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COMPARATIVE STUDIES AND MILITARY HISTORY

A KEY TO THE FUTURE OF HISTORICAL SCHOLARSHIP IN THE UNIVERSITY

CONDITIONS FOR THE GRANTING of tenure and the qualifications governing promotion or appointment to full-professorships are subjects at present exercising the mind of the academic community in Canada. The Canadian Association of University Teachers wishes to press for the granting of tenure after five years as a university appointee, regardless of rank.¹ The argument is heated. A similar battle rages over the question of appointment to the rank of professor. The convention of awarding full professorships primarily in recognition of scholarship and academic achievement has for some years now been more generally honoured in theory than in practice. In the course of a recent visit to a major Canadian university I had the opportunity to discuss these two questions with the chairman of a department of history. He told me that in future tenure would be awarded in his department, and, in his opinion, in all departments at all Canadian universities, on the basis of four criteria. These were: teaching ability; participation in the life of the university; scholarship; and the individual's record of activity in the community, the press and on local and national radio and television. These criteria would not, of course, receive equal weight; but it was very clear that in the mind of this chairman scholarship ran a poor third to teaching and university participation. It was admitted that the fourth criterion, that of public exposure, had as yet not been fully established. Nonetheless, the chairman was confident that it was becoming a factor of growing significance in deciding university appointments. Teaching ability was to be measured not merely by acceptability of performance but by the yardstick of "relevance". With increasing numbers of non-specialist undergraduates, history must be made relevant to their interests and current preoccupations. Broad thematic studies relating past to present were regarded by this

chairman as being of the utmost importance. "Research scholarship" could not meet the requirements of such curricula and must, therefore, give way in importance to teaching relevance.

All of this was and is profoundly disturbing. It is disturbing because, on the one hand, it is typical of the overreaction of many departments of history to the pressures of the past five years. On the other hand, it is disturbing because it savagely overstates what is otherwise a good case for improving university curricula in history.

No case can be made against excellence in scholarship. But a case may be made for improving curricula, and therein lies the saving grace of these alarming statements. However, that redeeming feature must be fostered and encouraged before the trend against scholarship gains a momentum that cannot be arrested.

In recent years there has been a resurgence of interest in "thematic" studies. There has been an attempt to get away from the more traditional chronological treatment of historical subjects and to take up themes such as "revolutions", "radicalism" and "war". Chronologically-based courses, however, like chronologically-oriented historical writing, are and will remain the basic stuff of historical scholarship. Nonetheless, more and more attention is being paid to social, intellectual, constitutional, economic and even military history. The case for "relevant" historical studies, however, goes much further than the case for more courses on, say, "the social history of Manitoba" or even the establishment of courses on "social conflict in Manitoba". It suggests the introduction of courses in "social conflict".

It is not, however, readily apparent that the proposed change in favour of broad theme studies tending towards relevance is in harmony with the current school of research excellence founded upon meticulous detail. The approach of the research specialist conflicts with that of his colleagues devoted to more general studies, whether those studies are of the more traditional chronological or broad relevance-oriented sort. Indeed, the conflict which underlies this contrast in styles of scholarship, between the special and the general, arises now as it has in the past from what is often the conflict between the teaching requirements of the university in which the scholar teaches and the research methods that he uses in his own field. The desire for broad thematic teaching and for "relevance" go hand in hand. But whereas, for example, a relevant course on "social conflict in Manitoba" perhaps lends itself to the detailed treatment of the research scholar, one on "guerrilla warfare" or "revolution" generally does not. And so it appears that there is a conflict

in the new history as in the old between the approach of those who are research-based in their teaching and those whose sources are in essence a re-working of a series of basic textbooks. But it is a conflict and not a confrontation: the demand for relevance is identified so closely with the insistence upon breadth that the assault of relevance and the trend towards broader teaching are virtually synonymous.

Departments of history have reacted to the demand for relevance either by withdrawing into their shells or else with an over-sensitive proliferation of thematic and "service" courses. The former bury their heads in the sands, refusing to face the realities that began to press upon universities when they decided to seek their fortune in expanded student numbers and, so to speak, accepted the "king's shilling". The latter have adopted the opposite extreme. Both are wrong. The one, because they will not see that, regrettable as it may be, the clock shows no signs of being turned back. The other, because either they are deluding themselves or because they have an entirely novel conception of scholarship. Historical studies will prosper under neither.

It must be clear that historical scholarship in the main is not and cannot be "relevant". But that does not mean that historical studies in universities must become antiquarian. They can and must be made more acceptable—more palatable—to the now vast preponderance of non-specialist undergraduates. Nonetheless, seen in the most favourable of lights, the implications of this desire for a broader approach to historical studies in undergraduate curricula are serious. If historical studies are to survive, to preserve their scholarship and to foster a continued emphasis upon excellence, this conflict between teaching requirements and current research scholarship must be resolved.

A possible solution to this conflict lies in the thoughtful development of comparative studies. Comparative studies tend to be thematic in nature. Equally, however, they rely, or ought to rely, upon the *detailed* comparison of historical materials bearing upon a central theme. But comparative historical studies rest on a slender foundation of scholarship. They have been too few and too uneven to attract sustained interest. Comparative studies as a whole suffer from overexposure and underdevelopment. The term is impressive, the output is not. In history this is because the concept has yet to be defined by practice. The tendency has been to be too ambitious. The result is that little has been accomplished in the face of physical, financial and time limitations. The problem is one of implementation. The solution is one of precision.²

The comparative technique has been exploited by political scientists.

