issues actually forced women's groups to focus their ideas and to gain a sharper picture of the possibilities for actions. By the way, it is important here to note that when the author refers to women's groups, she means the bodies appointed by the federal or state governments to deal with women's issues. Zelman may be right that Johnson's professed interest in women's equality might have influenced these groups. However, the whole issue of the influence of both the old established women's groups and the burgeoning grass-roots feminism gets very little attention. Zelman acknowledges, for example, that the National Women's Party initiated the inclusion of a sex provision in the Civil Rights Bill in 1964, but she clearly does not feel its efforts were significant.

While these women, as the "tennis shoe ladies" had been somewhat of a joke in Congress for their persistent attempts to push ERA, their actions had been consistent since 1923, a factor which needs to be considered more seriously.

Zelman puts great emphasis on the fact that NOW was not established until 1966 and did not become a powerful national organization until some years later, yet she totally ignores the influence of the feminism which was emerging in less conventional political forms. This is a deliberate exclusion by the author because these groups did not work within the traditional political context. Yet since an important part of the thesis of the book is that the federal government "helped define the political and legal concerns that sparked the modern feminist movement,"(1) the exclusion from the analysis of the groups which made up the bulk of the feminist movement raises serious questions about the whole argument. Feminism raised an enormous range of issues about the structure and the functioning of society. How these issues reached public consciousness and influenced the actions of men like Kennedy and Johnson is certainly a complex story. While having the right woman in the right place at the right time may have been important in influencing a special phase of public policy, it is unlikely that, as Zelman maintains, this type of action was *sufficient*.

The influence of a growing feminist perspective was surely more compelling than this analysis would have us believe.

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James O'Toole, *Making America Work: Productivity and Responsibility* (New York: Continuum 1981).

JAMES O'TOOLE, AUTHOR of *Work in America*, has written a new capitalist manifesto for the United States. Underlying the present crisis in productivity is a complex of problems, the most important of which, according to the author, are the changing expectations about work by a new generation of workers; expectations that corporations have been unable to meet. This in turn has led to a decline in the willingness to work. O'Toole argues that what is needed above all is a new set of values and a new philosophy of organization and work which would meet the new expectations, and meet them in the context of the economic realities of the 1980s dominated by a decline in economic growths and individual purchasing power.

Change is going to be difficult according to O'Toole, since workers' expectations have been transformed into rights and entitlements (for example, job security, maternity benefits, cost of living

allowances, and others). This trend towards employer provided and government mandated entitlements has resulted in a situation of zero risk for American employees. O'Toole sees this stress on entitlements without concomitant responsibilities as the specific and immediate problem facing the United States. As workers' rights have expanded their responsibilities for the quality and quantity of their work has diminished. Since managers have created the industrial structures which prohibit or discourage workers from assuming responsibilities, the initiative for change must come from corporations and management. The author is convinced that "only corporations have sufficient leverage to produce an alternative future. . . ." (50) A just alternative would be one, "in which corporations created working conditions with the goal of *infusing concomitant responsibilities into the existing and inviolable arena of rights*." (51) O'Toole believes that this would entail moving the U.S. in the direction of industrial cooperations found in West Germany, the Scandinavian countries, and Japan and away from the confrontation politics of Britain and Italy.

The bulk of the book consists of a potpourri of innovative organizational changes and management styles within the United States and abroad, including examples of worker capitalism in several countries including China. Past efforts, such as job enrichment and the socio-technical systems' approach, are dismissed as either window dressing or inadequate, although the alternatives cited by the author do not in fact differ significantly from the socio-technical system model except for the added opportunity for workers to share in the material benefits derived from increased productivity.

This book is clearly written with tremendous enthusiasm and great conviction. The success stories of innovative U.S. firms like Donelly Mirrors, Lincoln Electric, worker owned plywood cooperatives in Oregon (worker capitalism), the Prudential Insurance Company, and several others, are given as illustrations of what is possible in terms of applying a new philosophy of management to organizational and workplace design. There are, however, a number of serious flaws and imperfections in the stories told by O'Toole. Workers and unions in particular should take note of these.

One of the initial points made by the author has to do with his implied assumption that worker rights have escalated in the United States beyond reason and without a corresponding increase in worker responsibilities. By continually comparing the situation in the United States to that of West Germany and the Scandinavian countries, where, according to O'Toole, the increase in worker rights has been accompanied by an increase in worker responsibilities, the author creates misleading perceptions. To put it bluntly, O'Toole exaggerates worker rights in the United States while simultaneously downplaying their responsibilities. The facts are different. Worker rights in Scandinavia and West Germany are far ahead of those enjoyed by American workers. What is of even greater significance, and the author neglects to tell us this side of the story, is the fact that the rights and the responsibilities enjoyed by workers in West Germany and the Scandinavian countries are the result of decades of struggles by the labour movements in those countries and are not unconnected to the fact that in all of those countries Social Democratic governments, in coalition or alone, have been in power for most of the post-World War II period.

The political situation in the United States is dramatically different from that in most of Western Europe. Unions in the United States, as well as in Canada, are weak in comparison to their counterparts in Scandinavia and West Germany and there is no equivalent in the United States to the social democratic and labour parties of most West European nations. While O'Toole wants to borrow the cooperative

aspects of industrial relations from those countries, he has neglected to inform his readers of the differing power relations in existence in the United States as compared to those in Western Europe. He is also advocating, perhaps in silent recognition of this very fact, a totally different approach. He suggests that, in the United States, corporations and their management are to be the initiators of the new cooperative approach to industrial relations. In Scandinavia, the initiative has come from unions and social democratic governments. This has been true of West Germany as well, although the support of the British government for co-determination after World War II is of some historical significance.

These omissions on the part of the author are linked to another underlying assumption which permeates the entire book. O'Toole does not like welfare states, corporatism, or socialism. Ideologically motivated changes are doomed to failure according to O'Toole. Corporatism, however, is an openly admitted fact in West Germany, the present Swedish industrial relations system has been created by a unified labour movement with a strong and openly expressed ideological direction, and both countries qualify for the definition of welfare capitalism. You cannot have it both ways, Mr. O'Toole!

The message of the book is essentially directed to *individual* owners and managers of corporations and not to collectivities like unions and employer federations. While O'Toole recognizes that in the United States context only corporations have the power to initiate such far-reaching changes, the application of the author's suggested innovations applies to individuals. Yet the changes in Western Europe have been the result of accommodation between powerful and centralized unions, employer federations and the state. The low unemployment rate in Norway and Sweden, around 2 per cent for most of the period since World War II, is the direct result of corporatist planning and elite accommodation between union federations, employer federations and the state. Close to 200,000 Swedish workers are presently involved in various activities, including training and retraining and sheltered workshops, under the Labour Market Board. There is, in short, a basic contradiction between O'Toole's desire for a pluralist, free market solution to the problems of the United States and his suggestions that Americans should learn from the success of West European nations. The application of the ideology of liberal individualism expressed in a free market entrepreneurialism and political pluralism cannot be expected to produce the results achieved in Sweden, Norway, and West Germany.

The author is either unable or unwilling to consider the historical roots and the social function of unions. To cite one example, O'Toole, like many social scientists is supportive of the notion of multi-skilled (autonomous) groups as part of the re-organization of workplace design. One can, of course, make a strong case in support of multi-skilling but the fact remains that the skilled trades view multi-skilling as a threat and many unions are therefore resisting this particular innovation. The historical experience of the skilled trades has been one of massive de-skilling, initiated by employers and frequently enforced by the coercive power of the state. One would have thought that someone who builds his new model of industrial relations on cooperation and trust would take into account the history and the legitimate fears of such an important segment of organized labour.

In more general terms, any approach to change the present adversarial reality of industrial relations to one based on cooperation with employees and unions, would have to be based on a few important changes that are fundamental to the union movement. First and foremost all workers should be able to exercise their basic democratic right to bargain collectively.

The removal of hostile and restrictive labour laws is an essential first step in any serious attempt to convince unions to throw their full support behind programmes such as those outlined in this book. O'Toole, however, remains silent on this subject.

The root of the problem in terms of the alternatives suggested by O'Toole rests on his rather simplistic analysis of the causes of the present industrial relations scene in the United States. By suggesting a change in the philosophy of organizational and workplace design to be undertaken by corporations and their managers, he implies that the problem lies in the mismatch between an anachronistic design of organization and work on the one hand and a new set of expectations and demands on the part of workers, on the other. The author offers no analysis of the underlying priorities of the social and economic systems which compels the owners and managers of corporations, as well as workers to behave as profit maximizing individuals. The result is less than satisfying. We are asked to become socially responsible in a system which encourages the individual pursuit of profit and power.

The repeated call, by the author, for worker capitalism in the form of worker owned enterprises, producer cooperatives, or other arrangements will no doubt strike a responsive cord in many readers. The economic reality in the United States, dominated as it is by a handful of giant multinational corporations, renders such proposals to the periphery of economic life. The historic role of worker-owned enterprises in whatever form, has, with notable exceptions, been confined to instances where the private sector has abandoned a given enterprise as unprofitable. O'Toole in fact admits this where he states that worker ownership tends to come about most frequently as a result of plant closures. Workers thus threatened with loss of employment are encouraged, sometimes with the support of public funds, to take over an enterprise which in the view of the former owners is no longer profitable. Whatever one may think of worker capitalism it is difficult to see how this particular form of innovation could spread into significant sectors of industry without far-reaching changes to the political and economic system in the United States.

I have been largely critical of this book. It does nevertheless represent an important contribution since the thrust of O'Toole's argument is one which is increasingly being supported by governments and progressive employers in both Canada and the United States. It may thus represent a coming reality and workers, unions as well as consumers and other interest groups should begin to take the vision presented by O'Toole seriously.

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Tom Kerry, *Workers, Bosses, and Bureaucrats* (New York: Pathfinder Press 1980).

TOM KERRY IS A LONG TIME leader of the Socialist Workers Party (Trotskyists) active in trade union work and this book is a collection of his speeches and articles. The issues covered range from the 1930s to the present, with the addition, occasionally, of further historical background.

Kerry was active as a member of the West Coast Firemen, Oilers, Watertenders and Wipers Association, the Seafarers International Union, and the National Maritime Union and his book is most useful and interesting in those sections which deal with the maritime unions and their struggles. He provides a valuable counterweight to the mythology of Harry Bridges as a union militant and documents the role Bridges played in bureaucratizing his union and helping to bring the maritime unions under the contol of government boards. The erosion of union independence and militancy began in the 1930s with the emphasis in Communist Party

politics of subordinating the union movement to the New Deal and the Democratic Party.

The book is also valuable to a degree as a corrective to much recent labour history which tends to take for granted that the Communist Party in the United States had a leftist, militant, and democratic influence on the labour movement. He reports significant instances, in the *maritime unions and elsewhere, in which* CP forces in the union crushed militancy, fostered collaboration with the employers and the government, and eroded democratic processes.

Unfortunately, the book has major limitations. It reads very much like a Party tract designed to educate and train new members in the Party Line. As a result it tends to be quite superficial in its treatment of issues and very uncritical of where the SWP stood at any particular point.

At one point Kerry justifies the so-called "French Turn" of the SWP. That was the decision of the Trotskyists, after the rise to power of Hitler, to terminate their attempts to reform the Communist Parties and, beginning in France, to enter the leftward moving Socialist Parties. In the United States this meant the entry of the Trotskyists into the SP in 1936 and their leaving the SP in 1938. This was precisely the period in which the CIO was being formed and deprived their people in the labour movement of the freedom to work in the new industrial unions as Trotskyists. Among the advantages of this turn Kerry notes, "we were able to make connections in the unions that otherwise would have been difficult." (47) Yet in the same lecture he is forced to admit: "If the CIO development had happened after the formation of the SWP, we could have gone much further and played a more decisive role in the organization of the CIO." (51) That is to say, the problem is not that the Trotskyists were bottled up in the SP of their own volition, but that the CIO did not wait for them to get out!

Another example is his defense of the demand in the Transitional Program of the Fourth International for a sliding scale of wages. He explains at length the rationale of the program. It was to present certain demands which would seem quite reasonable to the workers but would be basically incompatible with the capitalist system. Therefore, in struggling for these demands the workers would be moved to *come in conflict with the system* as a whole. That was the meaning of the term "transitional." It was transitional to the struggle for socialism. Kerry goes on at length to explain this revolutionary justification. The problem he has, however, is that the demand became a living reality when it was proposed (in the form of COLA) by C.E. Wilson, then president of General Motors. Kerry's response to that is that we thought of it first. And he ignores Wilson's claim after the introduction of the cost of living allowance into the GM contract that, "We have bought ourselves five years of labor peace."

This reflects something which is characteristic of the book as a whole. There is a free use of revolutionary terminology, but it is belied by the very limited and reform minded policies of the SWP. For example, he quotes Trotsky from the Transitional Program: "The prime significance of the [factory] committee lies in the fact that it becomes the militant staff for such working class layers as the trade union is usually incapable of moving to action. *It is precisely from these more oppressed layers that the most self-sacrificing battalions of the revolution will come.*" (243, emphasis added by Kerry) But the trade union policy of the SWP is overwhelmingly directed at winning influence in the official union movement, acting as a loyal opposition which limits its criticism to matters of quantity and does not challenge the fundamental nature of the union contract itself. In educating new members and sympathizers in the lectures which open the book, Kerry does not find it necessary to discuss the

nature of factory committees or anything else which might indicate that the American working class has a revolutionary potential.

Instead the emphasis in most of the articles and talks reprinted here is on the necessity for a Leninist vanguard party to lead the masses to revolution — but before that, the necessity of a labour party to lead the workers to a vanguard party. Reform the unions and build a labour party is the message of this book.

The real problem with Kerry's book is that it is too elementary and superficial. For Trotskyist theory it would be more rewarding to read Trotsky, and for labour history it would be more rewarding to read Art Preis' *Labor's Giant Step* (New York: Pathfinder 1972).

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Doris B. McLaughlin and Anita L.W. Schoomaker, *The Landrum-Griffin Act and Union Democracy* (Ann Arbor: University of Michigan Press 1979).

THIS STUDY AROSE out of a contract with the Labor-Management Services Administration of the United States Department of Labor to examine the effectiveness of the Labor Management Reporting and Disclosure Act of 1959. As a result, its purposes are rather limited and the book reflects these limitations.

The Landrum-Griffin Act arose out of extensive hearings in a Senate committee chaired by the conservative Senator John L. McClellan of Arkansas. These hearings exposed considerable corruption in the pension funds of certain unions, particularly the Teamsters, led at that time by Dave Beck. Beck was ultimately jailed and replaced by Jimmy Hoffa who, in turn, was also jailed. The scandals became the basis for a general anti-union campaign. But, like much U.S. legislation, the law as finally passed was a hodge-podge that combined both pro- and anti-union elements.

Title I of the law was known as the Bill of Rights of union members. It guaranteed free speech and assembly within the framework of the union, a fair system of dues, fines, etc., the right to a copy of the collective bargaining agreement, and the right to sue in federal courts to protect those rights. Title II required unions to file with the government and make available to members copies of constitutions and by-laws, rules for meetings and elections, and financial reports. Title III put a limit of 18 months on the length of time that a parent union could put a local union under trusteeship. Title IV was designed to assure fair and free elections. Title V involved the financial responsiblity of union officers. Title VI has some miscellaneous provisions. Title VII has some amendments to the earlier Taft-Hartley Act, some of which ease some restrictions on labour unions and some of which tighten those restrictions.

The methodology of the study was basically the extensive use of interviews with the various categories of people affected by the Act: union officials, both national and local, union lawyers, dissidents within unions, lawyers for dissidents, rank-and-file workers, employers, civil servants working for the Department of Labor. A major limitation of the study is the emphasis on the Department of Labor, because the responsibility of the Department in enforcing the law was very uneven. Some sections of the Act were enforceable only through court suits, some through the Department, some through other agencies. The study is therefore very uneven in how it judges enforcement of the Act.

In addition, the point of view is essentially bureaucratic. I mean this in several senses. The authors take for granted the importance of stability, that is, the preservation of continuous leadership, in the union movement. They recognize the contradiction in the law which seeks to protect the rights of rank-and-file members while at the same time preserving stable

labour-management relations. Nevertheless, except for extreme cases of scandalous behaviour by union leaders, they accept the need of union leaders to restrain and limit dissent. Conventions three years apart, taking over local unions for a year and a half, seem reasonable to them. In short, they see unions as ongoing institutions, not as organs of struggle. (Given the dozen unions they examined, their perception may be accurate.)

In addition, while they are careful to report criticism from various sources (sometimes from dissidents, sometimes from union officialdom) they accept without much question the overwhelmingly bureaucratic and legalistic framework of the law. It is, in fact, inaccessible to an ordinary rank-and-file worker. Besides having to stand up to the extensive pressure of union officers, the minimum required is access to lawyers and the money to pay lawyers. The only democracy that could be protected is a kind of institutional democracy.

In terms of the functioning of the Department of Labor, the authors offer some minor recommendations for change. Much of the ineffectiveness of the Department in protecting workers' rights they write off as the consequence of lack of funds and lack of staff. In fairness, however, it should be noted that they make clear the heavy politicization at the top levels of the government bureaucracy. There was a widespread belief in the Department, for example, that corruption in the Teamsters was not vigorously pursued because of political debts in the administration. (Which administration did not seem to matter much.)

There is another general problem with the study that limits its usefulness. Confidentiality is extended, not only to individuals, but also to the names of the unions which are studied. As a result it is impossible to break down some of the categories into a more useful form. As an example: statistics on how many locals were put under receivership, and for how long, do not tell you which were taken over by an international union because of militancy and/or wildcat strikes and which were taken over because of financial corruption in the local. Much of the information is difficult to judge without knowing anything about the union, whether it is large or small, whether it is craft or industrial, etc.

The authors, in their final summing up, judge that the Landrum-Griffin Act has improved the democratic rights of union members in the United States, directly (through enforcement by the courts and government) and indirectly (by modifying the by-laws and behaviour of many unions). But they admit that the effect of the law has been minimal in terms of involving rank-and-file workers in the life of their unions. I believe it is fair to say that genuine democratization of the labour movement was not the purpose of Congress in passing the law. Despite some useful clauses, it was basically an anti-labour law.

Because the book is concerned with how the Labor Department enforced the law, little is said about the general effect of Landrum-Griffin especially in combination with Taft-Hartley (1947). Landrum-Griffin reinforces the restrictions on "secondary boycotts" that were originated in Taft-Hartley with the result that the labour movement had no defense against runaway shops. Landrum-Griffin eased somewhat the restrictions on union shops, but it did not lift the restrictions on the closed shop or limit the right of states to be more restrictive than the federal law. The result is evident today in the blows being suffered by the building trade unions and in the difficulty in organizing in open shop states, mainly in the sunbelt.

Despite the limitations, this is a useful book. There is considerable information both in the text and in the appendices. The use of interviews and the use of quotations from interviews helps to provide a sense

of the underlying reality of the American labour movement today.

Opposition candidates are almost invariably referred to as "dissidents" if they find it necessary to use the machinery of the Act. Union officials with hardly an exception see any attempt to democratize their unions as an attack on the organization, an unnecessary expense, a weakening of collective bargaining, and so on. Union lawyers are somewhat more objective and recognize the value of some of the changes in union rules and procedures.

But what receives no attention is the overwhelmingly legalistic framework of unionism and labour-management relations in the United States. The laws, the court cases, the administrative rulings, on both the state and federal level, create a network of legal red tape the ultimate purpose of which is to bind the worker to the job and to put the practice of democracy in the hands of lawyers and bureaucrats.

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Bernard Karsh, *Diary of a Strike* (Urbana: University of Illinois Press 1982).

THIS SECOND EDITION of *Diary of a Strike* (originally published in 1958) provides us with an opportunity to study the limitations of traditional functionalist sociology as it attempts to explain trade union conflict. In a new preface Karsh makes no attempt to re-evaluate the usefulness of the original theoretical framework. Instead he gives us an overview of events in Marinette, Michigan since the initial union drive.

The book, a study "in behaviour of individuals in groups — what they believe, how they react, how they adjust to a conflict situation and its aftermath," follows the now classic paradigm of functionalism. Karsh utilizes Lewis Coser's reworking of George Simmel's functionalist conception of societal checks and balances, to suggest that conflict which aims to resolve tension between antagonistic groups in "open societies," has in fact a stabilizing and integrative effect on the social system as a whole. To put it more simply in the words of David Dubinsky, one time of the ILGWU, "trade unionism needs capitalism like a fish needs water."

The book focuses on the process of unionization in the Marinette Knitting Mill. A campaign by the ILGWU which started in August 1950, succeeded and forced a strike which continued until a contract was signed in October 1951 when local 480 was formed. Karsh uses his survey data, collected a few months after the strike ended, to present a readable document on the development of a union and on the effects of a strike on a small American town. The survey staff interviewed all the elected officers of the local, a sample of rank-and-file union members, "fence-sitters," and a very small number of non-unionists working in the mill. In addition to this data base, Karsh and his associates centered much of their research evidence on the reports of the main union organizer, Harry Bovshow, then ILGWU director for Wisconsin and manager of the Milwaukee Joint Board. Karsh seems enchanted by the charismatic leadership of Bovshow, or Phil Draper as he is named in the book, partially out of the personal power the man appears to hold, and also as a result of the theoretical assumptions Karsh has concerning the role of professional trade union leadership. The book looks at the success of the union drive in terms of the skill of the organizer rather than as a result of conditions in the mill at the time.

Throughout the study he selects information from "Draper" and Helen, the other organizer sent to Marinette, that reinforces his initial perception of the role of the union organizer. "He seeks out situations in which workers have become disaffected from the conditions of their jobs or from the plant society. Once he finds such a situation, he designs his strategy to organize the feelings of the workers

on behalf of the more or less powerful, and perhaps distant, union which he represents. . . . To paraphrase Mills, the union organizer is a manager of discontent, an agent in the institutional channeling of animosity." For Karsh, the organizer comes to symbolize the workers' struggle itself, her presence is both a cause and effect of the success of the strike.

American sociology of the 1950s and early 1960s, steeped in functionalist dogma, rested its philosophical assumptions on a series of socio-psychological concepts, dispensed with the problems of the historical origins of trade unionism, and searched for explanations instead in the leadership of social groups. Industrial sociology, based on its refusal to question the assumptions of the capitalist system, held comfort in the illusion that the study of industrial relations was a study of human relationships, of social mores, and of informal group dynamics. Conflict was not to be viewed as a protracted class war. Karsh upholds this view in his theoretical chapter where he states:

> None of this is to argue that indigenous anatagonism takes the form of a class struggle. Interest groups pervade all aspects of our society: political, religious, social, agricultural, business, and the rest of it. Modern American society can be viewed as an unending series of emergent and decaying interest groups which come naturally into conflict between and among each other. The conflict with which we are concerned is between interest groups, not social classes.

Instead, he suggests that after the passage of the unpopular Taft-Hartley in 1947 the professional organizer's role became a product of good advertising and promotion. "The personality of the organizer is important, for he sells himself along with, and perhaps as much as, his union." It is not much of a step to move from this conception of the union organizer to the conception of the union itself as big business, an attitude prevalent in the industrial relations ideology of the cold war era.

There is only a minimal attempt on the part of the author to situate this particular strike within the context of trade union growth in Marinette or within the turbulent history of the ILGWU itself. The relationship between Bovshow and the "top union officials" who provided support for the union drive is unclear and underdeveloped. Little sense can be made of the process of decision-making which occurs in the ILGWU leadership, and the union remains for the reader, as it appears to the local membership, a distant presence, little understood, making decisions which are not to be questioned by the rank and file.

For example, Karsh mentions in passing that "the international's regional vice-president approved strike benefits of $10 a week for women and $16 for men, the difference based on the assumption that women had working husbands." There is no discussion of the development of union policy on this issue or on other policy questions which were raised during the strike. The decision to strike is treated in much the same manner. The ILGWU's policy was to wait for a decision from the National Labour Relations Board on a company's final legal resort before recommending strike action. Karsh briefly mentions that the local labour leaders were dissatisfied with this policy, yet he fails to elaborate on the tension this created between rank and file, local labour support and the professional organizers.

In Karsh's attempt to capture the quality and content of the lives of the mill workers, he loses sight of broader trade union questions. He limits his attempt to situate the mill struggle within the context of trade union growth after World War II to a few paragraphs, and his description of the town's history is presented in only a few brief paragraphs in chapter two. In an effort to document the day-to-day growth of the mill union movement he isolates this process from the broader context of trade union relations within the commu-

nity and the local's relationship to the ILGWU itself.

Within the industrial relations school the question of unionization itself is posed in such a way as to suggest the act itself is abnormal. Karsh asks: "How did it happen that this group of workers, almost all women, came to reject an established way of factory life by joining a union and carrying on a bitter and violent struggle that lasted for many months and was felt for long years afterward?" The tendency in functionalist theory to perceive any action which upsets the equilibrium in the social system as deviant, forces this author to regard trade union growth and its resultant miltancy as problematic. Yet, from the author's own records he suggests that the attempt to unionize the Marinette mill was a logical step in the light of its recent trade union history. Most major industrial production in the area was unionized during World War II. The mill was perceived by the local Trades and Labor Council as a threat to union standards as wage rates there were considerably below the average for the state. In his documentation of the events of the strike it is apparent that local trade unionists both initiated the call to unionize and provided a basis for ongoing support throughout the strike. Yet Karsh in his attempt to look at the relationship of the union representative to the local rank and file and to provide us with a diary of events throughout the strike, outlines only briefly the role of the community and local trade union support which was so necessary in the fight for unionization.

The strength of the book rests in the presentation of the interview material. In chapter seven, in an analysis of the workers' views, he quotes at length the women's responses to picket duty:

> We made the picket line a lot of fun. We did all sorts of things — you've heard about the noise-makers and the mirrors and the barrels of tar. We made dummies of the scabs and the bosses and hung them on the poles around the factory. We came down in costumes and we sang all sorts of songs and had parties. It got so we couldn't stay away from the picket line for fear we'd miss some fun. We actually made fun out of a grim struggle.

Quotations such as this give the book a vitality and suggest a personal scale to the politicization process which occurred during the strike. It is unfortunate that this type of material was not used more extensively throughout the book. Oral reports are particularly useful in examining women's attitudes to the union process.

Karsh states, "at peak season the mill employed approximately 200 workers, all but fifteen of whom were women." Yet this factor of the overwhelming presence of female labour in the mill is never treated as a particularly important factor in the process of unionization or in the strike itself. The industrial strategies of union organizer "Phil Draper" continually overshadow the role the women play in the strike. The tension between women who supported the union and the "non-joiners" is ignored. At one point the author states that almost 90 per cent of the non-joiners were sole supporters of their family as compared to 45 per cent of the rank-and-file members, yet this obvious economic tension gets only a comment and is not developed further. Little attempt is made to study the women's role as trade unionists within the context of their position in the family.

Through the interview material we see that the women joined the union because husbands or brothers were active in trade unions. The fact that the "workers" were women is never treated as a significant variable, and as a result much of the time we are left with the impression that the strikers are mainly male. For example, when Karsh describes the personal approach of the trade union organizer called Helen, he states: "By playing the role of the sympathetic listener, Helen left it to the worker to define *his* dissatisfactions for *himself*."

As sociologists continue to search for explanations of trade union conflict within the confines of an inadequate historical

and theoretical paradigm, their efforts remain limited. Sol Chaikin, President of the ILGWU, suggests the book is "history as it actually was, and actually is" yet Karsh remains locked into the theoretical framework which hinders his description of the events of the strike. The explanation for trade union militancy in small town U.S.A. remains elusive.

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R.L. Johnstone, *The Scope of Faculty Collective Bargaining: An Analysis of Faculty Union Agreements at Four-Year Institutions of Higher Education* (Westport: Greenwood Press 1981).

THE AUTHOR, A PROFESSOR of Sociology and Acting Associate Dean of the School of Arts and Sciences at Central Michigan University, whose original interest in the subject was as a faculty member, negotiator, and president of his local faculty association, has now become a manager — if only acting.

The book gives a bird's eye view of the contents of faculty collective agreements at 89 four-year institutions in the United States: there were 94 such agreements in existence on 31 December 1979. Given the length of faculty collective agreements (typically over 40,000 words, the complicated inter-relationships between their hundreds of clauses and the dry contract language in which they are written, it needs a person devoted to the cause of collective bargaining to read all this stuff and to try to make some sense of it. Although Professor Johnstone avoids polemics in the book, he clearly qualifies as a devotee to the cause.

The material analyzed seems to cover everything except copyrights, patents, and authors' rights. Of course, the book is a guide and not a detailed review of faculty collective agreements; so for precise information on the content and language one would have to look at the documents themselves. There are some quotations from agreements, and some of them are rather amusing. For example, one agreement says, "If classroom temperatures become so hot or so cold or noise becomes so loud as to preclude the possibility of meaningful academic discourse, the faculty member involved shall first attempt to locate a suitable alternative location for the class, and if none is available, the said faculty member may dismiss class." Even better than that is the agreement that defines a tax-sheltered annuity option as, "a method of delaying taxes on an elymosynary institution's employee pension plan until the time when the employee retires," but having said all that the parties to the said agreement did not have the charity to incorporate that option! It is interesting to note that the pronominal forms "he or she" and "his or her" do not appear in any of the quotations from agreements, only "he" and "his" occur. I do not know whether this reflects a lack of sensitivity to this issue among the negotiators in the U.S.

Points of view on faculty collective agreements are not systematically presented in the book, but there are a number of remarks that are worth noting. Thus the author tells us that, "although faculty unions appear to have made straight salary gains in excess of national averages for college and university faculty, other forms of compensation provide new horizons to which faculty bargaining agents can still aspire." (68) Later he says, "almost no one debates whether bargaining over fringe benefits is appropriate" (it is). (71) Professor Johnstone has wise words here and there throughout the book. For example, "Although some faculty will disappoint their colleagues, the administration, and the board of trustees by taking advantage of the openness of the situation, the encouragement of collegiality for the majority should more than compensate for the shirkers and the disappointment they generate." (114) Another is, "the most serious mistake an administration can make is to treat the faculty as . . . people

interested solely in improving their own welfare" (118), and "The almost classic specter of time clocks, stop watches, and all kinds of numerical measures of productivity reminiscent of the factory setting has remained in the arsenal of opponents to faculty collective bargaining but has nowhere occurred." (171)

Is there good justification for long and complicated faculty collective agreements? Do academics believe they belong in an elite class in some way superior to the so-called "blue-collar" worker? How constrained in bargaining are faculty members by the legislation covering the process and by the arbitral jurisprudence surrounding it? Are the processes for collegiality likely to lead to employer-dominated unions? What are the advantages and disadvantages of internal grievance resolution compared to external rights arbitration? How do faculty unions develop from ground zero through various collective agreements, and how does the administration and the board of governors react to that? I find these "theoretical" questions to be fascinating, but Professor Johnstone does not address them. Although that is disappointing, it is not a fault: the book is addressed to more practical matters.

The author is quite clear in his intention. "Are we asking too much or too little? Is this item or that one normal and common in a collective-bargaining agreement of this kind, or is it unusual, unique, even bordering on the absurd? And how do we compare? It is precisely such questions that this book attempts to answer." (xiii) How relevant is all of this for Canadian institutions? I found the material covered to be typical of what we now have in about 50 faculty collective agreements in this country (over 40 of them with legally recognized faculty unions). The exceptions are the missing item on copyrights, patents, and authors' rights, previously mentioned, and there appears to be a much stronger emphasis in the U.S. agreements on employer-supported medical and hospital insurance plans. The reasons for the latter are obvious.

I can therefore recommend this book for those faculty members and their associations that are contemplating collective bargaining for the first time. It also contains a number of ideas which experienced negotiators, or their associations, may wish to consider for incorporation in future agreements.

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Gregorio Selser, *Sandino*, translated by Cedric Belfrage (New York: Monthly Review Press 1981).

A REVOLUTIONARY MOVEMENT led by the Sandinist National Liberation Front defeated Nicaragua's 43-year-old Somoza dynasty in July 1979. This victory, following a brief but destructive civil war, was the culmination of a struggle which had been begun in 1927 by César Augusto Sandino. A man of humble origins and profound patriotic convictions, Sandino led a seven-year guerilla war against United States occupation forces. Upon their withdrawal he was assassinated in February 1934 by Anastasio Somoza, the head of the United States organized Nicaraguan National Guard. It was Anastasio's son and namesake whom the Sandinists defeated in 1979.

