

## Reviews / Comptes Rendus

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[Aller au sommaire du numéro](#)

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## REVIEWS / COMPTES RENDUS

- Gerald M. Sider, *Culture and Class in Anthropology and History: A Newfoundland Example* by Susan G. Davis / 257
- B. A. Balcom, *La pêche à la morue à l'île Royale, 1713-1758* par Mario Mimeault / 258
- Paul-André Linteau, René Durocher, Jean-Claude Robert et François Ricard, *Histoire du Québec contemporain: Le Québec depuis 1930* by Phyllis M. Senese / 261
- Jean-Pierre Kesteman, en collaboration avec Guy Boisclair et Jean-Marc Kirouac, *Histoire du syndicalisme agricole du Québec. UCC - UPA 1924-1984* par Jean-Claude Robert / 263
- Frank R. Scott, *A New Endeavour Selected Political Essays, Letters, and Addresses* by Carman Miller / 265
- C.H.J. Gilson, ed., *Strikes in Nova Scotia 1970-1985* by D. Cruikshank / 266
- Byron Eastman, *Labour Market Theory and the Canadian Experience* by Frank Strain / 267
- Lynn McDonald, *The Party That Changed Canada* by Nelson Wiseman / 269
- David Milne, *Tug of War, Ottawa and the Provinces Under Trudeau and Mulroney* by Christopher Armstrong / 271
- William K. Carroll, *Corporate Power and Canadian Capitalism* by Peter R. Sinclair / 271
- Charlene Gannagé, *Double Day, Double Burden: Women in the Garment Industry* by Parvin Ghorayshi / 273
- John J. McCusker and Russell R. Menard, *The Economy of British America, 1607-1789* by Alan Gallay / 274
- Robert A. Rockway, *The Jews of Detroit: From the Beginning, 1762-1914* by Gerald Tulchinsky / 276
- Michael R. Weissner, *A Brotherhood of Memory: Jewish Landsmanshaftn in the New World* by Melvyn Dubofsky / 277
- Dolores E. Janiewski, *Sisterhood Denied: Race, Gender, and Class in a New South Community* by Joy Parr / 278
- Alexander Keyssar, *Out of Work: the First Century of Unemployment in Massachusetts* by Peter Seixas / 279
- Stewart Bird, Dan Georgakas, Deborah Shaffer, *Solidarity Forever, An Oral History of the IWW* par Catherine Collomp / 282
- Kathy Peiss, *Cheap Amusements: Working Women and Leisure in Turn-of-the-Century New York* by Marybeth Hamilton / 283
- Susan Porter Benson, *Counter Cultures: Saleswomen, Managers, and Customers in American Department Stores 1890-1940* by Veronica Strong-Boag / 285
- Alan M. Brandt, *No Magic Bullet: A Social History of Venereal Disease in the United States Since 1880* by Angus McLaren / 286
- Melvyn Dubofsky and Warren Van Tine, *John L. Lewis: A Biography* by Robert H. Zieger / 288

- John R. Munkirs, *The Transformation of American Capitalism. From Competitive Market Structures to Centralized Private Sector Planning* by Stephen J. Randall / 289
- Peter Wyncoll, *The Nottingham Labour Movement 1880-1939* by Allen Seager / 290
- J.M. Winter, *The Great War and the British People* by David Levine / 292
- Roger Geary, *Policing Industrial Disputes: 1893-1985* by Rennie Warburton / 293
- David Green, *Working-Class Patients and the Medical Establishment, Self-help in Britain from the mid-nineteenth century to 1948*, and Paul Weindling, ed., *The Social History of Occupational Health* by Roger Cooter / 294
- Ken Coates and Tony Topham, *Trade Unions and Politics* by Richard Hyman / 297
- Scott Lash, *The Militant Worker. Class and Radicalism in France and America* par Paul R. Bélanger / 297
- David Levine, ed., *Proletarianization and Family History* by Patricia Hilden / 300
- Kathryn E. Amdur, *Syndicalist Legacy: Trade Unions and Politics in Two French Cities in the Era of World War I* by Harvey Mitchell / 303
- John Tisa, *Recalling the Good Fight. An Autobiography of the Spanish Civil War* by Adrian Shubert / 305
- Lindsey Charles and Lorna Duffin, ed., *Women and Work in Pre-Industrial England* by Roberta Hamilton / 306
- Joseph Kestner, *Protest and Reform: The British Social Narrative by Women, 1827-1867* by Wendy L. Mitchinson / 308
- Joyce Kornbluh and Mary Frederickson, eds., *Sisterhood and Solidarity: Workers' Education for Women, 1914-1984* by Sherene Razack / 309
- Raya Dunayevskaya, *Women's Liberation and the Dialectics of Revolution: Reaching for the Future* by Sheila Delany / 310
- Judith Rollins, *Between Women: Domesticity and Their Employers* by Pat Armstrong / 311
- Ken Gagala, *Union Organizing and Staying Organized* by Gregory Albo / 313
- Paul Burstein, *Discrimination, Jobs, and Politics* by Sally F. Zerker / 315
- Claus Offe, *Disorganized Capitalism: Contemporary Transformations of Work and Politics* by Philip Resnick / 318
- Harry C. Katz, *Shifting Gears, Changing Labor Relations in the U.S. Automobile Industry* by Robert Drago / 320
- Robert McCarl, *The District of Columbia Fire Fighters' Project* by Alan O'Connor / 322
- Isaac Deutscher, *Marxism, Wars and Revolutions: Essays From Four Decades* by Mark A. Gabbert / 323
- Michael L. Conniff, *Black Labor on a White Canal: Panama, 1904-1981* by Jim Handy / 325
- Andrew Gordon, *Evolution of Labor Relations in Japan: Heavy Industry, 1853-1955* by Sinh Vinh / 326
- Zoe Landale, ed., *Shop Talk: an anthology of poetry* by Robin Mathews / 328

## REVIEWS / COMPTES RENDUS

Gerald M. Sider, *Culture and Class in Anthropology and History: A Newfoundland Example* (Cambridge: Cambridge University Press 1986).

GERALD SIDER HAS WRITTEN a challenging and difficult new chapter in the history of capitalism. *Culture and Class in Anthropology and History* is neither a history nor an ethnography in the conventional senses of those words: it is not a full historical narrative, nor is it an attempt at a full, local account of culture, based on participation and observation. It needs to be read on two levels, as a sketch, based on secondary and new primary materials, of three centuries of labour and class relations in the Newfoundland outports, and as a series of theoretical arguments for which Newfoundland provides particular but relevant examples. The issues tackled are large ones. How do we think about class as an experience as well as a relationship? How can we break with the conception of culture as static and outside history? How can we conceive the relations between class, culture, and the development of capitalism? More specifically, following the work of Eric Wolf and E.P. Thompson, Sider is concerned to examine the relationship between a dominant and a dominated society under merchant capitalism, the connections between economic and cultural domination, and the tension between culture as a seemingly autonomous source of meaning and resistance in everyday life and its role as an agent of hegemony. This is a vast agenda, and if all the pieces of Sider's argument do not fit neatly, it may be a measure of the difficulty of writing about the real relationships between a powerful but shifting and uneven international economic system and local experience. Most

social scientists are content to pretend such relationships either do not exist or cannot be described.

Sider places labour processes and their organization at the heart of Newfoundland history. But the fisheries, the prime exploitation of Newfoundland's resources, did not develop out of some inevitable process of growth. Rather, the fisheries were formed in the context of the innovations of English merchant capital, and one innovation, vis-à-vis Newfoundland, was total merchant control of the economies of the outports. Unlike other colonies, Newfoundland was not a settlement, but its waters were fished seasonally by merchant captains who paid wages to contract servants. Throughout the eighteenth and early nineteenth centuries, property in land was outlawed because it promised labourers too much independence from the merchants. Settlement and agriculture would have undermined the control of labour supply and wages.

From these origins grew a system of underdevelopment and what Sider calls "overdomination," which limited severely local political and economic control in the outports. Dependence on the merchants flowered in a second "family" phase of the fisheries. From the early nineteenth century on, resident kin groups organized fishery labour, while merchants controlled prices and a new wage form, the barter of fish for truck or "winter supply." The truck system dictated the need to fish in good weather in the hopes of making enough to survive the winter; merchants sought to reduce risks by manipulating prices, thus spreading out losses across communities. English accountants balanced starvation in the outports against losses from shortfalls and bad seasons. Other means of survival were nonexistent, capital accumulation for fisherfolk was impossible, and merchants saw no need to invest in

the outports. But under the family fishery, control of labour, property, and prices was incomplete. The sea, unlike land, is difficult to own or alienate. Fisher families reproduced skills, knowledge, and equipment, and fishing was difficult labour to supervise. In the nineteenth and early twentieth centuries, starvation was the main goad to productivity and sales outside the contract for truck were defined as theft and prosecuted.

Within this context of overdomination and underdevelopment, Sider takes up the questions of culture and custom. On one hand, culture was the medium through which the fishing skills and organization were produced and reproduced: without families there could be no boats, nets, or properly cured catches. In this sense, culture was power, but not autonomy. On the other hand, in the context of domination, the "cultures that provide their own meanings and that delineate their own social relations reproduce their own helplessness." Caught in structures not of their own making, the fisherfolk had to create social relations and symbolic forms that reflected and served those imposed structures; the creation of these forms and symbols was part of a struggle for survival.

Among symbolic forms Sider considers the older folk dramatic tradition of mummering and a newer custom, "scoffing," the theft of feasts. Each custom models reciprocity and interdependence within the village, reproduces a symmetry of kin and work relations, and symbolizes the asymmetries, tensions, and conflicts underdevelopment has built into those relations. Fisher families are heavily interdependent, yet they are forced always to consider self-interest first.

This is a powerful and persuasive argument, and will likely stir contention among anthropologists and folklorists who have worked in Newfoundland. Sider has made long strides toward ways of thinking about culture as processual and historical, rather than above — or accidental to — history. Greatly to his credit, Sider shows anthropologists that historical context can never be just local. The picture of folk cultures as first and foremost the products of exploitation and underdevelopment may not please folklorists. But the history of capitalism is part of the history of culture in even the most remote parts of the

globe. Gerald Sider has provided a careful example of how to write that double history, keeping the labours of people at the center.

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B.A. Balcom, *La pêche à la morue à l'île Royale, 1713-1758* (Ottawa: Parcs Canada 1984).

Roch Samson, *Pêcheurs et marchands de la baie de Gaspé au XIX<sup>e</sup> siècle. - Le rapport de production entre la compagnie William Hyman and Sons et ses pêcheurs-clients* (Ottawa: Parcs Canada 1984).

L'A. DU PREMIER OUVRAGE, B.A. Balcom, le dit dans son introduction: "Par rapport à la place accordée à d'autres produits importants dans l'économie de l'Amérique du Nord, comme les fourrures, le bois et le blé, les historiens ont très rarement rendu compte de l'influence de la pêche sur la côte atlantique." Charles de La Morandière a étudié les structures de la pêche française en Amérique et Harold A. Innis présente une synthèse qui s'articule autour des grandes politiques économiques. Ces approches sont globales; elles fournissent peu de détails sur les économies locales. L'année 1984 se révèle donc une année faste pour les spécialistes des pêches. Les ouvrages de Balcom et Samson s'y attachent en effet pour la période du régime français et pour le temps de l'Union et de la Confédération.

B.A. Balcom étudie le système des pêches développé par les habitants de l'île Royale entre 1713 et 1758. Son approche se place uniquement au plan descriptif. Le premier chapitre situe la pêche par rapport à l'économie de la petite colonie. Si les historiens ont insisté sur le rôle militaire joué par Louisbourg, l'économie de l'île n'en reposait pas moins sur la poursuite de l'effort de pêche français dans le golfe Saint-Laurent. Pour installer sa colonie, le gouvernement français n'a pas lésiné sur les argent. L'île Royale en recevait de six à huit fois plus par habitant que le Canada. Mais où se situe l'importance des pêches dans

une économie où les dépenses gouvernementales occupent une bonne place? L'A. essaie de nous le faire comprendre par comparaison: les quatre millions de livres consacrés à la construction des fortifications n'auraient représenté que trois années de la production halieutique de l'île. Après la pêche et les déboursés du gouvernement, le commerce constituait le troisième pôle de l'économie locale. Louisbourg devint en effet rapidement l'axe par lequel les grands ports de France maintenaient leurs liens avec les colonies d'Amérique. Dans ce circuit, les produits de la pêche représentaient, certaines années, jusqu'à quatre-vingt-dix pour cent des exportations de l'île Royale.

D'autre part, l'A. entend montrer le lien qui existait entre la pêche française et l'établissement de l'île Royale. Par un cheminement ardu, où il nous parle des pêcheurs basques, de la dégradation de la morue séchée, des types de pêche française, il en arrive à revoir le développement des pêches métropolitaines depuis le XVII<sup>e</sup> siècle jusqu'à la fondation de la colonie de l'île Royale. Il eut gagné en clarté à commencer tout de suite par ce point. Par la suite, Balcom expose les règlements qui régirent le commerce entre les pêcheurs résidents et les marchands forains, ou qui protégeaient la situation financière de l'habitant-pêcheur (entrepreneur local) face aux pêcheurs itinérants. Le volume des exportations de morue lui permet de discerner trois "périodes" dans le développement de l'industrie halieutique locale. Il termine par la place que la pêche occupait dans l'économie de l'île, mais il explicite un peu plus qu'au premier chapitre.

Dans la suite, l'A. traite de l'installation de l'habitant-pêcheur et des multiples aspects d'une opération de pêche. Il nous explique le système d'attribution des lots de terre, il fait état des quelques inévitables tentatives de monopolisation des espaces propres à la transformation de la morue puis il décrit l'aménagement d'un "terrain de pêche." Il parle aussi des ressources humaines, de la productivité des intervenants, de la pêche d'été et de celle d'hiver, du type d'embarcations utilisées et de leur coût, de la disponibilité des graves. Il consacre enfin quelques paragraphes aux relations de travail qui se sont développées dans le métier de la pêche.

Le coût des entreprises de pêche fait l'objet d'un chapitre entier. L'habitant-pêcheur devait calculer dans ses frais d'opération le loyer de sa grave, la location et l'achat d'une embarcation, le prix des agrès et des ustensiles de pêche, celui du sel et des vivres. Dans ce tableau du financement d'une campagne de pêche, la main d'oeuvre représentait la principale dépense, allant parfois jusqu'à 54.2% des sommes engagées. L'A. fait état d'une rémunération à la part, faisant remonter cet usage au temps de Plaisance. Cette modalité de paiement existait en France, bien qu'il n'en parle pas. Dernier problème soulevé, la rareté de la main d'oeuvre, qui entraîna la concurrence dans l'embauche et la surenchère dans le versement des salaires. C'est aussi l'entrepreneur local qui en fit les frais.

A l'opposé de ces obligations, l'habitant-pêcheur comptait sur l'entrée des argentés que lui procuraient la vente du poisson, mais aussi la fourniture à ses hommes des boissons alcooliques et des vêtements essentiels aux activités de pêche. La vente de ces derniers items, avance Balcom, aurait démarqué la marge des profits des pertes possibles.

B.A. Balcom a manifestement essayé de couvrir tous les aspects du thème étudié. L'ouvrage présente un plan uniforme, traitant uniformément chaque grande idée. Huit pages de texte à chaque chapitre, ou peu s'en faut. Une présentation à deux colonnes accompagnée d'une iconographie abondante couvrent les quatre-vingt-quatorze pages du volume. L'ensemble de ces illustrations, les appendices, les pages de notes et des renvois ainsi que les tableaux de statistiques en moins et il ne reste qu'à peine quarante-six pages de texte sur le fond. C'est vouloir en dire beaucoup en peu d'espace et le développement en souffre. Le travail demeure quand même intéressant tant par la qualité des images reproduites que par les lumières apportées sur l'industrie de la pêche du régime français.

Roch Samson étudie pour sa part le système des pêcheries canadiennes à une autre époque que celle choisie par Balcom, le XIX<sup>e</sup> siècle. Ce chercheur analyse en profondeur les rapports de production d'une compagnie de pêche de la baie de Gaspé, la William Hyman and Sons, et ses pêcheurs.

L'industrie de la pêche a fait de Gaspé, aux XVIII<sup>e</sup> et XIX<sup>e</sup> siècles, l'un des grands

centres maritimes de la côte est canadienne. L'économie locale reposait sur la production et le commerce d'un produit unique, la morue séchée et légèrement salée. Dans le dernier quart du XVIII<sup>e</sup> siècle, et tout au cours du XIX<sup>e</sup>, plusieurs compagnies anglo-normandes investirent dans ce secteur industriel. Dans l'ensemble, elles venaient de l'île Jersey, une petite possession britannique située à proximité de la côte française.

La plus connue de ces entreprises est celle de Charles Robin qui s'installa en Gaspésie dès 1766. D'autres industriels l'ont par la suite suivi: les frères Janvrin, John et Elias Collas et William Fruing, pour ne mentionner que ceux-là. William Hyman dont les activités commerciales débutèrent vers 1845 était toutefois d'origine russe et d'appartenance judaïque. De surcroît, et bien qu'il emprunta les modes d'opérations jersiais, il trouva ses appuis financiers à Londres, Montréal et Québec, ce qui en faisait en quelque sorte, dans le milieu des hommes d'affaires locaux, un "outsider" comme l'écrit Samson.

La Hyman and Sons fut ni la plus grande, ni la plus puissante des compagnies établies dans la baie de Gaspé, mais elle est une des rares dont les archives ont survécu. Après un bref aperçu historique de cette société, l'A. situe son importance sur le plan régional et détermine la position de l'entreprise par rapport aux capitaux internationaux. Le corps de son travail porte d'abord sur les aspects techniques de la production de la morue (le mode de préparation, les embarcations requises, les terrains nécessaires) et les rapports sociaux qui se sont développés entre le marchand et les pêcheurs gaspésiens.

Samson va plus loin que Balcom dans son étude. Au delà de la description des procédés de capture et de transformation du poisson, l'A. démonte en effet les structures de la pêche et développe l'organisation du travail qui existait. Il établit en même temps "les rapports techniques et sociaux de la production," qui prévalaient d'ailleurs non pas seulement dans la baie de Gaspé, mais dans tout le milieu des pêches de l'époque. Hyman est en quelque sorte un modèle, bien qu'il ne soit pas absolu, et que l'intention de Samson n'est pas de le présenter comme tel.

La dernière partie du livre contient une analyse quantitative et qualitative de

l'endettement du pêcheur vis-à-vis de William Hyman. Roch Samson analyse les procédés utilisés par la compagnie pour s'attacher les travailleurs et s'en faire des clients captifs. En se servant de ses livres de compte, il établit cette fois-ci les rapports de dépendance qui ont été créés entre elle et les pêcheurs.

Rien, dans les procédés, ne distingue les manières de faire de Hyman de ses concurrents. Dans un milieu où la circulation des marchandises et des produits passait par leurs mains, les marchands jersiais ont établi un système de crédit qui les avantageait et qu'il n'eut qu'à adopter.

Le point de départ se présentait au printemps alors que les pêcheurs se procuraient chez eux, contre paiement différé, tout le sel, les agrès de pêche et les vivres dont ils avaient besoin pour pêcher et faire vivre leur famille. Il s'agissait d'un achat fait à crédit pour le pêcheur indépendant, mais qui s'accompagnait bien souvent d'une avance pour celui que la compagnie employait à son service. Le remboursement s'effectuait une fois la campagne de pêche terminée, en capitalisant la somme du travail fourni à la compagnie, le salaire dû et la morue livrée. L'engagement du pêcheur vis-à-vis la compagnie variait d'un travailleur à l'autre, mais les conditions économiques faisaient que tous finissaient par se retrouver redevables en bout de compte. Le système débouchait sur un endettement généralisé du producteur et sur son contrôle par les capitalistes.

Mais Hyman lui-même n'était que le rouage d'un système économique plus vaste. Il avait aussi recours au crédit de ses fournisseurs de Québec et Montréal pour le moyen terme (de six à douze mois). Il obtenait de même, à chaque année, pour un plus long terme (douze mois et plus), des avances de ses créanciers européens à la livraison de ses premières cargaisons de poisson. De la sorte, l'économie gaspésienne s'intégrait au système économique international, celui du commerce atlantique britannique, mais, par son dynamisme interne, elle débouchait sur une situation de contrôle local par les compagnies.

Cet ouvrage en est un de commandite, demandé par Parcs Canada pour mettre en valeur l'élément culturel du Parc National Forillon. Il n'est qu'un regret à exprimer et qui ne concerne pas Samson, c'est que les autres

études faites sur le milieu n'aient pas toutes fait l'objet d'une pareille publication.

Quant à Samson, il fait montre de rigueur dans le traitement de son sujet. Clarté des idées et netteté de style caractérisent son discours. Sa préoccupation scientifique (l'A. est anthropologue), ne l'éloigne pas non plus du lecteur averti qui serait d'une autre formation. Le livre est l'objet d'une publication soignée. C'est autant par le fond que par la présentation de son ouvrage que l'auteur s'est mérité le prix Michel Brunet décerné en 1985 par l'Institut d'Histoire de l'Amérique française.

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Paul-André Linteau, René Durocher, Jean-Claude Robert et François Ricard, *Histoire du Québec contemporain: Le Québec depuis 1930* (Montréal: Boréal 1986).

AFTER THE WELL-DESERVED SUCCESS of the first volume of the *Histoire du Québec contemporain*, could Linteau, Durocher, and Robert produce a second volume at least the equal of its predecessor? Simply put, they, along with their new associate François Ricard, have written a sequel that is outstanding and in many respects an improvement on Volume I. Tightly focused, clearly directed, and explicitly historiographical in its approach, *Le Québec depuis 1930* stands on its own as a detailed examination of a society in evolution during one of the most important and exciting periods of its history.

The authors approached the daunting task of constructing this text with a clear purpose in mind and they have been strikingly successful in achieving it. As with Volume I the emphasis is on social and economic developments with the stress on evolution not chronological narrative to "expliquer les grandes phénomènes et les transformations majeures."<sup>5</sup> To explain present day Québec the authors insisted on industrialisation as a long-term development and modernisation as a long, non-linear process: Québec has become what it is over a period of

time, not just since 1960.

In considering the population of the province, the authors insist on referring to all residents of Quebec as *Québécois(e)*, examining ethnic origins more precisely in the context of specific demographic studies. As a result, a realistic picture of the province emerges that at once does justice to its essential French Canadian character while recognising the complexities of its ethnic diversity. Without so clear a discussion of this point, many of the language and education quarrels of the 1960s, 1970s, and 1980s would make little sense to those living outside Quebec.

*Le Québec depuis 1930* has been divided into three periods: 1930-1945, 1945-1960, and 1960 to the present. Each section opens with a general introduction followed by numerous small chapters on a variety of very specific themes such as demography, the economy, urbanisation, social groups and movements, labour, living conditions, religion, education, ideologies, politics, and culture. Women are introduced specifically at many points, but more could and should have been included, especially a more detailed discussion of the reasons for denying women's rights for so long in contrast with the speed of change after 1960. This type of organisation of the material keeps the focus sharply on social and economic questions but highlights a weakness in this book. The numerous small chapters open discussion in a variety of areas but fail to prevent a considerable amount of repetition. Perhaps that is helpful for students; other readers might find it an irritating distraction. Then too, in chapters that frequently run to no more than ten pages (interrupted by photographs and statistical tables), it becomes difficult to do much more than introduce a complex issue rather than examine it in detail. While the bibliographical notes accompanying each chapter give good direction for further reading, in many instances an extended treatment of an historical problem would improve this text. The photographs generally illustrate key points wonderfully, but the statistical tables are often left inadequately integrated into the text (as was true of Volume I) to be of complete benefit to students. The authors rightly set Quebec's economic development in the general context of North American trends and try to do the same, but with not as much success, when consider-



ing social, ideological, and cultural transformation. Often a sound balance is missing in describing what is unique to Quebec's experience and what it shares in common with the rest of the continent. On balance, these criticisms are minor; the book is an excellent introduction to the period.

The strengths of *Le Québec depuis 1930* are too numerous to mention; a few examples will have to suffice. What clearly emerges is a picture of a society that has moved through rapid and sometimes violent economic and social shifts since 1930. The roaring twenties gave way to the catastrophe of the Depression in which Montreal was particularly hard hit given its large metropolitan population and fragile economic base. As for many other Canadians, the war provided not only relief but the prospect of prosperity, only to have the post-war economy oscillate between extremes of boom and bust at a time when economic expectations were running higher than ever, technological change was transforming industry and the massive numbers of the baby boom were attempting to enter the work force. Accompanying these economic stresses, Quebec experienced the debate that was sweeping the western world on the proper role of government in managing the economy and in providing social security for its citizens. That the Duplessis government chose to intervene on the side of large scale capitalism and not the workers illustrates the extent to which unreformed political structures can distort public perceptions and thwart public concerns. The authors, in their detailed study of the post-war period, strip away a good deal of the mythology surrounding the Duplessis regime and describe accurately the complex, growing opposition to that administration. In an especially cogent discussion they speculate that the apparent collapse of Catholic institutions and influence after 1960 should be examined in light of the degree to which the Church was held accountable by public opinion, rightly or wrongly, for being implicated in the failure of the Duplessis regime to minister to the needs of people. By entertaining this kind of provocative suggestion, the stage is well set for the final section of the book; the *Révolution tranquille* and after.

Quebec's history since 1960 has been undeniably turbulent and extremely complex. So-

cial and economic change have occurred at a staggering rate; power relationships within the society have altered irrevocably, the language and educational face of the province has been radically transformed and new strains of Quebec nationalism have come into existence. To chart all of this in even a general way and to attempt to detail the cultural changes as well (in literature, cinema, television, music art), makes this part of the book a pioneering effort that generally succeeds. Rather than simply chronicle the events of the *Révolution tranquille*, Linteau, Durocher, Robert, and Ricard Stress instead the complexity of forces and personalities that lay behind the rapidly accelerating changes that occurred in Quebec after 1960. The bibliographical notes for each chapter demonstrate the extent to which the preoccupations of the authors have also dominated the historiographical trends since 1960. A great deal has been learned by many investigators, for example, about the intricacies of economic transformation in Quebec and the impact of language policy on society. On the other hand, little has been done to study what happened to the Church. The little that Linteau *et al.* can say about the drastic changes Catholicism in Quebec is a consequence, not of a lack of interest, but a lack of research. Even the Church before 1960 has been poorly studied; much is known about its positions, priorities, and preferences but too little is known about its actual presence in society and how society viewed that presence. *Le Québec depuis 1930* reveals, perhaps unintentionally, a great deal about the nature of scholarly preoccupations since 1960.

In summing up their work, Linteau, Durocher, Robert, and Ricard point to four specific myths they believe that this book has dismissed: that Quebec is, and has been, a monolithic society, that Quebec has functioned largely as a closed and isolated society, that Quebec was a traditional society suddenly catapulted into the modern era in 1960, and that until very recently there was a single strand of nationalism that ran through the province's history. In convincing fashion *Le Québec depuis 1930* (in conjunction with Volume I) has effectively dismantled these old stereotypes. Anglophone as well as francophone scholars will no longer be able to avoid serious examination of historical problems by resorting to

these tired clichés. This book makes it abundantly clear through its demographic studies that Quebec is anything but monolithic. Nor has it been isolated from any of the currents of experience or ideas common to the western world. At no time in its history could Quebec be *accurately* described as an exclusively traditional society. A thorough reading of the work of this talented *équipe* makes all this crystal clear. In its usefulness to students, one of the strongest features of this book is its patient unravelling of the tremendous range of nationalist ideologies that have surfaced in Quebec; there is not, nor has there ever been, a Quebec nationalism, only many nationalisms.

As a textbook *Le Québec depuis 1930* is a great success. It should stimulate students and scholars alike to ask many more questions of Quebec's past. Once again Linteau, Durocher, Robert, and Ricard have provided a new perspective on Quebec, an achievement which is a major contribution to recent scholarship. This book deserves a wide audience from coast to coast. [The English version should be available later this year.] Having provided a detailed outline and a clear point of view *Le Québec depuis 1930* offers other scholars a challenge: to fill in more of the detail and to do it as well. Linteau, Durocher, Robert, and Ricard leave their readers wanting more and more.

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Jean-Pierre Kesteman, en collaboration avec Guy Boisclair et Jean-Marc Kirouac, *Histoire du syndicalisme agricole du Québec. UCC - UPA 1924-1984* (Montréal: Boréal Express 1984).

À L'OCCASION DE SON soixantième anniversaire, l'Union des producteurs agricoles du Québec a voulu souligner l'événement en commanditant une histoire du mouvement. L'UPA a décidé de faire appel à la collaboration d'historiens professionnels et a demandé l'aide du département d'histoire de l'université de Sherbrooke. Cet ouvrage en est

donc le résultat. Jean-Pierre Kesteman professeur de cette institution, en a assumé la rédaction, assisté de Guy Boisclair et de Jean-Marc Kirouac. Ces deux derniers sont responsables du travail en archives et ont préparé les dossiers-synthèses utilisés pour la rédaction finale. Soulignons que J.-M. Kirouac est un ancien permanent de l'Union, y ayant travaillé pendant près de quarante ans. Ainsi ce livre peut apparaître à la fois comme une oeuvre de synthèse élaborée à l'extérieur du mouvement et aussi, à cause des antécédents de l'un des membres de l'équipe de recherche, comme un ouvrage informé de l'intérieur. Cette ambiguïté comporte ses avantages et ses inconvénients, comme nous le soulignerons à l'occasion.

La texte se déploie en quatre parties chronologiques et compte vingt chapitres. Le première couvre des débuts du mouvement à 1929 (3 chapitres), la seconde traite des années 1929-1952 (6 chapitres), la troisième de la période 1952-1972 (9 chapitres), et la dernière, des années 1972-1984 (2 chapitres). Chacune des parties comprend un survol de l'évolution de l'agriculture québécoise pour la période étudiée, qui occupe *généralement* un chapitre, les autres étant consacrés aux divers aspects de la vie de l'association. Ajoutons que l'iconographie est abondante, sinon toujours pertinente.

Ce livre soulève un problème intéressant, particulièrement pour les lecteurs de cette revue. Il s'agit de la place et du caractère spécifique du syndicalisme agricole dans l'évolution générale du mouvement syndical. En effet, même si l'UPA depuis une quinzaine d'années utilise un discours et des méthodes qui sont très près de ceux des syndicats ouvriers quand ils ne sont pas des emprunts, elle demeure une organisation de producteurs qui regroupe des individus dont l'activité se rapproche davantage de celle d'une PME que de celle d'un ouvrier d'usine ou d'une travailleuse dans les services. Il est vrai que le mouvement d'intégration de l'agriculture au marché signifie une perte d'autonomie des cultivateurs, mais cette dépendance n'est pas sans ressemblance avec celle d'une petite entreprise vis-à-vis des forces du marché. C'est dommage que les auteurs n'aient pas développé beaucoup cet aspect de la mutation de l'ancienne Union catholique des cultivateurs

*pour nous faire voir si cette transformation s'est effectuée sans heurt ou si au contraire elle a impliqué des débats profonds. Par ailleurs, on pourrait poser la question de savoir dans quelle mesure les agriculteurs n'auraient pas emprunté au syndicalisme uniquement ses modes d'actions, devenus plus efficaces dans le contexte socio-politique des années 1960?*

Dans l'ensemble, l'ouvrage représente un acquis important pour l'historiographie du monde rural québécois; c'est un domaine où les synthèses sont rares sinon vieillottes, je pense ici à l'histoire de l'agriculture de Firmin Létourneau. Justement, cette absence de synthèse a forcé les auteurs à donner une grande importance au contexte global du développement de l'agriculture. C'est donc un livre qui traite à la fois de l'agriculture et de l'organisation des agriculteurs. Cependant, il faut souligner immédiatement que cette vision de l'histoire de l'agriculture, surtout pour les années de l'après-guerre, est vue à travers les lunettes de l'UCC/UPA et donc fortement teintée.

