

Interpreting Historical Literacy: The Pattern of Literacy in Quebec — A Comment

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As claims and counterclaims about the quantity and quality of contemporary literacy frequently dot the pages of the press and periodicals, those regarding the changing distribution and possible significance of past levels of literacy are becoming more common in the historical literature. Roger Schofield's 1968 lament, "Despite its relevance to many kinds of historical study, literacy does not feature very often in historical discussion..." is now contradicted by the recent interest of social, cultural, and economic historians in literacy and by the corresponding volume of published research, in Europe and North America. About the challenge to the second part of Schofield's complaint, "and when it does appear a certain vagueness surrounds its meaning", I am less sanguine and optimistic. Historians might well take some small measure of satisfaction in these accomplishments and the signs of continued efforts.¹ It is very important that studies in a relatively new and still developing area be carefully assessed by constructive criticism, especially in a field of study in which paradigms and consensus are lacking. In this context, I propose to review Allan Greer's recent essay, "The Pattern of Literacy in Quebec, 1745-1899."² Though I take pleasure in seeing new literacy studies in these pages³ and though I admire Greer's efforts and his prodigious research, I have serious questions about the article's contents. My brief remarks focus on issues of conceptualization, method, source criticism, analysis and interpretation. Although the following comments concentrate on Greer's analysis of literacy specifically and the explicit ramifications for historical

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¹ SCHOFIELD, "The Measurement of Literacy in Pre-Industrial England," in *Literacy in Traditional Societies*, Jack GOODY, ed. (Cambridge, 1968), p. 312. See also, Harvey J. GRAFF, "Literacy and History," *History of Education Quarterly*, 15 (1975): 467-74; IDEM, *Literacy and History: An Interdisciplinary Research Bibliography* (Chicago, 1976; second expanded edition, 1979); IDEM, "'The New Math': Quantification, the 'New' History, and the History of Education," *Urban Education*, 11 (1977): 403-40; IDEM, *The Literacy Myth: Literacy and Social Structure in the Nineteenth-Century City* (New York and London, 1979).

² GREER, "The Pattern of Literacy in Quebec, 1745-1899," *Histoire sociale — Social History*, XI (1978): 295-335.

³ GRAFF, "Literacy and Social Structure in Elgin County, Canada West," *Histoire sociale — Social History*, VI (1973): 25-48; IDEM, "What the 1861 Census can tell us about Literacy: A Reply", *Histoire sociale — Social History*, VIII (1975): 337-49; H. J. MAYS and H. F. MANZL, "Literacy and Social Structure in Nineteenth-Century Ontario; An Exercise in Historical Methodology," *Histoire sociale — Social History*, VII (1974): 331-45; Jean-Guy DAIGLE, "Alphabétisation et culture populaire dans l'Angleterre victorienne: état de la question," *Histoire sociale — Social History*, X (1977): 5-23.

literacy studies more generally, they also speak to continuing problems in the epistemology of quantitative historical research and social historical reconstruction.

The conceptual level marks the most important point to begin. As historians seem to note increasingly, conceptualization is far from the strongest characteristic of historical studies; and this aspect of historiography has recently seen much greater and sustained interest as a result.⁴ Literacy studies are especially plagued by problems and shortcomings; and Greer's treatment is no exception. First, consider definition(s) of literacy. Throughout his article, Greer refers to such categories as literacy, illiteracy, and semi-literacy, and to reading *and* writing, reading *or* writing, reading, reading at least, and signing. A profusion of categories or types of literacy obtains, but neither typology nor consistent definition of the concept of literacy as a subject for analysis, as (dependent of independent) variable or as a level of abilities or skills follows. As I have considered elsewhere, literacy and illiteracy are far from clear, explicit, or self-evident qualities or characteristics; their very nature is problematic, and as such, must be so construed in studies. They require explicit and consistent definition and interpretation, if the historical analysis is to be accepted.⁵ Literacy, for example, can be taken as a functional or practical skill or as primarily attitudinal; it may be defined as functional or nonfunctional. Its impact can be conservative or liberating. Although virtually all historical sources present evidence about its distribution in dichotomous or trichotomous terms, we need to interpret it as a more continuous variable.

