

Ronald Rudin — *Making History in Twentieth-Century Quebec*. Toronto: University of Toronto Press, 1997. Pp. xiii, 294.

Ronald Rudin's combative *Making History in Twentieth-Century Quebec* has three principal aims. The first is to survey the work of French-speaking historians of French Canada and Quebec. Secondly, he proposes a substantial revision of previous accounts of this topic. Finally and most ambitiously, he attempts to convince his readers of what might loosely be called a postmodern reading of historiography. Of course, all three aims are intertwined and are based on what strikes me as a somewhat partial reading of Peter Novick's influential and much disputed *The Noble Dream: The Objectivity Question and the American Historical Profession* (1988). Rudin's book is well researched, provocative, and well written (though he needs to learn the difference between "scepticism" and "cynicism", p. 218), but, at least in its larger claims, unconvincing.

The work is most successful in pursuit of its first aim — as a survey of Quebec historical writing in French. Even here, the broad claim in the "Preface" that English Canadian historians "exercised remarkably little influence" on their Quebec counterparts is contradicted by his suggestion that Lower influenced Groulx, that Creighton and Innis played a part in the interpretations of both the Montreal and Laval schools of the 1950s, and that the "social science" approach of the so-called "revisionists" drew on some of the same sources as their English-Canadian contemporaries.

Beginning with two substantial chapters focused on the work and career of Lionel Groulx as a professional historian, Rudin then leads us through some previously uncharted territory to the so-called schools at the Université de Montréal and Laval and finally to the currently dominant figures, whom he calls "revisionist" on the grounds that they (Linteau, Robert, Courville, Séguin, and Bouchard) offer, for the first time, a radical interpretive departure. His last pages point to some evidence of dissatisfaction with the revisionist consensus in the work of Serge Gagnon and Jocelyn Létourneau, and perhaps even signs of defection from Gérard Bouchard.

Though the territory sounds familiar, Rudin's study is at once innovative and provocative. Almost everyone, except the remaining followers of Lionel Groulx (and even they may be uncomfortable with Rudin's emphasis on the professional historian as distinct from the nationalist *prédicateur*), will find something in this study to offend them. (In *Le passé composé* [1999], Serge Gagnon reveals an irritation so profound that he even defends Fernand Ouellet against Rudin!) Perhaps most controversial and unconvincing is Rudin's analysis of Groulx, who, unlike any other historian with the possible exception of Louise Dechêne, emerges from this study with an enhanced reputation. The argument goes something like this: though he began without professional training, viewing history as little more than an essential component of *éducation nationale*, Groulx gradually honed his skills as an historian (that is, discovered archival research and footnotes) to the point where he deserves to be considered the father of professional history in Quebec and, like Édouard Montpetit and Frère Marie-Victorin, a precursor of *modernité*. In support of these claims, Rudin points not only to Groulx's role in the founding of the Institut d'histoire at the Université de Montréal, the Institut d'histoire de l'Amérique française

and the *Revue d'histoire de l'Amérique française*, but also to the revisions that Groulx made in his earlier "primitive" work culminating in the multi-volume *Histoire du Canada-français depuis la découverte*. Much of this is convincing, though the assessment of the *Histoire* seems uncritical. What is unconvincing is what Rudin excludes on his way to this reassessment: all of the work that was clearly part of Groulx's nationalist politics. Like Groulx, however, Rudin never explains how the distinction between "scholarship" and nationalist advocacy can be made. Is there a clear point at which the lifelong, grudging reassessment of Dollard changed the story from propaganda to history? No one who has read Groulx's critique of the Parent Commission's discussion of the teaching and writing of history in Quebec will be convinced by Rudin's attempt to transform Groulx into Janus, one face an historian, the other a nationalist. For Groulx the "golden age" always lay in the past, though he sometimes looked forward to another in the future. Rudin shares this spirit.

After Groulx's retirement, historical research became fully professional and gradually secular in outlook. Rudin shows that the break between Groulx and his successors in Montreal and, indeed, the early Marcel Trudel was never abrupt or even complete as long as the priest-historian lived. (Had Rudin examined Brunet's obituary of Groulx, published in the *Canadian Historical Review*, he might have fortified this argument by noting that Groulx's nationalism, then and later, ensured his good reputation whatever his failings as an historian — the same standard of judgement that Groulx applied to Papineau and Lafontaine, and Brunet to Duplessis!) In contrast to Jean Lamarre's study of the Montreal school, Rudin rightly insists that the innovative character of the work of Maurice Séguin, Guy Frégault, and Michel Brunet has been exaggerated. Nor does he rate their combined contribution to scholarship very highly, Brunet being dismissed as one "fond of brash talk but who rarely set foot in the archives" (p. 118). Though he is not quite as dismissive of the Laval historians — it is impossible to ignore the massive quantity of research that went into the works of Trudel, Ouellet, and Hamelin — he nevertheless insists that Ouellet and Hamelin were motivated by an eagerness "to depict Quebecers in the worst possible light" (p. 163).

One of the most interesting sections of Rudin's study is his account of that part of Ouellet's career devoted to analysing the Papineau clan and the comparison with Groulx's treatment of the Papineau-Bourassa connection. Groulx's view, though different, was almost as critical as the view presented by Ouellet, but did not produce the same reaction. That reaction against Ouellet, which ended in a series of court cases and the shredding of Ouellet's study of Julie Papineau by the Presses de l'Université Laval, is fully described by Rudin for the first time. Here, as elsewhere, however, the book is marred by certain utterly undocumented claims. One, for example, is found on page 256 where Rudin asserts that the Canadian Historical Association's support for Ouellet would not have been given to "a more nationalistic historian". That, plus Rudin's equally fanciful explanation for the publication in the *CHR* in 1962 of Ouellet's study of the historical origins of separatism, amounts to the kind of "groundless attack" (p. 163) of which he accuses others.

