Daniel Drache has moved me to do what I have always avoided: respond to those who have distanced themselves from the interpretive direction of what they almost uniformly refer to as "the new labour history". The appearance of his article, "The Formation and Fragmentation of the Canadian Working Class: 1820-1920", in Studies in Political Economy no. 15 (Fall, 1984) — supposedly a socialist review that has, in the past, offered Marxist labour histories a warm, if critical, reception — was, for me, the last straw. To be sure, his rejection of the early writing of Greg Kealey and myself is hardly original, drawing as it does on critiques posed by McNaught and Bercuson. But this is precisely the point. For, regardless of which direction the attack takes — from Drache's unprincipled charge that Kealey and Palmer have embraced...
"the conventional wisdom of the government and big business", to professional historians' parochial assaults on "Thompsonian culturalism" or, indeed, any theoretically informed historical practice — the critics share a fundamentally ahistorical approach to the past. If the anti-Marxist, ideologically presentist and unashamedly empiricist views of McNaught and Bercuson could come to rest in the pages of SPE, it was clearly time to defend not only specific texts that have contributed to a socialist analysis of Canada but history itself.¹

Ahistorical Historians: The Origins of Labour History in Canada The assault on recent writing in Canadian working class history commenced with the publication of my study of skilled workers in Hamilton in the years 1860-1914. A Culture in Conflict drew the ire of many professional historians, but the most vitriolic antagonists were associated with or embedded within the social democratic milieu. The "liberals in a hurry" found my book too fast for their tastes, and from 1979 to the present, this text — admittedly not without its many flaws — has been sniped at from the ideological lair of a complacent consensus where the lions of an established labour history lounged for a few contented years.² They apparently thought, as did a generation of critics of the Hammonds in England, that "a certain professional scowl, a suggestion of anti-sentimental rigour, [would serve] to cover any lacunae in scholarship."³ With the publication of Kealey's Toronto Workers Respond to Industrial Capitalism in 1980, the critics broadened the target of their denunciations, publishing review essays that took aim at those who hoisted "the red flag" over Canadian history, at the "celebrants of the rich and vibrant culture of the artisan". Others have been all too willing to jump on the proverbial bandwagon. The titles of their polemics — "E.P. Thompson vs Harold Logan", "Through the Looking-Glass of Culture", and "E.P. Thompson in the Snow" — tell us much about their capacity to personalize and trivialize historical debate.⁴

Just why the initial response to Kealey and Palmer took the direction it did is related directly to why and how Canadian labour history evolved as an academic realm of inquiry. This is a large question and here only certain developments can be noted.⁵ Canada lacks a long pedigree of Fabian-type inquiry associated, in England, with the Hammonds and Webbs and, in
later years and in different forms, with G.D.H. Cole. As well, it has never generated a sustained school of labour economics led by those of equivalent stature to the American social scientists Richard T. Ely and John R. Commons. As a consequence, when labour history began to emerge as a recognized field in the post-1965 years, much preliminary examination of major event and institutional-political developments was absolutely necessary. This explains some of the character of labour history in its decade of professional legitimization (1965-1975) but does not adequately address the chronological narrowing of experience to the twentieth century years, which only non-historians such as Pentland, Watt, and Forsey proved capable of overreaching. Nor does it address the peculiar insularity of much early Canadian labour history (which unfolded outside of reference to Anglo-American studies of the social history of working class life) or the debates within and among various streams of Marxism relevant to understanding class relations in the past.

To comprehend this insularity, it is necessary to reach back into the formative years of labour history in Canada. These were years when the study of workers and the examination of political role of labour as one interest group in the progressive bloc came together in a moment of social democratic consolidation. Next to no labour history was written in this country before 1945, although worker advocates (D.J. O'Donoghue) and engineers of class harmony (William Lyon Mackenzie King) did produce studies of labour's agitations and conditions. With the collapse of the socio-economic order in the Great Depression, a contingent of social democratic intellectuals, led by Frank Underhill, F.R. Scott, Eugene Forsey and Stuart Jamieson, eased the study of labour, albeit gingerly, into the scholarly discourse. One young student, Kenneth McNaught, was drawn to this group and would later become a minor successor to Underhill at the University of Toronto. Because he came to intellectual maturity in the 1940s, McNaught's conception of labour was coloured by the social democratic commitment to reform, by its intellectual elitism, and by the institutional approach of the major labour economist of the time, Harold A. Logan.

Out of this context came a particular conception of what labour history should entail. In the social democratic vision of the immediate post-war years, what deserved study were the
institutional growth of the trade unions and the politics of progressive reform. These were the really useful realms that could feed into the political project of the day. Unlike Britain and the United States, the initial impulse of routinizing labour history in the direction of political and institutional studies came relatively late and was inseparable from the aims of a moderate socialism centred in the electoral and trade union activities of the Co-operative Commonwealth Federation — projects which inevitably clashed with the practice of the Stalinist Communist Party of Canada. The Webbs, the Hammonds, Cole and Commons might all share certain convictions that, "where capitalism succeeded in the most obvious material respects, class politics [tended toward] . . . a gradualist or reformist shape." But such scholars were drawn to these conclusions by more than blatant social democratic political need, which was sufficiently entrenched in England to not require such intellectual shoring up, and which barely existed in the United States. Not so in Canada. Here, labour history's beginnings coincided with social democracy's finest hour. The conception of labour history, predating the actual arrival of the field, was thus a presentist and ideologically motivated project, although its practitioners, imprisoned by the 1950s fetish for objectivity in scholarship, could not bring themselves to admit this.

It was for this reason that McNaught was apparently so drawn to Harold Logan's work. Active in teaching and writing labour economics since the 1920s, Logan produced the first serious review of Canadian trade union development in 1948. His book had a timely ring to it for those social democrats interested in workers because, when Logan's study appeared, the crucial struggle of Canadian social democracy was being waged against the Communist Party of Canada. Logan provided welcome words for those CCFers engaged in a battle for supremacy within the trade union movement:

Politically the communists operate formally through their own federal and provincial party, the Labour Progressive Party, one of their tenets being to prevent the Trade Union movement from aligning itself with the C.C.F. All in all the effort of the communists, for all their idealistic sacrifices, seems to have brought mainly loss to progressive trade unionism in Canada when viewed through other than Marxian eyes. The imposition of foreign thought, the secret tactics of Fosterism, with its essential disloyalty to acknowledged leaders of the unions, and the extreme break with and hostility toward cherished institutions have cramped the normal free expression of the constitutional progressives and strengthened the old line conservative leaders and
At stake in this confrontation was nothing less than the politics of the labour movement. As positions became frozen during the cold war, many social democratic intellectuals (McNaught among them) hardened understandably in their opposition to the Stalinism of the Communist Party. In the process they equated Marxism with Stalinism; and, as this link was forged and reforged over time, it became impossible to draw distinctions among various Marxisms, and difficult for many within the social democratic tradition to differentiate between Marxism as an analytic approach to society and history, and Marxism as an identifiable political practice associated with the Communist Party.

This crucible of the 1940s thus moulded social democratic intellectuals with an interest in Canadian labour in particular ways, confirming a specific conception of Canadian labour history. A Marxist alternative capable of challenging such a development did not really exist at the time: limited by Stalinist premises, Communist Party intellectuals and dissidents played a very minor role in Canadian intellectual life, offering only rare and easily dismissed commentaries on labour's past. The field of working class history was thus conceptually confined by the social democratic political needs of the moment, and reduced to the study of institutions, social reform, and the emergence of a humanistic leadership of the progressive movement and labour itself, all of which post-dated the nineteenth century. McNaught's 1959 biography of J.S. Woodsworth, father of Canadian social democracy and centrally-important figure in the Winnipeg General Strike, was the exemplary study in this genre. Blatantly anti-communist, sufficiently elitist to bypass Woodsworth's nativist edge, courageously defensive in the face of the cold war's caricature of social democracy, it was a book full of contradiction and ambiguity.

