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Geoffrey Bilson, *A Darkened House. Cholera in Nineteenth-Century Canada* (Toronto: University of Toronto Press 1980).

THE FIRST LINE of the last paragraph of *A Darkened House* reads: "The experience of cholera left its mark on Canada." (177) The key question is, what kind of mark? Unfortunately, Geoffrey Bilson does not adequately answer this perfectly legitimate historical question. Upon examination of the book, its failure seems to be due to a fascination with the picture of cholera as a terror visited upon the Canadian people — a terror which, Bilson constantly asserts, "had an impact on [Canadian] politics, medicine and society during the middle part of the nineteenth century." (4) This fascination pervades the work and interferes with Bilson's ability to make proper use of his extensive research.

No one would argue that epidemic disease was not a major factor in disrupting individual and community life in the nineteenth century. In fact, as Bilson himself documents, over a twenty-year period, Canadians had to deal with cholera almost as a fact of life. And that is the point. People learned to readjust their lives to take this disease and others into account. Bilson himself provides examples of life going on: Susanna Moodie taking a sight-seeing tour among the possibly cholera-stricken immigrants quarantined at Grosse Ile (10) and a wry remark from the author of a previous book on the cholera (C.M. Godfrey, *The Cholera Epidemics in Upper Canada, 1832-1866*) that descriptions of York as a town of "deserted streets, traversed continually by cholera carts conveying the dead to the grave and the dying to the hospital" does not accord with the fact that these very streets were being invaded by 40,000 immigrants that very summer. (63) Rather than work out the puzzle of myth versus reality, Bilson retreats into an insistence that both have their roles — an unassailable assertion but one without much historical worth. Simple

statement of such axioms without critical assessment does not advance very far the cause of historical knowledge. For example, nearly at the end of his text, after having systematically fallen short of convincingly portraying to the contemporary reader the horror he is sure cholera held for former generations, he retreats into a sort of historical mysticism:

In all epidemics, the psychological impact of the attack was expressed through the clichés in which it was described. A town or city suffering the disease was usually described as a place of empty streets, where nothing was to be seen but the cholera carts carrying the sick to the hospital and the dead to burial. Little could be heard but the sound of footsteps echoing from the walls as the doctor or priest hurried to the bedside of the sick and dying. Sometimes, and for some places, these descriptions were accurate enough — but not for all times and places. The fact that people chose to remember an epidemic in these ways is a measure of the effect of the disease. (168)

It might also be seen as a measure of the historical worth of such documentation. Besides, how can Bilson argue this in a book which has as a cover illustration a detail from Joseph Légaré's painting *Cholera Plague, Quebec* which shows a square filled with people — some sick, some dying, some seemingly still healthy — whose response to the epidemic seems to have been not to desert the streets but rather to crowd them in search of a little human comfort? Is this not evidence of an alternative myth? Again, Bilson's basic question is not at fault, but his failure to address it properly certainly is.

In fact, it seems obvious throughout that Bilson started his research armed with answers rather than questions and chose to stuff the documentation in wherever he could rather than reorganize his thinking. The result is a book that is not built around workable themes. The earlier chapter headings sound like those one would expect in an old political history on something like responsible government — what happened in what political jurisdiction of the country at what date. The result is repe-

tion and incoherence. The later chapter headings consist only of cryptic excerpts from quotes found somewhere in that chapter, such as "Shortcomings... exposed relentlessly." The result is not only uninformative but actually misleading. For example, a chapter entitled "Charlatanism of every description" deals only briefly with quacks or "irregulars" but rather discusses the foibles of the Canadian medical profession, a group which might be included under the designation of charlatan because of its pretensions to knowledge it did not possess, but which is clearly not meant to be by Bilson. A word must also be said about the tables Bilson has painstakingly assembled for the book. Four tables of cholera deaths for various communities for various years, evidence though they are of careful research, are useless in themselves without some interpretation — at the very least, an indication within the table of the population of the cities being tabulated.

It is unfortunate that Bilson chose to try to answer questions unanswerable in the context of his data. His attempt to show how the epidemics of 1832 and 1834 related to the radicalization of French-Canadian politics which erupted later that decade (50-1, 75, 177) is singularly unsuccessful. Instead, he would have done better to concentrate on and organize his work around the interesting and historically respectable themes of the struggle of the germ theory of disease for acceptance, the ravages of cholera in a country with relatively unorganized support systems compared with the older nations of Europe, the same comparison between Quebec with its religious institutions and Ontario with its less developed charitable network. Indeed, a coherent section on the character of the disease and an evaluation of remedies tried and their success would have been of interest and help. Bilson alludes to all these topics and from time to time, discusses some of them. Of note are his frequent references to the effect of cholera on public health reform but his

arguments in this and other areas are diffused due to anomalies of organization. Perhaps organization is the key point here. The research and its results should have been put together along different lines. An epidemic is not a political event like a war or the making of a country. That is not to say that it is less important; only that it has to be got at differently.

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A.E.D. MacKenzie, *Baldoon: Lord Selkirk's Settlement in Upper Canada* (London, Ontario: Phelps Publishing Company 1978).

OF THE THREE PLANNED settlements attempted in British North America by the Earl of Selkirk in the early years of the nineteenth century, Baldoon in Upper Canada is clearly the least well known. Red River has won Selkirk a place in national historiography, and the reputation of the Prince Edward Island venture, while perhaps confined to that province, is considerable among Islanders, most of whom would like to be able to trace their descent back to the passengers on the *Polly*, *Dykes*, and *Oughton*. But Baldoon has been lost in the early history of Upper Canada, which is in many ways a pity, not least because like Red River it is a fascinating tragedy, although not on such an epic scale. It is almost impossible to avoid seeing Baldoon as a dress rehearsal for Red River. The problem was that although Baldoon was an expensive flop, its producer/director failed to get the message, or to learn from it.

In this little book Doug MacKenzie attempts to tell the story of Baldoon more completely than it has ever been told before in print. The author's instincts are sensible. He recognizes Selkirk as a product of the Scottish Enlightenment, a point curiously neglected by most previous commentators on the Canadian career of the Fifth Earl. He places Selkirk's "philan-

thrope" squarely in the context of Scotland's cultural and social ferment at the end of the eighteenth century, and he makes intelligent use of the Selkirk transcripts at the Public Archives of Canada, which are more extensive on Upper Canadian topics than most scholars have appreciated. Yet the resultant study is strangely unsatisfying for most readers, with the possible exception of the descendants of Selkirk settlers in Wallaceburg to whom the last chapter is devoted.

The difficulty is not that MacKenzie is wrong-headed or unscholarly, but that he is too brief and sketchy. The tale of Baldoon, if it is to be told at all, needs a bit of room to breathe. The historical personalities associated with the settlement must come alive, for above all else Baldoon involved people, some behaving heroically and others stupidly, but all acting in ways which should have provided us a fascinating glimpse into the hazardous pioneer world of early Upper Canada. The cast of characters was a large one. It included Selkirk himself, that curious mixture of Enlightenment reform and Tory paternalism, fond of Highlanders and completely hostile to Yankees. Then there was William Burn, the young Scots farmer sent by the Earl to begin the settlement, who succumbed to the wilderness and weakened his constitution with grain whiskey to the point where he was one of Baldoon's first malaria victims. Add Alexander McDonnell, a man torn between his obligations to Selkirk and his fears for his life in the disease-ridden swamps of the Chenail Écarté. All these and others can be mixed with the settlers themselves, many of whom died without a whimper, victims of a place which was simultaneously unhealthy and the best agricultural land in the province.

Given such ingredients, it is difficult to tell the tale badly, and in fairness to the author, many glimpses of the possibilities do find their way into the discussion. But MacKenzie has little sense of the tragic, and makes infrequent efforts to bring his characters to life. The result is an opera lib-

retto rather than a full-scale drama, everything in its proper place but nothing fully developed. Perhaps the author will try again, since he obviously is on the right track.

As a physical object, the book is one of the least appealing products of the recent spate of locally-printed historical works found everywhere in Canada these days. Some of the original black and white illustrations are charming, while others are disasters. There is a bibliography but no index, the latter a peculiar failing in a book obviously intended in part for those interested in their forebears.

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Harvey J. Graff, *The Literacy Myth: Literacy and Social Structure in the Nineteenth-Century City*, (New York: Academic Press 1979).

"A LITERACY MYTH," writes Harvey Graff, "surrounds us." This "social lie" is never defined precisely but it seems to amount to the view that literacy is a Good Thing. The author cites a variety of writers from Egerton Ryerson and the other nineteenth-century "school promoters" to contemporary students of economic development and of the American occupational structure, all of them asserting or assuming that training in reading and writing was beneficial for individuals and societies. If this book were written a year or two later the Sandinista crusade to spread literacy through the Nicaraguan countryside might also have been mentioned as the ultimate embodiment of the literacy myth.

Graff's aim is to explode this widely-held delusion and, for some unexplained reason, he chose data from the cities of Hamilton, London, and Kingston in 1861 and 1871 as his main scientific ammunition. Information on illiterates and their families from census manuscripts and assessment rolls show that, although uneducated people tended to be poor and to

work at menial jobs, illiteracy was not a crucial factor determining their place in the social and economic hierarchy of urban Ontario. On the contrary, degrees of "success" and "failure" within the illiterate group, as in the community as a whole, were related to "ascriptive" characteristics, particularly ethnicity. The empirical core of this study provides a wealth of detail on the illiterates, the size of their families, their tendency to buy houses, the occupations of their children, etc. The convergent thrust of all these findings leads to the conclusion that illiterates did not always fare so badly and that, if they as a group were not as well off as their more educated neighbours, it was because they tended to come from disadvantaged backgrounds. Supplementary analyses of jail registers and the personnel records of a lumber company from roughly the same period suggest that the disproportionate illiteracy of criminals masks more decisive factors determining patterns of arrest and conviction and that illiterate workers could earn wages as high as those of their fellows.

Graff's methods and his use of sources, while generally quite meticulous, are open to a few objections. There is, for example, the problem of a lack of parallelism in comparisons, the combined illiterate populations of Hamilton, London, and Kingston being set off against the literate population of Hamilton alone. Another difficulty concerns the division between literates and illiterates — a distinction central to the analysis — on the basis of personal testimony recorded in the census. Readers may well wonder just how precise this border is, especially when they reach the final chapter, "Literacy: Quantity and Quality," which shows that people of this period could have in mind widely differing standards when they replied yes or no to the ambiguous question, "Are you unable to read or write?" These technical quibbles aside, the empirical element of the book seems to be quite sound.

There are problems, however, when

the discussion goes beyond statistics to the men and women they represent. In inferring motives from census figures, the author tends to fall back on some questionable presuppositions. For example, the poor and illiterate are consistently portrayed as locked in a struggle for personal "success" and this is always equated with greater wealth, "better jobs," and the ownership of houses. They show their greatest strengths in "adapting" and "adjusting" to an inhospitable environment. When the figures suggest that illiterates' families were frequently torn apart by their general poverty and their inability to support children at home, Graff is favourably impressed by the excellent "adaptive strategies" of parents who sent their children out to work. (He might also have considered the possibility of premature death as a factor affecting family "dependency ratios.") He seems to delight in the prospect of all these youngsters being emancipated from parental authority and taking advantage of the "work opportunities" offered them.

Nevertheless, Graff's major conclusion, that a person's fate was not determined by his literacy or lack of it, seems well established, if not startling to common-sense expectations. A more interesting argument, pursued intermittently through the text, suggests that, far from being a means of liberation or of individual advancement, literacy functioned as an instrument of social control. The difficult question of whether a knowledge of reading and writing makes workers more docile and regular in their habits is one that has been tackled by others in the past with varying degrees of success, but this work makes no real contribution to the discussion. Instead the author shifts the question from the effects of literacy itself to the effects of its acquisition in state-run schools. This leaves him exploring familiar territory and echoing the arguments of Alison Prentice and other proponents of the social-control theory of modern education.

Middle-class school promoters, it is argued, were responding to "massive social change" (235) when they advocated universal education as a means of achieving social stability. And yet, remarkably little attention is paid in this book to this "social change," particularly as it manifested itself in urban Ontario in the 1860s, though one might think this would be the main object of the social historian's researches. "Social change" crops up frequently in *The Literacy Myth* as an all-purpose explanatory factor but it remains vague, abstract, and apparently agentless and inevitable. In the absence of an appreciation of historical processes operating in a specific time and place, we are left with an implied traditional-modern polarity which reduces distinctions between different societies and historical periods to a single measure. This certainly seems to be the only framework that would permit anyone to attack, as Graff does, modern claims that educational attainment is the key to career success with 100 year old statistics from a society in which this dictum had not yet become a self-fulfilling prophecy.

In case anyone has doubts about the significance of *The Literacy Myth*, the author points out at least once in every chapter that his findings challenge "received wisdoms" of many sorts. Often though, one wonders whether earlier interpretations have been overturned or simply misunderstood. Certainly when he discusses educational theorists of the nineteenth century (and such discussions occupy a large portion of the text), Graff spends most of his energy battling strawmen. The central conclusion in his book, that education "did not significantly alter class stratification or structural inequality," for instance, is portrayed as though it gave the lie to early advocates of mass literacy. And yet there is no evidence that Egerton Ryerson and his fellows (in Canada, at least) wished to upset the social order or even to encourage "social mobility" except within narrow limits. Poor Ryerson

is Harvey Graff's favourite foil. Although he discusses the often ambivalent views of the Superintendent of Education at great length, he repeatedly reduces them to deterministic caricatures when he confronts them with the census statistics. In chapter 6, "Literacy and Criminality," one of Ryerson's wildest rhetorical flourishes is taken as the initial hypothesis and the empirical finding that "criminality... related to much more than illiteracy" (236) is presented as if it were not entirely consistent with most of Ryerson's published statements on the subject. Ryerson was no profound thinker, but surely he was not such a fool as Harvey Graff takes him for.

All works of history are in some sense "about" the present, but *The Literacy Myth* puts forward an explicit, though imprecise, argument about a contemporary issue on the basis of evidence from the past. The thesis, that literacy does not really matter, now or a century ago, therefore deserves attention. What is most striking about the argument is its narrow materialism. A poor person had no need for more or better instruction, it appears, because this would not get him a house, a better job or higher wages. When the working-class press urges the value of education and "serious" reading, Graff takes this as evidence of "acceptance of hegemony." (215) It never seems to occur to him that anyone might value learning for any but the most crass motives. When the author concludes that manual workers had no need for literacy since it would not help them to do their jobs (302), he is simply repeating the arguments of early nineteenth-century conservatives who saw no good purpose in giving training to the lower classes that would not aid them in the discharge of their duties. Fortunately, one does not have to agree with this thesis to benefit from the thorough quantitative portrait presented in *The Literacy Myth* of an important aspect of urban society in nineteenth-century Ontario.

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Paul-André Linteau, René Durocher et Jean-Claude Robert, *Histoire du Québec contemporain. De la Confédération à la crise: 1867-1929* (Montréal: Boréal Express 1979).

THIS BOOK IS A truly remarkable achievement. Inconceivable only a decade ago, it attests to the volume, variety, and above all the high quality of recent research and publication on post-Confederation Quebec. Moreover, the authors demonstrate a nearly complete mastery of this material and a special genius for rendering it comprehensible to students. Designed presumably as a senior undergraduate text, *Histoire du Québec contemporain* will also serve to report "the state of the art" to scholars well above that level.

The authors have divided their work into three major sections: one on geography, demography, and ethnic composition covering the entire period from 1867 to 1929, one on the three decades of modest economic development following Confederation, and one on the decades of accelerated economic and social transformations which followed. Within each of their sub-periods, they analyze separately and systematically the economy, society, politics, and finally intellectual and artistic endeavour. Texts frequently acknowledge the wisdom of such a broad scope in theory; seldom do they actually present such a balanced picture. This one however, does. Instead of ignoring an area (the arts) where they did not consider themselves fully qualified, they enlisted outside expertise.

The authors seem equally determined to be fair and objective. They acknowledge and explain conflicting interpretations and historiographical traditions, and scrupulously avoid emotive language and provocative references. In fact one of their major concerns is to reject racial stereotypes. They promise — and deliver — a history of all Quebecers, recording the experiences and contributions of cultural minorities with evident sympathy and

dealing frankly with the xenophobia of some members of the francophone majority. They treat the Conscription Crisis and the exclusion of French Canadians from real economic power so serenely that a stranger might not fully appreciate the intensity of earlier antagonisms. There is a perceptible bias in their portrayal of the federal government as unresponsive to Quebec's interests and outlook, especially in comparison with Ontario. Thus, the National Policy is blamed for the "marginalisation" of outlying Quebec regions because it so heavily promoted development in the Montreal area — a criticism whose validity rests on several questionable premises. Conversely, the influence of Quebec over Canadian foreign policy in the 1920s escapes mention. But this theme does not preoccupy the authors; the bias does not even seem conscious, and there is no reason to impute motives.

Objectivity does not, of course, preclude a point of view, and the authors never attempt to disguise their assumption that industrial capitalism was the prime mover, the independent variable in Quebec life throughout this period. This approach inevitably creates some cardboard personages and arbitrary categorizations. But even the most rabid anti-Marxists will have to concede that it contributes more to the volume than it takes away. It largely accounts for the refusal to blame French Canadians' "economic inferiority" on racial discrimination or to confuse class with ethnic divisions. The emphasis on class structure instead helps the authors destroy the myth of a homogeneous French Canadian society and provides them with the basis for a coherent if controversial critique of nationalist ideology. Finally, the authors' viewpoint encourages the structural rather than narrative organization of their book. This has in turn permitted them to introduce concepts and to describe certain institutions more smoothly and adequately than the writers of traditional texts are usually able to do. Similarly, they were able to devote entire

chapters to subjects like labour and women's history which have been so well served by recent specialized research. How often does new work find its way so quickly into a textbook synthesis?

The structural approach does nevertheless have drawbacks — largely problems of application — which should be acknowledged. One is repetition, no small matter in a book this size. Excellent photographs, charts, and maps along with generous spacing partly explain why 60-odd years of a province's history consume 640 pages. But on virtually every major subject, ranging from agriculture to public health to the industrial policies of provincial governments, precisely the same information appears in several chapters. Sometimes exact phrases recur, as on pp. 313 and 609 concerning resistance to state initiatives in social policy. (On a few occasions, repeated references to the same subject yield minor inconsistencies! For example, Lomer Gouin's legislative record is alternately judged too timid and reasonably impressive.) More seriously, some important historical problems fall between categories and never receive a full, satisfactory discussion. Among these I would include the tribulations of the French Canadian bourgeoisie and what some consider the unique role and importance of government patronage in Quebec society. There are cases where an obvious connection is not made between two topics. For example, the increasing clerical domination (ideological and institutional) of French Canadian society is ignored as a factor in English Canadian hostility to the extension of that society beyond Quebec.

While the author's emphasis on clericalism is certainly appropriate, most substantive criticisms of this book will concern their treatment of the Roman Catholic Church. This is the only area where objectivity, the awareness of existing literature, and the identification of historical problems seem less than complete. In the attribution of motives, clergy are sel-

dom granted any sincere concern, however misguided, for the wellbeing of the faithful. Thus agriculturalism derives only from the desire to preserve a monopoly of "spiritual and moral control" and to protect "important [economic] investments in the countryside which could appear threatened by the rural exodus." The harsh reality of industrial life, amply acknowledged in other contexts, goes unmentioned as do William Ryan's valuable distinctions about the economic role and ideology of the clergy. Similar assumptions underlie the discussion of Catholic unionism, where courageous activists like Abbé Fortin are not allowed on stage to shatter the reactionary monolith. The response to postwar pressure for the reform of education and social assistance is characterized as a blanket ideological rejection; there is no indication of diversity in clerical opinion, of complex institutional problems, and especially of positive co-operation with (or at least acceptance of) certain state initiatives.

Readers might also appreciate greater clarification of two wider issues relating to religious influence: the "cultural" explanation for French Canadians' inferior economic status and the relationship between conservative and liberal ideologies. The authors' economic determinism implies a negative position on the first question, and they do emphasize demographic and structural factors in explaining the generally lower per capita income of Quebec as compared to Ontario. But they also criticize Albert Faucher for dismissing culture too rapidly, attach considerable importance to the failings of the Church-dominated educational system, and do not try to account for the "limited horizons" of francophone entrepreneurs. The last is particularly surprising in light of their own previous writing. On the second question, two quite different approaches are evident. For the 1867-96 period, there is a somewhat forced separation between socio-economic, socio-political, and national ideologies; thus economic liberalism and a

clerically promoted conservatism can both be declared predominant. The discussion for 1896-1929 is more convincing; it describes the triumph of liberal materialism over clerical-nationalist resistance. I hope the authors will employ this more integral approach in their projected second volume (1929-present).

No textbook could satisfy every scholar on every point, and these criticisms serve primarily to fulfill the reviewer's obligation to think of some. The authors deserve nothing but congratulations and gratitude (perhaps royalties as well) for this most welcome publication.

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Jacques Rouillard, *Les syndicats nationaux au Québec de 1900 à 1930* (Québec: Les Presses de l'Université Laval 1979).

LES SYNDICATS NATIONAUX ont vécu et survécu au Québec avec beaucoup plus de vigueur qu'ailleurs. *Les Chevaliers du Travail*, le Congrès national des métiers et du travail du Canada après la scission de 1902, la Fédération canadienne du travail à partir de 1908, la Confédération des travailleurs catholiques du Canada, qui démarra vraiment pendant les années vingt, et certains syndicats indépendants, ont obtenu l'adhésion de cette fraction du mouvement ouvrier qui résistait à l'attrait des syndicats internationaux.

Jacques Rouillard s'est proposé d'analyser les raisons de ce choix et d'en narrer les principaux développements. De profanes à religieux pendant la période à l'étude, ces syndicats demeurent certes minoritaires, surtout dans le grand centre industriel qu'est Montréal, mais ils s'imposent à Québec et dans les petites villes. C'est la thèse de l'auteur que les syndicats catholiques ont récupéré ce qui restait des syndicats nationaux et des syndicats indépendants d'avant-guerre qui leur avaient, jusqu'à un certain point, préparé le terrain. (120) De château-fort des

syndicats nationaux, la ville de Québec devient la citadelle du syndicalisme catholique.

La question des syndicats nationaux remet sur le tapis le débat autour de la question nationale et économique. Quand les syndicats internationaux offraient des avantages matériels intéressants, un fonds de grève bien garni, des organisateurs payés, alors que les syndicats nationaux n'avaient pas ou peu de fonds de grève, et moins de services, le sentiment national devait prendre des proportions héroïques pour que les travailleurs optent pour ces derniers. "La faiblesse du sentiment national" est un élément d'explication de l'échec de la Fédération canadienne du travail. Or, si les syndicats nationaux se développent après la grande guerre on pourrait conclure à un regain du sentiment national.

Il faut surtout constater l'appui financier exercé par l'Église catholique au Québec. Contrairement à leurs homologues américains, qui s'appuyaient sur la Fédération américaine du travail pour faire échec au socialisme, les évêques québécois octroient des terrains et des bâtiments, défraient la pension de l'aumônier du syndicat, organisent des quêtes paroissiales et offrent à la CTCC quelques \$20,000 par année. (237) Vu "le faible sentiment national," le lecteur peut se demander ce qu'eussent été les syndicats nationaux sans l'aide matérielle de l'Église. L'obéissance aux chefs religieux, l'adhésion à la doctrine sociale de l'Église n'eurent probablement pas suffi à assurer la croissance de la CTCC. Elles n'influençaient sûrement pas les deux tiers des syndiqués qui appartenaient aux syndicats internationaux. Et pourtant ce tiers de syndiqués catholiques, selon Rouillard, appartiennent à des syndicats qui "se sont développés au Québec parce qu'ils ont trouvé appui sur un vieux fonds nationaliste qui existait bien avant que le clergé ne se penchât sur le sort des travailleurs." (120) Il est souvent difficile, dans cet ouvrage, de discerner la part jouée par "le vieux fonds nationales," "la faiblesse du sentiment nationaliste," et les

largesses de l'Église dans l'éclosion du syndicalisme catholique.