Cette question n'aurait pas beaucoup d'importance si nous disposions d'une bonne synthèse sur l'histoire agricole du Québec et si le lecteur se rappelait sans cesse qu'il s'agit de l'histoire d'une association agricole qui est devenue au fil des ans la principale intervenante dans le milieu. Malheureusement, on nage souvent dans l'ambiguïté. Ce livre peut passer à la fois pour une synthèse de l'histoire agricole des soixante dernières années et à la fois pour une histoire du syndicalisme agricole. Or, il est d'abord l'histoire d'un syndicat bien particulier ayant rivalisé avec d'autres formes d'organisations du monde agricole; je pense que la nuance est ici tout à fait justifiée.

Dans ce sens, il faut noter l'absence de critique des sources primaires et secondaires. Ce phénomène est perceptible à plusieurs niveaux et bien entendu d'abord à celui des documents de l'UCC/UPA acceptés souvent sans aucune réserve. On le retrouve aussi dans l'utilisation de travaux comme la thèse de Robert Migner sur les débuts de l'UCC ou de celle de Michel Morisset sur l'évolution de l'agriculture québécoise au XXe siècle. Il s'agit de deux thèses très importantes et très utiles mais comme l'auteur en fait un peu la pierre angulaire de son interprétation globale de l'évolution de l'agriculture au Québec, il aurait

*été préférable qu'il soit davantage circonspect devant certaines affirmations ou certaines interprétations. Par exemple, prenons toute la question de la modernisation de l'agriculture. J.-P. Kesteman suit Morisset et date le changement significatif de 1939. Pourtant, de nombreux travaux et je pense ici à ceux de Normand Séguin, font remonter ces changements bien au delà du début du siècle et montrent une certaine continuité dans la modernisation qui ne sera que momentanément interrompue par la crise de 1929. Il se développe en fait deux types d'agriculture qui peuvent, dans certains cas, se côtoyer sur le même territoire.*

L'intervention gouvernementale, singulièrement minimisée dans l'ouvrage, constitue un second exemple. On pourrait y voir un effet de l'interprétation de Migner qui, dans sa thèse et davantage dans son livre, campe le ministre de l'agriculture du Québec comme le vilain qui cherche constamment à empêcher les cultivateurs progressistes de s'organiser. Que le ministre ait cherché à contrecarrer les projets d'organisation et qu'il n'ait reculé devant aucun coup bas ne devrait pas occulter l'existence d'une politique du développement agricole dont les travaux des politologues Gendreau, Blais et Boismenu ont bien montré non seulement l'existence, mais également la cohérence et la continuité.

A cette sous-estimation systématique du rôle des gouvernements répond une surestimation de celui de l'UCC/UPA. On peut même se demander s'il n'y a pas là une sorte d'exercice téléologique pour montrer la continuité et la légitimité de l'action du syndicat. L'histoire de l'UCC/UPA n'est pas que continuité et il y aurait peut-être eu lieu de replacer les discontinuités dans un contexte plus vaste. Par exemple pourquoi les cultivateurs les plus touchés par les marchés sont-ils ceux qui semblent boudier l'UCC? Pourquoi d'autres organisations continuent à exister ou à se développer?

En particulier, toute la question des coopératives devrait être réexaminée, et avec elle, celle de la rivalité entre les deux types d'association. Dans un mémoire de maîtrise récent, et dont l'auteur ne pouvait avoir eu connaissance, Danielle Noiseux a montré comment à l'échelon local, ce sont souvent les mêmes personnes qui sont membres du cercle

de l'UCC et de la coopérative (D. Noiseux. *La modernisation agricole dans les comtés de Laprairie et Napierville, 1920-1970: les choix des producteurs*. Mémoire de maîtrise en histoire, UQAM, 1985). Il semble donc avoir davantage de complémentarité entre les deux organismes.

En fait, et c'est là une dimension du problème dont l'auteur est par ailleurs conscient, l'histoire de l'UCC/UPA qui est présentée ici est celle des instances centrales. Il manque donc la vision des cercles paroissiaux et celle des fédérations diocésaines. On peut même aller plus loin et se demander dans quelle mesure, en termes d'influence idéologique, le mouvement était plus sensible d'en haut que d'en bas? L'examen de la répartition spatiale du membership et des diocèses représentés fait voir un décentrage au profit des régions où l'agriculture entretient des rapports plus distendus avec le marché.

En dépit de ces quelques remarques, *l'Histoire du syndicalisme agricole au Québec* a le grand mérite de faire le point sur l'évolution de l'agriculture et de poser le problème des organisations du monde rural. Le volume contribue ainsi à combler une lacune importante de notre connaissance et, comme l'illustre ce compte-rendu, à stimuler la réflexion. Ajoutons en terminant que l'ouvrage est bien écrit.

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Frank R. Scott, *A New Endeavour Selected Political Essays, Letters, and Addresses*, edited and introduced by Michiel Horn (Toronto: University of Toronto Press 1986).

THE STUDY OF FRANK R. SCOTT has become a small cottage industry. Production began well before Scott's death in 1985, in the form of books, articles, papers, theses, a film, and even a Scott conference at Simon Fraser University in 1981. In public Scott dismissed this growing interest in his career with his typical

redeeming humour. He was not, however, a disinterested subject nor content to leave his reputation to chance. In fact during the last decade or more of his life he took an active hand in building his own monument. Not only did he sort his papers, grant interviews, and place his reminiscences on tape, but he undertook to republish carefully chosen collections of his writings on law, poetry, and politics. Both his *Essays On The Constitution* (1977) and his *Collected Poems* (1981) won Governor-General's prizes. *A New Endeavour* (1986), a selection of his political letters, essays, and speeches, 1931-71, completes the trilogy.

The man Scott would have us remember from *A New Endeavour* is Scott the internationalist, the socialist, the friend of labour and minorities, the advocate of civil liberties, social justice, cooperative federalism, and bilingualism. Though partial, this image of Scott is not inaccurate; and in the pursuit of these causes his language is powerful, his arguments convincing, and his passion contagious. During the last two decades of his life, however, Scott's political writing became increasingly repetitious, qualified, and conventional, as though he had become a captive of his public image, and largely reconciled to his society. By this time, except for the direction of French Canadian nationalism, Scott had many good reasons to be satisfied. He had friends in places of power and influence; he had become something of a national institution; and much of his political programme, a measure of government planning, medicare, bilingualism, and a Charter of Rights and Freedom, had become law. *A New Endeavour*, however, is but a partial portrait of Scott the political advocate and activist.

The book is also more than a self-portrait: it contains an introduction and editorial notes by Michiel Horn. The 38-page introduction is a respectful account of Scott's part in the founding and growth of the League For Social Reconstruction, the Cooperative Commonwealth Federation, and the New Democratic Party, an account which adds little to our knowledge of Scott or these organizations. More might have been said about Scott's earlier years, his relationship to his father, whom he resembled more than he cared to admit, and his talented wife, who shared his political commitment, if not always his more conservative

brand of socialism. More too, might have been said about Scott's anti-communism and its origins in the sectarian squabbles of the 1930s. Unfortunately the editorial notes are often superfluous, and sometimes incomplete and inaccurate. A reader of Scott's political writings is unlikely to need assistance identifying Winston Churchill, Nikita Khrushchev, Maurice Duplessis, Robert Bourassa and Claude Ryan. On the other hand Albert St Martin was more than merely a Montreal court stenographer who staffed a people's university. As for Sir Hector Langevin, he was never a Minister of Public Works in Sir John Thompson's Cabinet.

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C.H.J. Gilson, ed., *Strikes in Nova Scotia 1970-1985*, (Hantsport, Nova Scotia: Lancelot Press 1986).

THERE ARE NINE CHAPTERS in this book, each documenting a different strike occurring in Nova Scotia between 1970 and 1984. Although the chapters are authored separately, C.H.J. Gilson's editorial hand is evident throughout. All of the chapters are organized in a similar way, beginning with a discussion of the issues of the strike, continuing with a description of the bargaining process, and concluding with an assessment of the terms of settlement. While this format has limitations, it certainly makes it easy, as Gilson had hoped, to identify a number of themes that carry over from strike to strike.

The most obvious of these themes is the inadequacy of labour relations legislation in Nova Scotia. Lorna Darrah and Rosiland Belland, for example, describe how the fishermen of Canso Strait struggled for over a year in 1970-1 to gain recognition as "employees" under the Trade Union Act, only to be denied the union of their choice by another loophole in the law. Susan MacNeil's and Sheree Delaney's account of the Keddy's nursing home strike of 1982-3 and Isabel MacAdam's and Colleen

O'Connor's of the Lunenburg scallop fishermen's strike of 1983-4 show how difficult it can be to obtain union recognition within a legal framework that does not provide for the imposition of first contracts. Lawrence Currie's piece on the Digby school bus strike of 1979-81 reveals perhaps better than any of the others how a particularly stubborn employer can successfully ignore the Labour Relations Board even in areas where it clearly has jurisdiction. The chapters on the nurses' strike of 1975, the hospital strike of 1981, the Halifax police strike of the same year, and the Sydney police strike of 1984 (by Judith MacLean, Tony Thompson, Larry MacMaster, and Kyle Ferguson respectively) all demonstrate the tenuousness of legislation granting public sector workers the right to strike. Similarly, Sheri King's study of the Scott paper mill strike of 1982 shows how the threat of back-to-work legislation can also influence the outcome of disputes in the private sector.

All of this is useful information and definitely worth recording. Nevertheless, I suspect that the three audiences Gilson had hoped to reach — students of industrial relations, "practitioners" of industrial relations, and interested "laymen" — will want more than just examples of how the industrial relations system operates under stress. (9, 11) Presumably they are also interested in *why* the system functions the way it does. With the exception of a few vague references to the attitudes of the province's employers, the book does not address this question.

Gilson states that his aim was not "to develop a casual theory of strikes." (13) Nobody would question this decision. As Silver Donald Cameron explained in his *The Education of Everett Richardson*, a far more detailed account of the same Canso fishermen's strike that Darrah Belland describe in the opening chapter of Gilson's book:

It's easy to spin fancy theories about what the strike meant. It's all the easier because the strike was a classic strike, the kind of strike which almost seemed *designed* to substantiate a theory. But if we're going to have theories about it, they'd better be complex and sophisticated theories, because the way of life in which the strike had its roots is a great deal more complex and sophisticated than it appears to the casual eye.

Gilson, however, has gone further than this and

has "purposefully sacrificed explicit theoretical discussion" altogether. (9) His excuse is not that the theory is too simplistic, but rather that his audience is too broad. But surely if the material for a more sophisticated theory of strikes exists in this book, then the reader deserves to know about it. If, on the other hand, there are only the makings of casual theory, then, at the very least, the reader deserves to know why this is so.

I think that the latter is true and that the explanation lies mostly in the format of the book. Gilson sees the book as a "series of snapshots . . . which catch provincial industrial relations at their most vulnerable and revealing moments." (14) The problem with snapshots, however, is that they present a selective and static picture of society. In this particular album, the viewer is faced with nine different portraits of collective bargaining each containing images of employer intransigence, worker and community solidarity, and government interference, but is given no sense of how representative the portraits are or of how the images are changing over time. This difficulty might have been overcome if the editor had provided some historical context, but Gilson considers "even a review of the last two decades" of industrial relations to be beyond the scope of the book. (11) Alternatively, the contributors could have been given greater scope to formulate their own arguments and draw their own conclusions about each strike individually. Since neither of these options were taken, we are left only with snapshots. And while the images themselves might be easy to recognize, the connections between them and their relevance to the industrial relations system as a whole are not.

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Bryon Eastman, *Labour Market Theory and the Canadian Experience* (Don Mills: Harcourt Brace Jovanovich 1987).

LABOUR ECONOMICS CAN BE ONE of the

most exciting areas of study in the discipline. In part, this is a consequence of subject matter. Few students are not intrigued by the questions addressed in labour economics. Why unemployment and discrimination? Why are some rich and others poor? Do unions improve the quality of life for people in our society? Or are unions really, to quote Michael Parkin's widely used macroeconomics text, "villains?" Moreover, many questions addressed in labour economics not only provoke academic interest but they force students to address fundamental political issues as well.

The excitement of labour economics also arises out of the intense theoretical debate over the operation of labour markets. Since the early 1970s three competing theoretical frameworks, each reflecting a unique approach to social science, have been used in the study of labour economics. Today, the labour economist must admit that we just do not know that much; an admission which often excites the student since it opens up the possibility of new discovery.

A good textbook in labour economics must capture and convey this excitement. Unfortunately, Byron Eastman's new introductory labour economics textbook, *Labour Market Theory and the Canadian Experience*, fails on this score. This textbook is exclusively devoted to neoclassical labour market theory (which the author misleadingly calls classical theory). The work of Piore and the institutionalists, who believe that labour market economics should be based on theories of cognitive processes being developed in psychology and anthropology, is covered in a page and a half. Eastman then dismisses this approach in just two pages. Marxist theory is given even less space: one page summarized the theory, eight sentences are sufficient to "refute" the entire research program. Does Eastman not recognize that the neoclassical approach can also be convincingly — but wrongly — dismissed with equal brevity. This treatment of alternative theories might be forgivable in a technical monograph but certainly not in an introductory text.

Eastman's tendency to make strong and misleading claims for neoclassical theory is the fundamental flaw of this work. I recognize that subtle qualifications, if given prominence, can confuse a student. Ignoring all subtleties and qualification is still more problematic,

however, since it gives a student the wrong impression of what can and cannot be said.

The first paragraph of Chapter One is indicative of the problems with this text (and, more generally, with the way labour economics is sometimes taught in this country). Eastman begins, "Almost anyone with even the remotest contact with economics will tell you, the price of everything is determined by supply and demand." He then states: "This, of course, is true . . . of the labour market." (7) Students exposed to this statement would be surprised to discover that every few labour economists believe the demand/supply explanation of market price is true. To be sure many, including this reviewer, would argue that the demand/supply approach of neoclassical theory is a powerful analytical tool. Most labour economists, however, also recognize that one must *assume* that labour supply decisions are based on household utility maximization, that firms maximize profits subject to well behaved production functions, and that coherent market equilibrium outcomes exists, when conducting neoclassical analysis. These assumptions are not *true*, but instead fall within what Lakatos has *called* the "hard core" of neoclassical theory.

The importance of this methodological quibble is immediately seen when one acknowledges that labour economists in the institutionalist, Cambridge post-Keynesian, and Marxist schools have legitimate reasons for rejecting the "hard core" assumptions of neoclassical theory and replacing them with "hard core" assumptions of their own. Eastman gives no hint of these legitimate reasons.

Because Eastman ignores the possibility of reswitching and capital reversing in his lengthy discussion of production theory and derived factor demand, students using this text are not required to face the theoretical anomaly which led many post-Keynesians to reject the marginal productivity theory of wage determination. Because Eastman ignores the existence of pervasive uncertainty (although he devotes a full chapter to rational expectations), students will not realize that the very notion of equilibrium is problematic. Finally, because Eastman dismisses class as a "fuzzy" concept (fuzzier than utility?) students miss the insights of Marxist and radical economists who argue that labour relations systems and unemployment are neces-

sary features of a society based on wage labour and private appropriation of production for sale on the market.

Needless to say, students using the Eastman text will not be exposed to labour market theories that assume unemployment is the norm in capitalist society (that is, which assume labour is not scarce). This is most unfortunate. These theories suggest that social custom, trade unions, and ongoing conflict between capitalist firms and working people are most important than short run demand and supply forces in the wage determination process. Moreover, the associated applied research, with its attentions to the historical experience of real people, provides a useful contrast to the statistical approach typically used in applied neoclassical research. Students who are not exposed to these alternative ways of studying the labour market miss some of the most important contributions in labour economics.

Who, then, would be interested in this text? Instructors who choose, owing to the limited time available in an introductory course, to focus on neoclassical econometric research might find this book attractive. Not only does this text discuss Canadian econometric research but also the presentation is accessible to students who lack a strong statistics background. Research covered includes work on unemployment - insurance - induced unemployment, estimates of the Phillips Curve and the natural rate of unemployment (with and without rational expectations), and estimates of occupations wage differentials. The marriage of neoclassical theory and econometric research is probably more successfully consummated in this text than in any other of the same genre (for example, Addison and Seibert).

Still, this reviewer believes that students are not well served by this text. With its emphasis on tractable econometric problems (such as examining the relationship between unemployment insurance and the rate and duration of unemployment) rather than messier subjects (poverty, occupational segregation, income distribution, and the like) this text is unlikely to motivate students' interest in the "big picture" issues in labour economics. Moreover, and to repeat my main criticism, by stressing neoclassical econometric research and by dismissing crude characterizations of alternative theories, this text is likely to generate a dan-

gerous ignorance among the next generation of labour economists.

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Lynn McDonald, *The Party That Changed Canada*. (Toronto: Macmillan 1987).

THIS BOOK IS INTENDED TO WIN over the wavering, but it will only be consumed or touted by the converted. The story line is that the CCF-NDP has changed Canada for the better by pressuring government — usually Liberal — to enact social programs such as old age pensions, medicare, unemployment insurance, and family allowances. The party also gets the lion's share of credit for the creation of the CBC and for being the "best defender" of individual rights. The question that arises is: If the CCF-NDP has been so successful in accomplishing its objective while in opposition, why should it be given the reins of power? Wouldn't this spoil a good thing? The answer and the moral of this story is that the Liberals and Conservatives are untrustworthy manipulators whereas the NDP is reliable and good to its word. In this script, the older parties can do no good and the NDP does little wrong.

One by-product of the good vs. bad, right vs. wrong framework is the selective presentation of some facts. Deficits run up by recent B.C. Social Credit governments are condemned while the unprecedented deficits chalked up by Howard Pawley's NDP are ignored. Tommy Douglas's budgetary debt reductions in the 1940s and 1950s are "achievements," while debt reduction schemes by earlier B.C. Social Credit regimes and the Mulroney government are simply assaults on social welfare. The reader, in effect, is treated to a dose of social democratic realism, that is, propaganda. An example of a cardboard image is the characterization of Pierre Trudeau's 1983 peace initiative as but "diabolically clever as a public relations exercise." (92) In contrast, the NDP's foreign policy — including withdrawal from NATO — is lauded. Nowhere, in the book, however, do we learn that the CCF was so

caught up in the early stages of the Cold War that it expelled some sitting MLAs precisely because they exposed NATO.

To prove that the NDP can be relied upon as a government and not merely play the permanent, perpetual role of the social conscience party in federal politics, the author points to the accomplishments of provincial CCF-NDP administrations in Saskatchewan, Manitoba, and B.C. It is perhaps not surprising that she ignores recent and sometimes critical evaluations such as James McAllister's *The Government of Edward Schreyer* (1975). Rather, she cites a few provincial government publications and unpublished materials provided by past and present NDP cabinet ministers. Lamentably, she sighs, "There is no readily available work on the successes of the CCF/NDP provincially." (227) The myopic result is an insufficiently objective approach. Two Manitoba examples: The first NDP government is praised for changing the tax system so that those earning less than \$20,000 paid less in income taxes. But nowhere are we told that those earning over \$50,000 were also paying less. Thus, gains by the less fortunate were not made necessarily at the expense of the more wealthy. Social democrats are committed to reducing the inequality of condition of rich and poor. There is not a shred of evidence presented that CCF-NDP governments have narrowed the gap or that their performance on this score is any better than those of the other parties in these or the other provinces. Like governments elsewhere, and in opposition to a long-standing CCF policy, the NDP is keen to rely on and expand lotteries which, objectively, redistribute income regressively.

Manitoba example number two is a historical nugget subjected to an NDP revisionist analysis: The bad Conservatives are lambasted for banning French instruction from public schools (153) but, actually, this was done by the Liberals who reversed Conservative policy in 1916. Moreover, the policy was not rescinded by the NDP as implied by her account which is described as "worth retelling" but by Duff Roblin's Conservatives. Her claims that Franco-Manitobans who became professionals or civil servants "were assimilated into the English-speaking majority" and that "generations of Franco-Manitobans and Métis lost their French heritage" are insulting to Franco-



Manitobans and pure malarkey. French speakers did decline to 5 per cent of the population as she notes, but this was due to the impact of immigration rather than the government policy. The facts are that the legal prohibition against French-language schooling "was never enforced in French-Canadian communities" and that in the mid-1960s, long before the Manitoba NDP ever pronounced a party policy on French-language rights, "L'Association d'éducation des Canadiens français du Manitoba [claimed] . . . 65,000 of the province's estimated 85,000 French-Canadians . . . were still French-speaking." (*Canadian Annual Review*, 1966, p. 135).

It would be unfair and inaccurate to claim that this book is totally devoid of partisan self-criticism. Most of it comes from a feminist perspective. Ed Broadbent is faulted for not recruiting more women candidates in winnable seats; Lynn McDonald resents the party's preference for running women as sacrificial lambs in hopeless ridings. She also wants — contrary to party and CLC policy — to extend Canada Pension Plan benefits to homemakers. The culture of the party, until quite recently heavily imbued with the smoke-filled beer parlour odours of the union hall, is derided. This culture made the NDP too much of a "macho organization," (98) one that repelled rather than attracted women voters. She also calls on the party to fight more vigorously against pornography and claims, without evidence, the pornography (which goes undefined) is a threat to the physical security of women. McDonald is frustrated by the partisan aloofness of new interest groups such as the peace, women's, and environmental movements which she feels have more in common with the NDP than they do with the other parties.

Some of the more stimulating parts of this book are the reflections upon why the CCF-NDP has not done better at the polls. One reason cited is the affliction of false consciousness among rank-and-file unionists. But McDonald also feels that the party's candidates and its membership have been unrepresentative of Canadian society. Liberal and Conservative candidates like the local mayor and hardware store owner have been more positive models for voters than teachers, preachers, and social workers. (Yet, teacher Schreyer, preacher Douglas, and social worker Barrett all succeed-

ed). She also attributes much to the influence of the media. No country, we are told, that exposes itself daily to hours of American television has ever elected a social democratic government. What countries besides Canada and the U.S. are subject to such an influence is unclear; moreover, why such a barrier has been surmounted in three provinces is not reconciled with the analysis.

In addition to an overview of the party's history, McDonald offers an outline of its current public policy prescriptions for a healthier economy and more compassionate social policy. There is nothing new in the history to catch the eyes of party historians, although she makes some good use of old Hansards to capture the flavour of policy debates. In terms of economic policy, she ignores former NDP research director James Laxer's critique in *Rethinking the Economy* (1983) in which NDP positions are depicted as hopelessly outdated and woefully inadequate. One chapter sketches the history and performance of social democratic parties in Western Europe, Australia, and New Zealand. Her favourite model is Sweden. (Alas, there is no discussion of why pornography and NATO are tolerated by social democratic governments elsewhere.) Although she is keen to applaud the CCF-NDP fight for labour and civil rights and the Charter of Rights and Freedom, she is silent about the fact that the Charter is now being used to challenge many of organized labour's past gains such as collective bargaining.

All in all, from an NDP point of view, this book is "politically correct" and will lead a long life at the literature tables at NDP and CLC conventions. In contrast, this review is politically reactionary and does not acknowledge that the transformation of capitalist society into a socialist commonwealth is no easy task. It is a formidable and constant struggle. Politicians are easy, too easy, to attack. NDP politicians, like Lynn McDonald, deserve praise rather than criticism for working in a party that, despite its failings, is critical of the status quo and is committed in principle to improving the lot of majority rather than securing the interests of a privileged minority.

Nelson Wiseman  
University of Toronto

David Milne, *Tug of War, Ottawa and the Provinces Under Trudeau and Mulroney* (Toronto: Lorimer 1986).

DAVID MILNE HAS WRITTEN a very clear and cogent analysis of the conduct of relations with the provinces by the federal government during Pierre Trudeau's prime ministership, and a brief account of how those relations began to change during the first year in office of the Mulroney government. Milne's argument is that the policies of the federal government in most important areas were designed to strengthen Ottawa's hand vis-à-vis the provinces and to forge attachments between individual citizens and the national government at the expense of provincial authority. He examines, in turn, the rewriting of the constitution, energy policy, and the management of economic and social policy to demonstrate his thesis.

In the case of the constitution, Milne argues, Trudeau was insistent upon the inclusion of a Charter of Rights to provide a focus to tie individual Canadians to the central government, and on the protection of minority language rights while refusing any special status to Quebec. The National Energy Policy was designed to seize control of a key sector from the provincial resource owners, while measures like the Canada Health Act aimed to impose Ottawa's will upon a key shared-cost programme. At the same time the federal government began to withdraw from such programmes and substitute block grants to the provinces under the Established Programmes Financing Act, because they were refusing to accept Trudeau's priorities while claiming the lion's share of the credit for social policies.

At the end of each chapter Milne provides a brief look at the way in which Conservative policy has been evolving since 1985. He notes Brian Mulroney's more consensual, non-confrontational style in constitutional matters and the abandonment of the NEP as evidence of the end of an effort to direct Canadian society from the centre. The drive towards privatization and free trade with the United States he identifies as complementary elements in a move away from interventionist government towards market forces.

Reading Milne's book in the aftermath of

the Meech Lake accord this dilatory reviewer was struck by how much of his analysis has been borne out. If the prime minister really did turn to the premiers of Ontario and Quebec at the end of the meeting in the Langevin Block, and ask them each, "Well, have you got a deal?" then clearly the internal workings of federalism in Canada have been mightily transformed for better or worse since Pierre Trudeau's departure. And *Tug of War* goes a good way in helping to explain how that has occurred.

The strength of the book is the clarity and focus of the argument, which Milne uses to bring coherence to a great mass of information. If I have any complaint, it is only that sometimes I think that Milne exaggerates the desire and ability of political actors (even Pierre Trudeau) to pursue their objectives unswervingly in the face of conflicting pressures and contradictory goals. In my experience the evolution of policy is rather more messy than one would gather from this book. But by setting out his case so clearly and forcefully David Milne has given us much to chew over as we observe the evolution of the Canadian federal system in the years to come.

Christopher Armstrong  
York University

William K. Carroll. *Corporate Power and Canadian Capitalism*. (Vancouver: University of British Columbia Press 1986).

WILLIAM CARROLL ADDRESSES AN important analytical problem with reference to a large, carefully analysed body of data in a high quality book that should become the starting point for subsequent discussion of the development of a capitalist class in Canada.

This book adds to a large literature on the dominant capitalist class in Canada and the corporate organizations through which economic power is most visibly concentrated and expressed. The primary theoretical question is to account for the pattern of development. Is it properly explained by viewing Canada through the lens of dependency and under-

development or is Canadian corporate power largely the product of the emergence of an indigenous financial-industrial elite at the core of a national capitalist class, which takes its place in the international economy of advanced capitalism? Carroll defends the latter position and takes issue with others, particularly Kari Levitt, Tom Naylor and Wallace Clement, for whom Canadian capitalism has been largely *mercantile and financial in orientation with the elite occupying a subordinate or comprador position relative to a dominant American capitalist class*, which was allowed to control Canadian industry. Thus Canadian development was said to be deformed, distorted, and dependent on a foreign class.

The theoretical arguments are set out exceptionally well in the first three chapters. A cogent summary of the thesis of Canadian dependency is followed by a trenchant critique of both theoretical concepts and evidence. It is difficult to read this analysis and remain convinced that dependency theory comes close to an adequate account of capitalist development in countries like Canada. Nevertheless, it becomes evident that part of the dispute rests on the definition of industrial capital, which some dependency theorists equate with the manufacturing of finished products, in contrast with a definition based on activities that produce new use-values, including resource extraction, transportation, and utilities. Thus to some extent the argument as to the degree to which Canadian capitalists have been commercial rather than industrial depends on how these categories are defined. Nevertheless, Carroll is correct that the dependency theorists' answer is quite inadequate from the perspective of a marxist analysis of capitalism.

Carroll himself offers a succinct, yet sufficient description of a marxist model of capitalist development towards the stage of monopoly capitalism dominated by an integrated financial-industrial elite and characterized by the export of capital, such that capitalism becomes an international system. Finance capital is based on coalitions of capitalists, whose integration depends principally on either block share ownership, which provides direct, operational power, or the allocation of capital through financial institutions. Carroll's task is then to display such a process in Canadian history and to document his claim that the Cana-

dian economy is largely controlled by a Canadian financial-industrial elite. Utilizing secondary literature for the period from the 1870s to 1945, Carroll reports the appearance of a block of finance capital after which the Canadian capitalist class consolidates its power in the context of "the internationalization of capital." Foreign capital participates in the Canadian economy without making Canadian finance capital dependent, while Canadian corporations themselves are major multinational actors.

The primary empirical base for Carroll's conclusions is an analysis of share ownership and interlocking directorships from 1946 to 1976. I can do no more than indicate the broad conclusions of this elaborate analysis. Based on total assets, the top 70 industrials, 20 financial firms, and 10 merchandisers were selected at five year intervals. On other criteria, three dominant trust companies and ten investment companies were added to the analysis. Although he identified a stable core of firms throughout the period, substantial restructuring was evident, especially in the industrial sector. American capital controlled a slightly higher share of industrial capital by the end of the period, but this expansion was not based on the takeover of major Canadian companies and actually reached its peak in the 1950s. Indeed, when the analysis is brought up to 1985, the conclusion that American involvement was decreasing seems fully justified as Canadian capital increasingly penetrated the manufacturing and mining sectors and six of the top 70 industrials changed from American to Canadian control, including Bell and Inco, both companies with close ties to Canadian finance capital.

Carroll furthermore demonstrates patterns of corporate interlocking that point to a dominant block of finance capital under domestic control. A network analysis is used to map clique structure in which cliques of closely connected firms are related to other cliques through overlaps on their peripheries. In general the spatial representation of these relationships works well. Notably, a much higher percentage of domestically controlled firms than foreign controlled firms are clique members. By 1976, cliques centred on the major investment companies, while financial institutions served as points of interconnection

among cliques.

I believe that Carroll has defended credibly his thesis that Canadian finance capital exists as a relatively independent component of international capitalism. It will have to be left to experts in the analysis of ownership and networks to raise questions about the procedure of the study. My reservations refer to matters of emphasis or omission, not to the core of the analysis. Thus I found relatively little attention to the relationship of finance capital to the Canadian state and the degree of impact of state policy on the pattern of capitalist development. Although reference was made to the foreign holdings of Canadian companies as evidence of their participation in "capitalist cross-penetration," I prefer to see more data than increasing rates of foreign investment by Canadian multinationals. For example, it would be helpful to have an international comparison of trends in foreign investment relative to total investment. The concluding chapter begins to locate Canadian finance capital in world capitalism as an evolving structure, but this herculean task is sketched out rather than completed. Carroll succeeds in showing the inadequacy of dependency theory to account for class structure, but scarcely mentions another claimed consequence of dependency - that Canada imports a relatively high volume of manufactured goods and exports a relatively low volume, despite having a strong position in this respect in the late nineteenth century.