In the post-1965 years, the sporadic and marginal character of writing on labour history was finally supplanted by a wave of published work. This breakthrough in labour history, however, would be rooted in unquestioning acceptance of the institutional-social democratic approach that would find expression in a deepening routinization and continuing pres-
entism within labour history. One need only read the major
texts of this period — Abella's *Nationalism, Communism, and Ca-
nadian Labour*, and Bercuson's *Confrontation at Winnipeg*, to
name just two that had their origins in doctoral theses super-
vised by McNaught — to grasp the character of the emerging
field of labour history. If the overt politics of social democracy
occasionally succumbed to a federalist liberalism (as did
McNaught, like many “lifers in a hurry” before him) the
premises of a labour history first conceived in the context of
the 1940s nonetheless slipped unobtrusively into a new epoch.
When different approaches to the Canadian working class ex-
perience began to be espoused in the post-1975 years, there
were those lining up to take aim.

The 1970s gave rise to a radical historical practice different
from the earlier social democratic project. Untainted by the
anti-Marxism of the 1940s, the labour history of the late 1970s
and early 1980s is, for the most part, a reflection of an entirely
different set of assumptions and concerns. Its focus on class
formation and its concern with the totality of working class life
indicate the extent to which the routinized histories of Mc-
Naught and others have been bypassed. As our work has ap-
peared, it has drawn the ire of those, such as McNaught, who
see it as a challenge to a labour history rooted in essential re-
formist premises. Quick to defend men like Scott and Forsey,
McNaught sees their example as confounding those “who han-
ker after the purities of class conflict and deny any role to mid-
class concern in the evolution of the left in Canada . . . cer-
tainly they cannot be taken as proving that the ‘real’ left is
confined either to orthodox Marxists or to the culture of the
inarticulate.” More blunt is this comment: “The enthusiasm
with which the neo-Marxist cultural conflict crowd studies oc-
casions of ‘worker violence’ as evidence of ‘revolutionary po-
tential’ also intimates a basic preference for extraparliamentary
methods of change nad political education”, a statement that
co-exists easily with the following:

Rumbling on the left has come from a variety of Syndicalists and Marxists,
each claiming scientific purity and each exhibiting a kind of branch-plant ide-
ology, or left-wing colonial complex, which has ensured an exotic and often
ephemeral existence for Marxism in Canada. The most audible source of
such criticism today is a group neo-Marxist academics within the Committee
on Canadian Labour History. Devotees of the British labour historians E.P.
Thompson and E.J. Hobsbawm, these young Canadian scholars profess to be
restructuring Canadian history to show that class conflict has been its main-
spring and that 'the rich and vibrant culture of the working class' has been the definitive impulse sustaining that conflict as industrial capitalism raged triumphant across the land.  

That this is an overstatement of our political importance is clear; that it is an argument which proceeds on the basis of little direct analysis of texts, arguments and evidence is disappointing; that it is red-baiting of an extremely ugly sort, coming from a figure who himself took principled left stands in the inhospitable climate of the 1950s, is tragic.

McNaught ends his caustic assessment of labour and left studies with a reformulation of the social democratic premises of the 1940s, jarring in their elitism, presentism, and economism. What needs study, he claims, is the smart union leadership that won so much for labour in the immediate post-war period. "That smart union leadership," he writes, "was not the product of any autonomous working class culture. It grew out of increasing sophistication and education. And its goal was not to defend an Archie Bunker-charivari culture, but, rather, to liberate those who had been entrapped by the economic-cultural constraints imposed by political capitalists."  

No one will quarrel with McNaught's argument that certain developments of the 1940s need study; nor will anyone challenge his insistence on the importance of the labour movement's leadership. But surely this is not the entire history, and contexts other than that of the second quarter of the twentieth century deserve treatment. Moreover, there is a need to connect contexts. What McNaught sees as the great victories of the 1940s — and the post-war settlement was, at the time, a significant accomplishment for labour — take on new meaning in the context of the 1980s. Now, with the post-war settlement eroded and dismantled, the legalism and bureaucratization of the labour movement that flowed out of the victories of the 1940s appear, more and more, as confining disabilities for Canadian workers. This reveals the essentially ahistorical foundations of McNaught's self-proclaimed realistic assessments of the roles of leadership, militancy on the economic front, and parliamentarianism.  

In posing these areas as the privileged targets of appropriate historical investigation, McNaught leaves those of us studying other experiences — the family, the work process, periods when social democratic leadership was not on the agenda — out in the cold; those interested in women or in the
nineteenth century can only puzzle over their peripheral inquiries. When McNaught is called upon to offer his view of historical process, he dismisses, in one quick and easy value judgement, an entire experience that his own ideological proclivities cannot assimilate. What is not of his experience is relegated to nonexperience; what is not understood is caricatured as an "Archie Bunker-charivari culture." In short, analysis is stopped before it has had a chance to proceed.

Bercuson, a student of McNaught, bases his arguments against Kealey and myself upon similar anti-Marxist, economistic and ahistorical foundations. McNaught can at least pose as a social democratic polemicist with a history of commitment to the CCF behind him. Bercuson has no such political facade to hide behind, and his personal history reveals a striking descent into overt alliance with rightist anti-union forces. As an arch-defender of empiricism and a hard-line detractor of Thompsonian culturalism, his critique of the Kealey and Palmer work is more simplistic and less structured around social democratic concerns than that of McNaught. Ironically, however, in the name of "empiricism" Bercuson's opposition continues in the ahistorical tracks of his mentor.

Bercuson's major critical effort has been directed toward an attack upon Kealey's and my insistence that at the close of the nineteenth century a working class opposition thrived in industrial cities and manufacturing hamlets of Ontario. For Bercuson the existence of gender and ethnic divisions are two telling repudiations of the experience of a class which Kealey and I have argued was in some senses made in the 1880s. What is crucial here is the tautological and ahistorical formulation of his argument. Because women experience different oppressions, and because ethnic divisions abound in Canadian history, Bercuson claims class lies more in our imaginations than in historical experience. His argument here is tautological and ahistorical, for the point is surely historical context: classes, as structural entities, exist in capitalism and, as social and cultural expressions, are made, unmade and remade in particular historical periods. Bercuson seems innocent of this basic understanding and, in his attempt to argue that ethnic divisions inhibited class, leaps from the divisions between Irish Protestants and Catholics in the pre-1850 period to the post-1900 clash of immigrant and English-speaking cultures.
To be sure, fragmentation is a part of the historical experience of Canadian labour, as I have argued in *Working-Class Experience*. But what Bercuson's critique glides over is the relative cultural homogeneity of the 1880s which Kealey and I have examined. It was then that ethnic divisions within the working class seemed least significant — the Irish having been assimilated, the new immigrants not yet arrived, and the bloody shirt of sectarian religious concerns having been bleached rather clean by common experiences of exploitation and oppression. Then, too, the workers' movement took up the particular oppression of women, of the unskilled, and of the non-organized more fully than it ever had before, venturing into unified struggles that linked French and English Canada for the first time. Couple this with the humanistic reform orientation of workers and their intellectual allies, and we can see the 1880s as a unique moment in the experience of solidarity — a moment Bercuson wants to obliterate with his denial that workers ever articulated bonds of unity, struggled for anything other than larger paychecks, or lived within a culture of promise and alternatives. That this moment was lost requires an explanation of subtle and complex formulation, but it cannot be denied.16

Both Bercuson and McNaught obviously find the sympathies of younger historians for Thompson's stress upon human agency and culture as a part of the process of class experience unsatisfactory. They claim a creeping culturalism has invaded Canadian working class studies. And yet, even if we accept the term culturalism, which I would not, this mode of analysis has been with us for some time. McNaught offered us his most innovative and theoretical work in an assessment of progressivism and socialism in North America that was unashamedly culturalist (in the Hartzian sense) even as it railed against Hartz.17 Gad Horowitz embraced a culturalist approach enthusiastically as early as 1968, proclaiming: "Institutional analysis can show why permanent, significant third parties arise, but it cannot explain why one of these parties should be socialist. The cultural analysis is necessary to explain the relative strength of a socialist ideology in Canada."18 But McNaught and others posed no "culturalist" critique of this early text. The point, then, is that these critics are attacking more than culturalism: they are taking aim at attempts to root culture in a
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class experience in which the material moorings of class, and the many conflicts associated with class society, are important influences in everyday life.