Ces trois facteurs jouent beaucoup moins à Montréal: est-ce dû à l'origine ethnique plus diversifiée des travailleurs ou à leur situation dans la structure économique? Le problème reste posé.

L'importance numérique de la CTCC est depuis longtemps sujette à caution et varie selon les auteurs consultés. Rouillard fait bien de distinguer entre les effectifs déclarés par la CTCC et ceux rapportés par les unités syndicales, ces derniers étant toujours de beaucoup inférieurs aux premiers. Depuis des années, chefs syndicaux, politiciens et historiens ont utilisé l'un ou l'autre rapport publié annuellement par le ministère du Travail. Seulement une partie de l'écart entre ces données est expliquée par le fait que des unités syndicales négligent de faire part de leurs effectifs au ministère. Rouillard fait la moyenne des adhérents des unités qui déclarent leurs nombre et multiplie celle-ci par le total d'unités pour ainsi obtenir ce qu'il qualifie de "résultats proches de la réalité." (233) Il n'est pas certain qu'ils en soient si proches puisqu'on peut supposer que les syndicats qui ne fournissent pas de statistiques sont peut-être moribonds et on ne peut présumer que la moyenne des membres pour ces unités soit la même que pour celles qui informent le ministère du Travail.

Rouillard nous présente le fruit d'une recherche minutieuse dans les archives syndicales et c'est peut-être cette familiarité avec les sources qui, d'une part lui inspire une grande prudence et, d'autre part, conduit parfois à de subtiles contradictions. Ainsi, au début du siècle, certains employeurs, dont le sénateur Forget, préféraient le CNMT aux internationaux du Congrès des métiers et du travail du Canada: sans fonds de grève, sans organisateurs permanents pays, en 1903 les syndicats nationaux lancent vingt fois moins de grèves que leurs rivaux et, moins radicaux, ils s'opposent au Parti ouvrier. Pourtant Rouillard nous assure qu'il serait

exagéré d'accuser les syndicats nationaux de complaisance à l'égard des employeurs. (95) Sans préférer une telle accusation, il est permis de constater que les employeurs, qui préféreraient les sociétés de bienfaisance aux syndicats, optaient pour les nationaux, si syndicats il devait y avoir, parce qu'ils représentaient une moindre menace que les autres.

Malgré sa circonspection, Rouillard laisse parfois deviner ses préjugés: quand la CTCC, qui group 27 pour cent des travailleurs syndiqués, ne conduit que 13 pour cent des grèves de 1920 à 1930, "la différence [avec les internationaux] n'est pas tellement appréciable." (243) S'agit-il ici d'une réhabilitation de la CTCC qui, selon l'auteur, s'apparente de plus en plus aux syndicats affiliés à la FAT? Que de chemin parcouru depuis l'oeuvre de Harold Logan sur les syndicats au Canada qui décrivait la CTCC comme "catholique, raciale, conservatrice et charitable." Rouillard nous présente une centrale qui se rapproche progressivement de la FAT et on peut l'accuser d'anticiper un peu puisque c'est plutôt vers la fin des années trente qu'on peut risquer la comparaison.

L'auteur constate aussi qu'avant la première guerre "la majorité des travailleurs était alors convaincue de la supériorité des organisations internationales dans leur lutte contre le capital." (132) La majorité des travailleurs, cependant, ne luttait pas contre le capital et les membres de la FAT en particulier comptait bien en profiter.

L'ouvrage de Rouillard est plus qu'une simple chronique des étapes qui marquent l'évolution des syndicats nationaux au Québec. Contribution importante à l'histoire du mouvement ouvrier au Québec, il demeurera un outil de base pour tous ceux qui voudront suivre cette évolution et formera la thèse à attaquer, confirmer, aiguïser, et raffiner par les recherches plus en détail qui suivront.

La bibliographie des sources, des études et des thèses fournira une aide précieuse aux étudiants non seulement du mouvement ouvrier mais de toute cette

période de l'histoire du Québec.

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John Stanton, *Life and Death of a Union: The Canadian Seamen's Union* (Toronto: Steel Rail 1978).

AS JOHN STANTON, the author of the *Life and Death of a Union: The Canadian Seamen's Union*, states, the history of the CSU is almost a textbook of union issues and problems. The rise and fall of the CSU illustrates the century-long struggle in Canada between labour and management, the struggle to shape a national as opposed to an international trade union movement, the debate over the role of government in labour-management relations, and the workings of a communist-led union. Such an important subject deserves far better treatment than is offered here.

The author does little to relieve the paucity of published works concerning the CSU. His book is an anecdotal sketch of the 13-year history of the first successful union of Canadian sailors on the Great Lakes and the coasts. It is most disappointing because Stanton explains in his preface that, while his work on the CSU was originally intended as a chapter in a book on trade unionism, the history of the CSU can not be told briefly. However, he accomplishes just that. Not only briefly, but sketchily and, unfortunately, based on too few and sometimes questionable sources. Considering the bountiful primary sources available concerning the CSU, this lack of research is unforgivable.

One can not help but feel that this book is only an elaborate, mildly embellished outline which offers the general facts but lacks any further depth. Three examples follow. Accurate background information relating to the rapid early success of the CSU is provided but not dealt with fully. The author omits details crucial to a thorough understanding of the status of the CSU. Although he does describe the CSU

affiliation with the Trades and Labour Congress, he does not explain that the payment of the per capita tax directly to the TLC made the CSU a national affiliate. Such a statement is essential to understand completely the ensuing struggle between the national and international labour bodies. In a second example, the writer states that the CSU won the 1946 strike, which tied up the movement of essential products for 28 days, because the members were not misled by employer propaganda which portrayed the leaders of the union as "communists and thus dangerous scoundrels." (91) That is essentially correct, but there were other factors involved as well. He fails to mention the favourable post-war conditions created by PCI003, which remained in effect until the end of 1947. The CSU was able to milk the legislation which had placed shipping under federal jurisdiction during the war.

The chapters devoted to the Canadian Maritime Commission entitled "Mandarins and Ships," though one of the redeeming qualities of the book, is a third example of the sketchy treatment of an important subject matter. While many secondary sources decry the Liberal decision to scuttle the merchant marine, few offer insight into the formulation of the decision. In 1944 and 1945, the Canadian Maritime Commission headed by J.V. Clyne, made several reports on merchant shipping. The final report of the commission's subcommittee outlined ways in which the fleet could be disposed of after the war. This decision was based on the accurate belief that Canada would not be able to compete financially with Britain and the United States. The decision to scuttle the fleet was based on Liberal unwillingness to subsidize heavily the Canadian shipping and ship-building industry. Though Stanton should be credited for his use of these sources, his use of them is restricted to a "good-guy, bad-guy" theory of history which prevents looking beyond some sort of insidious conspiracy. For example, he states:

These reports can be said to cast light upon that special skill, unique to the ruling élite of Canada, to damage and even cripple an important section of the country's economy in order to protect private interests. (53)

His interpretation may be entirely sound, but Stanton should at least attempt to present all the facts. He ignores that the commission successfully predicted the demise of the Canadian shipping industry in 1948 following the transitional period after the war. It was not entirely a case of predestination as the reader is led to believe. By neglecting the full impact of the Marshall Plan on Canadian shipping, the writer again illustrates his lack of research. The Marshall Plan had unfortunate effects on Canadian shipping, since it restricted 50 per cent of the available American cargoes for native vessels. Domestic preference legislation was enacted by those receiving Marshall Plan aid to ensure that as much as possible of the balance of the cargoes were carried by the importing countries. As a result, ships under the Canadian flag were all but eliminated from the competition. Though the commission did initiate the unfortunate decision to scuttle the merchant marine, their reasoning can not be totally condemned.

Trade-union histories written by participants, though often marred by their one-sidedness, have their place among Canadian labour histories. Their value as important reminiscences is indisputable. John Stanton can not claim the status of a true participant, having had only a tenuous relationship with the CSU as a west-coast lawyer trying cases involving individual seamen. He was, at best, a very interested observer. Neither can he claim to be a historian, having failed to research his subject matter fully. He largely ignores the available secondary sources and primary documents to rely heavily on the *Searchlight*, the CSU organ. He is also convinced of the *Searchlight's* infallibility. As an example, the newspaper made several serious allegations concerning employer intransigence

during the strike of 1946. This opinion follows:

It seems to me that such serious and specific accusations made by the small union newspaper *Searchlight* would have resulted in charges of criminal libel... unless... what the paper reported was literally true. (89)

Such blind faith in the organ of the CSU is not only incredible, it is naïve.

Stanton fails to use even the small amount of material he has selected to its full potential. His chapter on the "Yellow Press" might have been more effectively used within the body of the text. The press coverage did change drastically from placing the CSU in a favourable, heroic light, into anti-communist hysteria. Instead of as an afterthought, the references to the press would have offered a better explanation for the ebbing public admiration to which he often refers after 1947.

In conclusion, though there are highlights to John Stanton's book, such as his description of actual cases of discrimination and his adroit handling, without overstating, the issue of communism, the overall effect is marred by brevity and lack of research. Finally, to a reader unfamiliar with the subject matter, the *Life and Death of a Union: The Canadian Seamen's Union* suffices as a basic, though opinionated, synopsis of the general facts. One would hope that the second book in progress to which the author alludes in his footnotes, is more than a primer on CSU history.

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Terry Copp, *The IUE in Canada: A History* (Elora, Ont.: Cumnock Press 1980).

A HIGH POINT IN THE CIO's anti-communist crusade was the expulsion in 1949 of one of its largest affiliates — the United Electrical, Radio and Machine Workers (UE) — and the chartering of a new electrical union — the International Union of Electrical, Radio and Machine Workers

(IUE). The IUE proudly proclaimed itself "A CIO Union in the Electrical Industry Free From the Domination of the Communist Party." The Canadian Congress of Labour followed the CIO's lead a month later when it suspended the UE for non-payment of dues and chartered the IUE.

The IUE proceeded to apply for certification in virtually every plant organized by UE. While it cut UE's membership to a fraction of its former total in the United States, the IUE had relatively little success in unseating the UE in Canada. It won only one major local — Phillips Electric in Brockville — and had to content itself with organizing new plants and picking up the remains after the Duplessis government literally kicked UE out of Quebec.

Terry Copp's book is a history of the IUE in Canada, written at the invitation of the IUE "for the Union's own active labour education programme." It is an interesting but disappointing account that suffers from many of the weaknesses of "official" histories. It is probably unfair to call this an official history since the IUE gave Copp free reign, and it reserved no right to review the final manuscript. On the other hand, Copp relied entirely on IUE sources — the IUE Archives, interviews with IUE leaders and members, and materials in the possession of the persons interviewed. The failure to consult the numerous other relevant written sources (e.g., the United Electrical Workers Papers, the Oliver Hodges Papers, the Canadian Congress of Labour Papers) and the failure to interview non-IUE participants result in an incomplete and occasionally inaccurate portrayal of events.

The first half of the book relates how the IUE arose as part of the anti-communist fight within the labour movement. Brief and often superficial, this section has a recurring anti-UE bias that detracts from the scholarly character of the account. For example, "... the Ontario Labour Relations Board, despite the anti-communist sympathies of its members was quite unwilling to establish new ground rules for

locals who sought to secede from the UE. The Board's insistence on applying these rules *inflexibly* [my emphasis] may be admired as evidence of impartiality and commitment to the law, but it should also be noted that the effect of this policy was to deny many workers the right to vote for a union of their own choosing." (24) Since Ontario law at that time allowed applications for certification by a new union ten months after a contract was signed, workers were hardly denied the right to vote for a union of their own choosing, provided the union could sign and collect dues from 45 per cent of the members of the bargaining unit.

Or, "UE [quarterly district] council meetings were packed with [Communist] Party members and supporters, so there was little doubt that the leadership's views would be supported." (15) Copp cites no source for this charge and also fails to indicate that all council delegates had to be elected at local general meetings. While the views of the leadership were generally supported and while there were a number of Communist Party members in UE, Copp's statement denies the formally democratic structure of UE which characterized it throughout this period.

The first half of the book also contains a number of factual errors — largely resulting from Copp's restricted use of sources. For example, in 1947 the CCF Trade Union Committee hired Oliver Hodges to be its full-time organizer. His job was to encourage CCF members to become active in union locals alleged to be dominated by communists. Much of his time was spent on UE. In 1948 he helped establish a UE-CCF Unity Committee which was a grouping of leading anti-communists in the UE. Jack Morton, president of the UE Local 524 at CGE Peterborough and later first Canadian Director of IUE, chaired the Committee. Copp's account of this key group claims that Hodges had a secret agenda of which Morton was unaware, that the Committee's condemnation at a UE council meeting resulted in the Committee's secre-

tary, Joe Bacon, leading "his local into the waiting arms of the Steelworkers," (18) and that the Committee disbanded after being ordered to do so by UE's Council.

In fact, the direction of the group was debated at great length over a series of meetings, and there is no evidence that Hodges had plans of which Morton was unaware. Bacon's taking of his local into Steel was precipitated by a different and later series of events. And the Steelworkers were not the passive recipients of this dissident local; they had actively courted Bacon. Finally the members of the UEW-CCF Unity Committee continued to meet long after they had been ordered to dissolve. The members of this group were the nucleus a year later when the IUE in Canada was launched. Errors such as these limit the usefulness of the book for both the background to the IUE and this crucial period in Canadian labour history.

The second half of the book, on the IUE after battles with UE ceased, is better. Because he wants to present a short, readable history, Copp elects to discuss a selection of key events in the IUE's life rather than to present a detailed history. Unfortunately the second half of the book has an "official" character in that all of the present IUE leadership are portrayed as somewhat larger than life. But Copp does deal quite openly with some difficult and unflattering parts of IUE's history, especially the vicious, internecine warfare in the early 1960s between IUE's first president, Jim Carey, and IUE's secretary-treasurer, Al Hartnett. Copp tells the story honestly and with sensitivity and lets us understand the difficulties this fight created for leaders such as Canadian District President, George Hutchens, now the International Secretary-Treasurer.

In a chapter on the Procter-Silex strike, Terry Copp is at his best. Procter-Silex in Picton decided to prevent IUE from getting a decent first contract. Copp's rendition of the 17-month strike against the vicious, anti-union company points out the problems faced by workers and their unions in

similar, small town plants across the country.

For internal union use, this book is not bad. It avoids much of the sycophantic quality of most union-sponsored histories. And, it manages to capture many highlights of IUE's past in a brief and interesting fashion. But it is not satisfactory as a scholarly history. Copp ignored too many essential sources to have been able to write a complete and accurate history. And his comments on the UE while attempting to be fair, continue to be tinged by an anti-communism that has prevented adequate understanding of the strengths and weaknesses of UE and other left-wing unions in Canada.

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Rick Salutin, *Kent Rowley. The Organizer: A Canadian Union Life* (Toronto: James Lorimer 1980), and The Bank Book Collective, *An Account to Settle. The Story of the United Bank Workers (SOWUC)* (Vancouver: Press Gang Publishers 1979).

"WHO THEN WAS Kent Rowley?", Rick Salutin asks his readers at the beginning of his biography of the Canadian nationalist trade-union leader who died in 1978. Salutin is quick with his answer: "The greatest labour leader of his generation." The reason why most Canadians have never heard of him, Salutin maintains, is to be found in the last 40 years of labour development, a dark and dismal history of fanatical anti-communism, near-sighted and corrupt union leadership, and particularly the betrayal of Canadian workers to American interests. While the biography properly pays homage to a courageous and devoted labour organizer — it is essentially an enlargement of Salutin's funeral oration for the memory of his friend and ally¹ — its

¹ Rick Salutin, "Kent Rowley 1917-1978," *This Magazine*, 12, 2 (1978).

uncritical approach will not likely satisfy many labour historians. Nor are a number of salient developments in Rowley's career covered in a thorough or analytical way.

According to Salutin, Rowley stands in the tradition of other Canadian heroes, such as William Lyon Mackenzie, Louis Riel, and Norman Bethune. These men struggled for Canadian independence, the Quebecois nation, and better labour conditions, respectively, causes central to Rowley's career. And similarly, he is suffering from the treatment accorded Canadian heroes by historians, writers, and the media. All four men have been rejected because their impatient radicalism is considered un-Canadian, while their valid demands are supposedly implemented by "reasonable" and moderate elements. This treatment, Salutin argues, denies Canadians their heroes and falsifies their past, for the challenges issued by men like Rowley have not been satisfactorily met.

Born in Montreal in 1917, Rowley came to labour's cause with the decline of his family's fortunes from middle class to working-class circumstances. The Great Depression made him a life-long socialist. Salutin does not explore the nature of Rowley's socialist beliefs, instead assuring readers that he "never used jargon," a claim more than amply contradicted throughout the book. (Of the AFL presidents, he wrote, "was there ever a sornier list of scoundrels in history? . . . They are on the CIA payroll. They are the true and faithful servants of American imperialism.") Young Rowley cast himself into the labour movement, attempting to organize office workers and operating close to the Communist Party, though Salutin claims that he never belonged to it. For Rowley, the Communists' great mistake was the disbanding of the Workers' Unity League and the integration of its Canadian workers into American unions. This, he felt in retrospect, had tragic consequences. But Salutin's account of the WUL is misleading. It was hardly a viable and powerful Canadian alternative, still less was it

"independent, principled," and the embodiment of a "cooperative stance toward other trade union centres, including the TLC." The WUL episode, as well as Rowley's disgust with the AFL's craft exclusiveness and its jurisdictional disputes, convinced him that American unions in Canada did more harm than good.

Nevertheless, it was to an American union that Rowley turned when he resumed his organizational activities after emerging from a detention camp for opposing manpower registration in 1940. In late 1942, he became an organizer in Valleyfield, Quebec, for the United Textile Workers of America, an AFL affiliate. The fight to organize Dominion Textile Company plants in Quebec culminated in lengthy and violent strikes in 1946. The UTWA was maligned as a communist plot and bitterly opposed by the Duplessis government, entrenched corporate interests, and a section of the Catholic church. The following year Rowley was jailed for his part in the strike (Salutin is unclear on the charge for which he was convicted). After several stormy years confronting American UTWA chieftains, raids from the CIO's textile body, and hostile government and business officials, Rowley and his staff were fired in 1952 as Canadian UTWA leaders by the union's U.S. headquarters, "a corrupt and degenerate cesspool," according to Rowley. "Where Duplessis and Dominion Textile had failed, American unionism succeeded: it separated Kent from the workers he had led." He and his wife, Madeleine Parent, transformed the Canadian district council they had built within the UTWA into a new body, the Canadian Textile Council, but it was a union without a significant following. Thus he was virtually forced into "the Canadian course" of fighting for Canadian unionism. Salutin might have considered the question of Rowley's timing: why did he wait until his 1952 expulsion instead of turning to nationalist unions much earlier?

This "was no well-travelled route," at

least not until the late 1960s, with the creation of the nationalist Confederation of Canadian Unions. The CCU unionism of the 1970s, from the Texpack and Artistic Woodwork strikes in Ontario to the break-away movements of smelter workers in British Columbia, intensified Rowley's socialist and nationalist beliefs. The first and essential battle, he felt, had to be that of Canadian workers against American business unionism. The American presence "blocked the progress of Canadian workers toward the kind of consciousness and organization which could lead to socialism." U.S. union leaders were so dishonest and collaborationist that they had "corrupted" Canadian labour, while Canadian business and the state used the Americans to defeat Rowley's unionism. Only the Canadian working class could successfully counter the American imperialism from which it suffered. "But for the Canadian working class to assume such a leadership role it would first have to free itself of the main constraint on its own action: its absorption within the American labour movement."

Rowley read Canadian labour history through nationalist lenses. His nationalism "barked back" to the struggles of 1837-8, and he cited William Lyon Mackenzie as a friend of Toronto printers in the 1830s. "History will show," Rowley asserted, "that *not one single union of any importance* in Canada was organized by the Americans." (emphasis in original) Robert Babcock is thus criticized for allegedly reporting that Flett, an AFL organizer, established 140 locals in one year, not only a physically impossible task, but unlikely to have occurred. Organizers' reports to headquarters were unreliable, and he, Rowley, had organized in most of the towns visited by Flett, "and I am sorry to say that, when I arrived there, there were no unions." Rowley and Salutin are badly mistaken in their reading of Babcock. Not only was Flett a Canadian in good standing, Babcock merely states that "AFL organizers had appeared responsible for

the organization of over 60 locals of the 140 or so established in Canada in 1900. . . ." It is curious that Rowley used Babcock as a straw man rather than as counsel for the defence. Moreover, Salutin and Rowley do not acknowledge the material benefits historically conferred upon Canadian unionists by the cross-border relationship.

Salutin's enlistment of Rowley in the pantheon of Canadian national heroes removes the organizer from discerning or critical consideration. In the last analysis his book remains what it was in embryonic form: a eulogy for a courageous and devoted labour organizer.

An Account to Settle is also in some sense a eulogy. It is a narrative by union leaders of their unsuccessful campaign to organize employees of chartered banks in British Columbia between 1976 and 1978. The United Bank Workers was established in September 1976 as an affiliate of the Service, Office and Retail Workers' Union of Canada. SORWUC, a small west-coast federation with a feminist orientation, devotes itself to unionizing women workers in restaurants and the like.

The UBW had no shortage of complaints. Tellers started at \$525 to \$600 a month. There appeared to be little in the way of a rational or fair system within bank branches for determining merit increases, overtime hours and pay, seniority and promotions, and job tasks. Once the UBW began to sign some members, bank management used a variety of threats to prevent workers from joining. They were told to sign anti-union documents; UBW members and sympathizers were harassed, demoted, and fired; and some improvements were offered by the banks, such as two coffee breaks a day and new electric typewriters. A brave new world of regimentation and regulation was predicted by management should the union succeed. Finally, employees of unionized branches had their wages

² Robert H. Babcock, *Gompers in Canada: A Study in American Continentalism before the First World War* (Toronto 1974), 51.

frozen. These tactics, and the refusal of the banks to negotiate contracts in good faith with certified locals, along with the apparent disinterest of most B.C. bank employees, halted the UBW drive by mid-1978. On 31 July, the union, in a highly unusual and self-destructive move, withdrew unilaterally from further negotiations with the banks. Only 24 of 836 bank branches in B.C. had been certified for the UBW, which at its peak could muster no more than 422 members. Even these gains were completely lost when the union was decertified, following its withdrawal from negotiations.

Ironically, while the book is intended to inspire bank unionization, it unavoidably illustrates the enormous barriers faced by UBW leaders. Not only were the banks hostile and workers indifferent, but maintaining the support of other unions and manoeuvring through lengthy and delayed hearings of the federal Labour Relations Board seemed to bring constant difficulties. While the CLRB declared in 1977 to the union's delight that individual bank branches were appropriate bargaining units, it also determined that the wage freeze directed at certified branches was valid, and that a fired union activist did not have to be reinstated. According to Graham Lowe, author of the first detailed study of bank unionization, the UBW's troubles were magnified by its own behaviour. Its wage demands were unrealistic and called into question its professed disgust for "business unionism." Its hostile rivalry with the separate bank drive of the Canadian Labour Congress, its increasing isolation in the labour movement, and the disparity between the leadership and rank-and-file all contributed to the union's demise. "Ideological principles seemed to cloud perceptions of the realities of the industrial relations climate existing in the banking industry. Bank officials evidently sensed this and took full advantage of it." The UBW withdrawal "had a chilling effect on bank organizing in B.C., and did little to enhance the credibility of other fledg-

ling banks unions nationally."³

The UBW veterans, however, claim a positive balance sheet, and expect a "second assault" on the banks. This attempt is unlikely to occur, as the union presently has no certified locals. Most of Canada's 73 certified bank locals are claimed by the Retail Clerks International Union and the CLC's Union of Bank Employees, with 26 certifications apiece. Less than 1 per cent of Canada's 145,000 bank employees are union members.⁴ The unrealized potential of bank unionization should focus attention on the question of why the mostly female bank employees have shied away from unionization. One possible factor retarding union growth may be the high turnover rate and the low level of job commitment. As these factors are reduced in importance because of economic and social stringency, the demands of bank workers for greater influence in the workplace will probably increase.