Does Greer conceptualize and define literacy as analytic variables satisfactorily? Never in the article is the reader rewarded with a developed definition or one which is applied regularly. We are told that literacy is "one basic skill" (p. 296), but never what kind of skill, for what uses, or in what contexts. Further, we are told that "Literacy is part of elementary education and it deserves treatment based on direct evidence since it is not simply a reflection of schooling as it is often assumed" (pp. 296-97). Yet, what part of primary schooling and what place in education and socialization literacy holds, what changes occur in that place throughout the lengthy period from 1745 to 1899, or what functions and meaning literacy acquires are never evaluated. In addition, the directness of the evidence and the comparability of measures remain problematic throughout, as we shall observe below. The principal issue is a critical, and simple, one: methodologically and epistemologically, researchers seek to measure and/or to develop from data — Greer's main aspirations — without establishing clearly the parameters and values of what they attempt to measure. In other words, to measure literacy, especially from a number of different

⁴ See, for example, David H. FISCHER, *Historians' Fallacies* (New York, 1970); Lee BENSON, *Toward the Scientific Study of History* (Philadelphia, 1972); William AYDELOTTE, *Quantification in History* (Reading, Mass., 1976); Arthur STINCHCOMBE, *Theoretical Methods in Social History* (New York, 1978); the writings of J. H. Hexter; the pages of *Historical Methods* (formerly *Historical Methods Newsletter*) and *History and Theory*; GRAFF, "What the 1861 Census can tell us about Literacy," among many examples.

⁵ GRAFF, "What the 1861 Census can tell us about Literacy."

sources and over a period of time in which we may expect and know the very value of literacy to change and to increase in significance as in scope of its distribution, without conceptualizing just what literacy (and levels therefore) is interpreted to signify seriously limits the usefulness and reliability of the numerical exercise itself while severely restricting the possibilities of drawing inferences or making deductions from any patterns claimed.

This is hardly a trivial issue. In contemporary and historical writings on literacy, we find a disturbingly large number of claims made for the significance of literacy, claims which culminate in a canon of social thought and ideology which I have come to term the "literacy myth."⁶ Without empirical examination, confirmation, or refutation, many qualities and attributes are claimed for literacy; often they are said to be of a universalistic nature. These include a wide range of attitudes and values which tend to dominate over practical or functional daily skills, as well as some lower-level, more specific functional skills. The very nature of literacy, its impact on individuals, groups, and societies, its uses and abuses, its very meaning, are all quite controversial. Commentators either make undeveloped and unverified assertions or they skirt these crucial matters. The historical study of literacy and all those concerned with that history suffer as a result. Greer, unfortunately, does not fare well in this conceptual arena; the remainder of his article reveals the consequences of this neglect and some important opportunities which his data might allow are lost in the process. He has established patterns, no doubt, but patterns of what is quite another question.⁷

Conceptualization enters the evaluation on yet another level. Greer sets forth his purposes (p. 297) for the presentation of *nus* numbers. These include the exploration of the relations among education, society, and economy; the resolution of questions regarding the educational consequences of the English conquest; the measurement of literacy rates; and the evaluation of the cultural, social, and economic factors influencing literacy. This list embraces a number of important issues, but the research strategy developed to implement these concerns, which are rarely translated into explicit and answerable questions, is not one which makes satisfactory and persuasive interpretations likely. Research design and modes of social historical inquiry are the principal issues here; from this promising roster of implicit questions, the author retreats into a largely descriptive presentation of his data, which while addressing honestly their deficiencies never quite succeeds in bringing them to bear on the larger, encompassing issues. The reasons for this are several; furthermore, they reveal serious problems in the linkage between conceptualization and empirical strategies, both of which impinge directly on interpretation. The fact of the matter is that the research design, of alternatively comparing the results, largely chronolog-

⁶ GRAFF, *The Literacy Myth*, Preface, Introduction, *passim*.