Other critics, most notably Desmond Morton and Laurel Sefton MacDowell, do not stray far from the McNaught-Bercuson path. Both, for instance, are vocal advocates of institutional and political study of labour's past, and both are skeptical critics of studies that reach out to attempt more in the way of empirical research and analysis. Both shy away from any theoretical exercise that smacks of Marxism. MacDowell, the daughter of one of McNaught's smart labour leaders of the 1940s, Larry Sefton, is perhaps the most unwittingly revealing in her views on recent studies of nineteenth century workers. She applauds institutional studies because of their "concreteness", an interesting display of the way in which conceptualization of historical process can be forged on a lowest-common-denominator basis: unions exist; we can touch their records; therefore, let us study them instead of intangibles like culture or consciousness. She also fears that the Marxist framework of some of the new writing threatens to create "yet another kind of 'official history'". Curiously, however, her own critique distances itself from bodies like the Knights of Labor, and from historiographic tendencies to "search for a revolutionary tradition", while she nods approvingly in the direction of those institutional winners, the AFL/TLC and CIO, warning the romantics that "without their union . . . working people have nothing." I would not want to quarrel with this kind of either/or dichotomy. The point that I want to stress is the emphasis MacDowell places on union survival as the ultimate aspect of workers' lives, "even if it involved some compromise of principled objectives." To support her argument she quotes Lynn Williams' "Larry Sefton Memorial Lecture". On wonders just who is advocating and producing official histories of Canadian labour.19

There are, of course, others who would lay emphasis on a variant of labour's official history. They question our attempt to estimate the numbers drawn to the Knights of Labor, and challenge our analysis of the significance of the percentage of the workforce organized throughout Ontario in the 1880s. Like MacDowell (whose review of Dreaming of What Might Be suggested strongly that in studying the losers of the 1880s —
the Knights of Labor — Kealey and Palmer had focused on the wrong sector of organized labour), these opponents argue that we have studied workers best left forgotten. While Kealey and I opened the door to these critics by our questionable handling of data that are, by their nature, incomplete and problematic, any mistakes that we did make never seriously undermined our demonstration of the quantitative breakthroughs the Knights of Labor accomplished in the 1880s; and those mistakes have now been rectified. Our critics, however, continue to make serious errors, and often present our cautious and honest findings with the deliberate intention of distorting the actual situation in the 1880s. Moreover, given the nature of the statistical evidence, we presented our findings as but one part of a larger argument; we always stressed issues that reached well beyond numbers. Those who want to assail us will also have to direct their arguments to this qualitative realm of experience. And it would undoubtedly raise the level of debate if some of them actually took the trouble to work through nineteenth century sources, enabling them to avoid elementary errors and to root their disagreements in an understanding of a period remarkably different than the twentieth century years they have studied.20

What is uncontestable, then, is the ideological character of the assault on recent writing in Canadian working class history. As an overt attempt to reassert the primacy of social democracy, McNaught’s remarks are the most blatantly anti-Marxist and the most thoroughly inattentive to historical context; but other commentary from professional labour historians — Ber cuson, Morton, MacDowell — is also strikingly ahistorical. Any attempt to study class relations as a conjuncture of economic, social, political and cultural forces will be met by a strong attempt to reroute historical inquiry back into the mainstream, regardless of how much this violates historical context.

This brings us back to Drache, who, of course, is not in the main stream. He poses a sharp critique of some of the writing in Canadian working class history that reaches conclusions quite the opposite of those espoused by McNaught and Company. But he shares with these social democratic detractors a capacity to ignore the actualities of historical context. Radical nationalist and right-leaning social democrat meet in their inability to contextualize workers’ struggles and experiences; in
their search for the ideal working class, be it revolutionary and regionalist or pragmatic and respectable; and, in their moral capacity to condemn Kealey and Palmer for studying the wrong workers.

History as Fantasy An historian cannot read Drache and remain tranquil. The very ground upon which all historians walk — the past — and the very procedures which guide us in our travels — attention to context and sources — are so thoroughly violated that Drache’s “The Formation and Fragmentation of the Canadian Working Class: 1820-1920” can only be read as an exercise in uninformed fantasizing about class formation as an historical phenomenon. Five related aspects of Drache’s argument deserve comment here.

First, the essay is literally riddled with historical errors, not of interpretation but of fact. The first page of the article contains three glaring mistakes: the Rideau and Lachine canal systems were built, not in the 1830s, but largely in the 1820s, with construction at Lachine commencing as early as 1817; central Canada gave rise to small factories, not in the late 1890s, but at least forty or more years earlier; the Trades and Labor Congress was founded in 1883, rather than a decade and a half later as is suggested here (and its leadership could thus not have been influential in the drift away from militancy in 1872, as is stated later in the argument).

Second, Drache is equally unreliable in terms of situating particular concepts and historical developments. He asks, rhetorically, if the working class had “a free-standing class culture”, and if an organizational unity capable of “bridging occupational and regional differences and enabling the working class to confront the state and capital” had been forged by the 1920s. His article is meant to reply in the negative, but it is a wasted effort. For no one, to my knowledge, has ever argued for the existence of such a “free-standing” working class culture, whatever that may be. Nor has anyone suggested that by the 1920s — a period of decisive setbacks for the international workers’ movement Canadian labour was unified and capable of battling the state and capital. Drache’s capacity to literally force history in directions that his preconceptions demand is nowhere more apparent than in his description of the conventional trade union movement, which, late nineteenth and early
twentieth century workers would have been astounded to learn, was “organized with the help of the state”. For Drache, the entire Trades and Labor Congress was “a labour aristocracy” constituting “an elite in a very special colonial context: white, Anglo-Saxon, politically conservative, fervently patriotic of king and country, xenophobic, sectarian, anti-Catholic and anti-French.” One is not required to be an advocate of the incompletely bureaucratized layer of TLC leadership to dismiss this blanket condemnation as a gross caricature of the late nineteenth century situation. In the period of the 1890s that Drache refers to, D.J. O'Donoghue, founding father of the TLC and a staunch Catholic, was still active in the Congress; Laurier's and King's liberalism — about to give way to the rising cause of the Independent Labor Party — were the dominant political tendencies within Labour's organized mainstream; and the conscription debates that rocked TLC conventions in 1915-1917, while more than a decade and a half away, were testimony enough that “king and country” were, at best, problematic identifications for many of the most staid unionists.

Third, Drache is facilitated in his tendency to recreate the past in an image acceptable to him by his promiscuous “handling” of sources and theory. Texts and arguments by other authors, rooted in particular notions of periodization and meant to apply to specific contexts, are drawn on indiscriminately. Labour market segmentation theorists, such as Richard Edwards et al. for instance, are marshalled to show how the labour market has fragmented the Canadian working class. The problem is that Edwards and others see such segmentation rooted in the post-1920 years, most directly in the post-World War II period. They argue that during American capitalism's first century, it inherited and recruited a highly heterogeneous labour force but reshaped its wage labourers into an increasingly homogeneous class. Drache, focusing his argument on the 1820-1920 years, skips this problem of context and periodization. Scholars from any discipline or tradition should not rest easy with Drache's cavalier citations either. When I went to Richard Edwards' Contested Terrain to check two of Drache's direct quotes, I found they did not exist on the pages he noted. Were I Michael Bliss — remember his review of Naylor's History of Canadian Business? — I would have done a count of such
errors and moved back into my booth at Toronto's Rare Book Room.23

These kinds of deficiencies hint at a fourth area of difficulty in Drache's presentation: his capacity to contradict himself and the avoid confronting the implications of such inconsistencies. As noted earlier, Drache is able to situate the emergence of the TLC in different periods when it suits his argument. More seriously contradictory are his totally opposing views on the role of Irish Catholic immigrants in the creation of a reserve army of labour. On one page, he argues that the “Irish or Catholic southern peasant” came to Canada, worked for wages, saved a stake and bought land. Three paragraphs later, we read that Catholic Irishmen preferred wage employment and clung to the few “benefits” that the labour market, as opposed to individual proprietorship, had to offer. Since Drache makes no attempt to differentiate these dichotomous trajectories according to distinct periods, we are left wondering what is happening. To complicate matters further, sandwiched between these divergent assessments, we read that masses of workers took their leave from the Canadian labour market between 1840-1880, an historical process that no doubt occurred, but one that fits poorly with: (a) labourers settling on the land; and, (b) Irish Catholics clinging to wage labour and stocking the labour pool.24

With an approach and argument littered with these kinds of deficiencies, it is not surprising that Drache's conclusions are a fifth and final area of contention. In his explanation of why what he calls the radical resource proletariat did not win over the Canadian working class movement, Drache is moulding the history of labour along lines dictated, not by what was possible or, indeed, politically advisable in the real world of the past, but by his own nationalist project.