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Dennis Olsen, *The State Elite* (Toronto: McClelland and Stewart 1980).

IN THE LAST FEW YEARS, the seminal work of John Porter has been brought up to date in a number of studies. Wallace Clement's books on the corporate elite took Porter's data and brought them forward in time, and even something as unscientific and unanalytical as Peter Newman's *The Canadian Establishment* owed much to the late Carleton sociologist's work. Now Dennis Olsen, a Carleton University sociologist as well (and one who is described on the dust-jacket blurb as "a former 'working man'") has updated *The Vertical Mosaic* in its treatment of the

³ Graham S. Lowe, *Bank Unionization in Canada: A Preliminary Analysis* (Centre for Industrial Relations, University of Toronto 1980), 32-42.

⁴ *Ibid.*, 1, 111. Figures given are as of 31 January 1980.

political, judicial, and bureaucratic élites. Olsen had Porter's cooperation and advice in his work and, apparently, access to some of Porter's data cards. The results are very valuable tables and charts that carry Porter's series up to 1973.

As might be expected, Olsen demonstrates that the state élite is still markedly unrepresentative of Canadian society in ethnic, class, and sex terms. There are still too few women, too few members of the non-charter groups, and too few sons and daughters of the working class in the élite. No one can be surprised at that, but there are some differences in the composition of the 1973 élite from Porter's day. Most striking is the increase in French Canadian representation. In the political élite, French Canadians increased from 21.7 per cent of the élite from 1940-60 to 24.5 per cent from 1961-73, a gain of 2.8 per cent; in the bureaucratic élite, the gain over the period from 1953-73 (unfortunately not all Olsen's tables use the same time periods or dates) was 11 per cent, all the more notable as the percentage of French Canadians in the population declined over the same two decades. Olsen's data does not let us see just when the change became most noticeable, but one can safely conclude that it was not during the Diefenbaker years and that the key acceleration in bringing Québécois to Ottawa took place in the last years of Pearson's administration and the first years of Trudeau's. When the situation requires change, then there can be change. Olsen fails to make very much of this.

This is all interesting and useful, the data splendid fodder for lectures and for cocktail party conversation. What it means, however, is less certain. Olsen (as Porter before him) tends to assume that one élite member, as defined for the purposes of his study, is much the same as another. The Deputy Minister of Finance, therefore, is for statistical purposes the same as an SX3 in Veterans Affairs or the Dominion Archivist. The Prime Minister is the same statistically as some undistinguished hack brought into the Cabinet because he happened to be the only Grit or Tory

electd from Saskatchewan or P.E.I. Obviously, they are not the same, and simply to state the problem is to demonstrate the rather fallacious nature of this kind of counting.

My own recent work has been on "the mandarins" of the years from 1935-57. I have tried, unlike Olsen, to identify the members of the bureaucratic élite who exercised *real* influence and power, and my efforts have produced a list of 20 men that includes such as Skelton, Clark, Towers, Robertson, Bryce, Rasminsky, Deutsch, Mackintosh, Gordon, Wrong, Pearson, Pickersgill, and Heeney. Arbitrary and unscientific to be sure, but my choices are based on archival research and interviews and my case studies can, I hope, show just how and why they acted as they did.

Olsen cannot do this. His two case studies are important ones — the decision to regulate and control oil prices in 1973-4 and the decision to impose wage controls in October 1975. For both, he provides a succinct account of what took place; for neither can Olsen demonstrate how his state élite functioned, operated, or interconnected. In a sense, this is not his fault for he is writing too soon after the events to have access to the records or to have secured frank and honest recollections from the participants. About all he can conclude from these two case studies which sit mutely in the middle of his book is that the decisions taken in no way served the interests of the working class. Surprise, surprise. Would it have been any different if 30 or 60 per cent of the élites had sprung from the loins of the proletariat? Olsen never asks that question, fortunately sparing himself the necessity of having to answer it.

This is an interesting, useful, and very brief study, open in its biases, clear in the presentation of its data. Sociologists, however, will be able to use it and rely on it more than historians.

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John Porter, *The Measure of Canadian Society: Education, Equality, and Opportunity* (Toronto: Gage 1979). Foreword and appended list of Porter's writings by Wallace Clement.

JOHN PORTER DESCRIBED the task of social science in the Prologue for this collection of essays: "to abstract from the confused flow of events perspectives which clarify and which permit some judgment about a society in the light of moral principles." His persistent concern for the moral implications of inequality attests to his commitment to that task. Some social scientists, he observed, attempt to be neutral when in fact they are merely neutralized to serve the status quo better: that, he charged, should be the position of the bureaucrat, not the intellectual.

As an intellectual, his stance was hardly in tune with his times while he wrote *The Vertical Mosaic*. Canadian social science, a disembodied reflection of British functionalism and American behaviourism, was so neutered that to not a few of its practitioners, Porter's philosophic insights and conclusions (if not indeed, the very nature of his inquiry) were less than scientific. There is little doubt that he suffered from this stultifying environment in Canadian universities. As many have critically noted, the *Mosaic* flirts with but never consummates a genuine analysis of power; it fails to probe the continental nexus within which power is exercised, or the regional disparities that power engenders. The Cold War fear of Marxist thought inhibited him, and he finally chose an approach to classes that removed them from their source in relations of production. All of that may be said, yet it remains true that *The Vertical Mosaic* was in the 1960s, and remains in the 1980s, a deeply moving account and a trenchant critique of Canadian class structure.

This collection includes one of Porter's earlier essays, "Power and Freedom in Canadian Democracy" (1961), essays from the mid-1960s on ethnicity, mobility,

federalism, education, and industrialism, a discussion of the research enterprise for the *Mosaic*, and a brilliant last article, "Education, Equality, and the Just Society." (1977)

Several of these essays will be unfamiliar to Canadian social scientists who no longer follow American publications as did Porter's generation. It is another mark of his times that so much of his work was delivered at American symposia and published in the United States. One reads them now with a certain bitter awareness that they were not only published there, they were, as well, designed for an American audience even when they were most explicitly about Canadian society. His perceptive essay, "Trudeau and Canadian Federalism" (1968), for example, was written for a seminar at Duke University; another on the concepts of melting pot and mosaic (1976) was prepared for Duke's bicentenary celebrations. Duke, of course, had a sophisticated Canadian Studies programme, and its faculty, unlike so many of their compatriots who were senior members of Canadian departments, knew what John Porter was about. So, to point out that these are pieces intended for Americans is not to criticize Porter but rather to lament that his time did not coincide with a time in which Canadian Studies could flourish in Canada itself.

This American orientation seems to have been a persistent obstacle to his understanding of international power, and that failure had effects on his most vital work, his inquiries into educational mobility and the nature of classes. But it is one of the strengths of Porter that, in the light of new evidence or better arguments, he was capable of reassessing his positions. There is evidence in this collection, as well as in his comments to students and colleagues, that his perception of regionalism and his understanding of sexual inequalities had long outdistanced the *Mosaic* by the late 1970s.

This ability to reconsider is particularly evident in the passage between his 1966

essay, "The Future of Upward Mobility," delivered to the American Sociological Association when he received the Maclver Award, and his 1977 lectures delivered at York University as "Education, Equality, and the Just Society." He notes in his preface to the earlier piece that it reflected the "dominant thought of the time," in its easy assumption of continued economic expansion in North America and its optimistic belief in the capacity of education to increase social equality. The lectures for York were undertaken "in part to reassess my own position," in the light of the various crises in western capitalist economies in the 1970s. The reassessment becomes one of the strongest indictments of the educational system in print, all the more powerful because it is Porter; the same John Porter who so fervently believed in the value of education for its own sake as well as for its mobility-potential for working-class students, who argues that:

This training for servitude at work, a lifetime without spontaneity or creativity or individuality — and that continues to be the condition of work for most — helps to produce the necessary false consciousness to legitimate alienated labour, where work is marked by a fragmented division of labour and over which the workers themselves have no control. (256-7)

This final essay evaluates the studies by Jencks and Coleman, and reflects on the philosophical treatise by Rawls in the light of Porter's own, voluminous work on the Canadian educational system. In the second part, he evaluates the contributions of Braverman in a critique of "credentialism," a critique as much of his own, earlier position as of capitalist educational systems. It is clear Porter was on a new avenue in these final years; had he been able to revise the *Mosaic* or to write its 1980 sequel, one imagines it would have been a profoundly disturbing document.

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John Allan Fry, ed., *Economy, Class and Social Reality: Issues in Contemporary*

Canadian Society (Toronto: Butterworths 1979); J. Paul Grayson, ed., *Class, State, Ideology and Change: Marxist Perspectives on Canada* (Toronto: Holt, Rinehart and Winston 1980); and Richard J. Ossen-berg, ed., *Power and Change in Canada* (Toronto: McClelland and Stewart 1980).

ALL OF THESE TITLES attempt to offer a dynamic, critical, conflict-centered approach to the analysis of Canadian society. Each of them is edited by a sociologist who subscribes to the political economy tradition of Canadian social science. One of the hazards of any collection of readings by multiple authors is that the individual selections may not add up to an integrated whole. The success of such undertakings thus depends very heavily on the extent to which the editor manages to create a measure of intellectual coherence through a judicious selection and organization of articles. This consideration has been a prime factor in the assessment that follows.

Of the three books lowest marks in this respect must clearly be assigned to Fry's *Economy, Class and Social Reality*. Indeed, apart from its physical properties this is virtually a non-book. The 19 articles between its covers "seek to analyze various social issues in Canada from a critical perspective." Although most of the articles were specially commissioned for this volume, the scheme utilized for soliciting the papers is anything but apparent. In his two paragraph "Introduction" the editor simply states that the book's "parameters . . . have been in part determined by the general theoretical posture common to all contributors." Beyond this, its scope is said to reflect "the particular interests of the editor." Unfortunately, the nature of either these commonalities or particularities receives no amplification. The various articles are grouped under the following headings: Economy and Class; Unemployment and Underemployment; Class Consciousness; and Selected Social Issues. The nine essays comprising the last

section are a real potpourri, containing everything from Gerald Sperling's whimsical "Confessions of a T.V. Addict" to Louis Feldhammer's shrill polemic "The Crisis of Confederation" to Gary Rush's abstract theoretical piece on "Political Economy and Social Movements." However, the rationale governing the other three sections is scarcely more evident, a shortcoming that is compounded by the fact that neither the sections nor the individual articles are preceded by editorial comments which might assist the reader to gain an image of the forest as well as the trees. The absence of editorial surveillance is further manifested in the unevenness of the essays in terms of their quality, length, and style. These serious deficiencies notwithstanding, it should be noted that this volume is partially redeemed by the inclusion of several illuminating contributions. Among the more familiar analyses in this category are Wallace Clement on uneven development, Leo Johnson on income inequality, and Pat Connelly on women's labour-force participation. Less well known but equally worthy of mention are Roy Bowles and Prudence Craib's "Canada: Economy, Opportunity, and Class," Harley Dickinson's "Canadian Foreign Aid," Robert Stirling and Denise Kouri's "Unemployment Indexes — The Canadian Context," and Singh Bolaria's "Self-Care and Lifestyles: Ideological and Policy Implications." Again, the pity is that these essays are not framed within a context that would enhance their pedagogical value.

Richard Ossenberg's *Power and Change in Canada* cannot be faulted on this score. It too is a compilation of essays written expressly for this volume but, in contrast to Fry, they were clearly prepared with a definite purpose and audience in mind. Consistent with its title, the seven essays examine the forces generating change in contemporary Canada as well as the role of those who wield power in preventing or controlling those changes. After Ossenberg's general overview of "Approaches to Power and Change" and

Daniel Glenday's probing essay on the political economy of Canadian dependency, the specific topics treated are labour (Paul Willox), education (Robert Pike), sports (Richard Gruneau), law (Laurreen Snider and Gordon West), and the military (Terry Willett). These are paired into three units, each of which contains an editorial introduction situating the articles and the issues they raise vis-à-vis the broader concerns outlined above. This is surely a plus. However, the purported unity linking the various articles occasionally comes off as a bit contrived. This is no doubt largely a result of the omnibus character of the book's proclaimed theoretical orientation — i.e., "conflict theory." Ossenberg's useful summary of the assumptions and themes of this approach in the book's preface only obliquely conveys that the term is mainly a convenient label for identifying otherwise diverse critics of a dying sociological orthodoxy which stressed harmony and stability by downplaying the impact of class and other inequalities on societal functioning. As such, this loose category encompasses a variety of both Marxist and non-Marxist perspectives whose internal disagreements are arguably at least as substantial as their mutual opposition to what is variously called order, consensus, or functional theory. In fairness it should be said that one of the contributors, Pike, acknowledges this very point in his impressive chapter on "Education, Class, and Power in Canada." Lest there by any misunderstanding I would hasten to add that I regard this as a relatively minor flaw in what is on balance an unusually well integrated and readable set of essays.

The Grayson reader differs from the others in at least three respects. The first is that it consists entirely of previously published papers. A second difference is that the contributing authors represent a wider range of academic fields, thus giving the collection a more interdisciplinary character. Thirdly, the editor explicitly adopts Marxism as a framework for organizing

and presenting the materials. The 22 articles selected generally give a good account of recent Marxist-oriented scholarship in Canada. For example, the anthology opens and closes with key essays by two prominent exponents of the resurgent political economy tradition — namely, Tom Naylor's "Dominion of Capital: Canada and International Investment" and Mel Watkins' "The Staple Theory Revisited." Two other notable features of this collection are the inclusion of several first-rate historical studies (by Steven Langdon, Craig Heron and Bryan Palmer, Allan Smith, and David Frank) and three essays on Quebec by francophone authors (Bernard Bernier, Marcel Rioux, and Paul Belanger and Céline Saint-Pierre). Many of the more interesting readings have hitherto been rather inaccessible (e.g., Jack Layton's "Nationalism and the Canadian Bourgeoisie: Contradictions of Dependence") and their republication in this volume should assure the wider audience they deserve. Like Ossenberg, this book has been thoughtfully assembled. Grayson has prepared a nine-page "General Introduction" that is intended to "provide those new to Marxism with a very broad idea of the ways in which the concepts class, state, ideology and change are used by some well known theorists." These concepts serve as dividers for the first four sections of the book; a fifth and concluding section deals with "Marxism and Canadian Political Economy." Each of these sections is accompanied by a brief introduction that attempts to relate relevant aspects of the articles that follow them to matters discussed in the General Introduction. Nevertheless I suspect most undergraduates will find their engagement with this book pretty tough sledding. For novices who are not already conversant with the fundamentals of Marxist theory the editor's introductions are probably too sketchy to supply the background necessary for a proper appreciation of many of the readings. What is more, because nearly all of the essays were originally written for

scholarly journals, they frequently employ a language and set of assumptions that many students are likely to find offputting.

By way of conclusion, might I suggest that perhaps the time has come for a Canadian version of Edwards, Reich, and Weisskopf's outstanding reader, *The Capitalist System: A Radical Analysis of American Society*. In my judgement the latter (especially the second edition) is a splendid model of intellectual craftsmanship for books of this genre. The recent burgeoning of Canadian political economy, a small sampling of which is reflected in the contents of the volumes reviewed above, would seem to indicate that there is no shortage of excellent raw material for a project of this sort.

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Maris Vinovskis, ed., *Studies in American Historical Demography* (New York: Academic Press 1979).

ONCE CONSIDERED the sole property of genealogists, antiquarians, and a small number of scholars, the study of population patterns is now a major ingredient of academic research in a wide variety of disciplines. Within the historical profession, the field has grown apace and now includes well-developed debates concerning both theory and research strategy. The emergence of these debates in recent years is clearly illustrated by the collection of 25 previously-published articles which Maris A. Vinovskis has brought together as part of Academic Press's series, "Studies in Population." All the articles have appeared during the past decade and for researchers familiar with historical demography, they are well-known. The dominant author in the volume is Vinovskis himself with five contributions including an introductory overview of the state of the art. Other major authors include Daniel Scott Smith, John Demos, Philip J. Greven Jr., Robert V. Wells, and Richard Easterlin. In all the

collection of articles represents the work of 21 researchers.

The topics and historical periods covered by the volume are diverse. Colonial America is well represented as is appropriate. Smith's general approach in "The Demographic History of Colonial New England" complements a variety of detailed community studies: Demos on Plymouth Colony; Greven on Andover, Massachusetts; Demos on Bristol, Rhode Island; Susan L. Norton on Essex County, Massachusetts; and Smith on Hingham, Massachusetts. Other articles focus on specific topics during the colonial period. Wells examines fertility and family size within Quaker families, Vinovskis and Edwin S. Dethlefsen discuss mortality in three separate articles, and Darrett B. Rutman and Anita H. Rutman consider the demographic impact of malaria in early Chesapeake. The Black experience is specifically studied by Russell R. Menard for Maryland slaves, 1658 to 1730; Jack Eblew for the nineteenth century; and Elizabeth H. Pleck for late nineteenth-century Boston. The largest group of articles focusses on the fertility debate beginning with Easterlin's important theoretical statement on the importance of environment, and continuing with Nancy Osterud and John Fulton on fertility decline in Sturbridge, Massachusetts; John Modell on Indiana in 1820; Wendall H. Bagh on Madison County, New York in 1865; Vinovskis on interstate patterns in the mid-nineteenth century; Tamara K. Hareven and Vinovskis on late nineteenth-century Boston; and Peter Ulhenberg on Massachusetts, 1830-1920. The volume concludes with Wells' well-known overview "Demographic Change and the Life Cycle of American Families."

The major strength of the volume lies in its successful gathering of important articles which are not always accessible. Thus, advanced undergraduate and graduate classes can now be exposed to this important field in a straightforward way. The major weakness of the collection

is largely a product of the field of historical demography itself. As Vinovskis' lead article admits, conceptual confusion is currently rampant and this confusion is reflected in the articles when considered as a package. Beyond the basic premise that population patterns must be a major consideration for historians, there is very little agreement among researchers on any aspect of the topic. At this time, problems of sources and measurement seem paramount and they threaten to undermine efforts at substantive progress. Taken together, the articles present a very disconcerting picture of historical demography and reveal that the early pioneering fervour of scholars is now giving way to a somewhat reluctant recognition of the tremendous complexity of the processes under investigation. Fortunately, this recognition has already and will continue to engender research that is increasingly valuable to scholars working in other areas. It is the integration of demographic analysis with the study of political, cultural, social, and economic history that makes historical demography such a challenging and exciting field. In this sense, the Vinovskis collection is valuable both as a statement of difficulties and achievements and as a promise of things to come.

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Gregory A. Stiverson, *Poverty in a Land of Plenty: Tenancy in Eighteenth-Century Maryland* (Baltimore: Johns Hopkins University Press 1977).

AT THE END of the colonial period about half of the householders in the largely agricultural economy of the Chesapeake did not own land. Until recently they, and tenants in other colonies, have constituted a historiographical blind spot in early American history. When noticed at all, the tenants' absence from the ranks of gentleman freeholders has been assumed to be either temporary or institutionally

anachronistic. Tenantry must have been a phase of individual life cycles before upward mobility worked its magic in a context of abundant land, or, in the case of New York, an exception proving the rule with land rebellions. Now several fine books have begun to compensate for the lapse, most notably, Sung Bok Kim, *Landlord and Tenant in Colonial New York*, which thoroughly examines landlord-tenant relations where they had the most enduring political and economic significance.

Gregory Stiverson has examined those tenants who leased their land from the proprietor of Maryland, whose "manors" had some systematic administration. To represent different subregional geographies and periods of settlement he selected eight manors for intensive study. In the absence of the necessary complementary studies, he has been unable to compare his findings with the more general characteristics — if any — of early American tenantry, but for these tenants at least he has provided a thorough and detailed description of their social and economic lives.

Maryland was a proprietary colony. The proprietary family, the Calverts, owned ungranted land and controlled the governorship and most appointive offices. The proprietor could create freeholds by outright grant or by sale, but through the colonial period the proprietor remained Maryland's largest landholder, with nearly 200,000 acres. Proprietary manors were simply those tracts of land on which there were no freeholds. As absentee landlords the Calverts had little direct concern with tenants' use of land. They expected to realize income by selling tenanted lands after the initial settlers had improved them as agricultural properties — a form of land speculation without capital that Stiverson refers to as "developmental leasing."

The terms of leases were simple and easy, while demand for leased land was steady after 1710. Thereafter Maryland's population nearly doubled every 20 years, and without extensive western land to take

up, the option of frontier settlement was more difficult than in Virginia, Pennsylvania, or North Carolina. The annual proprietary revenue from manors was never over £1000. Most leaseholdings were over 100 acres; the rent per 100 acres was usually only ten shillings; and most tenures were long, three lives being usual. Developmental leases specified that timber not be wasted, houses be built, and orchards planted. Labour services were almost non-existent, and the clearing and use of land went unspecified. Laxness characterized the administration of the proprietor's interests. Manor stewards had little to do besides collect rents, and the proprietor's agent usually ran several years behind in accounts. Developmental leasing was a financial failure. In 1766, after a desultory effort to have the Governor improve the management of the proprietary manors through better land records and increases in rents and alienation fines, Frederick Calvert inexplicably ordered the sale of all his lands. Most of the land went unsold.

Tenantry did not itself define a social group; landowners, non-residents on the manors, might lease proprietary land as well. But those resident tenants without other land did have a similar social character. They were poor. Most proprietary tenants left estates at death of under £100, with animals and household furnishings making up over half the value. Few had slaves, and most relied on their families for labour that made self-sufficiency a marginal proposition: the labour necessary to raise sufficient food had to be diverted from the cash crop of tobacco which might have produced a surplus. Half the cultivated acreage was required for the production of food; income from the sale of tobacco and wheat barely met the demands of store credit and rent. It was the shortage of capital and labour, not the shortage of land, which kept them unpropertied in the Chesapeake's staple economy. Alternatives to agriculture for employment were few and scarce. They were tenants because they were poor, not poor because they

were tenants. Their restraints on prosperity, from sharing in the promise of plenty suggested in the book's title, were similar to those bearing on poor landowners as well. In the context of the eighteenth-century Chesapeake, tenantry provided a relative prosperity and security. The commercial agriculture of the Chesapeake already had its rural proletariat with the slaves, and the demands for cultivable land were not strong enough to displace the small-holders from a meager economic independence. By the 1760s most tenant families had lived on their leaseholds for decades, had numerous collateral and affinal kin in the locale, and had only their leaseholds to provide a livelihood. Resident leaseholders almost never sold their tenements to take up freeholds. They composed a "stable rural community." (40)

There were two sources of large-scale social change for the tenants, the adoption of wheat in place of tobacco as a money crop and the confiscation of the proprietors' property during the Revolution. The most valuable chapter in Stiverson's book is that on tenant agriculture, though what he has to say about tenants would apply to small freeholders as well. Grain had commercial advantages as prices rose and new markets developed, but tobacco had structural advantages for the tenant. The traditional staple could be grown on small acreage, and its labour demands gave little advantage to scale or fixed capital while favouring intensive, constant labour. Wheat required plows and extensive land, and the crop posed acute labour demands that hired labour could best meet. Those tenants with access to grain markets were, by definition, the most prosperous Stiverson studied.

They were also the ones who suffered most from the revolutionary government's sale of confiscated proprietary property. The state refused to substitute as landlord, so the policy of land sale eliminated the security tenants had with the proprietor. The state had more success than the proprietor at selling land. It offered lower prices,

and the wartime increase of paper money and other forms of public credit had provided numerous potential purchasers with cheap money. By the end of the war most of the proprietary-manor tenements were sold. Tenants on the poorest soil with the fewest improvements were usually able to buy their land because they had little competition in bidding. Those tenants who had best been able to produce a surplus were almost completely dispossessed, as outside money, and frequently the land commissioners themselves, dominated the state's auctions. By an exquisite irony, a revolution prizing independence, as embodied in the freeholder, had abolished economic privilege, and provided the means for the landless to lose their independence.

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Jerry M. Cooper, *The Army and Civil Disorder: Federal Military Intervention in Labor Disputes, 1877-1900* (Westport, Ct.: Greenwood Press 1980).