⁷ Greer's occasional disclaimers about only measuring and not explaining cannot be accepted. In the first place, he does offer interpretive comments and conclusions throughout the essay, as I have noted above; furthermore, his goals and intentions go well beyond measurement alone. The research that is required even to establish time series on literacy require, it should be clear by now, interpretive actions in order to proceed. I believe these comments keep well within the bounds of the essay as set by the author.

ically, of four different sources and four different measures of literacy, by definition can only bear on the third of his four major themes or issues: the measurement of literacy. Different strategies and the fuller exploitation of the sources in combination with other social, economic, and cultural measures and information are required for the other, larger purposes as they are for the interpretation of what these literacy rates mean.

Greer's treatment of the other issues is at best descriptive, cursory, anecdotal, and, in the main, unsystematic and unanalytic. With the exception of gross differentiations by occupation (an imprecise measure of status, class, or wealth especially in a primarily agrarian region dominated by farmers), urban-rural residence, religion, and ethnicity (three rough and independent dichotomies), no direct socio-economic or cultural factors are introduced into the presentation. Indirect references to such factors are made consistently, with the intent of explication and interpretation; these are unsatisfactory, unconvincing, and insufficient to advance his arguments. The questions which follow from Greer's main concerns are resolvable on several different levels of aggregation; they may be closely approached on a provincial level, a regional one, or on the level of the community itself. Different kinds of data and, correspondingly, different kinds of techniques are required, from large-scale ecological correlations to individual-level analyses. The strategy required for Greer's purposes would then follow from his election of the level of analysis appropriate to his goals. Rather than make that kind of decision, the author replaces inquiry and design with generalizations and assumptions about the nature of religious influences (crudely dichotomizing Catholicism and Protestantism) and of popular mentalities without attempting to investigate directly any of their effects; caricature and stereotype substitute very uncomfortably for historical analysis. As a close reading of the text reveals, inadequate conceptualization of the problem and design of the research task can end only in superficiality, inconsistency, contradictions, and unacceptable conclusions.

Consideration of the role of religion and socio-economic factors provides the most serious examples of this problem and its impact. Take religion first. Sprinkled throughout the essay, but stated more cogently in the conclusion, is Greer's attribution to Catholicism and its impact on popular mentalities and "religious traditions" the responsibility for a relative indifference to literacy and elementary education on the part of the clergy and the rural masses (pp. 333-34). Latent within this understated casual framework are implicit assumptions about the relationships between religion and literacy and an attempt to come to terms indirectly with the kinds of issues regarding the Church's educational contributions with which Greer commences his discussion. Dominant here are his assumptions, derivative perhaps from earlier and traditional secondary accounts and contemporary opinion, that Catholicism correlated negatively with an impetus toward mass literacy while Protestantism ("the religion of the book") related much more positively and directly.⁸

⁸ See esp. Greer's conclusions, pp. 330-35, and 317-18. Repeated references to Carlo CIPOLLA, *Literacy and Development in the West* (Harmondsworth, 1969) are insufficient for his purposes. See also the literature cited in note 9, below.

Conceptualization of the role of religion's impact on literacy and education is the central problem; this in turn influences Greer's entire efforts at conclusion-making. In Greer's view, the question is not one of material resources, class or wealth ("rural Quebec had a fairly undifferentiated agricultural economy," p. 334); ethnic origins per se ("the fact that anglophone Catholic school attendance was so low," p. 333, but was literacy?); density of settlement (here he is contradictory, but rather "religion was as the root..." p. 333). Perhaps! but the evidence presented is certainly not sufficient to make or break the case. Whether Allan Greer in fact means Catholicism and Protestantism generically *or* whether he discovers, at base the effects of religion only in interaction with local conditions, the relative weight of which is problematic itself, is a separate and more central question.

What is found here, and it is not uncommon, is more the influence of traditional assumptions and expectations about the nature of Catholicism and Protestantism than the investigation of factors which influence the social structure of literacy directly in specific local and regional contexts. Consequently, alternative causal frameworks are not proposed; the assumptions themselves are not explicitly tested — only gross distributions are presented.