In *Sandino*, first published in Spanish in 1955, Gregorio Selser tells the story of the United States interventions, the corrupt complicity of Nicaragua's elite and politicians, and Sandino's courageous peasant supported guerilla resistance. *Sandino* is not an academic treatise. The book is written for a broad audience by a skilled journalist who focuses on events and personalities — their passions and greeds, strengths and weaknesses. Selser does not hesitate to take a position. Like Sandino, he is an anti-imperialist and a Latin American nationalist. However, the

author's commitment to the cause of the guerilla leader whose struggle he chronicles in no way detracts from the value of his work. For Selser is not an apologist or a propagandist. *Sandino* is a meticulously researched work, based on a thorough reading of primary as well as secondary sources, including United States government documents and the periodical publications of the period covered in the work.

The first chapters of the book review the history of United States interventions in Nicaragua's affairs from the mid-nineteenth century business ventures of Cornelius Vanderbilt to the landing of the Marines in 1909 and their final withdrawal in 1933. Tennessean William Walker's 1856-57 attempt to conquer Nicaragua and establish a model slave state to bolster the cause of the South against Northern abolitionists might be dismissed as the bizarre adventure of a fanatic were it not for the fact that U.S. President Pierce recognized the government Walker briefly set up. Corporate expansion, diplomatic meddling and threats, and the inevitable armed interventions to protect growing investments were presented, in the words of an American Ambassador to Nicaragua, as the "moral mandate" of the United States "to exert its influence for the preservation of the general peace of Central America." (36) Following the Russian Revolution, communist subversion was added to the United States lexicon. Throughout the work, Selser quotes extensively from government sources to illustrate the "clumsiness, stupidity and arrogance" (37) of United States diplomats, bankers, and policy makers, their contempt for the nations and peoples they were subjugating.

Of course, the American expansion could not have taken place without the local collaboration which Selser also describes in some detail. As well, he cites the critics of these policies in the United States, among them W.E. Borah, Chairman of the Senate Foreign Relations Committee at the time of the 1927 armed intervention into Nicaragua. On that occasion Borah told the press: "This dishonest action by the U.S. government is so objectionable that I have no words to comment on it. What I would like to say could not be printed in the papers." (60)

Both the rationalizations for intervention and the minority criticism have an impressively contemporary ring. Similarly, just as the Reagan administration prefers to call Central America's contemporary revolutionaries "terrorists," so did his Republican predecessors in the 1920 brand Sandino and his guerilla army as "bandits." It is to the life and thought of Sandino as a guerilla leader and to the military action of 1927-33 that the major part of Selser's work is devoted.

In fact, little is known about Sandino's life before 1927. He consistently refused to discuss it in any detail, convinced that only his leadership of the nationalist struggle was publicly relevant. However, his experience of representatives from the powerful Northern neighbour preceded the engagement with the U.S. Marines. Sandino had worked as a mechanic for the United Fruit Company in Guatemala (1922) as well as the American owned Huasteca Petroleum Company in Mexico (1923). And it was as a worker in the American owned San Albino mines in Nicaragua that Sandino first became politically active.

In 1927, Sandino joined the Nicaraguan Liberal Party's opposition to yet another U.S. military intervention. When the Liberals worked out an accommodation with the U.S., Sandino continued the resistance. With a force of 300 men, which eventually grew to 3,000, he harassed the Marines until their final departure at the end of 1933. In the course of the struggle, Sandino became a folk hero not only in Nicaragua but throughout Latin America. He also displayed an intuitive military genius for the organization of guerilla war against a modern professional army. His single minded and sole objec-

tive was the withdrawal of U.S. forces from Nicaraguan soil.

Ironically, it was Sandino's success that ultimately defeated him. In 1933, "the objective fact that they [the marines] were going confronted a leader who had made their presence the heart of his whole campaign." He had "always insisted that his struggle had no ideological, political, or social motivations, only patriotic ones. . . ." (151) "Sandino neither would or could see beyond his immediate objective, to stop the intervention. That once achieved, he thought, Nicaraguans — and by extension Latin Americans — would solve their problems, the parties would be honest, the military less ambitious, the businessmen more honorable, and the workers and *campesinos* less despoiled. His political ingenuousness made him commit gross errors, one of which cost his life." (201-2)

Sandino thus negotiated a peace settlement with a new Liberal administration which was elected into power in 1932. Step by step, he demobilized his army despite ample evidence of bad faith on the part of the U.S. organized National Guard. "Sandino's politically unforgivable good faith or ingenuousness put him at his enemies' mercy." (168) Newly elected President Sacasa could not control the Guard, and its commander, Anastasio Somoza, first organized the assassination of Sandino in 1934 and then in 1936 established the family dictatorship which was to rule Nicaragua with the active support of the U.S. until 1979. Appropriately, Selser's work concludes with a description of U.S. foreign investment in Nicaragua and the fortune that the Somozas amassed.

Although Selser's story effectively ends with Sandino's death in 1936, the work provides a wealth of information as well as insight for understanding the contemporary crisis in Central America and the profound roots of the region's anti-Americanism. In fact, the reader without a basic acquaintance of Central American or Nicaraguan history may occasionally get lost in the mass of detail that Selser provides. The work could also be criticized for lack of a general interpretation of the socio-economic and political processes underlying the events described. But that is not what Selser set out to do. His goal in 1955 was to dig out the nitty-gritty on what, at the time, was a little known and badly distorted history of a national liberation struggle. This he has accomplished most effectively in *Sandino.*

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Walter Rodney, *A History of the Guyanese Working People, 1881-1905* (Baltimore: Johns Hopkins University Press 1981).

IN THE HISTORY of the Caribbean, Guyana, then British Guyana, has an honoured place: it was the first country to generate and elect, in 1952, a party whose leadership identified itself as Marxist. The People's Progressive government was, consequently, the first Caribbean victim of the Cold War, overthrown by a British government, spaniel at the heels of the Truman Doctrine. The groundworks of Guyana's militant political tradition have been carefully excavated by Walter Rodney whose opposition to the dictatorship spawned by Cold War politics led to his recent assassination.

His posthumously published *History of the Guyanese Working People, 1881-1905* records the struggles of the Guyanese people in the grip of the great depression which followed the collapse of sugar prices in 1884. In the next 20 years the number of operating sugar estates fell from 105 to 46. Two thirds of these properties were abandoned completely, including the largest single property in the colony, Bel Air. And this in a society where there was already, arguably, a relative surplus of estate labour. In these conditions the struggle for subsistence neces-

sarily led to resistance and Rodney attempts to elucidate the resulting formation of political consciousness among the people. In doing so, he has made a notable contribution both to the history of Guyana and to a frontier area of current research, the history of unorganized labour.

The margins for resistance to the terms of employment offered by the sugar estates was always, in British Guyana, extremely narrow. The free villages established after emancipation were on a coastal plain maintained by an elaborate and expensive system of drainage canals. The ex-slaves were condemned to fight an endless battle with scarce resources to maintain sea defenses and drainage systems while the planters appropriated public funds to keep their lands in order. The planters intensified these natural difficulties by setting high purchase prices and high minimum acreages for land sales. By 1861 this legislative encirclement of the peasantry was complete; the free villagers remained dependent on the estates for part-time work.

They used their subsistence base as best they could to maintain some freedom of choice in their lives; they organized themselves into work gangs, hired to the estates by the job on a contract basis. They established a monopoly of some skilled jobs: shovellers for canals and dams, cane cutters, and factory workers. But the power of free-wage bargaining was denied them because the bulk of estate labour was performed by indentured labourers imported from Madeira, Africa, China, and pre-eminently, from India. The wages paid for indentured labour determined, largely, the price of contract labour; the rate for a day's task remained until well into the twentieth century, at the level fixed at emancipation in 1938, 32 cents. As the villagers themselves expressed it in a petition to the governor, "peasant labourers hesitate between starvation and the price which these [immigrants] in their serfdom are compelled to take."

Throughout the century alternative employment opportunities were extremely limited; lumberjacking for hard woods opened up in the 1850s, gold mining in the 1880s. Upward mobility was limited to those who won promotion on the estates, or found the means to set up as farmers, traders, money lenders, or skilled workmen. Rodney celebrates the creativity of the Chinese immigrants who introduced rice farming and won government support; the Indians who emerged as cattle farmers, or "resurfaced retained skills" as metal workers or scribes; the African Creoles who set up plantain farms and produced techniques for controlling plantain disease. These groups established places for themselves in the interstices of the rural economy and created a small, but significant, petit bourgeoisie and a thin layer of professionals. It was symptomatic of the low level of subsistence which the country afforded working people that in the 1870s the number of Indians who chose to return home at the end of their indentures almost doubled. They could not find work, or when they did, the wages were inadequate to support their families. Only the crisis of the following years forced the planters to staunch this flow by offering land in lieu of passage money.

The planters' command of the land and of a reserve army of labour constituted in themselves powerful instruments of social control; the fact that the workers were themselves divided between African creoles and Indian immigrants or Indian Creoles, constituted another. Rodney combats the crude stereotypes of imagined history which projects into nineteenth-century Guyana the racial antipathies and communal violence which preceded independence. The cultural creolization of the immigrant population is documented in a vivid detail; competition for employment is distinguished from racism as such. But it is clear that the planters used sophisticated methods to maintain a racially-divided working population, exacerbating the tensions naturally created by competition for work, and suc-

ceeded in establishing a racial divide in Guyana, which like the religious divide in Ireland, presented a formidable obstacle to the creation of a united working class and one capable of delaying the process for generations.

The workers' rage and frustration spilled over into strikes, riots, and the occasional murder. One small constellation of organized groupings, however, made their appearance in the wake of the 1886-1891 agitation for constitutional reform and one of them provided in turn the starter motor of the 1905 riots.

Strike action had a long tradition. In 1842 the free villagers actually won a round in the wage-bargaining process, only to be defeated in 1848 when the estates were kept ticking over by newly imported indentured labour. But the indentured labourers themselves, subjected to the same pressures at the point of production, developed the same techniques of individual and collective protest. The frequency of individual protests — absenteeism, non-compliance with work regulations as indicated by the number of cases brought before the magistrates, increased in the last two decades to as many as 20 per cent of the labour force. The super-exploitation of indentured labour, also led to strike actions, often accompanied by arson, violence, clashes with the police, and loss of life. One such clash in 1870 led to the appointment of a commission of inquiry into the treatment of immigrants.

Conditions after 1884 made strikes a regular feature of labour relations; 31 in 1885, 15 in 1887, rising to a peak of 42 in 1889. There were similar waves of strike activity from 1895-7 and 1900-03, in spite of the fact that estate managers stepped up efforts both to punish and co-opt. These isolated estate based protests were easily contained; the potential power of an agricultural labour force whose implements, as one governor observed, "constitute them at any time an armed force" was, unfortunately, never demonstrated.

Workers' organizations began to appear in the free villages and the towns in the 1880s and took the form of Benefit or Friendly societies, centred sometimes on the nonconformist churches. By the end of the decade teachers, bankers, mechanics, printers, and compositors had formed associations and unions. These initiatives reflected the new political horizons created by the campaign for constitutional reform, headed by young, black professionals, supported by white and brown merchants which was launched in 1886. The reformers addressed the "intermediary classes" and advocated reduction of the property qualification for the franchise from $600 to $300. Their campaign gained impetus from the governor who, in a dispute with the planters himself, advocated widening the franchise to include $480 voters. Thereafter constitutional reform campaign became a bandwagon for reform in general; it reached into the villages, briefly made the Georgetown public buildings the scene of mass meetings, and prompted trade associations and other ad hoc workers' groupings including lightermen, coopers, bakers, printers, stevedores, and grooms into a spate of strikes for higher wages.

Rodney's claim that this extra parliamentary dimension of the campaign made the reform petitions effective is not altogether convincing. The reform achieved was the reform the governor had originally suggested: a $480 franchise. It gave Guyana one voter per hundred of the population, compared to one in fifteen in Jamaica. The Reformers themselves considered they had asked for bread and received a stone, although, even a decade later they were, as Rodney comments "far from achieving (or perhaps even conceptualizing) the degree of power required to neutralize the planters."

The Reform campaign nevertheless stirred interest on a country wide basis and won positive support from wage earners, artisans, and small cultivators who were outside the range of the $300 prop-

erty qualification but who "set out to be seen and heard advocating a radical position," developing their political skills and ideas. And the disappointed intermediate class, in due course coalesced into a "progressive" or "people's" party, supported by a penny newspaper called *People* which fought the 1901 elections on the issues of education and an end to state-aided immigration.

The grand conflagration of 1905 when Georgetown was held, briefly, by a levee en masse of the population owed nothing, however, to these first sprouts of parliamentarianism and everything to union militancy. Stevedores, who had long disputed a wage rate of six cents an hour, were suddenly offered, by one company, a special rate of sixteen cents to complete a particular job. The offer precipitated a strike and protest marches in Georgetown which won response from employed and unemployed alike. Within two days, workers who were not on strike were being pushed off the job. Action spread to the sugar estates where the police, accustomed to quelling disturbances with a whiff of grapeshot, seriously injured four workers. When this news reached Georgetown three fourths of the population stormed the public buildings, and forced the governor to refuge behind locked doors. Seven people died and seventeen were wounded in more shooting that day; but strike action spread to more sugar estates, where wage demands and expropriations were the order of the day. Six hundred bluejackets shipped from Barbados and Trinidad by cruiser had to join the 300 police to restore law and order. Though the strike wave caught up workers in the hinterland gold fields as late as 12 December, Georgetown was quiet by 5 December.

Repression was complete. The governor, however, confronted by middle-class elements anxious to mediate the conflict processed the rioters through the magistrates courts. Although the Riot Act had been read, no one was charged with riotous assembly, or sedition, felonies punishable by penal servitude for life; instead the charges were of "disorderly conduct," "throwing stones," "assaulting the police." Men were often flogged and women, who made up a good 50 per cent of the rioters, had their heads shaved.

The 1905 riots marked a high point in sustained, popular agitation. The scale of the strikes and riots was itself significant. For the first time action in Georgetown excited responses along the East bank of the Demarara and up the Puruni. More significant even than the scale is the scope — urban workers and unemployed plus rural workers, Afro-Guyanese plus Indo-Guyanese, momentarily united and acting with the de facto connivance of the middle classes who discreetly omitted to report for militia duty. As a result, Rodney comments, "small qualitative changes in the consciousness of the work force" were registered which became operational after 1906 in attempts at organized struggle.

Arguably, however, the most innovative aspect of the 1905 riots was the element of strategy and planning present in its origin. The Colonial Company stevedores were offered sixteen cents an hour on Sunday, 26 November. Strike action started Tuesday, 28 November. It is said that the stevedores waited until there were six ships to load before they struck and the strategy is attributed (perhaps inevitably) to Hubert Critchlow, founding father of the trade-union movement in the Caribbean. Wherever the idea came from, the first workers off the job on Tuesday set about systematically immobilizing the whole water front. Their orderly marches with banners (rapidly sewn, presumably, by their women folk the previous day) proclaiming "16 cents an hour or no work," plus police bullets triggered the rest. Spontaneous actions by specific groups of workers flaring at the point of production, flinging up leaders to serve each event, had developed a new dimension and the stevedores, organized since

the 1880s, in charge of a key link in the colonial economy, were the catalyst.

The consequences of this development lie, unfortunately, outside this book; but, hopefully, the depth and penetration of Rodney's analysis in itself an admirable contribution to the historiography of the Caribbean, will inspire its own successors.

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George Rudé, *Ideology and Popular Protest* (New York: Pantheon 1980).

GEORGE RUDÉ'S CONTRIBUTIONS to the social history of France and England from the perspective of subordinated social classes has been immense. *Ideology and Popular Protest* is a continuation of this earlier work, but it is not a richly detailed account of particular events or of a restricted period; rather it presents Rudé's interpretative framework and a series of fleeting images of a wide array of social movements intended to demonstrate the utility of the theoretical framework.

The concepts of culture and ideology have received renewed interest in recent years from Marxist-inspired social scientists. As he certainly acknowledges, Rudé's development of these themes owes much to Gramsci who stressed the importance of developing a working-class ideology and also of taking into account the loosely structured beliefs of peasants and artisans. But Rudé's particular concern is with the interaction between these "inherent," rough ideological notions carried by the "popular" classes and the more sophisticated ideologies emanating "from a higher social group." Thus, influenced by Gramsci's concepts of "non-organic" ideology and his advocacy of the establishment of a counter-ideology by "organic" intellectuals committed to a working class faced with capitalist ideological hegemony, Rudé develops his own concept of popular ideology. An important condition of historical social movements, he argues, particularly revolutionary ones, is the existence of a broadly accepted popular ideology composed of (i) traditional elements based on direct experience and folk memory, and (ii) more structured, "derived" or borrowed ideas. Constant interaction between the two sources of ideology is common and, in time, the second category may merge with the first. The derived component of ideology may be, in fact, an intellectual formulation of the experience of ordinary people, which returns, in a sense, to make its own contribution to popular ideology. How to distinguish between these two aspects of ideology in empirical research is not at all clear.

By itself, the inherent or traditional ideology can promote only an undirected, rebellious pattern of social action; it is inadequate to spawn a social revolution which requires the grafting of derived ideological notions. Furthermore, popular ideology may move in revolutionary or counter-revolutionary directions according to the content of the derived beliefs, current conditions and experience, that is, the interaction of belief and circumstances determines the course of action. It is worth observing at this point that Rudé's Marxism has moved to a position very close to and perhaps identical with a considerable body of sociological theory on social movements. At this point we have reached the basic explanatory principle which the remainder of the book is intended to illustrate or even to prove. Yet, not a single reference can be found to the sociological literature in which these ideas have for long been commonplace. Furthermore, the independent significance which sociologists have traditionally attached to beliefs as an explanation of social movement activity (a path which Rudé now follows) have been sharply assaulted by authors such as Charles Tilly and Mayer Zald, who place much more emphasis on the social context and the capacity to mobilize resources as determinants of collective action. The neglect or relegation of the importance of

ideology has been an over-reaction in my opinion, but it is remarkable and disappointing that this whole body of literature should be completely ignored by Rudé, considering that his book purports to establish a general theoretical framework for understanding popular protest.

In the remainder of the book the reader is treated to a series of thumbnail sketches of a vast range of collective action. Part two addresses peasant rebellions from Medieval Europe (particularly the English Revolt of 1381 and the German Peasant War) through France, Austria, and Russia under absolutist monarchy to more recent uprisings in Latin America. In part three, we sweep quickly through the English, American, and French Revolutions of both the eighteenth and nineteenth centuries. Then in part four, the series of popular protests in England during the emergence of industrial society are considered with similar brevity.

In each case, the point being pressed home by Rudé is that the course of popular action can be explained according to the framework which I have outlined above. Thus, the sketches contain a description of social context, the main historical actors, and the ideologies which motivated them. He is particularly concerned to trace the elements of derived and inherent ideology. With careful selection a surprising amount of relevant information has been crammed into a small space. The numerous examples do appear to support Rudé's argument, although no case is presented in sufficient detail to permit the non-specialist to evaluate the validity of the author's interpretation and disputed issues are rarely mentioned. More important, perhaps, is that the theory itself needs much more development. If inherent ideology by itself is not the spring of revolutionary action, then under what circumstances can the required, derived elements appear? If popular ideology itself is insufficient to generate popular protest, then what additional social conditions are necessary? Rudé does not face these questions directly. His indirect answer is to provide descriptions of the ideology and context of particular historical instances, but there is no attempt to investigate situations where some but not all of the elements are present. The ad hoc collection of case studies does not permit us to determine what conditions are necessary, sufficient or merely contribute to the appearance and course of popular protest.

While *Ideology and Public Protest* is not the path-breaking book that its publisher would have us believe, it may convince doubting historians that a sociological perspective can indeed contribute to the explanation of collective action. Sociologists are not likely to find many new ideas here, but the cogent summaries of many pre-industrial movements may well encourage them to delve further into the historical analysis than they would otherwise consider worthwhile. Despite my critical comments, a brief volume that reaches such ends is no small achievement and should certainly be read by everyone interested in social movements. In another sense, the book is also a source of encouragement to radical intellectuals to whom Rudé ascribes a real and important role in bringing about social change.

Peter R. Sinclair
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Dick Geary, *European Labour Protest 1848-1939* (New York: St. Martin's Press 1981).

THIS IS AN IMPRESSIVELY ambitious essay, the best short survey of European labour history currently available. Its quality is a tribute to the author's abilities in synthesis, and also to the considerable progress that has been made in studying various forms of working-class life and protest over the past 15 years — for Dr. Geary draws on wide reading in the secondary literature, despite a somewhat misleading claim that the field is in its infancy.

The book's coverage has several defining features. Emphasis is on England, France, and Germany, with more scattered treatment of Russia, Italy, Spain, and occasionally Austria. There is no effort to fill the pages with chronological narrative. Rather, the book focuses on some major issues in each principal stage of labour's development. Overall, this attention to leading processes is informed by an effort to deal with labour history from the bottom up — to explain differential participation, timing, and to some extent characteristics in terms of the actual interests of the workers involved. Thus ideology is played against the reading habits of ordinary workers, the timidities of union bureaucracies against some persistent radical impulses in the rank and file.

As crossnational history, the book stresses common features among ordinary workers as against the distinctive pressures of national political forms, police strategies, and employer policies. This interpretive framework means among other things that English or German workers are tacitly regarded as "normal" in their protest behaviour — with skilled workers first leading the way, pressures of deradicalization soon setting in — and more persistently revolutionary areas explainable by aberrant political and entrepreneurial forms. This schema may do less than full justice to the popular origins of Russian or Spanish radicalism, though Dr. Geary does give some attention to rural roots. Specialists may quarrel with speculations that the "real" interests of Russian workers were the same as those of their Western counterparts, simply requiring different forms of expression. The approach would have been more plausible had the author been able to spend more time on the special cases. Still, while issues must be signalled, the approach is basically a sound one, congruent with available data and interpretation. And the author strengthens his case by maintaining an open stance, raising questions as well as suggesting lines of response.

Dr. Geary divides his treatment into three principal periods. A section on origins to 1890 deals with the transition from pre-industrial through early industrial to modern forms of protest. Admirable care is taken to delineate explanatory models without falling victim to their potential rigidity. Thus residential, organizational, economic, and ideological explanations are explored, with strengths and weaknesses indicated. This is not mindless theory-mongering; the author is careful to show where some approaches do not work at all, and where some elements simply must be included in any final synthesis. Similarly typologies of protest, drawing on a particularly rich recent literature, are used intelligently to organize masses of narrative data. In this first main section, the author falters a bit only in dealing with early politicization, where some recent work is missed or downplayed.

The maturation of labour movements from 1890-1914 properly revolves around the central issue of deradicalization or radicalization, and symptoms and causes of both tendencies. The author's boldness in dealing simultaneously with strikes, unions, and politics is particularly marked in this segment.

One must also admire the effort seriously to include treatment of the interwar period. Themes here are less well defined, the presentation less coherent — which accurately reflects a more poorly developed synthetic literature. Leading issues include divisions within the labour movement and the widespread failures or limited successes of working-class efforts.

Inevitably, there are flaws in the book; no such brief survey could be without them. Some readers will find the absence of extended ecstasies over familiar landmarks in the labour narrative disconcerting. More relevant is the possibility that some students will find too few factual pegs to hang their memories upon. I do not think the book errs in its balance

between fact and process, but I am sure that a few will disagree. I was more distressed by slightly less from-the-bottom-up material than existing research allows. The "play" element of protest is thus largely ignored. The author offers frequent apologies to women for their substantial absence from his account, but makes no effort to interweave family life, and its varieties and changes, into his interpretation. Changes in consumer standards are appropriately noted, but comparable attention is not given to life on the job itself. One of the causes of the relative lifelessness of the final main chapter, on the interwar period, is the failure to continue a lively social-historical substructure. We learn nothing, for example, about the rise and impact of white collar labour. Dr. Geary's periodization, though perfectly defendable, is ultimately a descriptive one, rather than an effort to balance changes in the labour movement as symptoms with changes in the position of workers in society as cause.

The book's final conclusions are somewhat drab and discouraging. Dr. Geary notes labour's failure to achieve professed goals, its frequent divisions and defeats. I have no quarrel with this judgment up to a point, but wish that more attention had been given to the partial successes, including the origins of the welfare state. In this respect, a terminus in 1939, though preferable to the outdated fascination with 1914, is somewhat misleading.

I emerged, however, primarily impressed with Dr. Geary's capacity to synthesize around leading issues. The book is a marked step forward in comparison with earlier short, crossnational treatments. One can only wish it a somewhat more durable fate than its predecessors. The book can be immensely valuable in introducing students to the field, deserving a wider audience than its current price will probably allow.

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André Liebich *The Future of Socialism in Europe?/L'Avenir du socialisme en Europe?* (Montréal: Interuniversity Centre for European Studies/Centre universitaire d'études éuropéenes 1979).

THIS BOOK IS an eloquent testimony to the "objective fact" that theoretical examinations of socialism thrive best in capitalist countries! It contains the complete *Proceedings* of the 3rd International Colloquium held by the Interuniversity Centre for European Studies at Montreal in March 1978. The Colloquium was sponsored by the Canada Council, the Ford Foundation, the Canadian Department for External Affairs, and two local universities. With one exception, all the major contributors came from Western Europe and North America: "Canada," noted one of them — either innocently or archly — "seems to be very often the host for discussions which raise the question of socialism in other continents."

In addition to 16 papers, the volume includes a transcript of the ensuing discussions and post facto introductions to the five sections into which the meetings were grouped. The latter, and a general introduction by the editor, have been translated into English or French as the case might be. The book has been ably edited by André Liebich though, without wishing to be pedantic, one must fault the irritating (and incorrect) fashion of rendering the French past historic and imperfect tenses into the English present.

The question in point, "The Future of Socialism in Europe?", proved singularly elusive — despite the Canadian organizers' premise "that certain broad social, political and economic conditions make a possible transition to socialism in large parts of Western Europe more than an academic alternative." No one, of course, could say as did the Webbs after their visit to the Soviet Union in 1932 that "We have seen the future and it works!" But if several contributors were chosen for their leftish political commitments as well as their

academic credentials few appeared confident that socialism in Europe had any assured future or, if it did, cared to predict what form it might take. Rather did they err on the side of caution and warning. The largest common denominator of agreement, or hope, favoured "Eurocommunism" as the answer to the statist versus social democratic dilemma. Yet the more closely Eurocommunism was examined, the more obvious its ambiguities and shortcomings became. Little consolation, apparently, was to be found in Henri Lefebvre's observation that "la voie de la révolution n'est pas une autoroute."

The nature of the book both precludes and makes superfluous any detailed comment. What must be said is that it was well worth doing. The papers were on uniformly high, though different, levels: the discussions were stimulating and the summaries are balanced and useful. Read as a whole, the volume provides a sparkling intellectual entertainment.

As Liebich points out, the opening session successfully established the themes and set the tone for the rest of the colloquium. Lefebvre asserted that either the old communist parties "se renouvellent, ou ils disparaissent, ou ils dégénèrent." Perry Anderson responded by emphasizing that the Eurocommunist parties exist in countries with no experience of reformist working party governments. He posed three questions: How could the sterility of the Northern European labour parties be avoided? What model of state was needed for a transition to socialism in capitalist Europe? What would be the effect on any such transition of the international conjuncture? The subsequent presentations and debates did not produce many answers to these questions though Pierre Hassner, in the last meeting, rather convincingly suggested that the third question was the most substantial one.

Two papers were devoted to the role of the state: Leo Panitch offered some interesting re-definitions of several Marxist clichés, while Claus Offe analyzed the necessity for, yet dangers of, statism. Ghita Ionescu and Giuliano Procacci discussed the role of the party: both condemned the idea of a "power monopolizing centre," but neither were very clear as to what could or should replace it. Similarly in regard to alternative forms of socialist ownership, Andras Hegedüs and Pierre Joye were mostly agreed in their rejection of Soviet and East European concepts, especially bureaucratism; but they were less effective in their suggestions about self-management. Hegedüs, indeed, figured in the most poignant scene in the colloquium. He, an ex-prime minister of Hungary, was confronted by a former Czechoslovakian worker, a shop-steward until 1968 in one of Prague's biggest factories. Now in Canada, still a socialist trade-unionist, involved in mining and strikes, the worker contrasted his experience of capitalist managerial efficiency to the detriment of East European practices. Hegudüs had no rejoinder to make.

Still less solace could be obtained from two solidly academic papers devoted to planning and marketing in East Europe. Zbigniew Fallenbuchl, in the course of an historical survey of the Council for Mutual Economic Assistance, had no difficulty in demonstrating some of the Soviet and East European economic systems' larger deficiencies. David Granick argued that to the extent Western countries were also adopting CMEA goals — full employment, job security, income distribution and the like — they too were becoming, or would become, inefficient!

The question of workers' management in industry, or participation in management, figured in several papers: it was examined specifically in the cases of West Germany and Sweden, Portugal, and Yugoslavia by G. Hunnius, M.V. Cabral and A. Whitehorn respectively. The practical nature of this session provided, as Liebich notes, "a valuable counterpart" to

the more speculative suggestions advanced elsewhere in the colloquium.

In fact, the lingering theoretical Marxism of some contributors and many discussants, which undoubtedly had an adverse effect on the quality of the colloquium, was much in evidence in the lively reaction to the two papers devoted to "Classes Under Socialism." The principal speakers roundly denounced the entire Marxist approach. Charles Sabel made a systematic and informed attack on the classic conception of the relationship between social classes and political behaviour, while Frank Parkin stood the colloquium virtually on its head by arguing that social democracy was the only viable way to socialism in Western Europe. The robustness of Parkin's contribution may be indicated by his reference to "a long tradition in Marxist theory which rests on a firm belief in the innate gullibility of the working class," or by his aphorism, "Marxists collect crisis theories like some people collect postage stamps."

Whether the colloquium's organizers quite expected this déroulement is a moot point; but for making the *Proceedings* readily available in convenient form they deserve to be congratulated.

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Jorgen Haestrup, *European Resistance Movements, 1939-1945: A Complete History* (Westport, Ct.: Meckler 1981).

WHAT ORGANIZED AND UNORGANIZED groups of opponents of the German Occupation of Europe experienced during World War II bears some similarities to the less crushing Napoleonic absorption of independent, and the creation of satellite, states within the boundaries of the First French Empire, however spurious its legal status. Many more vivid parallels may be found in present-day governments which, by means of martial law, terrorization, brutal and systematic mass murder, keep the examples of resistance constantly within our sights, but without thereby enhancing our ability to comprehend and condemn, except by the use of stock phrases. At the same time, we do not know enough to insist that we have a greater capacity for insensitivity, or to argue with utter conviction that a greater sense of helplessness may heighten our sympathies but dull our guilt. Much of our uncertainty probably arises from the unexpected and ironic ways in which victims became oppressors and oppressors become victims in response to the shifting and unpredictable demands of politics, revolution, and war. It is not only that roles are actually reversed, but that onlookers, perplexed by moral incertitude and driven by political expediency, are partly responsible for the transposition. Perhaps Freud was right when he put it to Arnold Zweig, in much darker words, that he could not help but surrender to "his wholly anti-scientific belief that mankind on the average . . . are a wretched lot."

These remarks are prompted by the observations and allusions to some of the larger questions of the politics and morals of human nature, scattered almost invisibly in Jorgen Haestrup's exhausting and dense history of the European Resistance. It is only when he takes some of them up, however allusively and deliberately distanced, that his book threatens to become absorbing and rescue us from tedium. For some specialist historians and general readers who are content with an organized presentation of materials demonstrating a fairly comprehensive grasp of the literature — in some instances Haestrup relies excessively on a handful of the available authorities and ignores much that has been written in the last ten or so years — the present volume will answer the less complex questions related to the forms of resistance, ranging from intelligence, clandestine journalism, civil disobedience of several kinds to the more violent expressions of protest, including sabotage, partisan warfare, and assassination.

Haestrup's major purpose is to disabuse those whose main understanding of the war in Europe rests on a reading of Liddell Hart's massive study which barely alludes to the military importance of the Resistance in helping to undermine the military power of the Axis powers. Haestrup's evidence, in part quantitative and assembled in the long section he calls "Politics and Supplies," is meant to put as strong a case as possible for the significance of the military operations of the Resistance movements.

The duration of the war itself gives strength to his thesis. As the Germans extended their operations to the peripheries of Europe, particularly to the east, south-east, the near east, and to northern Africa, the Resistance also gained a greater impetus and, almost from the start in some regions, especially in the overrun parts of Russia, assumed a military character. Incidentally Haestrup does not think to relate Russian partisan activity to the unmistakable encouragement the Nazi invaders received in the Ukraine, where a mixture of frustrated nationalism and pure hatred of the Communist regime, intensified the fury of the Russian authorities and dictated a policy of resistance without limit. The ancient anti-Semitism in the region was not, it should be added, a deciding factor.