Although there is a mild whiff of conspiracy in Rudin's explanation for the rise

and triumph of the “revisionists”, the account is very enlightening and perceptive. Both the claims of the new social science approach and the argument that Quebec has always been a “normal” society are sceptically assessed. He might have made more — especially given his insistence on continuity rather than rupture as the essential theme of his historiography — of the source of the “normal society” concept: namely in Séguin’s famous *Les Normes* and in the writings of both Frégault and Brunet. The trouble with “normal” is not just, as Bruce Cockburn observed, that it gets worse, but that as an historical concept it is meaningless. The “normal society” concept, as Rudin remarks, allowed the “revisionists” to practise “national” history even while emphasizing those characteristics of class, gender, region, and ethnicity that have elsewhere fragmented “national” history. Here lies the essential continuity of Quebec historiography’s mainstream.

Underlying Rudin’s often tendentious account of Quebec historiography lies a large, and I think indefensible, postmodern assumption. He writes, “I am not convinced that historical writing at the end of the twentieth century is inherently any ‘better’ than that produced earlier in the century; nor do I believe that there is such a thing as a truly ‘objective’ view of the past” (p. 6). What does this ambiguity-laden sentence mean? Who, for example, has ever claimed that one work of history was “inherently” superior to another? And what does “better” mean? Is it a moral term or simply one referring to accuracy of information and sustainability of argument? If the former, then a history of Nazi Germany that discusses the holocaust is surely “better” than one that denies it. Even in the second sense of accuracy and argument, the first is “better” than the second, just as E. E. Rich’s account of the Hudson’s Bay Company is “better” than Peter Newman’s: better research, better argument, better context.

And what about “truly objective”? Does that mean “perfectly” objective — history as science in its outdated sense? Who was the last historian to make such a claim, H. A. L. Fisher? Today even undergraduates know that written “history”, like Magritte’s famous pipe, is not the “past” (or the pipe), but a representation of the “past” (or the pipe). They also know that some representations of the past — and representations of pipes — are more accurate, more complete, more fully documented, more effectively reconstructed, “better” than others. Partly this is because historians build on the work of others. Of course any understanding of the past remains incomplete, since history is not the past but a selection and reconstruction of it. Nor can one expect to discover a final “meaning” or interpretation — the end of history. None of that means that historical writing is, as White and LaCapra suggest, merely fiction in another guise. (The difference between an historian and a writer of historical fiction, Margaret Atwood once remarked, is that novelists can fill in the gaps with imagination, while an historian must leave the gaps.) When a student asks Rudin for readings on, say, New France, does he reply, “read whatever you find, all accounts are equal”? Of course not.

Indeed, if Rudin adhered with any consistency to his ill-conceived postmodernism, he would have written a quite different, blander book. He could hardly have insisted that the second edition of Groulx’s *La naissance d’une race* was “better” (more thorough) than the first. Nor, surely, could he have described Louise

Dechêne's *Habitants et marchands* as a "masterpiece", for such a category would not exist unless one work of history was not "better" than another. One can only conclude that Rudin himself has succumbed to a weakness he ascribes to a major Quebec historian: "a tendency ... to take a good idea and push it to an illogical conclusion."

Ramsay Cook

*Dictionary of Canadian Biography*

David W. Lloyd — *Battlefield Tourism: Pilgrimage and the Commemoration of the Great War in Britain, Australia and Canada, 1919–1939*. Oxford: Berg, 1998. Pp. xi, 251.

The cultural historiography of the First World War has long been dominated by a single debate: did the war usher in the modern era, or did it affirm the resilience of the traditional world? For years, one was either a Fussellite or an anti-Fussellite, a classification that determined one's view of everything from postwar literature to art to social relations. However, the polarization of the field is beginning to change. In a fine addition to Berg's series "The Legacy of the Great War", David Lloyd takes another step in the right direction by interpreting the pilgrimage movement as a complex mixture of high and low culture, the sacred and the profane, and tradition and modernism.

The book's scope is actually broader than the title suggests. Studies of postwar pilgrimages are often confined to journeys to the old front lines, but Lloyd makes a convincing case for including visits to the major war memorials in Britain and Australia, like the Cenotaph in Whitehall and the Tomb of the Unknown Warrior in Westminster Abbey. These, too, were discussed in terms of pilgrimages and were endowed with the same sacred aura. The exception to this occurred in Canada, where the word "pilgrimage" was rarely applied to visits to domestic memorials, as it was in Britain and Australia. Lloyd puts this down to "a level of understatement in the tone of commemoration in Canada" (p. 188).

It has long been realized that the study of commemoration must account for the interplay of competing interests, particularly the struggle between official and vernacular memories, and Lloyd does not shy away from this thorny issue. He raises some provocative notions, like the suggestion that the Tomb of the Unknown Warrior was a plot by the Church of England to counteract the immense popularity of the non-denominational Cenotaph. He explores this theory fully and effectively, but comments less on an even more interesting revelation, that planners of the Australian pilgrimage of 1938 were interested in including only tall, healthy veterans with no physical disabilities to project an image of a strong, vibrant, manly nation. The participation of two disabled veterans, as suggested by the Blinded Soldiers' Association, was summarily rejected, and only considerable public pressure convinced the planners to allow seven nurses to join the pilgrimage, thereby compromising its masculine identity. Lloyd might have made more of this matter, given his otherwise