He suggests, first, that socialist and western militants invited defeat because they neglected to realize how essential it was to destroy the craft unions organizationally. Drache contends that in fighting business unionism on the ideological plane alone, the radicals motivated by the idea of the One Big Union (OBU) left themselves open to constant harassment and challenge by the established craft unions. But Drache is at his reductionist worst in adopting such a posture. He collapses craft unionism into American business unionism, neglecting to deal with the
militancy of craft workers and the extent to which the idea of the One Big Union was sweeping through AFL/TLC ranks. To be sure, a layer of AFL/TLC leaders did their best to stifle industrial unionism and class struggle in the first two decades of the twentieth century, especially as the challenge of combativity climbed in the years 1918-1920. But the advocates of the One Big Union idea always knew that their enemies within the working class were a particular stratum of trade union bureaucrats, and not the institution of craft unionism and its mass constituency of skilled workers affiliated to the TLC. They knew, moreover, that in the context of the post-war crisis of economic reconstruction and a continental Red Scare, it would have been folly to wipe out the craft union — the longest-standing, if problematic, line of defence that workers' possessed. Many radicals had come out of the craft unions; many, such as W.A. Pritchard, drew their illustrations of the transformation of class relations, in the wake of capitalist restructuring, from the experience of craft rather than resource workers. One Big Unionism struck some of its most decisive blows within craft union and AFL/TLC circles, as the general strikes in Seattle, Winnipeg, Toronto and Amherst, Nova Scotia, demonstrated. Equally impressive were the major battles that had erupted on the New Orleans docks (1907), on the Illinois-Central and Harriman Railroad Lines (1911-1915), and among garment, steel, packing-house, and maritime workers (1919-1921). The radical project, then, was not to exorcize craft unionism but to win the craft workers, the resource proletariat and the unorganized to One Big Union and, in the process, displace the labour “fakirs” and collaborationists at the head of the AFL/TLC. Had socialists and western militants launched an indiscriminate assault on the craft unions as unions, the OBU challenge would only have been weakened, the workers' movement rendered even less effective and more divided, and the state and capital left more securely in the saddle.

Drache endorses such a suicidal path for the workers' movement because of his nationalist dislike for the American Federation of Labor's domination of Canadian unions. Indeed, he claims that a second failing of western radicals was their inability to grasp the power of “nationalism and the ways it could be used as a political weapon in a colonial context.” What this fails to appreciate was that the labour revolt of 1919 was a pro-
foundly internationalist development. Western radicals, regardless of what Drache would prefer them to be, were men and women more concerned with the British shop stewards movement and the Bolshevik revolution than with the complexities of the Canadian character. Immigrant workers, at the very centre of upheaval in the west and also involved in the east, opened their meetings with "The International". If there was a nationalism they would have embraced, it was the nationalism of radical independence movements endorsed by many Polish, Croatian, Slovak, Ukrainian and Finnish workers — the nationalism of armed revolt in the Old World. Drache would have the leaders of the OBU conjure up some mystical Canadian nationalism that could effectively mobilize such workers against the reactionary patriotism pushed by the state. The OBU leaders knew better: they sent their organizers to Seattle, Chicago, and Lawrence where they scored some significant successes. It was not for nothing that James Riordan, a delegate from British Columbia, had travelled to the "Continental Congress of the Working Class" in Chicago, in 1905, to argue that the name of a new and revolutionary workers' movement be not the Industrial Union of America but the Industrial Workers of the World (IWW).25

Finally, Drache concludes that a part of the radicals' failure can be attributed to their own ideological shortcomings: "the leaders of the western-based syndicalists too often became prisoners of an abstract, doctrinaire, sectarian Marxism that spoke in general terms of the class conflict between capital and labour, or categorically opposed all forms of private property, or, at the extreme, simply denounced trade unions as bourgeois institutions which, along with capital, had to be destroyed." "In this caricature of reality", argues Drache, "doctrine mattered more than a sense of realism." The tables are easily turned: Drache, the doctrinaire nationalist, is himself caricaturing the western radicals. For, as Friesen, Peterson and Kealey have recently argued, especially against Drache's major source, Martin Robin's Radical Politics and Canadian Labour, syndicalism and Socialist Party of Canada (SPC) Marxism (the variant endorsed by all of the leading figures of OBU) are not collapsible into one uncongealed ideological mass. Any sensitive reading of the labour revolt of 1919, moreover, would force one to recognize that for all the "impossiblist" rhetoric of the
early SPC (1903-05), western radicals were well beyond mouthing generalities concerning property and class struggle in 1919. Nor were they given to wasting two words in a denunciation of "trade unions as bourgeois institutions". Indeed it is Drache himself, as we have just seen, who argues that western radicals should have launched an all-out assault on trade unions that he describes as nothing less than bourgeois institutions. Drache wants to have it both ways: the western radicals failed because they did not attack "reactionary" craft unionism forcefully enough; and, the western radicals failed because their attack on "bourgeois" trade unions was too sectarian.26

Drache presents such a contradictory caricature of western radical leaders because he wants to close with a critique of their limited sense of politics. The western radicals' final failing, he claims, was in rejecting a political alliance with an admittedly reformist and "petty bourgeois" farmers' movement. What Drache fails to address is that when their alliance was consolidated in the 1920s, it spelled defeat for radical labour. When its political program was articulated in the 1933 Regina Manifesto, it spelled a triumph of reformism over revolution that the advocates of the OBU would have turned away from in disgust during the moments of upheaval around 1919.27

Thus, for Drache, "the key issue emerging from all those events is the failure of the resource proletariat to come to terms with the national question." He suggests that the Quebec working class was different (which it is) and that it was "politicized by Quebec's continuing colonial status in Confederation" (which it has been), but he neglects to mention that that politicization took a long time and did not manifest itself concretely until the late 1940s, and possibly not until the 1960s. The national question, moreover, is obviously unavoidable in Quebec, but it is more problematic in English-speaking Canada. There is no doubt, with the post-1970 shift within the Canadian Labour Congress toward the dominance of public-sector unionism, the movement to sever ties with American bodies, and the current discussions of trade union federations and amalgamations, that national as opposed to binational unionism is on the rise. But this process, emerging out of historical developments, cannot be read back into the past. It is true that regional fragmentation and the size of the country inhibited co-ordination of events in Winnipeg and Amherst in 1919,
just as it separated the struggles of miners in District 18 and District 26. But this, one suspects, is not what Drache means by "the national question". Since he never points to just what western radicals should or could have said and done to realize the potential of nationalism with the working class, it is difficult to engage with Drache concretely on this matter. But everything we know of the labour revolt of the opening years of the twentieth century suggests strongly that, for the overwhelming majority of Canadian workers, the words of a BC miner rang true. "There is no 49th parallel of latitude in Unionism," he said. "The Canadian and American workingmen have joined hands across the Boundary line for common cause against a common enemy."\(^{28}\)

The defeat of 1919, like the upsurge of 1919, thus had little to do with nationalism. Far more important in the rise of radicalism were a complex set of structural and international realities: new modes of production and managerial innovations that diluted skills, degraded work, and homogenized the labour force; the emergence of new organizational and ideological developments in Australia, Britain, Russia and elsewhere; the material context of wartime in which double-digit inflation, full employment and a desperate need for production conditioned specific relations among labour, capital and the state, and pushed labour in the direction of a combativity that (for the first time in living memory, for many workers) brought tangible results and victories; and, finally, in the Canadian west, the peculiarities of a social formation in which capitalist development and class formation had been "telescoped" into two decades rather than, as in the case of central Canada, spread over more than half a century — a process which meant that the long, sociological gestation of a distinct layer of reformist and bureaucratized labour leadership was but in its infancy rather than (as it was in the east) fully developed. Such factors — not nationalism — spread the powerful idea of One Big Union among masses of workers.