The strength of this book lies in elaborating the internal organization and processes of capitalist development within Canada from a perspective that looks outside it, if not in terms of dependency and underdevelopment. It is a most valuable contribution.

Peter R. Sinclair

Memorial University of Newfoundland

Charlene Gannagé, *Double Day, Double Burden: Women in the Garment Industry* (Toronto: The Women's Press 1986).

CHARLENE GANNAGÉ USES WORKERS' experiences in a small garment factory in Toronto to make a very important point — women's work experience is intertwined with women's family experience. This case study makes it clear that it is impossible to understand women's position in the workplace or union without taking into account women's role in the family.

The author criticizes Marxist work on the labour process for being sex-blind and for viewing women solely in economic terms. She is equally critical of some of the feminist literature for reinforcing a separation between women's domestic work and their paid work outside the home. Gannagé, by drawing examples from her interviews, argues that we cannot view the family of working women solely as an institution for women's oppression. The story of women at Edna Manufacturing, the shop under study, clearly shows that even though authoritarianism permeates their marital relationships, the family's help and support ease women's double day of labour. Moreover, for many of these immigrant women, family and ethnic ties provide the necessary network for sponsorship, finding friends and employment, and participating in cultural activities.

A very important part of the study is to show the intertwined nature of women's domestic labour and their work outside the home. "Women's working day was always interrupted for family responsibilities — whether it was to do the shopping during lunch hour or to answer the telephone if an emergency arose at school, or to take care of the sick children." (178) Women's domestic labour and family responsibilities have important implications for their social relations in the labour process and outside work. The striking result of women's double day of work is that men and women do not go through the same work experiences.

Gannagé criticizes the nature and organization of business unionism that has been instrumental in dividing male and female workers. She argues that favouritism, discrimination, and gender bias were common practice in the Cloakmakers' Union. The leadership has been dominated by skilled male workers whose policies tended to favour the men at the expense of the women in the union. Business unionism reinforced the ideology of women as economic dependents. This case study allows

Gannagé to bring out the complex relationship between women's double day of work, gender ideology, ethnicity, and their participation in union activities. In the Edna factory ethnicity was instrumental in deciding who would hold a union position. The double day of labour made it difficult for women garment workers to fully participate in union activities. Lack of familiarity with English was another factor which impeded active union participation of immigrant women.

Gannagé calls for an "overall systematic theory of women's work" (19), which realizes the complexity that arises from the interplay of the gender division of labour, gender ideology, business unionism, and the ethnic division of labour. In my view, this study succeeds in filling some of the existing gaps in the literature and poses important, critical questions essential for the understanding of women's work and the labour process.

It is important to emphasize, however, that we cannot understand women's work without understanding the industry within which women are working. To explain the nature of women's work we need to identify factors affecting the industry and the firm within that industry. The factors of family, ethnicity, gender ideology, and gender division of labour are necessary but not sufficient to explain women's work in the garment industry. For fully comprehending the change in union militancy, the nature of control, and the organization of work in the garment industry it is essential to look at the overall structure of this industry and the factors affecting it.

It must be underlined that firms in the clothing industry range from large, international establishments to small enterprises with less than 50 employees, like Edna Manufacturing. Most employees within the clothing industry are in large and medium-sized firms. The nature of the control of the labour process varies greatly among these firms. The "simple" form of control described in this book primarily characterizes the smaller shops. In the larger shops more sophisticated forms of control are at work. Apart from variation in the size of firms, clothing establishments can also be divided into inside shops and outside shops. The latter are operated by sub-contractors managing small shops or hiring homeworkers. Many clothing manufacturers in Canada use

homeworkers either directly or indirectly.

The combination of small and large firms, inside and outside shops, international competition, technological changes, and changes in the ethnic composition of the labour force have affected the nature of the labour process in the garment industry and the relationship between management and union. These factors are not dealt with in this case study. Neither does the author put the Edna Manufacturing plant in the context of the clothing industry in Canada as a whole or of the larger economy. An "overall systematic theory of women's work," if at all possible, must take into account differences among firms within a given industry as well as variations among industrial sectors.

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John J. McCusker and Russell R. Menard, *The Economy of British America, 1607-1789* (Chapel Hill: The University of North Carolina Press 1985)

JOHN J. MCCUSKER AND RUSSELL R. MENARD have provided scholars with a well-organized and sharply written study of the British American economy. They survey the colonial economy by topic, region, and methodological approach. By integrating their own research with an impressive array of secondary sources, they have created a work that synthesizes the current state of colonial economic history and directs scholars to profitable avenues for further study.

According to the authors, the traditional notion of deficit-laden colonies is wrong; generally, the colonies enjoyed a healthy economy which grew at faster rates than England and France. By adapting to market forces and imperial regulations free British Americans enjoyed a higher standard of living than their European counterparts. The authors link the prosperity of British America to participation in the international market economy. The "expansion of a staple export," they maintain "determines the rate of economic growth in 'new countries' or 'regions of recent settlement'." (20) The production of staples encourages investment in the commercial services and

domestic manufactures that facilitate the transport of goods to market. In turn, invisible charges such as insurance, freight, and credit outlays — which were often more important than the visibles — created a favorable balance of trade for many colonies. (Deficits not made up by the sale of commodities and invisibles were redeemed by the sale of ships in England.) McCusker and Menard believe that examination of the commodity trade and the “spread effects” of staple exports provides the best way for exploring the British American economy.

The authors recognize that the staples approach does not explain the entirety of colonial economic development. They offer an alternative method, a Malthusian approach that “locates the central dynamic of early American history in internal demographic processes. . . .” (18) Although they recommend some combination of the two methods, they maintain that the staples approach bears more fruit; the Malthusian approach only fills in the details that the staples approach cannot explain.

This work has its shortcomings. The authors are so intent on displaying the efficacy of the staples approach and the prosperity and contentment of Americans within the British imperial system that they dismiss or downplay aspects of the colonial economy that undermine their thesis. For example, they minimize discontent with regulation of the Indian trade, the resentment of mechanics against British manufactures and of merchants against the power of the sugar lobby and the confinement of trade to particular ports, the frustration of entrepreneurs with restrictions on manufacturing, and the widespread vexation over imperial interference with colonial issuance of paper money. There is no doubt that Americans benefitted from British mercantilism, particularly by protection of shipping and the bounty system, but widescale smuggling, the frequent calls for paper money, and the creative ways with which Americans avoided the Navigation Acts illustrate the great energy and effort Americans expended on escaping British regulations.

The most widespread form of colonial disobedience of British laws can be found in the favorite American pastime of smuggling. McCusker and Menard recognize smuggling’s importance in the seventeenth century but ar-

gue that “by the 18th century smuggled goods accounted for a tiny fraction of all quantities handled.” (77) The authors dismiss the comments of contemporaries on the widespread nature of smuggling and exclude illegal trade from their tables and equations on the colonial economy. Although it is difficult to quantify, it may be more hazardous to exclude estimates of smuggling than to ignore it completely, especially since more ships plied the trade between New England and Louisbourg than between New England and the Caribbean. John McNeil, in his *Atlantic Empires of France and Spain: Louisbourg and Havana, 1700-1763*, has creatively applied new methods for assessing the impact and extent of smuggled goods in colonial economies and has documented a large illicit trade between Jamaica and Cuba, as well as between New England and Louisbourg.

The staples approach does not always explain the growth of colonial economies. For instance, the authors show that the middle colonies, particularly Pennsylvania, had the most balanced economy. Philadelphia merchants opened new markets for exchange and thus induced the hinterlands to produce a greater variety of goods. This claim, however, can be contradicted by the tables of export statistics that the authors provide for the period 1768-1772. Except for New England (which had a far more diversified list of exports) and the West Indies (which was far less diversified), all of the colonial regions relied upon one or two commodities to provide between 72 per cent and 75 per cent of their exports. If only the variety of exports is considered, one could argue from their tables that the Lower South had a more diversified economy than the Middle Colonies. Thus, we need other forms of measurement to show that Pennsylvania possessed a better balanced economy than South Carolina. We need to reassess differences in the social and labour system, the importance of ethnicity and class upon economic choices, and especially for Pennsylvania, the importance of the exchange of goods and services between rural and urban areas.

The authors also challenge the large and growing body of evidence that shows that many subsistence farmers made economic choices based upon attitudes and customs rather than market forces. McCusker and Menard present

both sides of the argument fairly, but show exactly where they stand: "The fully sufficient yeoman farmer of colonial America is largely mythical: almost all colonists were tied to overseas trade." (10) Whether or not the authors are correct in ascribing entrepreneurial market behavior to American farmers or a staples approach to the British American economy, they have made a notable accomplishment, and one well worth arguing with. McCusker and Menard beckon (or perhaps challenge) scholars to formulate new methods for examining the colonial economy and addressing the larger questions of synthesis that they have wrestled with themselves.

Alan Galloway  
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Robert A. Rockway, *The Jews of Detroit: From the Beginning, 1762-1914* (Detroit: Wayne State University Press 1986).

ROBERT ROCKWAY'S GENEROUSLY ILLUSTRATED history of the Jewish community in Detroit is a useful example of the strengths and weaknesses of many works of this genre. By focusing on the evolution of Jewish religious, social, and cultural institutions, Rockway has provided a valuable study of some of the major pillars of communal existence between 1762, when the first Jews arrived in what was then a major fur trading outpost, and 1914, when the great wave of Eastern European Jewish migration to North America was curtailed by the outbreak of World War I. He tells us that by then the structure of institutions and social relationships between elements of Detroit Jewry had become firmly established. The Germans, who had arrived in mid-nineteenth century, had fairly quickly achieved financial success, and enjoyed a considerable degree of social integration into the non-Jewish community; they belonged to the Reform temple, spoke English, and looked askance at their East European "brethren." The latter, drawn mainly from the western domains of the Russian empire, the eastern provinces of the Austro-Hungarian empire, and Roumania, were gener-

ally poor, Yiddish-speaking, and Orthodox, if they were religiously observant (a feature which was by no means universal amongst them), or adherents of one or more Zionist or Socialist ideologies. Coming, therefore, from two radically different cultures, the German and Russian Jews, Rockway concludes, were never able to bridge the socio-economic gulfs and tensions that had arisen between them.

In many ways, this is an already familiar story, which was as broadly true in most of the Jewish centres of roughly equal size throughout the United States, with the minor differences reflecting local general circumstances. Rockway's treatment suggests, however, that there are some differences between aspects of the Jewish experience in Detroit and in other American centres like New York and Chicago and Canadian cities like Montreal, Toronto, and Winnipeg. For example, he makes the point that there were few Jews in the local labour movement because Detroit was a non-union town in the late nineteenth and early twentieth-century era. This seems odd in view of the fact that there was a substantial factory clothing industry in Detroit. Rockway in fact reveals that Jewish class structure in Detroit was fairly complex and that there existed strong Jewish union activity in the burgeoning local garment trades. According to the community directory of 1907, it appears that there was a sizeable Jewish working class but there is no discussion here of its being employed in the great clothing factories owned by German Jews. Nor does Rockway discuss this relationship, and the problems emanating from it, as a factor contributing to class and national tensions between these two segments of Detroit's Jewry. And he attempts no extended analysis of occupational and geographical mobility, a feature which distinguishes what is probably the best of the recent Jewish community studies, Steven Hertzberg's *Stranger Within the Gate City: The Jews of Atlanta*. Rockway argues, too, that Detroit's large population of Polish origin created a certain local atmosphere of anti-Semitism. Yet, apart from discussing one painful anti-Semitic incident, he does not explain the local economic and social factors that contributed to this phenomenon. What is equally jarring is the omission of any discussion of a major source of local anti-Semitism emanating from Henry Ford's *Dearborn In-*

*dependent: Ford's International Weekly* and its effects on Detroit's Jews during the early 1900s.

Gerald Tulchinsky  
Queen's University

Michael R. Weisser, *A Brotherhood of Memory: Jewish Landsmanshaftn in the New World* (New York: Basic Books 1985).

MICHAEL R. WEISSER HAS WRITTEN this book as much (perhaps more) for a general audience nostalgic about the past as for scholars. Consequently, he uses minimal documentation in the form of skimpy endnotes, gives short shrift to other scholars on the subject in a brief appendix, and relies heavily on what he called "*bubbe mayse* history," oral tales told by grandparents (defined and translated strictly *bubbe* means grandmother). "It is," writes Weisser, "a mixture of facts, anecdotes, fairy tales, fables, innuendos, truths, half-truths, memories, experiences, and even some outright lies." (ix) Such history undoubtedly can tell and teach us much that we fail to find in more traditional or conventional narratives and documents. But, as Timothy Garton Ash has reminded us (in a late 1985 review of "Shoah" in the *New York Review of Books*), the historian has an obligation to protect us against the vagaries of memory. "Memory," wrote Ash, "is treacherous. Memory is amoral. Memory is also forgetting. There are things that memory cannot look in the face.... So the historians are our protectors. They protect us against forgetting — that is a truism. But they also protect us against memory."

How good a guardian of the past has Weisser been? The answer to that question is not at all clear. Certainly, in part, he protects us against forgetting. His book is a welcome reminder that not all Jewish immigrants to the land of "white bread and chicken" acclimated well economically or socially. Weisser's subjects, the men and women who joined the *landsmanshaftn* — 100s of mostly small fraternal benefit societies based upon old-world villages of origin — consist of those Jewish

immigrants most averse to diluting any part of their cultural heritage. They were men and also women who preferred to converse in Yiddish, worship in traditional Orthodox synagogues, dwell in inner-city ethnic neighborhoods, work for themselves in tiny "mom and pop" retail establishments, and stay away from trade unions and political parties. They were men and women who were in the new world but not of it. Indeed, family, street, synagogue, and *lans-leit* seemed to encompass their entire world. They even chose to leave this world in old-world style. The most imaginative parts of Weisser's book describe the burial practices of the *landsmanshaftn*, which, he makes clear, were the societies' *raison d'être*. Born mostly in small villages scattered across the "Pale of Settlement" in Tsarist Russia, Weisser's subjects brought their village mentality and culture across the ocean to the United States, clung to it tenaciously, and went to their graves with old-world pomp and circumstance. These people will probably never find a more sympathetic or charitable historian. Weisser has done their individual and collective memories justice.

In the greater obligation of the historian to protect us against the vagaries of memory and to measure oral history (*bubbe mayse*) against the written record, Weisser proves less successful. His failure are several. His version of immigrant history remains shackled to the interpretation Oscar Handlin first presented in *The Uprooted* more than 30 years ago. If Weisser describes the old-world *shtetls* from which his subjects emigrated with images of poverty and filth that might shock those more familiar with the idyllic village of Broadway and Hollywood's "Fiddler on the Roof," his description of life in the late nineteenth-century Russian Empire is as static as Handlin's was. Weisser's Jewish villagers, like Handlin's "uprooted" immigrants, live in an unchanging world in which sons do as their fathers did and daughters inherit their mothers' roles. It is a universe untouched by the intrusion of capitalism or the influences of the world economy. Thus Weisser's people depart not because the economy and society are changing before their eyes nor because the "demographic transition" has created a growing gap between population and available resources, but because they are the victims of antisemitism and increasingly



violent pogroms. Certainly, there is a grain of truth in Weisser's explanation for the motivation of his immigrants. But that explanation leaves two questions unanswered. First, why did only a minority of the victims of anti-semitism choose to leave? Second, why did their non-Jewish neighbors choose in massive numbers to leave the same regions at the same time? Those questions have been answered satisfactorily by the generation of historians of immigration who have succeeded Handlin. The intrusion of capitalism and the world economy into the south and east of Europe coincided with a demographic transition which increased population in the Russian and Austro-Hungarian Empires. The result was a series of social and economic changes which altered the bases of traditional village life and caused millions of those affected by the transformation to opt for a better life elsewhere. Those who chose to emigrate, always a small minority of the total population in any region, were, according to most recent research on immigration, precisely those most affected by change and most successful in responding to it. There is no good reason to think that Weisser's Jewish immigrants were any different. The East European Jews who chose to emigrate were undoubtedly less traditional than the majority who remained behind. Yes, they were different in one essential respect from their "gentile" brother and sister immigrants, having no true land of their own to which they might return. Jews, as we know, were aliens almost everywhere in eastern Europe, which is why they were more likely than other immigrants to depart as family units. Yet, once in the United States, the Jewish immigrants behaved much as did the Roman Catholic and Orthodox immigrants from the same regions of Europe. *Contra* Weisser, the Jewish members of the *landsmanshaft* were not singular in their devotion to family, friends, and faith. In fact, they were much like the immigrants John Bodnar has described in his many books and articles on the subject. Like Bodnar's people, Weisser's Jewish immigrants lived largely for and through their families. Families struggled and laboured together to build a modicum of security for all members in a new land. In so doing, they clung dearly to tradition while adjusting hesitantly to new ways.

In short, this book works best as a serv-

ice for the children and grandchildren of Weisser's *lansleit*, who assimilated more fully to the culture of their host society and need to be reminded of the far different universe inhabited by their parents and grandparents. For historians, Weisser offers far less. His introduction suggests the datedness of his approach. Frankly, it seems hard to imagine a historian writing in the year 1985, as Weisser did, that "Most sociological and historical analyses of ethnicity deal primarily with the pace of assimilation from the perspective of those who assimilated." Or that it might be novel to suggest that the immigrants "came to a free land, but they did not arrive free of their past experiences." (8-9)

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SUNY at Binghamton

Dolores E. Janiewski, *Sisterhood Denied: Race, Gender, and Class in a New South Community* (Philadelphia: Temple University Press 1985).

THIS IS A STUDY OF GENDER, race and class in Durham, North Carolina and its agricultural hinterland in the years 1880-1940. It is about blacks and whites, both entrepreneurs and working people, less women's history than a study of community gender relations generally. Janiewski explores "racial, gender and class solidarity as competing forms of group and self-discovery," attempting to understand how in this unequal "complex six-sided negotiation," claims to sisterhood held, relatively, so little power. (5-6) Hers is not an argument against gender as a useful category of historical analysis, but an explanation of why in this particular conjuncture, the experience of womanhood was so fragmented by identifications through race and class, and class-based strategies for change were so weakened by unions' insistence that class issues be defined in male terms. (177) She concludes that through time the balance of power began to shift between the classes and the races, but that women "remained isolated from one another and from power on either side of the color line."

(176) Her challenge to historians and to theorists is to develop a way of seeing class exploitation, racial domination, and gender subordination which links "these experiences of oppression, as inextricably as they" are "bound together in daily life." (152)

Janiewski begins her study in the countryside, in the "factories without walls" where the raw materials for Durham's industries were produced, sketching the relationship between the capitalisation of agriculture, the narrowing of opportunities for agricultural proprietorship, and the growing numbers of rural workers, particularly women workers, available to take up urban manufacturing employment. Single women were less valued workers in agriculture than single men; wives left alone by death or desertion could not command either the authority or the labour force to remain on the land.

In the mill villages the power employers claimed from proprietorship extended to the bedrooms and the nurseries of the company-owned cottages and accepted race and gender hierarchies "offered ways to divide and control the labor force." (96) Alliances across class, race, and gender were curious and contingent. Black and white women were divided from one another as Christians in racially segregated churches, and from their men in class-based organisations which "located exploitation in the sphere of exchange" and not within the patriarchal household. But bosses and men allied to exclude women from desirable classes of work, and both "Negro women" and "white girls" made common cause with mill patriarchs against their male kin on questions of sexual morality and temperance.

Janiewski argues that through time the experiences of women and men in the market economy converged. The large number of waged jobs available in local manufacturing meant that family incomes were jointly contributed rather than garnered by a male breadwinner; female patterns of labour force participation resembled male patterns; for black and white women the "experience of class relationships in the formal workplace became increasingly similar." (116) The dilemma was that outside market relationships, the powers and responsibilities of women and men remained starkly different, unequal, and racially specific. Janiewski concludes that it was

this different experience of authority and obligation in family and intimate life which most separated Durham women one from another by race, which led them to deny that they were "sisters under their skins."

This conclusion is plausible, but less well substantiated than the subsidiary themes in the book. The problem is one common to all research which seeks to understand the power of knowledge from intimate relationships upon the social forms we have come to call material, market, or public. It is not only that our theory inadequately comprehends these simultaneous systems, and we are inclined to retreat to less adequate but more familiar paradigms which assume the primacy of class identifications. Theories of class relations are older and more elaborated than those which take the questions of primary social identification as contingent. Problems in class relations are also more readily researchable, and likely to remain so. This study depends upon the conclusion that black and white women were alienated from one another because the patterns which governed their family lives were different. Yet, Janiewski candidly acknowledges that the work is heavily slanted toward activities in public and work in the factory because "interviews between two strangers separated by race, class, age and sometimes gender were not likely settings for revelations about private lives." (6) Janiewski has done a wonderful job sketching the forces at work in the "complex six-sided negotiation" which governed social life in Durham, but at the core of the study is an evidentiary problem not hers alone and not likely to be soon resolved. Is class conflict central in the shaping of the social order (6), or like liberal journalists on a bad day, could it be that we are confusing what is most frequently offered up for public view with what is most important?

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Alexander Keyssar, *Out of Work: the First Century of Unemployment in Massachusetts*

(Cambridge: Cambridge University Press 1986).

AS INVESTIGATOR FOR THE NEW YORK Commission on Employers' Liability in 1909, William Leiserson had the opportunity to do extensive empirical research on the extent and impact of unemployment. He was among the earliest North American reformers to recognize the multiple personal, social, political, and economic consequences of "normal" levels of unemployment. He testified before the U.S. Commission on Industrial Relations:

The bottom of everything is unemployment. It is unemployment or fear of unemployment which keeps us from regulating trusts, from changing our currency system and from reducing our tariffs; it is the fear of the unemployed men that keeps our trade unions from organizing; it is the fear of losing a job that keeps a man from voting as he wants to vote; it is the fear of losing a job that jeopardizes a man's rights.

Alexander Keyssar takes us back to Leiserson's astute observation that "the unemployment problem. . . is basic in everything."

But not in all times and in all places, for unemployment has a history and *Out of Work* is the first major attempt to trace it. A handful of scholars have given us histories of thought, politics, and policies surrounding unemployment. But Keyssar attempts to get at the historical phenomenon itself through a study of Massachusetts, a state with unusually rich early records on the problem.

Keyssar's "first hundred years" of unemployment stretch backward from the Depression decade of the 1930s. The opening part of his account traces the beginnings of unemployment as part of the process of proletarianization which took place in Massachusetts between the 1830s and 1870. Using the ample labour historiography on ante-bellum Massachusetts, Keyssar traces the different paths toward dependence on wages taken by workers in the state's two largest industries: textiles and shoes. Urbanization, industrialization, and Irish immigration swelled the ranks of people who had few alternative support systems when opportunities for wage labour decreased. With the market economy dominating both work and consumption, what had been relatively benign "slack time" between tasks or jobs assumed a new meaning. Time became money for wage

labourers dependent upon cash incomes.

But "the social origins of unemployment" really stands as a prologue to the remainder of the volume, which deals with the period from the 1870s, when awareness of unemployment first surfaced, to the 1920s. During this time, which he calls the "era of uncertainty," Keyssar argues that unemployment and the threat of unemployment affected all workers and profoundly influenced working-class culture and life. "To understand how workers coped with joblessness is, in part, to understand how they lived."

To clarify the extent of the threat, Keyssar has devised a new measure of unemployment which he terms "unemployment frequency." The unemployment rate measures only the percentage of the labour force unemployed at any one time; Keyssar's unemployment frequency measures the percentage of the labour force which experiences unemployment of any duration over the course of one year. Unemployment frequencies of 20 to 40 percent and as high as 57 percent among trade union members during bad years dispel any illusions that unemployment affected only a small minority of workers.

Using unemployment frequencies Keyssar explores the dimensions of unemployment by age, sex, race, ethnicity, and location. Within the working class and particularly among "blue-collar" workers, immigrants were only slightly more vulnerable than natives, older men just as vulnerable as younger, and women in a particular industry just as vulnerable as men. Nor, except for Boston, did the size or growth rate of towns and cities have a strong bearing on unemployment frequencies. The largest variations in unemployment frequencies stemmed from differences in the industries in which the populations were employed. Keyssar examines a number of smaller variations and offers hypotheses to explain them (for example, the relative security of older men in smaller towns was a vestige of a more personalized social order in decline.) But his major finding is the almost random "democratic sharing of misfortune" throughout this period.

After establishing the extent of the threat of unemployment throughout the working class, Keyssar explores workers' responses on three levels: first, working families' methods of coping during spells of unemployment; se-

cond, trade union responses to unemployment; and third, the organizations of the unemployed themselves. Again, Keyssar finds little change between the 1870s and the 1920s. A small minority of workers received marginal help from union "out-of-work benefits," emergency relief committees, overseers of the poor, and private charities. Far more important were relatives, friends, and neighbors. Odd jobs, children's work, extra boarders, and debt to local grocers and shopkeepers made survival possible, but not without sometimes debilitating emotional strain.

One of the most provocative questions raised by *Out of Work* concerns the relationship of trade unions to unemployment. In the nineteenth century, the centerpiece of labour's campaign to deal with the problem was the eight-hour movement. Whatever the weaknesses of the economic argument, and they became manifest as shorter hours were achieved without appreciably reducing unemployment, the campaign was one which fostered values of mutuality and solidarity. In contrast, in the early twentieth century the AFL response took the shape of a variety of restrictive and exclusionist measures: immigration restriction, apprenticeship restriction, opposition to women working (and thus no support for organizing among them), regulation of lay-offs by seniority, and campaigns for "localism" in the awarding of municipal contracts. By fostering conservative business unionism, Keyssar concludes, "the problem of unemployment almost certainly had a more profound impact upon the labour movement than the labour movement had upon unemployment."

The third level of workers' response consisted of demonstrations by the unemployed themselves. Keyssar explores the impediments to organization of the unemployed and then follows the successful efforts of Morrison Swift and Urbain Ledoux, "two eccentric and charismatic men who seemed to appear out of nowhere to ignite the simmering discontents of Boston's unemployed."

Not until the final chapter do we encounter the history of social thought and social policy which has — until *Out of Work* — made up the bulk of the historical treatment of unemployment. As Keyssar deals with the discussions of economists, reformers, and social workers, the pace of historical change appears

to quicken visibly: only in this chapter does he mark significant change between 1870 and the 1920s. Here Keyssar sets the stage for the new political economy of unemployment of the 1930s and afterward: the success of the unions, reformers, and employment managers, Keyssar argues, finally put an end to "the era of uncertainty" in which unemployment was spread evenly throughout the working class. Seniority rules, internal labour markets, and new federal unemployment programs including expanded public works and insurance did not stop unemployment. Rather, they opened a new era of "managed unemployment" which helped to shift the burden onto particular segments of the labour force, while somewhat cushioning the hardship of unemployment itself.

In the context of the new working-class history *Out of Work* is a pioneering study. Ironically, however, considered in the context of early twentieth-century reform thought it is somewhat less so: parts of the book recall studies by William Leiserson, Henry Seager, Philip Klein, Frances Kellor (whose 1904 book was entitled *Out of Work*), and Mary Van Kleeck. These Progressives used empirical studies to show the extent of unemployment and to demonstrate its pervasive effects on the entire working class in the late nineteenth and early twentieth centuries. Their vantage point was different, of course, in that their studies were contemporary and lacked a historical dimension.

But even their ahistoricity has a parallel in Keyssar's book. While Keyssar explores historical change in his first chapter on pre-1870 proletarianization and in his last through foreshadowing the segmentation of labour following 1930, the bulk of *Out of Work* treats the intervening period in a way which is curiously static. Keyssar uses quotes, for instance, from 1921 to amplify census data from 1885. He appears not to need to explain change between 1870 and 1930 because little in his account, aside from social thought, has changed. This apparent stasis must be reconciled at some point with the generally accepted view that the period between 1870 and 1920 was one of dramatic transformation in virtually all other aspects of North American political economy.

A pioneering work, of course, cannot do

everything. One of its functions is to help point to new paths for further exploration: this Key-sar's creative and thorough study has certainly done.

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Stewart Bird, Dan Georgakas, Deborah Shaffer, *Solidarity Forever, An Oral History of the IWW* (Chicago: Lakeview Press 1985).

*SOLIDARITY FOREVER* PRÉSENTE LE TEXTE des entretiens avec d'anciens militants des *Industrial Workers of the World* à partir desquels deux des auteurs (Bird et Shaffer) avaient réalisé le film *The Wobblies* qui fut primé au New York Film Festival en 1979.

Mais Hyman lui-même n'était que le rouage d'un système économique plus vaste. Il avait aussi recours au crédit de ses fournisseurs de Québec et Montréal pour le moyen terme (de six à douze mois). Il obtenait de même, à chaque année, pour un plus long terme (douze mois et plus), des avances de ses créanciers européens à la livraison de ses premières cargaisons de poisson. De la sorte, l'économie gaspésienne s'intégrait au système économique international, celui du commerce atlantique britannique, mais, par son dynamisme interne, les divers aspects de l'action, du militantisme, de l'idéologie et de l'héritage des Wobblies dans l'histoire ouvrière américaine. Aucune des étapes essentielles qui constituèrent l'expérience Wobbly n'est absente. Dès lors le livre peut-être lu avec profit par les étudiants-es et le public non spécialisé. Les introductions, la bibliographie et les notes d'accompagnement permettent en outre au chercheur-e d'aller plus avant dans les questions qu'il-elle se pose.

Si dans l'ensemble la thématique historique utilisée n'est guère différente de celle qu'on trouve dans Joyce Kornbluh, Melvyn Dubofsky on même l'autobiographie de Ralph Chaplin, ce recueil apporte cependant des illustrations et informations supplémentaires sur l'ampleur et les manifestations de cet épisode flagrant de la lutte des classes et la

répression qu'il suscita. L'aspect le plus novateur consiste dans le fait que ces témoignages n'émanent pas des leaders, dont les faits et gestes sont connus, mais de militants de base, restés pour la plupart jusqu'alors anonymes. La recherche de la trace de ces personnes "ordinaires" a permis parfois de révéler l'implantation des IWW dans des secteurs peu étudiés comme celui des marins et dockers des ports atlantiques où Noirs et Hispaniques constituaient une part très importante de la main-d'oeuvre. En outre, ces témoignages permettent de répondre à certaines questions soulevées par l'histoire des IWW. On notera par exemple l'insistance avec laquelle les témoignages concordent pour montrer l'initiative de la police, du patronat ou du gouvernement (fédéral ou d'Etat) dans l'usage de la violence. De même le concept de sabotage est ici affiné par la définition qu'en donne Jack Miller pour qui il s'agit davantage d'un "*conscious withdrawal of efficiency*" que d'une incitation à l'action destructrice qui aurait même été légitimée par l'extrême dureté des conditions d'emploi. Et George Hodin ne récus pas que la lutte ait été motivée par des "*bread and butter issues*" dont l'amélioration nécessitait l'action héroïque. Autre élément intéressant: la position des IWW sur l'effort de guerre n'est pas uniformément décrite à travers ces récits. Ainsi le dockeur de Philadelphie James Fair rapporte que les IWW dans cette ville n'étaient pas opposés au soutien des Alliés: "Il y avait des hommes là-bas qui se battaient et risquaient leur vie à notre place, il n'était pas question de ne pas leur envoyer les armes et la nourriture dont ils avaient besoin". Malgré cette attitude les leaders locaux Ben Fletcher et Walter Nef furent arrêtés, jugés à Chicago et condamnés à des années de prison.