DURING THE GILDED AGE, American workers found themselves confronting the United States Army in a number of bitter disputes beginning with the widespread railway strikes of 1877 and culminating in the destruction of the Western Federation of Miners at Coeur d'Alene. In a work of essentially military history, Professor Cooper has told us more about the impact of such episodes on the army and its generals than on struggling workers. Like most historians, Cooper may not favour the big battalions but he does know where the sources are richer.

Though it was small, scattered, and largely manned by recent immigrants, the American regular army was a remarkably effective and relatively bloodless strike-breaking weapon. In 1877 and 1894, tiny detachments overawed strikers and dispersed rioters even after much larger forces of state militia had failed. Cooper's expla-

nation weighs heavily on the Civil War experience of senior officers and their lively awareness of what carnage they could cause. General Winfield S. Hancock in 1877 and General John M. Schofield in 1894 certainly produced calmer and more realistic appreciations of the situation than did most of their civilian superiors. The vain and insubordinate General Nelson A. Miles, busy finding anarchist plots behind Eugene Debs's Chicago-based American Railroad Union, was an exception but even Miles kept his soldiers under tight control and his arrogant insubordination probably caused more trouble than good to the railroad proprietors and their political lieutenants.

The real exception was Brigadier-General Henry Merriam, an inexperienced second-rater, obviously not wanted for the Spanish-American War. By handing over his soldiers to Governor Frank S. Steunenberg for use at Coeur d'Alene, Merriam not only compromised the army's reputation; he also broke the law. As in Canada, legislation created some unexpected obstacles for the use of troops in strikes. The *posse comitatus* law of 1878, passed under southern pressure in Congress, prevented the widespread use of federal troops common during the Reconstruction period. Ironically a law designed to protect groups like the Ku Klux Klan made it harder for state governments to use federal troops against organized labour.

Professor Cooper has, of course, chosen to tell only the smaller part of the story of labour-military conflict. In a far larger and bloodier list of episodes, workers were pitted against state militias or national guards. In many cases, such forces were euphemisms for company guards or hired gunmen; in other circumstances, like Pennsylvania in 1877 or in Washington and other states in 1894, militia sympathies were all too plainly with strikers or the unemployed.

Except at Coeur d'Alene, where Merriam's troops added to their unpopularity by the unfortunate coincidence of being

from the black 24th Infantry, American regulars were often initially welcomed by both sides. Federal troops benefited from the nationalist mood after the Civil War and the very infrequency of their appearance suggested an objectivity which no one claimed for state militias.

Any strikers who expected neutrality were, of course, sadly deceived. The army and its generals, Cooper insists, were firmly on the side of order. Senior officers might not be bloodthirsty but they were easily flattered by railway magnates and corporate executives. They hoped — vainly as it proved — that the army's effectiveness in 1877 and 1894 would lead to increased strength and fatter appropriations from Congress. In fact, the speed and efficiency of military intervention reassured the politicians that the army was strong enough for their purposes.

American experience has some Canadian parallels. In both countries jurisdiction and control remained almost deliberately confused by the refusal of politicians to set clear guidelines for very predictable problems. Though Canada's volunteer militia resembled the state-controlled national guards in training and efficiency, it seems to have performed as effectively as U.S. regulars in its frequent summons to "strike duty." That might be small comfort to Canadian workers but it spared Canada's past from such tragedies as Milwaukee in 1886 or Ludlow in 1914.

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John N. Ingham, *The Iron Barons: A Social Analysis of an American Urban Elite, 1874-1965* (Westport, Ct.: Greenwood Press 1978).

THE IRON BARONS, recipient of the Ferguson prize of the Canadian Historical Association, considers American businessmen in their social and cultural setting. It seeks to describe the institutional context —

namely the marriages, kinship ties, clubs, schools, and neighbourhoods — necessary for economic success and the eventual ascription of upper class status. Inspired by E. Digby Baltzell's study of Philadelphia's upper class between 1880 and 1940, Ingham has compiled biographical data on 696 iron and steel executives active between 1874 and 1901 in six cities: Pittsburgh, Philadelphia, Cleveland, Bethlehem, Pennsylvania, Youngstown, Ohio, and Wheeling, West Virginia. As more than half the sample commanded Pittsburgh steel companies, that city receives the most attention. Ingham studies not only the executives themselves, but also considers their success in passing on their social and economic status to approximately 12,000 descendants.

Ingham states that the social origins of the iron barons belied the "rags-to-riches" myth associated with the spectacular career of Andrew Carnegie. His findings dovetail with earlier studies of the national business élite at the turn of the century: that is, roughly two-thirds of the iron and steel men came from families prominent in business or the professions prior to the Civil War, while one-third were products of middle class or working-class homes. Only two per cent had genuinely poor or disadvantaged backgrounds, and they tended to work for an employer like Carnegie who had similar roots.

Predictably, economic opportunity varied with "the age and maturity of the economic and social system" in each city. Thus Cleveland, a midwestern city, had a more fluid structure than Philadelphia. But even interior towns tended to be dominated by a single cultural group — the Scots-Irish in Pittsburgh, New England Yankees in Cleveland and Youngstown — who quickly established themselves as the local social arbiters. Where several waves of migration overlapped, as in Wheeling, those with non-élite social backgrounds found fewer obstacles to economic advancement. "Horatio Alger would have been proud of Wheeling," Ingham con-

tends, for 27 per cent of its iron and steel leaders were "sons of men who had been in the working class (primarily as skilled iron workers) at least part of their working lives." (71, 78) Like Paterson, New Jersey the city offered abundant opportunity to the aristocracy of labour to become capitalists, but even in Wheeling, Ingham observes, access to corporate office was effectively limited to the "top third of the American occupational and social order." (78)

Even so, enough of the iron and steel barons belonged to the *nouveaux riches* to threaten the continued power and prestige of the antebellum élite in each city. The most amenable and presentable of the *parvenus* had to be co-opted into the local upper class if it were to sustain itself. Before the Civil War, the economic élite had been relatively small and homogeneous and did not require complex institutional arrangements to sort it into different social strata. But in the 1880s, Ingham asserts, the boundaries of the upper class became increasingly "extra-communal and associationally defined." (84) The selection process devolved on such upper class institutions as the boarding school, the ivy league college, and the gentlemen's club. Over a series of social hurdles the families of the *nouveaux* vaulted until they passed the final barrier of marriage into an undeniably patrician family. Ingham states that the "formal institutions such as prep schools and social clubs . . . functioned in large measure as a pre-screening process for marital selection." (99)

Marriage, on the other hand, was the inner sanctum of upper class society, and the best index of a family's social standing, Ingham declares, is its marital pattern. The urban upper class tended to be endogamous, its rare marriage outside the fold playing an important role in the co-optation process. Ingham believes the élite families in each community can be ranked socially according to their success at marrying into the local upper class. At the apex of the social pyramid he locates the so-called

"core families," in other words the "very core of the social upper class of [each] city, families which intermarried with one another extensively, almost to the exclusion of other families, and had the highest indices of membership in the other prestigious social institutions." (99) These families, he contends, guarded the gates. They were the heart of the upper class and responsible for its preservation. In a city like Pittsburgh and Philadelphia the most prominent families at the turn of the century proved quite parochial in their social interaction, especially marriage, prompting Ingham to question Baltzell's contention that a cosmopolitan, national upper class took shape by the end of the nineteenth century. Status, as defined by the local social registers, depended upon the quality of one's local alliances.

Ingham largely derives his model from the Pittsburgh experience where a "viable, functioning social network system at the upper class level [existed] with marriages, families and kinship systems lying at its very core." (152) Pittsburgh, its dependence on steel scarcely diminished over the years, not surprisingly has consistently accorded high status and power to those families most adept at making the marital and business alliances necessary for pooling the resources of the local upper class sufficiently to preserve its influence over the mammoth steel companies. Even so, Ingham's core families do not include the Mellons, despite their uncontested pre-eminence in Pittsburgh and the nation, for they tended to marry outside the local upper class. That omission gives one pause.

Even more disconcerting are Ingham's findings for Cleveland. There core families had generally less preferred social, cultural, and economic origins, and therefore status, than the non-core families whose exogamous marriages eventually caused them to drift away from the city. As a result, a viable upper class community failed to emerge. Ingham attempts to explain this phenomenon in terms of the

Yankee descent of the city's old families, as he argues plausibly that transplanted New Englanders might have been more tempted to look eastward for a spouse than the Scots-Irish of Pittsburgh. Yet his own data fails to substantiate this hypothesis, for Cleveland's Yankee families, while cosmopolitan in their marriages, were not especially prone to return to the ancestral fold.

Ingham perhaps places too much stress on marriage as the key social variable. With the rising scale of business in the late nineteenth century, fortunes were accumulating that had little need for the social sanction of the local upper class networks. Henry Ford, for example, flouted Detroit's social conventions at every opportunity. The local patricians tried to freeze him out socially, but found that impossible when the Prince of Wales insisted on a guided tour of the Ford plant or when the Ford fortune became so large that the city's private charities became dependent on it. According to one chronicler, Ford had to stop going to Detroit's elite social clubs and receptions because he found himself "besieged" by the local patricians, each pleading for his or her own pet cause. Of what relevance, then, was Detroit's upper class social system in the presence of such raw power? Each of the major cities had an individual or group by 1930 with comparable overriding influence. *The Iron Barons* does not adequately consider the impact of the massive concentration of economic power and resources since 1901. Nor does it really confront C. Wright Mills' observation in the *Power Elite* that urban élites have become increasingly powerless and irrelevant in this century.

Ingham's book lacks the dynamism that a close observation of economic change in each city might have afforded. After a brief overview in chapter one of the steel industry, the analysis neglects the economic structure of urban society. The disintegration of Cleveland's upper class institutions and the wholesale exodus of its

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iron and steel heirs Ingham attributes to the unique social composition of the city's élite. This explanation is unconvincing, for it ignores a revolution after 1900 in the city's economic foundations. Elsewhere, when speaking of industrial satellites like Bethlehem and Youngstown, Ingham notes:

Generally, the strength and viability of the upper-class stratification system could be ranked according to the size of the city. . . . The degree of economic and social independence of a community was important. This factor had little impact upon the larger cities . . . but was critical for the smaller ones. Wheeling and Bethlehem tended to operate within the economic and social orbits of Pittsburgh and Philadelphia, respectively. This tended to aid in the retardation of their upper-class social systems. . . . (218-219)

But why should Ingham limit the impact of dependence to the smaller centres?

Cleveland, despite its metropolitan stature, was also a satellite, first of New York, and increasingly of Detroit, as auto parts and machine tools developed into its most important industry in the 1920s. The big Detroit automobile companies had considerable leverage with their Cleveland suppliers, and some of the largest factories in the city by 1929 were merely assembly operations. Moreover, Cleveland's dependence within the iron and steel industry deepened. The most important Cleveland-owned firms were probably Pickands, Mather, H.M. Hanna Company, and Cleveland-Cliffs. All of them dealt in iron ore, and by the 1920s found their room for manoeuvre sharply constricted by growing concentration within the steel industry.

Each made its own bid for survival — Hanna by selling its ore operations to National Steel; Cleveland-Cliffs by backing the ill-fated attempt by Cyrus Eaton to build a Cleveland-based steel empire; and Pickands, Mather by helping Bethlehem Steel, a New York-controlled company, to contain the Eaton challenge. The Cleveland upper class was thus as fragmented in business by the 1920s as Ingham found it to

be socially. But the dramatic proxy battle in 1930 for control of Youngstown Sheet and Tube, which split the Mather family, then the city's most prominent, suggests that it was economic dependency — in this case on ore sales to outside steel companies — that caused the disintegration of Cleveland's upper class social system, rather than the opposite, as Ingham tends to argue.

One final observation is in order: the *Iron Barons* frequently imputes motive from the statistical data. For example, it attributes marriage patterns to a conscious desire either to win upper-class acceptance or to strengthen existing class arrangements. Marriages certainly had these effects, but without resort to diaries and letters Ingham can only surmise the degree of self-consciousness involved. Ingham might have made use of society page reports on dinner parties, debutante balls, and the like to ascertain whether some families deliberately, as he states, set forth to bring new blood into the social establishment. (133) Despite these caveats, the *Iron Barons* is a most impressive accomplishment, praiseworthy for the research behind it and the interdisciplinary tools used.

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Edward Greer, *Big Steel: Black Politics and Corporate Power in Gary, Indiana* (New York: Monthly Review Press 1979).

THIS IS A well-written, interesting, and provocative book. Edward Greer is a lawyer with a master's degree in public health who was special assistant to Richard Hatcher, the first black mayor of Gary. To a limited extent the book is an examination and a defence of the actions of both Hatcher and Greer, who was head of the Office of Program Coordination in Gary. In this defence, Hatcher emerges as a well intentioned man who truly has the interests of

the black community and the people of Gary at heart. But because of the prevailing political-economic framework of the city, he has had only very limited success in achieving his stated goals. Greer appears as the only true radical in the administration; a man whose vision and understanding of the stakes involved transcended that of Hatcher and the other members of his administration. Whether or not this is true will await other perspectives from the individuals involved.

Yet Greer's book is far more than an *apologia* for either himself or Hatcher, and the significance of the work derives from the theoretical framework for the study of community power developed and applied to the Gary scene. Greer rejects both the pluralist and power elite theories of community power, employing instead the more complex formulations of the Italian Marxist, Antonio Gramsci. Whereas the pluralist theory asserts a fundamentally democratic society, in which the "people" are sovereign over the state and real reforms are possible, the power elite theory views the political system as being in thrall to monopoly capital with reform merely an apparition rather than a reality. While Gramsci's theory recognizes that monopoly capital has the power to protect its fundamental interests — its ownership of the means of production and its right to extract surplus value from wage labour — it also concludes that it must make real and substantial concessions to other social classes. This allows the capitalist class to attain a position of "hegemony," which enables it to contain class antagonisms on a level at which its legitimacy is not dangerously questioned. The key group in the creation of this hegemonic position, at least in Greer's application of the theory, are the group he terms "competitive capital" or the "petty bourgeoisie" — the small and medium-sized competitive elements within the local capitalist community. By making certain very real concessions to this group, monopoly capital is able to protect its most treasured interests

from any fundamental attack by the working class.

The great contribution of Gramsci's theory, and Greer's use of it, is to alert the academic community to the overweening importance of *class power*. Greer continually forces the reader to address the realities of class in relation to race and political power and decision-making. This is, to my mind, a tremendous advance over earlier assumptions of either a classless society, or, more often, a class-neutral state which simply responded in a non-biased manner to the needs of the peoples and/or their organized pressure groups. To recognize that the political and social system exists within a value-laden atmosphere, and that these core values are either controlled or fundamentally influenced by the predominant capitalist class, is an important conceptual advance. Yet the concept of cultural and ideological hegemony, and its domination by the capitalist class, is often taken as a given, a perceived truth, by many analyses which employ Gramsci's theory. In their disdain for the mechanistic empiricism of the pluralists, they have often rejected the need for detailed evidence or logical argumentation to substantiate their claims. It is in this area that Greer's book, like several others in the genre, is seriously lacking.

One evidentiary problem with Greer's work lies in the unrepresentative nature of Gary — a one industry, one company city, with a black majority among its population. Among major United States cities in 1970, only Newark, New Jersey, Washington, D.C., Atlanta, Georgia, and Compton, California, along with Gary, had a majority of blacks in their population. None of these were single industry cities, and certainly none had a single company dominating their economic scene as U.S. Steel does in Gary. Greer anticipates this criticism by answering that "every city is distinctive to some degree," and that "every problem and aspect of community life here addressed is essentially replicated

in numerous other cities." (15) This, of course, is not the point. While on one hand Greer insists upon the necessity of understanding the manner in which the political economy of an area, along with its indigenous social and cultural groups, creates the dominant cultural system which proscribes the limits of political action, it is precisely this aspect of Gary which is unique — which is *not* representative of other American cities.

One wonders why he did not, at the very least, compare his findings to those of another steel city. Pittsburgh was older, more economically and culturally diverse, with a steel industry made up of several larger employers, rather than dominated by a single firm. Further, Roy Lubove, in his *Twentieth Century Pittsburgh*, followed a roughly similar pattern of analysis of power and decision-making, and addressed many of the same issues considered by Greer — housing, planning, taxes and environmental regulation. Yet Greer not only makes no explicit or implicit comparisons between the two cities, he does not even cite Lubove's book in his notes or give any indication of being aware of it. A rigorous comparison between the two might have allowed the reader to see more clearly the role that a single large employer or a black majority played in the political framework.

Even if we do accept Greer's dictum that Gary *is* typical, I have some difficulties seeing how his mode of analysis produces a picture of power and decision-making in Gary which is fundamentally different from that of the pluralists. He deals with four major policy issues of the Hatcher administration in historical terms: the police department, housing reform and urban renewal, property tax politics, and environmental regulation. The first two issues pose the most paradoxically difficult problems, while the latter two seem to demonstrate a powerful hegemony on the part of U.S. Steel. As Greer makes quite evident, for a significant portion of its history, the steel company considered the

local police to be an important adjunct to their control of wage labour, as their use in the 1919 Steel Strike amply demonstrates. As Greer says, "Gary's police force functioned as an auxiliary of U.S. Steel." (113) Yet, in the 1930s the company lost effective control of the police to the unions and the Democratic machine of the white working class. In turn, during the Hatcher administration, wresting control of the police from the white lower middle classes and working classes was the *major* goal of the blacks. And, as Greer makes evident, in this goal they have been largely, if not totally, successful.

Urban renewal poses another interesting series of developments. It was opposed, unsuccessfully, by U.S. Steel throughout the 1950s, because it might lead to an increase in their taxes. Later they were to give the issue at best a grudging neutrality. It was also strenuously opposed by small businessmen and the black community, and the local Chamber of Commerce did not support it until *after* 1968, when Hatcher had greatly strengthened the public housing component for blacks. The main advocates of urban renewal were a segment of the local "petty bourgeoisie," the banks, realtors and construction companies and unions. They were able to carry the day with the support of the local Democratic machine.

The issue of property taxes in Gary, and U.S. Steel's determined and large scale avoidance of same, poses another interesting problem. From its very founding in 1906, U.S. Steel's Gary plant has persistently underestimated its plant assets, resulting in lower property taxes and correspondingly higher profits. This can be perceived to be a core interest of U.S. Steel, and one in which they have exercised hegemony over the years. Yet the major opposition to their tax position, beginning as early as 1907, came from the other business classes in Gary, whose taxes were proportionately higher as a result of the steel company's tax avoidance. The working class, black or white, never

seemed to consider this an issue of major importance, assuming that lower taxes for U.S. Steel meant more jobs and higher pay for them. Thus, U.S. Steel was able to fight off all challenges over the years, and Hatcher quickly backed down on this issue when he became mayor.

Again, in the area of environmental pollution and its regulation, even though it affected all residents and fell (literally) most heavily on the black poor and the black and white working classes, making Gary generally a terribly undesirable place for anyone to live, most evidently did not feel it was an issue of major importance. The prevailing motto in Gary was "no smoke, no jobs," and although Hatcher made some abortive efforts at reform in this area, it was the federal government, unfettered by the socio-political considerations existent in Gary, which made the most dramatic moves on this issue.

Greer argues that an analysis of this constellation of issues shows that "political power in the United States is in large measure shared between competitive and monopoly capital. . . ." (205) Yet this does not seem to be self-evident from his argument. A pluralist would argue that these salient issues simply demonstrate "trade-offs" between and among classes, as each defends and protects the issues most important to them and compromises on those of less direct importance. The blacks considered the police issue most important, and on this they largely carried the day. U.S. Steel conceded control of the police, grudgingly accepted urban renewal and housing reform, to protect its vital issues of taxes and environmental regulation. It is the position of the petty bourgeoisie which is least clear in Greer's analysis. Although he claims that "[t]his close alignment of the petty bourgeoisie with monopoly capital in control of the government and civil society . . . explains contemporary American political life," (205) this does not seem evident from his analysis. They opposed black control of the police, and lost. A large segment of

them opposed urban renewal (especially the smaller businessmen), and lost. They opposed U.S. Steel's preferential treatment in property taxes, and lost. Only in the area of environmental pollution does there seem to be some perceived community of interest with U.S. Steel, but hardly any direct benefit. Although I think that the concept of "hegemony" is a valuable one, Greer's application of it in Gary seems to demonstrate the pluralist side of American urban politics.

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Daniel Nelson, *Frederick W. Taylor and the Rise of Scientific Management* (Madison: University of Wisconsin Press 1980).

THERE HAS BEEN in recent years quite an extraordinary revival of interest in the historical phenomenon of Taylorism or scientific management. Much of this interest has been generated by left-wing historians and sociologists of labour, drawn with horrified fascination to Frederick Taylor and his disciples who spelled out in such breathtaking clarity the repressive power relations of the capitalist workplace. Of particular interest is the response of the industrial working class to the introduction of the stopwatch and other forms of managerial control over the pace and conditions of work, as well as the relation between Taylor's brutal realism and later, more deceptive, versions of managerial ideology still with us.

All the renewed interest in Taylor has not, however, been on the left. A distinguished addition to this literature is Daniel Nelson's new biography. This is a crucial source: no one seriously interested in the subject can in future neglect Nelson's basic spadework. *Frederick W. Taylor and The Rise of Scientific Management* largely supercedes the "official" biography by F.B. Copley published in 1923 and entirely dispenses with a reductionist psychobiography by Sudhir Kakar pub-

lished ten years ago. But by no means is it the last word on the subject. Nelson's considerable strengths conceal certain weaknesses.

Nelson's greatest strength is his penetrating and detailed knowledge of late nineteenth and early twentieth-century factory organization, exemplified in his earlier work, *Managers and Workers: Origins of the New Factory System in the United States, 1880-1920*. What is most refreshing here is his impressive grasp of the myriad practical details of how different kinds of technical processes gave rise to different kinds of factories with specific types of managerial control and differing labour relations. Labour historians will find Nelson's work worthy of careful consideration, for he has sorted through a wide array of primary and secondary sources on factory organization with a practical eye for the kind of detail which gives solidity and context for changing forms of labour organization and action in the era of the birth of corporate capitalism.

This is Nelson's most valuable contribution to our understanding of Taylorism. Others have spoken in general terms of Taylor's innovations: systematization of procedures; specialized planning of production control; functional foremanship; time study of the labour process; and 'incentive' wage plans to induce higher productivity. Nelson tells us what actually happened to these ideas in practice, and how they became absorbed into the day-to-day workings of American industry. He is particularly useful in showing how unsystematic Taylor often was, how expedient in compromising his "principles" in coming to terms with the owners who paid for his advice while always retaining control, and how much of the success of "scientific management" was in fact the result of clever packaging of a concept which seemed timely, even at the expense of the actual core of organizational methods. Drawing on the Taylor archives with greater resourcefulness than other users, Nelson provides a detailed picture of Tay-

lor's career as a management consultant to various enterprises — sometimes severely undermining Taylor's own well-known reminiscences of his lessons learned from experience. This constitutes the best part of the book.

Nelson does not probe very deeply into Taylor's personality, eschewing judgments on motives and psychology. Granted the dangers of facile speculations, many readers will find this reticence disappointing. After all, Taylor was a major ideological entrepreneur of managerial capitalism and his own personality seems to speak eloquently of certain obsessive qualities of an earlier era of capitalist culture: a driving mania for the efficient utilization of human creativity for material production; a moral horror of "soldiering" or reduction of output by workers; a burning desire to establish managerial control over the production process even at the expense of a certain tension with the capitalist owners; and above all a relentless will to dehumanize labour by "scientifically" transforming men into machines. Taylor, who invented a special efficient club to play golf, and whose last observed act before dying was to sit up in his hospital bed to wind his watch, is a peculiar case the complexities of which Nelson never seriously engages.

Nor is Nelson very informative on Taylor's relationship to the intellectuals and publicists of the Progressive era. This story has been told better elsewhere, for example, in Samuel Haber's *Efficiency and Uplift* published in the early 1960s. Once outside the familiar boundaries of the factory system, Nelson generally fails to find very sure footing in the wider world of politics, society, and ideas.