The demarcation between the military and non-military aspects of the Resistance throughout Occupied Europe is much easier to detect than the distinction between guerrilla warfare and the organized military units of partisans, not only in Russia, but also in Yugoslavia, Italy after the fall of Mussolini, Greece, Poland, and France. If the Resistance had remained relatively non-violent, its psychological effects, by measuring its arousal of anti-Nazi feeling and pro-allied support, would have to be cast within the general framework of general psychological warfare conducted by the major allied powers. As it was, advances in military technology, the increasing size of land, sea, and air forces, together with insistence to make the occupied populations increasingly a part of a larger military operation, makes it difficult to argue that the war against the Axis powers would have been won without the considerable support of the Resistance. If the European populations had been decisively compliant, the outcome might in fact have been different. That they were not and that the discussions on how the war should be fought included the roles to be assigned to and sometimes imposed on the various Resistance movements are shown to be part of serious British strategy as early as summer 1940. Haestrup also deals with such issues as the disputed relationship between governments-in-exile and refractory, potentially revolutionary, movements in Occupied Europe, whose perspectives, both military and political, clashed more often than they coincided. He cannot omit, of course, an analysis of the great distances between Russian and Western views of the Resistance forces that should be given aid and those that should be repressed. The rifts between the two Polish partisan armies, the equally prolonged, though more quickly settled conflicts between the Chetniks and Tito's forces, and the bitter struggles in Greece which were never resolved during the war, are among the controversial episodes that Haestrup takes up. There are others, but perhaps his discussion of the Danish reaction to the Nazis and their eventual contribution to the Resistance is singularly enlightening, because of Haestrup's direct and intimate knowledge of his country's role during the war. What is missing from the more familiar themes is some greater appreciation of the pre-1939 political struggles and dilemmas, many of them centred on the policies to be adopted towards local Nazi and Fascist groups, and which, once the war came, rapidly became the single most important issue in fighting the war and thinking about the peace. Haestrup does not give us a sense of the extent to which, if for the sake of

brevity we take a simplistic viewpoint, one of the disputants looked on the war as an opportunity for revolutionary action, and the other hoped to salvage as much of the pre-1939 world as possible. He also fails to tell us how much of this was public knowledge in Britain and the United States where these issues were openly and hotly debated.

One of the significant problems which he does take up with greater acuity is the controversy between the advocates of strategic bombing and on-the-ground sabotage carried out by units of the Resistance. The allied staffs generally favoured the first as being more effective, while many within the Resistance lamented the damage this was doing to civilian morale, because of the inaccuracy of the bombing missions, which took civilian lives. In Britain, the issue was wonderfully alive, a fact, like many others, to which Haestrup does not allude. It was taken up by many prominent publicists and intellectuals, among them Liddell Hart, Vera Brittain, and George Orwell. According to Orwell, Liddell Hart had very early in the war expressed his horror at the prospect of English bombing sorties plunging the English people into the same barbarism in which the Germans had become ensnared. Orwell differed both with him and with Vera Brittain who believed that there were rules in war that had to be obeyed. Orwell had been arguing since 1937 that the lives of civilians were not be thought of as more sacred than those of men in uniform. He believed that an antidote against much intellectual lightheadedness on the Left, which was one of his preferred targets, could be prepared by giving up any pretence that there were some privileged places and persons that should be spared. His passion to challenge, ridicule, and destroy hypocrisy clearly brought him to the edge of the apocalyptic.

The bombing question and others like it crop up in Haestrup's book, but they are rarely an integral part of it. Had they been more fully exploited, it would have been a more commanding one. The extent to which it was possible to resist, and the extent to which it was expedient to feign collaboration, in short, the best forms of Resistance in circumstances that could make the difference between economic survival and starvation, deportation, and murderous reprisals, are, to be sure, not ignored, but Haestrup is more concerned with the art and politics rather than with the ethics of deception, a tangled question which, it is often said, war transforms into a speculative luxury. Whenever customary forms of authority vanish or are discredited, questionable ones put in their place, as occurred during the Occupation, the foundations for individual choice may be marvelously focused and the quest for legitimate political action may achieve startling clarity — at least for some. The full history of the Resistance therefore remains to be written. An excellent continuing point, for example, would be to ponder the implications of the actions of the villagers of Le Chambon, whose reverence for life was enough to impel them to resist the Nazis and their supporters in a most unorthodox way. Haestrup's interest in his subject is compelling and should lead him to Philip Hallie's book which tells us about this side of the Resistance. It cannot be ignored in any general account.

One final point. Haestrup's book first appeared in its original Danish version in 1976. An English translation was published by the Odense University Press in 1978 under a title that differs slightly from the present one. The text remains the same, including, unfortunately, a number of curious neologisms and flaws in diction that a conscientious editor would have rejected.

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Anthony Sutcliffe, *Towards the Planned City: Germany, Britain, The United States and France 1780-1914* (New York: St. Martin's Press 1981)

I HAVE LONG ADMIRED urban scholars who had the courage to look beyond the merely parochial and delve into phenomena in cross-cultural settings. Professor Sutcliffe's latest book traces for four countries the emergence of a revolutionary idea in the years leading up to the outbreak of World War II. That revolutionary idea was urban planning, which Sutcliffe defines as "the deliberate ordering by public authority of the physical arrangements of towns or parts of towns in order to promote their efficient and equitable functioning as economic and social units, and to create an aesthetically pleasing environment." (viii) This achievement was reached in different ways within varying cultural contexts. For the most part Sutcliffe succeeds in his attempt to account for the emergence of the urban planning idea in four countries. In the process, he also has been able to outline in some detail the interrelationships between planning movements in different settings.

Towards the Planned City is organized into seven chapters. The first of these is devoted to establishing the context within which planning emerged during the period in question. Focus here is placed upon such matters as urbanization, industrialization, changing technology, and evolving governmental structures. All of this is covered in less than eight pages, which makes for a rather sweeping and highly generalized discussion. Statements concerning urban spatial structure are particularly loose in their presentation. (3) Chapters two through five deal, respectively, with German, British, American, and French experiences with urban planning in the years prior to 1914. Here the detail concerning the individuals and organizations involved in the planning movement in each country is especially noteworthy. The tortuous road to the passage of planning legislation is well documented in each instance. In all four chapters a keen appreciation for the importance of both the rate of urbanization and the sheer magnitude of the urban population is displayed by the author. The final two chapters of the book are more synthetic in their approach to the topic. In the penultimate chapter an evaluation of planning as part of a burgeoning wave of international movements is presented. The final, brief chapter considers the legacy of the pre-1914 planning movement. Overall, this rather slim volume provides a useful introduction to the topic, but at times its intended audience remains unclear due to the somewhat uneven treatment of some issues.

As is the case with most syntheses, Professor Sutcliffe is forced to rely on secondary sources for a good deal of his evidence, a debt which he readily acknowledges. (ix) For Germany, Britain, and France material culled from such sources is interwoven with Sutcliffe's own research into urban development in these countries. The result is convincing narrative. The same level of familiarity and expertise, however, is less evident in the case of the analysis of planning in the United States. Here the discussion relies upon quite dated secondary sources and some important works are not cited at all. As a result the chapter dealing with the United States is less even than the others. This situation is not aided by the rather meagre use of illustrations. Only five are included here (compared to eleven in the chapter on Britain) and of these three deal with Daniel Burnham's 1909 plan for Chicago and the other two pertain to the New York city zoning ordinance of 1916. Important events to be sure, but they by no means capture the spirit of the many components that came to comprise urban planning in the United States. Illustrations showing the contributions of landscape architects and the large-scale builder/developers who had emerged by the 1890s

would have been useful. While such diagrams have been published elsewhere, they would not have been out of place in this volume. I might add that the remarks concerning the use of illustrations in the chapter on the United States apply to the rest of the book as well. My feeling is that roughly twice as many maps and diagrams would have better suited Sutcliffe's purposes. Planning, after all, is an extremely visual process. Well chosen illustrations are mandatory in any attempt to capture its evolution.

The real strength of *Towards the Planned City* lies in Sutcliffe's examination of the cross-cultural and inter-cultural influences in the emerging planning movement prior to 1914. Clearly, Germany was the urban planning leader at that time, but Professor Sutcliffe demonstrates that the German planners were influenced by what was taking place in the other countries, especially in Britain. In this regard a summary table or some sort of schematic diagram would have been helpful to capture the sense of this complicated process of cross-fertilization.

What place is to be accorded to Sutcliffe's volume within the literature of planning? First of all, his subject matter is certainly worthy of consideration. The emergence of urban planning, as Sutcliffe suggests, "in just twenty-five years from a morass of partial and often contradictory urban policies to the point where it caught the imagination of the whole of the urbanized world must rank as one of the most dramatic episodes in the whole history of public administration." (208) While providing useful insight into the pre-1914 era, *Towards the Planned City* does not stand on its own. It is too slender a book to be the definitive work in its field. This may well reflect the format of the series in which it was published rather than the wishes of its author. As it is Professor Sutcliffe has done an admirable job in a limited space. His book should serve as a valuable text at the undergraduate level, and I would not hesitate to recommend it for that purpose. Serious scholars, no doubt, will join me in encouraging Sutcliffe to commence work on an expanded treatment of the topic.

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The Institute of the International Working-Class Movement, U.S.S.R. Academy of Sciences, *The International Working-Class Movement: Problems of History and Theory, Volume I, The Origins of the Proletariat and Its Evolution as a Revolutionary Class*, with an introduction by B.W. Ponomarev (Moscow: Progress Publishers 1980 [Russian edition 1976]).

THIS 674-PAGE BOOK IS the first volume of a seven-volume project written by Soviet researchers largely associated with the Institute of the International Working-Class Movement in Moscow. Though a collective work, the names of the principal contributors for each of the eleven chapters are given. Besides having responsibility for the Introduction, Boris Ponomarev chaired the 21-member General Editorial Committee of the project. A.S. Chernyayev was Chief Editor of the four-member Editorial Board.

The general political objective of the project is clear and explicit — to study the working-class movement in order to advance the struggle for socialism and communism. As Ponomarev writes: "Marxism-Leninism proceeds from the need for research into the whole wealth of international experience of proletarian struggle and for securing a situation in which class-conscious workers could 'have an *understanding of the significance* of their movement and a *thorough knowledge* of it' [Lenin]. This is precisely what the production of generalizing work on the history of the world working-class movement is called upon to facilitate." (9)

The central theme is the development of the working class as the mainspring of social progress, its constructive, trans-

forming, revolutionary role, which is crystalized in Marx's comment that "the proletariat alone is a really revolutionary class." (627-8) Given this interpretation of the leading social role of the proletariat, Ponomarev states that the working-class movement is taken as "a totality of all forms of working-class activity in implementing its world historic mission. Viewed from this angle, the working-class movement originated at the same time as the working-class itself." (10) However, its history is not isolated but "part of general social history." (11)

The first volume covers "the initial period" of the working-class movement from its origins up to, but not including, the Paris Commune of 1871. The approach is evolutionary, considering the movement with respect to its origin and certain developmental stages, in order to determine "the fundamental laws and major characteristics" of its evolution. (12) Besides the basic socio-economic stages (pre-proletariat, manufacturing proletariat), with the main emphasis on the industrial revolution, there is considerable discussion of the political and ideological development of the working class from "a passive, exploited mass" to "a growing factor in economic and social struggle" to "a conscious social force." (14)

There is some treatment of labour and co-operative organizations and the class-forming role of the economic struggle, but more attention is given to the achievements of working-class political organizations, especially the Communist League and the First International, and to "the culminations" of the class struggle (the European revolutions of 1848-1849 receive nearly 100 pages). Along the generally "ascending line" of development are mentioned: April 1820, Scotland, "*the first general political strike* in the history of the world labour movement" (216); July 1830, Paris, "the first massive action of proletarians in history that toppled a political regime" (291); November 1831, Lyons, "the first armed confrontation between the workers and the bourgeoisie in history" (295); and early 1840s, Britain, "the first broad, truly mass and politically organised proletarian revolutionary movement" (Chartism) (319); June 1848, Paris, "the first great civil war between the proletariat and the bourgeoisie" (434); the European revolutions of 1848-1849, late 1850s, "the proletariat came forward as a revolutionary class on an international scale" (536); *1868, Brussels, "the first time in history* a forum of representatives of mass workmen's organisations . . . expressed itself in favour of socialism" (590); late 1860s, "independent *proletarian* parties came into being" (597); 1860s, "the first mass actions of the workers of Europe and America against militarism and the ruling circles' aggressive policy" (660 - 1); 1871, the Paris Commune, "the first workers' state in history". (601)

Regarding ideological development, there is an entire chapter (chapter 4) devoted to Utopian Socialism, mainly Owen, Saint-Simon, Fourier, and their followers. Utopian Socialism is recog*nized for its contribution in the formation* of the social ideas of the workers and in important social reforms such as protective legislation on child labour and the ten-hour day. It is recognized as well for the beginnings of such important concepts as classes and class struggle, productive forces, and surplus-value, for phrases such as "abolition of the exploitation of man by man" and "From each according to his ability, to each ability according to its work," and for the very words "socialism" and "communism." Special *attention is also given to the Utopian* Communists (mainly Cabet, Dézamy, Blanqui, and Weitling) and revolutionary Chartism: "this proletarian revolutionary part of Utopian Socialism had an important role to play in the struggle of the proletarians for political independence." (329)

But an historic watershed in the initial period of the working-class movement appeared with Marxism. Its emergence beginning in the 1840s is seen as "the greatest revolution in science," (363) bringing about the transition from Utopian Socialism to Scientific Socialism. Marx's economic theory, in particular, is taken as being the only theory able to explain scientifically the position of the working class in the capitalist system, as being "the *bedrock* of the proletarian party." (532) This theory substantiates the leading, revolutionary role of the proletariat, which "arises from its objective position as a class lacking private property, selling its labour power, and exploited." (642) Marx's political theory is seen as developing key ideas needed for the working class to achieve political power, especially those related to the dictatorship of the proletariat, national liberation, and the international unity of the working class.

Rejecting the "armchair scholar" picture of Marx, the volume treats Marx's theoretical evolution in relation to the period's general political background and to his own (with Engel's) ongoing practical political activities as a revolutionary. Much of Chapter 9 on the formation of the First International is devoted to Marx's central role. In Engel's words: "His real mission in life was to contribute . . . to the liberation of the modern proletariat." (558) In this commitment, argues the volume, there is no necessary contradiction between scientific objectivity and political partisanship; "so far as a 'man of science,' of economic science, is concerned, he must have no ideal and must obtain scientific results, but if on top of this he is also a Party man, he must work for their materialisation in practice." (Engels, 552)

A considerable part of the volume contends with the character and social roots of anti-working-class ideology. Chapter 10 is devoted entirely to nineteenth- and twentieth-century bourgeois historiography on the proletariat's historical role. It is suggested that at least four general trends have appeared, one treating the working class as "an 'unbridled' force, disrupting the natural course of the historical process," the second as "a passive mass in need of enlightened leadership," the third as "inevitably 'integrated' into the capitalist system," and the fourth has "extolled the spontaneousness of proletarian movements, idealized their early forms, likened the proletariat to the lumpen-proletariat." (618)

The concluding chapter (Chapter 11), which surveys some of the results of the initial period and summarizes various obstacles to the developing unity and independence of the working-class movement (particularly its social heterogeneity and inexperience), also emphasizes the importance of the theoretical/ideological element. By the end of the volume, having ranged over a great variety of often sharply contentious issues, many readers will agree with Ponomarev's comment that "The cardinal problems of the formation and ascension of the working-class are the subject of an *acute ideological struggle, which is the main line of long ideological development*." (29)

Within the general perspective of the project and given the vast scope of its subject matter, the volume must be judged a major achievement. Moreover, if this first volume is a fair indication, during the 1980s the project as a whole will probably become the leading general work on the international working-class movement representative of the mainstream of Marxist scholarship in the Soviet Union and the international communist movement. Even for adversaries of socialism and Marxism, the volume will provide a measure of the theoretical development of their opponents.

However, a warning is in order. This volume is a systematic, summarizing work: it will not appeal much to those who have an anti-theoretical orientation to historical studies or who want light reading. Nor will it be easy going for those who are squeamish about Marxist terminology or

who reject out of hand Marxism's claim to be a *scientific theory* (and, hence, the possibility of Scientific Socialism). Those who have an exclusively antiquarian interest in the working-class movement will not find a great deal of excitement in the first volume, although by way of exception one can note there is some very interesting material from the repository of the Institute of Marxism-Leninism used in treating the 1848 Revolution in France. Other especially interesting points can be found in the sections treating ideological development in Russia and in various smaller passages such as those treating Marx's concepts of material prerequisites and "continuing revolution" and his criticisms of "barrack-room communism" and the idea "the worse, the better." Most sources used in this volume are accessible; the most frequently cited secondary authors are P. Foner, E. Hobsbawm, F. Potemkin, A.L. Morton and E.P. Thompson.

This is the first summarizing work on the international working-class movement (though the pioneering work of Jurgen Kuczynski deserves an honourable mention). Inevitably, there are some areas of unevenness and weakness. One hopes the publishers will contemplate a second edition in which appropriate improvements can be made, such as the following.

To begin, in certain areas, especially in treating smaller countries, there needs to be deeper familiarity with the available literature, including the Marxist literature. For instance, in Chapter 2 the quality of the material on Canada is inadequate: there is no mention of national division in the Canadian proletariat, immigration figures are not properly interpreted, and only W.A. Carrothers' 1929 book — hardly the best available work — is cited. It is difficult to blame this problem on a single individual; an even greater collective effort is needed to involve the contributions and criticisms of more national and regional specialists. Furthermore, when summarizing particular national characteristics, the presentation could be strengthened by a more thoroughly structured and systematic approach, perhaps utilizing charts and tables. This would enable additional socio-economic evidence, especially further quantitative material, to be incorporated without cluttering the main arguments. Without appreciably extending its length, the volume could — and should — introduce general analytical material on the role of women in the working-class movement, on the effects in Europe (and not only the United States) of slavery and racism, and on the role of religion, the question of church-state relations, and anti-clericalism. On a smaller point, contrary to what is implied in the otherwise excellent Chapter 8, Marx *did* consider (for example, in *Capital*, I, 477 and *Theories of Surplus-Value*, Pt. I, 166-7) that wage labour producing immaterial commodities (as services) could be productive labour, a principle that does not contradict the primacy of material production. Finally, the volume could be made more useful for reference purposes if the name index were supplemented with a general index. Also, since the volume will be read by some unfamiliar with Marxist terminolgy, the Introduction or first chapter should include a developed definition and etymology of both the terms "working-class" and "proletariat."

Such comments as the above do not suggest a dilution of working-class partisanship in favour of "value-free" and purely "technical" scholarship. But they do suggest that, for a future stage in the advance of working-class studies, a more direct and broad international involvement of progressive scholars in the process of collective research and criticism will be necessary. In the meantime, this first major step by Soviet scholars is well worth the attention of all those seriously interested in the international working-class movement and in Marxism. At $8.50 per hardbound copy, the price of the first

volume is low, except for the many unemployed and destitute.

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Fernand Braudel, *Civilization and Capitalism, 15th-18th Century,* Volume I: *The Structures of Everyday Life*. Rev. ed. trans. Siân Reynolds (London: Collins 1981).

AT CLOSE TO 80 YEARS OF AGE Fernand Braudel continues to astonish. Despite many administrative and professional responsibilities he publishes large volumes distinguished by their breadth of vision, daring analysis, and irrepressible curiosity. Consider the record of the last fifteen years. A year after the second French edition of *The Mediterranean and the Mediterranean World under Philip II* (2 vols. 1966) came the first of three volumes of *Civilisation matérielle, économie et capitalisme*. Volume one appeared in English as *Capitalism and Material Life, 1400-1800* (London 1973) translated by Miriam Kochan, before being superceded by a revised edition (3 Vols. Paris 1979). *The Structures of Everyday Life*, a translation of volume one of the second edition, acknowledges a debt to the Kochan version, offers new material, especially on diet and on money, and includes the detailed table of contents and footnotes omitted from the first English edition. It marks a happy collaboration between translator and publisher. Having cut her teeth on *The Mediterranean* (2 Vols. London 1972-3) Siân Reynolds is thoroughly at home in *le monde braudelien*. Collins has provided a handsomely designed and illustrated volume in a typeface (Sabon) which is a pleasure to read. Two more volumes are promised, barring further revisions in a work modestly described by its author as a progress report.

The French title *Material Life, the Economy and Civilization* offers a clearer indication of the place of volume one in the series than does the choice of *Civilization and Capitalism. The Structures of Everyday Life* details the material existence of ordinary men and women across the globe during three centuries of the pre-industrial age before 1800. Their often desperate search for food, housing, and clothing was circumscribed by physical factors which they were powerless to affect: the harvest, prices, transport, population growth, high mortality rates, death, famine, and plague. Mentally these "limits of the possible" were seldom tested. Inherited custom, passivity, and inertia ("one of the the great artisans of history") mitigated against an impulse to innovate, much less to work for radical change. Wherever they lived the masses functioned in "a world of self-sufficiency and [the] barter of goods and services within a very small area" which aimed at survival.

Where change appeared it was hesitantly proposed and widely resisted. Europe led the way towards modernization: in monetary affairs in the widespread use of currency, the provision of credit, and the refinement of bookkeeping and commercial practices, and in urbanization. But these were features of life at a more sophisticated level of economic activity, distinct from material life. Bankers, merchants, administrators, and some professionals and artisans comprised "active social hierarchies" within a market economy which are reserved for extended discussion in volume two (*Les Jeux de l'échange*). The elite of this minority, termed nascent capitalists, partook of the delights of high finance, currency manipulation, decision making in the multi-national corporations (Jacques Coeur, Fuggers), entrepreneurial innovation, and transport and trade on a continental or global scale. They sought power, enjoyed its exercise, and appropriated its fruits. In time they would profoundly affect the lives of those below them in society and extend their domination (in *Le Temps du monde*) to the rest of the world.

The ground floor of this *édifice braudelien* is laid in *The Structures of Everyday Life*. To describe "the repeated movements, the silent and half forgotten story of men and enduring realities, which were immensely important but [which] made so little noise," the author employs a series of "parahistoric languages" — demography, food, clothing, housing, sources of energy, technology, transportation, money, and towns. These themes serve three aims which have appeared consistently in Braudel's work. They enable the reader to see things whole: "if not to see everything, at least to locate everything." They "reduce this disparate material to the bold outlines and simplifications of historical explanations," and help to distinguish long term continuities. *La Longue durée*, what the study of history for Braudel is ultimately all about, is said to be especially relevant in a period when life conformed to such slow rhythms.

The case of the potato, new to this edition, illustrates the strength of popular resistance to innovation. For two centuries after its introduction to Europe from Peru it was disdained as an unacceptable substitute for bread. The voice of enlightenment, the *Encyclopédie* of 1765, perpetuated the myth that the potato might carry leprosy. Certainly it gave rise to flatulence: "But what is a little wind to the vigorous organs of the peasants and workers!" Grudging acceptance came with its subterranean survival from marauding armies during the incessant wars of the late seventeenth century. But its major success came in Ireland, a response to the threat of famine, while it has continued to be shunned by the non-European world. New stimulants like tea, coffee, chocolate, tobacco, and distilled spirits were more readily accepted, but their relative popularity is reflected at the level of cultures and of civilizations. Tea triumphed in non-vine growing Europe, while vines did not invade the Far East. Chocolate remained a passion with the Spanish and London preferred gin. In its diet Europe had been carnivorous for a millennium, while India, China, and Japan remained overwhelmingly vegetarian.

Europe's concern for fashion, dating from about 1700, owed as much to snobbery as to a desire to innovate. The barefoot masses in their locally manufactured homespun perpetuated, like the social elites of Islam, India, China, and Japan, the garb of preceding centuries. Even among the European elites inertia and resistance ruled. Louis XIV — *le roi soleil* — forbade the use of dining forks, and not just because of his acknowledged skill in eating chicken stew with his fingers. In winter his guests shivered in furs, and at table they found both water and wine frozen in the goblets. The huge fireplaces of European chateaux were built not to roast whole oxen but to permit the privileged in the household to huddle on benches along the hearths's interior walls. Was it the lowly chilblain which breached "the limits of the possible" when chimney design by 1720 advanced to the point of controlling draughts, and the long hesitant progress from brazier to fireplace to stove was given a nudge forward?

Progress, then, was neither dominant nor uniform. Dietary standards declined with the failure of agricultural production to keep pace with population growth in Europe's gradual recovery from the Black Death. In 1800 no public bath house remained in London, though Braudel judiciously notes that neither underwear nor soap was used in China.

Braudel's enthusiasm for all of this is infectious. With great good humour he passes between cities and continents, describing, analyzing, and classifying on a global scale. He estimates the world's population, ranks the sources of energy employed in the pre-industrial world, and offers a tripartite scheme for classifying all cities. He serves up contemporary parallels without apology: "Shouldn't a historical explanation be valid for the present too? Shouldn't the present cor-

roborate that explanation?" (*Afterthoughts on Material Civilization and Capitalism*, 1976) Thus, the global recession since 1973 has brought a reversion to practices of the pre-industrial economy: barter, mutual exchange of services, and moonlighting. The similarity of causes in each case: human greed (the unregulated activities of multi-national corporations, whether the great Indies companies or OPEC) and the vagaries of climate (the European drought of 1976) is evident across the centuries. And what reader of this journal will fail to nod agreement with the contemporary example which illuminates the clash between the pretensions of Europe's burgesses and the royal authority: "What would happen if modern states were suppressed so that chambers of commerce . . . were free to act as they pleased?"

Happy the reader who is able to follow the intricacies of all the debates raised by Braudel. The uninitiated were prudent to approach *The Mediterranean* atlas in hand. (Where was Ragusa and what is it called now?) An attempt to distinguish rice and wheat from their graminaceous cousins, maize, millet, maislin, manioc, and spelt, may have readers reaching for their dictionaries. But such efforts will be repaid. The antiquarian, the selective browser, the undergraduate, and the professional academic will find here a fascinating reconstruction of an often overlooked world and an incentive to read the volumes to come.

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Ronald Aminzade, *Class, Politics, and Early Industrial Capitalism: A Study of Mid-Nineteenth Century Toulouse, France* (Albany: State University of New York Press 1981).

THE GENERATION OF scholars formed during the 1960s — the era of student unrest and the Vietnamese War — is making its debut, revealing in published work a distinct social and political consciousness that is remarkable for its focus on "grass roots" issues, cultural matters, and the contest for state power. A set of values, indeed a credo of beliefs, inform that spirit of the 1960s: preference for the principles of responsive democracy, commitment to political activism, appreciation for practical organization, and a high estimation of cultural determinants of human behaviour. Ronald Aminzade is a good example of this generation. In his *Class, Politics, and Early Industrial Capitalism*, we have local history written at its best. He orchestrates a massive array of sources with sensitivity to the peculiarities of local developments and broad national forces. And he displays a theoretical comprehensiveness that cogently embraces the contradictions of society undergoing capitalist industrialization and emergent popular party politics.

Toulouse, in the mid-nineteenth century, held sixth place among France's urban centres. It dominated the southwestern region as a commercial depot of the grain trade and as a major seat of administrative-military activities. Socio-political upheaval marked the city's history, from the decline of popular royalism in the 1830s, the revolutionary fervour of 1848, the repression of the Second Empire, to the commune of 1871. Throughout this period, Toulouse's working class manifested an increasingly radical attitude and an ability for structured organization. Aminzade's task is twofold: to explain why the city's workers became radicalized, why they adopted an egalitarian collectivist ideology, and how they were able to engage in sustained struggle. Thus, the central focus of the study is on politics, the importance of which, he argues, "lies in its centrality for understanding the dynamics of social change [and] . . . provide a crucial arena for class struggle." (xiv) Linked to the analysis of politics is a concern with the role of the state as it conditions class conflict and facilitates or inhibits workers' capacities

to organize in autonomous opposition against the ascendant bourgeoisie. The focus and concerns of Aminzade's investigation show the strong influence of Charles Tilly in particular, whose book, *From Mobilization to Revolution*, would serve as an appropriate prelude to *Class, Politics, and Early Industrial Capitalism*.

Aminzade's approach to the radicalization and organization of the working class is basically Marxist, though modified by his emphasis upon cultural and political factors that transcend the purely economic sphere of production. He subscribes to the viewpoint that early industrial capitalism proletarianized the artisanal sector, undermining the job security of handicraftsmen and drawing them into sweated trades and factories where loss of control over the workplace constituted a primary grievance. But he goes further and stresses the point that the capacities of worker artisans to organize in sustained fashion derived from their cultural heritage and community mediated by the economic crisis and the political process. Aminzade aptly notes the importance of what he calls the formation of working-class "counter-hegemonic institutions," such as mutual aid societies. Along with meeting places like cafes, cabarets, and taverns, these institutions provided both a place for escaping the dreary world of exploitation and a secular environment of camaraderie that fostered a political ideology which opposed aristocratic-clerical paternalism and bourgeois possessive individualism. In a word, artisanal association counteracted the demoralizing forces of proletarianization and enabled the Toulousian working class to envision an alternative society that was fundamentally egalitarian and collectivist. Moreover, despite the attempt of bourgeois republicans to coopt the working-class movement in the battle against various political regimes (the Bourbon monarchy prior to 1830, Louis Philippe's reign backed by financiers, and Napoleon III's emperorship), workers maintained their autonomy through the continuation of their counter-hegemonic institutions. Ultimately, during the 1860s, they created their own group of leaders and reinforced their commitment to socialist ideals.

Aminzade concludes his analysis with an insightful assessment of the economic, political, and ideological obstacles which hindered the bourgeoisie from creating and stabilizing outright hegemony, that is, "the diffusion of dominant class assumptions and values throughout society such that they become the established limits of common sense." (269) His position follows Antonio Gramsci on several accounts, although previous chapters in the book make no reference to the Italian communist's writings. Nonetheless, Aminzade reasonably asserts that the bourgeoisie's resort to violent repression was a sure sign of its precarious domination. And while the working class failed to supercede the hegemony of the upper class because of its urban isolation, concessions had been won without the cost of complete and acquiescent subordination. In short, one can say that Aminzade has done justice to the artisanal working class of mid-nineteenth century Toulouse.

Unfortunately, what Aminzade seeks to prove in detail often evades verification. Besides an uncritical acceptance of statements made by government officials like supercilious imperial prosecutors (277), the use of quantitative sources is frequently haphazard. For example, when describing the class structure of Toulousian society, census data for 1830 and 1872 are presented, but the category of *proprietaires* is just too vague to warrant even impressionistically support of the view that aristocrats were in decline. And coupling their case with that of *rentiers*, whose presence within the ranks of the bourgeoisie increased from 6.1 to 39.9 per cent, does not justify the assertion about "the growing importance of liquid as opposed to landed capital as a source of wealth of the city's property owners."

(28) Given the fact that those engaged in commerce of grain could be merchants or nobles, or both, and that they suffered a similar setback due to the internationalization of their trade (41, 61), what happened to the upper classes remains unclear. As for the groups comprising the working class, Aminzade makes some peculiar decisions. Artisans employed in the factories are categorically combined with those from the independent handicrafts sector, the reason being that, "factory workers were not a homogeneous group during that period." (30) So the category of *factory workers* is assigned a catch-all role for the unskilled, the young and females of large-scale industry who presumably fall prey to bourgeois hegemony — a logical implication of Aminzade's classification and utilization of source material. Moreover, once having given his summary overview of the city's class structure, he neglects to compute even simple participation rates of groups of militants relative to their proportions within the active population. (See his tables 7-9) At base, to carry out a longitudinal analysis or data linkage concerning militants that would involve more intense study of archival documents (censuses, notarial records, etc.) is doubtless beyond the capacities of a single researcher, but the omission engenders a weakened thesis. The book's index, however, offers little compensation to aid the reader.

Class, Politics, and Early Industrial Capitalism indeed bears the imprint of the 1960s. Along with its undeniable strengths, there are some significant drawbacks to the work. If inspired by interest in "grass roots" movements, it telescopes attention on a culturally articulate group, leaving by the wayside those who still remain in the shadows of social history: the poor, the downtrodden, the outraged, in a word the proletariat. If it designates politics and the struggle for hegemonic rule as crucial, it tends to mistake ephemeral dissensions within the elite as an absence of ideological cohesion among the bourgeois. While the proletariat keeps falling out of theoretical reach, so the heights of power escape the grasp of an analysis that is first rate as long as it focuses on the moderately cultured in society. On balance, it is a book worthy of close attention by social historians engaged in local studies.

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Jean H. Quataert, *Reluctant Feminists in German Social Democracy, 1885-1917* (Princeton, N.J.: Princeton University Press 1979).