Au delà des informations historiques complémentaires que contiennent ces récits, le recueil met aussi en relief la modernité de la pensée et des formes d'action des Wobblies et souligne même la nécessité d'un retour aux valeurs qu'ils défendaient et à certaines de leurs stratégies. Plusieurs des témoignages recueillis mettent ainsi en évidence l'importance de la tradition Wobbly dans la mise en place du syndicalisme d'industrie qui s'institue avec le CIO (Roger Baldwin, Vaino Konga). Présentés par d'anciens militants actifs au cours des

années 1910, 20, 30 et au delà les IWW ne paraissent plus comme une moment unique de contestation sociale mais plutôt comme les initiateurs d'un profond mouvement de remise en cause du fonctionnement des institutions américaines tout au long du XXe siècle.

En effet la modernité de l'idéologie et des tactiques Wobbly est aussi mise en lumière par les thèmes autour desquels les réflexions des personnes interviewées s'ordonnent. La participation des femmes dans les luttes, l'enjeu communautaire des grèves, la solidarité à l'égard des groupes raciaux minoritaires sont autant de points caractéristiques de la tradition Wobbly dans lesquels les anciens IWW reconstruisent le développement de leur action lors des mouvements contestataires des années 60. Pourtant la continuité est aujourd'hui interrompue. Même si certains de ces témoins d'un mouvement ouvrier plus dynamique gardent bon espoir, nombreux parmi eux critiquent vivement l'AFL-CIO pour son auto-satisfaction bureaucratique, et le matérialisme et le cloisonnement, du mouvement syndical actuel. Du reste, Dan Georgakas affirme dans l'introduction que le renouveau syndical ne saurait s'instaurer sans un retour au principe Wobbly de l'autorité démocratique de la base.

Cependant, si l'expérience du syndicalisme industriel des IWW a sans doute constitué un lien qui permit la création du CIO; lors des mouvements des années 60, non issus du monde ouvrier, c'est plutôt l'inverse qui s'est produit. Les activistes ont a posteriori trouvé dans les IWW un modèle de radicalisme qu'ils présentent toujours comme une source de renouveau pour un mouvement social plus large, sans pourtant se demander comment ce modèle pourrait s'exercer dans les conditions sociales actuelles. Il est frappant de constater que les IWW ont souvent suscité chez les historiens une prise de position militante. Il suffit en effet de laisser parler les textes ou les anciens protagonistes du mouvement. Mais rares sont les ouvrages qui insèrent cet épisode de l'histoire syndicale dans la réalité socio-économique de son époque. La Nouvelle histoire ouvrière Gutmanienne, qui a produit par exemple tant d'études permettant de placer les Knights of Labor dans un contexte économique et social minutieusement décrit, ne peut-elle s'appliquer à l'étude des IWW? La geste Wobbly demeure certes exemplaire bien

que les temps aient changé, mais la nature du sujet est-elle vraiment à ce point actuelle qu'elle en éclipse l'historicité? Il ne saurait bien sûr être question de reprocher à un recueil d'histoire orale sa spécificité parfaitement légitime et ici particulièrement intéressante. Mais la démarche même nous semble caractéristique de la nostalgie admirative avec laquelle l'histoire des IWW est presque toujours contée.

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Kathy Peiss, *Cheap Amusements: Working Women and Leisure in Turn-of-the-Century New York* (Philadelphia: Temple University Press 1986).

AS A STUDY IN WOMEN'S LABOUR HISTORY and American cultural politics, Kathy Peiss's *Cheap Amusements* breaks important ground. Peiss's book focuses on "the trivia of social experience" in the lives of working-class women in the early twentieth century — their participation in New York's glittering array of commercial entertainments. If the work of Roy Rosenzweig has demonstrated the use of cooperative leisure by working-class men as a refuge from competitive individualism, Peiss's study points to another dynamic — the appropriation of commercialized leisure by working-class girls as a means toward individuality and self-expression.

Peiss's story has its roots in the reorganization of women's labour in New York City at the end of the nineteenth century — the movement of female workers into service and industrial jobs as New York became the nation's center for corporate industry. Unlike their nineteenth-century predecessors, domestic servants and sweated labourers whose task-oriented, household-based employments had followed the rhythms of women's traditional housekeeping chores, the department store saleswomen, factory labourers, and office clerks who formed the bulk of the turn-of-the-century female workforce sold their labour for

clearly demarcated periods of time — a circumstance, Peiss stresses, that many of them relished. Mostly young, single first- or second-generation immigrants who lived with their parents, these women gladly abandoned traditional “women’s work” for jobs which claimed them for specified periods but gave them the rest of the day to call their own. That free time was treasured and zealously guarded, for, as Peiss demonstrates, it was in leisure that working women sensed their best chance to exercise a measure of autonomy in dreary and limited lives.

By leisure these women meant heterosocial leisure: the “mixed-sex fun” and promise of romance held out by New York’s expanding network of commercial entertainments, its dance halls, amusement parks, cheap theaters, and nickelodeons. To young working-class women, the romance and sexual expressiveness involved in commercial recreation offered a degree of excitement, independence, and personal fulfillment that seemed otherwise inaccessible. Their experiences in the workplace reinforced that perception: there, gossip about “gentlemen friends” and descriptions of dances and theatre trips formed central conversation topics. The lure of heterosocial amusement was reinforced as well, paradoxically, by the women’s low wages, which compelled them to rely on men’s “treats” for access to entertainments. By actively seeking male advances and adopting an attention-getting, sexually suggestive manner, working-class women found a means to participate in commercial leisure. At the same time, Peiss asserts, they found and revelled in a flamboyant and expressive public style that horrified middle-class observers.

What emerged from the culture of the workplace, Peiss argues, was the flashily dressed, sexually assertive “spieler,” the working-class girl addicted to dancing, men, and good times. Using the reports of journalists and reformers, Peiss explores that working woman’s world, describing and interpreting her sense of fashion, her style of dancing, and her excursions to amusement resorts, vaudeville theaters, and nickelodeons.

The heart of Peiss’s argument lies in her insistence on the importance of those activities. Working women’s leisure was no mere grab-bag of fads and fancies; instead, it constituted

a pathbreaking cultural style. The flamboyant working girl may have appalled some members of the middle class, but she fascinated others, particularly those among the growing numbers of urban professionals who chafed under the restraints of bourgeois decorum. Amusement entrepreneurs drew in that audience by borrowing elements of working girls’ style — for example, their boisterous, sexually explicit dancing, adopted by a middle-class public in the 1910s in the tamer form of Vernon and Irene Castle’s “one-step.” By the 1920s, working women’s low-cut dresses, sheer stockings, and garnish make-up led the way in mainstream fashion, taken on by middle-class “flappers” as badges of modernity and independence. Turn-of-the-century working girls may indeed have been shaped by commercial culture; but, Peiss asserts, they in turn shaped that culture, influencing its course through their provocative conception of social freedom.

Yet that conception, as Peiss points out, carried with it enormous drawbacks. Young working-class women who flocked to amusement parks, movies, and dance halls sought self-expression through the pleasures of consumerism and romance. In so doing, they helped develop a formulation of feminine fulfillment that would constrain women of all classes through much of the twentieth century.

The great strength of Peiss’s work, however, lies in her refusal to dismiss these women as victims of “false consciousness.” While acknowledging that the freedom they sought in commercial amusements ultimately proved illusory, she insists that their pursuit of pleasure be itself taken seriously, as a revolt (albeit a haltingly articulated one) against lives limited by class and gender oppression. Defying parents and middle-class reformers, working women forged in dance halls and amusement parks a style that aggressively claimed the public sphere and the pleasures of sex for their own. Their triumph may have been hollow — certainly, the increased freedom of sexual expressiveness women gained in the twentieth century has meant little when unaccompanied by greater access to economic independence. But, as Peiss rightly insists, that should not detract from the value and the militance of working women’s claim.

Peiss’s beautifully descriptive book offers

a welcome change from too many scholarly works that dismiss the desire for pleasure as trivial and remain blind to the significance of cultural style. Yet, understandably eager to counter their viewpoint, Peiss has written a book that is itself one-sided.

In part, the problem is one of tone. Throughout her study, Peiss reminds her readers of the dangers of exploitation and harassment to which working women's pursuit of pleasure made them vulnerable. The tone of Peiss's book, however, downplays those cautionary words. Her frequent assertions of working women's "enjoyment" of their "free-and-easy sexuality" and "newfound sense of social freedom" lends her book a romanticized air that occasionally detracts from its analytical rigour.

A more serious problem lies in Peiss's use of her sources. She finds her window into working women's lives through the words of middle-class observers — in particular, social workers engaged in investigations of commercialized vice. Determined to read past what she sees as moralistic class bias, Peiss leaves unexamined even reformers' most substantive objections to commercial entertainments. Yet not all social workers viewed the amusements as havens of vice; many, like Pittsburgh's Elizabeth Butler, saw them as essentially harmless, even necessary in bring diversion to work-weary girls. What was objectionable, in Butler's eyes, was that the need for diversion could increasingly be met only through a cash transaction. As Butler wrote in her 1909 study of Pittsburgh's working women:

Nickelodeons and dance halls and skating rinks are in no sense inherently bad, but so long as those maintained for profit are the only relief for nervous weariness and the desire for stimulation, we may well reckon leisure a thing spent, not used. These amusements take a toll from the people's income, disproportionate to the pleasure gained. . . . A diversion is needed which shall be a form of social expression, and with slighter toll from strength and income, be of lasting value to the body and spirit.

Peiss, focusing on the liberating effects of commercial nightlife, gives such arguments little attention, a lack of balance that weakens her book's force. Strong as her argument is, it could only have benefitted from a more even-handed, evaluative perspective.

Those drawbacks, however, do not diminish the overall importance of Peiss's study. In its imaginative use of unconventional sources, the book is lively and illuminating. And in its respect for the integrity of working women's choices, the book provides a model for future historians of women, consumerism, and mass culture.

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Susan Porter Benson, *Counter Cultures: Saleswomen, Managers, and Customers in American Department Stores 1890-1940* (Urbana: University of Illinois Press 1986).

BUMPER STICKERS PROCLAIMING 'Born to Shop' and 'When the Going Gets Tough, the Tough Go Shopping' suggest, only half humourously, that shopping is one of the major phenomena of modern life. Despite its ability to engage large numbers of people as workers and clients and its significance for the gross national product, shopping and its counterpart, selling, have received little attention from scholars in any discipline. In response to the steadily increasing interest in women's history this neglect is changing. An employment that enrolls a large part of the female white collar labour force and an activity that consumes the time of a legion of female customers are at long last receiving the attention they merit. Susan Porter Benson's *Counter Cultures* makes a major contribution to our understanding of the evolution, the dynamics, and the contradictions of life within the American department store between 1890 and 1940.

Chapter One outlines the appearance of this 'New Kind of Store' with its diversified merchandise and services from 1850 to 1890. In these years Porter Benson locates the "seeds of conflicts. . . among managers, customers and saleswomen." (7) The effort to create a 'palace of consumption' brought in its wake a set of contradictions that engaged all who came into its confines. No one group held absolute control over the environment they shared, although each attempted to maximize their influence over the others. Shifting



alliances made the department store a vital arena in which the consequences of class and gender were spelled out on a daily basis in ways that could be both contradictory and reinforcing. Chapter Two examines the attempts of department store managers to rationalize their firms. Here the public nature of their operations and the particularities of sales left them in a significantly weaker position than contemporaries attempting to transform factories along the lines of scientific management. Chapter Three with its discussion of managers' efforts to mobilize and manipulate an ever larger pool of female clients illustrates the strategies by which urban middle-class women were wooed as consumers not only of necessities but of frivolities. As Chapter Four indicates, the critical link to this female clientele was an increasingly female labour force. In contrast to industrial workers who often were encountering a determined effort at deskilling by their employers in these years, 'shopgirls' were transformed through elaborate training and welfare programmes in to a professional salesforce. Chapter Five points out that saleswomen faced a better future than many of their waged contemporaries until the very end of this period when their advantages tended to deteriorate, in large measure in response to the growing use of part-time competitors. And finally, in perhaps the best chapter of all, the culture of this female workforce is detailed as a rich series of exchanges among workers, workers and managers, and workers and customers. Working lives were far from simple, reflecting as they did a complex blend of gender and class solidarities. Porter Benson provides us with a brilliant portrait of a "work culture" that "expressed the saleswoman's three identities — worker, woman, and consumer." (9) The result is not always an easy book to read but it is essential for anyone attempting to grapple with the nature of women's complex lives.

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Alan M. Brandt, *No Magic Bullet: A Social History of Venereal Disease in the United States Since 1880* (New York: Oxford University Press 1985).

ALAN BRANDT'S THOROUGH ACCOUNT of the debate over venereal disease in America between 1880 and 1980 will, given the current public preoccupation with AIDS, be guaranteed a wide audience. Potential readers should be aware, however, that the title of the book is somewhat misleading. Brandt has provided not so much a social history of venereal disease and those whom it afflicted as a painstaking analysis of the public discussions which its fear elicited. It is thus in some ways representative of the new crop of texts in the social history of medicine, providing a rather cursory account of the scientific study of specific diseases for the purpose of better understanding the cultural context in which such investigations took place.

The two most dreaded venereal diseases, syphilis and gonorrhea, were for centuries neither fully understood nor differentiated. In 1837 Philippe Ricord established that they were in fact two distinct diseases; Rudolph Virchow subsequently tracked syphilis's transference through the blood stream to the internal organs and determined that it was a systemic condition. In the late nineteenth century Alfred Fournier revealed to frightened readers the extent to which even "innocent" women and unborn children were subjected to the ravages of syphilis. Much of the sterility, miscarriages, neonatal blindness, insanity, and paralysis of the late 1800s was traced by medical scientists to sexually transmitted diseases.

No effective therapy for such complaints was available in the nineteenth century; many thought that none but punitive treatments should be sought for what could be regarded as just punishments for moral transgressions. One doctor suggested that a genital complaint that resulted in the curvature of the penis be dealt with by holding it "with the curve upward on a table and [striking it] a violent blow with a book. . . and so flattening it." But by the beginning of the twentieth century successful therapy for syphilis (the disease that receives most of Brandt's attention) was made available. In 1905 Fritz Schaudinn and Eric

Hoffmann identified the causative agent of syphilis, the *Spirochaeta pallida*. In 1906 August Wassermann announced his success in devising a diagnostic test for the disease. The therapeutic coup came in 1909 when Paul Ehrlich discovered Salvarsan, the first effective treatment for syphilis. It was, he hoped, the remedy which would trigger the body's antibodies which like "magic bullets" would destroy the invading microorganism. The complete annihilation of syphilis appeared to be promised in the 1940s when a group of Oxford researchers produced penicillin.

Such a satisfying account of the ultimate triumph of science over disease was what one usually expected in most histories of medicine. But Brandt uses this chronology of medical advances merely as a backdrop; his chief interest is in the way in which concerns about venereal disease have been inextricably tied to social and cultural values relating to sexuality, gender, ethnicity, and class. Fear of disease has been repeatedly used by moralists, he successfully demonstrates, as a means of social control.

Prior to World War I vice crusaders began playing up the stigmas and taboos associated with v.d. to turn public concerns to moralistic purposes. Middle class commentators, preoccupied by their endangered social position, asserted that deviant behavior was symptomatic of disease. Accordingly they attacked as carriers of v.d. those whom they feared — migrants, urban workers, and women who violated appropriate gender roles. Brandt's sympathy obviously goes out to the dedicated physicians, birth controllers, and sex educators who attempted to counter such hysteria and "attacked the conspiracy of silence" sustained by the moralists. Many an expert was also a moralist, however, and progressive reformers of both stripes were linked to the Rockefeller-funded social hygiene movement.

The conflict between the "uplifters" on the one hand and the medical scientists on the other had serious national significance at the time of America's entry into World War I. The vice reformers, headed by Raymond B. Fosdick went off to Canada in 1917 to see how an army could be kept out of the arms of prostitutes. Fosdick returned with the message that red-blooded American boys could be continent and

so protected from v.d. if provided with distractions in the form of athletics and recreation and if prostitutes — likened to mosquitoes carrying yellow fever — were eliminated. Thirty-two American states ultimately passed compulsory medical inspection laws that resulted in over 18,000 women suspected of having venereal disease being forcibly confined and treated. Once in France, however, the American military came to the conclusion that practical preventive methods had to be employed even though the moralists might howl that this was tantamount to condoning vice. It was a simple fact that the New Zealand army with its "dangle parades" and provision of condoms had proven the effectiveness of prophylaxis. When informed of such reports Secretary of War Baker's shocked responses was, "For God's sake don't show this to the President or he'll stop the war."

Shocked or not twentieth-century Americans increasingly accepted a more instrumentalist attitude to venereal disease; efficiency was to be valued as much as if not more than morality. But as Brandt points out the battle between the two points of view had to be fought out almost every decade — in the New Deal era over the use of clinics, in World War II over the confinement of prostitutes in "civilian conservation camps," and in the 1980s over the appropriate response to AIDS.

The great value of Brandt's book is that it reveals the ways in which generations of moralists have presented venereal diseases as a sign of their victim's guilt. Hence the concern for "innocent" sufferers which implies that the so-called "guilty" deserve their fate. Those preoccupied by the need to regulate public and private conduct have repeatedly argued that in fact venereal disease is only a *symptom* of far more dangerous evils — promiscuity, perversity, bad behavior. This stress on individual responsibility has served to shore up the pretensions of the "respectable" but failed to stem the tide of disease in the past and will just as surely fail in the future.

If the villains in Brandt's account are the moralists, the heroes are the dispassionate, rational, medical scientists. Problems arise, he argues, when the moralists "obstruct medical efforts." But in such a game medicine always wins: if a doctor is wrong he is acting like a

moralist, if he is right he is acting like a medical scientist. Presumably medicine was therefore not responsible for the Tuskegee Syphilis Study that ensured over a 40 year period that 400 black sharecroppers would never receive treatment for their infections.

The one real weakness of this otherwise excellent study is that Brandt rarely views diseases other than as discrete biological entities. This allows him to subject the mouthings of the moralists to savage critiques, but does not provide him with a vantage point from which to subject scientific medicine to an equally searching analysis. He acknowledges at the outset that social or external determinants of disease and health exist, but in this study illness is almost always viewed from the doctor's point of view, rarely from the patient's.

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Melvyn Dubofsky and Warren Van Tine, *John L. Lewis: A Biography* (Urbana: University of Illinois Press 1986).

READERS OF LABOUR/LE TRAVAIL need no introduction to *John L. Lewis: A Biography*. Published in 1977 by quadrangle/New York Times, it quickly gained recognition as an outstanding biography. The authors' exhaustive research, mastery of complex issues, and pungent writing produced one of the most genuinely useful and intelligent books in the labour history field. Informed by recent developments in labour and social historiography, the authors brought a new dimension to "institutional" labour history. *John L. Lewis* reflected the post-Vietnam/Watergate skepticism of beneficent government and of heroic styles of leadership, while avoiding the errors of presentism. The John L. Lewis who emerged from the book's 529 pages of text was a secretive, power-obsessed man, at once deeply flawed and heroic in dimension. In the 1930s, his boldness and brilliance rebuilt the American labour movement. Yet throughout his long tenure as president of the United Mine workers he crushed democratic governance, surrounded himself

with yesmen, and led the union to the brink of its sordid and violent career in the 1960s. Through 50 years of labour conflict, part of Lewis remained detached, above the battle, brooding on the vagaries of human folly and fearful for the fate of the republic.

Lacking the sources and inclination to probe very deeply into Lewis's psyche, Dubofsky and Van Tine found their theme in their subject's long record of involvement with the federal government. For a half-century he was alternately beneficiary and victim of government actions in economic and labour affairs. He rose to power in the wake of federally-supported UMW growth during World War I. His first test as president came in 1919 when a federal injunction and governmentally orchestrated surveillance and harassment wrecked a soft coal strike. Through the dismal 1920s, a now-beleaguered Lewis sought to use the government to rescue his crumbling organization. The long record of mutual exploitation between Lewis and Franklin D. Roosevelt, extending from the dawn of the New Deal almost literally to the latter's death in 1945, is part of the folklore of the twentieth century. Lewis's intricate and venomous confrontations with Harry S. Truman are hardly less fabulous.

In contrast, the authors paid little attention to the coal miners themselves. The reader looks to *John L. Lewis: A Biography* in vain for detailed information on the lives of miners and their families, whether on or off the job. Nor do we gain specific insight into the nature of the relationship between Lewis and his members. Lewis — and the public Lewis at that — is properly at the center of the book. The theme of the meshing and clashing of two powerful forces — Lewis's vague but profound determination to lead an independent mass labour movement and the relentless tendency of the modern state to subordinate all corporate groups — provides the conceptual thrust.

Yet *John L. Lewis* is not a thesis-driven book. The authors' interpretive scheme is implicit and episodic. It never interferes with their telling of the Lewis story. Their fresh and detailed examinations of 100 controversial episodes can stand alone. Correcting other scholars here, exploding hoary legends there, Dubofsky and Van Tine bring to bear the

powerful artillery of disinterested scholarship to clear away the debris left behind by myth-makers, notably Saul Alinsky. If the portrait of Lewis that emerges seems sometimes too credulous of Lewis's superiority in judgment, if his adversaries in union and public life seem always to come off second best in their encounters with the Mine Workers' chief, the authors have earned the right to be sympathetic. Lewis's many sins are compendiously catalogued and the jaundiced or patronizing views that Dubofsky and Van Tine take of such adversaries as Philip Murray, FDR, Truman, and Walter Reuther are delivered without malice.

The abridged edition of *John L. Lewis* is still 377 pages long. It is neither a streamlined, sharply focused biography in the Little-Brown tradition nor a full scholarly publication. It retains the original edition's perspective and judgments, much of its language, and all of its judiciousness and intelligence. The authors have excised the footnotes and have omitted the bibliographical essay. They have condensed the text, rectified minor errors, and dropped some (but by no means all) historiographical disputation. The authors' purpose herein is to make *John L. Lewis*, as it was conceived and written in 1977, available to a new audience at an affordable price.

Noting that few new archival sources have appeared since 1977, Dubofsky and Van Tine defend convincingly their decision to forego additional archival research. Less defensible is their decision to ignore new publications dealing with coal, politics, and the labour movement. While no doubt the story that they tell holds up in broad outline, a number of works have appeared that would clarify, extend, and illuminate their portrait of Lewis and his times. Keith Dix and Alan Jay Singer, for example, probe the structure of work in soft coal and connect changing work structures with Lewis's quest for power in the union. James Johnson's monograph on soft coal legislation, work by Paul MacEwan and David Frank on Cape Breton miners, Nelson Lichtenstein's important book on the CIO during World War II, works by John Hevener and David Corbin on Harlan County and West Virginia, respectively, Dorothy Schwieder's book on Iowa coal mining communities, Joseph Gowaskie's re-assessment of the dynamics of unionism in the anthracite region, Carlton Jackson's mordant catalogue

of the carnage in the coal mines, John Gaven-ta's brilliant analysis of the UMW political legacy — surely this solid body of recent work merits at least a note of update in a book aimed at the audience that Dubofsky and Van Tine target. This is especially true in that in the 1977 bibliographical essay to which readers of the current volume are directed the authors lament the paucity of published material on many of the subjects dealt with in this recent literature.

The abridgement and reissue of *John L. Lewis* excites reflections on the historian's project. Exhausting the available sources and sensitive to the broad implications of their subject's activities, Dubofsky and Van Tine accomplished much. Shunning metaphysical speculation and psychoanalytic temptations, they fixed Lewis firmly in the context of twentieth-century American experience. They brought a major public figure out from under a tangle of myth and obfuscation. They set the record straight. They raised important questions about the implications for republican values raised by the rise of the national security state. In clearing Lewis of the demonological taint so long surrounding him, Dubofsky and Van Tine permitted their readers to see the structural and institutional forces that shaped the great episodes of labour-government cooperation and labour-government conflict that Lewis so dramatically personified. Despite the lamentable lack of updated bibliography, scholars must applaud the appearance of this version of *John L. Lewis*. It is rare that a labour leader has been so admirably served by biographers. It is heartening that a new generation will be exposed to such a splendid example of the historian's craft.

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John R. Munkirs, *The Transformation of American Capitalism: From Competitive Market Structures to Centralized Private Sector Planning* (New York and London: M.E. Sharpe 1985).

THIS IS IN MANY WAYS A curious book, writ-

ten at times as though it were intended as an elementary economics textbook, yet in a prose too jargonistic to be of utility to the audience normally intended for textbooks; it also advances an important and convincing argument on the degree of economic concentration in contemporary American capitalism. If the book is of value, it is for the latter dimension.

The author traces the development of American financial and economic institutions in the United States from the late nineteenth century through the 1980s, examining both the theoretical and the empirical dimensions of American corporate history during that period. The main theses of the work are several: first, that in spite of the evidence, students of American economic history continue to be taught that the American economy actually does resemble Adam Smith's conception of a competitive market structure and that they do so in part because economic ideals are invested with quasi-religious overtones which lead people to believe what they wish to believe regardless of reality. Second, Munkirs rejects the opposing contentions that economic forces are anonymous in nature or that there is a conspiracy among those who wield economic power to limit the participants. Munkirs nonetheless concedes that corporate leaders have created an institutional structure that promotes industrial cooperation not competition. Third, in examining the concentration of economic power, he places more emphasis on indirect than on direct interlocking corporate relationships, and in this sense differs from the classic 1913 account by Louis Brandeis, *Other People's Money*. The author introduces as his main thesis the idea that there is Centralized Private Sector Planning, with an identifiable tableau of corporations involved in the process, as well as secondary corporations controlled by the main firms. This centralized private sector planning is thus, according to Munkirs, the result of a non-conspiratorial evolutionary process.

Munkirs is not content to be descriptive and analytical without advancing some prescriptive notions. He notes, quite rightly in this writer's view, that centralized private sector planning has serious implications for the functioning of democracy, although critics of American capitalism have been expressing these concerns for a century. To Munkirs, the criti-

cal issue is the reality of centralized power itself, not the fact that that power resides in the private sector. He suggests that the answer is to "begin creating institutional structures that allow all citizens to take part in molding their own destinies." Precisely what those institutions might be, Munkirs offers no clue, nor does he suggest what would enable such institutions to possess the economic and political power to create countervailing influence against the Central Planning Tableau. Few would quarrel with the suggestion that there is a need to "reconcile the dichotomy between economic beliefs and economic reality," and to "take a long, hard look at the economy's dominant structural and functional characteristics." Yet, there is nothing here that would hint at the means by which actual change could be effected.

The main contributions of this book lie in the restatement of a critical perspective on American corporate capitalist development and in the author's painstaking effort to provide extensive empirical data on the extent of corporate power and influence. The evidence he provides also suggest convincingly that interlocks have increased significantly even since World War II and certainly beyond the level of corporate concentration that gave rise to the first period of extensive analysis and criticism in the Progressive era. During a period of sanctionious deregulation in the United States, it is well to have authors such as Munkirs remind us of the economic reality that underpins the American dream of individualism and free competition. Unfortunately, the heavy prose of this book, with its jargon laden phrases will have difficulty reaching the wider reading audiences that need to be better informed on these issues. There are simply too many "informational conduits" and "intra-core interlocks" to salvage basic English prose. In the process, the author weakens what was otherwise a valuable contribution.

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Peter Wyncoll, *The Nottingham Labour Movement 1880-1939* (London: Lawrence and Wishart 1985).

PETER WYNCOFF, AUTHOR OF TWO BOOKS on Nottingham labour from Chartism to World War II, was a trade unionist by profession who never enjoyed the comparative luxury of the academy. Born in 1939, he was structured as a young boy into the practical stream, dropping out of school at age 15. Through Labour Party and Mature State scholarships he was nevertheless able to attend Ruskin College and the University of Hull in the 1960s. During the 1970s he had the support of his organization, the National Union of Public Employees, to pursue part-time doctoral studies at the Open University, writing a dissertation on Nottingham between 1880 and 1939, the manuscript that became this monograph following the author's untimely death in 1982. Wyncoll's supervisor, John Saville, his colleagues at NUPE, and his widow, Wendy Wyncoll, collaborated in bringing an important thesis to publication by Lawrence and Wishart: a task for which sincere congratulations to all from this journal are in order. Amidst the contemporary British crisis in the universities, the trade unions, and the public sector, this kind of scholarly and human story is bound to become even rarer; Thatcher, let it out during the recent elections that she favours the reintroduction of some form of the infamous eleven-plus examinations. The British system, which has endured a century of economic decline, class struggle, and all manner of political initiatives, including five Labour governments since 1923, seems destined to last for some time yet. Studies such as this one, focussed on the political dimensions of the labour movement in a single town, help explain why.

Nottingham is well known for its lace and hosiery, its original "staple trades," Raleigh cycles, Boots drugs, and coal mining, a more lately-developing staple in the nineteenth century. Colliers alone, Wyncoll estimates, formed about 10 per cent of the electorate in this mixed industrial area at the time of World War I, and it is worth noting that this field would be one of the few to survive the worst depredations of what Welsh economic historian J. S. Williams calls the "soft-shoed and civil assassins" of the National Coal Board in a later epoch. A combination of geological, sociological, and historical factors account for the relative "moderation" of local coal-mining communi-

ties over the last century, laying the groundwork for the Notts miners' revolt against King Arthur Scargill and the National Union of Miners in 1984, a development that Wyncoll could have easily, if without enthusiasm, predicted. Nottingham is also famous for a remarkable array of individual figures of significance to British labour history, and the interplay of character and circumstance is important to the Nottingham labour past. Nottingham's best-known radicals — the mythical Ned Ludd and Fergus O'Connor (England's only Chartist MP) — are found mainly on the other side of the historical divide of the mid-Victorian compromise. Henry Broadhurst and employers A. J. Mundella and J. E. Ellis were major architects of Liberal-Labourism in late-nineteenth century Britain, and Gladstone's followers in Nottingham look, and were, progressive by comparison with some of the town's twentieth-century notables, like George Spencer, "blackleg of the worst order" and leader of the so-called non-political labour movement rooted among the defeated miners between 1926 and 1936.