To my mind, however, Nelson's greatest failing is his reluctance to consider Taylor's attitude to workers as evidence of a larger class concept of capitalist society. Nelson seems embarrassed by Taylor's views on labour, and constantly suggests that he gave undue prominence to this aspect of his theory to the detriment of

more technically functional features. The problem is that Nelson is forced to go against the grain of his own subject, whose obsession with the "labour question" bespoke a clear and penetrating insight into the central conundrum of capitalist production: control and expropriation of surplus value. Nelson insists on a kind of technocratic reading of the workplace, but both Taylor and the capitalists who hired efficiency engineers knew very well that it was control of labour which was the crucial problem, one which far surpassed the difficulties of organizing new productive techniques into an efficient factory system.

It is symptomatic of Nelson's blindness to the importance of class relations in the workplace that he makes a single dismissive footnote reference (in his preface) to Harry Braverman's *Labor and Monopoly Capitalism*. As a historian, Nelson is suspicious of generalizations not based on familiarity with the primary sources. But Braverman's generalizations raised profound questions about the significance of Taylorism in the development of the capitalist labour process. Nelson, for all his detailed knowledge of specific factory systems, fails in the end to give Taylor his historical due, while Braverman at least paid him the respect owing a leading theorist and practitioner of capitalist domination of the working class. It is thus something of an irony that Nelson's technocratic history of Taylorism, however accomplished and informative in detail, needs to be read in conjunction with Marxist interpretations in order to bring out the real historical significance of Frederick Taylor, father of scientific management.

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Mimi Conway, *Rise Gonna Rise: A Portrait of Southern Textile Workers* (Garden City, N.Y.: Doubleday 1979), and Susan Estabrook Kennedy, *If All We Did Was To*

Weep At Home: A History of White Working-Class Women in America (Bloomington, Ind.: Indiana University Press 1979).

THESE TWO BOOKS represent opposite poles in working-class social history. One is a sensitive example of oral history at its best, while the other suggests that the ravages of inflation have now hit the market place of ideas.

Mimi Conway's book, *Rise Gonna Rise*, represents the former. Through a series of interviews which are complemented by an evocative group of Earl Dotter photographs, Conway explores the history of Roanoke Rapids, North Carolina and the struggle between workers and management at the J.P. Stevens Company. Conway allows the people of the town, both workers and managers, to explain themselves and their origins. Their own words combined with the author's sensitive intervention give the reader a sense of the texture of social life and relations in a southern textile milltown. Conway's book suggests the strength and staying power of mill workers determined to organize whether in the Carolina Brown Lung Association (CBLA) or in the Amalgamated Clothing and Textile Workers' Union (ACTWU). It also raises some serious questions about the power and appeal of southern paternalism for those same individuals.

A major theme which emerges from Conway's interviews is the contradictory nature of southern paternalism. On the one hand the voices in the book consistently rail against J.P. Stevens for its speedups and the depersonalization of work relations. On the other hand, even the most militant workers interviewed fondly remember Stevens's predecessor, the Simmons company. Louis Harrell, lifetime mill worker and CBLA activist notes, "When the Simmons Company had the mills, they didn't do you like this." (19) Frankie Woods who, with her sister Eula, provides one of the most moving chapters in the book recalls, "Now that Sam Patter-

son [the former manager], he treated you right. He wouldn't let *nobody* say nothing about mill people. . . . And he'd give you a ride if he saw you walking up from the mill. He'd tell his chauffeur to stop, and he'd pick you up. . . . And I'd get right in back with him. I'd sit there next to him, and he'd ask me how I was getting along." (36)

Stevens took over the Roanoke Rapids mills in 1956 and with their arrival paternalism ended. The new management insisted on a speedup of work. But what upset the paternalistic equilibrium even more was the new owner's disregard for the old social relations between mill workers and mill owners. Louis Harrell observed, "See, before it was overseers. Now they were reasonable people, not men like what they have in there now. They treated men like people not cattle. . . . I'll tell you the thing that makes me angrier than anything else is to go to a supervisor and tell him something and have him turn away from you." (20)

Conway postulates an intriguing set of distinctions among the workers which shaped their attitudes towards paternalism and also towards the union. The black workers Conway spoke with are devoid (not surprisingly) of the memories of the old days and old company management. Most came from rural families and maintained their links to the land even while working in the mill. This leads Conway to an interesting observation. Only those workers, whether black or white who maintained their ties to the land, escaped that collective memory of paternalism. Lewis Edwards, a white worker, "considered himself a farmer first" even though he had worked in the mill for 15 years. His identity with the land, Conway argues, "gave him more in common with rural blacks, newly hired in the mills than with older, white mill workers with their long experience with the mill's history of paternalism." (120) Conway does not draw out the implications of her observation, but her interviews with union and Brown Lung

activists become all the more striking in its light.

Conway's book begins with the union victory in 1974 and ends with the funeral of Louis Harrel who died of Brown Lung Disease. Although the union and the CBLA are not always in agreement they unite in *Rise Gonna Rise* in their opposition to the Stevens Company's long and bitter attempt to prevent organization among its employees. Brown lung victims' words haunt the reader as Frankie Woods exposes the scars of her years in the mill. "She's been blessed, I think. Don't you?" asks her sister, Eula. "We've both been blessed. I have brown lung and I'm near blind, but I'm still alive." (40) And the optimism and strength of the union fight is echoed in the words of Maurine Hedgepeth as she returned to work after being fired for union activity, ". . . the workers do have rights . . . just because I was involved in the union and the company didn't want me back didn't mean that I was going to disappear or blow away." (196) In the sparse field of southern labour history, *Rise Gonna Rise* stands out as a study of a town and its people and gives us an unusual glimpse at the everyday lives and values of southern workers.

If Mimi Conway's book is a sensitive treatment of a hitherto "inarticulate" group, unfortunately the same cannot be said of Susan Estabrook Kennedy's treatment of working-class women in *If All We Did Was to Weep at Home*. From her definition of working-class women in terms of income level ("caught between poverty and the middle class" [239]), to her conclusion that working-class women have "finally reached the outer fringes of the middle class" (239), the book is fraught with inconsistencies and downright absurdities. Two major problems pervade the book. First, it is poorly written and virtually un-edited. Sentences or even whole paragraphs startle the reader with their awkward construction and often garbled message. For example, in trying to characterize contemporary white working-class

women Kennedy writes "More recently, however, blue collar women, white ethnic women, and working-class women have begun to emerge in all their facets — sex, race, class, caste, minority, organized and unorganized, single and married, private and public, employed and nonearning, and even historical." (241) One can only guess what she means here. Kennedy's use of words, particularly the term working class itself, is fundamentally ahistorical and renders many of her arguments absurd, as for example her statement about social life in the seventeenth century: "During the seventeenth century, working-class women had remained almost indistinguishable from all but the most prosperous planters' and merchants' ladies." (9)

The more serious problem with Kennedy's book lies in her basic argument that white working-class women have spent the last 300 years struggling to escape from the working class into middle class respectability. Kennedy asserts that "while industrialization widened the gulf between middle-class and working-class women, the same phenomenon did not sharpen class perceptions; by presenting ladyhood as the goal for all women, society insured the commitment of working-class women (and men) to the struggle for upward mobility." (18) Kennedy brings no new evidence to bear on this complicated assertion nor does she acknowledge the wider debate into which she implicitly falls.

Equally serious is the fundamental disdain Kennedy shows for her subject and her total disregard for the historical arguments she so copiously cites in her notes. In unblinking and uninformed assertions we are told that "most working-class women have been discouraged from developing attitudes on anything." (xvi) "The Irish were concerned with earning for survival rather than asserting their character and intelligence," (50) and even that "Adopting the values and aspirations of the native born population Irish Americans with few exceptions plodded diligently in the direction of the middle-class." (53)

Working-class women and men, as Mimi Conway's book reveals, surely deserve better from both historians and their publishers.

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Martin Glaberman, *Wartime Strikes: The Struggle Against the No-Strike Pledge in the UAW During World War II* (Detroit: Bewick Editions 1980).

THIS BOOK CHRONICLES events surrounding the UAW no-strike pledge and, based on the dramatic inconsistency of workers' attitudinal and behavioural reactions to the pledge, explores the broader question of the relationship between working-class consciousness and activity. While the data were culled from numerous sources, including materials from Wayne State University's labour archives, they are refracted through Glaberman's wartime experiences (unacknowledged in the Foreword) as a Detroit production worker, UAW member and shop steward, and member of the now defunct Workers Party.

Four days after the Japanese attacked Pearl Harbor President Roosevelt initiated a series of meetings with representatives of business and labour which led to a no-strike pledge. Labour leaders throughout the United States quickly fell into line, even though rank-and-file unionists had not been consulted. Top UAW officials adopted the no-strike pledge as well as a ban on overtime pay, and then went through the motions of "consulting" the membership at a special convention. To minimize rank-and-file input, the UAW Executive Board (which included Walter Reuther) ruled out the election of conference delegates and refused to allow either members or delegates to see the programme to be "debated" prior to its presentation at the conference. As Glaberman observes, UAW officials rushed to enlist workers' cooperation with the war effort, while "with very few exceptions business

leaders never permitted patriotism to interfere with profits."

The wartime programme of unions was buttressed by the Communist Party, which endorsed an absolute ban on strikes. So unyielding was the CP on this matter that it opposed a strike waged by employees of Montgomery Ward's, a retail department store chain. Even the officialdom of labour backed this strike, because of the company's irrelevance to war-related production and its repressive labour policies. The CP, of course, did affect auto workers' views on the no-strike pledge and their propensity to strike. However, because the party concentrated on forging ties with the union bureaucracy, its influence was neither as direct nor as strong as it might have been had it been more in touch with ordinary workers.

The two principal Trotskyist organizations — the Socialist Workers Party and the Workers Party — opposed the no-strike pledge. However, there is little evidence that either party influenced to any appreciable degree the actions of rank-and-file auto workers. The main membership base of the WP was in New York City. The SWP was cautiously militant, especially in regard to strike action, because it wanted to protect the positions and clout of party members in the UAW. Like the CP, both of these organizations pursued an elitist strategy of seeking union offices and courting union bureaucrats. "It can be argued that this elitism, which dominated the SWP and the WP, as well as the CP, was more important than the obvious differences in concrete political policy in separating the militants of the left from rank-and-file workers and led to the loss of support and isolation from the working class of these organizations after the end of World War II."

Glaberman argues that the Mechanics Education Society of America and the United Mine Workers exerted more influence on workers than parties of the left. MESA, formed in 1932 to organize skilled workers in the midwest, was one of the few unions which refused to go along with the

no-strike pledge. "The vituperation directed at [MESA leader] Matt Smith by the Detroit newspapers for daring to lead his union in strikes... was unparalleled." (Smith's principled testimony to a Senate Sub-Committee investigating production in Detroit is intriguing.) A series of wildcat strikes in the coal fields, beginning in December 1942 and continuing sporadically throughout 1943, ultimately ended in a victory for the miners. These strikes made crystal clear the government's double standard of rigid wage controls and flexible price controls, undermined the prestige of the UAW leaders, who had openly opposed the miners' actions, and served as a model of defiance.

In 1944 there emerged within the UAW a movement to rescind the no-strike pledge. A rank-and-file caucus was formed, headed by militant local union leaders, a handful of whom were affiliated with the Trotskyist parties. Glaberman describes the proceedings of the 1944 UAW convention — the manoeuvring of diverse factions, the various majority and minority platforms, and the role of the rank-and-file caucus. The result was a resolution calling for a membership referendum on the no-strike pledge. In the ensuing February 1945 vote, the no-strike pledge was upheld by a more than two to one majority; however, 79 per cent of the workers did not bother to cast a ballot. The greatest opposition to the no-strike pledge came from metropolitan Detroit, the Flint-Lansing (Michigan) area, and Canada, "where because the government had not cooperated in maintaining union membership, the no-strike pledge had always been shaky."

Despite the presence of uniformed military officers in the plants; an anti-labour press which (in Detroit) published the names and addresses of strikers; company dismissals of militant stewards and committeemen; and the use of draft boards to get rid of militants, the number of strikes (*all illegal*) and strikers during the three years and eight months of the war were greater than in any other comparable

period of time. Half of the labour force in the auto industry was involved in strikes in 1944 and three-quarters in 1945. It is true that the 1945 rate was inflated by the escalation of strikes in the second half of 1945 (after the war had ended). But these figures probably err on the conservative side, since many in-plant work stoppages were not officially recorded.

Glaberman probes the implications of the referendum, particularly the contradiction of simultaneous attitudinal support for and behavioural defiance of the no-strike pledge. He maintains that apathy and political backwardness cannot be attributed to those who struck but did not vote in the referendum. On the contrary, they were undoubtedly the most militant category of workers. The refusal to vote reflects estrangement from and distrust of societal institutions, as well as a belief that significant change in their conditions of work and life are not likely to be altered through *any* institutionalized channels. Put differently, such "workers do not have sufficient loyalty to the institutions of this society (including 'their' institutions) to prevent them from abandoning those institutions in a revolutionary situation."

Glaberman is most interested in workers who favoured the no-strike pledge and also went on strike. This contradiction of attitudes and action (also reflected by white migrants from the South, who were the most patriotic and the most strike-prone auto workers), of course, is not unique to the wartime era. The simultaneous espousal and behavioural denial of hegemonic values is generally characteristic of the working classes of advanced capitalist nations. Glaberman infers that while attitudes and activity are interdependent, the former do not govern the latter. On this point a passage from Marx's *The Holy Family* is cited: "The question is not what this or that proletarian, or even the whole of the proletariat at the moment *considers* as its aim. The question is *what the proletariat is*, and what, consequent on that *being*, it will be compelled to do."

Because the purposes of production and the use to which labour power is put are determined by employers and their agents rather than the direct producers, the workplace is not organized to suit the inclinations and interests of workers; it is arranged to meet the employers' need to generate profits and accumulate capital. This leads to daily problems such as speed-up, onerous discipline, work rationalization, lay-offs, and unsafe and unhealthy work environments. This daily reality compels workers to act independent of and sometimes in contradiction to their own values. And in the course of acting, beliefs often are modified.

Glaberman concludes that when events such as these wartime strikes are interpreted objectively rather than from the point of view of the participants, the revolutionary potential of the working class is confirmed. "When thousands of workers are striking for a variety of ends, all of them local and narrow, the accumulation of strikes makes for a qualitative change in the objective reality. Objectively it is a threat to the existing social structure, no matter what the participants believe." Glaberman singles out two factors to account for the containment of the strike wave: the granting of fairly substantial concessions to workers and the incorporation of the residue of accumulated wartime discontents into the many official (hence controllable?) strikes which occurred immediately after the war ended.

This is a worthwhile book not only because it focusses on a relatively neglected slice of labour history but also because of the conclusions Glaberman draws from the jarring discrepancy between consciousness and activity manifested by workers in this era. Individuals who infer from surveys, elections, or other modes of attitudinal expression that the working classes of advanced capitalist nations are contented, apathetic, or politically backward will be obliged to rethink their views on this question.

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John Ehrenreich, ed., *The Cultural Crisis of Modern Medicine* (New York: Monthly Review Press 1978); and Susan Reverby and David Rosner, eds., *Health Care in America: Essays in Social History* (Philadelphia: Temple University Press 1979).

THE RAPID DEVELOPMENT OF medical science and its institutions has created an enormous health care industry and at the same time fundamentally altered the social experience and cultural perception of illness. John Ehrenreich's *The Cultural Crisis of Modern Medicine* reflects the growing criticism of this expansion; Susan Reverby's and David Rosner's *Health Care in America* searches the past for the complex sources and meaning of medicine's twentieth-century transformation.

In a stimulating introduction, John Ehrenreich outlines the sources and assumptions of the "cultural critique" of medical care, the approach which unifies the 12 diverse essays reprinted in *The Cultural Crisis of Modern Medicine*. Much liberal and radical criticism, Ehrenreich argues, has concentrated on the unequal distribution of health services, a focus which he characterizes as the "political economic critique." These critics share the implicit assumption that "... American medicine at its best is unquestionably beneficial," (2) and hold the common goal of extending access to services. In contrast, the cultural critique questions the efficacy of medical services, and assesses the quality of the social relationship between provider and patient. In probing the character and quality of health care, the cultural critique raises intriguing new questions and introduces troubling ambiguities into activist strategies.

The essays which follow overwhelmingly emphasize professional dominance and the cultural hegemony of medicine. In part one, "The Social Functions of Medicine: Some Theoretical Considerations," Barbara and John Ehrenreich

argue that medicine acts as social control both by overselling services to middle- and upper-class people and by withholding services from working-class and poor people. Irving Kenneth Zola considers the moralistic conceptions we hold of disease, and the ways these models reinforce medical dominance and an individualistic system of health care. In a departure from the theme of social control, Marc Renaud's essay "On the Structural Constraints to State Intervention in Health" discusses the epidemiology of disease in advanced capitalist societies and concludes that the sources of disease in the workplace and the environment cannot be eradicated under the present cultural assumptions and economic organization of health services. Next, two groups of related essays present specific examples of social control. "Medicine and Women: A Case Study in Social Control" includes five essays which dissect sexism with skill and wry humour. But all deal with women only as consumers of health services, and the section duplicates the medical obsession with women's sexual, maternal, and reproductive functions. "Medicine and Imperialism: Of You the Story Is Told" contains four chilling and effective analyses of medicine as an instrument of domination at home and in the Third World.

The essays document aspects of medicine as social control insightfully, and argue persuasively that the form and content of medical care as well as its distribution require profound restructuring. Lucid and effective, this view of medicine is nonetheless limited by its abstraction and generalization. The essays present medical care as monolithic, overlooking both the contradictions and conflicts within medicine itself and the unevenness of medicine's cultural hegemony. First, while the essays go well beyond the simple demand for more medical care, their analyses still proceed from a consumer consciousness, at times with the injured tone of those disillusioned with the discovery that science is not pure and doctors not

omnipotent. There is little appreciation for the special peculiarities of medical science and practice: for the objective uncertainty of many medical situations, for the dilemma of practitioners who confront human need with limited resources, for the conflict between researchers and practitioners within medicine, for the internal battles which accompany the rise and fall of medical orthodoxies. One does not have to end as an apologist for professional dominance to present a more closely observed view of medicine from the "inside," as indicated by such ethnographies as Marcia Millman's *The Unkindest Cut* or Renée Fox's and Judith P. Swazey's *The Courage to Fail*. Second, I think the authors overstate the reach of medicine's cultural hegemony. The focus on contemporary organized medicine and the many striking descriptions of medicine as social control make current practices seem timeless and absolute, obscuring the relatively recent legitimacy of "scientific medicine" and the history of vigorous resistance to medical professionalization. In arguing the case for medicine's cultural hold today, most of these authors assume that laypersons uncritically accept medical models, but present little convincing evidence for this. For example, in "Medicine and Social Control," Barbara and John Ehrenreich assert that lay expectations for medical care have escalated with new medical technology, and they support this claim by citing magazine articles which describe and promote medical innovations. (53, 75) From the evidence of such prescriptive literature, can we make arguments about the audience's response, much less conclude that popular resistance to medical models is "largely vestigial"? (70) It would be foolish to deny the influence of medical ideology on twentieth-century social life, but at the same time we need to consider the ways in which laypersons reinterpret, misunderstand, modify, or consciously reject current medical orthodoxies. Physicians continue to bemoan the high rate of "non-compliance"

among their patients, a curiously irrational response for a lay clientele utterly convinced of the efficacy of medical care. And as Ehrenreich acknowledges in his introduction, the last few years provide a remarkable number of examples of the recent revolt from medical control. The campaign for abortion and against sterilization abuse, the criticism of radical mastectomies in breast cancer therapy, the demand for woman-controlled obstetrics, the debate over the legalization of Laetrile, the growing activism for occupational safety, the broad politics of the anti-nuclear movement, the Nestlé's boycott with its exposure of multi-national industries' medical exploitation of the third world are part of the cultural critique that Ehrenreich describes. A closer analysis of the different assumptions and goals of these efforts would modify the grim impression of complete medical control and would advance our theoretical understanding of medicine as a complex and still contested social domain.

The historical perspective provided in *Health Care in America* enriches the cultural critique which Ehrenreich sets out. Susan Reverby's and David Rosner's historiographical introduction, "Beyond the Great Doctors," links the traditional narrative and interpretation of medical science to recent revisions which emphasize the social and cultural character of medicine. "The Public, Physicians, and Politicians: The Shifting Boundaries of Medical Care" offers four articles which illustrate the diversity of lay and medical perceptions of health care, from Lawrence Miller's account of the controversy over "twilight sleep" in obstetrics to Judith Walzer Leavitt's analysis of lay resistance to the Health Department in Milwaukee's small-pox epidemic. The next section, "Charity, Science and Class: The Institutions of Medicine" examines the social and economic transformation of hospitals from the nineteenth to the early twentieth century. Barbara G. Rosenkrantz and Maris A. Vinovskis examine different attitudes toward

mortality in two ante-bellum asylums, while Morris J. Vogel evokes the world of the nineteenth-century caretaking hospital. David Rosner analyzes early twentieth-century Brooklyn hospitals in the shift from charitable organization to modern business management. E. Richard Brown discusses medical foundations' influence on new conceptions of medical education and practice. "Doctors, Nurses, and Workers: Scientific Medicine and Scientific Management" contains three articles on the medical work force. Gerald E. Markowitz's and David Rosner's "Doctors in Crisis . . ." critically reinterprets the history of Progressive "reform" of medical education. The article fits logically into this section, but it might also have been placed with Brown's to emphasize their contrasting interpretations: Markowitz and Rosner suggest that medical professionalization, attained through the sponsorship of powerful foundations, enhanced physicians' power, while Brown argues that the intervention of the foundations placed new limits on physicians' autonomy. Two other articles provide rare and useful insights into other members of the hospital work force. In a provocative article on nursing and hospital rationalization, Susan Reverby asserts that nurses themselves were largely responsible for the growing division of labour among graduate nurses, practical nurses, aides, and orderlies. Leon Fink and Brian Greenberg interpret the sources and outcome of Local 1199's successful campaign at New York's Montefiore Hospital, offering a perceptive analysis of union strategies and management ideology in the special environment of an urban hospital.

In a thoughtful concluding essay, Robert Crawford addresses broad themes of contemporary health politics, criticizing the self-help movement as a retreat from demands for greater professional and corporate accountability and as a cost-cutting alternative to more threatening demands for workplace and environmental safety. Crawford argues convincingly that self-

help is ultimately "the politics of diversion," (258) although I think he underestimates its potential strength as a challenge to the cultural legitimacy of medicine and the professional dominance of physicians.

These two anthologies complement one another well and taken together, offer an exciting overview of ongoing revisions in the sociology and history of medicine. Ehrenreich's *Cultural Crisis* presents sweeping, sometimes contentious, always stimulating theoretical essays on modern medicine. In *Health Care in America*, Reverby and Rosner have collected historical articles into an unusually coherent and well-integrated anthology: the more contained scope of these articles permits vivid observation and closer analysis of a variety of relevant subjects in the social history of medicine.

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Bernard Capp, *English Almanacs 1500-1800: Astrology and the Popular Press* (Ithaca: Cornell University Press 1979).

IN *RELIGION AND THE Decline of Magic* Keith Thomas doubted whether all the divining agencies of the seventeenth century ever "swelled to fill the gap left by the confessional and the saints," but he had no doubt that astrology underwent a massive boom after the Reformation and that the profusion of almanacs and astrological guides that came into circulation was without precedent. Bernard Capp's book is the first to study in detail the diverse contents of these guides. Along with tracing the development of astrology and the publishing history of almanacs in early modern Europe, he assesses the guides' contemporary political, social, religious, literary, scientific, and educational relevance and impact. His modest ambition is to persuade us that the almanacs constitute a neglected yet valuable source in the study of all of these fields.