Likewise, structural and strategic realities were far more formidable in labour's defeat than Drache is willing to concede. To elevate the radicals' failure to exploit "the national question" above the chaotic socio-economic context of 1919-1920 as the cause of the failure of radicalism is voluntarism of the worst sort. Dominated by spiralling unemployment (which may
have reached as high as 16-17 percent by the end of 1920), the problems of post-war economic conversion to peacetime production, and an hysterical anti-Bolshevism that allowed the state and employers to plant their heels in a vicious anti-labour stand, 1919 created a context in which labour's revolt was bound to run into difficulty. No doubt, the radicals of 1919 committed a number of errors. They were surprisingly inattentive to the realities of state power and the machinations of the emerging "secret" state. They trailed events rather than directing them. And the One Big Union, for all the euphoria, was always more a powerful idea than a stable institution capable of defending the working class an advancing the possibilities of a new proletarian order. These — not failure on the national question — were the shortcomings of the practice of western radicals.

Beyond Conventional Formulations The points discussed constitute, for many conventional historians, sufficient grounds for rejecting Drache's argument. Yet, in spite of himself, he does have something to say. He raises questions of significance, although he poses them neither as acutely nor as innovatively as he imagines. What Drache does raise are questions concerning the colonial impact on class formation, and the role of labour market segmentation in the making of a fragmented working class. These are areas, admittedly, where all too little work has been done. They deserve further and deeper commentary.

But, in directing such questions solely towards the Kealey-Palmer studies of Toronto and Hamilton, Drache ignores our later work which addresses much of his criticism implicitly. Drache's condemnation is expressed in the following rhetorical question: "Must it be said for the hundredth time that the Toronto-Hamilton corridor does not represent the totality of Canadian capitalist development in this and succeeding periods?" It is necessary to reply that: (a) no one ever said they did; and, (b) when this charge was levelled, I had already published Working-Class Experience, (which Drache cites once and then proceeds to ignore) a book that reaches beyond Toronto and Hamilton; while Kealey had published his important examination of the national nature of labour's revolt in 1919, the first serious attempt to examine the Canada-wide contours of this
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upheaval. More importantly, the problem with Drache's own
treatment of these questions is that it is either entirely descript-
ive and unsubstantiated (as in the case of colonialism) or
ahistorical and abstracted from its material moorings (as in the
case of labour market segmentation).

In terms of colonialism, Drache uses the word as a pejora-
tive adjective. Thus "the colonial state" divided English Cana-
dian and Quebec working classes "along racial and linguistic
lines" (one would have thought little state effort would be re-
quired to accomplish this tautological end). Colonial policy ex-
cluded "the surplus Quebec peasantry" from entry into the
wage labour market, preferring instead to import "English-
speaking immigrants" (the much-loved famine Irish, no
doubt). Craft unionism's power rested, in part, on "the special
corporate relation it had formed historically with the colonial
state," a proposition that, eight pages later, is extended to read:
"In central Canada, skilled craft workers organized a colonial
trade union movement with the aid of the state." In all of these
examples we see degeneration into negative labelling, in which
what is wrong is uncritically chastised as "colonial". But just
what it is that is colonial is not specified or contextualized.

Nor can Drache help us much with labour market segmen-
tation. To Drache, labour market segmentation is — like all
phenomena in the material world of Canada — a product of
the dominance of the staple and the resulting economic rela-
tions and political policies. It encompasses regional economic
inequalities, wage differentials and ethnic/linguistic factors. Its
negative features are felt only on the periphery, where labour
is — apparently unlike the working class of central Canada —
"exposed" to wage cuts, layoffs, and other "personal" attacks
on its standard of living. In western Canada, because of the pe-
culiar and oppressive character of the resource proletariat's
segmented labour market, workers were driven early on to
search for "class unity" and to struggle against "the colonial
structure of the trade union movement to allow for a working
class organization on an all-Canadian basis." In Quebec, the la-
bour market has long been governed by Anglophone privileges
in terms of "wages, job opportunities, and language of work." On
the east coast, apparently — where Drache is surprised to
find an historically "sophisticated labour force" — the collapse
of the regional economy led to emigration and the decline of
the working class by the 1920s. Labour market segmentation is thus, for Drache, a mixed bag of imposed forms of subordination. But it is not tied to any sense of historical development. Nor is it concretely rooted in capital accumulation, state policy and the uneven character of economic growth and class formation (see Appendix 1).

This is so because, for all of his abstract tossing around of quotes from Bruno, Edwards and Boyer, Drache is trapped in the Innisian quandary. He insists on basing his views on the rise of wage labour in Canada upon a conceptual foundation that posits "the absence of industrial employment", maintaining staunchly that, in a staples economy, "the gradual development of the working class reflect[s] the productive forces of each successive staple." Something can be made of this for the period before 1850 — the epoch about which Innis wrote most extensively. A series of staples — fish, fur, squared timber, and wheat in Upper Canada/Canada West — dominated economic life and influenced the character of labour-capital relations, the consolidation of the labour market, and the peculiarities of state formation and policy. Innis charted (often all too schematically) such developments in this pre-1850 period. With the obvious (and, for Innis, contemporary) penetration of the Canadian economy by massive doses of American capital in the years 1900-1930, the founding father of political economy was able to posit a new context of dependency, and gestured toward the importance of new energy staples in the early twentieth century. But there was a blindspot in Innis's periodization — an important, "moment" when no staple did, indeed, dominate and indigenous industrial capitalism consolidated. Innis, to my knowledge, never tried to will this moment away. He simply skirted it, for it was not central to his task.29

Drache, however, tries to bury these years. He turns a blind eye to the significant industrial transformation of urban Quebec, the impressive economic diversity of small-town Ontario, and capital's penetration into parts of the Maritimes. Drache also marginalizes the importance of industrial capital in Toronto and Hamilton by and throughout the 1880s. As well, he understates its potency in the Canadian west on the eve of the Great War. Wherever industrial capital appears, it is characterized as a "zone" wherein the working class are privileged and, consequently, "enfeebled". Kealey and I, in studying this
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sorry excuse for a working class, have looked at a fake proletariat: we have studied the wrong workers. Drache's perspective, however, substitutes moralism for analysis.

Let me suggest other ways in which we might discuss the colonial context of class formation in Canada and the importance of labour market segmentation. We must start with a coherent sense of periodization. In the years of unquestioned staples dominance, overt colonial subordination, and limited indigenous industrial activity prior to 1850, one necessary task in exploring class formation will be to link the needs and orientations of the British empire and the subservience of colonial (primarily merchant) capital with the set of determined socioeconomic and political realities and the limitations and confines of class formation. Out of this would emerge an appreciation of uneven development within the colonial context, as well as a firmer understanding of the ways in which structures of plebeian life circumscribed the formation of the working class. Simply substituting a denunciation of practice and policy as "colonial", à la Drache, will not do here; although he is right to point out the need to probe into the ways in which colonial forms of rule - manifested in state and capital - set the peculiar framework in which a working class was first (however incompletely) formed. Such an analysis will be enriched, no doubt, by Innisian insights. Indeed it will be, in part, complementary to the staples interpretation, in as much as Innis was at root a materialist. But, at one crucial point, it will move past Innis and look to Marx. Once that step is taken, Innis will become at best a limited guide to the kinds of questions we must ask.