Nottingham, never regarded as promising territory in labour's forward march in England, was a town whose slum-dwellers were for generations as solidly Conservative as the labour aristocracy and mining community was Liberal. (In a 1906 internal ballot on affiliation to the LRC, the 23,774 members of the Nottingham Miners' Association returned only 1,806 votes in favour of the idea.) A rainy election day could decide the outcome in apathetic local government contests, and the mainstream leaders of the Labour Party after 1918 represented an uninspiring coalition of "dangerously insular, smug, and bureaucratic" trades unionists (189), as well as a rag-tag-and-bobtail of middle class ex-Liberals. Wyncoll, who has a very clever way with quotations, finds in 1931 a militant who claimed that "90 per cent of the Socialist leaders of the City were businessmen misleading the masses and obtaining their money by a system which they denounced from the platform"! Nottingham's Labour MPs in the post-Liberal epoch included G. W. Holford-Knight, a barrister Socialist who took the trades unionists' capitulationist policies only to their logical conclusion by defecting with Ramsay MacDonald in 1931, and Arthur Hayday, a representative of the

miners who remained loyal to the party in 1931, but who deserted his constituents in 1926 on grounds that the miners "were not trade unionists in the general sense. They were ignorant of the position. They lived in villages and thought in the mass." A rather more attractive figure than these Labour partisans was Nottingham's popular interwar Tory MP, Lord Henry Cavendish-Bentinck, whose party at least stood for the protection of threatened home industries (at that time), and whose 1918 book, *Tory Democracy*, called on Britons to "resist the plutocracy" who were conspiring to turn the Empire into a "bagman's paradise." Yet it was in this very same Nottingham where the ILP in 1895 boasted 1,000 members and once captured 19,500 votes for the local School Board, where radical Labour Churches and the William Morris Institute were very active, and which shocked the British press by hosting the Communist parliamentary candidacy of Tom Mann, polling a respectable 2600 votes in 1922. During the Nine Days in 1926, Nottingham workers were briefly freed from the dead hand of entrenched leadership and parliamentarism, took matters into their own hands in numerous clashes with "Fascisti," and, at one Labour Party meeting, "went absolutely mad [forming] chains singing the 'Internationale' and the 'Red Flag' as they marched round and round." The success and militancy of the General Strike, 'even in Nottingham', lends credence, if more evidence was necessary, to the general Marxist interpretation. The "most pitiful story" of the TUC's surrender, as it was remembered by one of numerous individuals interviewed by the author, meant that, "The strike was a tragic defeat for the workers of the town, and the repercussions of the failure were to scar the local labour movement so deeply that the marks remained for many years." (207)

While respectful of the role of a parallel leadership in Nottingham, from Hyndmanites in the 1880s to the Communists in the 1930s, Wyncoll resists the temptation of posing simplistic vanguardist solutions as a historical alternative for Nottingham or British workers. Emphasizing their educative function, he is forced to confront the reality of the cultural barriers they strived to overcome. Doubtless his own too-brief career as a democratic-socialist activist and trade union organizer sen-

sitized the author to the enduring presence of "a web of controlling influences" over the British working class of which reformism (a word he purposefully does not use) and sectionalism were the expressions. Wyncoll concludes what was the second chapter of his opus on Nottingham labour on what must have been, however, a characteristically optimistic note: the survival, against long odds, of a "militant and articulate section" of the labour movement "who appealed to the rank and file sufficiently to count many thousands amongst their supporters" during the 1930s, which was equally important as the war itself in producing a clearer Labour programme and the Labour landslide of 1945. It is a pity that later chapters of the story will be unwritten by this hand.

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J.M. Winter, *The Great War and the British People* (Cambridge: Harvard University Press 1986).

FOR THE SOLDIERS WHO FOUGHT in the trenches at Passchendaele and the Somme one of the most haunting effects of their descent into hell was its lingering presence:

When you see millions of the mouthless dead  
Across your dreams in pale battalions go,  
Say not soft things as other men have said. . . .

It is to J.M. Winter's credit that in this splendid book he has not said only soft things but also hard things with hard data. With a keen eye for individual experience, Winter nonetheless keeps a balance between the randomness of that individual experience and the larger historical forces within which such individuality orbited. Indeed, it is one of the most significant aspects of this book and one which most impressed me.

History, is of course, replete with ironies. In Winter's telling the British people's experience of the Great War has to be understood within a larger framework than the commencement and cessation of hostilities along the

Flanders salient. The war began after a long and protracted series of flashpoints and almost immediately the massive popular support among the populations of combatant nations seemed to read the expectations of socialists right out of court. Yet, it was not quite so. In Winter's tale the whole question of recruitment is set into a longer perspective and is explicable in terms of a national pre-occupation with 'physical deterioration' which had reared its head after the appalling performance of the expeditionary forces in South Africa. Working-class politics and imperialism fused a most peculiar marriage of convenience presided over by that man, Winston Churchill. The English Lion early believed that no matter what England expected, the English ruling class should not expect to get much from its subordinates if it gave them little in return. He said, "However willing the working classes may be to remain in passive opposition to the existing social order, they will not continue to bear, they cannot, the awful uncertainties of their lives. Minimum standards of wages and comfort, insurance in some effective form or another against sickness, unemployment, old age — these are the questions by which parties are going to live in the future." How right he was! The Great War was the forcing-house for these changes which the last Liberal governments had so hesitantly put forward in their so-called preparations to do battle with the Kaiser. More yet. As a result of the abominable living conditions and awful rates of pay — cross-cut with the inevitability of unemployment and/or short-time — about one in two working-class men was unfit for military service even during the most dire manpower shortages. So while the upper-classes volunteered in disproportionate numbers and — as subalterns — died in disproportionate numbers, those at the bottom of the heap had a comparatively 'good war'. For the working class, and in particular the 'residuum' of casual labour, "The state established a hold over its citizens which, though relaxed in peacetime, was never to be removed. . . . [so that] the history of the English state and of the English people merged for the first time." For Winter the demographer the single most effective measure of this 'good war' is to be found in the very significant improvements in life expectation of children, mothers, and non-combatant males.

This can be largely attributed to very dramatic gains in the regularity of employment and the rates of pay provided to unskilled workers. For those who had not heeded the clarion call to arms but had been relegated to the home front, a 'good war' was made palpable in the food they ate with an unknown regularity.

Ironically, then, the working class turned out to be the major gainers in the social experience of mass war even though — in absolute numbers — they died in far greater number than their masters. "The flower of English youth" sacrificed to the demented stupidity of the general command was disproportionately composed of members of the upper class. This irony runs through Winter's book and all its massive scholarly apparatus. It enables him to say soft things with hard data. *The Great War and the British People* is a very good book which deserves a close reading from scholars interested in both working-class history and those who are simply grateful to have been spared "Beauty in Death."

David Levine

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Roger Geary, *Policing Industrial Disputes: 1893-1985*. (London: Methuen 1985).

IN THIS BOOK ROGER GEARY, a lecturer in law, provides detailed descriptive accounts of industrial disputes and police reactions to them during five successive periods of British history since 1893. His sources are newspapers, parliamentary reports, Home Office correspondence, autobiographies, police and trade union histories, and interviews with police officers and union officials.

The most violent repressive measures taken during the period covered were in response to extensive property damage and the throwing of rocks at police. Such was the case when soldiers made up for a shortage of police personnel to deal with the 1893 miner's strike in the West Riding of Yorkshire. After other intimidating measures failed the soldiers were ordered to shoot "directly and at point blank range into the mass confronting them." (12) Two miners were killed as a result.



Subsequent case studies, including the South Wales coal strike of 1910-11 and the General Strike of 1926, show the police using batons as a method of control. By the time of the miners' strikes of the 1970s, however, police controls took less violent forms (that is, pushing and shoving, cordons and wedges, and symbolic confrontation). The shift is attributed to public opinion about police violence, some of it due to media reporting, and to the watchfulness of civil liberties organizations.

The present decade, however, has seen that historical trend towards non-violence reversed. Rock-throwing pickets have been charged by baton-wielding policemen. Public order training has become riot control training which, accompanied by a new hardline attitude, escalated industrial confrontation to the level of physical violence reached in the bitter 1984 miner's strike.

Geary thinks that the latter type of confrontation will not generally be repeated in the future. He believes that it occurred because the miners received weak support from the union movement, lacked internal unity, and were led by militants, all of which "prevented the constraints on violence which had proved effective in the past from being fully operative". (147) But he notes that modern police tactics "tend to generate a vicious spiral of violence and destruction," adding that hardline Thatcherism implies that baton-wielding riot squads are here to stay. Geary claims, however, that the degree to which repressive measures are taken depends on the way the political consequences of repression are perceived by the authorities. Repression of race riots is seen to have fewer undesirable political repercussions than repression of strikes.

While noting that investigations by civil liberties groups and media coverage act as brakes on police behaviour and that opposition politicians' criticism may also be a deterrent, Geary is concerned about the decline of informal community control of the police, due to centralization in the Home Office and reliance on supporting constables from other areas. Widespread use of intelligence and surveillance methods is also prevalent. He maintains that community representatives need real power to stem a drift from a democratically responsible police service to an Orwellian police force.

Perhaps because he is mainly concerned with legal issues, civil liberties, and the quality of policing, Geary does not consider the political economy of state-worker relations. But the book will whet many readers' appetites for a meatier analysis of the repression of workers' protests in advanced capitalist societies faced with economic recession and social instability. It also highlights how one type of worker can be used by the state to repress another.

The shift to training police and soldiers in riot control and anti-terrorist tactics is a response to international attacks on capitalist imperialism. Once they are in place capitalist states will be tempted to use them for various types of repression. Whether they do will depend on the degree to which police officers are controlled by elected representatives not beholden to the capitalist class. This is a major implication of Geary's descriptively valuable but theoretically limited study.

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David Green, *Working-Class Patients and the Medical Establishment, Self-help in Britain from the Mid-Nineteenth Century to 1948* (London: Gower/Temple Smith 1985).

Paul Weindling, ed., *The Social History of Occupational Health* (London: Croom Helm 1985).

ONE OF THE MOST IMPORTANT everyday aspects of working people's lives has been the securing, insuring, and otherwise contending against sickness and injury. Yet this can hardly be told from the bulk of what passes for labour history. Nor can it be ascertained from the history of medicine, where even discussions of 'the people's health' tend to refer more to disease entities, medical men, and medical institutions than to working people's personal, occupational, and political experiences with health and disease, prevention and cure. The two books noticed here, in substantially contributing to these concerns, thus constitute an important watershed in both the history of labour and the history of medicine. Not only

do they expose what has hitherto been mostly hidden from history, but in so doing they also collapse the arbitrary academic divisions that have served to aid and abet the concealment.

David Green's monograph is the first sustained account of the successful initiatives taken by British workers to organise by and for themselves a comprehensive medical service. Focused on the Friendly Society Sick Clubs, to which over 9½ million workers were contributing their weekly pence by 1911, it offers more than merely a detailed account of the societies' operations, and more than merely a history of their rise and decline. Foremost, by revealing that the Societies provided a far from second-rate, limited medical service, it challenges the long-standing view of Lloyd George's National Insurance Act of 1911 as the 'first great step' towards a national health service. Powerfully and provocatively, Green demonstrates how the 1911 Act undermined workers' control over and freedom of choice in health care. Further, he reveals how the orthodox view came to be established in the course of the Societies' destruction: on the one hand, by the advocates of and apologists for state centralism (from the Webbs to Titmuss); and, on the other, by an increasingly antagonistic and organised medical profession determined to have monopolistic control over the medical market. Alike, both interest groups sought to justify the appropriation of worker-controlled medical care in terms of the inability of the 'rude and credulous populace' to know their own good.

Green might be accused of painting too rosy a picture of the Friendly Societies, and, given the book's title, might be criticised both for failing to extend his thesis to the various other forms of medical care over which workers were beginning to exert control by the turn of the century, and for failing to pay sufficient heed to the vast body of persons for whom the Sick Clubs were either financially out of reach or/and were politically and culturally suspect. The Societies, after all, besides embodying a providential ethic (and mostly excluding women and children), were also business enterprises fully capable of leading workers into the ways of capitalist marketing, accounting, and management.

More worrying, though, is this book's own co-optive potential. At a time when the National Health Service is itself on the brink of ruin,

and has been rendered thus by the same government that deems it appropriate for one of its senior Cabinet ministers to preface a re-issue of Smiles' *Self-Help*, there is reason to be concerned about any celebration of proletarian self-efforts in health care, however well meant. As ex-Labour Councillor Green can doubtless appreciate, working-class advocacies of 'self-help' have also had a tragic history of backfire. But such thoughts serve only to temper, not to remove, the pleasure of reading this scholarly polemic against embedded arrogance and historically reproduced condescension.

The volume edited by Paul Weindling, though more academic in tone, is no less political in its outlook and commitment. Prefaced by the Labour shadow spokesman on Health and Social Security, it seeks to make occupational medicine fully a "part of the social history of industrialisation," hence to comprehend the nature and the kind of historical forces that have acted to facilitate and to constrain this area of medicine and health. Hitherto, as Weindling makes clear in his introduction, the history of occupational medicine has had a haphazard treatment at the hands of antiquarians, students of industrial legislation and social policy, and retired industrial medical officers, hygienists, toxicologists, psychologists, and the like. In part because of this handling, as Alfons Labisch observes in his review of the German literature, the tendency has been towards scientific reductionism, and therefore to the suppression, or 'dethematising', of social perspectives. Labisch thus justifies the need to reconceptualise occupational medicine as part and parcel of "the interrelation of the labour process with social conditions."

The eleven other papers in this volume — though somewhat arbitrarily arranged according to 'social conditions and risk factors,' 'compensation,' and 'preventive policies' — all contribute to elaborating this 'interrelation' and, hence, to liberating the subject from its fragmented and positivist past. Some of the papers do this with rather less theoretical self-concern than others: Gill Burke's "Disease, Labour Migration and Technological Change: the case of the Cornish Miners," and Linda Bryder's "Tuberculosis, Silicosis and that Slate Industry in North Wales, 1927-1939" being straightforwardly descriptive. Others pursue

cautionary tales: that by Mel Bartley on coronary heart disease, and that by Rainer Müller on the history of the gathering and non-gathering of occupational health statistics in Germany, draw attention to the social and ideological interests behind the supposedly value-neutral 'hard facts' of epidemiology and pathology. Lothar Machtan, on worker's insurance in Imperial Germany, and Peter Bartrip on the rise and decline of workmen's compensation in Britain, successfully disentangle the histories of those subjects and provide interpretative pathways through the political struggles behind them, while Helen Jones, on the factory inspectorate in interwar Britain, and Perry Willson, on industrial health and scientific management in Fascist Italy, move closer to the shop floor to decipher there the welter of competing and conflicting interests over health and safety. Like other papers in this collection, these last mentioned avoid simplistic portrayals of the conflicts between the interests of labour, management, the state, and the medical profession. They illustrate, rather, the real difficulties involved in endeavouring to distinguish within each of these groups, or within different factions of the same group, at one or different moments, the varied political, economic, and social motives behind the interests in (or resistances to) industrial health. Although the health hazards of work were common sites for struggle (as Antonia Ineson and Deborah Thom indicate in their paper on T.N.T. poisoning and women workers during World War I, witnessed in these papers, and in the volume as whole, is the variety of the particular expression of those struggles according to the specific socio-economic and socio-political contexts in which they transpired. Certainly it was not always, as it was in the case of T.N.T. poisoning, that "medical and managerial interests became clearly combined."

The other two papers in this collection, those by Dietrich Milles and Karl Figlio, further expose at more conceptual levels the historical complexities of the subject matter. Milles's is a critique of the way in which the concept of 'occupational diseases' (as devised by 'experts' and built into the German legislation of 1925), displaced an earlier concept of 'workers' diseases' as based on a straightforward causal understanding of the relation be-

tween illness and work. In effect, the notion of 'occupational diseases,' he argues, removed industrial illness from the political agenda of workers; conceptually disassociated was the production of industrial diseases from the material means of industrial production. Thus those who were closest to the workers' movement and who pressed for a widening of accident insurance and workmen's compensation to include 'occupational diseases,' can be seen to have "accelerated the reductionist trend of 'workers' diseases' being termed 'occupational diseases.'" The 'occupational diseases' legislation of 1925 was, in this respect, a dead end in which individual improvements looking promising, but structural changes were more or less stillborn."

Figlio's paper is in a like manner addressed to a subtle displacement and transformation. Innocuously entitled "What is an Accident?," it explores at a profoundly conceptual level how came about the principles, made explicit by the Workmen's Compensation Act of 1897, that "an injury could occur which was nobody's responsibility, but which fell to the employer to compensate, because it arose 'out of and the course of employment.'" [sic] By reference to key terms and key cases in medico-legal jurisdiction, Figlio establishes that the notion of an 'accident,' as a non-responsible neutralised event (as opposed to a motivated act), only became historically possible through the shift from master/servant to contract-based social relations. Analysing this shift at several levels, he offers what are literally cosmological insights into the social universe which, through contract-form, was no longer dense with personal responsibilities, motives, and causes, but was "an aggregate of events, rather than intentions," and in which a person's occupational illnesses were to be experienced as unrelatable events, rather than as a part of a single life-story. Apart from anything else (and there is much else) Figlio's paper demonstrates to a very full extent the possibilities for the reconceptualisation of occupational medicine once the shackles of positivist epistemology have been thrown off.

Although this volume is entirely Eurocentred, and is weighted, in particular, to Britain and Germany in the late-nineteenth and early twentieth centuries, the social perspective on occupational health that it succeeds in ad-

vancing transcends spatial and temporal boundaries, while many of the subject matters it treats could also be studied in other Western industrial societies. As such, whether wholly or in part, this volume constitutes essential reading for all future research in this field. Readers may dissent from particular points and interpretations, and they may be dismayed by some of the Germanic density of its thought and prose, but, as a whole, it is open to criticism only for the richness and multiplicity of the problematics it exposes. Like David Green's book, it reveals horizons which cannot now be seen as other than integral and crucial to the social history of labour.

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Ken Coates and Tony Topham, *Trade Unions and Politics* (Oxford: Blackwell 1986).

ONE APPROACHES WITH KEEN anticipation a work on this topic by two of the most prominent left-wing writers on British trade unionism, both mainstays of the Institute for Workers' Control since its creation twenty years ago. But alas, they have produced a curious, rambling text which commences powerfully but then falls apart at the seams. The opening chapter is an effective survey of the impact of mass unemployment on British labour: trade union membership cut by one sixth; extensive redundancies with only intermittent instances of resistance; weakness in pay bargaining; the erosion of worker militancy; shop-floor organisation "shorn of power and function." The persuasive conclusion is that "with bargaining inhibited, restrained by economic adversity, the primary trade union response to the present crisis must now be political."

So far, so good; but what kind of politics? The rest of the book consists of a series of largely disconnected essays, lacking clear overall integration and often weak in internal coherence. 'The Corridors of Power' is a critical assessment of the rise of 'corporatist' institutions in Britain: the web of government

patronage in which union leaders have long been enmeshed; and which, though denounced by current Tory ideology, seems central to the strategies for union-government relations in any future Labour administration. Another chapter outlines the 1984 Trade Union Act, which requires secret ballots for executive elections, official strikes, and political action. Coates and Topham then rehearse at length (though three years too late), with a digression on the miners' strike, the reasons why Conservative models of union democracy are hypocritical and unworkable. The three subsequent chapters cover the history of the union-Labour Party relationship and the 'political funds' established by legislation in 1913; the successful campaign for membership backing for the funds in the ballots required by the new Act; and the sectarianism of post-war union leadership involvement in the Party. Next comes a discussion of general strikes and the miners' dispute (again). Finally there is a particularly discursive exploration of 'class politics' including, *inter alia*, the need to win public support during strikes; the necessity for planning to restore full employment; the dangers of centralised planning, and the importance of industrial democracy; the value of legally based schemes of worker representation during periods of recession; legislation for minimum wages and shorter working time; and the importance of an international trade union response to the crisis.

From this catalogue it should be clear that the book covers a wide range of issues of importance for socialists and trade unionists (and particularly those who are both). But what Coates and Topham have compiled is a lengthy set of notes. The book remains to be written.

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Scott Lash, *The Militant Worker: Class and Radicalism in France and America* (London: Heinemann Educational Books 1984).

LES THESES QUI SOUTIENNENT que la technologie ou le développement technologique

affecte directement ou détermine la conscience ouvrière, ont connu deux grandes périodes de succès dans la littérature sociologique depuis la fin de la seconde guerre mondiale. La première s'est développée dans les années 1950 au moment de la généralisation de la mécanisation dans l'industrie; la perte de contrôle ouvrier liée aux nouvelles technologies de l'époque devait entraîner la radicalisation et les révoltes ouvrières.

La seconde apparaît dans les années '70, après la résurgence des luttes ouvrières et l'émergence des mouvements sociaux, avec ce que Lash appelle la "Bravermanie" et les théoriciens du procès de travail suite aux travaux de H. Braverman (1974), B. Coriat (1979) et autres; ces derniers, quoique ayant plus de "panache théorique" n'en représentent pas moins un retour à l'étude des conditions objectives de la classe ouvrière et même à une explication par la technologie.

Entre ces deux grandes périodes, l'accent avait été accordé aux orientations, aux valeurs, à la conscience de classe. C'est aussi à l'importance de la conscience de classe que Lash propose de revenir contre ce courant de la "nouvelle sociologie industrielle" (l'étude du procès de travail).

L'intérêt du livre de Scott Lash est donc très actuel en cette période de nouvelles technologies, et ce d'un triple point de vue. Lash s'appuie d'abord sur une recherche théorique fort bien élaborée à partir de la critique de Weber, Marx et Foucault; ensuite il met en perspective historique les divers courants sociologiques; enfin, il présente les résultats d'une recherche empirique bien soignée. Nous nous attarderons surtout aux deux derniers volets du livre de Lash, et nous ne retiendrons que quelques exemples de la vaste littérature passée en revue par l'auteur.

Les premières analyses du changement technologique ont presque toutes constitué la technologie comme variable indépendante. L'étude la plus célèbre à cet égard est celle de C. Walker et R. Guest (1952) chez les ouvriers de l'automobile. Ils soutenaient que les techniques de production de masse, en s'attaquant directement au contrôle ouvrier, favoriseraient le radicalisme; le militantisme ouvrier était donc anticipé chez ces ouvriers sur la base d'un déterminisme technologique. De même, R. Blauner (1964) avait montré que

l'aliénation ouvrière croissait avec le développement de la production de masse, mais qu'elle s'atténuait dans les entreprises automatisées. Or il ne semble pas que la mécanisation ait, pendant cette période, suscité beaucoup de contestations de la part des ouvriers. En particulier en Angleterre, selon Lash, le mouvement ouvrier était de plus en plus conservateur.

C'est alors que s'opère un changement de perspective et que la critique du déterminisme technologique se fait vigoureuse. J.H. Goldthorpe et D. Lochwood (1968) en particulier, en ont proposé une formulation simple. S'il est vrai que la mécanisation appauvrit la situation de travail et qu'en même temps il n'y a pas de contestation, c'est bien la preuve que les conditions objectives ne déterminent pas directement les réactions des ouvriers; entre les deux il y a les orientations au travail, la valeur accordée au travail. Or, disent-ils, l'éthique du travail a changé, les ouvriers s'embourgeoisent; ils valorisent non plus le travail (ils se seraient révoltés. . .) mais le hors travail, ils ont une conception *instrumentaliste* du travail. La croissance des salaires réels, mais aussi le style de vie et le réseau de relations sociales sont les principaux traits de cet embourgeoisement. Le travail n'a plus de sens pour lui-même mais parce qu'il permet de faire autre chose.

Ce rejet du déterminisme technologique par l'embourgeoisement des ouvriers s'inscrit, selon Lash, dans les thèses plus générales de la modernisation, du passage de la société traditionnelle à la société moderne dans laquelle les ouvriers acquièrent les pleins droits de citoyenneté, accroissent leur niveau de vie, changent leur mode de vie, réalisent leur "inclusion" sociale. De ce point de vue, le radicalisme peut se produire dans les périodes de transition mais non plus dans les sociétés pleinement modernes comme l'Angleterre ou les Etats-Unis. On reconnaît ici facilement les argumentations de Daniel Bell, T.H. Marshall, T. Parsons, etc.

C'est précisément au moment où ces thèses atteignent leur apogée que, fin des années 1960 et début des années 1970, les luttes ouvrières et celles des mouvements sociaux vinrent remettre à l'ordre du jour la question: pourquoi la classe ouvrière est-elle toujours militante? Le théorie de l'embourgeoisement

et de la modernisation perdait de sa crédibilité. La sociologie retourna à nouveau à l'étude des conditions objectives de travail et plus spécifiquement encore à la technologie sous la forme du procès de travail: c'est l'ère de la "Bravermanie", en Angleterre, mais aussi en France et aux Etats-Unis, chez les historiens autant que chez les sociologues. A l'encontre de Braverman lui-même, pour qui la technologie est une variable intermédiaire entre l'accumulation du capital et les rapports de classes d'une part, et les luttes ouvrières d'autre part, la nouvelle sociologie du travail développe, à toutes fins pratiques un nouveau déterminisme technologique: le procès de travail explique les luttes ouvrières des années 1960-1970.

C'est dans ce débat théorique que Scott Lash inscrit sa démonstration empirique appuyée par des entrevues, des questionnaires, des observations d'entreprises et enrichie d'une comparaison internationale entre la France et les Etats-Unis. Il a interviewé 239 ouvriers, dont 120 dans 6 entreprises américaines et 119 dans 5 entreprises françaises. Cent d'entre eux ont retourné le questionnaire laissé après l'entrevue.

A l'aide de ces données il construit deux index de radicalisme: sociétal (politique) et syndical ("industrial radicalism index"). Le radicalisme est défini d'après une longue élaboration théorique sur la conscience de classe et les droits des travailleurs: il est un ensemble d'attitudes que manifestent une adhésion plus ou moins forte aux idéologies que véhiculent les droits des travailleurs, comme par exemple les droits aux fruits de son travail, à l'égalité dans la distribution des richesses.

La recherche des déterminants occupe une place centrale dans le livre. Scott Lash constate que ce sont ni l'embourgeoisement, ni le niveau de vie, ni les qualifications, ni la technologie qui expliquent le mieux les variations observées dans la conscience de classe ou le radicalisme. Aucune de ces variables dites objectives, y compris le changement technologique que Lash étudie dans chacune des entreprises, ne possède une capacité significative d'explication. Il affirme que ce sont plutôt la socialisation politique et la socialisation syndicale qui sont les principaux déterminants du radicalisme; les partis politiques et les syndicats sont les agents

de cette socialisation dans la mesure où ils sont porteurs, à des degrés divers, des idéologies défendant les droits des travailleurs, et dans la mesure où ceux-ci sont susceptibles, aussi à des degrés divers, d'adhérer à ces idéologies.

Le rôle des partis et des syndicats est donc déterminant; non pas, selon Lash, dans une perspective exclusivement stratégique, à la manière de E. Shorter et C. Tilly (1971), mais comme véhicules de valeurs, de doctrines, d'idéologies, médiatisées et traduites par le discours et la pratique des militants de la base ("Shopfloor militants"). Lash, explicitement, se réclame de Gramsci par l'importance accordée aux idéologies, à la culture, par opposition aux interprétations "économistes" et aux variables objectives.

L'autre conclusion soutenue par Lash résulte de la comparaison France-Etats-Unis. Il constate que, contrairement à une certaine tradition marxiste, le radicalisme politique et le radicalisme syndical ne varient pas nécessairement dans le même sens. En effet, si les ouvriers français sont plus radicaux sur le plan politique, les ouvriers américains le sont plus sur le plan syndical, au moins quant à certaines dimensions de l'index du radicalisme. De plus, lorsqu'elle existe, la relation entre radicalisme politique et radicalisme syndical est très tenue dans chacun des pays. Scott Lash y trouve une excellente occasion pour discuter la thèse du passage à la conscience de classe par la conscience syndicale. Il n'y aurait pas, selon Lash, "d'évolution" de l'une l'autre puisqu'elles sont relativement indépendantes.

Enfin, toute la dernière partie du livre est consacrée à la question: d'où viennent ces différences dans les idéologies? Scott Lash fait une analyse historique des mouvements ouvriers américain et français pour montrer que la nature des alliances de classes permet de rendre compte des différences entre les deux pays, car ces alliances recouvrent des formes idéologiques et organisationnelles. Alors que les deux mouvements ouvriers avaient adopté des idéologies de type coopérativiste et/ou populiste, au moment du passage au capitalisme organisé, le mouvement ouvrier français a fait la transition vers des formes idéologiques collectivistes alors que le mouvement ouvrier américain s'est orienté vers le syndicalisme d'affaires, imposé par ses alliés de classe du *New Deal*.

*The Militant Worker* constitue une suite indispensable à *The Affluent Worker*. Plus précisément, chacun est un excellent révélateur de périodes historiques différentes. Goldthorpe et Lockwood avaient raison de souligner le caractère instrumental du travail puisque le compromis salarial fordiste définissait le salaire comme un équivalent général à toute agression sur les lieux de travail (changement technologique, monotonie, pénibilité, dangerosité, etc). De même les alliances de classes du *New Deal* ont produit cette idéologie de la croissance par le progrès technique, ce qui explique bien que les sociologues aient élevé la technologie au statut de variable indépendante et qu'ils en aient étudié les effets sur les divers aspects de la situation de travail. S'ils ont erré en théorie, ils ont été adéquats à leur période. Mais la crise et les révoltes ouvrières ont remis en question les idéologies du progrès ininterrompu. C'est bien ce que Scott Lash vient démontrer d'excellente façon.

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David Levine, ed., *Proletarianization and Family History* (Orlando, Florida: Academic Press 1984).

THIS COLLECTION IS BOTH MISNAMED (only two of the essays can really be said to be about family history) and very uneven. In fact, taken together, the essays offer examples of the best and worst of recent social history.

Charles Tilly's essay, "The Demographic Origins of the European Proletariat" opens the book. In it he deploys the apparatus of social science to argue for a modification of the "Marxist" (by which he means Marx's) "account of European proletarianization." (55) In his view, "proletarianization" occurred at varying and occasionally reversible rates as a result of two factors: natural increase and "movement into the proletariat." Unlike Marx, Tilly thinks that the former had more

to do with the increase in numbers of wage workers during his period than the latter.

The essay is vintage Tilly, replete with equations and numbers (and *caveats* about the general unreliability of those numbers). For those unwilling to accept bald assertions about the rate at which a given population turned from various pre-wage economic pursuits toward waged work, Tilly offers the following reassurance: "...the crude expression for the rate of proletarianization,  $P: P = \dot{w} + \dot{e}$ , where  $\dot{w}$  is the rate of change of wage dependence and  $\dot{e}$  is the rate of change of expropriation" allows one to conclude that, "If the sum of the two rates is positive, the population is proletarianizing. If it is negative, the population is deproletarianizing. If it stands at or near zero, the population's structure is remaining the same." (18)

Although space precludes a thoroughgoing attack on the epistemological assumptions which underlie such pseudo-scientific posturing, one must at least note two things. First, the whole concept of "proletarianization" is almost meaningless in this context as it includes vast numbers of real people occupied in such varying occupations as agricultural labour, artisanal crafts (both in homes and workshops), domestic service and, latterly, factory production. That all these people in various countries at various times experienced a single process is at least open to doubt. Second, an overemphasis upon a single model of a single process has abolished human agency from the story altogether. At one point, in fact, Tilly offers an odd description of the 'proletarian' approach to death and childbirth: "...on average, proletarians responded to economic expansion with greater declines in mortality and greater increases in fertility than nonproletarians did and responded to economic contraction with greater increases in mortality but no greater declines in fertility than nonproletarians did." (39)

This kind of work may still be of interest to those who remain convinced that history can be used, in Geoffrey Elton's words, "for the discovery of laws governing human behavior." Happily, however, most now would agree with Elton that the effort has led only to "repeated failure." And in fact this essay suggests the extent of the failure. Some 60 pages of numbers, graphs, and charts do not suffice to an-

swer the historian's key question: so what?