Recent research in the history of post-medieval history of European literacy provides good reason to question and qualify this simplistic but powerful ascription wholly to religion's impact on popular mentalities, on one hand, and its impact on educational promotion, on the other. This research, which should be used to establish comparative perspectives and parameters for the evaluation of the Quebec distributions and trends, encompasses parts of Western, Central, and Northern Europe from the Reformation to the nineteenth century. Taken collectively, it demonstrates, among other important issues, that neither Catholicism nor Protestantism themselves or in isolation from other factors is sufficient to correlate simply and independently with educational promotion or literacy rates. For example, high literacy Catholic areas and low literacy Protestant ones are not unusual; reforming Protestantism's educational influence was a profoundly conservative one; the Counter-Reformation had important positive educational results; neither Church hierarchy nor local clerics can be assessed as negatively or equivocally in their educational opinions or actions as Greer seems to suggest. The French (which receives virtually no attention in this article) and the German ones are the most suggestive cases; the criticism of the traditional Catholic-Protestant dichotomy in those nations ranks among the most powerful of the findings of recent research. In this regard, it is also interesting to note that the recent work of Furet and Ozouf found the clergy in parts of France more opposed to instruction of the masses in writing but not in reading. For such a context, which may have Quebec parallels, we need to assess carefully the levels of literacy deduced from signature data. Similarly, a common theme has been the close connection tying literacy to social stratification and inequality.⁹

⁹ Among a large and growing literature, see Gerald STRAUSS, "Success and Failure in the German Reformation," *Past and Present*, No. 67 (1975): 30-64; IDEM,

Thus we cannot settle for religious explanations apart from other issues or for references to popular mentalities based solely in religious beliefs, without regard to socio-economic, political, cultural, or other issues. The cases cited here, especially those which focus either on other Catholic areas or on Protestant-Catholic comparisons supply comparative grounds at least as useful, if not more so, than those of New England and Great Britain. Comparisons thus illustrate that not all Protestant regions (or migrants) were high in rates of literacy and not all Catholic ones were low; that poor and primarily agrarian places (i.e., Sweden and Scotland) could have very high rates of literacy. In New England, for example, as Lockridge argues, initial rates of literacy were far from universal (if still comparatively high), educational emphases were not among the successes of the early generations of settlers as literacy spread very gradually, and literacy rates grew only with maturity of settlement.¹⁰ Protestantism in the aggregate may relate to higher levels of literacy than Catholicism, but as universal, independent causal factors they are less than enlightening for interpretation. In Quebec we need to go beyond abstract notions about religion in isolation from other factors to account for rates of literacy, differentials, and changing patterns: to make sense of the numbers, in other words, and to discover if the numbers themselves make sense (these intellectual activities cannot be separated).

There are suggestions of other important factors within Greer's materials, which are either ignored or inexplicably rejected. The most important relate to socio-economic conditions and material resources. Allan Greer implies that Quebec before at least the 1840s was an undifferentiated agricultural society and that the preponderance of farmers in occupational lists (including those in his sources) supports a conclusion of a relative lack of social stratification. For example he asserts, "The communities considered here are rural (except for Three Rivers) and there is no reason to assume that the English ones would have had a different occupational structure from the French ones." (p. 317) Implicitly rejecting socio-economic or materialistic arguments and contradicting some of the evidence relating to occupational differentiation presented earlier (pp. 303-04, Table 3 although urban, is revealing), Greer resorts immediately to arguments based in the differences between Catholicism and Protestantism (pp. 317-

"Reformation and Pedagogy: Educational Thought and Practice in the Lutheran Reformation," in *The Pursuit of Holiness*, Charles TRINKAUS and Heiko A. OBERMAN, eds. (Leiden, 1974): 272-93; IDEM, *Luther's House of Learning* (Baltimore, 1978); Lucien FEBVRE and Henri-Jean MARTIN, *The Coming of the Book* (London, 1976); Egil JOHANSSON, *En Studie Med Kvantitativa Metoder av Folkundervisningen I Bygdeå Socken, 1845-1873* (Umeå; Sweden, 1972); IDEM, *The History of Literacy in Sweden* (Umeå, 1977); JOHANSSON, ed., *Literacy and Society in a Historical Perspective: A Conference Report* (Umeå, 1973); Kenneth A. LOCKRIDGE, *Literacy in Colonial New England* (New York, 1974); Mary Jo MAYNES, "Schooling the Masses: A Comparative Social History of Education in France and Germany, 1750-1850" (unpubl. Ph.D. diss., University of Michigan, 1977); François FURET et Jacques OZOUF, *Lire et écrire: l'alphabétisation des Français de Calvin à Jules Ferry* (Paris, 1977); John BOSSY, "The Counter-Reformation and the People of Catholic Europe," *Past and Present*, No. 47 (1970): 51-70; Natalie Z. DAVIS, *Society and Culture in Early Modern France* (Stanford, 1975) pp. 189-226; and the unpublished research of Lionel Rothkrug.