RELUCTANT FEMINISTS in German Social Democracy joins a growing literature on German feminism, much of it on the lively, and influential, socialist wing. Canadians can be envious that there is more available *in English* on the German women's movement than in any language on the Canadian. In spite of German women's handicaps in education (women were admitted to German universities only in 1901) they published extensively: feisty and analytical journals; fully, whopping, Wagnerian-length treatises; and memoirs, vituperative and intimate. August Bebel's *Woman under Socialism* (1883) was a major impetus, and Bebel himself continued to support women socialists in their struggles. The German women's movement was the largest on the continent, and, by reasonable criteria, enormously successful. Women gained the vote in 1918, and relatively large numbers of women were elected to the Reichstag in the early Weimar period. Equality for women became part of the platform of the German Social Democratic Party, and German women were leaders in the socialist women's movement internationally. While women socialists in Austria and Belgium gave way to their men on the demand for the vote for women, German socialist women kept their men honest.

Quataert makes a persuasive case that it was tough for German women socialists

to be feminists at the same time. Further, when the crunch came, it was the feminism that gave way. Radical feminism was an adjunct to socialism, never fully integrated in theory, much less in practice. The ambivalence is traced back to the earliest days of the German Social Democratic Party, an amalgamation of a Lassallean organization that would have sent women back into the home, and a workers' association that saw the need for female labour, and urged trade unions for women. The immense difficulties of organizing women in Imperial Germany are described. Not only were women not allowed to vote, in some states they were not allowed to belong to political parties (and socialist organizations were themselves banned for some years). Regulations were variously enforced forbidding women's speaking at, or even attending, political meetings. Ability to outwit the police was an absolute requisite for leadership in the socialist movement.

The issue of special measures for women is dealt with as an early example of the affirmative action debate. For a while "equality" within the party was the women's goal, hence the end of measures, for example, to insure the selection of women delegates to national and international congresses. This did not work, for the simple reason that men would not vote for women delegates. The women soon went back to special measures, on the grounds that until real equality was achieved, legal equality would be a farce. Special measures were also a matter of dispute between the socialist women and the bourgeois feminists. (Feminism was a pejorative word for the socialists, used only for the bourgeois women.) Suitable for the jobs middle-class women were seeking, for women factory workers in the late nineteenth century the situation was quite different. Thus socialist women continued to press for protective legislation, a ban on night work for women, and the like.

Reluctant Feminists includes a number of biographical sketches, and competent-enough chapters on internal party organization, trade unions, socialist education, and the impact of World War I. The analysis on internal party structure is especially instructive, showing how women *lost* ground when, in 1908, they were finally allowed to become full party members. As women's colleges produce more women scientists per capita than co-educational institutions, so also there is something to be said for separate political organizations for women. They at least allow women to run their own show, and to choose their own leaders. Now brought into the regular structure women had to justify their projects to a nearly all-male executive committee. Women found their concerns were not taken seriously.

The book gives an unusually balanced account of the protracted conflict between Clara Zetkin, leader of the winning, socialism-first faction, and Lily Braun, who would have co-operated with the bourgeois movement. Clearly a feminist in our terms, Zetkin saw the class struggle as the more fundamental. Ultimately the salvation of the working women would be achieved by the establishment of socialism. Braun had a keener sense that many reforms could, and should, be made now. Thus she crusaded for communal kitchens, housing co-operatives, and a more equitable sharing of the domestic burden. Braun would have recruited women school teachers and clerks, who, for the Zetkin wing, were not proletarian enough. Quataert's portrayal of both women is sympathetic. To my taste she fails to do justice to Zetkin's vitriolic worst, but then she does not quite capture the fire or steel, either. Favourable treatment of Braun has been rare, for she remained outside the bourgeois movement, and was on the losing side within the socialist movement.

Quataert defends the non-co-operation decision of the socialist women. The

bourgeois feminists equivocated too much on basic issues, and they failed to get the support of liberal men in the Reichstag. For all the problems the socialist women had with their male comrades — and many are documented in this book — social democracy provided the most congenial milieu for women in Imperial Germany. Women activists in present-day socialist parties may find things have not changed all that much. The accounts of the ambivalence of German socialist men sound all too *au courant*; some, for example, were reported to be more concerned with their own domestic comforts than achieving equality for women.

Reluctant Feminists does not suffer from having been a thesis but it is written in that graceless, American, academic style. With little humour, few anecdotes, and no understatement, it plods along. If you miss the point the first time, you can be sure it will be repeated at least once. The author did include, however, one of my favourite pieces of academic trivia, on changing sexist language. When the system of party representatives was established in the German Social Democratic Party, 1892, the term for party representative was changed from *Vertauensmann* to *Vertrauensperson*. The editors of *Labour/Le Travailleur*, unless they wish to plead ignorance of our other official language, might take note. The hundredth anniversary of this step *Vorrwärts!*, coming up in only ten years, might be a suitable time for the Canadian brothers to catch up.

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David D. Roberts, *The Syndicalist Tradition and Italian Fascism* (Chapel Hill: University of North Carolina Press 1979).

HAVING SPENT MANY years reading and dissecting the writings of the syndicalist theorists who became fascists, David Roberts' goal is to correct misconceptions and simplistic interpretations of fascism. His study is an informed, stimulating, but, in the end, unpersuasive account which is often uncertain what questions it is addressing.

Roberts starts by reviewing recent interpretations of fascism (such as those of De Felice, Lyttelton, and Vivarelli) which see fascism as a more complex phenomenon than a simple capitalist counter revolution or reaction to a perceived socialist threat. These revisions are inadequate, he argues, for although they are correct in showing the petty bourgeois socio-economic origins of fascists and their initial supporters, they either underestimate the reactionary element in fascist ideology (De Felice), or mistake its essential component as nationalism (Lyttelton and Vivarelli). To counter these interpretations, Roberts demonstrates that although many syndicalists became fascists, as is well known, most were neither Mussolinians or nationalists. He insists that we take their contribution to fascist ideology seriously on its own merits, as a populist, totalitarian corporativism, "a political alternative to liberalism." To support his case, Roberts does three things. First, he writes biographies (personal and intellectual) of a number of syndicalists — Panunzio, Olivetti, Orano, and Lanzillo, primarily, but many others are covered more briefly. Second, he analyzes their ideas. Third, he traces the interplay between their ideas and fascist policy and practice as it emerged, from 1919 to 1943. The context is the political history of Italy from the fall of the right in 1876 to the fall of Mussolini in 1943.

His biographical detail and exegesis of syndicalist thought are most useful. His discussion of the way in which Italian syndicalism diverged and developed along a different path than Sorel and French syndicalism is especially clear. He finds the origins of syndicalism within the socialist party, among elements dissatisfied with its bland reformist practice. In its early years, he concludes, Italian syndicalism "was a quest for Marxist solu-

tions to 'populist' problems, and the result, inevitably, was an unstable combination of elements." (57) The experience of working within Marxism, however, led these syndicalists to a fresh approach to Italian problems and started them toward the usual synthesis which some of them later brought to fascism.

The key difference from Marxism in the theories of these syndicalists was their belief that the state, not capitalism, lay at the root of many contemporary problems. The solutions they developed were new political institutions and exhortation about values, not economic revolution. Nevertheless, syndicalists never attracted many working-class followers: the proletariat failed them in their view. They became idealistic interventionists in World War I, arguing that war could be a constructive experience which might bring forth a new international order. Here they disagreed, as they often did also in later years, with the chauvinistic aggressiveness of the nationalists. In the massive post-war political mobilization, syndicalists opposed socialist revolutionism and socialist refusal to build coalitions for peaceful political change. For their part, the syndicalists developed an alternate plan — the organization of "producers" (including bourgeois and intellectuals) into economically based syndicates, which would replace the geographical political unit, representing individuals, as political units of the state. The syndicalists then moved toward a theory of a "new populist revolution against liberalism, to be spearheaded by a new elite defined in terms of values or psychology rather than socio-economic class." (153) Among the intellectual antecedents of this new political conceptualization, Roberts identifies Francesco Saverio Merlino and Giusseppe Mazzini. The syndicalists, he writes, hoped to overcome the crisis of the liberal state "by making the people more political, by involving them more directly and constantly in public life." (173) Some of their ideas were indeed adopted by Mussolini, and some syndicalists joined the fascists in order to forward their program, in what Roberts calls an overreaction to threats of disorder and socialist failure to face them constructively.

Once the fascists were in power, syndicalists worked for the implementation of a political system based on syndicates of which both workers and employers would be members. These syndicates would be legally recognized, their labour negotiations mediated, and collective contracts enforced by the state. In reality, those institutional changes which did not conform to syndicalist theory; they were slow to be implemented, and when they were, the influence of nationalists rather than syndicalists was often dominant. (Such was the case of the Syndical Law of April 1926, in which the nationalist Rocco was most influential.) Roberts is so insistent upon the constructive and rational coherence of syndicalist ideology, as compared to other aspects of fascism, that he tends to overestimate its influence which was, by his own account, very modest. The methodological problem here is that picking out a thread — of ideas and actions — and highlighting it without adequate attention to the whole of fascist ideology and practice distorts its importance and exaggerates its coherence.

Robert's work suffers also from lack of definitions and systematic development of his argument. Although he calls them left fascists, populist fascists, occasionally neo-syndicalists, it is hard to see the people he writes about as anything but a rather varied group of writers and politicians. Sometimes they are, in his terminology, post-liberal, sometimes modern, sometimes totalitarian. On p. 267, Roberts tells us that totalitarianism is not merely the invention of cold war political scientists: it was the proud label invented by Italian Fascists themselves. But what is totalitarianism? Not a system, he writes, but "a post-liberal direction." To move in this direction means "to extend the sphere

ended up regimenting the working class, undercutting the possibility of popular participation in decision making, and enhancing the power of the traditional bureaucracy within the state." (307) Yet he again insists on what he sees as the constructive element of fascism, derived from syndicalism, that it was not a revolt against modernity, but a revolt to promote modernization. Here he favourably contrasts fascists to the Nazis, for left fascism at least offered "a more plausible, rational, forward looking blue print for change." (322)

Two more fundamental problems may be noted in conclusion. First is the ineffectual style of presentation and argument. Roberts combines narrative political history, personal life histories, intellectual analysis, but he moves back and forth uneasily among these methods, with much awkward repetition and many ungainly transitions. He writes about ideas and practice without establishing clear boundaries between them. Although he generally eschews causal explanation, he occasionally slides into causal claims, as when he declares that his problem is "to determine . . . operative causes of the rise of fascism." (24) More disturbing still is his willingness to call on loose psychological explanations. For example, he characterizes Italian politics in the pre-war period as a politics of pessimism, which he attributes to Italy's political inexperience and social fragmentation. (Here the unspoken comparison is to the politically experienced, and presumably socially unified — France of the Dreyfuss Affair, liberal England whose "strange death" George Dangerfield so evocatively described? — northern parliamentary regimes.) He sees the political crisis of the post-war period as a consequence of inflation, and accelerating industrialization, both producing the "classic insecurities [which?] of the lower middle class." (193) He sees the left fascists as "extremists with a low tolerance for the ambiguities and conflicts of genuine politics. . . . The individuals who became syndicalists and left fascists had in common personal values and psychological needs, not derived directly from their socioeconomic needs," (269) and it is to these personality traits that he attributes their extreme reaction to the post-war crisis. Perhaps such an interpretation — provided it had, at minimum, much more systematic and full textual and biographical evidence — could stand in the realm of ideas. It is, quite simply, inadequate as an explanation of a social movement. It is a long leap from individual psychology to collective action, one that must be filled in with organizational detail and examination of political behaviour of groups, not ideas. Roberts is making a causal argument with inadequate evidence. Thus, although Roberts' study provides a thoughtful exegesis of syndicalist and left fascist ideology, it does not persuade when it moves beyond intellectual history to politics.

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Richard B. Day, *The 'Crisis' and the 'Crash': Soviet Studies of the West (1917-1939)* (London: New Left Books 1981).

STALIN'S COUNTERREVOLUTION of the late 1920s, together with the rise of fascism in Europe and the prevalence of anti-communism in the West all combined to bury the political and intellectual legacy of Bolshevism so deep that portions of it are still being excavated 65 years after 1917. Part of the post-1968 "re-birth of marxism saw a concerted effort to revive the debates and theoretical contributions of a diverse collection of pre-Stalinist and pre-fascist marxists,

from the Frankfurt school to the council communists, Pannekoek and Gorter, and from the Austro-Marxists to little known Russian marxists like I.I. Rubin (in economics) and E. Pashukanis (in law). The rescue of their work from obscurity, at least for marxists in the west (and particularly for those working in the English language), has contributed enormously to the development of marxist theory. The rich ferment of ideas that surrounded the international working-class movement in the era of Bolshevism have enabled some marxists to build on the accomplishments and failures of the past decade and failures of the past despite the absence of a mass international workers' movement devoted to revolutionary socialism today.

The revival of English Canadian marxism in the last decade and a half has centered around a few select theoretical and historical problems: the state, the national question, capitalist development in Canada, and working-class consciousness and organization through history. Few marxists working in English Canada have concerned themselves with more general theory, and only a handful have followed the debates in marxist economics. With his new book on Soviet economic debates about western capitalism between the wars, Richard Day has contributed to our meager knowledge of the lively controversies among marxist economists in Russia, and hopefully raised the profile of marxist economics among historians and social scientists. Most importantly, *The 'Crisis' and the 'Crash'* provides a valuable — though flawed — introduction to what may become the central theoretical and political preoccupation for Canadian marxists during the 1980s: capitalist crises.

Writing in a history of ideas vein, Day's book systematically reviews the major positions and analyses of the Soviet marxists as they analyzed the state of the world capitalist system. In less than 300 pages of text he marshals some 900 odd references to original Russian sources — including monographs, theoretical journals, and newspapers — to present a convincing account of the triumph of the "neo-Luxemburgist" school of economic analysis (most identified with Stalin and Varga) over the "Hilferding tradition" (associated with Hilferding, Bukharin, and Lenin) which predominated immediately after 1917. This central theme of the book provides the key to the title: the "Crisis" and the "Crash" refer respectively to the Hilferding tradition's focus on period crises stemming from the anarchic character of the capitalist business cycle, and the "neo-Luxemburgist" concern with the general crisis and terminal crash of capitalism because of chronic underconsumption. (In marxist parlance, a periodic "crisis" refers to a short-term downturn in capitalist production which is self-limiting and therefore cyclical, since it generates its own conditions for recovery. A secular crisis or "crash," on the other hand, stems from a long-term downward trend in the system from which recovery cannot be assumed without massive structural change — change that either boosts aggregate demand (the underconsumptionist view) or restores the rate of profits.)

In his presentation of Soviet debates on the "crisis" and the "crash" of Western capitalism, Day does little to foster our understanding of the connection between capitalist business cycles, and the longer term epochs of capitalist development delimited by structural overhauls of the system. To understand why, the crucial first chapter of his book, which presents his theoretical introduction to marxist crisis theory, has to be evaluated.

Chapter one summarizes Marx's theory of the business cycle and the process of capitalist accumulation through expanded reproduction/periodic crises (based largely on the discussion in Volume II of *Capital*). Day does not discuss Marx's formulation of the law of the tendency for the rate of profit to fall or its countervailing tendencies from volume III of *Capital* — a point to which I will

return. From Marx, Day turns to the account of capitalist development and crisis provided in the writings of the founders of the underconsumptionist tradition: Engels, Kautsky, and Luxemburg as compared with that developed in Hilferding's *Finance Capital*, and the writings of Lenin and Bukharin on imperialism. The latter group analyzed the institutional developments of monopoly, finance capital, and capital exports to the colonies to explain the alteration of the classic capitalist business cycle and the rise to prominence of new forms of capitalist competition (for example, military competition by national blocs of capital for colonies) that compounded the contradictions caused by continuing price competition.

By contrast, first Engels and then Kautsky saw the rise of monopoly and the development of imperialism as a development which replaced the periodic cyclical crises of classical capitalism with a generalized and chronic shortage of external markets of goods. Here the business cycle was thought to have given way to a period of chronic stagnation leading to the final and ultimate "crash" of capitalism, since the supply of markets outside the capitalist system was being exhausted. Rosa Luxemburg raised this thread of argument to a new high with her classic formulation of the position in *The Accumulation of Capital*. She attempted to prove Marx's reproduction schema from volume II of *Capital* internally inconsistent and inadequate, hoping to bolster the claim that the root of capitalist crises lies in the restricted aggregate demand that the system generates.

In Volume II of *Capital* Marx had divided the total social capital into two sectors of production, capital goods production in Department I and consumer goods production in Department II, demonstrating how the system was able to reproduce and expand itself. Studying Marx's analysis of capitalist production, reproduction, and consumption, Luxemburg asked a most pertinent question: after the income of capitalists and wage earners has been expended in the purchase of consumer and capital goods produced, from whence comes the additional demand for that portion of the commodity capital that embodies the surplus value? For her, as for all subsequent underconsumptionists, the gap in aggregate demand could only be filled by a third — a market outside the the capitalist system. The absence of such a demand would guarantee persistent crises as masses of commodities went unconsumed, and huge portions of surplus value went unrealized, leading inevitably to the crash of capitalism. Economic stagnation and revolution would be the inevitable result of the exhaustion of new noncapitalist markets, as imperialism subordinated the entire world.

While Luxemburg saw the demand for consumer goods as determinant over capitalist production, Marx took the opposite view. As Luxemburg's many critics have pointed out, Marx discounted the importance of external markets because capitalist production produces its own internal markets. In *Capital*, a demand gap and the overproduction of commodities only arises when there is a failure by capital to use that part of the social product which represents surplus value as investment in new and expanded productive capacity. In other words, when a depressed rate of profit weakens the motive for new investment, crises of realization may occur, but are only the symptom of a more fundamental problem. By misunderstanding Marx's methodological purpose in volume II of *Capital* and by misreading his argument, Luxemburg *transformed underconsumption into the* only contradiction of capitalism.

This underconsumptionist view of capitalist crises has come to enjoy wide popularity among marxist theoreticians, from Varga and Stalin to Baran and Sweezy and beyond. Formulated initially as a theory which guaranteed the crash of capitalism once the whole world had been

conquered by imperialism. Luxemburg's theory was used by Soviet economists like Stalin and Varga to legitimate the view that capitalism could avoid crisis and breakdown through state manipulation of the levels of aggregate demand. In this latter capacity, the "neo-Luxemburgism" of the Soviets became synonymous with efforts to achieve a "peaceful co-existence" (the early version of "detente") with the capitalist west. Attention was shifted away from the internal contradictions of capitalism to the external threats posed to the system by the decline of external markets and the rise of the "socialist" world. Not surprisingly, this economic theory was easily combined with a reformist political practice in the west — where working-class revolution was replaced by the parliamentary road to socialism led by a "progressive" anti-monopoly coalition of class forces.

By documenting the rise to orthodoxy of the "neo-Luxemburgist" interpretation of marxist economics in Stalin's Russia, Day has provided a valuable explanation for the widespread popularity of underconsumptionism (and some of its later guises as "dependency theory") since the 1930s. *The 'Crisis' and the 'Crash'* adds important evidence to the growing understanding by marxist scholars that Stalin's victory affected a paradigm shift in marxist economics. Attention shifted from a growing understanding of value as a social relation focused on the concept of abstract human labour (see Rubin's writings) towards an interpretation of value as embodied concrete labour (see the writings of Stalin on why the law of value ostensibly operates under socialism); away from a theory of capitalist development rooted in production towards a "neo-Smithian" theory concerned with the sphere of circulation (as recently explained by Robert Brenner), and away from a crisis theory centered on production towards one based on consumption and distribution. Only the excavations of Day and others have enabled contemporary marxists to begin to appreciate the massive assault on Marx's *Capital* unwittingly launched by Engels and Luxemburg, but carried to its political conclusion by Kautsky, Stalin, and their followers.

As "neo-Luxemburgism" gained prominence in Stalin's Russia, Russian marxists lost their ability to analyze the development of western capitalism, and eventually their revolutionary critique of capitalism, as a form of society. Crucial to the success of "neo-Luxemburgism" — a theoretical stance which Lenin totally rejected — was the division of the "Hilferding tradition" into left and right wing versions. In examining the institutional developments associated with the rise of imperialism, Hilferding's *Finance Capital* (1910) was ambiguous about the extent and implications of the trend towards monopolization and away from domestic capitalist competition. In post-1917 formulations by Hilferding and Bukharin this ambiguity became the basis for the view that capitalism had eliminated its domestic contradictions. Internally generated crises could no longer arise, because the anarchy of capitalistic production had been overcome through the organization and planning imposed upon the system from above, by the state and finance capital. In this "right" version the Hilferding tradition led to the reformist conclusion that the last major contradiction — the threat of war — could be overcome with the working-class capture of the existing "state capitalist trusts."

In the "left" version of Hilferding's argument, Lenin, Preobrazhensky, and some lesser known Soviet economists of the late 1920s claimed that the concentration and centralization of capital had not totally replaced domestic competition by monopoly and planning. They argued that the addition of monopoly to the non-monopolized sectors of the economy compounded the existing contradictions of capitalism. When the "neo-Luxemburgist" and the "right" interpret-

ers of Hilferding abandoned the study of the capitalist business cycle as irrelevant to the "*general crisis*" *arising* from the inadequacy of markets, or rendered unnecessary by the planned character of state capitalism, they jettisoned the economic contribution of Lenin. With the apparent "stabilization" of Western capitalism after 1922 the debate among Soviet economists led to "a disintegration of Lenin's theoretical synthesis and a new polarization in Marxist economic thought." (74) Specifically, what both the "neo-Luxemburgists" and the "right" interpreters of Hilferding rejected was the need to follow the business cycle as the source of periodic crises in capital accumulation.

In *Capital*, Marx saw the business cycle and the continual reproduction of capital (punctuated by crisis) as the product, in part, of the disproportionate development between Departments I and II. The shifting rate of profit and the fluctuating demand for capital and consumer goods forced the system through an inexorable circuit of boom, slump, crisis, and recovery. In Lenin's view, neither crises of disproportionality caused by the anarchy of capitalist production, nor the business cycle had disappeared after World War I. (This explains his partial abstention from the immediate post-revolutionary binge of catastrophism which swept the leadership of the Bolsheviks.) Though he believed that capitalism had entered a fatal downturn, Lenin did not believe its decline would be free of cyclical upturns, or inevitable. Yet most of the Bolsheviks believed the periodic crises of the business cycle had been replaced by a prolonged period of secular crisis leading to the terminal crash of capitalism. As Day points out, this catastrophism repudiated the analysis in volume II of *Capital*, detaching the theory of imperialism from the theory of the business cycle.

Through eight chapters Day provides an account of the shifts in the relative strengths of the "neo-Luxemburgist" and Hilferding traditions of analysis. His subtle and seemingly comprehensive narrative of every twist and turn in the debate of the 1920s — only the final chapter is devoted to the internal debates of the victorious "neo-Luxemburgists" during the 1930s — defies summary in a short review. Suffice it to say, that it remains consistent with the theoretical framework developed in Chapter one. Therein lies his problem, despite the very valuable discussions of Preobrazhensky's *The Decline of Capitalism*, the attempts to revive cycle theory, and other important developments previously unavailable in the existing literature.

The 'Crisis' and the 'Crash' is flawed by a preoccupation with periodic crises of the business cycle to the virtual exclusion of any consideration of the long term tendency for the rate of profit to fall as discussed in volume III of *Capital*. Day clearly rejects the "neo-Luxemburgist" analysis as incompatible with Marx's project, and rightfully so. But his simple equation of Hilferding's disproportionality theory of crisis with Marx's own is extremely problematic. As other commentators have suggested, Hilferding's argument focuses on the institutional changes that affect the interrelations between Departments I and II (for example, the concentration and centralization of capital, the development of finance capital, the growth of imperialism and the export of capital, etc.) without addressing the problem posed by the tendency for the rate of profit to fall. Thus, the Hilferding tradition incorporates most, *but not all* of the factors needed to explain the cyclical movements and secular periods of capitalist development.

Because he uncritically accepts the left variant of the Hilferding tradition as the best available version of Marx's analysis of crises, Day misrepresents Marx's position. A more satisfactory account of the problems posed by the Soviet debates would need to recognize

that Marx's theory of capitalist crisis and development includes a discussion of the insufficient production of surplus value, in addition to an account of the uneven accumulation of capital across departments and the overproduction of commodities.

The Hilferding tradition, in both its "left" and "right" variations takes no account of Marx's arguments in volume III of *Capital*. Day does Lenin a disservice by pretending that *Imperialism, Highest Stage of Capitalism* was something more than a political intervention geared to mobilize the revolutionary sections of the working-class movement against support for World War I and those Social Democrats who took a more compromising position. True, Lenin's book had the considerable merit of emphasizing the uneven character of monopoly development on the level of both the national and world economies. But Lenin's writings — and here I include his earlier contributions on the development of capitalism in Russia — do not contain a theory of crisis which goes beyond the analysis of disproportionality to mesh the business cycle with the falling rate of profit. For example, while *Imperialism* examines the institutional relations between industrial firms and finance houses, these developments are not analyzed in terms of Marx's categories dealing with credit, the turnover of capital and so on.

These shortcomings of even the best Soviet economist — Lenin — made it very difficult for the Bolsheviks to analyze the economic developments of the 1920s on the basis of Marx's theory. Neglect of the importance of credit in temporarily extending the business cycle and the failure to appreciate how the destruction of capital through war could temporarily offset the falling rate of profit plagued Soviet economic debates during the 1920s. Unfortunately, Day's account of their debates does little to help us get beyond the confusion among Soviet marxists during the 1920s. Ultimately, he fails to recognize that even the "left" interpreters of Hilferding did not have the tools to link the cyclical crises of 1920-21 and 1929-33 with the secular crash that was the great depression. Those tools could be found by incorporating some of the insights of volume III of *Capital*.

Because he skates over the difficulties of the "left" interpretation of Hilferding — a procedure which is facilitated by an unwillingness to comment on some of the recent secondary literature on Marx's crisis theory — Day only provides us with an excavation and interpretation of Bolshevik theory, rather than a critical development of marxist crisis theory. Nonetheless, for those dedicated to extending Marx's argument and his political project, *The 'Crisis' and the 'Crash'* is a welcome introduction to past debates.

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Seweryn Bialer, *Stalin's Successors. Leadership, Stability and Change in the Soviet Union* (Cambridge: Cambridge University Press 1980).

STALIN'S SUCCESSORS was published two years before the death of Leonid Brezhnev. Its author did not foresee that Yuri Andropov would be selected as the new General Secretary of the Communist Party of the Soviet Union. Andropov, in fact, is referred to only briefly at two points throughout the text. Nonetheless, anyone with an interest in the current political succession in the USSR would serve themselves well if they were to consult this book.

Its author, Seweryn Bialer, currently a professor at Columbia University, is one of the leading authorities in the United States on Soviet politics. He has focused his book upon "the content and interrelationship of leadership, stability and change in the Soviet Union." It deals primarily with the period of Brezhnev's leadership but contains a brief look at the past and contains a more cursory speculation on the future of Soviet politics.

In so far as the past is concerned, Professor Bialer argues that during Stalin's lifetime a distinct and complex system of rule was established in the Soviet Union. In taking this position the author shows that he is not afraid of controversy for Soviet studies specialists are locked in a major historiographical dispute over the degree of continuity and discontinuity between Lenin's and Stalin's periods of leadership. Irrespective of one's views on this matter, it is still possible to admire the clarity and economy of Bialer's analysis of "mature Stalinism." This system of rule expires with its creator in Bialer's estimation because the Soviet elite could no longer bear the insecurities which attended pursuit of a political-administrative career during Stalin's lifetime. This position places the author solidly in yet another scholarly and political controversy as to whether the contemporary Soviet Union is viewed more appropriately as a totalitarian or an authoritarian state. Professor Bialer favours the latter view.

He emphasizes that especially during what we might call the Brezhnev years the Soviet Union has beome a "highly governable and routinely functioning bureaucratic state." One of the author's principal objectives is to try and account for the stability of the Soviet state during this period. This is a goal, in my view, which he succeeds largely in fulfilling. The author pinpoints four factors which underlie this stability and his discussion of each of these is very cogent. These are: the performance of the regime with special reference to its ability to improve Soviet citizens' standard of living, the peculiarly modest material or consumer expectations of the Soviet population, the changing nature of bureaucracy and political participation in the USSR, and the system's generally high level of social mobility and openness of recruitment into the political-administrative elite. This segment of the book contains as well remarkably even-handed discussions of the demonstrated ability of those who administer the Soviet state to foster a sense of legitimacy in their regime and to cope with the pressures which arise from the multinational character of the Soviet population.

This latter feature is the one which Professor Bialer reckons contains the greatest potential to undermine the current stability of the Soviet system. Economic and demographic trends indicate that problems are deepening which will test seriously the political abilities of Soviet leaders throughout the next decade. Reference to any specific personality is avoided quite deliberately here for as Professor Bialer demonstrates it is an entire generation which must soon relinquish power and make way for another age group at the political summit of the Soviet system. The author's analysis of the entire succession issue is outstanding particularly in its uses of historical and sociological perspectives. Especially interesting are Professor Bialer's surmises about the generation which began to pursue political careers after Stalin's death. He anticipates that they will prove to be a reform-minded group once they assume the top-most positions in the Soviet system. At the same time, he expects they will be divided over the sorts of reforms they favour. At the moment, this generation is well-represented in the leaderships of the many regions in the USSR, but the all-important positions in the central apparatus are staffed by men who are contemporaries of Brezhnev. These latter, it is argued, exhausted their own reformist impulses long ago and have drifted along since resisting change and innovation.

The final portions of the book address the central issues both foreign and domestic with which the incoming leadership will have to deal. It is made clear that if these are to be managed successfully bold and imaginative responses will be required. Professor Bialer wonders openly whether these will be forthcoming in a period of

complicated transition from one set of leaders to another.

Each distinct segment of this book makes a valid and important contribution to our knowledge of the Soviet system. There is room, however, to complain about various of its features. One of these is that the book's parts are too distinct from one another. This is particularly noticeable in the segment which deals with international affairs. It seems here that the author wants to try and pack a short course on Soviet foreign policy into his text. (It is a course, though, that one cannot help wishing that Ronald Reagan's appointees to the American foreign affairs bureaucracy would take.)

The author's discussion of political change in the Soviet Union is very persuasive in my view but underplays the very real mechanisms of intimidation now operative in the Soviet system. While Stalinist mass terror is no longer resorted to, the authorities know very well how to stimulate fear of loss of position and privileges, at least among the white-collar groups in Soviet society, as is shown so well in Alexander Zinoviev's recently published work.

Most of *Stalin's Successors* is written in clear workmanlike prose and occasionally its author turns a very good phrase indeed. At times, however, he lapses into what for want of a better term, I will call "sociologese" and at other points, Dr. Bialer tries to take such pains to be circumspect in his formulations on the future that he sacrifices readability through loading his sentences down with qualifying phrases.

Overall his accomplishment is striking. He has written a timely book, but his knowledge and understanding run so deep that his study is not one just for this time of uncertainty alone.

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Daniel Singer, *The Road to Gdansk. Poland and the U.S.S.R.* (New York: Monthly Review Press 1981).

FOR DANIEL SINGER, THE ROAD to Gdansk is the road from the disintegration of the Marxian socialist ideal in eastern Europe to the earliest pangs of its rebirth. It is a commonplace that Marxism has been discredited in the Soviet Union as an instrument of élite domination and in the satellite countries as a tool of imperialist control. But Singer, like many other western socialists, insists on the socialist intent of the October Revolution and refuses to believe that the revolution was a dead end; rather he looks for a revival of the socialist ideals of October in the collapse of Stalinism. He develops his thesis in series of loosely linked essays on Solzhenitsyn, the Soviet Union, the events in Poland from 1956 until the signing on 31 August 1980 of the agreement that legitimized Solidarity, and incorporates a sermon on the responsibilities of western socialists.

On Solzhenitsyn, Singer chides the western left for its failure to credit the accuracy of Solzhenitsyn's exposé of Stalinist Russia simply because what he calls Solzhenitsyn's "messianism of backwardness" is unacceptable. Since, for Singer, to bury Stalinism is to revive socialism, he urges the left to take Solzhenitsyn on the gulag seriously. Otherwise, he rejects Solzhenitsyn's critique of Marxism which, he says, is based on Soviet practice rather than knowledge of Marx, scorns his "religious equality" in favour of "real equality," and scoffs at his appeals to Soviet leaders to abandon an internationalism in which, Singer points out, they never in any event believed.