David Levine's purpose in the book's second essay, "Production, Reproduction, and the Proletarian Family in England, 1500-1850," is different from Tilly's. Here Levine is concerned to bring the lives of ordinary people back into the description of a process (also, in his case, of "proletarianization"). He is concerned to outline three stages of change — from peasant life to proto-industrial life to life in industrial cities. In the first stage people lived in agricultural communities that exerted controls over their lives, especially over child bearing. As a result of several factors (including, of course, enclosures), many people next found themselves living and working in "proto-industrial families," which provided male family members with more freedom than they had enjoyed previously. We can only infer what women's lives were like from this statement: "...the proto-industrial labourer was the master of his own household and was responsible for organizing his production and socializing his children." (98) After the proto-industrial family had provided a venue in which men could adjust themselves to wage labour, people undertook factory work. Here, fertility rose as community controls disappeared altogether, and "Wage workers [again all male]. . . married younger brides." (110) In the end, Levine agrees with Tilly that the English proletariat's growth was less a result of what he calls "peasant expropriation" than it was of "proletarian demographic growth." In other words, the proletariat created itself.

Unfortunately, Levine's effort to identify and describe the effects of change in the lives of ordinary people fails. There are only the briefest assertions about the behaviour of real people — all in the vein of that quoted above. People in general and women in particular are almost entirely absent from this account of the past. In fact, agency is almost always reserved for abstractions or events. One example must suffice: "By killing off children," Levine insists, "plagues and epidemics made little impact, since children were, in effect, easily replaced." (107) At the very least one might have expected a quick look at a second set of figures — maternal mortality rates — which might have modified this curious conclusion.

The fourth essay in the collection, Catharina Lis's and Hugo Soly's "Policing the Ear-

ly Modern Proletariat, 1450-1850" suffers from all the problems found in the first two, as well as from a very clumsy handling of difficult categories, including "the state" and "social control." In this very ambitious attempt to show how "authorities" (sometimes states, sometimes municipal officials, sometimes *notables*, sometimes, even, the Church) reacted over time to changing perceptions of "the poor" as well as to the growth of urban poverty, the authors have written a piece that is a confused muddle of assertions bolstered by a random assortment of evidence taken from their book, *Poverty and Capitalism in Pre-Industrial Europe* (1979). As that book is, in fact, considerably less confused (although it exhibits a similar lack of sophistication), it may simply be best to recommend that readers ignore this essay in favour of the longer work.

So that's the bad news about this collection. Before giving up on it entirely, however, it is important to note that the good news is very good indeed. Both John Gillis's "Peasant, Plebeian, and Proletarian Marriage in Britain, 1600-1900," and Bryan Palmer's "Social Formation and Class Formation in North America, 1800-1900" are truly fine pieces, albeit very different ones. The first shows just how informative, intelligent, and entertaining good social history can be. The second offers a model of the sophisticated deployment of marxist theory in a highly original and successful pursuit of the story of people in the past.

John Gillis's examination of wedding practices shows the extent to which they were embedded in various social formations, and how they changed when conditions changed. He outlines here three broad (and overlapping) types of weddings: "big weddings", which were heir to practices associated with peasant weddings in an earlier era, "plebeian weddings," typical of proto-industrial life, and "proletarian weddings," which grew out of life in working-class cities in England. Big weddings involved entire rural communities, including both the gentry and the poor. Rituals demonstrated both male dominance and female subordination. Thus, for example, mothers took no part in weddings. Brides walked behind husbands, and accepted ceremonial presentations of the symbols of their prescribed role: fire tongs, brooms, and keys. The com-



munity's sanctions against unions between social unequals helped maintain the existing social order.

As social hands loosened in the face of changing economic conditions, however, marriage practices became much more restrictive. As a result, clandestine marriages, which, in Gillis's words, provided "a safety valve for what otherwise was a potentially explosive situation" (137), multiplied. Until they were abolished by the Hardwicke Act in 1753, such unions offered the only means by which village youths could escape both parental and community authority.

Plebeian weddings followed, growing out of proto-industrial communities. These weddings reflected the extent to which this economic situation by offering economic independence granted women some autonomy. Both men and women in fact selected their own spouses, and selected them on the grounds of strength, skills, and physical prowess — all necessary to the economic well-being of proto-industrial families. Because both young women and young men could earn their own money — and enough of it to survive independently — many abjured formal weddings altogether, preferring informal, flexible arrangements called "little weddings."

As women increasingly lost their economic independence in the course of the nineteenth century, however, marriages became the site of struggles between economically dependent women (concerned with the support of their children as well as with their own) and independent men. Many women found themselves forced to accept common law unions with men who refused to bind themselves into legal unions which obligated them to support wives and children.

As the industrial capitalist system took hold, however, marriages returned to legal, formal forms. Weddings — increasingly the white weddings now customary everywhere in the western world — grew bigger and considerably more patriarchal, although Gillis insists that the overt male dominance embodied in the rituals of the white wedding was only gradually assumed by the working classes.

This brief overview cannot give any sense of the richness of Gillis's approach to his subject. His highly original research has given him abundant examples which allow him to develop

a sense of 'real people' living in the past. At the very least, this essay shows how a skillful and imaginative social historian can connect the most quotidian practices of ordinary life with transformations in the larger society — and can do so without oversimplification or overabstraction.

The collection concludes with Bryan Palmer's "Social Formation and Class Formation in North America, 1800-1900." It is, in many ways, a *tour de force*. Not only does Palmer succeed in including all of North America, but he constructs a very complex argument about the processes of class formation during the nineteenth century without losing either nuances or human protagonists. Moreover, he looks carefully at problems of gender and race as well as class, showing how such categories co-existed, interrelated, and changed.

Palmer has utilized Marx's stages of primitive accumulation, manufacture, and modern industry as the contexts from which he examines the development both of corollary formations (for example, the internal colonization of Native Americans) and of the social groups which emerged from each. He demonstrates the extent to which each "stage" overlapped, left traces in succeeding economic and social formations, and was affected by the people who lived in and through successive transformations. His essays suggests, moreover, that the simple-minded process of "proletarianization," used so unsatisfactorily by others in this collection, was never a single process, but rather a whole collection of processes and events which existed in complicated relationships with each other and with people of various classes, races, and genders. Unlike the other writers, Palmer rejects the implication that the process — which he calls "capital's project" rather than "proletarianization" — is complete. Indeed, he concludes that "the 1980s are one of those historical moments which make all turning back impossible. . . ." (289) because new social and class structures are in the process, still, of being made.

In short, Bryan Palmer has here written a very abbreviated "Making of the American Working Class" that I think lives up to its forerunner. His work is, in fact, a synthesis of the best of E.P. Thompson and the best of *New Left Review* writers. On the one hand he has

kept in mind that structures such as class are, to paraphrase Thompson, "relationships, not things. . ." while at the same time he has not avoided engaging "high theory." He includes most of the recent marxist theoretical accounts of the coming of industrial capitalism, selecting elements he finds useful and rejecting those for which he finds contradictory evidence. With a light touch, the author employs a wide variety of evidence — from Defoe, to colonial secretaries, to contemporary observers, to union militants, and so on. Finally, Palmer also incorporates much of recent feminist historical research, as well as that which treats the history of Native Americans and of slaves. In this piece, women and men, whites, blacks, Native Americans, are all together part of the story. None is patronized, or added as a token.

If indeed this essay has a flaw, it is its sophistication. The discussions of recent marxist theory are not easy-going, especially for those not thoroughly versed in the various debates. However, Palmer's writing is lucid, and has, I think, made his difficult points as clear as possible. In fact, he often renders the difficult arguments of some theoreticians, Hindess and Hurst for example, far more comprehensible than they are in their original version.

In short, this book should be read for this essay and for Gillis's. Perhaps, too, one should read the entire book as a sort of 'dos and don'ts' for social historians. It certainly makes strong cases on both sides.

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Kathryn E. Amdur, *Syndicalist Legacy: Trade Unions and Politics in Two French Cities in the Era of World War I* (Urbana: University of Illinois Press 1986).

THE OUTBREAK OF WORLD WAR I did not mark the end of revolutionary syndicalist ideology Kathryn Amdur argues in her study of the French labour movement. Although she examines the period spanning the years from just before the end of peace to the onset of the depression, she concentrates mainly on the

period of the war and the early twenties, which she contends contains precious clues to the appeal of syndicalism in the prewar years, and to its ability to survive wartime conditions, the postwar period of economic recovery and slump, and the highly uneven technological and structural changes in the productive processes. It was only in the aftermath of this period that a more decided realignment of a complex of forces and a new generation of workers undermined its power, though not without summoning the strengths of its myths and realities to send out weak signals to the demonstrators of 1968. The schism on the political left is a secondary, not a primary theme of Amdur's analysis. As she shows, it did not by itself determine in general or in detail the divide in the labour movement. Moreover, it did not split only in two ways, but had to make room for a third strand (not just a splinter) constituted by the tenacious capacity of syndicalism to maintain the loyalties of workers, whose dislike of the CGT's reformism began long before 1914 and did not lose its intensity for at least another generation, and to resist the newest disciplinarians in the post-war CGTU who, while speaking in the name of authentic revolutionary action, actually worked towards the "bolshhevization" or centralization of the labour movement and against the deep feelings for and beliefs in local autonomy. By tracking developments in the cities of Saint-Etienne and Limoges in the contrasting departments of the Loire and Haute-Vienne that hug the eastern and western parts of the Massif Central, Amdur ably contributes to an understanding of changes on the Left throughout France, for, as she puts it, "The two cities may have diverged from the national patterns but they were hardly unique cases of provincial particularism." (10) She may very well be right.

Amdur's approach is familiar to students of labour history. Indeed, it is one of the best recent examples of the prevailing orthodoxy, and once again it does not lay to rest the nagging feeling that, though, of course, nothing is transparent, the deepest digs have not yet uncovered the richest ores. The fault, if there is any to attribute, does not lie in an absence of dedication to painstaking research. In the present case, it was carried out in the national, departmental, and municipal archives, in the private collections of three unions and one

chamber of commerce, and it was rounded out as well by the author's inferences from her interviews with union officials. The notes that follow the text take up more than 100 pages, and although they testify to the author's sense of purpose, their fullness is no substitute for a more extended interpretation in the body of the work. As it stands, it exhibits some of the features of overkill that has become a feature of historical monographs. Amdur does more. She has critically absorbed the work of her predecessors, discreetly distributing approval and expressing dissent as she makes her way through compilations of evidence, including, we should add, the yellowing newspapers that she uses to complement, verify, and challenge the assessments found in the departmental and national records of the reports submitted by informers and informants to the police, the ministry of the interior, and the military concerning union strength, plans, strikes, agitation, inner strife, union and political rivalries. The book she puts before us has all these virtues; it is to be trusted on that account; it exists as a legitimization of the way historians confirm both the past and their professional status; but it prompts serious questions, raised by Sewell, Sheridan, and Hunt, among others, about the wisdom of a structural and institutional analysis only incidentally, if at all, informed by evidence gained from the culture of popular protest that incorporated work attitudes, patterns of deference, and the limits of forms of resistance.

What does Amdur tell us? Some old things. There are complex relationships between different kinds of work, occupation, and skill: age and sex differences; the size of shops, factories, and mines; the rural and foreign origins of workers; the movements of the business cycle and the fluctuations of internal and external demand for products — all of these *and other variables determine* levels of productivity and wages, of degrees of dissatisfaction and insecurity, and shape the nature of labour protest. Hence it is no surprise to find differences, for example, in the organized responses of miners, metalworkers, railroad workers, shoemakers, porcelain makers, and weavers, who made up the majority of urban workers in the Loire and Haute-Vienne.

The specific findings are, however, more interesting, if not always conclusive. For in-

stance, strike rates (periodicity, duration, size, and shape) — Tilly's and Shorter's work proved indispensable and is often quoted — alone do not tell us about the strength or weakness of prewar revolutionary syndicalism among Loire miners or metalworkers. Hence an attempt is made to give due weight, such as pride in artisanship and the middling size of the factories in which most metalworkers were employed. During the war, skilled workers felt threatened by the introduction of forms of Taylorism and other strategies to control the workforce and increase productivity. How this was translated into resistance, culminating in the crisis of 1917, which, with the mass general strike at the Holtzer steelworks as its centre, moved on to include other munition workers as far away as Lyon and Le Creusot, is rightly seen in a perspective that includes many of the variables underscored above, but it does not, even as Amdur asserts her conviction that the strikes of 1917 and the attempted insurrection of 1918 "remained in the forefront of the country's syndicalist agitation," tell us very much about how these events were related to "the anarcho-syndicalist traditions deeply rooted in the conflicts of the past." (81) The vagueness of what these traditions meant to their adherents lies at the heart of the problems posed by this book.

Just as we think we may be grasping how those traditions played so significant a role in mobilizing protest, Amdur relies on an explanation that thrusts them aside and instead relates extremism to the small firm and moderatism to large-scale operations in the mines and railroads. This is not an arbitrarily chosen example. She clearly feels more comfortable pursuing this angle of thought. As she plots changes in the post-war period, she has continual recourse to carefully assembled data, restructuring it in graphs and tables. In this way, she succeeds inevitably in illuminating a number of questions. One particularly vexing problem is how to judge the growth of militancy and the increase in the number of strikes in 1919. At the time, Alphonse Merrheim refused to see in them any evidence of revolutionary consciousness; and he was a seasoned union veteran and observer. Amdur does not accept Annie Kriegel's calculation of the lower than average ratio of Socialist Party to union members in the Loire, as compared with

the Haute-Vienne where it was twice the national average, as an adequate explanation of revolutionary awareness in Saint-Etienne. (Amdur challenges Kriegel on other points as well, see 119 and 136.) By inferring instead that the small number of union members relative to the Loire's large industrial population was behind the excessive revolutionary rhetoric, Amdur demolishes a common misconception and relates militancy to union weakness, not to its strength. (127) Amdur also eschews obvious conclusions in her discussions of the tactics of the Communist CGTU, when she argues that these were often the ingenuous result of a faulty misreading of economic and technological developments, or the wishful thoughts of men eager to see greater class divisions where none as yet existed. (250)

With these nuances of judgement, Amdur's book may be turned to as a reliable source for the fate of revolutionary syndicalism. It nevertheless shows few signs of graceful writing. This may be connected with what may be the major shortcoming in her study. Despite heroic efforts, the methods she deploys cannot touch the textures of affect that lie at the root of deeply held convictions, powerful enough to move men and women to undertake or at least to contemplate heroic collective actions as a continuing assault on structures of work and life that cramped the spirit and weakened bodies. One way to make these feelings accessible is to study the language of labour and the language of labour politics. Such an approach would not detract from the one that Amdur adopts; it would not only enhance it, but give a larger entree to such questions as "backwardness" and syndicalism, the coexistence in some cases of corporatism and militancy, and most intriguing of all, the "distinct vision of the future" (269) that Amdur says inspired syndicalists to withhold support from the Communist leadership and seek instead some way to meet their practical needs and keep alive their revolutionary hopes. Such a combination might very well be an example of grand self-deception or wishful thinking, as bad as any indulged in by the Communists, masters at denouncing false consciousness, but surely worth further interpretation.

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John Tisa, *Recalling the Good Fight: An Autobiography of the Spanish Civil War* (South Hadley: Bergin and Garvey 1985).

AS A HISTORIAN OF MODERN SPAIN I have long been intrigued that an event in Spanish history should have had as much resonance and remained in the historical memory so strongly as the Spanish Civil War. After all, even for specialists in European history, Spain barely exists in the nineteenth and twentieth centuries. This persistent interest has nothing to do with the Civil War as an event in Spanish history; rather it has been internationalized and denationalized, appropriated both by the foreigners who fought in it and many subsequent historians. Some have even come close to constructing a Spanish Civil War without Spaniards. John Tisa's memoir of his experiences in Spain as a member of the International Brigades does not go this far but it is a fine example of what can be called the foreign ownership of a national history.

The basic problem is that most non-Spaniards have seen the Civil War in terms of their own political concerns, not in terms of the Spanish realities from which it sprang. In Tisa's book this starts with the sub-title: how can one man's experiences be an "autobiography of the Spanish Civil War?" It continues with the prologue by Frank Ryan, a member of the IRA who fought in Spain, was captured there by the Italians who handed him over to the Germans, and who died in Dresden in 1944. Ryan claims that the entire Spanish population opposed Franco (a patent untruth), that the Civil War was a clear struggle between Fascism and Democracy, and, in a revealing phrase, that the International Brigades were the "forces of international democracy" fighting in Spain "for their own cause." (xv) Ryan's prologue was written in 1937 for the *Book of the XV International Brigade*, of which Tisa was co-author, so he can be forgiven the historical inaccuracies and the propagandistic intent. Tisa, writing almost 50 years later, must be judged much more harshly.

It is not my intention to suggest that Tisa's memoirs are in any way false or without interest. Quite the contrary, for they tell a story of commitment and courage and, more impor-

tantly, they show how political events in a distant, little known, and previously unimportant land could galvanize political and labour activists in the United States. They also remind us once again that those who had fought in Spain were subsequently discriminated against by their own government during World War II. What this book does not do is tell us much of anything about the Spanish Civil War, and the vision it does offer is a highly questionable one.

Both the Republican and Franco governments presented the Civil War as a struggle between good and evil: as Democracy versus Fascism on the one hand, as Christian Civilization versus Godless Communism on the other. The reality was much more complex. Neither side was a political or ideological monolith, but the problems caused by diversity were much more serious among Franco's opponents, who disagreed over what the war was about and how it should be fought. These conflicts culminated in the May Days (1937) in Barcelona so well described by Orwell in *Homage to Catalonia*.

Tisa's memoir reveals little of this. Indeed, the overall impression is that he was largely oblivious to the politics of the war. There are few references to Spanish political organizations and no discussion at all of political developments. Insofar as he does mention political matters Tisa holds unquestioningly to the view propounded by both the Republican government and the Communist Party, that the war was simply a struggle against Fascism. He repeats the charge that the dissident Communist party, the POUM, was a counter-revolutionary organization which represented a "potential stab in the back." (173)

The anarcho syndicalists of the CNT, who were instrumental in defeating the military uprising in Barcelona and who saw the war as simultaneously an antifascist and revolutionary struggle are largely invisible, even though Tisa spent ten months in Aragon and Catalonia, where they were the most important political force. When they are mentioned it is without any sympathy, or even any attempt to understand or assess their position. Indeed, they too appear as little short of traitorous. In December 1938 Tisa moved into an apartment with two Spaniards who "were presented to me as anarchists but loyal to the Republic." (201)

The implications of this remark are clear and Tisa makes no suggestion that it was in any way inappropriate. He makes only one, fleeting reference to the collectivizations which took place in the summer and fall of 1936, describing them as "forced." (182) There is no indication that these formed part of a largely spontaneous social revolution of massive scope triggered by Franco's military revolt nor that the agricultural collectives of Aragon were dissolved at gunpoint by Enrique Lister's Fifth Regiment.

In one sense all of this is unimportant. Tisa is entitled to his views on the Spanish Civil War, however much I may disagree. After all, he earned them. In another sense, however, this is a question of urgency and relevance. Respect for the complexity of history will, surprisingly perhaps, clarify more than obscure. The same appropriation and simplification of a national history which led Tisa and many others like him to fight for the Spanish Republic contributed significantly to the decision of the democracies to hide behind non-intervention and this decision was crucial to the outcome of the war. It is now being used by the Reagan administration to justify a proxy war against the Sandinistas. A better knowledge of Spain's history would only have confirmed that the Republic represented the forces of progress, even if not everyone agreed that this meant bourgeois democracy. A better knowledge of that country's history would show that in Nicaragua the Sandinistas, and not the contras, are the forces of progress, even if they are far from being the liberal democrats many North Americans would like them to be.

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Lindsey Charles and Lorna Duffin, ed., *Women and Work in Pre-Industrial England* (London: Croom Helm 1985).

THE FIVE PAPERS IN THIS VOLUME — four by historians, one by a sociologist — originated as a series of seminars convened by the Oxford University Women's Studies Committee. Un-

like the papers in many anthologies these share a common theme: that in the centuries preceding industrial capitalism the sexual division of labour was much more complicated and more highly variable than previously thought. Three of the historians, Kay Lacey, Diane Hutton, and Sue Wright, arrive at this conclusion through close encounters of an archival kind, Michael Roberts through literary sources, and Christopher Middleton through his continuing theoretical interrogation of the secondary literature.

In a sense then they all, including Lindsey Charles in her introduction, take on what Middleton describes as the "critical-pessimistic" tradition in women's work set out by Alice Clark in 1919 and accepted by the next generations of feminists: namely, that "early capitalism and/or industrialisation restricted women's work opportunities and reduced their economic importance." (182) They are not arguing that capitalism ushered in a truncated version of the golden age, but rather that the job ghettos of capitalism, the wage discrimination, and the ideas about women and their proper sphere of activity, were all powerfully prefigured during the preceding epoch.

Yet they are quick to point out that these various manifestations of sexual inequality were uneven developments shaped by the conditions prevailing in particular occupations, localities, points in the life-cycles, households, and economic fluctuations. In "Women and Work in Fourteenth and Fifteenth Century London" Lacey explores the discrepancies between legal thinking (itself inconsistent and conflicting) about the abilities and status of women, and the actual occupations and roles they performed.

When the dust settles (there are 309 footnotes) how much of a revision of Clark is offered here? As Lacey concludes, "despite the constraints which were in theory placed on them by the various overlapping bodies of law," women could be members of nearly every guild (though not hold office) and engage in trading and manufacturing provided "they declared themselves sole." (57)

Eileen Power's famous comment that a "rough and ready" equality predominated between men and women during this period is disputed in Diane Hutton's "Women in Fourteenth Century Shrewsbury." She finds

a clear sexual division of labour, with women concentrated in lesser paid, lower status tasks associated with domestic labour. Yet she also points out that, nonetheless, women played a key role in the medieval economy and provided an important supplement to family income that "may have made the difference between starvation and subsistence." (97) In this connection we should remember that Power's expression was a description of the husband-wife relationship in feudal society, not of that between men and women. Sue Wright's study "Cheermaids, Hhuswyfes and Huchsters: The Employment of Women in Tudor and Stuart Salisbury" confirms Hutton's conclusion, however. Women's work outside the household in this locality at least was "at the casual, menial end of the market." (116)

In "Words they are women, and deeds they are men: Images of Work and Gender in Early Modern England," Michael Roberts explores the different approaches to work that coexisted during the period, in order to discover how these affected ideas about women's work. Perhaps most interesting to contemporary students of women's work is how some male work was beginning to be defined as an "occupation," however disjointed a series of activities that it might describe, while women's work continued to be perceived as "the social obligations of a wife." (144) Still, by the end of the period he argues that the new organization of the market was giving some "girls" — like the fictional Moll Flanders — new chances for independence. This is perhaps a development that is too easily swept away in the critical-pessimistic perspective of Clark *et al.*

Middleton's more theoretical piece is also revisionist work. As in his previous articles he wants to show how the discriminating sexual division of labour endemic to capitalist society "had much earlier origins." (191) Furthermore, using Hans Medick's work he shows how the transition to capitalism — and in particular the processes of proto-industrialization — had a more diverse set of possible consequences for women's work than the "critical-pessimists" allow.

This is a book of careful, and useful scholarship. Serious students of women's history and women's work should read it. Yet Clark's work, written with passion and prejudice 70 years ago, should be read first.

Her thesis needs and deserves revision, but it has not been overturned.

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Joseph Kestner, *Protest and Reform: The British Social Narrative by Women, 1827-1867* (Madison: University of Wisconsin Press 1985).

BEING A LITERARY STUDY, this is a difficult book for an historian to review. Nevertheless, it focuses on a crucial era in British history. The increasing number of people living in urban areas resulted in housing shortages, crowded conditions, and public health problems. The competition for employment was aggravated by changing industrial technology and workers, skilled and unskilled alike, were contemplating a world in which their contributions were and would continue to be devalued. It was not a totally uncaring world, however. Numerous government commissions testified to the problems and by their very existence reflected a realization that something had gone dreadfully wrong. If the social order was to be maintained, the condition of the masses would have to be stabilized if not improved.

Historians have been well aware of the various studies, both public and private, undertaken in response to the perceived problems of these years. What they have been less aware of is the broad impact that the "condition of England" had on a literate public. It is the purpose of Kestner's study to reveal that impact through a study of social protest novels.

What is interesting about Kestner's work is his emphasis on the women authors of so many of the social narratives. Rather than being the writers of romance as traditionally depicted, women seemed to dominate the protest novel. What is more, most of their works were extremely well researched. The writers were familiar with the various government studies being published and utilized these studies for their source material. In many cases, they disseminated the work of these government reports in a form the wider public could digest. In addition, because of the freedom of

the novel form, they could breathe life into 'dry' facts so that those who suffered from the problems of unemployment, poverty, and powerlessness became real to those reading about them rather than remaining depersonalized statistics.

The appreciation of the problems facing many of Britain's citizens did not happen overnight. It slowly evolved and the organization of this study reflects that fact. Kestner has organized his book according to decades and the novels of each period seem to reflect the level of awareness in the society as a whole. By the 1840s the novels have increased not only in number but also in the reality and sympathy of description.

The authorship of the novels under review is intriguing and significant. The fact that so much of the social protest literature was written by women points toward an interventionist aspect to their lives which historians have only recognized in recent years. What a study of these novels suggests is that it preceded formal organizational efforts. What historians have perhaps overlooked is how close to poverty many middle-class women were and how vulnerable they were to those with power. The act of writing was in and of itself a form of protest on the part of women who wrote and although their class and religious bias is often evident in their writings, the authors are never totally separated from the problems they write about. What is particularly interesting is their focus on the economic sphere. In almost all the novels under review the female characters are linked to the world of economics in a significant way. The social novels written by women do not depict their women protagonists in a domestic sphere cut off from the world of commerce and enterprise. Rather, they are tied to that world to the extent that only by changing it can they truly change their own lives. It is this realization which all the novelists shared.

*Protest and Reform* underlines the value of literary sources for the historian. As Kestner recognizes, "they are a complement to primary evidence." Each source has its own bias and if the historic approach is needed in the study of literature an appreciation of the literary aspects is equally demanded in historical studies.

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Joyce Kornbluh and Mary Frederickson, eds., *Sisterhood and Solidarity: Workers' Education for Women, 1914-1984* (Philadelphia: Temple University Press 1984).

FOR YEARS I HAVE DREAMT of a school for women activists, a school for learning not so much about sisterhood and solidarity, although this would happen, but a school for planning (and plotting) collective action for change. Women would come from trade unions, grass roots community groups, feminist academe, factories, offices, and homes to define their oppression, feel safe about acknowledging its effects, and begin to travel the endless individual and collective paths to freedom. I could go on but the point is that this a dream and one that seems to have little chance of becoming reality. What the book *Sisterhood and Solidarity* offers me is the historical roots of my vision and the knowledge that women before me both dreamt about and created women's education for change.

Working women resisting oppression and finding community across the barriers of race, religion and class: this is the vision documented by the ten contributors to *Sisterhood and Solidarity*, a solid body of research on American working women's educational activities from 1914 to 1984. The dates are somewhat misleading since the essays deal for the most part with the schools for women workers that flourished across the eastern United States during the 1920s and 1930s. (There is one uninspiring essay on women and labour education today.) The time period, the format (all articles end with selections from the original sources), and the fact that the authors generally argue along the same lines, give the collection a unity and depth often lacking in anthologies.

For anyone who has not previously heard of these early schools for women, and particularly for women educators who struggle daily to define and practice feminist education, *Sisterhood and Solidarity* offers inspiration, and some important lessons in survival. We are apt to think that ours is the first generation of women to discover the power of the "personal is political" as educational technique. Without the historical roots of what we now call "naming of the world" and "building critical con-

sciousness," we can easily lose perspective on the limits as well as the potential of education for change.

In the current political and economic climate, we need to remind ourselves that there was a year-long residential program to train women as union organizers in 1914; that women of the International Ladies Garment Workers Union recognized the male cast of trade unionism and used education to help women experience the union as community; that the women who organized the summer school for office workers aimed at nothing less than teaching working-class solidarity in order to counteract managerial attempts to co-opt women in new office occupations. Often, these bold educational experiments were initiated by middle-class women whose dream was "to incorporate blue-collar women into the liberal humanist tradition." (113) Their class bias notwithstanding, such women demonstrated an astonishing flexibility and they were always clear that women needed individual *and* collective change. At the Bryn Mawr summer school, where Hilda Smith and M. Carey Thomas founded the first residential summer school for women workers in 1921, they insisted on the kind of student democracy later recommended by Paolo Freire. When the students took over and radicalized the curriculum, their changes were respected. The women behind the YWCA's industrial education programs for women took up the cause of working women when no one else would. Animated by ideals of cross-class cooperation, and rooted in a tradition of religious reform, they nonetheless managed to politicize women workers to an extent we can only envy today. These women and their students left us with an inspiring message: As one student put it then, "I will not say you are no different from me. Strangers are different all; they make me shy. But we are nearer, nearer body and mind, than me and a debutante, or a legion man." (202) This vision of sisterhood is hard to resist.

*Sisterhood and Solidarity* would be of little value, however, if all it left us with was a dream. There is sound historical analysis throughout to remind us that the schools were the products of unique historical circumstances. In the 1920s and 1930s, middle-class women could forge a collective solidarity with working women on the strength of their belief



in education for a better life. They believed that they would always recognize when workers' education became education for social control. More important, there was no one to challenge the limits of the strategy. Trade union men, who might have objected, could scarcely be bothered about women workers, and, in any case, their own fortunes were at a low ebb. When unions finally woke up to the importance of education as a strategy for change, they were more interested in building union, rather than critical or class, consciousness. As the authors in this collection consistently argue, when class struggle gave way to arbitration and conciliation, and when men took over workers' education, it lost its revolutionary edge. With it went women's education. Women's concerns were dismissed as naive, their dreams sentimental. Union education was to serve the union, not the world. In the union as social institution, there was little room for the special needs of women.

Few of the schools for women workers survived the 1940s. Those that did dropped their radical and feminist components. One can read in their history the importance of women organizing autonomously and the power and energy when they do. The schools' ultimate demise is a reminder of what women must confront when they choose education as a strategy for change.

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Raya Dunayevskaya, *Women's Liberation and the Dialectics of Revolution: Reaching for the Future* (Atlantic Highlands, N.J.: Humanities Press International 1985).