We cannot just leave off in 1850 and negate the industrial-capitalist transformation of the late nineteenth century - a transformation that came from somewhere. In this context, the years before 1850 - which Drache (if not Innis) sees only in terms of the staple and its imposed forms of subordination - were more than just an economic succession of key exports. They were years of primitive accumulation in which the social relations of labouring life, the nature of political rule, cultural developments, and investment and exchange patterns were all (often unwittingly) setting the stage for a new industrial capitalist order to break the fetters that a staples economy had erected everywhere. As each succeeding staple inevitably gave
rise to more diversified demographic and economic consequences, the home market was shaped. Gradually yet decisively, the old economic and political limitations were shattered. The last half of the nineteenth century saw the rise of the factory system and the consolidation of entrepreneurial capitalism; the break from colonial subordination and institutionalization of home rule; and, the charting of a national policy for development that would structure particular regions and classes along uneven and unequal paths of development and underdevelopment.

We know something of the character of class relations in this period because of our studies of Hamilton and Toronto — though we by no means know all that we need to, especially vis-à-vis class formation in Quebec, the Maritimes, and in the pockets of economic transformation in the nineteenth century west. Drache is right to insist that we need more careful studies of particular labour markets. But he is wrong, dead wrong, to imply that only those so-called “exposed” workers on the periphery had a right to speak for labour; and that only they suffered through capital’s project. As Kealey and I have shown, the 1880s witnessed an impressive upheaval of Ontario’s workers, organized in the Knights of Labor — a body composed of craft workers and factory labourers, men and women, Catholic and Protestant, Irish and English (and, in Quebec, of French and English). They tried, for the first time, to speak for all of Canadian labour. This was not, whatever Drache may want to believe, “a politically enfeebled working class centred in South-central Canada ... that organized, with the help of the state, a narrowly based trade union movement dominated by skilled craftsmen ... that benefitted from the economic strategy of export-led growth.” By ignoring capitalist development in late nineteenth century Canada, Drache can conveniently forget this moment of the 1880s and mistakenly claim that the 1872 struggle for the nine-hour day was “the most important fight” labour engaged in throughout the nineteenth century. He can thus shift the burden of debate on to a dichotomy between eastern-based Gompersism and western resource worker radicalism. As should be apparent, this reductionism will not work. Its error becomes even more fundamental if we are concerned with the national process of class formation.
To make sense of the different responses of western and eastern workers to conditions of work in the opening decades of the twentieth century, it is vital to situate the labour revolt of 1919 historically. It is only when this particular class uprising is viewed ahistorically, as isolated onto itself, that an image of western labour exceptionalism can be sustained (or a view of the resource proletariat as "more powerful, more representative, more politically advanced and organizationally sophisticated" put forward with any seriousness). Instead of idealizing and isolating 1919, let us consider it alongside of the labour upheavals in the 1880s and the 1940s. When that is done, it is obvious that the first significant challenge to capital and the state was mounted, not by resource workers, but by an alliance of central Canadian workers built upon the unity of skilled and unskilled— a broad front that attempted to organize French and English, miner and manufacturing worker, man and woman, from British Columbia to Cape Breton.

It was in the centre, not on the periphery, that labour first tasted and responded dramatically to the fruits of wage labour. That response proved inadequate and was swept aside by the staunch opposition of employers and state— opposition that hardened as concentrated economic power gave way to monopolies and innovative managerial assaults on working class power, at the point of production. Thirty years later, however, with the rapid penetration of the west by industrial capital and central Canadian capital's accommodation to the branch-plant economy, the uneven character of class formation was structured into particular labour markets. This uneven character to class formation revealed itself in quite different ideological/organizational trajectories among eastern and western workers. To be sure, central Canadian labour was not entirely a captive of what Drache dismisses as "a colonial trade union movement". But in the east, after the defeats of the 1880s and early 1890s, the idea of the One Big Union lived on at the margins of the class in a militant minority of craft workers, socialists and, perhaps (in the form of an intuitive understanding) among many rank-and-file unionists. Nevertheless, it was the residue of craft unionism that was unquestioningly dominant— a residue that now harboured a conscious and powerful layer of bureaucratized and thoroughly reformist labour leaders. In the west, that layer had not yet congealed. The accelerated capitalist development of the early twentieth century swept class
formation along with such rapidity that it did not yet have a chance to mature within the womb of capitalist hegemony.

In this context, so-called western radicalism (which always had it counterparts in central Canada and on the east coast) was moved by the same hostility to capital's enroachments as it displayed during the upheaval of the 1880s; and it advocated the same class unity and organizational challenge to craft exclusionism. But it appears necessarily as a more advanced and coherent form of this earlier agitation, emerging as it does in the epoch of monopoly, rather than entrepreneurial capitalism, against an overtly interventionist state with a program of class conciliation and a rudimentary sense of industrial relations policy — rather than developing, as did the 1880s confrontations, in an age of Macdonald-type ad hoc Tory responses to "the labour problem". Moreover, the 1919 revolt was in part guided by a new discourse on labour's possibilities that emerged out of the Bolshevik experience and the organizational breakthroughs associated with Soviets, workers' councils, and in response to the threat of dilution and degradation of work promised by Taylorism, Fordism and mass production. There are two points here. First, the class battles of the 1880s and 1919 are connected, bridging the regional separation of west and east. Second, what is new about 1919 has relatively little to do with a staples economy and an insistence that resource workers are the repository of class virtue. The similarities and differences of the labour upheavals in the 1880s and 1919 are explainable, not in Drache's terms, but in terms of capital's uneven development, the timing of class formation, and the consequences for workers' consciousness.

This fundamental premise is born out if we focus on the next period of class upheaval — the post-war organizational drives in mass-production that achieved what Drache at one point defines as central features of the class struggle: the right to organize; national unifying structures; and, the defence of workers from attacks by capital and the state. In the years 1946-47, the resource proletariat and the industrial workers of central Canada rose up as one, organizing logging, mining, automobile, rubber, electrical, steel, textile, meatpacking, shipping and transportation workers from coast to coast. There is no western exceptionalism here, no sign of a struggle between radical resource workers and an enfeebled industrial wing of
the workers' movement imprisoned by its colonial conservatism. This was so because capitalism had caught up to itself. To argue in this way is not to deny regional inequities, segmented labour markets, and the extent to which a host of forces keep workers divided and weakened. It is simply to suggest that the formation and fragmentation of the Canadian working class has been an historical process rooted in east and west, conditioned not just by a succession of staples but by capitalist development, however uneven its character.

Drache is right to insist that labour historians address the peculiarities of that development's colonial origins, continued dependencies and segmented labour markets. He could have made that suggestion in two pages. Instead he gives us almost fifty pages of garbled argument, for anyone who can uncritically accept the views of Bercuson and McNaught is destined to mire their views in ahistorical formulations and ideological posturing.

Drache's own ahistorical reading of labour's past is thus reinforced by that of the historians. He is enamoured of critiques that are — in spite of his endorsement of them as "telling" and "valid" — deeply problematic. He embraces McNaught's view that "Canada's neo-Marxist working class historians have come up with 'no new thesis or new explanation of our labour structures, of government policy, nor of the nature and role of the political left'." But the real questions remain. What was the original thesis and/or explanation? Who before had presented a compelling framework that offers us answers to such questions? McNaught himself certainly provided no adequate answers to the question he asks. Innis provided no such convincing depiction, Drache is even less successful precisely because he claims to have delivered just such an interpretive tour-de-force. What he provides is a chaotic whirlwind composed of equal doses of unsubstantiated assertion and historical fiction tied together by reductionism, voluntarism, and moralism.

In our collective work, whatever its flaws, Kealey and I have contributed a modest but enduring foundation upon which the study of labour can proceed. We have not been alone in this endeavour, and others, including many non-Marxists, have been anything but bit players. To strengthen that foundation, it is necessary to close with some final words of self-criticism; to do the job that critics as divergent as McNaught and Drache have been unable to accomplish.
The Errors of Our Ways In the litany of sins committed by those of us studying workers in ways different from those advocated by past practitioners of a routinized labour history, none is more regularly remarked upon than our theoretical excesses. McNaught deplores the “turgid neo-marxist theoretical framework” of A Culture in Conflict, and concludes in his comment about writing on labour and the left that: “A survey of the recent writing leaves me with the strong apprehension that the more concerned an author is with description and narrative, the more implicit his analysis, and the less obtrusive his ideological imperatives, the more effective is the result.” Bercuson, never as elegant as McNaught, is more blunt. “Surely the rescue work,” he concludes in innocence, “should begin with old fashioned empiricism.” This message is everywhere. One thoroughly mainstream reviewer of Kealey’s Toronto Workers Respond to Industrial Capitalism found this book less “irritating” than my study of Hamilton because “Kealey displays fewer pretensions about the theoretical significance of his work.” When Laurel Sefton MacDowell reviewed Working-Class Experience, a book she introduces as “a good survey”, she felt compelled to spend fully half of her comment on the study’s “less satisfying aspects, including Palmer’s theoretical framework set out in the introductory chapter.”