Singer introduces a more perceptive and interesting essay on the USSR with the observation that Solzhenitsyn's constituency, the peasantry, has increasingly become smaller since the death of Stalin. By the mid 1980s only 30 per cent of the population will be rural. The new urbanism is only one indicator of profound changes in the Soviet Union of Krushchev and Brezhnev. The demographic shift from village to city has been

accompanied by the prodigious growth of the working class and the steady raising of its educational level, by the ever-growing significance of education as the key to success and increasing differentiation in the party. In these changes, Singer sees new strains that create favourable conditions for the renewed growth of working-class consciousness.

In the Soviet Union, the hierarchical division of labour and the extraction of surplus value in an unequal society persist — Soviet labour remains alienated labour. Labour relations are circumscribed by wide wage differentials, an absence of political channels for the expression of discontent, and limited opportunity to advance in society through education beyond the ten-year school. As a matter of course, women are largely excluded from high-paying, high-status jobs. In those skilled professions where women do predominate wages are low. Interestingly, and no doubt rightly, Singer sees women's liberation at the very centre of any egalitarian programme in the USSR. The principal compensation for the shortcomings of labour relations has been the traditionally low productivity demands the state has made on the workers. As Singer points out the Polish joke — "they pretend to pay us, and we pretend to work" — has long been at the heart of worker-state relations in the USSR.

Singer holds out little hope of reform from the top. Since, for example, tertiary education and a party card are the keys to success in Soviet life, he predicts that the élite will jealously guard its privileges and resist the democratization of education. Furthermore, changing social conditions have left their mark on the party. Beneath the surface, the rift between the old bureaucrats and the new technocrats has steadily widened. The *apparatchiki* of the Brezhnev regime built their success around the goal of economic improvement. Economic growth defused social tensions. But the USSR has not escaped the global economic crisis. Under conditions of constraint the political advantage may pass to the technocrats. The technocrats, Singer rightly points out, are not social reformers but economic tinkerers. The changes they would bring would only increase wage differentiation and exacerbate social and sexist discrimination. The Soviet leaders must raise productivity but politically cannot offer the social and educational incentives required to pacify labour. The massive effort needed to develop the riches of Siberia and the Far East are beyond the resources of capital and manpower of a dwindling population. The decentralization of the economy by weakening economic power to the producers is unthinkable to the Soviet ruling élite. So demands for greater productivity can only increase existing tensions and focus worker discontent.

Singer does not see the intelligentsia as a force for major change. Instead, he views the intelligentsia as a fragmented group that is both a pillar and an opponent of the regime. Further developments can only widen the cracks within the intelligentsia, and only a fraction of it will recognize the need to join forces with the workers.

Turning to Poland, Singer focuses on the development of the workers' veto through the strike from 1956 to the end of August 1980. In particular, he notes the gradual drawing together of intellectuals and workers in the struggle with the state. In 1968 the intellectuals faced the government alone; in 1970 it was the turn of the workers; then, in 1976 the intellectuals formed KOR (Committee for the Defence of the Workers) to provide financial and legal assistance to worker victims of repression. Subsequently, they supported a *samizdat* press and established the flying university on behalf of workers.

Singer praises the maturity of the Polish workers' movement, its belief in its power, its self-restraint that resisted anti-Soviet outbursts, its lack of illusions, its liberation from belief in the socialist myth

propagated by the state, and its strict egalitarianism in wage demands.

The author sees no single path to socialism in eastern Europe. Conditions in each country, he points out, are strikingly different. What they all have in common, however, is a severe economic crisis and the urgent need to move from nationalization to socialism. Singer is no optimist. He does not believe that a socialist opposition will inevitably grow in eastern Europe but only that conditions favour its growth. He urges western socialists who face a related crisis in their own countries to look for allies among the emerging socialist movements of the East.

Singer's exposition is as interesting for what it omits as for what it includes. Like many socialists he seems ideologically unprepared to deal with the consequences of nationalism. In Poland, nationalist feelings militate towards resistance to a Soviet-backed regime; in the USSR, they are skillfully manipulated to reinforce the regime. Whatever its consequences, nationalism cannot be overlooked as Singer does. Singer points out that Solidarity claims to speak for the whole nation and interprets the claim in a Marxist direction. At the same time, he laments the appearance of banners bearing the Pope's portrait instead of Marx's at the head of Solidarity marches, a fact which he attributes to an as yet immature socialist consciousness. But does Solidarity speak for a Marxist future, or for the the historical and religious wrongs of the past, or simply the economic wrongs of the present? In any event, it does not speak with a single voice, as Singer suggests. What apart from socialist faith convinces Singer that Solidarity is a force for Marx's classless society? It is not inconsistent with the facts to argue that at the heart of Solidarity is the trade unionist's struggle with the employer, the state. Is the Soviet economy in the desperate straits that Singer describes, or can some combination of careful planning and foreign investment tap the wealth of Siberia and the Far East? The answers are uncertain, but Singer does not even pose the questions. He has written an interesting and instructive book that ought to be read with one's ideological blinkers off.

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Perry Anderson, *Arguments Within English Marxism* (London: New Left Books 1980) and Bryan D. Palmer, *The Making of E.P. Thompson: Marxism, Humanism, and History* (Toronto: New Hogtown Press, 1981).

THESE TWO BOOKS are not about the process of labour or social history. Rather they are, in part, about the discipline of history as practised by one man — Edward Palmer Thompson. That one historian has had two books written about him in the space of two years is testimony enough to his impact. Author of *William Morris: Romantic to Revolutionary* (1955, revised 1977), *The Making of the English Working Class* (1963), *Whigs and Hunters* (1975), *The Poverty of Theory* (1978) and *Writing by Candlelight* (1980). Thompson has also become a most effective spokesperson for a socialist humanism, the traditional British legal liberties, and a nuclear free Europe.

Perry Anderson's book is a response to Thompson's recent attack on the Marxism of Louis Althusser in *The Poverty of Theory* (1978). Anderson ably defends Althusser's political record, if not his theorizing, against the Thompsonian onslaught. But the weakness of the Anderson book lies partly in its structure. He does not communicate his position as clearly as he might have. He begins with a pretentious and superficial chapter which defines history as a science in opposition to the more restrained claims for history as an "approximate" discipline on the part of Thompson. After this flourish Anderson proceeds with a mixture of obfuscatory language, Marxist jargon, and the

dropping of dozens of names of continental European Marxist scholars.

In the area of British labour history Anderson takes issue with several aspects of Thompson's *The Making of the English Working Class* (1963). He doubts that such a class could have been formed by the 1830s only to be remade some 40 years later. (44-7) Anderson sees this as a great contradiction and marshalls studies of various labour historians of late-nineteenth-century Britain to his side. Unfortunately, for Anderson, the concept of the dual nature of a working-class movement in Britain is a very old and serviceable one. It certainly predates Thompson's efforts and its validity is undented by Anderson's objections.

This is Anderson at his worst. But at his best he is the most effective left-wing critic of the Thompson approach to British labour and social history. Anderson alternately challenges the reader to examine Thompson's sustained passages of solid criticism from a Marxist perspective. His attack on Thompson's view of Walpolean England found in *Whigs and Hunters* (1975) is very perceptive. Thompson is shown to have an exaggerated view of the corruption of Walpole's regime which is largely drawn from the ravings of Tory critics.

There is also a long section which stakes out an alternative to the Thompsonian view of William Morris' place in the evolution of left-wing thought in Britain. At a more personal level, Anderson gives his side of the story about what happened to the editorial board of *New Left Review* when Thompson and the "old guard" departed in the early 1960s (or were they purged?).

Bryan Palmer's book also suffers from an ill-structured set of chapters. He appears to have had a manuscript for a book about the background and thought of Edward Thompson, then he read Anderson's attack. This apparently caused Palmer to leap to the defense and attach a preface to deal with Anderson's critiques. This makes for an extremely unwieldy book for the uninitiated reader. Both books suffer from this defect as their initial chapters assume an intimacy with the internecine battles of the post-war British left from the outset.

Palmer appears sensitive to the charge of "guruism" with regard to his relationship to his subject. After declaring that his "sympathies lie, unambiguously, with Thompson" (9), he parts company with him "in certain ways" in the practice of politics (131). It is not clear from what follows in what way Palmer's politics differ from those of Thompson but earlier on he writes:

> ... Thompson forged a conception of both history and politics, and of their relationship. One cannot, in all consistency, praise his historical writing and damn his politics. (63)

This inconsistency is never resolved in the book and, moreover, it is set out as a criticism of Perry Anderson which, I think, is an unfair one.

What Palmer has done is to provide us with a useful survey of Thompson's intellectual and political background. Previously this information was scattered in Thompson's writings and interviews. Palmer has pieced it together and further placed it in contexts of British, Canadian, and American labour history. The encyclopedic nature of his footnotes attest to a breadth of scholarship which alone makes his book worthwhile. Of the two books, his is the more successful in placing Thompson's work in the context of British post-war Marxism.

Both the Anderson and the Palmer books have a number of things in common. Each attempts to provide a bibliography of Thompson's writings. While neither purports to be complete, it is only by comparing the two lists that the reader can gain an appreciation of the prolific nature of Thompson's contribution as a political commentator.

The two books provide a useful common function in a second respect. After reading these works it is not possible to

misinterpret Thompson (as has apparently been the case in some quarters) either as a romantic or as a former Marxist.

Thirdly, both authors have a different view of the watershed year of 1956 than does Thompson. 1956 was the year of the Twentieth Communist Party Congress in Moscow, the Suez crisis, and the Soviet intervention in Hungary. These events led to a rupture in the British communist party with many writers and academics, including Thompson, making their exit from it. For Thompson, and some other historians, this marked the time when they began to reason and British labour history has been but one beneficiary. But Palmer would diminish the importance of 1956 by antedating Thompson's emergence as a critical thinker to the early 1950s.(36) Likewise, for Anderson, the importance of 1956 is diminished as far as Edward Thompson's intellectual development is concerned. (118, 145-6) For Anderson, Thompson is ignorant of Trotsky's criticism of Stalinism on both sides of the 1956 watershed.

In the Thompsonian view, 1956 represents a fundamental turning point after which no quarter can be given to advocates of rigid theoretical Marxism in the political or academic arena. Some may be surprised by this uncompromising attitude, but it is one which is consistent with Thompson's intellectual route of the past quarter century. By contrast we have the intellectual route of the past quarter century. By contrast we have the somewhat surprising words of Anderson, the defender of theoretical Marxism:

> Thompson's work as a historian is, simply, incomparable. What rather needs to be said is that the Left as a whole . . . benefits rather than suffers from a diversity of interests and outlooks. (206)

Finally what are we to do with this man Thompson? He writes in brilliant style from a humanistic perspective, yet in a 200 page relentless assault on Althusser's thought he declares that his own work is a "polemical political intervention and not an academic exercise."(*The Poverty of Theory*, 385n). Analysis of Edward Thompson is both difficult and premature, as Bryan Palmer reminds us. (29) For Thompson has the knack of continually surprising us with new ventures that turn out to be rigorously consistent with his post-1956 vision of the world.

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Harold Perkins, *The Structured Essays in English Social History* (New York: Barnes and Noble 1981).

THIS COLLECTION OF ESSAYS, occasional and scholarly, from the pen of one of the pioneers of the new advance of English social history, spans three decades of publication. The first, written when the author held the only separate academic appointment in the discipline, is an attempt to sketch the potential field of the subject; the last is a backward-looking survey from the professorial chair of today over the accomplishments since then. This is courageous, for it enables us to see that the subject has not developed in quite the way envisaged by the author; it has advanced in a more fragmented and also in a more radical form. He envisaged it in 1953 as study of small comunities, delimited by the student, in terms closer to those of social anthropology than of sociology or economics, encompassing a consideration of that community's physical environment, governing ideology, social structure, political economy, social conflicts, and social significance; all of course in a process of social change as they move along the historical axes from feudalism to post-industrial society. The communities in question, he emphasized, need not be geographical but might be occupational or institutional or what have you — so long as they avoided antiquarianism and related clearly to the mainstream movement of history. What he has to chronicle at the end includes

some of that — especially the enormous growth of significant local history, of deviant groups and of elites, much of it in his own department. But striking developments have also been made which depend on shifts of methodology — particularly the growth of demographic studies at Cambridge, and that of Oral History at Essex of which he particularly approves. There have been also the studies of the moral economy school under E.P. Thompson, which specialize in subcultures; and the History Workshop Movement led by Raphael Samuel, which guides non-professionals to the study of their own environment. Add to this the renewed interest in working-class autobiography, and in detailed social reporting such as that of Mayhew and Booth, together with new fields opened in women's history and history of the family and one has a range sufficiently illustrating that role of surprise development which, historians know, makes it nearly impossible to forecast the future with any accuracy. Perkin certainly knows it, for he comments on it here in "The History of Social Forecasting." Futurology is a pastime with a peculiar relevance to history but mutation of any one variable is likely to plunge the complex game into chaos. In general Perkin shows no resentment of the Left-leaning trend of modern English social history. For one group of historians, however, he reserves a special critique in his "Middle-class Intellectuals and the History of the Working-Class." These are the intellectuals such as those of the Althusserian schools who "think that theorizing about the working-class is an adequate substitute for objective study." In his scathing attack on the condescension of those historians who regard working-class reality from a supercilious height, seeking to fit the facts to a theory, Perkin makes it clear that he is aligned with "objective Marxists" such as E.P. Thompson and Eric Hobsbawm and with those like Raphael Samuel and Paul Thompson who seek a basis for historical knowledge in the records and memories of the working class themselves. Perkin describes himself as "a middle-class intellectual of working-class origin." But it is in his approach to the study that he shows most empathy with historians of the labouring class for as a practising craftsman Prof. Perkin's real interest is in the elite — most particularly in the landowning nobility and its interaction with the English middle-class. The most gripping of his essays are on this theme, and one sees constantly the underlying belief that land-ownership, its forms, its psychology, its politics, underlie his interpretation of social phenomena. For him the root cause of the Industrial Revolution and the reason for its happening in England lies far back in the defeat and early dispossession of the English peasantry by the great landowners. This created a tradition of unfettered property in land, an elite class open to attainment through wealth, and a class of landless poor who learned early to spend their surplus on consumption goods rather than ploughing it back into small holdings. Essential to this interpretation is the fact that it de-emphasizes exploitation and class antagonism or rather pushes it back to a remote period where present day workers find little basis for self-identification. Industrialism by this rubric benefitted every class — except the extinct peasantry. In "Land Reform and Class Conflict in Victorian Britain," landownership is seen as the touchstone of nineteenth-century party politics: alarm over the attack on great landowners as the reason why many of them left the Liberal Party for the Conservatives in the 1880s. The detailed study of the progression from liberalism to socialism which forms the basis of "Individualism versus Collectivism" with its level-headed conclusion that the real leap in ideology is between large-scale state provision of a public service and public ownership of the means of production, is undertaken in the first place to show that the Liberals who moved to Conservatism

at the end of the nineteenth century did so in the grip of a misconception — a fear for their private wealth which was totally unnecessary in the light of the really moderate nature of Liberal reform. Similarly in a survey of the social character of Victorian seaside resorts based on student theses, he indicates that the differences between them can be traced back to the tone of the dominant elites, itself determined by the underlying pattern of land distribution.

This whole question of social mobility has a concomitant interest for him and he explores it in two of these essays — "The Professionalization of University Teaching" and "Who Runs Britain?" He is still most interested in the movement between middle and upper classes and sees with pleasure the shift of university training from the needs of the aristocracy to those of the middle class in the nineteenth century, but seems to view with less enthusiasm the rise of the modern mass universities. In "Who Runs Britain?" he researches with meticulous care the shifts of elite control *within* the upper classes; his conclusion here is that the middle-range elite is now drawn from a much wider band of society, distinguished less by great wealth than by the kind of income which can afford private shools and university education for their children. The very top elite, however, remains a charmed inner circle of great landowners together with bankers, brewers, and shipowners (whom he elsewhere distinguishes as the *older* bourgeoisie) educated at the top "public" schools, controlling the banks, insurance companies, and civil service.

The other elite in which Prof. Perkin shows a polemical interest in this book is that of the labour aristocracy. Just as patronizing intellectuals obscure the facts of working-class history, he implies that the middle-class revolutionaries of the far left parties confuse the real aims of the British proletariat, which are best understood and articulated by the top echelons of the working class itself. There may be truth in this but Prof. Perkin confines his discussion to attacking the thesis that the labour aristocracy is timid by conditioning and isolated from the rest of the working class by the self-generated bureaucracy of the trade union offices. He does not offer here any analysis of his own to replace it, although these opinions are obviously linked to his expressed approval for oral history and working-class autobiography where the working class may speak for themselves. Politically this connects him with the position of British unions and the Labour Party against the far left.

In all Prof. Perkin is inclined to see English class society as a hierarchy of many finely distinguished gradations rather than as a setting for massive conflict. He is interested in the exploration of those gradations, especially at the upper ranges, but he also approves and encourages the investigation of the lower ones by new methodologies. He is inclined to emphasize the role of elites, including that of the labour aristocracy. Marxism he recognizes as a socio-political rather than an economic doctrine, but his own work rests on firm economic foundations. We may perhaps paraphrase his self-description by calling him a "working-class intellectual with middle-class interests."

Mavis Waters
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A.D. Murray, ed. *John Ludlow: The Autobiography of Christian Socialist* (London: Frank Cass 1981), and Chushichi Tsuzuki, *Edward Carpenter, 1844-1928: Prophet of Human Fellowship* (Cambridge University Press 1980).

THE ECONOMIC AND SOCIAL crises that led nineteenth-century Englishmen to question the viability of capitalism drew the attention of the first historians of socialism. Only in the last two decades have academics paid serious attention to the cultural, religious, and moral concerns of the pioneer socialists. The autobiog-

raphy of John Ludlow (1821-1911) and the biography of Edward Carpenter (1844-1929) are accordingly valuable to the extent that they offer insights into the issue of raised consciousness that propelled two respectable middle-class, professional men into the labour movement. These two fascinating individuals are, of course, not without interest in their own right but few beyond those seeking information on social reform in the last century will likely peruse these volumes.

Ludlow was an important force in the Christian socialist cause sparked by F.D. Maurice. Though the Christian Socialist impact on the labour movement was later to be minimized by the Webbs, Ludlow did in fact do yeoman service in pushing the voluntary cooperative movement, in campaigning for legislation to provide legal safeguards for it, and in improving the position of friendly societies. In the 1870s he served as secretary for the Northcote Commission on Friendly Societies, drafted the 1875 Friendly Societies Act, and sat from 1875 to 1891 as registrar of the societies. Moreover in assisting Maurice and Kingsley in the establishment of the Working Men's College and through tracts and pamphlets, Ludlow won many to the socialist cause.

A biography of Ludlow that drew heavily on the manuscript autobiography was produced by N.C. Masterman a number of years ago. It was criticized at the time for not fully explaining the elusive, obscure character of Ludlow. One would hope that the autobiography itself might help to clarify matters but it rarely does. The first third, devoted to Ludlow's early family life, does provide a refreshing picture of a nineteenth-century family that was apparently not subjected to the whims of any domestic tyrant. In addition Ludlow, who spent his first 20 years in France, makes a series of interesting comparisons of French and English political and cultural life. But the meat of the book is in the later two-thirds focused on the years 1848 to 1854, the years of Ludlow's participation in the Christian Socialist movement. Unfortunately one comes away with only the vaguest idea of what Ludlow and Maurice were about. They were appalled by poverty and alarmed by the threat of atheistic socialism but where one proceeded from there was unclear. This haziness could be a reflection of the vagueness of the doctrine propounded by Maurice. Much of the blame must be attributed, however, to Ludlow's own ambiguous state of mind. He repeatedly declares Maurice to have been the greatest personality he ever met, but punctures such expressions of praise with repeated references to the man's timidity and confusion. The same two-faced quality comes out in Ludlow's portraits of his fellow Christian Socialists like Kingsley and Neale whom he lauds and betrays on the same page, in his account of the revolution of 1848 which he glorifies and denigrates, in his reports of the workers of the cooperative societies who are honest and hard-working at one moment and "levanting" with the coop's funds the next. One wonders if the autobiography does justice to its author because the picture it presents is chiefly that of a restless Christian celibate bachelor barrister both drawn and repelled by the men and movements which he hoped would give some meaning to his life. Skillful editing would not only have made the material more accessible to the general reader; it would also have made Ludlow's actions more intelligible. This has not been provided. A six page introduction and a handful of footnotes is simply not sufficient to rescue Ludlow from the obscurity in which he stubbornly remains.

F.D. Maurice, who had such an impact on Ludlow in the 1850s, played a somewhat similar role in the life of Edward Carpenter, a clerical fellow at Cambridge in the 1870s. Carpenter was to move, however, from liberal Anglicanism and Christian Socialism to more or less active participation in the emergence of Hyndman's Social Democratic Federation

(Carpenter provide the funds for *Justice*), and Morris' Socialist League. He was also friendly with the members of the Fellowship of the New Life, but whereas many of them advanced on into Fabianism Carpenter shifted in the 1890s away from urban politics and parties to pursue the vision of a rural communal life on the land. His interests — anti-vivisection, vegetarianism, smoke pollution, criminal reform, ecology — were increasingly focused on the issue of the quality of life. This inevitably led to him being attacked as being softheaded, but Carpenter could humorously parry such criticisms. To the queries of a friend alarmed by Carpenter's first communal experiments with farm labourers he replied, "... of course I share the gruel with them.... We *all* dress in rags. Come!

Underlying Carpenter's shift away from a strictly political and economic approach to socialism was the discovery of his own homosexuality and his attempt to understand the ways in which sexuality functioned in a capitalist society. Indeed Carpenter is probably best known today because of Sheila Rowbotham's recent brief study that highlighted his attempts along with those of Havelock Ellis and J.A. Symonds to gain tolerance for a variety of forms of sexual expression. Carpenter did not abandon politics, however, and the Second International, the suffragette and syndicalist movements, and the Union for Democratic Control all benefitted from his interest and involvement. Like Ludlow he condemned capitalism because he saw it as immoral and in his poem *Towards Democracy* and in works such as *Love's Coming of Age* held out hopes for a new politics in which emotion and spirit would find a place.

Carpenter certainly deserves a full scale biography but one only wishes that the author had spent a bit more time locating his subject in his cultural and political context. That this has not been successfully accomplished is particularly surprising because no one is better qualified than Chushichi Tsuzuki, the author of fine studies of Hyndman and Eleanor Marx, to provide such an account. Presumably Tsuzuki refrained from drawing too many generalizations in order to produce the "balanced" account of Carpenter's life he promises in the introduction. What this lamentably results in is a study with no discernible point of view. Rowbotham made it clear that Carpenter's importance lay in the fact that he sought and articulated the demand for "not just a political democracy but a personal democracy of feeling." Tsuzuki more or less follows the same line but only by referring to the letters and papers of Carpenter. The enormous literature dealing with the emergence of socialism in the 1880s is virtually ignored. As a result we hear a good deal about obscure friends of Carpenter's such as Clemas and Sixsmith but little of Ruskin or Morris to whom he could be usefully compared.

The most memorable passage to be culled from these two books is Ludlow's description of Lord Brougham's nose. Ludlow reports that the tip of Brougham's nose "...was moveable, and could be directed — whether voluntarily or involuntarily I cannot say — up and down, from side to side, or even made to perform a complete gyration. It is said that a worthy Quaker who went up to him with a deputation as its spokesman was so amazed at the behaviour of Brougham's proboscis that he entirely lost the thread of his thoughts, and was obliged to sit down." One suspects that the Quaker's amazement at Brougham's proboscis was matched by Murray and Tsuzuki's awe at the elusiveness of the characters they sought to portray.

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Eileen and Stephen Yeo, eds. *Popular Culture and Class Conflict, 1590 - 1914: Explorations in the History of Labour and Leisure* (Brighton: Harvester Press 1981).

AS ITS TITLE SUGGESTS, this is an ambitious book. Originating from the 1975 conference on leisure and the working class held by the Society for the Study of Labour History at the University of Sussex, *Popular Culture* covers a lot of ground in terms of subject and methodology. Individual essays examine rural alehouses in the late sixteenth and seventeenth century, the rise of Methodism, popular church music, the cultural dimension of mid-nineteenth century working-class movements, the transformation of the Whitsun holiday, and late nineteenth-century working men's clubs in London. Perhaps the eclecticism of the contributors explains in part the six year gestation period between conference and publication as the editors worked on an analytical framework which attempts to bring together common themes from these studies. The Yeos' contribution, contained in two separate chapters, will certainly be read with interest by those studying popular culture.

The majority of the individual essays study aspects of nineteenth-century popular culture, and though uneven in quality, several deserve the wider circulation that this book should bring them. Anthony Delves' study of the suppression of street football in Derby during the first half of the nineteenth century shows both the careful eye for local detail and the appreciation of the complexity of the forces which swirl around popular recreation that are so necessary in this new field. By examining street football as it relates to the traditional Shrove Tuesday holiday, the growing concerns about public order in the street, and the shifting influence of temperance workers, Delves demonstrates why the search for a rational alternative to football reached its height during the 1840s, a decade of political rioting by the working class. In another excellent essay, Penelope Summerfield's examination of popular musical entertainments from the "free and easies" of the local pub to the London music halls of the 1890s illustrates how state control of licencing and the extension of capitalism into the entertainment industry shaped working-class culture and eliminated undesirable centres of recreation. Summerfield's work benefits from a wide range of primary sources, making the only effective use of statistical data and contemporary pictorial evidence in this volume. The less successful contributors to *Popular Culture* stumble mainly over their attempt to marshal convincingly the historical evidence about popular culture into a larger discussion of social control and the conflict of the working class with the emerging industrial capitalist society.

The editors of *Popular Culture* have supplemented these individual contributions with two theoretical essays which examine the evolution of the theory of social control, assess the current state of the literature on popular culture, and suggest a tentative periodization for the development of nineteenth-century popular culture. The Yeos argue strongly for the study of social control through popular culture because such control in the cultural sphere is increasingly what modern capitalism is all about. The demonstration of continued resistance throughout history by the working class to these cultural transformations is necessary to destroy the present illusion of the inevitability of capitalist change. The editors urge historians to recognize the "real possibility of other forms [of association], less adequate for emergent shapes of capitalist social relations" (135) manifest in the variety of popular recreations of working men and women.

Much of the literature on popular culture, such as A.P. Donajgrodzki's *Social Control in Nineteenth Century Britain* (London 1977), the editors dismiss for its portrayal of cultural transformation as smooth and unopposed by British workers, thus legitimizing the present domination of capitalism in social production. "It is these versions," the editors state, "which we want to search out and destroy,

in order to replace them with other histories which may be more usable for change, in alternative directions. We have to try to get behind results into the processes and struggles producing them." (279) Any attempt to discover these processes and struggles behind popular culture from within a base/superstructure Marxist framework only marginalizes a sphere of working-class life which the editors consider fundamental to a clearer understanding of the forces at work within capitalism.

In terms of periodization, the Yeos suggest that the late 1830s witnessed the first self-conscious middle-class penetration into popular culture as capitalists provided a guiding patronage to working men's clubs, athletic associations, and other groups in an attempt to encourage alternatives to less desirable forms of entertainment. During the 1840s and 1850s, middle-class intervention in the working-class world intensified as the profitable nature of popular entertainments, this powerful leisure industry, in conjunction with the emerging social sciences, reached a stage of cultural imperialism, "altering people's senses of their own past, altering taste, altering architecture, altering rural and urban space, transforming the conditions within which invention, spontaneity, cultural innovation and production from below can take place."(292)

Summary always performs its own necessary violence on books like *Popular Culture and Class Conflict*. Its contributors, however, do present the possible rewards, and pitfalls, of research into new areas of historical inquiry. While its particular concerns might be limited by the "insularity" of British popular culture, on questions of periodization and the relationship between culture and class conflict, the book will prove useful to North American readers. Yet few historians, I believe, will comfortably submit to such a direct link between contemporary political concerns and a historical study of popular culture which the book advocates.

Peter Delottinville
Public Archives of Canada

Steven Tischler, *Footballers and Businessmen: The Origins of Professional Soccer in England* (New York: Holmes and Meier 1981).

DURING THE 1970s social historians discovered that sport, heretofore beyond the pale, was in fact an integral part of society reflecting the social and economic relationships that predominate in society and thus worthy of academic interest. The result has been a number of books examining leisure, recreation, and sport in Victorian England. Steven Tischler's book is one of two that specifically examine soccer as it relates to society. Underlying Tischler's work is the assumption that it is innovative, breaking new ground, dealing with topics that have never been dealt with. This assumption is not entirely valid. While the works on sport in Victorian England have been sparse, there is a significant body of material on the history of sport in North America which would have provided some fruitful insights into the questions raised by the author. His awareness of the literature is less than complete. On the other hand he does address questions that have not been tackled, in particular the relationship between the players and the directors and football as a business. The result is a work that moves on two levels; one clearly defined and developed, the other less clear and of questionable merit.

Providing a degree of continuity is class analysis. Tischler argues with some force the appropriateness of a class analysis. The result is a tendency to oversimplify a complex process, to such a degree that it loses some of its force. This is particularly evident in the sections on the origins of professional football. In fact, the strongest section is contained in the chapter on "Players versus Directors" where the complexity of class relations is

suggested and the dangers of too rigorous an adherence to class as an explanatory mechanism recognized.

The book falls into two distinct sections, "Footballers and Businessmen" and "The Origins of Professional Soccer in England." The first section on the origins provides a thin and often erroneous account of several topics; the different forms of football, their class basis, the decline of common recreation, the emergence of new forms of sport, and the changes in the game itself. This provides a list of sometimes interesting facts and ideas but not a well developed and coherent analysis of the origins of professional soccer. For example, while recognizing the importance of time he does not explore its dimensions, in particular the importance of the changing concept of time and the acquisition of free time. These topics are related to the questions of industrialization and urbanization which are recognized but never developed. Therefore, the author never delineates what is essential to an understanding of "Footballers and Businessmen," thus the introductory chapters fail to set the stage for the main thesis.

Perhaps more important is the thinness of the analysis which is at best embryonic and at worst erroneous. An example will suffice to illustrate the nature of the problems. In his examination of the different forms of football he has failed to consider some important sources of information. This results in a simplistic analysis. More important still is an underlying assumption never stated but nevertheless implicit that the working class were victims with no control over their own lives. He states, for example, that "With the decline of many leisure activities that had thrived in an earlier period, commercialized sports and games could be introduced virtually without competition from recreational activities which were no longer feasible in an industrializing economy." (7) The history of recreation amongst the miners of the North East of England suggests a different view with a resilient rejection of bourgeois domination. Commercial sport came to the coalfield in the early 1850s based on indigenous mining sports with a long history in pre-industrial England. Later in the 1880s when football became popular the miners after an initial affiliation with a middle class dominated Football Association broke away to form their own association comprised solely of miners. Tischler's account suggests a degree of inevitability that does not take into account the subtlety of the relationships. While this problem runs like a thread throughout the whole book, it is particularly noticeable in the introductory chapters.

The second section provides a stark contrast to the introductory chapters. By focusing on process, the author establishes the overriding primacy of business interests in the development of professional football and clearly demonstrates that in the last analysis football was just another business. He achieves this by examining, in separate chapters, the Directors and the Players versus Directors. Tischler provides strong support for his thesis that "Class roles and relationships were made to conform with those of everyday life." (144) I would argue that this is not surprising since professional/commercial football was a business therefore why should it differ from ordinary life. Perhaps the answer lies in the fact that a game or sport could not, by definition, be important and certainly not a vocation. What Tischler and other historians fail to recognize is that for a large cross section of society sport is of vital importance. If sport fulfills different functions, it will be within working-class culture itself and not in bourgeois dominated professional/commercial sport. The rejection of bourgeois ideology is to be seen in working-class rejection of certain sports and games and the continuance of indigenous games. This, however, was not the focus of this book. Thus within the context of the bourgeois the justification

of sport as a vocation or business remained problematic.

In conclusion, the overall judgement must be that while the central thesis is sound providing an alternative hypothesis to those of Mason and Vamplew, the overall objective is not met. The introductory chapters are unsatisfactory throwing little light upon the process by which professional football developed. Perhaps this could have been achieved by focusing upon the period 1863-85 and particularly upon the question of why the working class chose football as opposed to rugby, a game more akin to pre-industrial folk football. One is left with the impression that an excellent paper on the relationship between footballers and businessmen was expanded, artificially, into a book length manuscript. The result is a less than satisfactory volume.

Alan Metcalfe
University of Windsor

John MacLean, *In The Rapids of Revolution: Essays, Articles, and Letters, 1902-23* edited by Nan Milton (London: Allison and Busby 1978).