THE BOOK ANNOUNCES ITSELF AS "a 35-year collection of essays," and indeed the earliest material goes back to 1950: a short article published in *The Militant*, press of the collection also includes assorted lectures, abbreviated summaries of lectures, radio interviews, newspaper articles, unpublished rough drafts of essays, reports by the author and by others on political events abroad, correspondence to

and from publishers abroad, a newspaper account of the author's visit to Mexico City, a draft chapter of the author's 1982 book on Rosa Luxemburg, and selected (thank heavens!) letters to friends "on the process of writing" that book. Some of the material seems to bear little relation to the title, apparently functioning rather to bolster the author's credentials. The tone is often irritatingly arch as a result of such phrases as "today-ness" (234), "today-ish" (193), "the very, very final sentence" (217), "my very first sentence" (234), or "Do you think I forgot women's liberation? No, not I" (244). One senses that, in person, Ms. Dunayevskaya might come across as the Dr. Ruth of the semi-organized left. Except, of course, that Dr. Ruth worked for Israeli intelligence and Raya Dunayevskaya was a secretary to Leon Trotsky in Mexico.

The politics of the book are as mixed a bag as its form. No one can be all bad who, like Ms. Dunayevskaya, despises Evelyn Reed's *Woman's Evolution* on both political and anthropological grounds (223, 233); who criticizes "the so-called Left's tail-ending of Khomeini" (236); who refuses to wax enthusiastic about the Russian "socialist-feminist" Tatiana Mamonova (250); who defends Rosa Luxemburg from the accusation of feminists that Rosa failed to concern herself with women's issues; and who stresses "liberation of humanity" as the necessary perspective for women's liberation (48).

On the other hand, Ms. Dunayevskaya's third-campist position with respect to the Soviet Union carries with it some very uncomfortable consequences. This position, which holds that the USSR is neither capitalist nor a workers state of any kind but rather some third form ("state-capitalist" most frequently), developed in European left circles during the 1930s. It surfaced in the (American) Socialist Workers Party during the 1940s as the Johnson-Forest tendency, a faction led by C.L.R. James (Johnson) and Ms. Dunayevskaya (Forest). The importance of "the Russian question"—the nature of the Soviet state—as debated then and now centers on the concrete politics flowing from it; for the logic of the third-camp position is an anti-Sovietism which inevitably leads its adherents into objective alliance with anti-communist crusaders. As I do not follow *News and Letters* (the press of Ms. Dunayevskaya's

Detroit-based group), I am unable to document this trajectory closely. The present volume shows it nonetheless in sympathizing with Solidarnosc (55), the Polish social-democratic intellectual/student/worker organization whose connections with the CIA have been amply documented.

The author's major project, reiterated in several of the lectures and writings in the collection, is to drive a wedge between Marx and Engels. In *The Origins of the Family, Private Property and the State*, Engels (so the argument runs) distorted Marx's thinking on the question of women and the family. Marx's real ideas are expressed in his last writings, the little-known *Ethnological Notebooks* (transcribed and edited by Lawrence Krader in 1972). What is wrong with Engels? Not much of a case against him emerges from these pages. The essay in which one would expect to find it lucidly explained (Chapter 24) deteriorates into a polemic against Hal Draper and a somewhat wandering consideration of Marx's general interests toward the end of his life.

It seems obvious, though, that what is really "wrong" with Engels's book is that it impedes the adaptation of socialism to feminism and must therefore be disposed of. It is simply too class-conscious: "Feminists are right when they refuse to follow the so-called 'orthodox' who insist that we must overthrow capitalism 'first' " (60); "More relevant than the ceaseless question of private vs. collective (or state property that calls itself Communism) is Marx's articulation of Man/Woman as the fundamental relationship." (207) And so we are given a comfortably rewritten Marx who never really believed in the primacy of class struggle, relations of production, or a full revolutionary programme. With friends like this, who needs enemies?

I suppose that the thing one would most like to say to Ms. Dunayevskaya's followers who seriously want social change is: You can't get there from here.

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Judith Rollins, *Between Women: Domesticity and Their Employers* (Philadelphia: Temple University Press 1985).

THE QUESTIONS RAISED BY JUDITH ROLLINS in her study of white employers and black domestics are crucial and enormous: "how class and race inform this female-female work relationship and the meaning of the relationship both to the women involved and to the larger society." (7) Unfortunately, while her interviews with 20 employers and daily domestics as well as her own work for ten employers provide some interesting views and pieces of information, Rollins offers little help in sorting out the relative importance and interaction of race, sex, and class or, as she also promises, of material conditions and consciousness.

The overall theme of the book is summarized when she concludes, in a manner typical of her style, that "domestic servants have always been an exploited group of workers" and that this exploitation has "the two essential functions of affording the employers the self-enhancing satisfactions that emanate from having the presence of an inferior and validating the employers' lifestyle, ideology, and social world, from their familial interrelations to the economically and racially stratified system in which they live." More "profoundly exploitative than other comparable occupations" she argues, the relationship is based on "rituals of deference and maternalism." (155-157)

Following Goffman, Rollins outlines several forms of deference. Linguistic deference is evident when employers call their employees by their first names but expect to be called by deferential terms such as *madam*, or to be addressed more formally by their last name. Deference is reflected in the structure of communications when mistresses can "ask more personal questions than domestics have the right to ask" and when it is considered the employers' prerogative to initiate conversations. (163) At the same time, employers use domestics as confidants precisely because social distance can be maintained. "Spatial deference takes two main forms in domestic service: the unequal rights of the domestic and the employer to the space around the other's body and the controlling of the domestic's use of house

space." (171) Gift giving is a deference ritual as well because "no return is expected by either party and the gifts are almost always second-hand or discarded articles." (192)

While there is little to quarrel with in these examples, it is not clear how these rituals of deference between women differ from those Goffman describes between men or between men and women. With the exception of second-hand gifts, many secretaries for instance, may readily identify these as rituals common in their relationship to the male boss. Moreover, although Rollins argues that no domestic believed herself inferior to her employers and that most were conscious of putting on an act in performing these rituals, she seems to suggest that employers are not similarly conscious, without indicating how we are to understand these differences in awareness or meanings.

In discussing maternalism, Rollins does address the question of what is unique about the female-female relationship. Maternalism "is a concept related to women's supportive intrafamilial roles of nurturing, loving, and attending to affective needs." (179) How it is related is not made clear, especially given that she goes on to argue that women were "willing to take full advantage of the class and racial inequalities generated by this social system to mitigate against their gender disadvantage." (185) What does this exploitative behaviour have to do with loving and nurturing? The question is not answered simply by stating that, "While maternalism may protect and nurture, it also degrades and insults." (186) After all, Rollins herself provides a similar description of the contradictory paternalistic treatment evident in male relations with servants of both sexes. Maternalism and deference rituals may indeed provide keys to the particular nature of female-female relationships but this is not firmly established by Rollins's discussion of them.

Her other points about the specificity of these relationships are more illuminating, however. She suggests that a mistress may treat a domestic "as an extension of the more menial part of herself rather than as an autonomous employee" (183) because women share a similarly inferior position and because they do similar work. In addition, because both are female "the success of the arrangement is measured by both more in terms of the quality of the rela-

tionship than the practical work aspects." (185) Arguing that the "main function of the domestic is the confirmation of the inferiority of the domestic," (198) she suggests that this function is better performed if the domestic is black and therefore more clearly inferior to the white employer. (129) She does not link these patterns to maternalism however or explore how such employer/employee relationships may differ from mother/daughter ones when both are engaged in performing menial tasks at home.

As well as providing an analysis of the data she collected through interviews and her employment experience, Rollins also offers a brief history of domestic service. The feminization of domestic service in North America is explained primarily in terms of the jobs available to men elsewhere which left "the females confined to the service section by restrictive social values and by their lack of training." (34) But, although she explains the later movement of women out of domestic service in terms of a weakening ideology," (37) she does not offer an explanation for this change in ideas about women's place.

Here, as elsewhere, she seems to argue that those who are accustomed to employing domestics make much better employers, because such women require fewer symbols of their superior status. Current domestics have, in addition, an "expected preference for younger women, who are seen as less prejudiced and less "picky." (148) Why this is the case, especially when they seem to have dirtier houses, is not fully explored. Jews, she says, are often better, but black employers are best. "Black employers, lacking racism and with a class prejudice modified by the heterogeneity of most black families and a history that has bred a sensitivity to the less fortunate, appear to establish a very different kind of relationship with their black domestics than white employers do perhaps even with their white domestics." (149) The existence of the class and race differences Rollins describes mean that her data on the experience of working for such employers are limited, however.

While the book does not provide answers for some difficult but important questions, nevertheless it is a useful and important study because it raises issues that are central to any theory of women's work. And it does provide

some provocative hypotheses which require further exploration. We do indeed need to know whether women are different kinds of employers, whether women as employers are different in the home than they are in the labour force, and whether race and maternalism are central to relations between women.

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Ken Gagala, *Union Organizing and Staying Organized* (Reston, VA: Reston Publishing 1983).

IS THERE A MORE IMPORTANT QUESTION facing the American working class than the issue of 'getting organized'? This has two sides. There is, of course, the historical question of the political failure of American socialism to develop a mass party. But it is also the story of the small, fragmented, and decentralized structure of the trade union movement. As in Canada, unions are usually local and employer-specific. Nationally, American unions are marked by a heterogeneity of union types and jurisdictions and a weak and bureaucratic trade union central. U.S. trade unions now find themselves in an organizational battle for even their marginal place in American capitalism.

The strength of any union movement is ultimately related to its ability to recruit, organize, maintain, and mobilize its membership. In this area, American unions are suffering serious losses. With the burst of industrial unionism during the 1940s, U.S. unions could claim about 35 per cent of the non-agricultural labour force at the high point of 1954. This initial hopefulness has been followed by a remorseless decline to the present levels of only about 17 per cent coverage of the workforce. The sharp recession of 1982 provided another blow to unions, losing members in auto and steel, for example, of 240,000 and 100,000 to layoffs. The recession marked as well a general retreat of employment in mass production industries, where union organization has had its greatest strength in recent years, and a trend to service sector employment which has histor-

ically been difficult to organize. As if these structural shifts were not enough, employers have been on an offensive against unionization, dating back at least to the defeat of the 1978 Labour Reform Bill through to the 1981 destruction of PATCO and continued wage concessions bargaining. This has meant vociferous opposition to organization drives, often led by union-busting consultants, and a precipitous decline in success at NLRB representation elections. Moreover, because concessions at the collective bargaining table have not served to stabilize employment

bolden management, decertification elections are becoming an equally important front testing union strength. This combination of factors — long-term failure at organizing, decline in mass production employment, fragmented structure of the service sector, and an anti-union offensive — has produced the current organizational crisis of American unions. In this context, a book addressed both to 'union organizing and staying organized' is a welcome relief from the endless stream of management literature on union-free workplaces.

This book is a valuable and practical aid to overcoming some of the difficulties of organizing in the U.S. Written from within the mainstream of the American labour movement, the book is an attempt, as Gagala puts it, "to improve the odds for unions in meeting the major challenge confronting them today: organizing." To improve the odds, Gagala takes the reader through some of the general social issues confronting unions and a step-by-step treatment of the procedures for certifying a union. But this is only the first task. After certification, Gagala shows the battles and procedures for gaining a first contract and the necessity to fend off management attempts to cut the union out altogether. This latter difficulty is the now thriving and nasty business of union decertification, filled with company spies, intimidation, bad-faith bargaining, factory closings, and high-priced anti-union publicity campaigns.

Gagala does an admirable job of detailing some of the legal logistics unions must meet to certify a union in the U.S. For example, he covers the legal requirements of access to employees during a union drive, the proper procedures to follow in the certification vote, and how to document unfair labor practices for

presentation to the labor relations board. This is all aided by charts, checklists, tips on record-keeping, and sample forms, which gives the reader some practical guides to use for organizing. The book is on even stronger ground in covering some of the operational tactics that will be necessary in an actual campaign. These points often seem quite straight-forward, yet they are precisely the kind of instructions which can save considerable time for the new organizer. These include diagramming the worksite for best access to employees, detailing the target unit, and how to gather research materials going into negotiations for a first contract. As much as possible, reflecting the book's origins in union education courses, these practical guides are supplemented by case studies of organizing, often gathered from the author's own experiences.

Finally, Gagala goes over some of what a union local must do to stay organized. During a period when management opposition to unions, and their use of illegal tactics, is one of the most serious challenges to union organization, this is important material. The focus is restricted in the internal operations of the local union and its direct confrontation with management. The advice is conservative and within the legal parameters of the NLRB; but within these limits, there is much that is helpful. The local must carefully assess the balance of power and plan each collective bargaining round accordingly. It must also keep in touch with members as much as possible. Surveys provide a good way of canvassing opinions, but training stewards and giving them an active role is necessary. In fighting decertification, the union must be equally aggressive in making its case, but also ensure to document management violations. And so on. While the book does not venture into the wider arena of strategic debate about organizing, it ends on the note that there is now the need for U.S. unions to address their larger public policy stance.

There is all of this and much more. The book's practical usage for union organizing in the U.S. is readily apparent. Canadian readers will note the comparison to the excellent similar book, with reference to Ontario, by Mary Cornish and Laurell Ritchie, *Getting Organized: Building a Union* (Toronto: The Women's Press 1980). While suffering partially the fate of handbooks in needing updating al-

ready, the Gagala book misses the 1985 AFL-CIO new strategy on organizing and the further legal restrictions from the Reagan NLRB, the two books compare favourably. The additional strength of the Gagala text, which arises out of the more hostile American environment, is the attention paid to combatting the union-avoidance techniques of union-busting management consultants.

Gagala's comments tend to be of the operational sort on how to rebut management ploys such as delays, false information, and dismissal threats, in situations where the union base is tenuous. At times he suggests efforts to involve the community in specific labour struggles. One can point to any number of recent incidents on both sides of the border where community involvement has been vital to blocking management efforts to break unions. However, Gagala keeps close to the line of offering the technical and practical advice to aid the union cause, most of which follows legal precedents. This is all to his favour, but it does leave certain shortcomings if it is building unions that is at stake. For example, as the book itself indicates, many of the largest areas in need of organization are the service sector and the new jobs created by new technologies. Many of these occupations have concentrations of non-white and women workers and may require special organizing efforts which are not to be found in previous organizing drives. These new organizational innovations are not obvious, but they require discussion even by the seasoned organizer.

Nonetheless, this study is original in bringing together many of the social and practical issues of organizing unions in the U.S. By that fact alone, the text has a valuable role to play as a handbook for organizers, a text for classroom teaching, and as a practical run-through the topic for academic researchers. But union organizing is never exclusively a matter of technical and legal issues; it is equally an issue of politics and culture and these can not be neglected in a full treatment of union organization. This criticism should not be taken to be a mere academic shortfall. If unions are to regain a stronger hold over the American workforce, it will require a shift away from the legalisms of the NLRB and a break with the strategy of union leaderships making deals with the business elites at the head of the Democratic Party.

With the recent publication of the AFL-CIO's *The Changing Situation of American Workers and Their Unions* (1985), which continues to emphasize locally-based accommodations with management, this is not the most hopeful of periods. Yet the task of adequate workplace representation remains one of the most basic of democratic struggles.

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Paul Burstein, *Discrimination, Jobs, and Politics* (Chicago: University of Chicago Press 1985).

PAUL BURSTEIN HAS WRITTEN A BOOK that focuses on a relatively narrow subject, but in the process he has asked the broadest and perhaps two of the most important questions one needs answered about liberal democratic society. His subject matter is Equal Employment Opportunity (EEO) legislation in the United States. His goal, however, is not merely to explain the history and nature of EEO, which he does rather well, but to use EEO to test out two very significant issues relative to the American political system.

First, the manner in which Burstein reviews the evolution of the EEO concept to its ultimate transformation into law directs one's attention to the relationship between the passage of this particular piece of social reform legislation and the changing climate of public opinion in the U.S. with respect to discrimination in employment. In other words, Burstein implicitly asks if, or to what extent, democracy works. When, why, and how did the U.S. Congress overcome the barriers to EEO that had been impairing its passage ever since it was first introduced in the late 1930s? The second vital question Burstein asks is whether, ultimately, anti-discrimination laws make any difference to the groups they seek to protect, or whether, as conventional economists' literature suggests, legislation has no impact on discrimination in the labour market.

The history of American federal EEO legislation is important to these enquiries. Employment discrimination has a longstanding record of its own in American society. Why then was it not until the late thirties and early forties of this century that it first appeared as a serious object for federal legislation? Burstein argues that prior to that period it was virtually impossible for the federal government to intervene in the labour market, given the then-dominant ideology with respect to the role of the state, an ideology reflected in U.S. Supreme Court decisions which were enormously damaging to reformist goals, as the Court ruled against crucial parts of the New Deal program. Political change, he contends, does not happen simply because people are oppressed. It very much depends upon a basic psychological precondition involving a conviction that reform is possible. That conviction began to gain momentum after the Supreme Court started to reverse its stand on federal intervention in the economy in the closing years of the decade of the thirties. Thus, shortly after the Court's decisions began to indicate that an EEO law *could* be passed, members of Congress began to propose that one *should* be passed.

Burstein then goes on to code and analyse the hundreds of EEO bills which were sponsored in Congress between that period and the time when a bill was finally passed in 1964 (known as Title 7), and amended in 1972. Although there were numerous possible combinations of group categories to be singled out for protection against discriminatory practices in the job market, until 1964 the vast majority of bills included only three categories; race, religion, and national origin. Post-1964, discrimination on the basis of sex gained support and became part of the standard package. (This addition originated as a "joke," introduced by Representative Howard Smith of Virginia, a known opponent of all civil rights legislation, who meant only to complicate the debate; ironically, after his amendment was proposed, the sex category became broadly accepted as a necessary adjunct).

Using sponsorship for EEO bills as an indicator of the growth of support for anti-discriminatory legislation, Burstein finds that between 1941-42 and 1971-72, support in the House and Senate stayed the same or increased

between congresses twenty-one times, and it decreased nine times; most of the increases and decreases were relatively undramatic. Thus, support tended to increase gradually over time, consistent with the hypothesis that Congress responded over the years to variables that similarly changed steadily, such as increasingly favourable attitudes toward Jews, blacks, women, and other minorities.

The significance of this pattern of response is that it indicates that this legislation was not simply a novel, desperate attempt to silence civil rights demonstrators of the early 1960s. On the contrary, American EEO legislation as it stands today is essentially a completion of the ideas and goals generated during the depression of the 1930s. Although there is plenty of evidence that discrimination is still pervasive in the labour market, legislative efforts in this area virtually ceased in 1972. Burstein contends that this failure to tackle the problem with ingenuity and innovations is due to the fact that nothing new has been added to the concepts inherited from the past; indeed, members of Congress seem to have run out of ideas. The EEO law was born along with others aspects of the New Deal, and cannot be said to encompass sparkling, fresh, creative elements that reflect current social conditions. It is, as Burstein says, a manifestation of the fixity of ideas in the midst of social change.

Clearly then, Congress moved gradually towards implementing this well-established notion. What prompted the American legislators to finally come around? Those who think it was due primarily to the threat of civil riot and massive social unrest associated with the civil rights movement are wrong; nothing this complicated is as simple as that. Congressional action on EEO was the product of a complex set of forces, not the least of which was changing attitudes. Congress acted belatedly on this part of the New Deal agenda because of shifts in public opinion, affecting and effected in turn by more able lobbying on the part of representative minority organizations, by electoral competition, by the infusion of new blood in the Congress through turnover, and undoubtedly by the intense feelings aroused by the civil rights movement.

At the end of his book, in his conclusions, Burstein examines the relevance of Congressional responsiveness to the public, raising the

question of its meaning for the functioning of democracy. This is a big question and should not have been introduced as an afterthought. There are too many variables, as Burstein himself admits, in order to make the connection. Some might argue, for example, that Burstein's analysis of EEO does not prove the efficacy of democracy so much as the incompetence of political leadership. Nevertheless, the author maintains that the congressional system has proved itself a sensitive instrument for expressing very significant ideological shifts that took place in American society, in the three areas that mattered most to the public in the last fifty years — the economy, war, and civil rights. Since this reader saw this issue as a fundamental theme long before she came to the end of the book (and presumably others would too), and since the argument at the end has left too many unanswered questions, this is undoubtedly the weakest feature of this book.

Fortunately, the second part of the book in which Burstein asks the second dominant question noted above, is faced squarely and from the beginning. Did EEO legislation help those it was designed to help — blacks, Jews, women, etc.? Burstein finds that there are two standard sets of answers to this question. The first approach we might call journalistic and anecdotal, which on the whole takes a position that EEO legislation has had a radical impact on American society, much of which is seen as negative. The second approach is a statistical one, mainly undertaken by professional economists, who recognize that the incomes of nonwhite men have risen significantly since World War II compared to those of white men, but they claim that most of the income gain should be attributed to increasing relative productivity. Therefore, according to these studies, EEO legislation has been of little consequence for black men. A couple of such statistical analyses of male/female income differences conclude that EEO may have improved women's incomes very modestly, but other studies show that women's incomes compared with men's are actually lower now than they were thirty years ago. Thus, on the whole, the statistical approach suggests that the struggle for EEO was hardly worth the trouble.

It is at this point that the advantages of using an interdisciplinary approach to address a complicated social issue — as Burstein does

— become clear. Burstein draws on economists' formal models but places them in a larger context, emphasizing the political economy aspect in his analysis of the struggle for EEO. Economists' models are flawed, he reasons, because they leave out the social dimensions, including such important items as the "taste for discrimination."

Conventional statistical methods rely on published measures of income, from which are computed black/white or female/male income differences, which is followed by an analysis of the effect of relative productivity, job availability, and enforcement of EEO legislation on these differences. This method might appear perfectly sound and decidedly objective, but is it?

Let us say, for argument's sake, that a seemingly rigorous economic model ignores the real fact of prejudice in the hiring process. How will this affect the reading (or misreading) of the meaning of income differences and their changes over time? Let us also assume that attitudes towards blacks and women in the labour market became more favourable at the same time that EEO was passed and enforced (a not unreasonable assumption, according to the evidence produced by Burstein), gains in relative income may very well be due to both changes in attitudes and to EEO. If then only the enforcement factor is included in the analysis, obviously the resulting conclusions are necessarily suspect. Thus, Burstein sees good reasons to include the matter of attitudes to his model.

Burstein points to a second flaw in the those earlier statistical analyses, related to the way in which income is measured. Without getting too far into the details of his argument, it makes very good sense that he should reject a measure of the income of a group that excludes a high proportion of the adults of that group not in the labour force. To demonstrate the significance of this problem, let us consider the possibility that EEO legislation had the effect of driving low-wage blacks out of the labour market (as some anecdotal reports claimed), those who were pushed out would not have their incomes included in the census wage and salary data. Black incomes might then *appear* to rise relative to white incomes because those blacks with low incomes would not be averaged into the calculations. This is only one

of several serious problems Burstein reveals in regard to standard techniques of measurement used by economists who have studied this issue.

A third difficulty in the standard approach is that it deals separately with race and sex discrimination. In an areas such as this, where the permutations and combinations of discrimination are clearly relevant, this separation can distort the true situation. For example, it may exaggerate the progress of nonwhite women by comparing them with white women who are themselves discriminated against.

Burstein works through all these complications in a very rigorous way and comes up with some interesting and different results from that of his predecessors with regard to the consequences of the struggle for EEO. I will not attempt to summarize all the facets of his argument and conclusions, but the overarching result of his inclusive analysis demonstrates that EEO legislation *has* made a difference. It has been particularly beneficial to nonwhite women, but white women and nonwhite men have seen some improvement as well. Moreover, non-white men have not been harmed by EEO as some of the journalistic articles contend. That is not to say that discrimination is a thing of the past, not by any means, but it does suggest that when the struggle for human rights moves to the courts and administrative agencies, there is a real possibility for making an impact in a positive way.

I found the results of this study very interesting, but not nearly as interesting in themselves as the way that Burstein arrived at them. The author uses a number of theoretical and methodological techniques which have been developed and utilized by others, but his particular innovative contribution is that he applies them in combinations that reflect more accurately the social context in which discrimination and anti-discriminatory law operates. Far too often, to the intelligent non-specialist observer, academic disciplinary studies seem to negate his or her best judgement of a situation. Are such discrepancies due to the subjectivity of this casual observer or to his/her lack of scientific treatment — as professionals are likely to claim — or are they due to faulty techniques on the part of the single-discipline scholar?

Burstein's work makes clear that one must not only suspect intuitive reasoning, but that



scrutiny of conventional methodologies is absolutely essential. One is apprised here of the dangers of unthinking abstractions when applied to complex social questions. In my opinion, the field of economics is perhaps more open to criticism on this score than other social sciences (as an economist myself, I feel well-placed to say as much), because in this specialty analytical models are far more prized for their "elegance" than for their appropriateness to social reality. Time and again, economists' gross abstractions have misrepresented the social phenomena they choose to examine, which ultimately misleads a lay public that is unaware of the flimsy foundations upon which their arguments rest. In this book, Burstein has shown that interdisciplinary analysis is methodologically superior to that of the narrow disciplinary treatment, especially in the hands of a tough-minded, thorough, and thoughtful scholar.

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Claus Offe, *Disorganized Capitalism: Contemporary Transformations of Work and Politics* (Cambridge: MIT Press 1985).

THIS IS THE SECOND VOLUME OF Claus Offe's essays to appear in English (in rapid succession to *Contradictions of the Welfare State*, 1984), and should help round out our knowledge of one of West Germany's leading social theorists. Many of the papers in the present volume pertain to work, trade unions, and the labour market and are of particular interest to readers of this journal. A number of the others touch on important political themes such as collective action, the public realm, or the majority rule principle.

Let me begin with an admonition. Those coming to Offe's essays in search of a grand theory for our time will be disappointed. This is a careful, at times lucid, set of arguments and analyses, but the author does not claim to deliver a synthetic statement about where capitalism is heading or the alternatives that the left should be proposing in its place. There

are hints at such alternatives strewn throughout his discussion of the "informal" or "dual" economy, or his rejection of the role of work as the key sociological category. Still, Offe's is not a prophetic or messianic stance. At times, in fact, one wishes he had donned the mantic garb of Marx or Weber, allowing himself a poetic flight or two. What we get instead is good, even superlative, social science, but presented in a language that is dense, occasionally wooden, in character. What is the ordinary reader to make of phrases like "agent of the conscious synthesization" (139) or "reversal of the direction of the conversion process?" (304) This is sociologese, of which neither Offe nor his translators have reason to be proud.

If the reader, however, manages to surmount the formidable hurdle posed by social science terminology, there is much of value to be gleaned from this book. Offe's particular strength is subtlety of argument, and an ability to probe beneath the surface appearance of events. For example, in setting out to discuss the role of interest groups, Offe distinguishes among individual social actors, the organization itself, and the social system in general, what he terms perspectives from "below," "within," and "above." By combining all three in his own analysis, he is able to pinpoint the pervasive shifts that have occurred in western policies "from conflict over group interest to conflict over general rules, from the definition of claims to the definition of legitimate claimants." (231) New institutional actors come to be sanctioned, usually by the state itself, resulting in a form of corporatist development "that exclusively relies neither on the social class nor on the pluralist group paradigm." (240) The inability of political parties to perform their traditional integrating function has led to an increased role for major organized interests, but also for "marginal" segments of the social structure focusing on issues like peace or the environment.

In discussing the theory of collective action dear to writers like Mancur Olson, Offe shows that the maximization of rational self-interest that ostensibly stands in the way of large-scale co-operative behaviour is but one level of perception. Once one is prepared to question the parameters within which decision-making occurs, one moves from questions of distribution to the appropriate political and as-

sociational forms within which collective action takes place. This allows Offe to address the rather distinct role that unions play in representing workers' interests as opposed to that performed by business associations in relation to capital, and to further examine the risk of distortion to which unions are amenable. This leads him to posit a sociological theory of opportunism based upon a five-stage recurring cycle in which self-affirmation, institutionalization, reversal, and fresh mobilization of membership all play a part.

In a particularly fine piece of analysis, Offe illustrates the limitations of majority rule in the political realm. While "one person-one vote" has become the operating principle of our time, this was not always the case, as a brief examination of both conservative and socialist attitudes to the franchise suggests. Equally potent are the limits to majority rule today. How much does procedural fairness correspond to genuine citizen participation? How does one measure the intensity of citizen concern for matters on which they are voting? How does one balance out short-term interests with long, for example, the concern of a minority with environmental issues that may come back to haunt future generations and the decision of a passing majority to stock nuclear weapons or construct reactors? There are no obvious alternatives to majority-rule principles, though Offe suggests at least eight devices that could strengthen democratic practice and reduce the "self-legitimizing" role of the political system. These include decentralization, a greater use of proportional representation, completion of the civil rights and constitutional guarantees of the constitution, new forms of communication between science and the public, and the extension of welfare-state participation rights.

On the future of work, Offe is drawn to the anti-productivist arguments of Foucault, Touraine, and Gorz. Yet he is fully aware of certain weaknesses with "post-materialist" arguments about western society. The structures, actors, and rational principles of the latter are but vaguely sketched in the relevant literature. Nor does the latter provide a convincing explanation as to why the sphere of work and production have forfeited their power to determine social life.

The service sector looms large in contemporary sociological theory. As the traditional

wage labour system has contracted, there arises an increased demand for "mediation and conciliation" activities, sparked by changing social needs on the one hand, and the new supply-side interests of workers on the other. Still, there are limits to the growth tendency of this sector, stemming from budgetary factors, mechanization and rationalization, and an eventual saturation of demand for service sector products. (127)

Full employment seems to have vanished from the horizon of western societies. Right-wing orthodoxy has little use for it, criticizing "the alleged hypertrophies of the welfare state." (84) Even trade unions are by no means prepared to altruistically sacrifice some of the present or future income of their members for the unemployed outside their ranks. Realists for their part (shades of the Macdonald Commission?) have come to argue that full employment can no longer be guaranteed politically, that Keynesian instruments have become blunt. For Offe, ours is an economy in which adequate opportunities for work are vanishing, and where a system of "net" dividends may have to supplement the traditional labour exchange mechanism. He anticipates the type of alternative that Bill Jordan, in his 1985 study, *The State: Authority and Autonomy*, develops more fully, combining a guaranteed annual income with continued motivations to work.

I could go on at greater length, addressing various of the themes that Offe presents. Instead, in bringing this review to a close, let me consider the larger theme that figures as the title of his collection, *Disorganized Capitalism*. As Offe makes clear in his introduction, his is a conscious rejoinder to Rudolf Hilferding's phrase "organized capitalism," that back in the 1910s and 1920s posited a new ("higher") phase of capitalist development. For Hilferding in 1927, "We are moving. . . from an economy regulated by the free play of forces to an organized economy. . . This planned and consciously directed economy supports to a much greater extent the possibility of the conscious action of society" (that is, the state).

By emphasizing the "disorganized" nature of the contemporary system, Offe is highlighting its apparent contradictions, the dysfunctionality it gives rise to, the new forms of political movement it engenders. He is perfectly

within his rights to do so. Yet I cannot help feeling that he overstates the case. Just as the fiscal crisis of the state, so vigorously argued by neo-Marxists like O'Connor, has not brought with it the demise of the capitalist system (*tout au contraire*), so would it be prudent not to over-emphasize tendencies to disaggregation or breakdown in the present period.