There are two points that need to be made here. The first is how crudely and inadequately the critics understand the theoretical project that Kealey, myself and others are engaged in. Bercuson, for instance, is almost laughable in his reductionism, claiming that I believe “the only way to analyze the actions and attitudes of workers is to study their culture”, a view that finds an echo in more sophisticated commentaries. MacDowell’s review of Working-Class Experience so thoroughly misunderstands a brief five-page theoretical and historiographical statement that she spends her review arguing that we must study trade unions rather than workers’ culture, positing some dichotomy separating these supposedly divergent realms. But, as I indicated in the introduction and reiterated later, “a culture does not exist “above” institutions and politics, and in advanced capitalist societies where the institutional/political forces of repression are highly developed, the union and labour political action are of vital importance, and are themselves part of cultural experience.” This is not to say, however, that our own theoretical
exercise has been adequate, and this is the second point I want to stress. Much of the recent writing on Canadian labour history suffers not from too much theory, but from too little.32

Our theoretical shortcomings are perhaps most obvious in terms of our failure to challenge and deflect the way in which debate has been structured along the unfruitful lines of “institutionalists” vs “culturalists”. In England, of course, the lines of critique are drawn in a seemingly parallel fashion, with a “structuralist” promise — yet to be delivered — counterposed to a culturalist historical practice. But this is an argument within Marxism, among Marxists. Here in Canada the questions have been posed more bluntly, more ideologically, and in a manner more hostile to theory. Ask McNaught and company what is wrong with the so-called new labour history and they will reply, “They study culture and impose on the working class their own theoretical notions of what class should be. “What is needed,” they continue, Gradgrind-like, “is simple study of the facts.” In their ordering of “the facts” emerges the history they judge significant — a history made up of episodic but unintegrated, isolated struggles; a history governed by the progressive Whig march of legal victories and the emergence of stable structures and social democratic leaders; a history useful to Canadians in need of the kind of democratic socialism the NDP can supposedly provide.

Our project has not been this slavish service to social democracy. But neither has our project been a “culturalist” one. This is a term of derision and condescension. Moreover, those who would apply it to our work march themselves into a difficult corner. Invariably, critics such as Bercuson and MacDowell, for instance, deplore our culturalism and then turn around and attack us because large parts of work deal with the very subjects we supposedly deride: trade unions and labour politics. It apparently comes as something of a surprise to see us mucking about in the mundane matters of iron moulders’ and printers’ organizations, or the intricacies of partyism in Lambton County in 1886.

There is a way of answering such contradictory criticism. We are not culturalists and never were. But we have been inadequate to the task of explaining the significance of the study of certain non-institutional realms that we have located in a space which, for want of a better term, we have called cultural. And
in failing to do this we have allowed, unintentionally, the empiricist and thoroughly ideological critique of our work to proceed unchecked.

If our project is not a culturalist one, then what is it? It is a project premised on the essential tenets of historical materialism; a project that conceives class not in this or that particular, but as a totality resting on the essential economic relations of production yet emerging and making itself, and being made, in an integrated series of realms that, by the very nature of bourgeois society, are divorced from one another and carved up between the public and private arenas of an atomized social existence. The prime concern has never been culture, then, but the total way of life of the labourer. MacDowell instructs labour historians to study trade unions because they are concrete. We, along with Marx, reply that, “The concrete is concrete because it is a synthesis of many particular determinants, i.e. a unity of diverse elements.” While we do not downplay the history of the trade union, we see it not as some abstracted, reified ideal but as part of a larger ensemble of forces, developments and elements.

It is the commitment to study totality, not culture, which separates recent examinations of workers' experience from more traditional inquiries. This notion of totality has, I think, been with some of us since we began our first forays into nineteenth century workers' history. But we did not know how to handle it. My own ambivalence was reflected in my prefacing A Culture in Conflict with a quote from Lévi-Strauss: an apology for how written history must fracture the totality of experience as it has been lived by selecting regions, groups, periods and making them “stand out, as discontinuous figures”. But it was, nevertheless, totality with which we were concerned.

Totality, as Martin Jay has recently argued, is at the core of a host of problematic developments within twentieth century Marxism. From Lukács on, it has enriched Marxist theory at the same time as it led toward certain tendencies regarded as detrimental to materialism. Lukács, under the pressures of Stalinism in the early 1930s, himself admitted that in placing “totality in the centre of the system”, he erroneously understated “the priority of economics”. This may well be a part of the theoretical foundation of the “culturalist” vs “structuralist” controversy within English Marxism. And it must be confronted by
those of us in Canada committed to both materialism and totality.31

But it will require a more explicit theoretical confrontation. Certainly the structural and determining features of economic life demand more attention than we have in the past given them; and processes of capital accumulation and economic restructuring over the longue durée need to be explored in order to situate workers' lives more adequately. Realms of culture that we have seen only dimly or partially must be more clearly illuminated. The peculiarities of place should be probed with far more sensitivity than we have displayed. The state remains little more than a word to be trotted out when necessary — an indication of how little attention has been paid to the fundamental relations of power affecting all workers. Above all else, the two-sidedness of working class life demands scrutiny. We have depicted working class culture as struggle and resistance, and this is one important component. But we have been negligent in uncovering the ways in which workers internalize the values and practices of the established society, adapt them to working class realities, and reproduce the bourgeois order. We see hints of this in the patriarchal structure and content of the working class family, which can both challenge and reinforce capital's rule, depending on particular circumstances and contexts. In working class racism, too, this process is lived out in ways that, ironically, are capable of battling authority at the same time as racism divides and weakens labour's offensive. Finally, even some of labour's largest victories — given capitalism's dynamic capacity to reverse its losses — have a two-sidedness. The growth of unions, the attainment of legally recognized collective bargaining rights, and the securities of the "high wage" were all the result of struggle, and are rightly regarded as major triumphs for labour. But triumph can become, over time, a trap as bureaucratization, legalism, and consumerism circumscribe workers and their leaders.

Three points need to be made concerning this incomplete listing of considerations on how we must begin to proceed toward an understanding of totality. First, we receive little help from historians of business, economics, urban places, politics and religion in our undertaking. While we can draw on some literature, most of it is weak and ill-suited to our tasks. Concerned with totality, we are expected to answer all comers. A
good part of the critique of our work asks the purely mundane question, "Why don't you look to religion?" "Yes," we can reply to the religious historians who ask such a question but have never themselves bothered to look at religion and class in the period of our studies, "we must get to that." This brings me to my second point: we can list what must be done, as reviewers can list omissions in any work. But what seems answerable in easy ways to sociologists and political scientists often proves for historians to be a difficult, painful, complex process of reconstruction in which essential evidence is contradictory or, worse, just not available. Totality is hard enough to conceive in the abstract. In the concrete empirical investigation of the past, it is destined to be an elusive ideal, something to strive for; a way of conceiving class relations in the past that must inevitably fall at least a little short of our imagined possibilities. Third and finally, under no circumstances should anything said above be considered a concession to the likes of McNaught and Bercuson. While I have no trouble in acknowledging that more attention must be paid to relations of gender, ethnicity, skill level, region and religion to class, my view of these relations diverges dramatically from the conventional empiricist wisdom. Bercuson, for instance, argues that such "divisions" undercut the commonalities of working class culture; and implies, therefore, that class itself is nullified, whereas I would suggest that — whatever the complexities of class experience — gender, ethnicity, skill, region and religion do not obliterate class but mediate it and layer it in particular ways that historians need to explore. The layering may be different in Hamilton in the 1880s, and in Halifax in the 1880s; just as it may be different in the Hamilton of the 1890s and that of the 1940s. But however different, that layering is not proof, as Bercuson would have it, that a working class does not exist — that is culturalism, asserting the primacy of cultural division and distinction over material relations of productive life. It is also pluralism.