THE PUBLICATION OF THIS book confirms the unique position that John MacLean occupies in the history of British labour. He remains one of the leaders of the only revolutionary tradition in Britain, the Red Clydeside of the early twentieth century. This selection of his writings and lectures clearly identifies him as a thoroughly Marxist agitator, exceptional in any labour movement. *In the Rapids of Revolution*, edited by his daughter Nan Milton, is a useful and important addition to our understanding of this crucial period in British labour history.

The book itself covers MacLean's activities from 1902 to his early death in 1923. This is roughly the same period covered by Nan Milton's earlier biography of her father (London: Pluto Press 1973). The selections are presented in three parts. First, the "early" MacLean, including critiques of the Labour Party and articles on the Co-operative movement, housing, model farms, and local strikes. The second section reviews MacLean's anti-war agitation, his leading role in the defense of the Russian revolution in Britain,[1] and some excellent lectures and a pamphlet from the Scottish Labour College. The final section includes material about Ireland, American imperialism and world revolution, the revolutionary struggle in Britain, and the campaign for a Scottish workers' republic. The value of all of the selected material is not that it is particularly important for establishing the history of the period, but that it clearly demonstrates how John MacLean went about political agitation, and how he was able to translate Marxism into the everyday lives of the working class.

This does not fulfill entirely the aims of the editor, nor of the John MacLean Society, whose collective efforts are responsible for the publication of the two books and for rekindling awareness of the importance of MacLean.[2] Important to the John MacLean Society, and many of its supporters, is John MacLean's declared of law, to mobilize the masses in order to 'politicize' more and more of the individual's life, and ultimately to eliminate or devalue authentic politics." (267) This is not a definition but a mystification.

Roberts concludes that "fascism

[1] MacLean was named an honourary president of the first congress of Russian Soviets for his anti-war activities. Others so honoured were Lenin, Trotsky, Liebknecht, Adler, and Spiridonova. He was also named Bolshevik Consul in Glasgow, the first native representative of Soviet Russia in Britain.

[2] Their success is clearly demonstrated (ironically) by the fact that in 1979 the notoriously anti-union Third Eye Centre for the Arts in Glasgow felt able to mount a massively advertised opportunistic exhibition celebrating the centenary of his birth.

position in favour of an independent Scottish Socialist Republic. Some critics have attempted to dismiss MacLean's position by attributing it to the ravages of his years spent in prison, and the associated hunger strike. These criticisms are handsomely dismissed by the elegance and rigour of his arguments, particularly since they make it clear that much of the basis for his declaration was his realization of the pro-imperialist positions of these critics and their organizations, especially with respect to Ireland.

Nevertheless many of MacLean's present-day admirers would like to translate his position into a general call for an independent Scottish Socialist Republic. These people would be well advised to read the material published here once again. MacLean's call was based upon specific historical conditions that included: a) the Scottish working class was politically far in advance of the English; b) for several years well over 3000 workers *per week* had been attending MacLean's Scottish Labour College twice-weekly lectures on Marxist theory and politics; c) the Irish were winning a brutal war against the British; d) the British ruling class was weakened by the ravages of war and the rising strength of American capital; e) and organizationally the British working class was still in a state of flux. Never, in the documents published here or elsewhere, did MacLean put the argument for a Scottish Republic ahead of his anti-imperialist positions. Instead he consistently treated the Scottish independence as a tactical question, always subordinated to his opposition to the ruling class, whether English or Scottish, and argued for on the basis of the historical conditions prevailing at that juncture. Many of MacLean's supporters would have us lift this call from its historical context to a question of principle.

If the book fails to support the present call for a Scottish Socialist Republic it clearly succeeds as an example of how Marxist agitation can and should be carried out. The first pamphlet of the Scottish Labour College, "the war after the war" is a brilliant educational work, popularly explaining capitalist exploitation and profits, inflation, technical change, and the need for class struggle. Based on the development of the differences between Marxism as the science of the working class, and the apologetic explanations of bourgeois political economists, his "Plea for a Labour College in Scotland" argues superbly for an independent working-class education. "Time saving and Karl Marx" uses an exhibition of "all the latest devices and mechanisms that go to make up the modern office" to achieve the same end. Almost daily events can be used in a revolutionary way by a thoroughly-trained Marxist.

Finally, the book should be useful to labour historians elsewhere because of the frequent appearance of the Scots among leading trade unionists in Canada and elsewhere. There seems to be a tendency to assume that any connection with the west of Scotland in the first third of this century will explain the superior working-class politics of these emigrés. However, *In the Rapids of Revolution* clearly distinguishes between what was Marxist and revolutionary, and the rest, whatever we may call it. Thus a connection with Red Clydeside might not only account for the class consciousness of Scottish immigrant union leaders in, say, Nova Scotia, but also the reformist nature of their politics and their role in diverting the development of a revolutionary tradition in the Nova Scotia labour movement.

Andy Rowe
St. John's Newfoundland

David Smith, ed. *A People and a Proletariat* (London: Pluto Press 1980).

A PEOPLE AND A PROLETARIAT is an attempt to catch a collective breath. Welsh labour history has been developing at an astonishingly rapid pace both intellectually and in its links with the wider labour movement. *Llafur* (Labour), the journal

of the Society for the study of Welsh Labour History has been running for ten years. Clearly there is a desire for reappraisal and reflection which runs through the book.

This desire is not, however, immediately apparent in the first essay. Gwyn A. Williams takes off with a great flourish. His breathless style will be familiar to those who have read his excellent book on the 1831 Merthyr Rising. His imagery is often spectacular. He eschews "that acid-bath of clinical scholasticism which leaves the consumer feeling he has just got drunk on vinegar." His metaphor, however, is not always easy to follow. Gwernllwn Chapel, Dowlais is as symbolic to Gwyn Williams as Bow Bells are to Cockneys. For those born outside such cultural orbits it is sometimes difficult to grasp the speed and nuances of the language. Notwithstanding that his essay on the early days of the Welsh working class is subtle and incisive in its treatment of early class consciousness. He confronts certain myths about the Valleys and describes convincingly the pervasiveness of radical ideas in the early years of the nineteenth century. That radicals were more "visible" in industrial Merthyr should not disguise their presence in more rural areas. For those who only see the chapels as centres of sobriety and social control, Williams offers a different view. "It was the Unitarians of Wales who were the motor forces in the creation of the first Welsh democracy, the first Welsh populist nation, the first Welsh Jacobinism, the first Welsh working class movement." Indeed Williams goes on to show that in 1834 in Merthyr it was "two Unitarians who produced the first working-class newspaper, *The Worker/Y Gweithiwr* in the service of Robert Owen's syndicalist movement."

There are two other essays that are particularly interesting on class consciousness — this time, though, they are concerned with the coalowners. L.J. Williams assesses the economic and social influence of the owners and writes of the limited opportunities for social mobility. Williams, however, begins his essay defensively and feels the need to justify studying employers as part of labour history. His caution is understandable. Some labour historians are like sports commentators — they only wish to see one side in the game. This partial view can mean that the activities of labour are considered in a vacuum.

Kim Howells approaches the subject from a different angle in his essay on "Victimization, Accidents and Disease." He takes up a theme of Royden Harrison's on the potential for mythologizing class conflicts. Howells attempts to establish how far the images miners had of the owners were based on fact. Clearly many incidents were exaggerated, but his conclusion that the "mining population's image of the 'reality' of death at the coal face in south Wales during the 1930s was inextricably linked with its image of itself as the victim of blatant social injustice outside the pits" is correct.

An essay on language and community in nineteenth-century Wales by Ieuian Gwynedd Jones considers "the difficult and puzzling . . . relationship of language to social change." His essay unravels the complex links between language, religion, politics, economic development, transport, and social mobility. This essay may be the one that finds the closest echo among Canadian readers. The links between religion and politics and between language and social aspirations are carefully disentangled. For the newly-educated generation from working-class homes in Wales, this essay also has much to say.

Other essays look at the usefulness of carefully researched oral history. One by David Jenkins describes rural speech patterns and another by Hywel Francis explores areas of experience of miners that could never be found in documents. Another piece on the language of Edwardian politics by Peter Stead points to the

amount of their leisure-time Edwardian working men spent listening to sermons, talks, and public addresses. He touches on the fascinating way in which the pomposity of formal language began to change and how socialists like Keir Hardie were able to capture and give new meaning to the word "Labour."

As an antidote to the South and West Walian bias of much Welsh labour history there is an interesting essay by Mervyn Jones on the "marginality" of North Wales which challenges many of the myths of regionalism and class. Readers from the Atlantic Provinces might find this short essay of value.

An essay by Brian Davies endeavours to situate the unconventionality or "madness" of Dr. William Price in nineteenth-century Wales. Davies claims that Price made a major contribution to Welsh mythology. Nudist, Chartist, Celtic fanaticist, Price must have made a fair contribution to contemporary pub and chapel conversation. He became most famous at the age of 83, for "illegally cremating the body of his 5 month old illegitimate son, Jesus Christ Price, Son of God." Price's subsequent acquittal legalized cremation, Davies tells us.

The gaps in this book reflect the gaps in the area of study. Wales, as David Smith, the editor says, "is a plurality of Cultures." There are the sub-cultures of the Irish and the Spanish immigrants yet to be written about. Religion for Welsh historians is still dominated, understandably, by non-conformism. But the Catholicism of the Irish community has been a remarkably cohesive and resilient force. Although Hywel Francis refers to the Spanish in the Rhondda, there is little published about the anti-clericalism amongst some in Dowlais which developed, it appears, from their attitude to the Church in Bilbao before they came to Wales in 1911. Such cultures deserve further study.

Other omissions are mentioned by the editor. *Llafur* is addressing itself to the problems of women in Welsh labour history. It is also aware of the failure to make links with workers in more recent industries, although it has been remarkably successful in attracting workers from more traditional sectors. David Smith's excellent concluding essay is a salutary lesson for those concerned with the historiographical problems that have been thrown up by the rapid developments in the field of study. Although the book is sometimes disparate, it manages to avoid the eclecticism that sometimes besets *History Workshop* and also the maudlin nationalist sentimentality that sometimes passes for reminiscence in Wales. It is a useful and thoughtful pause in the exciting pace of historical work. It also bears out my old history teacher's phrase that to know history, you have to know your culture. The writers in this book clearly know theirs intimately.

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Doug McEachern, *A Class Against Itself: Power in the Nationalisation of the British Steel Industry*, (Cambridge: Cambridge University Press 1980).

WHAT ARE THE POLITICAL and economic roles of publicly-owned industries under modern capitalism? Whose interests are advanced by acts of nationalism? What underpins the relationships between government, labour, and capital? Such questions lie at the heart of Doug McEachern's analysis of public policy toward the steel industry in post-war Britain. In McEachern's view, the steel industry merits close examination for several reasons. First, steel, as an industry that lies somewhere between infrastructure and secondary manufacturing, plays a vital economic role in the advanced capitalist economies. Second, ownership of the steel industry was a question of recurring controversy in post-war British politics. For over a period of two decades, steel was nationalized by Labour,

returned to private ownership by the Conservatives, and subsequently renationalized by Labour. The analyst is thus able to explore the issue of ownership and the posture of state, industry, and labour over a prolonged period.

McEachern is highly critical of conventional approaches to the study of political power. In his view, pluralists are particularly guilty of ignoring the *consequences* of political decisions for concerned interests. They frequently assume, moreover, that those who "prevail" in stances of conflict are invariably those whose interests are served by the outcome. In rejecting such assumptions, McEachern maintains that steel nationalization, although undertaken by a social democratic government with working-class support and opposed by capitalists, ultimately served the interests of manufacturers and the capitalist class as a whole. Public ownership had such results for a number of reasons. First, nationalization of steel established limits to the mixed economy, laid down ground rules for the relationship between public and private enterprise, and most importantly, confirmed that state industry should operate in subordination to and in the interests of the private sector. Capitalists neither appreciated nor fully comprehended such consequences and opposed the public ownership of steel — hence the book's title. Although labour's role in nationalization is not studied intensively by McEachern, he does maintain that working-class interests were not advanced by public ownership. Indeed, the Labour government's renationalization of steel in the 1960s is seen as contrary to the steel workers' interests. For Labour's commitment to rationalizing the industry, if fulfilled, would reduce employment.

McEachern's volume is a significant contribution to our understanding of the political economy of nationalization. His book stands out in a field that has too long been dominated by economists, students of administration, and polemicists. McEachern demonstrates the complexity of nationalization, the diversity of capital's interests, and the need to consider carefully the consequences of state action. But a number of problems arise. First and most simply, the delineation of the consequences of state interventions is a difficult and perhaps ultimately subjective task. Yet McEachern says little about either the methodological problems at stake or his criteria for assessing evidence. He appears rather content to assert, rather than to demonstrate conclusively, that certain interests were served while others were thwarted. Second, *A Class Against Itself* does not provide a satisfactory explanation for the events examined. This is rather disappointing, for McEachern himself points repeatedly to a number of vexing questions. For example, how and why does state intervention serve the long-term interests of capital even when capital is either indifferent or hostile to the content of public policy? Far too little theoretical attention is paid to this question. The result of this imbalance between theory and analysis is an interpretation which successfully refutes pluralism but substitutes no alternative perspective. Perhaps not much more can be expected of a case-study.

Such flaws notwithstanding, McEachern's book is a thought-provoking and cogent account of an important question in post-war British politics. It should be read carefully not only by academics but also by those who naively believe that public ownership necessarily alters the essence of capitalist economies.

Allan Tupper
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Dan N. Jacobs, *Borodin: Stalin's Man in China* (Cambridge, Mass.: Harvard University Press 1981).

IN THIS FASCINATING, lively, and extremely well-written biography, Dan Jacobs has gone a long way to rescue Borodin from the "dust-bin" of history

where, until very recently, Soviet historiography had confined him. In so doing, he reopens the debate on the Soviet role in the Chinese Revolution of the 1920s. While he does not alter the essentials of the story found in the classic works on that period, Harold Isaacs, *The Tragedy of the Chinese Revolution* (2nd rev. ed. 1961), and Conrad Brandt, *Stalin's Failure in China* (1958) nevertheless, his use of newly available Soviet material does provide a fresh viewpoint that is more sympathetic to the ill-fated Russian venture than previous accounts. When read along with those earlier accounts, the student of Sino-Russian relations is able to develop a more balanced appreciation of those still controversial years.

The story that emerges is remarkablé. Born in the Jewish Pale of settlement in Yanovichi, Vitebsk province, Mikhail Markovich Gruzenburg (1884-1951), alias Michael Borodin, as a youth cut himself off from his Yiddish background and moved to Latvia where he immersed himself in the dominant Russian milieu. Attracted to socialism, he left the Jewish Bund to cast his lot with Lenin's new Bolshevik faction. A skilled organizer and linguist, he soon became involved in important Party work where he met the young Georgian revolutionary, Stalin. Buoyed by his early success and youthful prominence, Borodin was disheartened to have to leave for America following the repressive measures enacted after the failure of the 1905 revolution. He lived for a decade as a student, teacher, and socialist activist in Chicago, where his sons Fred and Norman were born. A man of great ambition, he chafed at this exile. With the Bolshevik victory, he became a delegate of the Comintern in the United States, Mexico, England, and other places, returning eventually to Russia. A fortunate meeting in Mexico City with the Indian communist M.N. Roy drew Borodin to an increasing interest in and identification with revolution in Asia.

In 1920, with the failure of the revolution in Europe, Lenin turned his attention to China. Through the efforts of the veteran diplomat A.A. Ioffe, the Russians came to view Sun Yat-sen and his fledgling Kuomintang (KMT) as "their standard bearer in China." Following the signing of the Sun-Ioffe agreement for aid and cooperation in January 1923, Borodin was sent to Canton as Sun's adviser. Thus began his career as midwife of the Chinese revolution. Along with other *sovietniki*, whose memoirs form the main source materials for this period, Borodin played a central role in the vast and complex events that ultimately brought Chiang Kai-shek to power in 1927. Through his organizational ability and by sheer force of personality, he forged and held together the shaky coalition that enabled the Northern Expedition to achieve the success it did in subordinating, though not eliminating, warlord anarchy to some semblance of central civilian administration under the KMT.

While Jacobs makes it clear that Borodin had great authority and flexibility in developing policy in China, in the end he concludes that Borodin acted always as "Stalin's man," adjusting to his needs and priorities. The final, fateful decision to remain in China and to support Chiang despite his turn to the right in the Canton coup of 20 March 1926 was Stalin's. Both the Chinese Communist Party and other of the Soviet advisers paid a high price for that decision in the years ahead, but Borodin was able to escape from China and live out his lonely and melancholy last years in Moscow protected by his powerful patron. But Stalin's paranoia and antisemitism eventually caught up with Borodin. He was arrested at the beginning of 1949, as Mao's forces were entering Peking, and died in a prison camp on 29 May 1951.

The controversy over the Russian role in these events has continued ever since. While Jacobs is right to emphasize the positive accomplishments of Borodin and

others, especially prior to March 1926, he comes dangerously close to falling into the trap set for him by his Russian sources of depicting Borodin as the "great white father" without whose presence the revolution would have come to naught. This is partly an excess of zeal — he is sometimes blinded by his hero's brilliance. Fortunately, however, he is able to see the flaws, and despite the occasional dramatic flourish, presents a balanced account that, in its depth of detail, clearly reveals how often events in China unfolded beyond Borodin's control. Like other national revolutionary movements of our time, the Chinese revolution proceeded from an inner dynamic of its own. Foreign advisers played their part, but China was not Stalin's to lose in 1926-27, just as it was not America's to lose in 1948-49.

This is an important story and it is very well told. Unfortunately, it suffers from defects of structure and form. For one thing, parts of the book are very poorly documented. The early career is particularly deficient here, and chapters 2 and 7 have one footnote each. Chapter 2 vigorously describes Borodin's role at the 1905 Unity Congress of the RSDLP in which we are informed as to what Borodin "disputed," "pointed out," and "proposed," without a single reference to any documentary evidence. This is either imaginative historical reconstruction run rampant or the account is based on sources that the author chose not to cite. Chapter 7 contains accounts of Borodin's relations with Louis Fraina and Claire Sheridan. In both instances, we are given direct quotes by the principals but no citation as to source. Only by cross referencing to the Principal Sources can one piece together the likely sources for these episodes. One cannot determine whether this represents the author's own approach to documentation, or an attempt by Harvard University Press to cut expenses by eliminating footnotes. If it is the latter, this is a most serious and unwelcome development, for it makes it all the more difficult to assess fairly the validity of scholarship when the scholarly apparatus is so truncated. The impression that Harvard was cutting corners to keep costs down is reinforced by the fact that neither in the Sources nor in the Notes are Russian titles ever translated! Moreover, there are numerous textual errors throughout, evidence of sloppy editing. For example, on p. 262 "Wuhan regimse" and "people of halt of China" appear within the same paragraph. It is truly regrettable that an otherwise excellent book has been so poorly served by its publisher.

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Stephen S. Large. *Organized Workers and Socialist Politics in Interwar Japan* (Cambridge: Cambridge University Press 1981).

WHY DID THE JAPANESE labour movement begin in the 1920s with high expectations and promise of remaking Japan into a democratic socialist society only to abandon that commitment and then die out in the 1930s? This is the central question of Stephen Large's important book, the most comprehensive political history of organized labour in interwar Japan available in English. Focusing on the Japan General Federation of Labor, Sodomei, the largest union federation of the period, Large carves out an answer somewhere in between those who have stressed unremitting repression of the labour movement from above and those who have emphasized the persistent internal weaknesses of Japanese labour organizations.

In the wake of World War I there was mass unrest amongst Japanese workers and a political awakening of a small number of labour leaders. The pre-war tradition of business unionism was replaced by wide-ranging ideological and organizational struggles amongst socialists, anarchists, and communists. Sodomei leaders like Komakichi Matsuoka and Suehiro Nishio, according to

Large, were strongly committed to an admittedly vague doctrine of socialism. In the political sphere that meant legislative reforms to reduce the power of the military, the bureaucracy, the Peers, and to accentuate the power of the lower House of the Diet. The power of the Emperor as symbol of the race went unquestioned. In economic terms socialism meant ultimately state ownership of capital and redistribution of wealth but, more immediately, the legalization of trade unions and other measures to improve conditions for workers. After joining with the communists to purge the anarchists from their union, the socialists in Sodomei were enticed by concessions from the government to purge the communists in 1925. The labour movement divided into two camps, making it easier for police repression of the communists and leading to the shriveling of Sodomei's socialism "into little more than an almost pathological anti-communism."

The split within Sodomei coincided with the passage of the Universal Manhood Suffrage law. Sodomei leaders jumped into the confusing politics of three socialist parties that were formed in 1926 to take advantage of the new labour vote and which were allowed to function at the sufferance of the liberal capitalist regimes of the late 1920s. But in the environment of the Depression, right-wing terrorism, and an aggressive foreign policy in China, Sodomei's interest in socialism weakened. Many Sodomei leaders were openly sympathetic towards Japan's imperialism and sought to prevent workers from strike and other protest actions on grounds that the unions were too weak. By 1932 Matsuoka, who had been active in socialist party politics, took over as head of Sodomei and effectively severed the connection of the union to socialist politics. Thereafter, Sodomei leaders pursued a solidly rightist political line in the hopes of preserving the trade-union movement. They were only moderately successful. Membership in Sodomei unions never exceeded even 100,000 workers, at most 25 per cent of all unionized workers. In 1937, Sodomei formally renounced resort to strike action and in 1940, cooperated in the government sponsored Patriotic Industrial Association or Sampo, effectively ending the last remnants of an independent union movement.

Throughout his account of Japanese socialist labour politics, Large brings out the enormous structural obstacles to workers' organizations. For example, the dual economy of a handful of modern large corporations along side the multitude of medium and small firms and the paternalism of the Japanese factory system that bred loyalty to the company and not to class. In a concluding chapter Large engages in fascinating comparisons of pre-war Japanese labour politics with that in Germany and Britain.

Large is extremely effective in documenting the compromising responses of Sodomei leaders to Japanese nationalism and imperialism. He holds that this accommodation was desired by the rank-and-file union members, though his evidence is sketchy on this point. He is even less convincing at demonstrating that Sodomei leaders were ever committed to a socialism that went beyond liberalism or to a socialist party that went beyond making trade unionism more secure in capitalist Japan. Their failure was not just a failure of nerve but resulted from a lack of any coherent analysis of Japanese capitalism. Sustaining themselves as union bosses always took precedence over political reformism. Thus rather than faltering on the high road to socialism, as Large argues, Sodomei leaders in the inter-war period appear never to have turned off the bumpy highway of Japanese capitalism.

In spite of the political reforms of the Occupation and an explosion of post-war labour organization, the politics of the Japanese labour movement today are an extension of those of the pre-war period. That is not so surprising when one consid-

ers that Matsuoka and Nishio were key figures in reestablishing the trade-union movement and socialist party after the war. Japanese labour and socialism still face a hostile external environment of business and government, internal factionalism, and status rather than class-conscious workers. Large's book, thoroughly researched and carefully crafted, is a model of useful history. He has significantly advanced understanding of contemporary Japan through his excellent study of the inter-war labour movement.

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Marianne Herzog, *From Hand to Mouth: Women and Piecework* (Harmondsworth: Penguin 1980).

AS PAUL SWEEZY POINTS out in his Introduction to Harry Braverman's *Labor and Monopoly Capital*, there has been an "almost total neglect of a subject matter which occupies a central place in Marx's study of capitalism: the labor process" (ix), especially by those with direct experience in actually doing the work. Marianne Herzog's *From Hand to Mouth*, a detailed description of women's piecework in a variety of West German factories, is intended to offer just such an examination of the labour process.

Using what Sally Alexander in the Introduction to this edition calls "a clipped, sparse prose," reflecting "a sharp but controlled anger at the way those who actually produce the goods — the vast majority of them women — spend their lives" (7-8), Herzog itemizes the details of her own experiences, and those of other women, welding tubes, packing boxes, olives and spare parts for trucks, assembling vacuum cleaners and manufacturing things in prison. The physical pain and numbing exhaustion created by dirty, noisy, and poorly lit and ventilated environments, as well as by the repetitive, often close or heavy work at inadequately equipped machines, the tight supervision, firing threats, and individual rates that are constantly lowered as output increases, and that effectively limit collective resistance, the low piece rates combined with the absence of coffee and lunch breaks or sick pay that are forced on women compelled to work by economic need and by their lack of job alternatives or training, the dreams and innovative practices designed by women to get them through the terrible boredom of jobs which seldom permit even conversations amongst workers let alone movement around the room — all are there for the reader.

The descriptions are in some instances eloquent, frightening, revealing:

> By 10:30, I'm in a nightmare. I take fright when a woman talks to me. I hear the machines whispering. I see travel-bags on wheels even though they are only metal cases. I light a cigarette and think, this is how it always is; I experience this condition whenever I work in a factory, it's the monotony which causes it. (101)

Other passages, like the outline of her medical examination at the Siemens plant, expose facets of degradation seldom discussed in the literature.

> "Is there any mental illness in your family?" the doctor asks. "Any tuberculosis?" "Have you had any serious operations?" "Is there cancer in your family?" You blow into an apparatus, have your teeth examined with a pocket torch — it's a slave market. (97)

They also sought information on convictions and threats of convictions, as well as posing that ultimately female question — are you pregnant?

Sweezy, however, is calling for more than details based on personal experience. To fill the gap, he argues, what is needed is "an attempt to inquire systematically into the consequences which the particular kinds of technological change characteristic of the monopoly capitalist period have had for the nature of work [and] the composition (and differentiation) of the working class, the psychology of workers, the forms of working class struggle, and so on." (ix) Instead of a systematic

inquiry, Herzog's book offers somewhat disjointed, fragmentary glimpses of women's piecework experience. There is no sense of historical development, of the specificity of the West German situation, or even of the relationship of this work to the larger economy and social structure. Technology plays a minor and rather static role, except for the comment that there is little difference between the conditions in a modern Siemens plant and an ancient vacuum factory. Nor do workers' struggles appear even as a possibility in the narrative. Indeed, attempts at individual resistance are dismissed as inconsequential. As Sally Alexander explains in her Introduction, "The factory totally dominates the lives of the women and the implication is that there is no separate space for the creation of a disruptive politics or culture." (33) Not only is history missing but so is the dialectic.

Analysis does not flow automatically out of the presentation of labour process details. As Sweezy makes clear, "it is not only direct experience that is needed for the scientific study of the labour process under monopoly capitalism. Equally important is a thorough mastery of Marx's pioneering work in this field and of his dialectical method." (x-xi) In *From Hand to Mouth* Herzog exhibits no such mastery, whether explicitly or implicitly. Nor is there evidence of a competing theoretical and methodological framework.

For clarification of the meaning of women's piecework, it is necessary to rely on Alexander but in attempting to cover a broad range of analytical and empirical issues, she ends up offering little systematic assistance in understanding the implications of the details which follow. Alexander contends that Herzog provides an alternative to "official, academic and journalistic inquiries" (9) which contain only demographic and cultural explanations for "inequalities," stressing supply side factors, legalistic reforms, and an optimistic view of the future while ignoring both the historical and national specificity of women's work as well as the system of social production which is "the source of divisions and inequalities among the working class." (13) Yet Alexander then proceeds to use a historical sketch of developments *in Britain* as background to this book on West German women. She also proceeds to argue that piecework is a symptom rather than a cause of women's industrial weakness, and that the work is explained by the supplementary character of women's work, in other words by supply factors.

Such arguments, she acknowledges, are not directly addressed in the text. According to Alexander, however, there is implicit in the details of women's lives an understanding that piecework and women's position in the family underline and perpetuate "women's vulnerability as waged workers." (26) It is true that Herzog stresses the mutually reinforcing nature of piecework and domestic labour. She also, however, emphasizes other factors like the lack of training and of alternative employment opportunities for women, and the recession. It is difficult to grasp how these factors fit together, if at all.

The book does reveal, as Alexander claims, "the monotony, ill health and debilitation of [modern industry's] working day." What it does not do is to fill much of the gap in our systematic understanding of the labour process.

Pat Armstrong
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Rae André, *Homemakers: The Forgotten Workers* (Chicago: University of Chicago Press 1981).

RAE ANDRÉ IS an assistant professor of industrial administration at the General Motors Institute in Flint, Michigan. As an organizational psychologist, the emphasis of her work is on "improving the quality of working life for employees in business organizations." (ix) In this book she has focused on homemakers — the more than

24 million (5 million in Canada) women who work full time in their own homes as wives and mothers doing domestic labour. She starts from the premise that "work is one of an individual's strongest bonds with society and a primary source of psychological well-being." (ix) She then applies the various insights, philosophies, and research techniques of industrial psychology to the occupation of homemaker.

The result is an extremely eclectic book that borrows bits and snatches from a variety of other studies but never succeeds in integrating them into a coherent whole. There are perhaps two themes which give her book whatever integrity it has. The first is a very strong commitment to women. The book radiates a warmth and compassion for women in general and particularly for women in the home. As she stresses throughout the book, her purpose is "problem-solving" (261): that is, she wants to develop policies and actions that will improve the status of homemakers and make things better for women.

She considers her book a contribution to what she calls "the new homemaker movement," which is apparently a large-scale movement of women and men involved in "the process of designing and implementing strategies for achieving the system of equality" that will give homemakers the same status as paid workers and will generally improve the status of women. (27) Unfortunately, she never clarifies who actually constitutes this new homemakers' movement although she indirectly implies that it is primarily a middle-class movement when she says that "today's average homemaker is probably part of a middle-class family." (48)

The second theme which runs through the book revolves around an underlying set of implicit assumptions that most of the world's problems stem from a lifestyle based on over-consumption. She argues that one area that housewives do actually control is their families' consumption of goods and services. She urges homemakers to curtail their spending by developing a simpler lifestyle. Such activities, she maintains would first of all eliminate a great deal of women's work in the home:

> My own preference would be to salvage the idea of the home as a place of peace — whether for individuals or for families — and as a physical expression of a person's interests and aesthetics, and to reject values that make the home a display area for affluence or for the products of creative consuming. The more we simplify our lifestyles, the more drudgery is removed from housework. (64)

She is consistently critical of "questionably high standards of consumption" (55) and frequently expresses a wistful nostalgia for a simpler Thoreauian past. (162) An advocate of the "small is beautiful" ecology movement, she argues that homemakers "can to a certain extent manipulate the family's style of consumption, and in doing so they have great potential power to reflect various ecological and social concerns." (259)

Three chapters are actually quite useful although they are so specific to the American context that Canadian women will have to extrapolate the general points and figure out how they apply here. The chapters on "Equality Begins at Home" (95-117), "The Displaced Homemaker" (186-206), and "Change in the House — and in the Senate" (207-228) are useful because they are concrete and suggest particular issues that homemakers can address and actually do something about.

Her ideas on various ways that homemakers can manipulate tax payments, pension plans, and Social Security payments to guarantee themselves some income and general financial security are quite ingenious. She also provides some concrete examples of ways that homemakers can encourage other household members to share in domestic labour. She includes models of marriage contracts. Most powerful is her description of the situation of displaced homemakers — women who through death, desertion, or divorce have lost their jobs. She includes

some exciting examples of campaigns that various women's groups have organized to help displaced homemakers get financial and emotional support while they retrain for paid employment.

Unfortunately, her analysis falls apart when she tries to identify the underlying problems which contribute to the situation of homeworkers. She describes some of the contradictory characteristics of the job and suggests that women are victims of a domestic double standard (9) where their work is considered valuable and priceless but receives no pay or benefits and has low status. This work itself has autonomy and a certain amount of variety but it occurs in isolation and is essentially unchanging.

She attributed this to current attitudes and values toward work which stress monetary payment as the measure of value and worth. This she considers to be "largely a male ideology" (53), which can be countered by "those values women espouse." (32) Such a viewpoint verges on a notion of maternal feminism and completely ignores the economic forces of industrial capitalism. She suggests that "a society's most powerful members determine its goals" and notes that "the powerful people have always been men . . . men who have a strong interest in the production of goods." (51)

What is particularly frustrating about such a simplistic analysis is that she is not unfamiliar with various economic theories that link domestic labour to the larger economy. In passing, she observes that housewives produce and reproduce labour power and she assumes that homemakers constitute a reserve army of labour. (59-60) Both these concepts have been well analyzed, but she never develops the implications of such theories.[1] Indeed, later in the book she employs a conventional Parsonian functionalist model when she argues that the primary function of the family "continues to be the psychological and physical nurturing of its members." (142) Further she argues that if reforms are instituted that improve the working conditions the homemaker will be more satisfied with her unpaid job in the home and hence she will be "less eager to take on an outside job, she will be less of a threat to workers already in conventional jobs. All women will thus be less exploitable." (116)

Her eclecticism and naiveté culminate in her discussion about organizing. She repeatedly asserts that women must become informed and conscious and she suggests that consciousness raising groups and other neighbourhood and community groups are a good place to start. She insists that personal change and individual actions are vital and suggests that networks of homemakers will emerge from local initiatives to lobby the various levels of government to demand better conditions for women. Emerging from such activities she envisions:

> . . . the new homemaker movement will find advocates worldwide among both women and men who see in it possibilities for a renewed emphasis on family and community networks and for a reinvigoration of such values as nurturing rather than conquering and cooperation rather than competition. (258)

As long as such a vision ignores the questions of who actually holds power and on what basis, it will go nowhere. As long as the new homemaker movement ignores the realities of labour in advanced capitalism and fails to understand what the family is and how domestic labour is part of the cycle of reproduction of labour power, it will at best win slight reforms for itself. Given the depth of the coming economic crisis and the profound hostility of the right wing to any family reforms, overall this book seems hopelessly out of touch with reality.