Departments of government and politics, institutions such as RAND, Brookings, Hudson, Defense Analyses, Chatham House and Strategic Studies, are familiar with comparative studies in politics and related fields. Indeed, the quantitative analysts are facing a counter-attack from their more empirically-minded colleagues.³ The empiricism of this latter group has sustained itself upon the current school of historical research, which has provided these scholars with evidence and essential data. But their lack of access to documentary evidence and their poor judgment in the use of the product of historical scholarship has restricted the development of these scholars.⁴ Something stands to be gained from a meeting of minds between the empirically-minded political scientist and the historian.

Such a meeting of minds might be achieved through the exploitation of historical comparative studies. A successful combination of comparative history and heuristic political science might not improve the scholarship of either, but it would certainly promote tolerance.

And so there are good reasons for pursuing comparative studies in history. But if comparative studies in history are rootless, if they are impressive in concept but hollow in achievement, how are they to be implanted and cultivated? Perhaps the beginning of the solution lies with military studies. This, at any rate, is the proposition that I intend to argue.

Military history has been the forerunner of the current school of "blow by blow" historical research. The official and quasi-official histories of the Franco-Prussian, Boer, Russo-Japanese, 1914-1918 and 1939-1945 wars are examples. The same may be said of the endeavours of Napier, Oman, Corbett, Fortescue, Firth and Clowes.⁵ This is not to say that military history can always, or even frequently, lay claim to the accolade of scholarship. But it is a field that lends itself to comparative treatment. The study of war is necessarily comparative. Military archives are depositories of historical materials that are themselves comparative in nature. Good military history embraces much that is not strictly military. Yet at the same time it is specific, hauling always to the line of war and peace.

The broadest of all military studies are those which deal with administration and organization. For they deal with the relationships of military establishments, especially in time of peace, with their environments. My continuing comparative study of defence organization and attitudes towards defence organization in Great Britain, the United States and Canada may perhaps be cited as a modest example of comparative studies in practice. This study is providing a far-ranging thematic appreciation based upon detailed research. It

has distilled certain governing factors concerning defence organization in Britain, the United States and Canada, which are in addition of wider interest to the empirically-inclined theoreticians. But there is nothing magic about comparative studies on an international as against the national level. They may be accomplished just as well on either. In the present case it is not possible to say that Britain, Canada and the United States have organized for the same reason, that their administrative structures have had the same prejudices and influence, or that their military services have reacted in the same way. But it is possible to say that they did organize for related reasons, that they did establish administrative structures that reflected current preoccupations, that those preoccupations did tend to distort policy-making, and that under similar circumstances service reaction conformed to a certain pattern.⁶ None of this is very startling to the political scientist. What is important is that such specific conclusions can be reached by historians on the basis of fact while they are more generally conceived by political scientists on the basis of theory. These are grounds for progress.

The historian cannot fill the shoes of the theoretician—however much certain departments of history may be tending in that direction in their search for “relevance”. The historian has no desire to do so. But he can do much to assist his colleagues who are political scientists, and it is conceivable that he may learn from them. The historian is notorious for his attention to detail. But it is the essence of his calling, and he would not have it otherwise. Subject the political scientist’s theoretical conception to a conclave of historians and in a trice the theory is riddled through with inescapable exceptions. Such gatherings fall apart in attempting—if indeed they try at all—to isolate what are and what are not “acceptable exceptions” insofar as the theoretical premise is concerned. It is here that comparative history offers a unique advantage. For in the hands of the skilful empirically-minded theoretician the historical evidence of comparative studies will begin to isolate that “acceptable exception”. In that event the union of the two disciplines will indeed bear fruit.

What emerges from this for the historian? The historian will be cheered because detailed studies such as this not only preserve historical scholarship but they permit a broadening of the teaching horizon. They make the scholar palatable to those who do not now and never will share his intimate concern for scholarship. They permit a union between research and teaching that is essential both to the teacher and the taught. They combine specialization with breadth. They eliminate over-specialization and over-generalization, both of which are unhealthy, besides being unacceptable in the current milieu

at most universities. Finally they will permit a restructuring of university curricula that will provide not only for the academic tastes of the preponderance of students, but also a measurable improvement in that curricula, ineluctably linking research with teaching.

NOTES

1. 'C.A.U.T. Policy Statement on Academic Appointments and Tenure' *C.A.U.T. Bulletin* February 1968, Vol. XVI, No. 3, p. 9.
2. Richard A. Preston's *Canada and 'Imperial Defense': A Study of the Origins of the British Commonwealth's Defense Organization 1867-1919* (Durham, N.C.: Duke University Press, 1967) approached establishing a model for the comparative historian to emulate.
3. See Robert Jervis 'The Costs of the Quantitative Study of International Relations' *Contending Approaches to International Politics* Klaus Knorr and James Roseman, eds. (Princeton, N.J.: Princeton University Press, 1969), 177-217.
4. This shortcoming is widespread. A comparatively refined example, and one that is well known, is Samuel P. Huntington, *The Soldier and the State: The Theory and Politics of Civil-Military Relations* (Cambridge, Massachusetts: Harvard University Press, 1957). His more recent *The Common Defense: Strategic Programs in National Politics* (New York: Columbia University Press, 1961) shows the excellence that the empirically-inclined political scientist is able to achieve when he is not reduced to seeking examples in potted so-called comparative histories.
5. Mahan, Clausewitz and Jomini do not rate here. They were gifted theorists and exponents of the principles of the military function, but they contributed only marginally to the technique of their art as historians.
6. I have established a number of interesting relationships between current policy-making and organizational problems and the historical development of defence administration in these countries. One in particular refers to the role of technical and managerial factors. In this connection see above footnote 2 and James R. Schlesinger 'Quantitative Analysis and National Security' *Problems of National Strategy* Henry A. Kissinger, ed. (New York: Frederick A. Praeger, 1965), 85-107.