¹⁰ LOCKRIDGE, *op. cit.*

18). Does he offer us any reason to accept his line of argument? First, there is no reason whatsoever to consider these rural areas as relatively undifferentiated, unstratified, or egalitarian. Virtually all the evidence developed over the past decade has suggested that dramatic economic inequalities exist in rural areas which sometimes parallel those found in the cities. Second, similar occupational structures tell us little about economic differentials or inequalities. Third, could not some fair measure of the English-French differences derive from distinctions in wealth and available resources, as from those of religion and mentality? Greer forces the reader to challenge his interpretation, when he points out that in two English Catholic communities (of three) literacy levels were much closer to English Protestant levels than to French Catholic ones (though these two were rejected from the main sample: (p. 318). The point is not that one must choose between economic and religious factors; understanding their relative contributions and, importantly, their interactions may well tell us more about the social and cultural relations of literacy and intergroup differences than either factor alone. Greer provides no reason at all for considering religion more critical than socio-economic and material forces. Although I cannot now muster the necessary data, what I do know about the social structure of Quebec in the eighteenth and early nineteenth centuries points to the importance of greater structural and ethnic differentiation and inequalities than Greer's comments allow. Much of the Franco-Anglophone variation in literacy rates might well be reduced if wealth and other economic data were introduced into the discussion and controls developed for their influences. The perils of studying literacy in isolation from closely interrelated ("multicollinear") factors are as dramatically apparent here as those of monocausal interpretations.¹¹

Local initiative and selective migration are other factors which critically influence levels of literacy. Their impact is usually no more independent or isolated than the others. Greer addresses migration usefully,¹² and he mentions local variations (which he again attributes to religious differences: "religion was at the root of this concern:" p. 333). Here it would be useful to consult M. J. Maynes' excellent dissertation on French and German education in a period which overlaps with much of Greer's 1750-1850. She shows that a great deal of the difference in educational opportunity and in rates of literacy between France and Germany derived from the latter's system of school finance, which included allotments of land and payments in kind to teachers, rather than to religious differences. Her German region (Baden) was more closely supervised by state and regional authorities than her French area (Vaucluse); this in combination with differing modes of finance accounted for much of the difference. Educational opportunities at the primary or literacy levels were lower and more highly stratified in the more localized situation, in which local differences in resources which might be spent on schooling were more im-

¹¹ Lawrence STONE, "Literacy and Education in England, 1640-1900," *Past and Present*, No. 43 (1969): 61-139, presents a roster of key variables which no student of literacy should ignore.

¹² See GRAFF, *The Literacy Myth*, ch. 1, and the references cited therein.

portant.¹³ The Quebec circumstances can be usefully interpreted in this manner; religion can then be placed in a meaningful context. Greer's own comments (p. 333) reinforce the point, although Protestants in many places showed great concern about types of schooling and control as well.

The four sources exploited by Greer and his analysis of them comprise the final major area for attention in this comment: parish registers, petitions, the manuscript responses to the inquiries of the Buller Commission, and the censuses of the second half of the nineteenth century, especially that of 1891. Overall, his description of the primary sources and particularly of their weaknesses is fair and instructive. His use of them — to establish patterns and differentials over the span of one-and-one-half centuries — and the inferences made from their data are less than convincing and occasionally contradictory and misleading.

