

failure to raise the discussion of any of the propositions much above the level of terse, logical demonstration, is in part the penalty it pays for its narrow, mechanical terminology. To the question of the degree of structural determinism in the growth of capitalism the author, in a passage cited above, answers with the obfuscations that result when the logic of an ambiguous specialist language is turned in upon itself. The inevitable difficulties on the theoretical plane are joined by others on the level of the treatment of historical data. To choose only one example, the author nowhere presents data from English medieval society to establish his crucial contention that peasants were successfully denied property in land on the scale that his theory requires. When most historians, Marxist and non-Marxist alike, suggest the contrary to have been the case, his failure here is particularly damaging.

If the book fails to make its case on evidential grounds it is, nevertheless, its attempt to unite a theoretical with an historical inquiry that chiefly distinguishes it. Its method forces on the reader questions that transcend particular ideological frameworks — the power of categories of analysis to privilege certain data, and hence the problem of the circularity of the explanations they offer for the phenomena they select. Its appeal to theory as well as experience as its grounds for placing the question of man's exploitation of man at the centre of its concerns challenges all those interpreters who ignore the question or assign it to the fringes of their inquiries. But if it challenges, it seems unlikely to convince. Its narrow insistence that the "economic" alone need be taken into account in the study of exploitation will strike many as a vicious circularity between a theory structured solely in categories of economic relationships and an explanation similarly structured. This narrowness of vision is likely to deny the author an audience who could learn from him — and from whom he too could have learned.

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GÉRALD CHOLVY — *Mouvements de jeunesse chrétiens et juifs. Sociabilité juvénile dans un cadre européen 1799-1968*. Paris, Éditions du Cerf, 1985. 432 p.

Les mouvements de jeunesse ont fourni au 20^e siècle les cadres de la société en Occident. Il était temps que les historiens se penchent sur ces milieux d'éducation entre l'école et la famille et qu'ils prennent plus au sérieux ces organisations liées à un âge de la vie que l'adulte a tendance à occulter. Cette histoire éclaire celle des idéologies, des « mentalités », voire l'histoire politique et religieuse. Tirant partie de la démographie et de l'histoire des couches socio-professionnelles, elle éclaire à son tour l'histoire de la société tout entière. Après les aînés et l'enfance arrivent donc l'adolescence et la jeunesse dans le champ de l'histoire qui se veut totale.

Suivant le mécanisme connu, l'histoire des institutions précède celui des classes d'âge ou des genres de vie. Aussi, l'histoire de la jeunesse restant *terra incognita*, on commence à peine à lever le voile sur les organisations de jeunesse accessibles à travers les archives souvent mal tenues (les organisations volontaires ont d'autres priorités!) et des témoignages oraux d'un usage délicat (tendance à l'idéalisation ou à l'apologie rétrospective).

Le présent ouvrage constitue une gerbe fort riche réunie par Gérard Cholvy, l'infatigable professeur d'histoire des mentalités contemporaines de l'Université de Montpellier. Plus de 25 historiens contribuent à un ouvrage d'une diversité remarquable quant aux organisations : du Sillon aux mouvements sionistes en passant par les scouts, la JEC, les patronages et les Y.M.C.A. Comme il arrive dans ce type d'ouvrage, chacun procède à sa façon et l'ensemble souffre quelque peu d'un décousu. À chaque auteur aussi sa conception du mouvement de jeunesse. Les périodes sont choisies au hasard des recherches. Tel article est moins intéressé à la jeunesse qu'à l'Église ou à l'idéologie.

Fort heureusement, une introduction magistrale de Gérard Cholvy pose des jalons précieux, fournit une bibliographie critique et propose des avenues de recherche fécondes (origines, implantation, méthodes, encadrement, relations avec les Églises, dimensions internationales, spiritualité). Un indispensable index des noms propres, une utile chronologie, une liste des abréviations (en réalité des sigles) qui permet de se retrouver dans ce monde quelque peu ésotérique et des notes biographiques qui aident à situer le regard de chaque auteur d'article (« ancien » du mouvement, universitaire en mal de thèse, etc.) complètent cette somme indispensable de *membra disjecta*. On peut regretter que, comme beaucoup d'ouvrages publiés dans l'hexagone à l'enseigne de l'Europe, la part du pays de de Gaulle et de Pétain occupe les deux tiers de l'espace. Si la Belgique, l'Espagne et la Pologne y figurent, l'Italie en est tout à fait absente et la Grande-Bretagne n'y apparaît qu'à travers des mouvements internationaux.

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PHIL GARDNER — *The Lost Elementary Schools of Victorian England*. Beckenham, Kent: Croom Helm, 1984. pp. viii, 296.

What is a school? To the producers of the official statistics of educational organization in Victorian England the term clearly did not apply to the myriad small, local and often informal places of learning attended by many working-class children. Official statistics in consequence do not take account of the educational reality of the industrial and urban working class. In this very important contribution to the history of English education, Phil Gardner demonstrates that historians of education generally — even those on the left — have reproduced a part of the official view of working-class schooling. He sets himself the ambitious task of rediscovering and rehabilitating those educational institutions consistently derogated as 'dame schools', and in my opinion he succeeds admirably. This book is interesting for the methodological issues it raises, for the empirical reality it uncovers, and for the absence of romanticism in its treatment of a subject where the temptation to romanticize is continually present.

Despite the impetus from the 'revisionist' educational history of the late 1960s and early 1970s, which encouraged researchers to raise questions about forms of educational organization, many historians of education have relied entirely upon official documentary systems to produce their accounts of educational development. This has often been from necessity — official knowledge is often the only record left to us — but often from preference as well. Yet Gardner points to real lacunae in the official documentary system of English education, lacunae which were opened both by the techniques of educational information-gathering and by the interests of information-gatherers themselves. The ways in which public authorities constituted 'educational intelligence' in fact generated the historical problem which Gardner seeks to address: the invisibility of working-class private schooling.

The statistics of educational organization in England from 1830 until the early 1900s were more or less consistently generated by groups and bodies interested in reforming working-class education. The efforts of the Statistical Societies in the 1830s and 1840s, the Educational Census conducted by Horace Mann in 1851 and the work of the Newcastle Commission produced views of popular and working-class education shaped both by class prejudice and by political interests in promoting 'public' education. A crucial part of Gardner's book is an attempt to measure the extent and nature of working-class private schooling, particularly in the city of Bristol. He attempts to correct the biases of the official record, by comparing educational censuses with population censuses, and by drawing upon other sources. Gardner shows that the official educational record consistently