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[1] See Bonnie Fox, ed., *Hidden in the Household* (Toronto: Women's Press 1980) and Pat Connelly, *Last Hired, First Fired* (Toronto: Women's Press 1978).

Anna Pollert, *Girls, Wives, Factory Lives* (London: Macmillan 1981).

THIS A VERY sensitive description and analysis of women's experiences as they worked in a tobacco factory in Bristol, England in the early 1970s. The study is based on informal interviews and observations on the factory shop floor in 1972, followed by subsequent contacts. It describes the everyday working lives of girls and women, and shows how they got on with their jobs, their bosses, and each other. It shows how their boyfriends, their husbands, and their families fitted into and affected their lives. The author analyzes all these strands and shows how they were woven together in women's experience and consciousness.

So far there is little empirical research on how gender affects working-class women's experiences in the factory. This book fills an important gap in the Marxist and feminist literature. It also makes delightful reading. In contrast with the highly general and sometimes rather abstract writings of some Marxists and feminists, this book is filled with down-to-earth interpretations of vividly portrayed incidents and situations. A considerable amount of dialogue is presented in the text, but these sections are always carefully chosen and excellently interpreted. There is no quotation just for the sake of quotation.

The first part of the book deals with the Churchmans company, with the long term decline within the industry, and with the company policies regarding the workforce. Part two deals with the ideology and experience of the women workers. The author shows how many women accommodate to hard and boring work by claiming that they are only temporarily in the workforce. Tamara Hareven found exactly the same pattern among the largely French Canadian Catholic mill workers at the Amoskeag textile mill in New Hampshire. Records showed that at the turn of the century Amoskeag women worked their whole lives in the mills, with breaks for childbearing. Interviews with old women, however, showed that they had accommodated to Catholic family ideals by claiming to be working only temporarily.

Part three deals with conflict at work. The author shows how women handle conflict on the shop-floor, in the union, and in negotiation with management. Chapter 8 has a fascinating section on how the young unmarried women dealt with male chargehands' disciplining that was continually laced with sexist patronization, "Hey, gorgeous," "Do us a favour, love," "Come here, sexy." The author shows how the young women fought back as a group.

The author describes the extreme boredom these women experience and has them describe the techniques they develop for coping with this. Some become "burned out." She also discusses how women workers recuperated from illness — women who fell ill and stayed home from work often had to catch up on housework and were not able to rest and recuperate.

The last chapter is ostensibly on the closure of the Churchmans factory in 1974. Its main theme, however, is the discussion of the use of women as a reserve army of labour, and the problems of any separatist women's movement that does not also deal with the implications of class relationships.

This is a well-rounded book. For example, the author recognizes the close relationship between her work and P. Willis' *Learning to Labour: How Working Class Kids Get Working Class Jobs* (1978). As one would expect, the author relates her findings to other European work, rather than to North American research. The introduction contains much useful material on women and the labour movement in Great Britain. Perhaps the author's greatest feat is her deft interweaving of the "macro" issues of class and power with the "micro" themes of

shop-floor interaction.

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Ann Oakley, *Subject Women. Where women stand today —politically, economically, socially, emotionally* (New York: Pantheon 1981).

AT FIRST GLANCE I thought Oakley had bitten off more than I could chew. After careful sampling, I conclude that she has so arranged this feast that it can be enjoyed and digested in one gluttonous sitting, or in a more moderate fashion in several smaller servings. *Subject Women* is indeed food for thought.

It is also a multiple pun. Oakley explores and analyzes, by drawing on a staggering variety of material from several disciplines, the experience of subjection and subordination of women, and women's response and resistance. She intends not only to "map the terrain of women's world and experience" but to write a "history and sociology of late-twentieth century industrialized society" in which "the spotlight is on women." All in one volume! The result is a Brit's-eye view (with a good deal of United States and some international, including Canadian, material); and to a certain extent, a limited view, although not by any means narrow in conception, execution, or usefulness.

Oakley's ambitious study is a welcome change from some recent attempts to delineate general trends and theories of society and the nature of human experience. Often these seem to be accounts of "work" and "workers" that are limited to unionized males in the paid labour force, and in fact touch on the partial experience of a minority of working (paid and unpaid) adults, not representative of the majority experience. These surely should not be used as the bases of supposedly universal or all-inclusive generalizations about culture or society. Oakley, on the other hand, is attempting to delineate the majority experience, in twentieth-century industrialized society admittedly, and on that basis to draw some conclusions about that society.

Oakley's approach and her choice of categories, reflect (as well as describe) recent shifts in feminist scholarship. In a recent interview, Nancy Chodorow explained that while she had intended to contribute to Marxist-feminist theory in her *Reproduction of Mothering* by focussing on parenting as social relations, linked to theories of class and labour. She would not add to objective social relations and class inequality, psyche and culture; all, she now believes, are just as fundamental in explaining gender relations and women's oppression — and hence the organization of society.

Similarly, Oakley includes not only material but emotional conditions of women's development, work, and lives. The description of society which emerges from Oakley's work is based on the standpoint of women, and as such is at least as complete and legitimate a picture as the usual malestream creation. Possibly more legitimate, for after all, women are a slight majority of the population, and world-wide perform two-thirds of the world's work, according to United Nations statistics.

The book begins with women as citizens: are women people? Her answer begins with the suffrage movement; cultural mythology dictates such a starting place, because of the "progress towards equality" set in motion by that movement. Oakley raises questions of cause and effect, of reforms achieved and their limits; it is a nice balancing act of taking into account the image of women's position, and the reality of our experience. The discussion deals well with the difference between the emancipationist goals of the last wave's efforts, and the liberationist goals of our own wave.

The second section considers the role of genes and gender in the making of a woman, considering in part the process of

"psycho-sexual differentiation of males and females." Pointing out that the real question is "what makes a male" (we all start out female, and at about eight weeks some foetuses get maleness added on to the universal female human form), she surveys existing research on sex/gender differences. She examines definitions of women compared to men-as-norm, the components of the stereotypic feminine personality, and related issues, all in the context of the subordination of women in male-interest oriented culture. She goes through the process of social amplification of biological differences that produces women's personalities, in childhood, socialization, education.

The third section looks at labour (what is women's work?) both paid and unpaid. Surprisingly, since housewifery is one of Oakley's major fields, this section lacks a discussion of inequality of allocation of scarce resources within the family and the intensification of female unpaid labour in situations of income decline; nor does it discuss at length the servicing-the-worker tasks of housewifery so fundamental to an industrialized economy. All of these areas are well-covered in the British and Canadian literature.

The next section focusses on women's relationships to children, men, and other women. An overview of the politics, social policy, whys and hows of child-bearing and child-rearing is followed by a dissection of the sexual politics of male-female interaction, especially in marriage and the family. Men, she shows, still have the power of the purse, backed up by the fist, and finally by the state. Oakley appropriately includes battery, rape, and prostitution, as well as female subversion, in her discussion of marriage. As for sisterhood, she describes it as a political issue of fundamental importance. Oakley discusses competition and cooperation between women: female solidarity has been and continues to be all-important, for physical survival, as well as for material, economic, cultural, and emotional well-being of women.

Section 5 focusses on power: what womanpower is and why there isn't more of it. The discussion begins with class (are women and men in different classes? what does social class mean for women?) and moves to poverty, where it is somewhat limited by its reference only to the British scene. Next considered is politics in a double sense — not only malestream politics as we know them, but feminist politics, within and without the movement. In this context, Oakley describes the growth of the Women's Liberation Movement and the procedures followed by consciousness-raising groups, and female support systems. Pointing out that women have some power but little authority, she comments on the folly of any political theory that "ignores the wider community" such as women's pressure and support groups which can and do successfully tackle male-run institutions at various levels.

The final section of the book is concerned with gender. Oakley describes the origins, functions, and growth of women's studies in Britain, the United States, and to some extent Germany and Australia. As well she discusses some of the current examples of feminist expression and construction of social reality: from feminist art and writing to male theft of women's thought, work, and discovery (such as Watson's and Crick's appropriation of Rosalind Franklin's DNA work), to theories of patriarchal phallacy, Mary Daly's work, and the links of recent feminist anthropological research on human origins, to the ideological differentiation between male and female self-concept.

Oakley concludes with an examination of the tenets and tendencies of feminism. The final paragraphs of the book touch on the paradigms of feminist science-fiction, to emphasize the necessity to imagine the kind of world we would have, and the importance of feminist imagination in bringing about social change. This obser-

vation is followed by a 50-page bibliography.

Of course, the book has limitations. Half of human experience cannot be tidily chopped and stored. But at least this is *our* half — in itself a recommendation. *Subject Women* is a useful corrective for the well-meaning but uninformed, and a more or less adequate overview of women's contemporary world. I do not wish to be taken as damning with faint praise, here. She has undertaken an impossible task and done an excellent job. To produce anything more than an adequate overview is probably impossible. I would recommend it be included in malestream courses and read by those involved in more traditional (men's) studies, including labour history. And of course it belongs on the reading lists and in the libraries of all students of women in the last century.

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Harry Cleaver, *Reading Capital Politically* (Austin: University of Texas Press 1979); Michael Eldred and Mike Roth, *A Guide to Marx's Capital* (London: CSE Books 1978); and Diane Elson, ed., *Value: The Representation of Labour in Capitalism* (London: CSE Books 1979).

THESE BOOKS POSE THREE intriguing, fundamental questions for readers of Marx's *Capital*: What is the scope of application of Marxian value theory? Is that theory a theory of class struggle? What is the meaning of the so-called "value-form chapter," the obviously crucial but agonizingly obscure section 3 of *Capital*, chapter 1, volume I? I will begin by sketching my version of the correct answers — a version based entirely on teachings of the Uno school of Japanese Marxian economics.[1] Then I will describe and comment on the books under review. I conclude that the Eldred-Roth and Elson books are commendable additions to the literature. First, what is the proper scope of application of Marxian value theory? In particular, what should be the relation between theory and reality?

I would answer that an essential aim of political economy must be empirical analysis of the actual state of capitalism both in the world as a whole and in different countries. But this neither means that value theory must be realistic nor that it must lend itself to immediate application. What is required is a three-level approach running from pure theory to stages theory to empirical analysis of current economic developments. This is obviously not the approach taken in *Capital*. Nevertheless, if political economy is to avoid both dogmatism and revisionism, it is the necessary approach. As Uno's *Principles* demonstrates, this is the only methodology which yields an objective and logical value theory. This methodology recognizes the uniqueness of historical events, the importance of historical research, and the need for history as a distinct academic discipline. No other economic methodology is so kind to history.

The pure theory is more specific than a theory of exchange-in-general. It excludes treatment of face-to-face barter to focus on impersonal and reified commodity trade. But such commodity trade is still too general. The pure theory deals only with commodity-economic relations which are stable and continuous, that is, *value relations*. Finally, specificity is taken one step further. Value relations are only studied in one context — in the society consisting entirely of value relations, the purely capitalist economy.

Alternatively, one can say that the pure theory is based on abstractions such as the following: all workers are obliged to sell their labour-power to survive; there

[1] See Kozo Uno, *Principles of Political Economy* (Sussex 1980), and Brian MacLean, "Kozo Uno's *Principles of Political Economy*," *Science and Society*, 45 (Summer 1981), 212-27.

is neither foreign trade nor foreign currency; the supply of money is regulated by the production of gold. The tendency to purification which English capitalism exhibited towards the 1860s provides such abstractions as presuppositions; the internal consistency of the pure theory itself explains both their validity and necessity. As features of a society composed solely of indirect human relations, these abstractions are tied together by a unifying principle, and this enables the pure theory to avoid the charge of arbitrariness. As features of a society abstracted from tendencies which English capitalism exhibited towards the 1860s, they give the pure theory a real historical basis, which falsifies any accusation that the pure theory is idealist or mechanical.

As stated above, the pure theory presupposes a society with an economic life in which all human relations are indirect relations based on commodities. But the extent to which reality is approximated by such a society depends on the use-values, and hence the technology, which form a society's economic base. Consequently, pure theory and reality must be mediated by a stages theory in which use-values appear as an aggregate type associated with a distinct technology. (The three stages of capitalism are mercantilism, liberalism, and imperialism.) On one side, an adequate stages theory removes the temptation to use the pure theory for purposes it cannot fulfill. On the other side, it removes the temptation to corrupt the pure theory in response to changes in capitalism.

Second, is the value theory a theory of class struggle? If not, what is it a theory of? To answer these questions I must initially clarify what is meant by the expression "value theory." I maintain that it can reasonably be given two meanings, which I shall term narrow and broad. In the narrow sense, value theory corresponds to those parts of *Capital* which deal exclusively with the law of value. (This law states that the value of a commodity is equal to the quantity of socially necessary labour directly and indirectly spent for its production). More specifically, value theory in the narrow sense consists of the value-form theory together with the necessity, the absolute foundation, and the concrete mode of enforcement of the law of value. The value-form theory develops the distinction and connection between value (an absolute concept) and exchange-value (a relative concept). The necessity of the law of value refers to the proposition that the viability of capitalist society is equivalent to the validity of the labour theory of value.[2] The absolute foundation of the law of value deals with the reproducibility of capitalist society insofar as the reproducibility depends on the periodic reproduction of means of production and articles of consumption in appropriate proportions. The concrete mode of enforcement of the law of value shows how the law enforces itself by determining production-prices not necessarily proportional to values and individual production-prices different from market production-prices. In the case of value theory in the narrow sense, few would argue that class struggle is the central theme.

Value theory in the broad sense is identical with the pure theory. And the pure theory contains many topics which might be regarded as elements of a theory of class struggle. Examples include the production of absolute and relative surplus value, and the co-operation-manufacture-modern industry triad. The pure theory, however, demonstrates how a commodity economy minimizes such extra-economic factors as class struggle, which, after all, involves relations among humans other than indirect relations mediated by commodities.

This is hardly to say that the pure theory is irrelevant — quite the contrary.

[2] See Thomas T. Sekine, "The Necessity of the Law of Value," *Science and Society*, 44 (Fall 1980), 289-304.

The pure theory constitutes the only completely objective knowledge of capitalism. Marxism, an ideology of replacing capitalism with socialism via class struggle, distinguishes itself from other socialist ideologies by standing upon a scientific analysis of capitalism. The pure theory is relevant to class struggle, then, by providing the objective knowledge that distinguishes enlightened from blind ideology. It tells us everything there is to be known about completely commoditized class relations.

Third, what is the meaning of the "value-form chapter," that is, section 3 of *Capital*, chapter I, volume I?

The undisputed aspects of the text have been usefully summarized as follows:

> The value-form means a specific form or mode of expression of value as exchange-value. Thus if the value of a commodity, say A, is expressed in a given quantity of only one other commodity, say B, this mode of value expression is called the *simple or elementary value-form*. If the value of A is expressed by definite quantities of many other commodities, B, C, D, . . . etc., the mode of value expression is called the *expanded or extended value-form*. If the value of A is expressed in the use-value of a commodity X in which all other commodities express their values, the mode of value expression is called the *general value-form* and the particular commodity X a *general equivalent*. When X is unique so that it is *the* general equivalent the same mode of value expression is called the *money-form of value*. The price of a commodity is its money-form of value per physical unit of the commodity. A commodity whose value is expressed is said to occupy the position of the *relative value-form* and the commodity in the use-value of which the value of another commodity is being expressed is said to occupy the position of the *equivalent form of value*.[3]

The following observations, which are not so obvious, should also be made. First, although the value-form theory brings out many subtle points, its major contribution is to develop the distinction and connection between value and exchange-value. Second, the real significance of the progression from the simple value-form to the expanded value-form to the general value-form to the money-form of value is that it corresponds to an increasingly adequate expression of the social uniformity of value. Third, the commodities whose value are being expressed are capitalistically produced. They are genuine commodities with no use-value for their owners. Fourth, the value-form theory seems much less mystical, if the owners behind the commodities are explicitly considered. Otherwise, the theory emits an aura of fetishism, since commodities appear to be going to market and engaging in exchanges of their own accord. Fifth, the simple and expanded value-forms should be regarded as expository devices analogous to the concept of simple reproduction under capitalism. Logically speaking, these concepts cannot have historical counterparts. But it helps to think of these incomplete value-expressions as classified newspaper advertisements for standardized commodities. Sixth, the quantity in the equivalent form is definite, whereas the quantity in the relative form is tentative. Marx's exposition obscures this point. For example, his illustration of the extended value-form should show that the linen-owner must vary the quantity of linen standing in the position of relative value-form according to the commodity assuming the position of equivalent value-form. Yet in Marx's illustration all the equivalent commodities face the same 20 yards of linen. Similarly, the value expression in Marx's money-form of value resembles pricing in a literal 10 cent store.

Let us now turn to the three books under review, beginning with Cleaver's *Reading Capital Politically*. Because Cleaver advocates a rather particular

[3] Thomas T. Sekine, "Appendix II: A Glossary of Technical Terms," in Uno, *Principles*, 176.

political reading of *Capital*, mention of his political views is relevant. He has strong anarchist tendencies: stealing, dropping out of school, and cheating on welfare and unemployment payments are all regarded by him as admirable forms of behaviour. Yet, unlike most anarchists, he is completely uncritical of Marx's economics. His intellectual influences include the Johnson-Forest Tendency ("which arose in the American Trotskyist movement in the 1940s and then split from it in 1950") and the European *Socialisme ou Barbarie* group (which arose when several French members of the Fourth International formed "an opposition faction and then an entirely separate group which published the review *Socialisme ou Barbarie* (1949-1965)."

Reading Capital Politically is divided into six chapters. The first is an historical overview of various approaches to reading *Capital*. The remaining chapters largely focus on the first 3 sections of *Capital*, volume I, chapter I. Cleaver's position on the scope of application of value theory is nicely summarized on the book's dust jacket: ". . . Marx can be both understood and applied directly and . . . Marx's categories still express the fundamental character of social relations." On the question of whether Marx's value theory is a theory of class struggle, Cleaver's answer is a resounding "yes." To support these answers Cleaver relies mainly on a variant of *argumentum ad superbiam*: if you do not agree with this reading of *Capital*, you must be an enemy of the working class. Cleaver's dubious contribution to the literature on value-form theory is to show how to use the most abstract Marxian categories to sermonize about current economic events. In short, *Reading Capital Politically* adds little to Marxian political economy. It is quite clearly written, however, and labour historians might find some useful facts in the introductory chapter.

The authors of *A Guide to Marx's "Capital"*, Eldred and Roth, are Conference of Socialist Economists (CSE) members whose reading of *Capital* is influenced by German Marxian economists who have worked under the banner of value-form analysis. The brief *Guide* (128 pp.) contains an introduction, five papers dealing with topics in all volumes of *Capital*, appendices on "Family in *Capital*" and "Science in *Capital*," and a lengthy glossary. The *Guide* is intended as a practical study aid for individuals involved in a *Capital* reading group.

Eldred and Roth lack Cleaver's clarity as to the proper scope of application of value theory. Occasionally they tend towards the vulgar view that value theory is directly applicable to reality. Several passages of their guide, however, suggest remarkable astuteness on this question. They recognize that parts of *Capital* do not strictly "belong to the dialectic which unfolds the systematic presentation." In particular, they argue that much of the historical material in *Capital* properly belongs elsewhere. I interpret this as an implicit recognition that there is a need for a pure theory lacking direct applicability to historical reality. Since explicit recognition of this point logically implies that value theory is not a theory of class struggle, it is hardly surprising that although Eldred and Roth want their *Guide* to be a practical tool for activists, they ignore capital-labour conflict to a degree that Cleaver would find scandalous. Finally, on the question of the meaning of the value-form theory, the authors adequately summarize the main concepts, and they recognize that the commodities are industrial commodities.

What makes the *Guide* worth reading is the scarcity of alternative commentaries on *Capital*. Eldred and Roth do make some important points. Like Uno, they recognize that: (1) chapters 1-6 of volume I form a unit (Uno calls it the Doctrine of Simple Circulation), and (2) that Parts VII and VIII of volume I stand out of systematic order, and that the material in Part VII really belongs at the end of volume II (in

what Uno calls the Theory of the Reproduction Process of Capital). Unfortunately, Eldred and Roth lack a sound methodological basis for deciding the proper organization of the pure theory so that even when they are correct, their reasoning is less than convincing. Another contribution of the *Guide* is to make explicit many of the assumptions of presentation found in Marx's value theory. But it fails to distinguish between assumptions of presentation and real abstractions. For example, equalization of the rate of surplus value is falsely treated as an arbitrary assumption of presentation, despite the strong basis in *Capital* for regarding equalization of the rate of surplus value as a real abstraction. (117)

The dreadful editing and production of the *Guide* deserve mention. The book contains many typographical errors, lacks necessary italicization, and has an ugly type face and cover design. As to editing, one wonders whether any was done. That this short book contains a 22 page essay on "Science in *Capital*," complete with a reference to a forthcoming article called "The Maturity of Avocadoes: in general and with special reference to the north coast of NSW, Australia" indicates a lack of balance.

In comparison with the *Guide*, *Value: The Representation of Labour in Capitalism* is a glossy publication. The major question emphasized in *Value* is the meaning of Chapter 1 of *Capital*. Papers by Banaji, Kay, Arthur, and Elson all deal with this question. By emphasizing the importance of the value-forms, these authors are united in opposition to the neo-Ricardians. By agreeing that Hegel's influence on value-form theory was positive and needs to be understood, the authors are united in opposition to the Althusserians. By stressing that the commodities considered are the commodities of industrial capitalism (as opposed to the commodities of a simple commodity economy), the authors are united in opposition to superficial "historical transformation" interpretations. In short, the authors provide a fine defence of Marx. The shortcoming of their work is that although they are generally critical enough to sense that Marx's "proof" of the labour theory of value in Chapter 1 leaves something to be desired, they lack the boldness necessary to construct a clear and logically tight value-form theory. The essays in *Value* contain little explicit discussion of the scope of application of value theory. Nevertheless, most of the authors, especially Itoh (who is a member of the Uno school) and Arthur, seem aware that the theory is not directly applicable to reality. The 65-page essay by Elson asks: what is the theory of value a theory of? Though she is sympathetic to the incorrect view that the theory is a theory of class struggle, she considers other views at length and provides an intelligent discussion.

Value is an outstanding collection of essays. The essays may be far from perfect, but several of them deserve serious reading and rereading. The essay by Aumeeruddy and Tortajada, "Reading Marx on Value," provides historical information on all that Marx wrote on value theory. Their acccount is marred only by their questioning of the importance in Marx's economic thought of volumes 2 and 3 of *Capital*. Banaji's essay, "From the Commodity to Capital," explores the connection between Hegel's dialectic and the early chapters of Marx's value theory. It is an extremely laudable, though basically unsuccessful, attempt. Kay's "Why Labour is the Starting Point of Capital" is an essay of superb clarity which previously appeared in *Critique*. It critically examines Bohm-Bawerk's attack on the opening of *Capital*. Arthur's "Dialectic of the Value-Form" is an excellent discussion of what the theory of value-forms is not. If only it also explained what the value-form theory is, it would be one of the best essays in the Western literature on Marxian economics. Itoh has contributed an important essay on

a much neglected topic: "Marx's Theory of Market Value." Because of its brevity, however, it leaves too many questions unanswered. Labour historians may be interested to know that Elson's stimulating essay makes numerous references to E.P. Thompson's *The Poverty of Theory*. The major problem with Elson's essay is that it denies that capital is the subject of Marx's value theory. Given the strength of "workerism" in current Marxist ideology, her view is quite in touch with the times. But *Capital* was named *Capital* with good reason.

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Ben Fine and Laurence Harris, *Rereading Capital* (New York: Columbia University Press 1979).

THE MODERN REVIVAL of Marxism has been slow in taking up the task of renewing and extending Marx's analysis of the capitalist economy. For a long period problems in Marxist philosophy, such as the theory of alienation, or issues in Marxist political theory, such as the nature of the capitalist state, dominated the literature. Since the early 1970s, however, Marxist economic theory has undergone something of a rebirth in Britain. In the forefront of this rebirth has been the Conference of Socialist Economists (CSE) which began producing a bulletin in the early 1970s and which today produces the journal *Capital and Class*. This book by Fine and Harris represents a balance sheet of the debates generated by the CSE over the past decade.

As a catalogue and a summary of the most important of these debates, *Rereading Capital* is a valuable work. The book is divided into two parts. In Part One, Fine and Harris review the literature devoted to the theory of value, the problem of the transformation of values into prices, productive and unproductive labour, and the theory of the tendency of the rate of profit to fall. In Part Two, they take up contemporary problems such as the nature of the capitalist state, the historic stages of capitalism, and the question of imperialism. Fine and Harris isolate two basic theoretical perspectives within these debates: Neo-Ricardianism and Fundamentalism. Neo-Ricardianism describes the orientation of those writers, best represented by Ian Steedman, who contend that Marx's theory of value is an entirely unnecessary construction. Marxist economics, they suggest, simply requires a theory of price which can be mathematically formulated and which demonstrates that the dynamics of capitalism are determined by distributional struggles between workers and capitalists over the division of the social product into wages and profits. As a result, Neo-Ricardians identify the sphere of distribution as the locus of analysis. It is there that they locate the class struggle and the basic tendencies of capitalist development. The Fundamentalists, on the other hand, *appear* to be closer to Marx in their emphasis on the primacy of production. It is within production, they maintain, that the basic dynamics of capitalism are determined — especially the crisis tendencies of the system which are expressed ultimately in the tendency for the rate of profit to fall.

Fine and Harris contend that both the Neo-Ricardians and the Fundamentalists commit a critical methodological error. Both are guilty of a form of reductionism insofar as they abstract one sphere through which capital passes in its circuit and make it the determinant of the system as a whole. Fine and Harris follow Marx's insistence that capital is value in motion which passes through the forms of commodity, money, and productive capital. Marx's approach, they argue, is one which grasps capital as a totality in motion. Capital, therefore, appears at different moments within the spheres of production and circulation. But understood scientifically, the circuit of capital reveals itself as a unity of production and circula-

tion. To make one moment or one sphere of capital's circuit the determinant of the circuit as a whole is to fetishize one form of the appearance of capital. It is precisely such fetishism which both the Neo-Ricardians and the Fundamentalists commit by designating the spheres of circulation and production respectively as the determining elements of the Marxist analysis of the capitalist economy.

Important as is this insistence by Fine and Harris on the need to conceive of capital in terms of the totality of its circuit, their work also is plagued by a certain one-sidedness. And their error too is methodological in nature. It consists in an excessively narrow conception of Marx's project in *Capital*. *Capital*, they write, "is the theory of the economic level in the capitalist mode of production." (17) They work with a model of capitalist society as a hierarchical system of levels or structures in which the economic level is ultimately dominant. Such a conception of capitalist society prevents them from grasping the truly revolutionary character of Marx's undertaking in *Capital*. Rather than seeing *that capitalist society is a* unity of processes directed by the production and reproduction of the capital-relation, Fine and Harris fracture the basic unity of society into "levels." Despite the odd statement to the contrary, they fail to understand that capital is a social relation and that Marx's analysis in *Capital* is designed to demonstrate that the motion of capital is a circuit in which the social relation between wage-labourers and capitalists is continually reproduced (and extended). Economic production is for Marx simultaneously a process of social reproduction. In the course of producing commodities, the working class continually reproduces the capital-relation. Thus, capitalist production is much more than the production of things (commodities); it is the reproduction of a social (class) relation. Marx writes in *Capital*, for example, that "The capitalist process of production, therefore, seen as a total, connected process, i.e. a process of reproduction, produces not only commodities, not only surplus value, but it also produces and reproduces the capital-relation itself; on the one hand the capitalist, on the other the wage-labourer."

Fine and Harris insist, however, that "the analysis of the economic laws of motion of capitalism can be undertaken in abstraction from the social reproduction of class relations as a whole." (94) Such an approach flies in the face of Marx's method and is incapable of making sense of much of *Capital*. By separating economic analysis from the analysis of the social reproduction of class relations, Fine and Harris shatter the unity of capitalist society as depicted by Marx in the unity of the circuit of the total social capital. They fail to understand that the unity of the circuit of capital is the unity of a process in which capital (the crystallization of abstract labour) reproduces itself.

Having failed in Part One of their book to grasp the essentially revolutionary character of Marx's project in *Capital, Fine and Harris tend towards the kind* of scholasticism which characterizes all attempts to theoretically reproduce capitalism as a structure of concepts which corresponds to the structure of "levels" in society. As a result, Part Two of *Rereading Capital* which takes problems such as imperialism and the nation state lapses into empty formalism. Rather than moving through mediations to concrete, historical analysis of capitalism today, Fine and Harris remain content with an essentially arid refinement of logical constructs. Their analysis of contemporary problems such as the stages of capitalism, imperialism, and the relation between state and economy is conspicuous in its failure to produce any genuine historical perspective on these problems. We are confronted, therefore, with the paradox of Marxists who attempt to discuss the historical stages of capitalism without producing any historical refer-

ents. As a result, Fine and Harris conclude a discussion of the role of nation states in a world economy dominated by multinational corporations with the statement that "there exists a complex structure of national and international state apparatuses, some of which are more distanced from the site of class struggles (the national social formation) than others." (160) Undoubtedly this is true. But what does it add to our understanding of world capitalism today?

Rereading Capital is, therefore, a highly uneven work. It represents a valuable summary of the debates that have arisen with the British revival of Marxist economics. It fails, however, to recognize the character of Marx's undertaking in *Capital;* it fails to understand that Marx was attempting to demonstrate the fundamental unity of the processes of economic production and social (class) reproduction. Despite its important insights into certain technical problems in Marxist economics, as an attempt to revive Marx's approach to the analysis of capitalism *Rereading Capital* is a disappointment.

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John M. Maguire, *Marx's Theory of Politics* (New York: Cambridge University Press 1979).

1848 IS CRUCIAL. 1789 saw the classic bourgeois revolution in France. The 1848 uprisings should have produced a similar result (a bourgeois government) in Germany, and the next stage (proletarian revolution) in France. So, at least, a reader of the *Communist Manifesto* might be forgiven for believing.

Unfortunately, historians like Cobban have shown that in 1789 there was little left in France which could meaningfully be called feudal, and the bourgeoisie were more interested in joining the aristocracy than in overthrowing them. Moreover, if 1789 was not a classic bourgeois revolution, 1848 in Germany saw the bourgeoisie gain electoral victory, only to lose office again as the state apparatus as well as allies from other classes turned against them. Finally, 1848-51 in France produced not a revolutionary advance but a relapse to the royalist-supported empire of Louis Napoleon. Thus the inevitability of the proletarian revolution suffered a major set-back. Marx retreated to make elaborate revisions.

John Maguire brings to the critical analysis of this political sketch both a good deal of historical detail from the crucial months, and an admirable familiarity with the Marx/Engels texts. He shows us Marx's sophisticated and empirically sensitive theorizing, and constantly pays attention to Marx's journalism of the period, to his assessments of events, and to his predictions. The latter, both successes and failures, are especially illuminating.

For example, there are textual grounds for distinguishing between a bourgeois government (when members of the class form the government), and a government which acts in the economic interests of the bourgeois class. The hypothesis that the growth of capitalism will give rise if not to the former then to the latter proves to be relatively accurate. Further, in both France and Germany the bourgeoisie lost formal power partly as a result of alienating peasant and proletarian allies, but in both cases they had had various potential allies, had had to make choices, and their downfall became "more and more inevitable" as they made fateful decisions. "Marx argues in terms of the real possibilities available at any juncture rather than in terms of a uniquely predetermined process or 'path'." (94) These historical set-backs disappointed Marx, who was consistently of the view that the workers should never join an anti-bourgeois alliance. Nonetheless, his *theory* of history, as Maguire presents it, is not falsified by such "developments."