The most general issue for Greer's own purposes and for the reliability of his results for readers rests on the comparability of these four different and distinct measures of literacy. The first complication addressed above, that of the lack of systematic and consistent definition of literacy and of the levels thereof, returns to haunt the attempt to erect patterns of serial data on literacy rates over a lengthy span of time. How are we to compare, for example, signers and markers of parish registers and petitions with responses to census questions or to the "slow and disappointing" questionnaires of local ("knowledgeable") informants to a government-sponsored survey, to which at least one bishop objected? Each source provides one kind of measure of literacy, but the measures are simply not the same. For example, if Schofield and others are correct, readers may outnumber signers by a ratio of about 3:2, while the number of signers understates the number of writers. These weights to be sure are hardly precise or certain. The number of signers does not correspond to either that of readers or of writers. In addition, such categories as "semi-literates" (an ahistorical category if my own analysis is correct) or "read at least" do not correspond to signature-marker data; they are also very difficult analytically. Base populations differ, as do tests and qualities of directness of the measures.

The result of the use of these four different measures is a proliferation of categories and abstract, presumed levels of literacy; the issue of their comparability is highly at risk. At best, their information can only be compared loosely and flexibly, *but only* if the nature and meaning of their data are explicitly interpreted, the varying measures compared and assessed, the biases and distortions admitted and controlled as well as possible.¹⁴ At worst comparison leads to misleading conclusions and distortions. Greer by no means hides these problems and complications. The thrust of his essay and the manner of interpretation reduces their significance and presents serial data without confronting these issues as directly as he might. For example, he could compare two or more

¹³ MAYNES, *op. cit.*

¹⁴ GRAFF, "What the 1861 Census can tell us about Literacy"; SCHOFIELD, *op. cit.*; Lockridge, *op. cit.*, expand on these points.

measures for one place at one time or for individuals. Before continuing, I wish to make it clear that each of these sources has some value for the student of historical literacy; the questions raised here derive rather from the divergence in their measures and the uses to which they are put. Let us consider them briefly.

The first source discussed in the article is parish marriage registers, which offer a measure of literacy by an individual's ability to sign his or her name. As noted above, this signifies to most interpreters an ability between that of reading and that of writing. Their reliability depends on several factors, including that of the proportion never marrying and of their coverage of different religious and ethnic groups, which Greer does not stress as much as others. I suspect generally that his claims (p. 299) for them are fair; others might well follow this lead in more intensive studies of localities, regions, and selected samples based in this source.

The sample of four parishes unfortunately is too limited and small for conclusions to be made. In addition the numbers in each sample are rather tiny for the eighteenth century and no evidence is presented for the seventeenth century or the first half or the eighteenth, despite their relevance for Greer's initial questions. Overall the statements made from their evidence are interesting especially in light of his larger purposes. First, we are told that "the signature rates are, in all cases, quite low" (p. 300); the standards or explicit comparisons which justify this conclusion are not presented. There were in fact parts of Western (including France), Eastern and Southern Europe with rates of literacy no higher in corresponding periods. Second, Greer claims improvement in rates throughout the period, although there is no evidence of change upwards or downwards in three of the four cases for the eighteenth century. The evidence does not speak to his concerns about the educational consequences of the conquest. Third is the issue of female literacy, one of the subthemes of the entire essay. At this early juncture, Greer is content with higher rates of bridal literacy than for grooms, while he later contradicts his stance when considering the evidence of the Buller Commission and the censuses. The mode and manner of inference-making begun here remains throughout the article. The register data and the measure of literacy they present are never evaluated explicitly in reference to non-signatory sources.

Petitions form the second source; their measure, as the registers's, is the ability to sign one's name. Among the several sources used for literacy studies, petitions have engendered perhaps the most controversy. Their interpretation has been debated especially by the English scholars, Stone, Schofield, and Cressy.¹⁵ Of this and of the need to consider the context, type, and purpose of each petition in order to understand its evidence and coverage Greer seems largely unaware. Petitions are highly

¹⁵ See, for example, the exchange between Lawrence STONE and David CRESSY, *Journal of Interdisciplinary History*, 8 (1978): 799-801, as well as CRESSY's "Education and Literacy in London and East Anglia, 1580-1700," (unpubl. Ph.D. thesis, University of Cambridge, 1972) and his articles in *Societas*, 4 (1974): 229-40; *The Historical Journal* 20 (1977): 1-23; *Journal of Interdisciplinary History*, 8 (1977): 141-50.

selective sources, as he acknowledges whether by status, ideology, or other beliefs. If they reflect divisions within a community, rather than consensus, the sorts of assumptions (pp. 301-02) Greer offers in their defence will not be met. They must be handled with great care and caution. They are also the source most susceptible to fraudulent signatures.