Certainly Capp's examination of the contents of the various kinds of English almanacs is thorough, especially for the seventeenth century. Continually one is impressed by his tremendous research efforts and, partly as a result of these, the way in which he has been able to extract from the almanacs the most appropriate and lively evidence in support of any particular point. It is doubtful, however, if any historian of the early modern period will now feel embarrassed about the relative neglect of these sources, over 1,000 of which Capp has gathered into a convenient appended bibliography. To the insights on seventeenth-century life and thought provided by Thomas, Christopher Hill, Lawrence Stone, Charles Webster, Margaret and James Jacob and other historians of high calibre, Capp offers little that is new and nothing that is challenging. In fact, with the exception of his chapter on the eighteenth century (which he admits was an afterthought), Capp hardly goes beyond the points raised and the conclusions reached in the hundred or so pages devoted to astrology and almanacs in Thomas' *Religion and the Decline of Magic*. Capp is fully conscious of the debt to Thomas but he is unconstrained by it, for his purpose is not to present a novel contextual reinterpretation of his subject. The historical merit of *English Almanacs* rests rather upon its confirming and adding texture to what is already known — to what is known, for example, of conventional habits, prejudices and attitudes toward diet, sex, family life, violence, lunacy, medicine, the multitude, social rebellion, and so on. On the concept of work, for instance, Capp nods appreciatively to the writings of Thompson and Hill in his observation that the almanacs seldom mirrored the ascetic life-style associated by Weber with early Protestantism. (118)

While it is of course interesting and significant that through the almanacs Capp confirms much historical opinion, it is hard to overcome the feeling that this confirmation would not have been as great had he

been guided less by the existing literature and had he not selected from the almanacs mainly those points which other historians have earmarked as worthy of elaboration. On the other hand, Capp's volume might have been far more intellectually stimulating had he chosen to be more explicit about his catalogue of historical wisdoms and related historiographical issues, and then used the contents of the almanacs more programmatically to address and explore these topics. Instead he makes only oblique reference to such concerns and quickly moves on. For instance, in allusion to the central historiographical problem raised by the discussion of the massive sales of some almanacs, namely the problem of the power of ideas thus disseminated, Capp merely remarks with typical candour and brevity that although many persons in authority considered the propaganda issued through the almanacs to be effective, it "is impossible to measure the success of such propaganda." (287)

In effect, because Capp has preferred mainly to detail the contents of these guides and chronicle their history, rather than use them to enter into historical and historiographical reflection, *English Almanacs* is itself far more a kind of historical almanac to the almanacs than a critical study of them in relation to their contexts of elaboration and use. Only after documenting the various changes in the form and purpose of the Hanoverian almanac relative to its Stuart forebear — the most substantial difference being the demise of astrological content — does Capp in his conclusions begin to consider the wider temporal significances of the almanacs. It is a tidy summation, again reminding us of the extent to which "Capp's almanac" has (like some of the tamer ones it discusses) assimilated within it, historically speaking, most current interests and most conventional wisdoms. Indeed, the book might usefully be read as an introduction to present priorities and concerns in the history of early modern England. *English Almanacs* doubtless will be primarily con-

sulted, however, as a reference work on almanacs, thus appropriately allowing it to share one of the main functions of the publications it describes: practical utility.

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Donald H. Akenson, *Between Two Revolutions: Islandmagee, County Antrim 1798-1920* (Port Credit, Ontario: Meany 1979).

IN THIS DELIGHTFUL LITTLE BOOK, Donald Akenson describes and analyses social life during the nineteenth century in the small community of Islandmagee, which is situated on the northeast coast of Ireland. Objectively and yet sympathetically, Dr. Akenson has collected a wide range of material on the island, its people, and their way of life, and has skillfully woven it together in an account that is both interesting and valuable. He claims, with considerable justification, that the things he describes in this book were far more important to the Islanders than the "high culture" and "high politics" to which the great majority of Irish historical writing is devoted. Although he occasionally exaggerates the point, he is essentially correct. Ordinary people — including historians and sociologists — are typically more involved in relations with their friends and relatives, and in coping with their house, their work, their taxes, and other immediate problems than they are with national and international questions.

In separate parts of the book, he describes the physical structure of the island, its architecture, and the modes of communication available to the people. In an excellent chapter on the economy, he indicates that most Islanders were reasonably well off, and that they had no burning desire to own the land, since they believed their tenancies at moderate rents were as secure as peasant proprietorship. He has

several good chapters on culture and social institutions. In spite of his insistence that social institutions were not very important to the people, he provides an interesting and perceptive analysis of their role in the community.

The only disappointment in the book is that there are few personal histories. For reasons that I fully understand, Dr. Akenson was obviously hesitant to say much about individuals who could easily be identified, and only rarely does he illustrate his arguments by reference to specific persons. The unfortunate result is that, notwithstanding the efforts he makes to portray the day-to-day life of the people, it is hard for the reader to get a feel for what the Islanders were really like.

For the sake of argument, I could also quarrel with several minor claims he makes. First, I am skeptical when he says that the Islanders did not become politicized until 1912-14. Although one can certainly identify periods of rapid mobilization, in my experience politicization occurs more gradually than his argument assumes. Secondly, I question his claim that no Islanders had the vote in parliamentary elections before 1850; elsewhere in Ireland the definition of freeholder was interpreted loosely enough that at least some tenants were able to vote.

And finally, I think he has misjudged the effect of the Great Famine on demographic patterns. He argues that in other parts of Ireland the Famine taught people a lesson about the dangers of high fertility, with the result that they began to restrict reproduction in the post-famine period; he contrasts this with farm families on Islandmagee where reproduction was not excessive either before the Famine or after it. More probable is the argument that in almost all parts of the country medium and large-size farmers (like those on Islandmagee) controlled their fertility both before and after the Famine. The people who reproduced heavily before the Famine were small farmers, cottiers, and labourers; and the post-famine decline in Irish

fertility was primarily a consequence of the sharp decline in the size of these social classes. It is highly unlikely that anyone consciously stopped breeding because of the Famine.

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Lynn Hollen Lees, *Exiles of Erin. Irish Migrants in Victorian London* (Ithaca: Cornell University Press 1979).

WORK UPON THE HISTORY of migrant groups is amongst the hardest of all historical research to do: neither country of origin nor of settlement collects records with an eye to anything but its own convenience, and rarely is there easy compatibility of data as between the two separate nations involved. Thus, historians usually have taken the easier (but not necessarily easy) route and written about the characteristics of emigrants from one country or about the nature of immigrants into another. Tight, trustworthy studies of the cultural transfer from one nation to another effected by migrant populations are heroic and rare.

Lynn Lees' study attempts to relate general information about the mid-nineteenth century social structure in Ireland to a bank of data on the Irish in London and if she is not always successful, the book is so good that one learns almost as much from her failures as from her successes. The style of the volume is a joy. Professor Lees writes with a mixture of vividness and of easy erudition that can only be envied. Moreover, she reads other people's material well. Time and time again she provides apt, economic summaries of relevant scholarly articles which not only do justice to the writer — but better — put his point in sharper and clearer relief than he was himself able to achieve.

In this book Professor Lees makes three points forcefully and convincingly. The first is that viewing Irish migration in terms of a Malthusian exodus is wrong.

(Historians of nineteenth-century Canada would do well to pay attention to this point). She argues that simple economic factors did not decide who migrated, but that cultural ones were enormously important. Moreover, it was not the poorest who left, for they could not afford to go. People were not forced to leave, bad as things were; they rationally chose to go. Second, Lees emphasizes that in the case of the London Irish, they did not merely wash up in the metropolis, but arrived as the result of volitional choice of destination on their part. Given a predisposition to emigrate from Ireland, those potential migrants with children and with limited resources were more apt to choose to travel to England than to North America. Women tended to choose London in large numbers, and in fact women outnumbered men amongst the London Irish.

Given that the Irish migration should be viewed as a rational process, mediated by strong cultural influences, it is not at all surprising that the Irish communities in London achieved a marked cultural coherence. This leads to Professor Lees' third major point, namely, that the Irish in London both perpetuated and adapted their culture with considerable resilience. She argues forcibly against the conventional wisdom which stresses the potential disorganization and alienation sometimes experienced by ethnic minorities in urban settings. Lees suggests that the nuclear composition of the Irish family did not change as a result of the migration to the metropolis. With this domestic stability the Irish sub-culture survived with remarkable endurance, providing not only a cultural backdrop of intrinsic merit for its members, but a means of solidarity which enabled the migrant Irish to cope successfully with their new environment.

Given the merit of Professor Lees' main points and the strength of the evidence supporting them, one has to add that the filigree of her argument concerning specific changes in the London Irish over time is misdrawn. The reason for this is

that she does not recognize that brute fact of the Great Famine dictated that she use a different data base than the one she in fact employed. Put simply, the Famine re-organized Irish familial patterns. Although the family structure remained nuclear, the abandonment of sub-division of land put greater economic and therefore familial power in the hands of male heads of household and, further, condemned a large proportion of the population to life-long celibacy, and, those who did marry tended to remain single much longer than had their counterparts before the famine. Moreover the power of the Roman Catholic church in the society was greatly strengthened, partly in structural terms and partly because of the "devotional revolution" that followed the Famine.

The trouble is that Lees' data base is drawn only from the years 1851 and 1861. The sample itself is quite adequate, consisting of roughly 4,000 individuals in each census year, chosen from amongst Irish households in five Irish neighbourhoods in London. The sample seems sound in itself, but, unhappily, does not distinguish between those Irish who arrived in London prior to the Famine and those who came after. This omission is serious, as it is impossible to tell whether certain characteristics of the London Irish developed because of their experience in London, or actually were the result of the dilution of a London Irish population that was pre-Famine in origin with one whose background was from the post-Famine years. Emphatically, this problem does not affect the validity of Professor Lees' main conclusions, but does prevent her from drawing the fine shadings that one would like. Either she should have distinguished in her sample as to year of arrival in London, or, if this was impossible, should have provided a baseline sample of the London Irish derived from the 1841 census perforce.

It would be inappropriate to end a review of this book on anything but a positive note. Lynn Lees has given us not only

a fine volume in itself, but has shown the way for other scholars to engage the larger question of the Irish throughout England. The next step should be for someone to do a tight distributional analysis of Irish-born and Irish-descended persons throughout England over the entire nineteenth century. And some day, some one must begin the heroic task of actually tracing the migration patterns of a large sample of individual Irishmen from their home parishes to their several successive destinations across the Irish sea.

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Norman Longmate, *The Hungry Mills: The Story of the Lancashire Cotton Famine 1861-5* (London: Temple Smith 1978), and Francis Hearn, *Domination, Legitimation and Resistance. The Incorporation of the Nineteenth-Century English Working Class* (Westport, Ct.: Greenwood Press 1978).

THESE TWO STUDIES, though far removed from each other in style and theoretical pretension, have two common characteristics — their base in published rather than manuscript sources and their concern with the question of social control.

Norman Longmate's book, based on pamphlets, sermons, memoirs, and diaries of his period, is written in a light journalistic tone for popular as well as scholarly consumption. This account of the American Civil War era, although frankly descriptive, derives uniqueness and unity from its stress on a commodity — King Cotton — as the dominant clue to the politics and economic development of a period, transcending national boundaries. It ranges from the growing of the cotton in the slave states of the U.S. through the excitement of its marketing on Liverpool Exchange to the factories and towns of Lancashire where an industrial revolution built on its manufacture into cloth had created a new kind of labour concentration

of skilled wage-workers. It even touches (with tantalizing incompleteness) upon the wartime speculation over the possibilities for developing cotton-growing — and slavery — in India.

Its chief interest, however, lies in its study of the social control exercised over the English factory workers during the crisis of unemployment and extreme privation caused by the "cotton famine" of 1861-66. While various relief committees sought to dominate the situation by their own book of rules, and while northern manufacturers feared the intrusion into their own domain of London-based aristocratic politicians, all were agreed that the population must be retained on the spot during the crisis (emigration was not a solution, for this specialized labour community) and that somehow they must be restrained from a revival of hunger-rioting or Chartist militancy. The Relief Committee which finally took the lead under Lord Derby (both Liverpool land-owner and ex-Conservative Prime Minister) emerges as strong, centralized, and apparently efficient; it expended all the funds delegated to it, showed flexibility on the crucial question of out-relief, and averted starvation, depopulation, and popular revolt.

The eminence of the Committee's leaders and the public emphasis given to its work shows the extent to which militant protest was feared. No such danger materialized, however; the Lancashire working class did refuse to countenance the cutting of wage-rates but they in fact financed the depression to a considerable extent from their own small savings by supporting themselves in unemployment, while their anger was effectually contained by the relief distributions, by the sympathetic propaganda which recognized the guiltlessness of their idleness, and by the trade-and-reading-schools and the sewing-circles organized to occupy their time. The account of these schools makes fascinating reading. Many of the philanthropic middle class organizers who taught needlework to mill-girls and handcrafts to

power-loom operatives sought to impart their own (equally anachronistic) attitudes and values at the same time, though few perhaps equalled Sir James Kay-Shuttleworth, who lectured regularly at his own relief-centre on the wickedness of strikes and trade unions and prayed God "to teach working men to have more moderate and intelligent notions. . . ."

Mr. Longmate recognizes that the Declaration of Emancipation of Slaves was an important factor in inclining the sympathies of the "respectable" Lancashire working class to the Northern States. He is less clear about the role played by promise of the parliamentary franchise to this class as the "carrot" in the whole exercise in social containment which the discerning reader will perceive as the main theme of his book.

The imposition of social control on the English working class is also the theme, much more explicitly, of Francis Hearn's work, which is an overview by an American sociologist of important recent work in the field of British labour history in order to illustrate a sociological hypothesis based on Marcuse and Habermas.

Seeking to explain why the world's original industrial workforce, at first so turbulent, had been tamed down by the 1850s, he fastens upon the late 1830s as the crucial period of cultural transition. Before this period, he maintains, the working class was sustained by the memory of their traditional rights and by a popular culture expressive of them conveyed in song and story, fairs and frolics, pastime and holiday. This gave them a basis from which to criticize their new masters, the manufacturers, who were disinclined to accept the social obligation, previously incumbent upon the great, to protect and shelter the poor. As industrial production advanced, the old cultural tradition came under attack and was eliminated as wasteful of time and serious effort. The new rationality made a sharp distinction between work-time and play-time, turning the working world into one of sober responsible endeavour,

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trivializing play and relegating it to the world of the child and the immature adult. Working people thus lost the support given by the popular culture enshrined in their playful activities and were absorbed into Marcusean one-dimensionality, unable to criticize the capitalist world which dominated their lives. Responsibility of the individual for his own welfare was accepted and enshrined in rational self-help organizations such as consumer co-operatives and the new model unions which crystallized around a labour aristocracy hopelessly co-opted into the middle class ethic, while the labouring masses were disoriented and deprived of any culture at all.

Few historians probably will be inclined to follow this argument all the way, or to agree that sociological theory can create for the present generation an "open area" for the creative imagination such as that afforded by non-conformist chapels or occupational communities in the past. But, in his pursuit of proof, Prof. Hearn has rendered a most valuable service to historians, namely a descriptive synthesis of the work going on in the field of English labour history over the last two decades. Careful attention is paid both to accounts of the various social theories involved and to the detailed studies in an effort to create the "macro-societal connections" which make them more comprehensible to social theorists.

In such an exercise it is inevitable that every historian will find points for individual disagreement. For instance, in emphasizing the thoroughness with which the working day has been purged of playful and humorous content Prof. Hearn has been influenced too much by his knowledge of twentieth-century (Marcusean) America. In England there was and still is a much greater survival of such content. It is precisely this survival of idiosyncratic work practice and occupation-linked folklore which has inspired a good deal of the new social history.

A greater weakness in Prof. Hearn's

argument is one which he avoids by ending his analysis where he does in 1867. If the British working class had no community tradition left as a critical dimension by 1850, how does one account for the many-faceted revival of socialism in the 1890s and for the renewed militancy of the unskilled in the period up to 1914? This would be difficult to answer without reverting to economic categories of explanation, without considering how particular leadership groups in the workforce (such as the "labour aristocracy" of the 1850s) are in fact thrown up by an ever-changing economy. This whole issue is obscured by skipping on to "the workers of advanced industrial society," globally and contemporaneously conceived.

Nevertheless this work deals with a widely-acknowledged gap in the Marxist system of explanation (the function of play in balance with the function of labour) and at the same time provides a useful reference guide to the working historian, who may benefit by the orientation which it gives within a growing field of historical work.

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Kenneth J. Logue, *Popular Disturbances in Scotland 1780-1815* (Edinburgh: John Donald 1979); Norman Murray, *The Scottish Hand Loom Weavers: A Social History* (Edinburgh: John Donald 1978); Ian Levitt and Christopher Smout, *The State of the Scottish Working Class in 1843* (Edinburgh: Scottish Academic Press 1979); A. Allan MacLaren, ed., *Social Class in Scotland: Past and Present* (Edinburgh: John Donald 1976); and James D. Young, *The Rousing of the Scottish Working Class* (Montreal: McGill-Queen's University Press 1979).

THE FIVE BOOKS reviewed in this article demonstrate both the increased interest in Scottish labour history and the different approaches to the subject. As to the rea-

sons for this interest, it can only be suggested that the exploitation of North Sea oil, the rise of a Scottish National Party, and the recent referendum on the devolution of political power have stimulated academic researches into Scotland's past.

Kenneth Logue's study of Scottish popular disturbances focuses on a period which he identifies as a "watershed between pre-industrial and industrial society." He writes of these early conflicts largely from the perspectives pioneered by George Rudé and Edward Thompson, and, his conclusions do not differ substantially from theirs. He finds that Scottish crowds were composed of rather ordinary people and not a rabble of transient or criminal elements. Women played key roles in some disturbances; crowds asserted a "moral economy" in food riots against the imposition of a market economy on society; and, rioters were more violent against property than persons. There is some evidence of popular resistance to the Highland clearances and, after the outbreak of the French Revolution, increasing evidence of a radical political consciousness on the part of the common people. The widely-held belief that the Scots are a particularly martial people is called into question by the numerous anti-militia and anti-recruitment riots which occurred after the passing of the Scottish Militia Act in 1797. As might be expected for this period, industrial disturbances are rare and often confused with other issues. In contrast to Rudé's studies of French crowds, Logue finds that the Scottish crowds had a tendency to produce leaders from within their own ranks.

The main problem with this study is that it is unnecessarily restricted. We need to examine a longer chronological span to appreciate fully the author's contention that his period is indeed a watershed. We need some examples of Scottish disturbances from the post-Napoleonic War era, as well as a fuller discussion of some earlier eighteenth-century riots. In the case of the latter, there are some good studies

(including some work by the author) which could have been used to extend the data base beyond the 450 individuals identified as rioters in the years 1780 to 1815. The study is confined in another way as it is limited to those popular disturbances in which at least one person was charged with "mobbing and rioting." Possibly there were other disturbances which did not result in charges.

A final technical comment is required on the author's tendency to use the words "crowd" and "mob" as synonyms. Without any explanation he ignores the conventions established by George Rudé in his books *The Crowd in the French Revolution* (1959) and *The Crowd in History* (1964). Rudé suggested that crowds acted more or less in their own interest, while mobs were "hired bands" acting on behalf of other persons.

The handloom weavers are usually remembered in labour history as the earliest victims of technological redundancy. Yet, as Norman Murray explains in his well-researched study of the Scottish handloom weavers, their decline precedes the introduction of the power loom. In his first four chapters he traces the cyclical trends in the trade, the decline in real wages and the challenge of the power loom in the 1840s. The number of handloom weavers grew rapidly in the Scottish industrial revolution to reach a peak around 85,000 in 1838 and then their numbers fell off to perhaps 25,000 in 1850. The author argues that the majority of weavers were recruited from Lowlands Scotland and that it was the minority which came from the Highlands or Ireland.

In chapter five there is an interesting attempt to study the extent of poverty among the Scottish handloom weavers using the concepts of the twentieth-century social investigator, Seebohm Rowntree. Unfortunately the exercise is not entirely successful. The distinction between "primary poverty," resulting from an income below the poverty line, and "secondary poverty," resulting from mismanagement

of family budgets, is difficult to establish. The handloom weavers were simply too poor in the declining years of their trade for the historian to make this distinction. In 1834, for example, half are thought to have lived in a state of primary poverty. Given that harsh statistic, is it possible to separate out those who were poor through indebtedness to pawnbrokers, creditors, and employers, or those who took to drink?

The story of the handloom weavers is one of longer hours of work and lower and lower wages. This had a disastrous side effect on the social life of the weavers. They had less and less time for leisure activities, education, and religious observances. Further, their efforts to halt the process through trade-union activity was not successful in the long run. The author rightly attributes this to the "widespread dispersal and heterogeneous nature of the trade" rather than poor leadership, or a hostile legal environment, or the Irish component of the workforce. It was simply too difficult to organize and discipline a workforce which was scattered over towns and villages in small workshops rather than in factories.

At the end of the book the author considers the heavy involvement of the Scottish weavers in the radical movements of the years 1790 to 1850. Here the author is too quickly satisfied to inform us that the economic distress of the weavers led them into extremist politics and that political activism can be related to economic cycles. The explanation offered leans too heavily on a crude economic determinism and begs further questions. How did the weavers interpret their situation? Did they have an explanation for exploitation? And did they have a method to calculate the value of their labour? Indeed would a concept of alienation be appropriate to an analysis of the putting out system of production?

In *The State of the Scottish Working Class in 1843*, Ian Levitt and Christopher Smout have attempted a rather novel approach to social history. They have

taken the data from an extensive parish by parish survey of Scotland which was part of the *Report of the Royal Commission on the Poor Law (Scotland)* and analysed it with the aid of a computer. The authors explain their methods and the obstacles encountered in their research. In particular, there are no replies to the survey from Edinburgh, Glasgow, Paisley, and Perth. To a certain extent they have been able to overcome this difficulty by reference to census and taxation data. The result is a mass of statistics, tabulations, and computer print-out maps on everything from dietary patterns, to wages and unemployment. There is also a chapter on emigration which should be of interest to Canadian historians. The main conclusion the authors derive from their study is that regional economic social differences were accentuated during the industrial revolution. They identify a "zone of prosperity" in central Scotland and a "zone of poverty" in the Highlands. They contend that these differences were not so evident in 1750 and that they have subsided to a degree in the twentieth century. While most historians would accept these general statements, it must be remembered that we will not be able to verify them in quantitative terms for the mid-eighteenth century as we can for the year 1843.

The volume edited and introduced by A.A. MacLaren addresses itself to the broad question of social class in Scotland. Enid Gaudie traces the growth of residential segregation by class and MacLaren deals with middle class attempts to handle the cholera outbreaks in nineteenth-century Scotland. Christopher Smout finds differences between the middle and working classes in terms of sexual behaviour. Robert Gray uses a concept of labour aristocracy to argue that skilled workers in Victorian Edinburgh could practice thrift and claim to be "respectable," while still retaining a "sense of class identity." Malcolm Gray's study of the agricultural labour force in the north-east in the nineteenth century reveals the persistence

of the small landholder. Class divisions were mitigated by a life cycle pattern which allowed some young landless servants and labourers to become "crofters" when they grew older. Indeed, as Ian Carter argues in another contribution, the north-east retained many of the features of a peasant society through the nineteenth century. The main thrust of the various contributions to this volume is that Scotland was not less class-ridden or more egalitarian than England. At the same time it is maintained that Scotland's class structure is different from that of England. We must note, however, that this conclusion is weakened by the lack of a solid study of the Clyde area or trade unions in this collection of nine essays.

James Young's book is an attempt on the part of a committed socialist and nationalist to interpret the whole of Scottish working-class history from 1770 to 1931. It is also a deeply flawed work. There is a lack of subtlety in this book as the author has made no attempt to bring the reader around to his point of view. Instead we are subjected to a bombardment of ill-defined expressions such as "assertive metropolitan capitalism," "British bourgeois ideology," "cultural imperialism," and "Scottish plebeian radicals." Much of the book consists of undigested conceptual borrowings which are simply pasted on to the Scottish experience. The result is a series of extraordinary and harsh assertions. The government of Scotland of the 1790s is denounced as a "totalitarian" or "Asiatic despotism." (47 and 67) The industrialization of Scotland is viewed as a case of "cultural genocide" for the lower orders, (18) while the Highland clearances are dealt with as an example of plain "genocide." (94) Again and again the author repeats his claim that the Scottish working class is unique for its "inarticulacy, linguistic insecurity, drunkenness, women's oppression, sexual repression, the heterogeneity of the pre-industrial labour groups and the problems of Highland emigration. . . ." (23, 25, 98, 109,

134, 138-40, etc.) Yet these negative features made Scottish workers the most progressive in Europe. (25)

The treatment of historical sources is both loose and subjective in this book. For example, twice the author asserts that one aim of the rising of 1820 was to seize the property of the rich. (44-45) The manifesto issued by these same rebels is not cited by Young and it states, "That the protection of the Life and Property of the Rich Man is in the interest of the Poor Man. . . ." Instead the author refers us to a volume of poems published in 1885 as the supporting evidence for his interpretation. (67 n. 21) There is no attempt to reconcile the two sources and this raises serious questions about the quality of the scholarship in this book. There are some flashes of insight, but even sympathetic colleagues will find that the author has failed to measure up to his subject.