As difficult, as problematic and as complex as research into the Canadian working class experience premised on the need to explore and comprehend totality will be, it remains the way to proceed. Our project, after ten years of research and writing, has produced a substantial body of work, and established new priorities and perspectives. The errors of our ways were
the result of inadequate theory and its consequences in terms of research. But it remains indisputable that in attending to class formation in the ways we have, we — and not those who would routinize labour studies in the direction of narrow examinations of trade unions, labour politics and social-democratic leadership — hold forth the promise of a history that can embrace capital and labour, production and reproduction, struggle and accommodation, culture and not culture. If this kind of history remains to be written, at least a way has been shown, however dimly the prospective path is lit.

In the 1967 preface to a re-publication of History and Class Consciousness, Georg Lukács (a central figure in the attempt to grasp totality as the very foundation of Marx's method) argued that: “It is undoubtedly one of the great achievements of History and Class Consciousness to have reinstated the category of totality in the central position it had occupied throughout Marx's work and from which it had been ousted by the 'scientism' of the social-democratic opportunists.” After a decade of new research and writing on the Canadian working class, our accomplishments are more modest. But, in any serious reflection on recent writing on Canadian working class history, a similar conclusion can be drawn. That the McNaughts, Bercusons and MacDowells reject such a conclusion is not surprising. That Drache as well avoids it tells us much about the potential of those who would listen to certain historians rather than to history itself.

Appendix 1

A part of Drache's argument rests on a conception of the peculiar skewing of the Canadian working class away from industrial employment. He argues that in 1911, 66 percent of Canada's working population was engaged in resource related occupations. One can argue seriously with the method by which such figures are derived, since all workers in rail/sail-steamship are automatically categorized as resource transportation workers; construction workers are placed in the resource-related sector; and a host of processing workers, including those employed in rolling mills in 1911, also find their way into the non-agricultural resources and processing columns. Other objections could also be raised. It is necessary to point out as well that Drache's own appendix indicates that the 1911 figure of 66
percent dropped dramatically to 49 percent in 1921; and indeed stood at the lesser figure of 61 percent in 1900. In short, the 1911 percentage is a high-water mark, if not an aberration. Finally, what needs to be stressed is that nowhere does Drache establish that such “labour market segmentation” was peculiarly Canada, peculiarly rooted in a particular colonial political economy.

To establish this peculiarity it is, of course, imperative that elementary comparisons be made between Canada and other industrial-capitalist nations. Presented below is an admittedly problematic comparison between Canada and the United States, using Drache’s own figures for Canada and roughly comparable data for the United States employment structure. The key differences relate to Categories II, III and IV, where the US data are not developed with a sensitivity to primary processing and resource transportation/other transportation differences that are comparable to Drache’s and Helen Robinson’s figures; thus understating the United States “resource-related” sector and overestimating the United States manufacturing sector. This skewing thus works in the direction of Drache’s argument.

While there is a difference between the employment structures of Canada and the United States, it remains an open question whether or not that difference is sufficient to sustain an argument about the peculiar formation and fragmentation of the Canadian working class. Putting aside the figures from 1911, and recognizing that Drache’s Category II contains many workers who (in the United States columns below) will actually be categorized as manufacturing workers, we see that (by Drache’s own liberal reading of the resource-related employment sectors) they comprised 61 percent of Canadian occupations in 1901, compared to 53 percent of United States occupations in 1900; the figures for 1921-1920 are even closer at 49.3 percent and 45.6 percent. Can this 3.7 percent — even allowing for all of the statistical refinement necessary to make these figures truly comparable — account for differences between Canada and the United States? To be sure, the manufacturing sector appears stunted in Canada, with between 13.3-16.2 percent of occupations over the twenty year period compared to 21.8-26.1 percent in the United States; but some of this difference relates directly to the different methods of
categorization in the non-agricultural resources/processing and manufacturing sectors. Whatever peculiarities may emerge from an examination of the formation and fragmentation of the Canadian working class, however, will hardly be illuminated by the denigration (as "colonial", "enfeebled" or inhabitant of an "industrial zone") of those manufacturing workers that Drache's own figures show to have been the single largest Canadian occupational category outside of agriculture in the years 1900-1920.
Employment Structures: United States and Canada  
1900-01 — 1920-21

<table>
<thead>
<tr>
<th></th>
<th>I. Agricultural</th>
<th>II. Non-Agricultural</th>
<th>III. Construction</th>
<th>IV. Transportation</th>
<th>V. Manufacturing</th>
<th>VI. Other</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>N</td>
<td>%</td>
<td>N</td>
<td>%</td>
<td>N</td>
<td>%</td>
</tr>
<tr>
<td>1900 US</td>
<td>10,710,000</td>
<td>36.8</td>
<td>970,000</td>
<td>3.3</td>
<td>1,660,000</td>
<td>5.7</td>
</tr>
<tr>
<td>1901 Can</td>
<td>658,000</td>
<td>37.0</td>
<td>227,446</td>
<td>12.8</td>
<td>153,718</td>
<td>8.6</td>
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<tr>
<td>1910 US</td>
<td>11,340,000</td>
<td>30.9</td>
<td>1,300,000</td>
<td>3.5</td>
<td>2,300,000</td>
<td>6.3</td>
</tr>
<tr>
<td>1911 Can</td>
<td>874,772</td>
<td>37.1</td>
<td>341,429</td>
<td>14.5</td>
<td>222,063</td>
<td>9.4</td>
</tr>
<tr>
<td>1920 US</td>
<td>11,120,000</td>
<td>26.7</td>
<td>1,510,000</td>
<td>3.6</td>
<td>2,170,000</td>
<td>5.2</td>
</tr>
<tr>
<td>1921 Can</td>
<td>1,013,479</td>
<td>31.9</td>
<td>256,657</td>
<td>8.1</td>
<td>185,202</td>
<td>5.8</td>
</tr>
</tbody>
</table>

Sources: Figures from Drache, “Formation and Fragmentation,” 87; adapted from The Statistical History of the United States from Colonial Times to the Present (Connecticut, 1965), p. 74. Lest this appendix be misunderstood, let me stress that I myself do not put a great deal of faith in this kind of aggregate data which, by its very nature, often misleads. The above is presented only to provide some rough comparability with figures first presented by Drache.
Notes


10. This position represents a marked difference between Kealey and myself, for I regard his “Stanley Bréhaut Ryerson: Canadian Revolutionary Intellectual,” *Studies in Political Economy*, no. 9 (Fall 1982), 103-31; and, “Stanley Bréhaut Ryerson: Marxist Historian,” *Studies in Political Economy*, no. 9 (Fall 1982), 133-71; as, at best, an overstatement.


19. MacDowell, “Workers”. (See n. 4 above.)


21. For a corrective, note Craig Heron, “Labourism and the Canadian Working Class,” Labour/Le Travail 13 (Spring 1984), 45-76.


24. Note that Drache fails to consult the major historical study relevant to this whole discussion: Donald H. Akenson, “Ontario: Whatever Happened to the Irish?,” in Canadian Papers in Rural History III (1982), 204-56.


30. Here Drache's failure to confront our arguments in Dreaming of What Might Be is striking.

31. Quotes in the above paragraphs from McNaught, "Thompson vs Logan," 150, 147; Bercuson's review of A Culture in Conflict, American Historical Review (October 1980), 1021-2; Christopher Armstrong's, review of Toronto Workers Respond to Industrial Capitalism, Urban History Review (June 1981), 55. (Note, as well, Irving Abella's review of Toronto Workers Respond to Industrial Capitalism, American Historical Review (April 1981), 482; MacDowell's review of Working-Class Experience, Canadian Historical Review (June 1984), 267-8; and Palmer, Working-Class Experience, p. 4, 321.


33. Lukács, History and Class Consciousness, xx.