True, there are new forms of interest articulation, new roles for political parties, new social forces like the environmental, women's, or peace movements to be considered. The legitimacy of the welfare state has come under attack from the right in the English-speaking world, while tripartite solutions have been advanced and opposed in a variety of places from Scandinavia to Central Europe to Australia. Yet the logic of capitalism, both at the national and international level, continues to require a much more organized role for both state and economic actors than in an earlier day. "The deficiencies of existing forms of political mediation" (7) that Offe points to may be the logical accompaniment of an ongoing and far less catastrophe-prone system than he would acknowledge. The era of *organized capitalism* that Hilferding prophesied will be with us, I fear, for long decades to come. Paradoxically, it is as a result of its further development that hopes for a more de-centralized, democratized, and emancipatory form of politics may eventually come to fruition.

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Harry C. Katz, *Shifting Gears, Changing Labor Relations in the U.S. Automobile Industry* (Cambridge: MIT Press 1985).

PERIODICALLY, UNITED STATES LABOUR relations experts herald the arrival of participatory management. This noble tradition traces back to Sumner Slichter's "Labor Policies of American Industries" in the *Quarterly Journal of Economics* (1929), and up through the work of Elton Mayo, Douglas McGregor, James O'Toole, Richard Walton, and now Harry Katz with *Shifting Gears*, an examina-

tion of labour relations in the U.S. automobile industry. In this already widely-read book, Katz offers what is perhaps the most persuasive case to date regarding the potential for participatory management.

Katz cannot be faulted for lack of ambition. He begins the book by calling for a general framework for studying industrial relations. He proceeds to offer such a framework, applies it to the U.S. automobile industry, and then uses the lessons learned to consider the future course of labour relations in the U.S. both in and out of the auto industry.

Readers need not be put off by Katz's claim to offer a new framework for the study of I.R. His methodological claims, delivered in the first chapter, are few and not entirely controversial. He rejects as counter-factual the old convergence theory of I.R. (that is, the Kerr, Dunlop, Myers, Harbison view that I.R. systems are becoming more alike over time) in favor of a more historical approach. He believes that both micro-level statistical research and case study datum should be brought to bear when considering history. He believes that research should emphasize economic or environmental factors, while simultaneously incorporating the behavioral, strategic considerations of social-psychology.

The second chapter makes the case that three institutions characterized labour relations in the U.S. automobile industry from the close of World War II until 1979: a wage determination mechanism which regularized and stabilized wage increases; a connective bargaining structure which tied workers and firms throughout the industry and unified bargaining outcomes; and, a job control focus. Katz explains how these institutions channeled labour-management, investments, and profits. Labour became a quasi-fixed factor of production with largely standardized costs, offering workers high and reasonably stable incomes.

Katz's third chapter describes the breakdown of these three institutions beginning in 1979. He documents the well-known givebacks regarding wages, local agreements breaking national agreements (hence breaking connective bargaining), and pressures to reduce the extent of work rules. He makes a strong case that the old I.R. system in auto was shattered by enhanced international competition, severe unemployment, and the general economic malaise

of the late 1970s and early 1980s. Numerous other authors have argued similarly that there existed a post-War "accord" between big labour and big capital, an accord which broke down in the late 1970s. Surprisingly, Katz does not cite any of this literature (for example, Gordon, Edwards, and Reich's, *Segmented Work, Divided Workers*, or Bowles, Gordon, and Weisskopf's, *Beyond the Waste Land*).

Chapter four considers participatory management initiatives, arguing that pre-1979 efforts were mainly tacked-on to the traditional I.R. system, but that post-1979 efforts have been more substantive and offer the possibility of a coherent alternative I.R. structure. This argument rests mostly on the sort of anecdotal evidence common in the participatory management literature, yet it is researched and argued in a powerful fashion.

The fifth chapter seeks to show that the I.R. system has an intimate connection to the economic success of a firm, and that Quality of Work Life (QWL) programs, a form of participatory management, can have a significant, positive impact on both the I.R. system and economic outcomes. Katz is able to establish the former claim — that positive I.R. results are associated with positive economic outcomes — with relatively little difficulty. It should not be surprising that Slichter's claimed "close relationship between industrial morale and efficiency" receives support. However, Katz is not able to find any significant relationship between QWL programs and either I.R. or economic outcomes. How does Katz explain this weakness given his theory?

Basically, Katz argues that since his data mostly cover the 1970s (1970-79 in one division, and 1978-80 in another), it cannot capture the impact of newer, more substantive participatory management programs based upon work teams. That is, early participatory management initiatives were constrained by the three traditional auto I.R. institutions and exhibited little effectiveness for that reason. In the 1980s, since those institutions have been weakened, participatory management might be expected to have a more positive impact on I.R. and economic outcomes. The absence of recent evidence is extremely unfortunate since the data presented undermine any argument regarding the potential for participatory management. In fact, the article (with Thomas Kochan and Ken-

neth Gobeille) on which Chapter 5 is based is usually cited as showing the QWL is *not* the wave of the future. This hardly helps to establish Katz' argument.

Chapter six is an exercise in futurology regarding labour relations. Katz argues that depending upon economic conditions, the auto industry could return to normalcy (if conditions improve markedly) or face warfare between managers and workers (if conditions deteriorate seriously). Katz perceives the most favorable future environment for participatory management as a continuance of the current state of mild economic stress. On the one hand, if economic pressures are too severe, management will attempt union-busting, out-sourcing and other trust-destroying tactics. On the other hand, if economic pressures are too light, management will feel no pressure to gain workers' cooperation. For workers, similarly, serious wage cuts and frequent lay-offs are likely to produce militance and low morale, while a healthy economic climate could similarly lead to strikes and low levels of effort. In between these extreme scenarios, participatory management will offer firms the cost advantages of high worker morale, flexibility in terms of jobs, work assignments and pay (through profit-sharing), while workers gain influence over the labour process, a more humane workplace, and possibly employment security.

The final chapter considers lessons from the auto experience for other U.S. I.R. systems. Katz suggests that auto has historically been a trend-setter, but that this position may now be compromised by the ascendance of non-union operations throughout major sectors of the U.S. economy outside of auto. Recognition of the success of participatory management in non-union settings might suggest that unions in auto will be weakened by participatory management, yet Katz argues persuasively that the high degree of organization by the UAW in auto may prevent such an outcome in that industry (though not others).

My assessment of the current industrial scene is similar to Katz's. There is indeed a movement towards participatory management in a majority of Fortune 500 companies in the U.S., as well as a continuing decline in unionization levels. Further, in highly unionized industries, such as auto, participatory management may offer unions a renewed sense

of vitality and purpose. Finally, it is not difficult to accept the prediction that slow economic stagnation will continue for quite some time: neither the halcyon days of the 1950s and 1960s nor the upheavals of the 1930s seem to be in the cards.

Two fundamental problems exist in Katz's work. First, he seems to accept implicitly some sort of model of industrial pluralism (as did, of course, Kerr, *et al.*). If one accepts the pluralist view, then any voluntary practices which are acceptable to both labour and management must work to the *advantage of both labour and management*, similar to Adam Smith's defence of free trade as welfare-enhancing for all participants. Ultimately, Katz's defence of participatory management seems to rest on such logic. Nonetheless, it may be wrong. Two-tier wage agreements clearly work to the disadvantage of some actors — young or new workers; give-backs due to the decline of the auto industry operate to the detriment of some actors — all workers; and robotics act to remove customary skills from some actors — skilled workers.

If Katz rejected the pluralist view by focusing on the inherent inequality of the employment relationship, he would not have been so sanguine about the possibilities for participation. For example, Katz recognizes that high levels of unemployment in auto have made workers docile enough for participation, yet he does not stress the logical corollary that in the absence of such external coercion, participation will not function. This argument in itself should make us leery of the humane, satisfaction-enhancing quality of capitalist participation initiatives.

The second problem is that Katz misses the coincidental logic of outsourcing and participatory management. Katz argues that participatory management may require employment guarantees for morale purposes and that outsourcing may reduce employment security. Thus, he suggests a trade-off between the productivity advantages of job security and the short-run cost advantages of outsourcing. This logic elides the possibility that outsourcing may serve to increase employment security for a relatively small number of workers. The Saturn agreement between GM and the UAW, for example, defines two types of employees, those with job security and "associates" without it.

It seems plausible that the automobile industry is intent on creating an elite corps of workers who are flexible, loyal, highly paid, and company oriented. To ask whether these workers are pro- or anti-union, involved in participatory or paternalistic management, etc., seems to miss the point: most workers will be left out of such a system, relegated to jobs in the secondary and subcontracting sectors, and subject to the vicissitudes of frequent unemployment, low pay when jobs are attained, and (unless history is no guide at all) disproportionately composed of minorities and women. This scenario represents a reinvigorated setting for labour segmentation. If this argument is correct, then a critical element of future I.R. in U.S. auto does not involve the big three auto makers at all, or at least not directly, but rather the new, small firms which are supplying an increasing share of the auto makers' needs. This development seems to exactly mimic recent Japanese industrial experience, with its apparent participatory management for a small, elite core of unionized workers, and a larger, mostly non-union group of temporary and subcontracted employees who work in less fulfilling circumstances. Thus, Katz's vision of participatory management must be faulted for missing an obvious comparison with Japan, a rather surprising lacunae given that Japan offers the present model for such initiatives.

I recommend the book strongly, particularly to those in the field of Industrial Relations. Regardless of substantive problems, the book is well-written, well-researched, cogent, coherent, and is likely to gain some measure of influence in I.R. research and policy circles.

Robert Drago  
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Robert McCarl, *The District of Columbia Fire Fighters' Project* (Washington, D.C.: Smithsonian Institution Press 1985).

THIS BOOK IS ONLY HALF the story. Smithsonian Folklife Studies consist of a monograph and an accompanying film (not completed at time of writing). The idea of studying the "oc-

cupational folklife" of fire fighters may seem odd, even old-fashioned. It is neither. Robert McCarl is part of a group of researchers who practice a sophisticated, reflexive, and democratic type of social study.

The potential and enormous responsibility of work in Occupational Folklore is that the subject speaks, walks, and lives. Few historians face the possibility of those they have written about coming back to say that the book is untrue, incomplete, wrong, or actually damaging.

McCarl's book is about work rather than the history of institutions, workplaces, or unions (though he had the close co-operation of the fire-fighters' union). His topic is work as an activity, as technique, custom, talk, and narrative. Shouted messages or radio instructions are part of work. Conversation and narrative are part of the overall occupational culture.

There is some interesting labour history in this book, particularly about the racial segregation of fire-fighting in the District of Columbia which continued until the early 1960s. There is interesting material about women in the workforce. Yet the book as a whole should not be gutted for this material. It has an integrity that should be respected.

The central part of the book, "Good Fire/Bad Night" has already been published for the fire-fighters. (It raised \$900 for the local burn unit.) It is part of McCarl's approach to have given back the result of the study in this way. There was also an oral presentation, which was poorly attended.

"Good Fire/Bad Night" is a very moving and imaginative piece of writing. It is wrong to think of it as a report since it uses the words that are already part of the work, whether at the firehouse or at the scene of action. We do not have many descriptions of actual work in this detail and with this sensitivity.

It will be read in many different ways. Work is the site of the fundamental contradictions of our society. McCarl describes the fine detail of racism by these unionized workers: in the early 1960s not speaking *at all* throughout every day to a black fire-fighter. He describes the day-to-day culture that women workers face. And when the majority of administrative officers finally are black it turns out that they are very conservative.

The third part of the book is McCarl's

commentary and analysis of the material presented in "Good Fire/Bad Night." The focus of industrial folklore theory is the metaphors, habits, and techniques of the job. Work is understood as an embodied flow of actions, equipment, materials, and the world. The central attention in this case is when that world is on fire. Speech and narrative (of several kinds) are understood in terms of this embodied technique of work.

The first part of the book is an introduction which includes the full text of the response to "Good Fire/Bad Night" by the president of the fire fighters' union. This critique by Captain David A. Ryan is itself an extraordinary document. Shocked by the first draft of "Good Fire/Bad Night" he came to the conclusion that "the men themselves were living one life and apparently believing another." Thinking about this, he is brought almost to conclude that we are *all* living one life and apparently believing another. But he dodges this conclusion. There must, after all, be truth somewhere. Perhaps if McCarl had longer than one year to work on the project. Perhaps this kind of work is before its time. Captain Ryan's misgivings about what he has read are perhaps only his "personal opinion." Perhaps the academic community can solve this puzzle about the nature of truth. But then a wicked thought: what if the academic community is also living one life and apparently believing another?

I cannot doubt the success of this wonderful book. Not because it raised \$900 for a hospital. Not because it gave back something to those it studied. But because for one reader, Captain Ryan, it opened a sense of wonder — a gnawing question — this is education in its fullest meaning. How few, how very few, books ever do this.

Alan O'Connor  
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Isaac Deutscher, *Marxism, Wars and Revolutions: Essays From Four Decades* (London: Verso 1984).

ISAAC DEUTSCHER IS BEST KNOWN for his biographies of Stalin and Trotsky. They are marvelous works, full of learning and beautifully written; in them Deutscher emerged as a Marxist historian of enormous sophistication with a rare capacity to reveal the complex relations between individual character and economic and social reality. The present collection of essays is of the same fine quality and, as Perry Anderson points out in the introduction, has the added advantage of revealing more of Deutscher's personal views than his more strictly historical works. The personal element is further developed in a moving introduction by Deutscher's widow, Tamara, who has edited the collection. Though many of these pieces have appeared in earlier collections, some have not and others are now out of print, so that we can only welcome the appearance of this volume.

Tamara Deutscher has organized this collection into five categories: the USSR, the Cold War, Europe, China, and Marxism in Our Time. Despite their diversity, the pieces all reveal Deutscher's life long preoccupation with what Anderson calls "the fate of the Russian Revolution," (xi) and with the prospects for a world liberated from exploitation and oppression. Central to his understanding of these topics is Deutscher's focus on material scarcity and backwardness as crucial factors in historical development: the brutality of Stalinism; the bizarre cults of personality linked to Stalin and Mao; the irrationality, puritanism, and chauvinism of the Chinese Cultural Revolution; the creation of bureaucratic dictatorships in the aftermath of proletarian revolution; the pathetic vulgarity of "official Marxism" when compared to the theoretical tradition running from Marx through Luxemburg and Lenin; and the failure of socialist states to create a new socialist man — all these Deutscher attributes in large part to the absence of material abundance which would have supported an egalitarian, democratic, and humane social order rather than what emerged in the backward regions where revolution ended capitalism. Deutscher concludes that "mankind as a whole is paying for the confinement of the revolution to the underdeveloped countries." (211) But if Deutscher regrets the consequences of backwardness, he refuses to write off the Russian and Chinese Revolu-

tions as historical setbacks. Rather, he sees the creation of societies based on nationalized property as a step forward from capitalism which lays the groundwork for the ultimate triumph of genuine socialism rooted in proletarian democracy. He argues that, though such states may be bureaucratic dictatorships and hence not strictly speaking socialist, nevertheless collective property and non-capitalist industrialization create both the proletariat and productive capacity essential to eventual working-class rule. Deutscher thinks that even Stalin's foreign policy has played a progressive role: Just as Napoleon's armies spread the French Revolution to conquered territories from above, so Stalin, in imposing Soviet domination in Eastern Europe, laid the foundation for a progressive outcome which will compensate for the undeniable brutality of his policy.

Of course Deutscher's conclusions are debatable. No doubt scarcity is an overwhelmingly important factor in explaining much of what has gone wrong since 1917. But Deutscher's position is sometimes too rigidly deterministic. For example, Carmen Sirianni has argued convincingly that Stalinism was not the inevitable consequence of backwardness: the collapse of worker participation in the Soviet state and economy after the October Revolution was due significantly to defects in Leninist theory which assured that the problems raised by scarcity would be managed in an undemocratic fashion. Nor are Deutscher's analogies between Stalin and Napoleon entirely convincing, since Stalin established not socialist but bureaucratic social relations in Eastern Europe after World War II. Deutscher himself points to the "rigidity...stupidity...and...almost zoological individualism" (144) promoted by bureaucratic dictatorship — qualities calling into question whether the allegedly progressive material achievements of bureaucratic rule will lead to socialism in any but the most problematic way. Contrary to Deutscher, there is much to be said for the view that bureaucratic regimes are exploitative class societies which present serious obstacles to socialist development. But however much one may disagree with Deutscher, these essays are full of penetrating insights and lucid analysis that no student of socialist history can afford to ignore.

Not to be ignored either is Deutscher's commentary on the role of the intellectual as a social critic and politically committed individual. In "The Ex-Communist's Conscience," Deutscher reviews the work of intellectuals like Arthur Koestler who, having left the Communist Party, turn against socialism the same uncritical fervor and hatred initially mobilized in the service of Stalinism. Deutscher sees such individuals as "inverted Stalinists" who persist in seeing "the world in white and black but now the colours are differently distributed." (53-4) Of course Deutscher himself was an ex-Communist, having been a militant in the Polish Party from 1927 until his expulsion for anti-Stalinism in 1932. For himself, however, inverted Stalinism would not do: like Shelley, Jefferson and Goethe, who refused to choose between the rotting corpse of Metternichian reaction and the tyrannies of Napoleonic imperialism, Deutscher refuses to choose the allegedly lesser evil between capitalism and Stalinism. Instead, he opts for a certain detachment — a position "outside any camp" from which the world may be observed and understood in hopeful expectation of social progress rather than through the fraud and hatred of Cold War politics. It goes without saying that for Deutscher detachment did not mean abandoning the struggle for socialism or a rejection of politics: Every one of these essays breathes a commitment to socialism driven forward by Marx's vision of human liberation, yet rooted in a passion for truth and honest interpretation. In a world where intellectuals so often act from sycophancy and careerism, or abdicate to the refuges of the lesser evil or just plain cynicism, Isaac Deutscher's example is salutary indeed.

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Michael L. Conniff, *Black Labor on a White Canal: Panama, 1904-1981* (Pittsburgh: University of Pittsburgh Press, 1985).

THE TITLE OF MICHAEL CONNIF'S STUDY is somewhat misleading. His work is not so much a discussion of West Indian labourers on

the Panama Canal as it is a study of the relations of the West Indian community in Panama with both the broader Panamanian society and with the United States Canal Zone administration. As such, Conniff has chosen a fascinating and potentially informative subject. As the British Minister in Panama in 1930 stated, "To the student of racial interactions and antagonisms, Panama offers an interesting spectacle just now." (80)

In the decade between 1904 and 1914, one hundred and fifty thousand West Indians came to Panama to work on the canal. Most of these immigrants stayed on in Panama to settle and raise families. In the process, Conniff argues, they created a defensive sub-culture that sought to maintain their own identity and bargain with both Panamanian and American officials. As Conniff shows, the focus of their attempts at cultural retention was the establishment of distinctly West Indian schools, businesses and associations. They used these organizations as defence against a Panamanian society that was intent on assimilation and an American canal administration that adhered to a rigid racial division in order to justify low wages and reduced benefits for the majority of the canal employees.

Conniff adopts a chronological approach to his study of the West Indians in Panama, dividing the West Indian experience there into a pre-construction period, the era of construction on the canal itself, a period of consolidation following the construction that lasted through the 1920s, the depression and second world war, a period in which the first generation of the families of West Indian immigrants came of age, and the final stage leading to the Panama Canal Treaty. He stresses various themes which continue throughout the book, while pointing out different aspects of West Indian immigrant life in changing circumstances. The dual focus is on the West Indians' relations with the Canal authority and their interaction with Panamanian society.

The Canal Authority employed a dual wage system, called the silver and gold standards, that effectively divided its employees into white and black labourers. Not only were they paid differently, but gold list employees were provided with significant perquisites denied to those on the silver list. As Conniff demonstrates, the Canal administrators stuck



determinedly to this racial division, despite increasing pressure from U.S. administrations to end it. Conniff aptly demonstrates how onsite administrators, cost cutting and strategic interests effectively undermined attempts to end the system. Conniff also discusses the attempts of black labourers on the canal to organize themselves and affiliate with American labour federations. By and large these efforts were disappointing for the canal labourers, and Conniff does an adequate job in detailing the various conflicts that soured these experiences.

These West Indian labourers and their families increasingly became a part of Panamanian society as their connections to the islands diminished over time, as employment on the canal was reduced and, ultimately, as Panamanian officials demanded a more active part in running the canal itself. Conniff describes relations between Panamanian society and the West Indian community that progressed from demands that the West Indians be repatriated to the islands, to aggressive assimilation, and finally to alliances between the West Indian community and Panamanian politicians and a workable accord. In this area, Conniff is particularly positive. He sees the integration and acceptance of West Indians in Panama as linked to Panamanians getting increasing control over their own destiny and the canal itself, as a process represented by the Carter-Torrijos Treaty. As Conniff concludes, "Perhaps it is too soon to conclude that the West Indians and their descendants in Panama have become fully integrated into and accepted by Panamanian society. Yet the process has advanced enough to make such a future probable." (179)

By and large, Conniff does a good job of outlining a complex and variegated history over a broad expanse of time. His sources are extensive and varied and he draws sensible and logical conclusions from them. As a study of the West Indian experience in Panama it is masterfully done and of immense importance to scholars seeking to understand modern Panama.

There are a few problems with Conniff's presentation however. While he is on firm ground in his discussion of the canal zone and Panamanian relations with the Canal authorities, he appears less sure of broader Panamanian history. Certainly there is little attempt to link the West Indian experience with a dis-

cussion of Panamanian history in general. The book therefore is less useful than it could be for students of Panama. While he is appropriately critical of the racist attitudes of the Canal authorities, he appears to play down unduly racism and racist attitudes among the Spanish descendants in Panama, suggesting that their colonial heritage had provided Panamanians with "an ethic of interracial harmony that discouraged segregation." (11) Certainly that assertion would raise eyebrows among scholars familiar with attitudes towards Indians and blacks elsewhere in Latin America. In addition, there is little discussion of divisions within the West Indian community in Panama itself, the tensions which accompanied these divisions, and little mention of growing wealth and class divisions among the West Indian community. This results in a kind of static approach to the West Indian attempts to solve many of their own problems, which, despite the periodization Conniff employs, robs his study (and their efforts) of the dynamism that would make it much more readable and more impressive.

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Andrew Gordon, *Evolution of Labor Relations in Japan: Heavy Industry, 1853-1955* (Cambridge: Harvard University Press 1985).

IN THE LAST THREE DECADES or so, there has been an enormous outpouring of writings in English on the subject of Japanese labour and management, generally known as the "Japanese employment system." As most of these works have been written by scholars outside of the historical discipline, they are primarily preoccupied with the present and show little interest in the past. The book under review is the first attempt of its kind by an historian to look into the century-long process by which a distinct pattern of Japanese labour relations emerged.

Andrew Gordon draws two major differences between previous works and his study. First, because of the preoccupation with the present in the former, an "implicit, linear view

of history emerged before serious historical study began." This tendency is particularly conspicuous, Gordon suggests, when the convergence thesis is involved: "If Japan is slowly converging with the West, it must have been even more different or 'traditional' decades ago and then gradually become less so. If it is not converging, the past is expected to reveal traditional roots of Japan's stubborn divergence." Gordon in fact convincingly argues that "Japanese labor relations resembled those of the West more at the outset, in the nineteenth century, than today," and that it was only as late as by the 1950s that "a social formation notably different from that in the West" took shape. (5, 413) Second, in contrast to the underlying view of the Japanese employment system in previous works as the result of "innovative managers making rational choices," which neglected the contribution of the Japanese worker, Gordon sees "the history of labor relations as a dialectic process involving the interaction of workers, managers, and bureaucrats, all taking initiatives at some point and responding to events at others." (4-6)

The author divides the hundred years from the opening of Japan to the West in 1853 into three major periods. During the first period from the 1850s to 1900 that corresponded with Japan's early industrialization efforts, management control was indirect as the managers lacked experience in direct intervention in the workplace. Gordon finds similarities between this system of indirect managerial control and practices in early industrial societies in the West. The same period, from 1900 to 1939, was characterized by management policies to impose more direct control and to develop "a coherent ideology of labor control, best characterized as paternalism." Within this period, the end of World War I marked an important turning point which saw tremendous growth of the labour movement and growing challenge by skilled workers against early paternalistic modes of direct control, even though "their demand for more systematic, favorable treatment within the existing hierarchy of the firm was probably more significant in the long run." (7) The third period, which extended from 1939 to 1955, is further broken down by the author into three sub-periods. The war years (1939-45) witnessed extensive control over labour by government bureaucracy and the cre-

ation of the Sampo (Industrial Patriotic Association) to meet the demand of wartime for economic mobilization. In the years immediately after Japan's surrender in 1945, backed up by initial U.S. Occupation policy of democratization of the nation, Japanese workers "very nearly established a labor version of the Japanese employment system: guaranteed job security, and explicitly need-based, seniority wage, and a significant labor voice in the management of factory affairs." (330) From the late 1940s to the mid-1950s, however, the managers retook control of the work place and it was only by the end of this period that the contemporary patterns of labour relations emerged.

Gordon's analysis is mainly based upon case studies of five large-scale privately managed heavy industries in the Keihin (Tokyo-Yokohama) industrial belt (Ishikawajima Shipyard, Shibaura Engineering Works, Yokohama and Uruga Docks, and Nippon Kokan, an iron and steel tube producer). The rationale behind the author's selection is that heavy industries firms "dealt with workers more committed to factory labor, and the importance of this sector derives in large measure from the appearance of Japan's first career industrial wage laborers in shipyards, arsenals, machine factories, and steel mills." Thus the "institutions that evolved out of their interactions with managers set trends for Japanese industry as a whole." (9) Although within heavy industry the *zaibatsu* (financial combine) giants in shipbuilding and metal processing (such as those of Mitsubishi, Mitsui, and Sumitomo) were not of primary concern in Gordon's study, he acknowledges that a good deal of research on labor relations in these *zaibatsu* has been done.

*The Evolution of Labor Relations in Japan* is impressive in both scope and detail. Professor Gordon uses a wide range of sources, from archival materials to government, company, and union publication, some of which were made available to scholars only recently. In conclusion, Gordon points out three important features in the history of labor relations in Japan: 1) the "emergence of a system of fairly unregulated capitalist enterprise in Japan," which "dictated constant managerial interest in productivity and efficiency" that in turn "shaped factory life and the labor response in

important ways"; 2) "Japan's particular status as a medium-late developer" that "led to some of the more distinctive managerial departures"; and 3) "the particular mix of pre-industrial practices and ideas brought into the new Meiji factory" that "accounted for certain critical concerns for the workers." (416)

Professor Gordon's book is clearly written and illustrated with many lively and telling examples and quotations. It should be of great interest to those interested in Japanese as well as comparative labour relations.

Sinh Vinh  
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Zoe Landale, ed., *Shop Talk: an anthology of poetry* (Vancouver: Pulp Press 1985).

THIS IS AN INTERESTING BOOK of poetry, worthy of a large readership. The concept has stimulated the poets involved, for the most part, and they have produced some good poems and some excellent ones. The average of high quality material is not better than in anthologies generally, but it is not worse, either.

Especially compelling about the book, of course, are the title and concept. Much of liberal mush-minded Canada spends a good deal of time saying that poetry is about beauty and there is no place for the "political" in art. Indeed, art about workers, working, the condition of class-structured life is, usually, simply ignored or dismissed as of interest only to a narrow (and narrow-minded) body of observers. Fear is always present among liberal critics that art of this kind will incite people to a real concern with change and the need for reconstructing the social order. Since critics are active spokespeople for the social order as it is, they want to see increasingly refined perception about its subtleties; these they praise ecstatically. Perception of the social order that suggests it is structurally unjust, that it guarantees disadvantage and destruction of human potential is not the kind of perception liberal critics want to have recorded.

The *Shop Talk* poets are interesting, therefore, on a number of levels. They are, for the

most part, not high school drop-outs; there's no Milton Acorn or Al Purdy here. Mostly they seem to be university trained or at least to have some higher education. They are not confrontational. They fall on the side of personal record rather than larger unit (group, class, nation) perception. That comes as something of a surprise. Indeed there are a few of the poets who might have appeared in any unspecialized poetry anthology. Most surprising, though, is an almost total absence of poems that come up sharply against the things that Social Credit has done to work and working people in British Columbia in the last decade. Social Credit is a Class act; it is anti-union; it is colonial-minded. It has affected life and work life in B.C. significantly. But a reader of *Shop Talk* does not have to think about who was running B.C. while the poems were being written.

Phil Hall's poems, moreover, have little to do with the shop — though that is not a big criticism. Rather, they seem small, unplaced, and sometimes unfinished. Except for one poem, "Steel Edges," Calvin Wharton's poems also suggest a learning poet who ventures little, either in emotion or reach of subject. That is the case, also, with David R. Conn. All are finding voice and demonstrating craft; but none seems yet achieved nor very relevant to a book with the title *Shop Talk*.

Zoe Landale makes the reader commit more to her subjects. In "Standards" she catches a male dilemma of the new liberated world, and she does so with a touch that is delicate, nowhere interfering with the experience in the poem. In "The Myth of the Self Made Man or: Casualties 1982" she probably stays at too great a distance from her subject. The collapse of fishing in B.C. is an intensely political, historical matter. It seems to be more complicated and painful than presented.

Glen Downie and Andrew Wreggit are not more politically *engagé* than the others, but their work reaches farther, dares more, and is often more accomplished. Downie's "Worker Classification: Material Handler" faces a reader with genuine pain at the same time as asking a fundamental question about human work. All of Andrew Wreggit's non-shop poems are strong and real, letting a reader into experience. "At the Wedding" is an especially powerful poem about family generations and love. And his "Fairview Floats" is a poem

about the unemployed that is both beautiful and hard.

Tom Wayman is in the book with talky poems that are fun, usually anarchist, pretty clubby, and no threat to the Establishment (anarchists almost never are). Two poems, though, have force. "Hammer" is probably about technology and exploitation: a simple, powerful poem. Wayman's surplus value poem is almost very good, but the image of surplus value as waste from the worker's efforts hurts the poem and weakens the effect of the Marxist idea that surplus value is real value stolen from the workers by the capitalist class.

Finally, two poets most prolifically represented in the book give it a tone that is both good and bad. Erin Mouré knows craft. She knows trains. But—reading and re-reading—I could not help feeling sentimentality breaking through craft. "Neighbours," about resistance to poverty, and "Craig's Talk" about injured merit on the train are both believable reporting. But they are not believable poems, as, say, the poem of longing "Who We Are" is. Erin Mouré shows she has great skill,

but she doesn't display enough of it in this book.

Kirsten Emmott is the discovery of the anthology. Perhaps not the best craftsperson yet, she manages a range and an intensity that are engaging and full of life. "Junkie/Mother" is moral and declarative about an unwanted baby, but it works. "Diagnosis: The Pain as Lover" and "Susie" deal with what is at once lunatic and natural about relationships. Generally, Emmott writes poems that are hard and clear and direct.

Considering the group's title, "The Vancouver Industrial Writers' Union," and the book's title, the writers are surprisingly "literary" often. They lack a sense of outrage or of fight. But they are working in a general area that is both interesting and informative. I hope to see a sequel to *Shop Talk*. But I also hope to see some tougher, grittier, angrier, more political, brilliantly poetical books rising out of workplace and work life. B.C. should be just the place to produce them.

Robin Mathews  
Simon Fraser University

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