Some general remarks are required on Scottish labour history in view of the recent outburst of publications in the field. In the first place it should be noted that many works on Scottish history assume that Scotland is both a political and a cultural entity. The traditional Highland-Lowland dichotomy appears to be out of fashion at present and emphasis is placed on both the uniformity and distinctiveness of Scotland. This is particularly true of James Young's book which puts the case at every opportunity. Labour historians, however, are sensitive to the broad affinities between the workers of Lowland Scotland and those of Yorkshire, Lancashire, and South Wales which cut across national boundaries. At the same time the Highlands have remained largely outside the traditions and movements associated with an emergent working class. A glance at the maps produced by Levitt and Smout would lend support to this view. If Scottish labour historians are going to assert a nationalistic interpretation of their history, then the case will have to rest on solid foundations rather than mere assumptions. In the second place, in the general study of

British labour history the distinction between the working class which emerged in the early years of industrialization and that which was fashioned out of later developments is a matter of great concern. The period roughly 1850 to 1880 can be seen as one of transition between two very different working classes. We must determine whether or not the same patterns emerged in Scotland.

The flurry of activity in the field of Scottish labour history has resulted in a number of almost simultaneous publications. The various authors and contributors whose works are reviewed here, for the most part, have not been able to take account of each other's work. We still await a broad work of interpretation which will make use of these studies to provide us with a history of the Scottish working class.

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Frances Finnegan, *Poverty and Prostitution: A Study of Victorian Prostitution in York* (Cambridge: Cambridge University Press 1979).

IN MAY 1871 Margaret Barrett appeared in a York courtroom where she was found guilty of "indecentcy" and sentenced to seven days imprisonment. Within a week of serving this sentence, she was returned to prison for a further month for the same offence. These were only two of fifty-three appearances before the bench by Margaret Barrett throughout more than thirty years as a prostitute, during which time York Castle prison was the closest thing she had to a permanent residence (her cumulative prison sentences totalled more than nine years). She apparently went on the "game" at about fourteen years of age and was soon described as "perfectly incorrigible," a habitué of the poorest, most unsanitary, and notorious streets in York, a frequent drunkard, a vagrant, an occasion-

al thief, and, almost inevitably, a syphilitic. Although her biography is considerably more detailed than most, Margaret Barrett's life and circumstances are reasonably representative of the more than 1,000 individual prostitutes and brothel-keepers operating in York between 1837 and 1887 who are identified by Frances Finnegan from local newspapers, lists of Quarter Sessions, Poor Law application books, and the records of the York Penitentiary Society. In this book Finnegan sets out to examine the prostitutes themselves, to identify their haunts, habits, health, and clients, and thereby to question "the validity of much that has been written on Victorian prostitution in general."

The author is particularly concerned to refute some of the conclusions drawn by Dr. William Acton in his famous studies of prostitution (whose influence on recent historical studies of the subject she over-emphasizes to some extent). Certainly Finnegan's evidence convincingly refutes Acton's notion that prostitutes had "iron bodies," healthier and more resistant to disease than those of her more respectable sisters; that most prostitutes sooner or later turned to a more "regular course of life;" or that "vanity, idleness, love of dress [or] love of excitement" figured significantly in the motives behind a woman's choice of prostitution as a means of livelihood. On the other hand, the findings of this study do confirm Acton's statement that "by far the larger proportion are driven to evil courses by cruel biting poverty." They also show that what most often distinguished the young prostitute from other poor women was a disturbed, neglected childhood and the absence of a family support system. A large number of prostitutes were also reported to be of inferior intellect or mentally defective. One wonders whether many women labelled as mental defectives were in reality so socially deprived as to appear defective, and consequently were so ignorant of everyday life as to be unable to hold even the lowest of domestic service jobs: an observation often made about girls

brought up in workhouses. Finnegan makes no mention of workhouse girls but the Seventh Annual Report of the Poor Law Commissioners estimated that in 1841 at least one-third of the lowest and coarsest prostitutes of London had been brought up in the workhouse. Finnegan also tries to undermine what she sees as the conventional class interpretation of Victorian prostitution by investigating the prostitute's clients. From the limited evidence presented (drawn in the main from newspaper reports of court charges pressed against prostitutes by their clients, usually for theft), the author indicates that the majority of these women's clients were drawn from their own class and that prostitutes were often the exploiters as well as the exploited (through thefts from gormless young servants and agricultural workers attending hiring fairs with half a year's wages in their pockets, or from merchants and farmers financially well-padded from transactions at the market). The evidence, however, has some biases. Information culled from Poor Law records and police reports inevitably emphasizes the poorest street-walkers, while the understandable middle class reluctance to face the publicity resulting from pressing charges probably results in an underestimate of the numbers of middle class men resorting to lower class prostitutes. On the other hand, the presence of the garrison in York undoubtedly did ensure a large clientele of working-class origin (while a garrison town, York was not included under the Contagious Diseases Act). But whatever the class dynamics that informed these relationships (and seduction by middle class men seems, in fact, to have been a minor cause of prostitution), it is clear that what needs primary consideration, as Finnegan points out, is the broader social and economic exploitation which created the misery, underemployment, and hunger that drove some women into prostitution.

While Finnegan mentions the work of Acton, Tait, and Logan, the reader is surprised to find no mention of G.P. Mer-

rick's *Work Among the Fallen*, the Victorian study which most closely parallels her own. This study, based on a longitudinal investigation of the lowest class of prostitutes and considered by some historians to be the best statistical survey of English prostitution, also emphasizes the social isolation and constant economic pressure to be found among prostitutes and points to a strikingly similar pattern of previous occupations (heavily biased towards domestic service). Absent also is any mention of two solid recent theses on prostitution by H.R.E. Ware and J.R. Walkowitz, and the articles based on the latter's work. More serious is the repetitiveness and lack of tight organization in parts of the book. One indication of this looseness is that in a single chapter of about 23 pages of text the reader is reminded six times that the points considered have been mentioned before and a further nine times that issues will be discussed in further detail in later chapters. Worrisome also is the fact that in a work based substantially on computer analysis there is not a single table or chart; it would have been useful to have been able to see at a glance how many of the prostitutes were mentioned in the sources on several or numerous occasions (as in Barrett's case), and how many only once or twice.

This work is strongest in its discussion of the York Penitentiary Refuge, with its fierce indoctrination of shame and guilt, its harsh two-year régime based on long hours at laundry work — a literal washing away of sins which overtaxed the strength of many inmates — and its lack of success in reforming even the women who were specially selected as hopeful cases (most of the inmates were teenagers and no woman who was diseased, pregnant, or particularly "hardened" was admitted). And yet, despite its bleakness, discipline, and monotonous routine, the Refuge provided for many girls the only home, the only education, and the only security they had ever known. A measure of the bleak lives of York's street-walkers may be found in the fact that almost half of the recorded women

were sufficiently anxious to escape from prostitution that they sought shelter in the Refuge. And, perhaps most telling of all, is the evidence recorded of a few former inmates writing to the matron as the only person in the world who might be concerned about their fate, and of others who, in the absence of friends and relations, sometimes returned to the Refuge for their holidays, having nowhere else to go.

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David Roberts, *Paternalism in Early Victorian England* (New Brunswick, N.J.: University Press 1979).

BY THE 1830S AND 1840S, not only had England's economic structure been transformed by industrial capitalism, but a substantial number of people had become acutely aware of the social consequences of this economic transformation. It was in these decades that the most significant nineteenth-century social theories first received widespread attention. Among the solutions that were proposed as remedies for the negative social effects of industrial capitalism was a return to the patriarchal social relationships of the past: a return to a hierarchical social structure in which those with wealth and authority would protect the poor, and the poor would, in return, give respect and deference to the rich. While this solution appears in retrospect unworkable, such longings for the past were widespread among upper-class people, and at times even manifested themselves in working-class political thought.

It is an analysis of this particular solution to the problems of industrial capitalism that is the subject of David Roberts' book, *Paternalism in Early Victorian England*. "Paternalism" is the term Roberts uses to characterize the social outlook that has as its fundamental precept a belief that authoritarian and hierarchical social arrangements will provide the most

satisfactory solution to social problems. While he acknowledges that the set of ideas that he is identifying as "paternalist" is amorphous, and while he points out that the term itself was never used during the 1830s and 1840s — the period with which he is primarily concerned — nonetheless, it is his contention that the ideology of the ruling class as a whole can be characterized as paternalistic during the early Victorian period.

Roberts first develops an analysis of paternalism, and then goes on to trace the way in which it was manifested by a number of groups, including novelists and journalists; aristocrats, country squires, clergymen, and captains of industry who were actively engaged in decision-making at a local level; and Members of Parliament. In the early chapters of the book, Roberts develops a definition of paternalism, a definition that he hopes will be all-encompassing enough to support the claim that a belief in paternalist ideas can indeed be said to characterize a wide spectrum of members of the ruling class. Paternalists, Roberts maintains, held "four basic assumptions about the structure of society": they all believed that society should be "authoritarian, hierarchic, organic and pluralistic." Furthermore, they believed that those in authority had "three principal sets of duties," namely, "ruling, guiding and helping."

Having developed this basic definition, Roberts goes on to analyze the historical development of paternalism. It was in medieval and Tudor times, when the social structure was genuinely patriarchal, that paternalism flourished. But even in the eighteenth century, when patriarchal relationships were, in fact, in a state of "deterioration," paternalism continued to function, virtually unquestioned, as the dominant ideology of the ruling class. In the nineteenth century, the nature of paternalist ideology changed. Until the end of the eighteenth century, Roberts points out, paternalist ideas were adhered to in an "unselfconscious and customary manner;"

in the early Victorian period, these traditional notions were "revived, amplified and transformed into a social theory and a social remedy for new and frightening problems."

Roberts' analysis of the ideology of paternalism is perceptive and useful, and his book provides a thorough treatment of the ways in which this set of ideas manifested itself. But while Roberts himself approaches paternalist ideology with a sympathetic ear, and with a willingness to believe that individual paternalists may well have been men of good will, the chief achievement of this book is to demonstrate the inability of paternalist theories to generate effective solutions to the problems created by industrialization. Paternalist theory was unable to do so because it was self-serving. As Roberts demonstrates, paternalist landlords were willing enough to support restrictions on the exploitation of women and children in industry, whereas they rejected the application of such restrictions to agriculture, just as captains of industry, as a group, could perceive that landlords were exploitative, while insisting that the principles of laissez-faire capitalism meant that any attempt to control industry would be self-defeating. All paternalists were, in the last analysis, more concerned to protect the rights of property than they were to defend the weak against exploitation.

There is one aspect of his subject that I wish Roberts had confronted directly, namely the way in which paternalism defined relationships between the sexes. In nineteenth-century society, social class was one indicator of status; gender was another. In analyzing the status of women in nineteenth-century English society, one factor that must be borne in mind is the extent to which the lower status of females, relative to that of males, could be negated by the status conferred by social class. Characteristically, the women of the landed classes wielded considerable power in their communities, in spite of their sex. The social outlook that Roberts defines as

"paternalism" gave validity to the power that upper-class women held. Moreover, as paternalism became a self-conscious social theory, a justification for the traditional power of upper-class women was incorporated into it, in the form of the nineteenth-century conception of the "lady bountiful." Surely it would have been worth examining the fact that many of the guiding and helping functions that Roberts sees as central to paternalism were designed to be performed by women, women who were themselves defined by paternalist doctrine as subject to the authority of the men of their own social class.

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Robert Eugene Johnson, *Peasant and Proletarian. The Working Class of Moscow in the Late Nineteenth Century* (New Brunswick, N.J.: Rutgers University Press 1979).

RUSSIAN HISTORIOGRAPHY provides two models, usually considered mutually exclusive, of the Russian factory labourer in the late nineteenth century. Marxists, and labour historians influenced by Marxist categories, portray the fully urbanized and proletarianized worker who has risen above his peasant past to higher levels of social consciousness and organized protest. Critics of the proletarian interpretation feature instead the uprooted peasant who, thrown into the factory environment, has retained his peasant outlook and protests in the violent, localized, unorganized, and purposeless manner of his ancestors. Professor Johnson observes, however, that, "the consciousness and discipline of prerevolutionary workers was often highly developed in the darkest, most backward corners of Russia, but neither the proponents nor the critics of proletarianization have explained why this should be so." (8)

Setting the models aside, Johnson seeks an explanation in the particular

social and economic conditions and peculiar historical traditions of the Moscow region. From a rich variety of archival and printed sources, many statistical, he finds that the factory experience combined in a symbiotic relationship with village customs and habits to produce in the Moscow industrial region a distinctive social type and way of life almost unknown in England or Western Europe but with close parallels in today's Third World. Several factors supported the symbiosis. Many Moscow factories had begun as small peasant enterprises, and their owners and managers still tended to preserve peasant values. A majority of factories were not in the city at all but in town and rural settings; but even in urban enterprises the demands and regimen of the factory shielded workers from urban influences. Worker marriage patterns, for example, generally continued to reflect village and not urban characteristics.

More significant was the agricultural poverty of Moscow province. Generations before the emancipation of the serfs in 1861 peasants, who remained legally attached to their villages, supplemented local incomes by seasonal or, more commonly, year-round factory labour. By the 1880s the practice of departure for factory work (*otkhodnichestvo*) was common and a high degree of continuity and generational succession was already established. A child, raised in the village, followed his father into the factory, then tended at age 40 to return to the village. Few workers, even after several generations, were entirely dependent on wages. Near the end of the 1890s, 40 per cent of hereditary peasant-workers held village land allotments and a much higher percentage had close economic and personal ties with their village. Moreover, persons from the same village or district tended to congregate in the same trade, factory, or factory division. Such clustering (*zemliachestvo*) transplanted local traditions, differences, and loyalties into the factory and strengthened ties with the countryside.

Consequently, peasant characteristics and beliefs such as distrust of strangers and naïve monarchism were perpetuated in the factories. Radicals had to contend, with limited success, not only with the usual store of workers' suspicion or indifference, but also with the special barriers of local loyalties with which *zemliak* ties confronted outsiders. Worker parochialism aided owners and police in isolating employees from external influences.

Moscow workers did protest. Professor Johnson subjects some 452 incidents at Moscow factories between 1880 and 1900 to quantitative analysis. Throughout the period, wages and layoffs were the primary causes of protest whereas non-economic grievances remained secondary. Years of peak employment saw the greatest unrest, and boom years also inspired workers to more aggressive and optimistic demands. More interesting, Johnson found no significant difference between the frequency and intensity of strikes in urban or highly mechanized factories on the one hand and more rural or technically backward factories on the other. Nor were the workers in the former any more disciplined, organized, or aware of broader issues and economic trends than their counterparts in the latter.

Johnson concludes that proletarianization did not of itself foster labour unrest. The sources of collective action lay instead in the meeting of factory and village. Russian peasant communalism was a better school of cooperative action than the atomism of urban life. Workers who had a stake in the village felt more secure and feared dismissal less than pure wage earners. Village ties and the phenomenon of clustering provided a basis of familiar support for joint activity and facilitated communications among workers within the factory. The same factors which promoted disciplined action within a factory also fostered isolation and inhibited coordinated action among factories. Johnson found almost no evidence of such coordination and remarks that workers' collective con-

sciousness was based on specific groups formed primarily by *zemliak* ties rather than on loyalty to the working class as a whole.

The argument is persuasive, although Johnson's sources permit only a glimpse at the "distinctive organizational features" he claims for the unrest of Moscow workers. Although we learn a great deal that is fascinating and useful about family and migration patterns and the daily round of factory life, we still know very little about how strikes in Moscow factories began, proceeded, and ended or about the leaders of collective action and their followers. Nevertheless, this book, which is clearly organized, concisely written, and attractively produced, provides a plausible framework for the further study of Russian labour history which historians cannot ignore.

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Barbara Evans Clements, *Bolshevik Feminist: The Life of Aleksandra Kollontai* (Bloomington: Indiana University Press 1979) and Alix Holt, ed. and trans., *Selected Writings of Alexandra Kollontai* (London: Allison and Busby 1977).

BARBARA EVANS CLEMENTS' biography of Aleksandra Kollontai and Alix Holt's selection and translation of Kollontai's writings should make this extraordinary woman and her seminal ideas about female psychology, love, and childcare more accessible to a general audience. While both authors treat Kollontai's personal life and general political evolution, both recognize that her original contributions to socialist as well as feminist thought and practice are to be found in her writing on sexual relations and family life and, secondarily, in the marriage, divorce, and maternity insurance reforms she sponsored as a People's Commissar in the early Soviet period. Indeed, Holt devotes over

half of her book to excerpts from such important pamphlets and articles as "Theses on Communist Morality in the Sphere of Marital Relations" and "Make Way for Winged Eros: A Letter to Working Youth."

Clements' book can be compared to the more psychologically-oriented biographies of Marxist luminaries, for it displays most of the virtues and few of the failings of this genre. Thus Clements sketches in Kollontai's childhood and early marriage mainly to show the origins of her continuing need for both independence and intimacy. Clements then argues, convincingly, that Kollontai embraced those elements of communist theory that resolved her psychological need for both autonomy and dependence. Socialist solidarity, as Kollontai understood it, allowed "connectedness" without sacrifice of self. This interpretation illuminates Kollontai's recurring theme of women seeking relief from solitude in love, then escaping "possession" in a collective; her idea of socialist erotic love as emotional commitment without possessiveness or separation from the collective; and even her decision to accept diplomatic exile and abandon her work in the mid-1920s. In the last — and most troubling — instance, Clements claims Kollontai opted for community over isolation because the party was the vehicle of revolution and the party and revolution were her life. She also mentions that Kollontai was 50 years old.

Clements' psychological interpretation provides a unifying theme to her book, a key to Kollontai's long and complex life, and insight into her writing on women, love, and communist morality. Anyone unfamiliar with Kollontai should begin with the biography, for it enhances appreciation of the often very rhetorical and sometimes sentimental written work represented in Holt's collection. However, the individualistic approach does not accommodate much speculation about Kollontai's isolation as a woman within a Marxist organization. For this kind of

speculation, the reader should turn first to Holt's brief remarks on women revolutionaries, then return to the more detailed descriptions of Kollontai's relations with her comrades scattered throughout Clements' book. Both works might have done more with this disturbing issue. Similarly, Clements' basically narrative approach permits pauses to outline and evaluate important published works but does not allow extensive historiographical reflection, while Holt simply notes recent Soviet attempts to revive a trivialized version of Kollontai's views on love and sexual relations and current Marxist debates on the value of domestic work. More historiographical background might increase the general reader's interest in Kollontai's theories.

Both authors deal critically with Kollontai's unoriginal and unsystematic Marxism, as well as her "anarchism" and "utopianism." But Clements praises "The Workers' Opposition" as "her finest political manifesto" and compares her to Luxemburg, while Holt sees the ideas of the Opposition as "unworthy" of a member of a Marxist party (156) and rejects any basic similarity to Luxemburg, saying Kollontai never overcame "the sectoral nature of women's experience" or formulated "a general course of revolutionary strategy." Holt reprints an English version of the document, so readers can decide for themselves. Many feminists may find Kollontai's critique of hierarchy and bureaucracy more attractive than either of these sober historians, obliged to take into account the chaos of post-revolutionary Russia, suggest.

Likewise, both historians explain Kollontai's aversion to "feminists" as a reaction to the bourgeois quality of contemporary feminism and a defence against her comrades' suspicions of separate women's organizations. Although they indicate how Kollontai differed from present-day feminists (with the notable omission of her support for sex-specific protective legislation), they insist she was a Bolshevik

feminist who argued that the transition to a classless society must also encompass the emancipation of women from domestic labour, the shift from maternal to social responsibility for childrearing, and a newer, less exclusive form of sexual relations. Holt treats the ambivalent nature of her attitude toward maternity as a social not a private matter more forthrightly than Clements. Holt's introduction to the section on "Women and the Revolution" claims that Kollontai's emphasis on childbirth as a social duty must be seen in the light of the urgent need to replenish the population after war and civil war, and in the light of Kollontai's proposals to relieve women of child-rearing (after weaning). On the other hand, she records that the devastated economy did not permit state help in childrearing (thereby answering her rhetorical question about whether women or the "collective" make sacrifices in the transition period). That section contains an excerpt from "The Labour of Women in the Evolution of the Economy" which expresses Kollontai's controversial ideas on maternity.

In conclusion, the Clements biography is a good introduction to Kollontai's life and thought, particularly on love, while Holt's collection is a more analytical introduction to Kollontai's philosophy, especially in relation to current Marxist and feminist concerns. The two books complement one another, for one puts down the biography with a desire to read Kollontai herself and one leaves the collection determined to learn more about this remarkable woman.

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Elzbieta Ettinger, ed. and trans., *Comrade and Lover: Rosa Luxemburg's Letters to Leo Jogiches* (Cambridge, Mass.: MIT 1979).

ROSA LUXEMBURG was a formidable woman. She rose to fame and leadership

within the radical wing of the pre-World War I Social Democratic Party of Germany. Her political writings constitute an original contribution to Marxist theory. She went to prison for her anti-militarist, anti-imperialist war pronouncements. She helped to found the German Communist Party shortly before her murder at the hands of *Frelkorps* vigilantes on 15 January 1919. We have Paul Frölich's and J.P. Nettl's biographical studies of Luxemburg the socialist politician and Marxist theorist. With a selection of Luxemburg's letters to Leo Jogiches, Elzbieta Ettinger has sought to show that "the formidable Rosa," as she [herself] remarked in a different context, was "quite human." (109) Luxemburg's almost 1,000 letters to her *Comrade and Lover* fill three volumes as published in their original Polish. Passing over those devoted mainly to questions of socialist politics, editor and translator Ettinger has taken as her principle of selection the goal of presenting a Luxemburg no longer "faceless." (ix) On the stage of revolutionary politics, Luxemburg ultimately donned the mask of tragedy; one finds from these letters that she harboured tragedy in her personal life as well.

Ettinger introduces the correspondence with a brief discussion of the nature of Jogiches's and Luxemburg's relationship, a ludicrously short review of "the woman question" and "the Jewish question" in Germany and Poland and Luxemburg's stand on them, and a cursory assessment of Luxemburg as a humanistic Marxist. Ettinger has then divided the letters into four sections, each of which she prefaces with a biographical note, and each of which marks a stage in Jogiches's and Luxemburg's tempestuous love. Theirs was a tug-of-war between strong personalities, a clash of incompatible temperaments, a mismatch of needs and desires.

From the start, they disagreed on the relation between love and work. Commitment to the socialist revolution intensified their mutual attraction when they met in Switzerland as two young exiles in 1890,

he from Russian-speaking Lithuania, she from Poland. She was 20; he, 23. While both were students at the University of Zurich, they lived at separate addresses but within walking distance of one another. Both combined their studies with devotion to the cause of socialism. But Luxemburg believed the academic and political work could easily be combined with a further commitment to their love. From Paris, where her doctoral research took her, Luxemburg wrote in 1894: "Your letters contain *nothing*, but *nothing* except for *The Workers' Cause* [Polish socialist party organ], criticism of what I have done, and instructions about what I should do." Hungry for more personal remarks in his letters, she argued: "It's not true that right now you're pressed for time. There's always something to talk about and time to write. It all depends on one's attitude." (8) While Jogiches would appear to have seen love and work in competition with one another for time and energy, Luxemburg saw them as mutually enhancing: "If I could kiss your sweet mouth now, I wouldn't be scared of any work." (14) It became a recurring refrain.