These claims have interesting philosophical consequences. There is no "one-motive theory" (that every action is

performed for economic self-interest) in Marx. What can be said is that *sometimes* people have economic motives which they do not recognize, and that crises will often *produce self-defensive economic motives where there were none previously*. Second, there is freedom of action, both individual and political. This is further illustrated in a sort of appendix on the question of whether the old Russian farm commune might survive to be a basis for post-capitalist communism in Russia. In what sense, then, does base determine superstructure? What remains of this central doctrine of historical materialism? Marx's materialism is not ontological, but primarily methodological; it combats a priorism in science and over-emphasis on the role of ideas in history. In his own historical explanations, Maguire insists, Marx did not presume, for example, that economic change must precede non-economic change. To give the famous claim some content, Maguire invokes Hegel's notion of the organic state. It is in the *nature* of the state that certain (economic) features are determining or sus*taining ones. During the historical genesis* of that state the same features may have been successors of, even been determined by, features which in the mature state are determin*ed*. This places Maguire considerably to the right, as it were, of Gerry Cohen, and even, I think, of John McMurtry, on this question. (The thesis does not simply disappear, however, as it does in Peter Singer's pot-boiler.)

Although Maguire handles such questions with clarity, he does so briefly and in the margin, so to speak. His central concerns are with the parts of Marxist theory which deal with class interests, political power, revolution, and of course with the state.

The problem which gets the book started is the tension between the account of the state as "servile" ("a committee for managing the common affairs of the whole bourgeoisie," as Marx once described a part of it), and as "dominant" (in which it controls and corrupts society, not merely serves and reflects it). It is not, Maguire claims, simply tautological that when a class is firmly in control, the *apparatus of the state will serve that class's* avowed interests. There can be a built-in propensity for what Maguire calls "pretention." In circumstances of bourgeois rule, for example, the democratic state should, by pretense, appear to rule equally in the interests of all, and the resulting institutions may in fact sometimes act against the ruling class's interests. This discussion of the state, and of the possible character of the "post-political" age (every society has government, only class-divided societies have politics), is particularly useful.

This hints at the ambitious range of problems which Maguire takes up. He leaves the impression that in all these matters Marx was concerned to adapt theory to the flux of contemporary history, and instead of clarion theses we have circumspect conclusions like: The Marxian approach has as one of its basic elements "a claim about the central role of the *economy which, whatever the logical and* conceptual problems of the general materialist conception of history, is applicable in the area of politics." (242) That such conclusions are reached by means of illuminating factual detail and real clarity of argument makes the book difficult but salutary reading.

S.A.M. Burns
Dalhousie University

Simon Clarke, ed., *One-Dimensional Marxism* (London: Allison and Busby 1980).

THE 1970S WITNESSED a flowering of Marxist intellectual work in Britain. Yet the garden, E.P. Thompson has argued, contained many weeds, some transplanted from foreign soil. *One-Dimensional Marxism* is a collection of four essays by authors who share Thompson's concern to root out the most noxious and ubiquitous

of these: Althusserian Marxism. The four essays, however, go about this task in different ways and the differences are important.

The editor, Simon Clarke, adopts a polemical style throughout his introductory essay. Althusser's work is dismissed as simply offering "familiar, if rather esoteric, bourgeois ideologies wrapped, often insecurely, in Marxist rhetoric, which serves to give both bourgeois ideologies and Stalinist politics an authentically Marxist appearance." (13) This style of critique — however lively and amusing when it flows from the pen of a superior writer such as Thompson — reproduces the very "intellectual terrorism" with which Clarke charges Althusser. We are effectively incited to delete Althusser's work from the list of "genuinely" Marxist readings because they are deeply contaminated with bourgeois and Stalinist ideas. There is no effort to assess the real contributions of Althusser to contemporary Marxism. Moreover, this method of attack effectively suggests a closure to work done by non-Marxist ("bourgeois") intellectuals — which Clarke's brief footnote, clarifying his use of such terminology, does nothing to correct.

Fortunately, the other essays adopt a more positive stance, recognizing the value of Althusser's emphasis on the specificity and the "reality" of ideology and on the insights which Marxists may gain from fields like psychoanalysis and semiotics. Their criticisms are designed not to root out the Althusserian weed but to pinpoint some of the problems.

The central concerns of these other essays in this volume are related to Althusser's stress on the "relative autonomy" of economics, politics, ideology, and theoretical practice, especially the last two. More specifically, it is argued that Althusser's emphasis on theoretical practice has the effect of sanctioning the division between mental and manual labour so characteristic of capitalist relations of production. This emphasis is also seen as a barrier to overcoming the isolation of intellectual from popular struggles, a problem which Anderson has argued is characteristic of Western Marxism.

The charge that Althusser and the Althusserians see as a trans-historical necessity what is really a characteristic of capitalism seems untenable in its general form. For instance, Poulantzas' work focuses precisely on this as a basic feature of capitalist social relations and in *State, Power and Socialism* develops this to include technocratic tendencies on the left in a way that should give pause to certain proponents of the labour left's correspondence between Althusser and Maria Antonietta Macciochi *(Letters from Inside the Italian Communist Party)* to recognize Althusser's own sensitivity to the problems posed by practical politics. Nevertheless, these essays show that these criticisms do have some validity.

For instance, the two essays on culture — K. McDonnell and K. Robin, "Marxist Cultural Theory," and T. Lovell, "The Social Relations of Cultural Production" — show how Althusser's conception of ideology has resulted in a rejection of "realist" works (films, for example, which work with the "familiar" and thus can only have an "ideological effect") in favour of avant-garde productions, accessible only to an intellectual elite. Althusser might reply that his conception of ideology strips the term of its necessarily pejorative connotation: ideology constitutes subjects/actors and the subject thus constituted could be a socialist working class. Yet McDonnell and Robin's detailed critique of the important British film journal, *Screen*, demonstrates that not all Althusserians have grasped this. Both essays also show that some Althusserians have ignored the implications of Althusser's qualifier, *relative* autonomy, confining their analyses of cultural products to "the text" and its internal mechanisms in a way that completely

obscures the broader relationships within which such texts are produced and consumed. Important questions such as the degree of penetration of capitalist "economic" relations into the sphere of culture and the role of the state in regulating culture are thus overlooked.

The capacity of Althusserian theory to inform political practice is questioned by Lovell and V.J. Seidler. For Seidler, Althusser may have taught us how ideology secures the "conditions of existence" of capitalist relations of production but he fails to illuminate the "conditions of resistance." The latter appear only in the form of abstract structural contradictions, distantly related to the concrete experience of struggle. It thus becomes difficult to draw out the lessons to be learned from participation in contemporary social movements such as the women's movement. Lovell argues that Althusser's emphasis on ideology as a mechanism of subjection to the dominant class leads one to overlook the way in which social relations are "lived" at least partly outside the terms of the dominant ideology. As Antonio Gramsci argued, there is always a nucleus of good sense within "common sense" ideologies and Marxist theory must be able to discern this nucleus if it is to be developed into a coherent world outlook.

Certainly it is not enough to locate the conditions of resistance at the level of structural contradictions characteristic of a mode of production. It is also true that, especially in his most influential piece on ideology — "Ideology and the Ideological State Apparatuses" — Althusser emphasizes its functional (for capitalism) character. Yet, in making a distinction between "mode of production" and "social formation," Althusser has opened the way for analysis of the contradictions operative in a society at a particular conjuncture. Others have begun to develop its implications. For instance, Laclau (the one Althusserian "kangaroo" for whom Thompson admits some respect) has worked with this distinction — and Althusser's broader, less pejorative conception of ideology — in a way which yields insight into the process whereby new, potentially revolutionary, subjects are formed. In Canada, Jane Jenson and Janine Brodie *(Crisis, Challenge and Change: Party and Class in Canada)* have drawn on Adam Przeworski's variant of Poulantzas' conception of class formation, to develop an insightful analysis of the history of struggles to make "class" a principle of political identity.

The point is not that "Althusserian Marxism" warrants the preeminent position that some may claim for it nor is it that the Althusserians have produced the "scientific problematic" which the rest of us need only work to elaborate. It is simply that contemporary Marxists do have something to learn from Althusser and at least certain Althusserians just as they do from Thompson and others. This, all but one of the contributors to *One-Dimensional Marxism* recognize.

Rianne Mahon
Carleton University

D. Ross Gandy, *Marx and History: From Primitive Society to the Communist Future* (Austin: University of Texas Press 1979).

IN THIS SHORT, but compact, volume D. Ross Gandy strives to present a sympathetic summary of the essential views of Marx and Engels on pre-capitalist modes of production, capitalism, communism, and some key methodological and theoretical components of the Marxist theory of history (base and superstructure, the theory of classes, and so on). Absent from the work is the kind of close analysis and evaluation of Marxist historiographical principles of the sort to be found, for example, in G.A. Cohen's *Karl Marx's Theory of History,* John McMurtry's *The Structure of Marx's World-View* or Melvin Rader's *Marx's Interpretation of History.* Absent, too, is the scholarship into the history surrounding Marx's thinking

of Leszek Kolakowski's *Main Currents of Marxism*. Nor is reference to be found to crucial debates over the meaning or adequacy of Marx's theories such as that of E.P. Thompson and others with the structuralist Marxists or those over how Marxists do or should regard the state. Nonetheless (or perhaps even partly *because* of these absences) I conclude that this would be a good textbook for an introductory undergraduate course in Marxism.

Whether this work is meant as an introductory text or not, it has some distinct advantages if regarded from this point of view. Gandy's book touches *almost* all the bases in reviewing Marxist principles of history; it simplifies without being simplistic; and it is organized in a way that is likely to hold the interest of history students, emphasizing as it does Marx and Engels' applications of their views to history. A distinct accomplishment of the work is that it incorporates many quotations from the broad corpus of Marx and Engels' works without being tedious. Gandy writes clearly, sometimes even crisply, and in a tone that avoids didacticism by revealing just enough of his admiration for Marx and Engels as people (including, alas, a chauvinist reference to their sexual exploits as part of an illustration that they were well-rounded men of their times, 93).

There are many loose ends in the work. Those who think there are radical differences between the early and late Marx will (whichever Marx they favour) find insufficient treatment of the development of Marx's views and of his relation to Engels. Marx's views on the state are compressed into only a few sketchy pages. The economic theory of capitalism is sparsely treated. The tentative speculations of Marx and Engels on reproduction and its relation to production are ignored. Considered as a textbook this means that one has something to lecture about. Indeed, as a textbook the main failing of Gandy's book is that it does not exist in paperback and hence is on the expensive side.

The parenthetical comment above regarding what is appropriate to an introductory book should be qualified by noting that striving for complete abstraction from inter-Marxist controversies and from an evaluation of shortcomings in Marx's theories makes for dull reading and, more crucially, foregoes that element of *Kritik* that helps to make Marxism a living and political force. Gandy would surely not disagree with this, and in fact he attempts to incorporate such elements into his survey. It is here, however, that I think his treatment is the most weak. In the introduction Gandy criticizes what he sees as the dominant Soviet approach to Marxist history and the exclusive Chinese one for being "unilinear." (Chapter 1) His specific criticisms of views that suppose crude "inevitable stages" interpretations of history in this connection are well taken, but Gandy makes reference several times to an alternate, "multilinear," view.

The problem is that it is never made clear just what this multilinear theory of history is. Gandy seems not to have anything like Althusser's concept of "overdetermination" in mind or Rader's notion of "the organic totality model," though perhaps he would be prepared to attribute either or both of them to Marx. Usually he seems to mean that a multilinear approach denies that all parts of the world can be explained by the same sort of historical theory. (See 106ff.) Thus, after summarizing the famous "Preface" to the *Introduction to the Critique of Political Economy* presented as the heart of "Marxist sociology," Gandy asserts that this sociology was designed only to "explain Western Europe's evolution out of feudalism into capitalism toward communism." (120) But later, when summarizing "materialist theory" in a way that on the face of it employs concepts no more general than those in the "Preface," Gandy says that "Marxists can use these concepts in the study of societies any-

where." (154) If the idea of multilinear history is to provide an organizing principle of interpretation in Gandy's presentation, it should have been more rigorously treated.

In an appendix entitled "Marx's Theory of Revolution," Gandy expresses his misgivings about the applicability of Marx's view of proletarian revolution to contemporary industrialized capitalism. He concludes that the proletariat is no longer a likely revolutionary agent due to its identification with capitalism and its relative well being and that "the future of revolution from below probably lies in economically backward regions of the world." (163) On the other hand, Gandy thinks that the question of "historical agency" is now "less acute for Marxists with the emergence of the Leninist Party," since intellectuals in the underdeveloped world can bring together members from a variety of classes in such an organization. (165) Now it is clear that there is much room for argument over these views, and there has been a lot of it. (To mention just one of many well chewed bones of contention with this sort of view, it can be countered that the problem in whatever industrialized capitalist countries Gandy may have had in mind has been less with their proletariats than with their Leninist revolutionary political organizations.) But this does not mean that it is inappropriate to make judgements like Gandy's by way of critically evaluating Marx's theory in an introductory text.

Gandy, however, does not adequately link his concluding views with his interpretation of Marx. Does he mean to say that it was inconsistent with Marx's own "multilinear" approach for him to predict proletarian revolution? Or does Gandy think that some specific features of Marx's theory were inadequate? In fact Gandy mentions that Marxists have "underestimated the expansionism of capitalism in Western Europe" and lack of "proletarian passion" on the part of those workers who have gained the vote and have better pay and welfare. (162) How central to the Marxist theory of history Gandy has summarized does he think these things are? These sorts of questions need to be answered so that readers of Gandy's book can be helped to evaluate Gandy's evaluation of Marx's theory of history and its practical relevance.

Frank Cunningham
University of Toronto

John Stanley, *The Sociology of Virtue: The Political and Social Theories of Georges Sorel* (Berkeley: University of California Press 1981).

MANY HAVE CLAIMED Georges Sorel as their ideological precursor. H. Stuart Hughes (*Consciousness and Society*, 161) recounts that in the early 1930s, the Soviet and Italian Fascist ambassadors to France both offered to erect a monument over Sorel's grave, which had fallen into disrepair. Frank Manuel (*Utopian Thought in the Western World*, 748) recalls, significantly, that Sorel's name was resurrected during the 1968 student uprising in Paris. That enemies of bourgeois parliamentarianism, anarchists, and advocates of heroic violence considered themselves Sorel's disciples, is no surprise. John Stanley wishes to confer a different pedigree on the man. For him, Sorel was a perceptive critic of Marxism, and a keen unmasker of apologias for domination. According to Stanley, Sorel's spiritual heir was Camus, not Lenin or Mussolini.

What Sorel criticized in Marxism was threefold: its "concept of totality" (311) or its claim to a total rationalistic, explanatory system encompassing all of reality; its determinism; and its "fixed and unitary notion of the political good." (111) For Sorel, Marxian pretensions to a system promoted the dominion of an elite of intellectuals and politicians — keepers of the theory — over the masses. Determinism fostered a view of men as passive, the tools of vast impersonal forces. Utopianism encouraged Statism, the use

of power to bring into being an ideal future of unity and harmony. For in the utopian vision, society was monolithic, conflict was dysfunctional, and would disappear. According to Sorel, nineteenth-century positivism sought the unity of knowledge, as the intellectual framework for utopias of unity and harmony. Intellectuals inflated the role of science, to legitimize their paternalistic dominion over the masses. But science as Sorel saw it, was no more than a pragmatic tool, while scientific knowledge was tentative and self-correcting.

In keeping with this interpretation, Stanley considers the *Illusions of Progress,* not the *Reflections on Violence,* as Sorel's "most interesting book." (188) Characteristically, Sorel analyzed there, the idea of progress as a mask for domination, as an ideology promoting social quietism. Advocates of the idea of progress viewed the direction of history as benign and consequently sanctioned historic trends toward centralized monarchy and monolithic sovereignty. As well, the idea of progress bestowed great importance upon the cultural and intellectual achievements of literary intellectuals in the Age of Enlightenment. As such, progress as an ideology fostered an exploitative division of labour in society. Society, in this view, was divided into consumers — engaged in "higher things" — and producers; consumers produced culture, while ruling over producers and living off their labour. Hence Sorel's approval of the execution of Socrates, whose intellectualism brought inequality and dominion into society. Socrates wished to replace the rough equality of rural citizens in the old polis and the uniformity of their poetic culture, with hierarchy and meritocracy, on the criteria of intelligence and oratory. He stood for an ethos of consumption and leisure, legitimizing the rule of "those who know." (38)

As an anarchist, Sorel was a critic of large-scale organizations and their exercise of remote and impersonal power, which saps civic virtue and independence and destroys the rough democracy of producers. For Stanley, Sorel has continuing relevance as a critic of these and all other strategies of domination.

Stanley, however, does not come to grips with Sorel's anti-intellectualist irrationalism. He emphasizes the Bergsonian and Jamesian elements in Sorel's theory to demonstrate Sorel's regard for intellectual clarity and theoretical consistency. For Bergson the psyche was not fixed and determinate, nor was its response habitual and repeatable, instead anticipations and future-pointed desires shaped behaviour. Adopting Bergson's theory, Sorel insisted that adherence to the myth of the general strike was an act of freedom; hence he was breaking the hold of Marxian determinism and allowing spontaneity to the proletariat. According to Stanley, Sorel's adoption of Jamesian pragmatism, "helped add coherence to ... [his] theory of social myths." (253) Sorel considered the monolithic claims of science and reason pernicious. In keeping with the doctrine of pragmatism, knowledge was pluralistic and intersubjective. Men grouped themselves in various *Cités,* historic communities they recognized as possessing social authority over them. One such was the *Cité savante,* "the scientific estate" (261); another was the *Cité esthetique* represented by the medieval craft guilds dominated by the "poetic spirit" rather than by science or philosophy, and by symbolism drawn from popular beliefs. Here lay the historic basis of the modern ethos of the producers; their claim to autonomy from the dominance of scientific intellectuals. (263) However, Sorel's ultimate appeal is not to intellectual coherence, but to mysticism. For him, the myth of the general strike was irrational, not subject to analysis; it lacked predictive value, and was not to be tested for its congruence to reality. The myth of the general strike was an appeal to conviction, strong emotions, and feel-

ings — not reflection — as the necessary springs of heroic action. Reflection weakens the activist and heroic will. Similarly, "Bergsonian" freedom was actually a self transcending war-like state, in which personality became a self-righteous instrument of absolute good — surely a narrow understanding of Bergson's conception of freedom.

Stanley righly emphasizes Sorel's critique of the authoritarianism inherent in the utopian vision of a conflict-free society. Yet is he, as Stanley suggests, even in part the progenitor of Camus' critique of revolutionaries, their tendency to be cavalier about means, because dazzled by a splendid and utopian end? Although Sorel rejected an eschatology of ends, he nevertheless embraced an eschatology of means. In the personality created through the war-like mythology of struggle, heroic, uncompromising, and absolutist, men triumphed over decadence. Heralding the "new man," made whole through heroic action, Sorel remained a utopian visionary.

Stanley's book is longer than it should be, weighted down with digressions, through which the reader must wade in search of the thesis. For example, in Chapter Six the analysis of Sorel's forays into biblical scholarship add little to the discussion of Sorel's critique of positivism's demystification of religion. In Chapter One, the discussion of whether Sorel's interpretation of Xenophon was correct, is at best a historical curiosity.

Stanley's scholarship is comprehensive, extending beyond the *Reflections on Violence* to a wide-ranging analysis of Sorel's works. This book will be essential reading for students of Sorel. However, the effort to raise Sorel to the rank of a major critic is unpersuasive. Sorel had none of the theoretical range or subtlety of a Max Weber. Beyond his *idée fixe* about prevailing ideologies, Sorel had little to offer. Concerned with the abuses of reason, Sorel proposed only its wholesale rejection. Troubled by the dangers of utopianism, Sorel shared its absolutism and apocalyptic vision.

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Philip Corrigan, ed., *Capitalism, State Formation and Marxist Theory* (London: Quartet Books 1980).

CONTEMPORARY CAPITALISM is in crisis and, given its oligopolistic character, it now looks to the state to solve the problems of stagflation. This world crisis has led a number of nations to impose austerity on its native working classes in order to restore investor confidence and ultimately the rate of profit itself. In Canada, for example, the federal government has imposed a two year package of wage restraints on its own employees, the infamous six and five formula. In England, where unemployment has passed the three million mark, the state assault on working class living standards has been on since the early 1970s. In response to how the state has taken on capital's bloodletting functions, in restoring capitalist profitability after crisis has struck, English marxists have begun to analyze state forms and practices in relation to the evolution of the capitalist mode of production. One recent example of this is the book under review.

Corrigan's collection consists of six essays. The first attempts to situate theoretically the state in Marx and Engels' writings. This is followed by two papers tracing the qualitative breaks in state forms over time; beginning with the Tudor Reformation, through the Cromwellian Revolution, the Glorious Revolution of 1688, the eighteenth-century attack on the provisions economy, to the Reform Bill period of the 1830s. Two other essays examine the modern state's reproduction of capital and labour power through the regulation of feminine and "deviant" sexuality and through such social welfare legislation as unemployment insurance. Finally, there is a penultimate essay by S.

Yeo on working-class alternative state forms and practices after 1850 as embodied in cooperatives, trade unions, and local politics; alternatives that were marginalized in the course of class struggle over the state.

In "The State as a Relation of Production," P. Corrigan, H. Ramsay, and D. Sayer look at a number of ways in how the state form relates to the capitalist order. They start with quotes from Marx and Engels on how the state materially reproduces the wage form of labour power, the source of capital's surplus value. Then they examine, working out from Gramsci, how the state ideologically reproduces itself as an independent and neutral form above the class struggle. They argue that this projection of the reformable state is possible given capitalism's fetishization of daily experience and oppression, thus fragmenting and dividing working-class perceptions and organizational responses. Lastly, they note how Marx and Engels' views about the state changed in relation to their political experience, moving from centralizers to seeing the need to smash the bourgeois state in order to establish a new order based on workers' power as shown by the Paris Commune. This last point is seen as particularly significant in bearing out the authors' redefinition of Marx's basic analytic categories — productive forces and productive relations — in a unitary, relational manner. To Corrigan, Ramsay, and Sayer such a redefinition, in attributing things and powers to both categories, is vital in overcoming crude marxism's inability to distinguish between form and essence; between the form and substance of worker's power as in such "People's Democracies" such as Russia or China.

The two papers on qualitative breaks in state forms are more descriptive. The first, by Corrigan, is on early modern England and relies on a historiographical discussion of major marxist authors for certain breaks: Perry Anderson on the Reformation, Christopher Hill on the English Revolution, and Edward Thompson on the eighteenth century. This period, Corrigan argues, was marked by two phenomena. There was a gradual disengagement of the state from religious and personal rule to the development of a set of comprehensive, systematic institutions which serves as an arena of intra-bourgeois class conflict in the eighteenth and early-nineteenth century. The other major feature Corrigan notes is how the state prepared the way for capitalist change, as in expropriating the peasantry through enclosure. The state, rather than "reflecting" economic change, was used as a class tool to create a bourgeois order.

In Paul Richards' "State Formation and Class Struggle, 1832-48," a more empirical case study is presented charting the agrarian and urban capitalist legislative offensive against the provisions economy in 1832-35 and the retreat from open class confrontation with the rise of the Chartists. As Richards argues, this period must be seen in its context of contending class purposes, between a working class trying to regulate the advance of industrial capitalism, as in the case of the handloom weavers, and a capitalistic class, spread across a spectrum of agrarian and urban capital, bent on creating a free and united national labour market. To Richards, the class struggles of the 1830s and 1840s over the state had little to do with the origins of the present welfare state as historians of the administrative revolution are so apt to claim.

Once the fate of the provisions economy had been sealed by the failure of Chartism, new class struggles emerged over the needs of industrial capital to both reproduce itself through the transmission of property and to secure its self expansion through the commodity of labour power. Rachel Harrison and Frank Mort examine the ambiguous impact industrial capitalism had on the struggle for sexual freedom as one example of this process. They begin by noting how the sexual pattern of inheritance changed in response to

the changing character of capital, from male primogeniture under landed capital to, by 1882, female partible inheritance under industrial capital. During this same period of legislative reform, from 1857 to 1882, middle-class and working-class women obtained greater access to divorce, though administered in a highly class specific way. Prostitutes and homosexuals, however, came under increased state regulation. Harrison and Mort see this in part as the result of the new ideologies of domesticity and imperialism, particularly in the latter's concern with national efficiency and "race suicide." The personal choice to have non-reproductive sex was not recognized in law until recently. The contradictory results of this period of sexual legislative reform, the authors argue, had to do with reproducing the next generation of labour power and to meet industrial capital's need for a modified sexual division of labour, a new patriarchy.

In "The State and Social Policy," C. Jones and T. Novak also draw out the contradictions of social reform presented in a humanitarian form while harnessed to capital's reproductive needs. Instead they emphasize the economic purpose of social welfare reform in renovating working-class labour power after a bout of devastating international competition, as in the 1906 Unemployed Workman's Act (to apply to a skilled few) and in the Beveridge Report (blueprint for the radical Labour government of 1945). They also reveal how the state, through its definition and monopoly of expertise, shapes the perceptions and strategies of its "clients;" dividing the working class internally between deserving and undeserving poor. Social Welfare, these authors argue, has become a subtle form of social control that carefully marginalizes alternative class strategies. But, they remark, the contemporary British ruling class cannot manipulate the welfare system at will. Economic recession, disloyal experts, and rioting youth all set limits to ruling class manoeuvres.

Throughout these essays there is a constant emphasis on working-class alternatives, from those who defended the provisions economy, through the Chartists, to the present day demand for the right to work. S. Yeo looks at one strand of this alternative tradition in the period 1850 to 1900 when a concept of self-emancipation animated working-class practice in the cooperative societies, trade unions, and local politics. These institutions of associational self-help, however, were incorporated by the state in the twentieth century; such as the Workers' Educational Association, the insurance societies in 1911, and in the transformation of the cooperatives into capitalist businesses in the 1930s. So, while Yeo usefully draws our attention to a neglected working-class, anti-statist tradition, he still does not come to terms with the failure of these radicals to recognize the interventionist nature of the bourgeois state leading to their own marginalization or incorporation. However, Yeo does make us aware of this tradition of the struggle for democracy or socialism from below on reform grounds, a tradition which finds a parallel in the Canadian example of Jimmy Simpson.

The one unfortunate feature marring these papers, with the exception of Jones and Novak, is their excessively abstract character. It often appears that the authors have sacrificed clarity of structure and readability to theoretical qualification in an effort to not appear reductive. The problems of structure and accessibility may also flow from an insufficient development of their unitarian redefinition of Marx's philosophical categories, or from an overemphasis of the relational side. This has led to problems in the priorization of subjects and the lack of an explicit discussion of socialist strategies for change in fighting the capitalist state.

If at times this book of essays seems too abstract or disjointed, hopefully to be remedied when the second volume on the modern state appears, it does have two

major strengths. First, the collection raises some very important questions about the class nature of state forms and practices in specific capitalist periods. Secondly, it raises the philosophical perspective of socialism from below; of workers' power based on freely associated councils rather than mere formal participation in states based upon the dictatorship of the bourgeoisie, whether east or west.

Robin Wylie
Carleton University

Social History

An International Journal
Edited by Janet Blackman and Keith Nield
Department of Economic and Social History, University of Hull

'**Social History** has without a doubt immediately established itself as enormously superior to all English-language journals in the field, with the possible exception of **Past and Present**.'
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Annual Meeting
Committee on Canadian Labour History
8 June 1983

CALL TO ORDER

Robert Babcock, chairman, called the meeting to order at 12:30 pm.

ATTENDANCE (20)

Members of the executive present:

R. Babcock	President
G. Kealey	Treasurer/Editor, *L/LT*
A. Seager	Secretary/Programme co-ordinator
Absent; owing to other duties	
D. Moore	Vice-President

ADOPTION OF MINUTES OF 1982 MEETING

Adopted as presented

AGENDA

Adopted as presented

PRESIDENT'S REPORT

Robert Babcock explained his recent correspondence with Robert Zeiger of the North American Labour History Association, regarding our formal participation in future arrangements dealing with the Association's annual conferences held in Detroit. After a discussion and reports on the Detroit conference by Bill Baker and Bryan Palmer, it was agreed that the President should act as our liaison with the North American groups and that efforts should be made to arrange a Canadian programme from this end for 1985.

Scholars interested in presenting papers at the North American Labour History conference in 1985 (or in future) are urged to contact Dr. Babcock, University of Maine at Orono.

TREASURER'S / EDITOR'S REPORT

Greg Kealey emphasized that the journal is in relatively sound financial condition at present, but faces the ever-present challenge of new technologies, government programmes, etc.

He wished to thank all subscribers for their prompt and thoughtful responses to the questionnaire circulated in the Autumn, 1982 number.

SECRETARY'S/PROGRAMME CO-ORDINATOR'S REPORT

Allen Seager gave a brief explanation of the fact that no British Columbia papers have been included on the CHA Labour History programme this year. However, a special session on "Class Struggles in B.C." had been organized; he wished to thank Elaine Bernard, Clay Perry, Legislative Director of the International Woodworkers of America, B.C. district, and Bill Clark, president of the Telecommunications Workers' Union, for their participation.

A discussion ensued on prospects for 1984 and 1985. It was agreed, upon the suggestion of Jim Thwaites, that a special effort be made to organize jointly with Québécois historians for 1985, when Montréal will host the Learneds. Upon Greg Kealey's urging, it was also agreed that efforts be made to address, in 1985, some pressing contemporary questions:
1) work relations in the public sector
2) the industrial relations system since 1945
N.B. Scholars working in these fields, in Québécois labour history, or other fields interested in presenting papers at the CHA are urged to do so. They should contact the programme co-ordinator, Allen Seager, Dept. of History, Simon Fraser University.

OTHER BUSINESS
It was M/S/C that the Canadian Committee on Labour History ask the executive to register our extreme displeasure with the practice of the CHA organizers in scheduling our annual business meeting alongside that of the Women's History Group. Many members of these respective groups are interested in attending both meetings and cannot do so under the present set-up.

TRENT BUSINESS HISTORY CONFERENCE
Tom Traves announced this conference, to be held in June, 1984, one week prior to the convening of the CHA. Labour historians were urged not to feel shy about attending or presenting papers.

HARRY CROWE ENDOWMENT CONFERENCE
Paul Craven explained that at some point in the not-too-distant future (1984-5) the planners of these annual conferences, which have dealt with a wide range of progressive issues at York University, may organize a colloquium on Canadian Labour Since World War II.
N.B. As the planners will make their decision on the basis of what resources are available, scholars working in this field are urged to contact Professor Craven, Social Science Division, York University, for further information.

COMMONWEALTH LABOUR HISTORY CONFERENCES
Given the election of a Labour government in Australia, it is possible that our colleagues in that dominion may be able to realize their longstanding desire to host such a conference. Terry Ruddell and Jim Thwaites emphasized that Québécois historians, among others, would be interested in taking part. It was agreed that should the contingency arise every effort will be made to co-ordinate the organization of a Canadian delegation. For further information, scholars are advised to contact Greg Kealey, History, Memorial University of Newfoundland.

CHA/LEARNEDS INFORMATION BULLETIN
Allen Seager was instructed to contact Professor Alan Brooks at the University of Guelph with an eye towards drawing up a newsletter, prior to the 1984 meetings, which would give those interested in labour issues an

opportunity of having the times, dates, and places of all sessions (regardless of discipline) relevant to the topic.

ELECTION OF OFFICERS

In accordance with the laws and customs of the organization, Robert Babcock ceded the chair and nominations for officers commenced:

for President	R. Babcock
Vice-President	D. Moore
Treasurer	G. Kealey
Secretary	A. Seager

All were elected by acclamation.

ADJOURNMENT

It was M/S/C that the annual meeting be adjourned, 1:40 pm.

Call For Papers and Panels
Social Science History Association — 1984 Meeting

The ninth annual meeting of the Social Science History Association will be held October 25-28, 1984 at the Ontario Institute for the Study of Education in Toronto, Canada.

Those wishing to organize a panel, present a paper, chair a panel, serve as a discussant or offer suggestions for the program are invited to contact:

Program Committee Chair:	Co-chair:
William Claggett	Marilyn Mavrinac
Department of Political Science	Department of Education/History
University of Mississippi	Colby College
University, Mississippi 38677	Waterville, Maine 04901
(601) 232-7401	(207) 873-1131, ext. 2196

The full program committee will be announced at the 1983 meeting.

Paper and panel proposals should include a short description of the paper or papers involved and the names, departments and institutional affiliations of all proposed participants. Panels may also be in the form of roundtable discussions. All proposals must be received **no later than** February 15, 1984.