A variation on the theme was their disagreement on the right to a little happiness, a little beauty and comfort. Jogiches regarded only a life of renunciation as appropriate for a revolutionary. Luxemburg thought she should be free to spend a bit on pleasure in the present while struggling for the future happiness and freedom of all mankind. In 1904 Luxemburg was given a three-month prison sentence for making an insulting reference to Emperor Wilhelm II in a public address. She wrote Jogiches from her cell: "That you live such a lonely life is insane and abnormal, and I take a dim view of it. My present mood makes me hate such 'asceticism' more than ever. Here I keep grasping greedily at each spark of life, each glimmer of light. . . ." (139) The conflict was exacerbated by the fact that Jogiches contributed monetarily to Luxemburg's upkeep out of money he guiltily received from his family's busi-

ness in Wilno. But the cause of the conflict lay deeper, rooted in their very natures. Luxemburg was a life-affirmer, a yeasayer, an ebullient, expansive soul. Jogiches was a life-denier, a nay-sayer, closed in defensively on himself, querulously alert to the negative. The combination was a recipe for disaster. Luxemburg knew the cards were stacked against her, but for 15 years she did not give up. In 1898 she wrote from Berlin to Jogiches who had stayed in Switzerland: "... I keep harping on my worn-out tune, making claims on personal happiness. Yes, I do have a cursed longing for happiness and am ready to haggle for my daily portion with the stubbornness of a mule. But I'm losing it." (33)

That her career soared while his never got off the ground compounded the difficulties. Adept at political intrigue and conspiracy in his native Lithuania, Jogiches was like a fish out of water in exile and only once regained the heights of his early activist life, during the Polish and Lithuanian revolutionary uprisings of 1905 against the Czarist regime. Luxemburg, in contrast, thrived on the challenge and stimulation of the larger theatre of German and European socialism. Even as a student she outshone him. While he never completed his doctorate, her dissertation, *The Industrial Development of Poland* (1898), was immediately published in book form, a rare distinction. Already in 1896 she was aware that: "My success and the public recognition I am getting are likely to poison our relationship because of your pride and suspicion. The further I go, the worse it will get." (27)

She was right. When they first met, he was the older and presumably the more versed in Marxism and socialist politics, and she gladly played willing pupil to his severe task master. But she did not stay a fledgling long. To hold his own against her powerful mind and political success he held on tenaciously to his critical mentor role. He cultivated her dependence at the same time that he punished her for it, by

carping at her and by being niggardly with any show of love or need for her. "Now everything I do is wrong. You find fault with me no matter what," she wrote in 1897 when they were living in Switzerland but "ten steps apart" and meeting "three times a day." "... Why am I writing instead of talking to you?" she asked rhetorically and answered: "Because... your least gesture, one chilly word, wrings my heart, closes my mouth... hurt by your coldness, my soul bleeds and I hate you! I could kill you!" (22, 24)

Earlier in the relationship she would convince herself that by sheer force of exuberant will she could reform him. In 1895 she wrote from Paris:

... once I'm back I'll take you so firmly in hand that you'll squeal. ... You're an angry man, ... I'll wipe that anger out of you, so help me God. ... I'll terrorize you without pity till you soften and have feelings and treat other people as any simple, decent man would. (17-18)

In 1898 he exploded with anger over her having sent him a birthday present. Dejectedly she replied: "When will I change you for the better, when will I tear this damn anger out of you?" (55)

Why did she bother? Because she loved and needed him. She turned to him for information and ideas and she sought his criticism of her written work. When she was writing her critique of Eduard Bernstein, she begged Jogiches for: "Help, for heaven's sake, help! Speed is essential because (1) if somebody gets ahead of us the entire work is wasted; (2) polishing needs plenty of time." (53) But she needed him for more than inspiration and editing. For two years (1898-1900) she lived without him in Berlin, while he hung back in Switzerland, ostensibly finishing his doctoral dissertation, possibly fearful of joining her only to live in her shadow. She learned to live alone. "So I started to live in complete solitude, knowing that I am, and always will be, alone. It made me feel slightly cold but proud, too." But she did not like it and longed for his presence. "If only you knew how hard it is for me to have

no one to give me advice, no one to lean on or share my thoughts with." (94, 95) And despite his niggardliness and carping, he was devoted to her. He painstakingly proofread her doctoral dissertation for publication and, even after their break-up, her *magnum opus*, *The Accumulation of Capital*. Although Luxemburg saved none of Jogiches' letters to her, he meticulously saved her every written word to him. So she persevered. But his embittered defeatism got her down. In 1902, only two years after he finally came to live with her in Bertin, she was writing: "Your constant complaints, 'I'm fed up with everything,' 'it bores the hell out of me,' are symptoms of a senseless, savage, spiritual suicide. . . ." (119)

Ettinger tells us that one of her objectives is to demonstrate that Luxemburg was a person "whose sex did not diminish her political stature." The English-speaking world has not waited for a selection of Luxemburg's letters to Leo Jogiches to learn that. Nor would I agree with Ettinger that her choice of letters serves to "expose the fragility of the concept that a woman cannot, without giving up love, realize her talent." (ix) It seems to me that the letters reveal much more poignantly the great hunger for love and understanding which can devour even a woman of such immense brilliance and ambition and dedication to a cause as Rosa Luxemburg. Long after she must have realized its hopelessness, she still clung to the dream of having a regular married life and founding a family with Jogiches.

Our own small apartment, our own nice furniture, our own library; quiet and regular work, walks together, an opera from time to time, a small, very small, circle of friends who can sometimes be invited for dinner; every year a summer vacation in the country, one month with absolutely no work! . . . And perhaps even a little, a very little baby? Will this never be allowed? Never? (73)

Ettinger's main purpose in editing *Comrade and Lover* was to give Rosa Luxemburg

a human face; in this she has succeeded.

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Rémy Cazals, *Avec les ouvriers de Mazamet dans la grève et l'action quotidienne, 1909-1914* (Paris: Librairie François Maspero 1978).

WHEN INDUSTRIAL workers stay within the purview of the Catholic Church, formulate neither short- nor long-term revolutionary objectives, and persist in voting *en masse* for a reactionary party whose interests coincide with those of the Protestant capitalists, the situation constitutes an anomaly in French working-class history that deserves close attention. Rémy Cazals discloses the peculiar experience of wool shearers and leather dressers of Mazamet (Tarn) during the period 1909-14. His book is part of the *Collection du Centre d'Histoire du Syndicalisme*, directed by Jacques Droz and Jean Maitron; it orchestrates a wealth of information taken from archival sources, union documents, and verbal testimony of contemporaries. After drawing a brief portrait of Mazamet's economic and political character during the "Belle Époque," when union organization momentarily flourished and faltered, the author concentrates on the five-month strike of 1909, which manifested unprecedented solidarity, led to workers' affiliation with the *Confédération Générale du Travail*, and brought significant pay increases and better work conditions. In the aftermath of this victory the union sought to impose obligatory membership on non-organized employees and fought against the rising cost of living — hardly an attack on the basis of capitalist society, Cazals stresses. Yet by mid-1912, the powerful union (which included about one-half of the district's workers) splintered as certain leaders resigned their posts and entered the municipal election,

prompted by the conservative Reillist party. With one wing reduced to docility and the other suffering from financial difficulties and top-heavy bureaucratization, rank-and-file became disenchanted and the organization of workers floundered. The author concludes that the working-class movement at Mazamet had been ill-fated because workers refused to recognize the connection between union and political activities: they looked askance at anything that smacked of "politics." Although already imbued with anarchistic sentiments (evident in their aloof relationship with the CGT), Mazametians had been conditioned by the adroit propaganda of the Reillists (supported by the industrialists) who played the "religious card" in the political game of culling votes. The upper classes, ostensibly differentiated by religion and types of wealth, conspired in reality, emasculating the union and enabling the patronat-bosses to deny economic concessions even though workers' demands never placed the system in doubt. In short, the working-class movement at Mazamet was co-opted.

Cazals' argument is generally convincing, but weak in certain respects. His assertion that the majority of workers voted Reillist is not clearly illustrated by data on elections — tables do not include information on the electorate's size or the rate of turnout. Moreover, the openly contentious political situation in the 1908 municipal elections in which the Reillists were defeated deserves greater emphasis in explaining the 1909 strike. Little reference is made to the position of the petty bourgeoisie, especially after the union began to found co-operative stores — an action that threatened retailers and perhaps explains why workers became so isolated in their battle against the industrialists. Finally, there is a tendency to identify the workers' efforts at unionization with the working-class movement *per se*, when hardly a majority of the labouring population became organized. Yet this is a well-documented local study that undercuts

myths surrounding the actions of Mazamet's workers and aptly reveals the complexity of French working-class experience in its battle to win just wages and dignity on the job.

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Rémy Cazals, ed. *Les Carnets de guerre de Louis Barthas, tonnelier 1914-1918* (Paris: Librairie François Maspero 1978).

PUBLISHED memoirs of notables in World War I are fairly numerous, but those of ordinary people, conscripted into the army with little formal education, are rare indeed. Louis Barthas, cooper by trade and father of two children, was 35 years old when called to serve in the French 80th infantry regiment. He spent four and a half years at the front, fighting at the Somme, Verdun, and in the 1918 German offensive. As a corporal, he commanded a squad of troops — a unit of men bound by close ties of solidarity which excluded most officers, and even sergeants. His memoirs have now been edited by Remy Cazals, in cooperation with the *Fédération audoise des oeuvres laïques*. Barthas recorded the fickle mood of civilians whose enthusiasm quickly changed to silent pessimism. But more revealing is his portrayal of the plight of soldiers engaged in continuous trench warfare. Attacks were ordered by glory-seeking generals without adequate preparation or strategic decisiveness. As a result, refusals to obey commands multiplied. Even Barthas was demoted (ostensibly) because he would not send his squad to repair trenches in broad daylight under enemy fire. Discontent found political expression, with soldiers singing the *Internationale*. The author relates how fraternization with enemy troops came about and how commanders connived to force the men to fight. In the wake of the Russian Revolution, he recounts the attempts of several regiments to elect "soviet" committees in order to oust their colonels and

to demand, "Peace or Revolution!" What he stresses is the helplessness of being caught up in a massive cogwheel-like machine wherein the brave protestor is treated as a criminal by disciplinary councils of officers — "these modern seigneurs." Weaponry became increasingly sophisticated: mustard gas, tanks, airplanes, automatic guns. . . . And the killing kept pace. Life was utterly fortuitous; death was avoided by intuition or instinct. Who profited from the slaughter? He was convinced that with each new offensive (each new order for weapons), the capitalist industrialists enriched themselves; and as soldiers fell on the battlefield, the profiteers benefited by the patriotic deaths of their political adversaries. In short, for this one-time farmer who helped the vinegrowers of Languedoc to unionize themselves, the horrible "Great War" was a conspiracy, in fact, a lie perpetrated by the rich, condoned by governments and journalists — a class crime against the people and humanity. Throughout, he remained an anti-militarist, a Christian socialist, and a humanitarian, resolute in his commitment to work for human fraternity. Although the memoirs are repetitious in places (because of the subject matter), they offer precious insight into the experiences of the common soldier and especially into the popular mentality and beliefs of the time when Europe was enveloped in a war to end all wars.

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Stephen Andors, ed., *Workers and Workplaces in Revolutionary China* (White Plains, N.Y.: M.E. Sharpe 1977).

THIS VOLUME OF rather uncertain value appears in the series *The China Book Project*. The series records full length translations of Chinese publications along with brief commentaries by the editors who are well-known scholars. The series is particularly useful for those who do not read

Chinese or do not know their way around the original source translations. The documents allow the non-Chinese specialist to get a sense of the Chinese view of their own contemporary problems and often a taste of the passion with which that view is articulated. All the documents have appeared in translation elsewhere. The first two sections, or more than half the book, appeared in *Chinese Sociology and Anthropology* several years ago. The remaining translations come from *Survey of the China Mainland Press* (SCMP), *Survey of China Mainland Magazines* (SCMM), and *Chinese Economic Studies*.

The collection is edited by Stephen Andors, author of *Socialist Civilization and Revolutionary Industrialization: China 1949 to the Present*, and former editor of the *Bulletin of Concerned Asian Scholars*. It begins and ends with materials which describe the working conditions of the Shanghai docks before and after liberation. The first document is a complete translation of a 137 page, powerfully descriptive account of the incredibly miserable conditions of the dock workers before Liberation and the change that unfolded after 1949. Published in 1966, just as the first battles of the Cultural Revolution began to envelop China, and especially Shanghai, its purpose was largely to provide Chinese readers with an account of how they had been exploited by western imperialism and its lackeys, from labour contractors and gang bosses to foremen and hired thugs. The two short documents of the concluding section published in 1974 reveal that despite dramatic changes since 1944 conditions are still very difficult and the tension between the theory of work participation and the reality of management pressure for greater productivity remains critical.

Section two of the book contains excerpts from a study of pre-liberation China's Nanyang Brothers Tobacco Company. First published in 1958 as part of the Four Histories Movement, this analysis of a large Chinese capitalist venture, and

other publications in the movement were, as Andors points out, "important in providing cadres with technical information and detail about production so vital in getting the economy back on its feet and establishing a firm base of support in urban centres for what had largely been a rural-based evolutionary movement." For this reader, the description of the 1924 strike, taken largely from accounts in contemporary papers provides the most valuable section of the entire volume and will be very useful for undergraduate students of Republican China.

The first two documents in Section three on "Revolutionary Management and Development" were published in the spring of 1958 just at the time China was jettisoning its Soviet model of industrial development for the more radical Chinese model of the Great Leap Forward with its emphasis on worker involvement in management and cadre involvement in work. Andors writes about the policy debates that followed the Leap and preceded the Cultural Revolution but he does not discuss the Leap's failures or the tremendous suffering of China's masses. Now that the "Myth of Infallibility" has passed and China is led by more pragmatic men, Chinese readily describe the bitterness of those years.

Section four's translation of "On the Management of Socialist Enterprises" was written by representatives of factories, government, and Fudan University. It includes an interesting essay on some basic theoretical problems of Marxism. Published in 1974 it offers a representative piece from the final years of the succession struggle.

In his very brief introduction Andors characterizes China's industrialization strategy as emphasizing "a planned and proportionate geographical and regional development" and "creatively built institutional support to balance the crucial relationship between industry and agriculture." He also calls it one that fostered "a good deal of equality within the country but also limited the distorting social and

economic impact that so often results from policies of those groups who seek integration with the structure of power and values which dominate the world market."

But the junking of so much of that model by Mao's successors is indicative of more than China's inherent political struggles and ideological conflicts. The point is, the model did not work as well as the Chinese proclaimed or many western observers wished to think. In a sense the choice of documents in this volume reflects more the vision of a revolutionary model than the reality of its defects and failings. While the documents are in themselves both interesting and important, they cannot be read outside the more analytical and objective context of both the years they cover and in particular the years since Mao in which the Chinese have dumped and continue to junk much of the Maoist model and the rhetoric in which it was enshrined.

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P.C.W. Gutkind, Robin Cohen, and Jean Copans, *African Labor History* (Beverly Hills: Sage 1979); Frederick Johnstone, *Race, Class and Gold* (London: Routledge and Kegan Paul 1976); Charles Perring, *Black Mineworkers in Central Africa* (London: Heinemann 1979); and Charles Van Onselen, *Chibaro* (London: Pluto Press 1976).

IN HIS INTRODUCTION TO *South African Labour History* Eddie Webster quotes Eric Hobsbawm: "Labour history is flourishing in most countries as never before." I shall try here to indicate the extent to which this is the case for Africa by examining some of the most recent and exemplary studies which are now accessible.

Any survey of labour studies in Africa must begin in South Africa, where capital and labour have developed together as nowhere else on the continent. Here the foundations for contemporary labour studies were laid in the 1940s, not so much

by academics as by militants within the labour movement itself.¹ A new impetus and a more theoretical focus came in the 1970s with the infiltration of students armed with a more humanistic concern with the social existence of the African working class on the one hand and a Marxism informed by the debates, especially within the English New Left, on the other. One of the first victims of these tendencies was the liberal notion that the focus on labour studies should be on race rather than class. The exemplars of this new thrust were Charles Van Onselen's *Chibaro* and Frederick Johnstone's *Race, Class and Gold*. *Chibaro* has become the classic of African labour history, largely on the strength of its own merits, but slightly, too, on the basis of Van Onselen's other studies of labour in southern Africa.² Van Onselen's trenchant style combining *Germinal* with the sociological imagination of E.P. Thompson has given us a study of one of the murderous moments in the development of the capitalist mode which we are likely to see. For here, in Southern Rhodesia in the first decades of the present century the strategies of the mine owners whose aim it was simultaneously to increase the supply of mine labour while reducing its costs were rooted in what Van Onselen has called "the economics of death." The combined effects of low money and social wages, spending on food, accommodation, medical facilities, compensation, and accident prevention, yielded working conditions which were more fatal than those found in POW camps. The means by which labour was controlled and disciplined makes chilling reading.

¹ For a fuller discussion of this see Eddie Webster (ed.), *Southern African Labour History* (Johannesburg: Ravan Press 1978).

² See Charles Van Onselen "Worker Consciousness in Black Miners: Southern Rhodesia, 1900-1920," *Journal of African History*, XIV, 2 (1973). Also "Randlords and Rotgut, 1886-1903," *History Workshop*, 2 (August 1976), and "The Regiment of the Hills: South Africa's Lumpenproletarian Army 1890-1920," *Past and Present*, 80 (August 1978).

And the struggles of the workers themselves to avoid the maw of this behemoth, are inspiring. Van Onselen has been criticized, with some justification, for concentrating on the specificity of the situation in Southern Rhodesia. The murderous coercion of labour, of course, is not unique to southern Africa or to early forms of capitalist accumulation. It is a persistent and not aberrant aspect of capitalist development on a world wide scale. But recognizing this, and the correlative problematic that the same capital, be it Falconbridge in Namibia or Canadian Javelin in Panama, imposes varied forms of exploitation on workers depending upon time and circumstance, all students of labour must recognize the exemplary qualities of Van Onselen's work: its assiduous use of empirical data, its inventive synthesis, and its compassion for the worker himself.

Race, Class and Gold seeks the answer to a different question: what is the utility of the concept of race in explaining the exploitation of Africans in South Africa? The answer is that "racism" cannot be explained by studying "race relations"; the concept is tautological. Racism can only be explained by considering the peculiar forms of exploitation of both men and minerals which arose in the gold mining industry. To do this it is necessary to comprehend the economic structures which were evolved by the mining capitalists in pursuit of profits. In his explanations of these structures Johnstone definitely exposes the theoretical shallowness of attempts to explain the exploitation of black South Africans on the basis of race relations.

It has been pointed out by Charles Perrings³ that a weakness in Johnstone's analysis arises from his failure to consider the process of production in the mining industry. The question here is why the min-

³ Charles Perrings, "The Production Process, Industrial Labour Strategies and Worker Responses in the Southern Africa Gold Mining Industry," *Journal of African History*, XVIII (1977), 131-2.

ing mainstay required "ultra-cheap labour." Part of the reason was that the product of the mines, gold, had a fixed price. But at least as significantly, it seems, was that South African gold mining required relatively more unskilled labour than elsewhere in the world and the cost of this labour, as a factor in overall production costs, was inescapable given the geological structure in which the mines were sunk.

Johnstone's book is as arid as Van Onselen's is vivid. In common with other political economics, Marxist or liberal, it leaps over the issue of how and by whom structures are made, resisted, modified, and demolished in order to examine their general forms and motions. Such a concern is a useful corrective to historical studies of labour which often fail to rise beyond the merely sentimental and empiricist. While Van Onselen's work may be located at the frontier of radical liberalism and Marxism, Johnstone has clearly crossed that frontier and has clearly been instrumental in assisting in the re-establishment of Marxism within studies of South African capitalism.

Charles Perring's *Black Mineworkers in Central Africa* is a consideration of capitalist strategies and working-class response in the Copperbelt of the Belgian Congo and Northern Rhodesia (today Zaire and Zambia) in the period 1911-41. Perring sees himself as writing within the discipline of history but is clearly influenced by Marxist political economy. His concern is somewhat more with structures than with experience: this yields an excellent sort of economic history but one shorn of feeling for its subjects. In Part Two of the book in a chapter entitled "Strike and Protest: Pointers to Proletarianization" there is some concern paid to three strikes and workers' resistance in the study of their failure but this seems to lack any sociological depth of analysis. We have instead a rather passive explanation regarding the difficulties inherent in organizing workers under the conditions of structural migrancy and some limited indication of how cul-

tural and religious institutions were used by strikers as cushions against the rigours of compound life. It may be that subsequent studies, profiting from the valuable disclosures made by Perring regarding the structural limits placed on workers' initiatives, will disclose more fully both early forms of labour protest, such as absenteeism, as well as later manifestations of collective protest.

All of these works have dealt with Southern Africa. They must be seen in the context of a wider debate which encompasses not only labour studies but the entire domain of capitalism in that region. The most notable forums for this debate have been in such journals as *Economy and Society*, the *Journal of Southern African Studies*, and the *South African Labour Bulletin*. It is from the latter that Eddie Webster has culled the studies presented in *Essays in Southern African Labour History*, a book which, with *Chibaro I* would elect as being among the major markers in the recent revival of Marxist labour studies in Southern Africa. *Essays* is but one in a series published by Ravan Press of Johannesburg which include: anon., *The Durban Strikes, 1973*; R.J. Gordon's *Mines, Masters and Migrants: Life in a Namibia Compound (1979)*; and Brenda Bozzoli's *Labour, Townships and Protest: Studies in the Social History of Witwatersrand (1979)*.

Beyond Southern Africa, in lands historically dominated by peasant and plantation agriculture there have been no notable book-length studies in the later 1970s. Foundations have been laid by works, on Kenya, for instance, such as Clayton and Savage's *Government and Labour in Kenya, 1895-1963 (London 1974)* and R.M.A. Van Zwanenberg's *Colonial Capitalism and Labour in Kenya, 1919-1939 (East African Literature Bureau 1975)* which, while full of suggestive material, lack either the imagination of Van Onselen or the theoretical precision of Johnstone. It is by comparison to these, not by any means pedestrian but certainly

conventional, studies of labour (but not the working class) that we can see the advances made by recent students writing on Southern Africa.

An attempt both to stimulate and consolidate labour studies in Africa more generally is P.C.W. Gutkind, Robin Cohen and Jean Copans *African Labour History*. As far as is possible in such a random collection of historians and social scientists writing in three continents this compendium of labour studies may claim to give some sense of the state of the art beyond southern Africa. Its contributions are informed by a general left liberalism with some dutiful acknowledgements to both the Marxist tradition of political economy and its Thompsonite variant in the area of labour history. Beyond this it is impossible to generalize about the book: it contains straightforward empiricist studies of strikes, mutinies, and labour rebellions, sketches of trade union development, and a useful overview mapping in general terms progress made in both the Anglophone and Francophone traditions in labour studies. It provides thus some sense of the experience of African workers under colonialism although, I feel, it is too modest in its attempts to insert the question of labour within the general schemas of Marxist and neo-Marxist theory in Africa. There are many threads which its editors might have

picked up and interwoven in the main fabric of the book but which they have left aside, perhaps because of space, although this is often an easy excuse. I think they might have at least raised the matter of the effect on the nascent working class on religious ideologies, Christian and Muslim, such as Lubeck has hinted at regarding workers in Kano, Nigeria and Ranger, and to a lesser extent Perrings, have taken up on the other side of Africa.

In sum, it is an exaggeration to propose that labour studies in Africa are flourishing. It is true that they are in southern Africa but elsewhere they are barely perceptible amid the debris thrown up by a couple of decades of intellectual busy work. The 1980s may see a shift, nurtured especially by a number of major studies in the works, but dramatic shifts are not likely. The main body of African history is thus likely to reel from one overtaxed liberal "theme" to another, rather like Canadian history. In the southern third of the continent we will see real advances, not merely in labour history, but in all areas of Marxist studies. The reasons for this are not far to seek.

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