Their data can be quite revealing nevertheless, as Table 3 shows (p. 303). Most interesting here is the pattern of occupational stratification by literacy and ethnicity. Almost seven times as many Francophones as Anglophones participated in this petition of 1840, but there were nearly as many English as French merchants. Francophone labourers were over-represented by a factor of 19:1, although the 50 percent of English whose occupations were "unspecified" limits these remarks. Ethnic occupational distinctions cannot be understood apart from data on wealth variations within ranks. The literacy rate of the French Canadian males from this source, 20.8 percent, is about 70 percent of that from parish marriage registers, 28.2 percent, while that of English and French combined is virtually equal. Overall these data support the kinds of interpretations which take socio-economic factors seriously into account. Both the rural-urban differences and the occupational variations underline the insufficiency of religio-cultural explanation in isolation from economic differentiation and stratification.¹⁶

The manuscript returns to the inquiry of the Buller Commission are the third source consulted. Although these documents receive the most attention, they are the most limited in chronological scope and the least reliable of the four which are analyzed. Greer gives the reader several reasons to question their accuracy, but few to confirm their utility. The data apparently were not considered satisfactory by those who sought it; its coverage was uneven; many were incompletely or carelessly presented, Greer informs us. The categories, as usual, are vague and undefined; their comparative value is doubtful. Nor do we know how these "very raw data" were compiled or in what ways the observers were "knowledgeable." I find no reason to presume their evidence superior to that from censuses (p. 309).¹⁷ Without any additional confirming evidence these returns must be taken as very indirect and quite questionable in their assessments of popular levels of literacy and in differentiating readers from those "who can read and write sufficiently well for all ordinary purposes," (p. 309). The latter may well be a rather low level of skills, if my own interpretation is in any way correct.¹⁸

Let us assume, for the purposes of argumentation, that these data are more trustworthy. They must first be transformed into rates of literacy, which Greer attempts. His estimates are less than persuasive. Though admitting "great local variation resulting from different levels of fertility and migration" but neglecting those from mortality and age structure, he

¹⁶ Both contextually and interpretively, I must take exception to Greer's characterizations of areas low in rates of literacy as "ignorant;" see pp. 307, 315.

¹⁷ See GRAFF, "What the 1861 Census can tell us about Literacy."

¹⁸ GRAFF, *The Literacy Myth*, ch. 7.

derives age-specific proportions from the 1844 census, taken five years after the Commission information was compiled (pp. 309-11). He apparently does not consider interpolating proportions from the differences between the censuses of 1831 and 1844 to allow for changing configurations of mortality, migration, age-sex structure or other checks. Rejecting some communities' returns on "admittedly arbitrary" criteria (apparently without checking census distributions for verifications), he discovers that for all of Lower Canada the percentage of the population aged under five years is 27 percent and for those five to fifteen years in the reporting communities is 19.24 percent, according to the Commission's informants. These figures suggest that the size of an age group spanning five years is over 70 percent of that of a group encompassing twice as many years. Unless there are extraordinarily high rates of child mortality over the age of five and massive migration under the age of fifteen these proportions are questionable. I suspect that either or both of two complications account for this problem which distorts his estimates. It seems likely that he five-year-olds may be double-counted, with age-heaping taking its effects. More importantly, this points to possible errors in the number of children five to fifteen, as tabulated by the local respondents to the Commission. If this is the case, those totals may not be trustworthy nor may other local tabulations. These reports may not then "constitute a reasonably accurate census" (p. 310). It is also unclear what is meant by "falsification" of statistics and "normal" age-sex structures in this context. The distinctions between his Groups A and B are similarly unclear.

Greer's major test of the reliability of these literacy rates comes from comparing them with signatures (from *either* marriage registers or petitions) for six (of 50) communities in which available sources overlap. The comparisons are very interesting (Figure 2, p. 312). He is quite correct that "the results are inconclusive"; for different levels and skills are measured, directly in one case, indirectly in the other, by the measures. Comparison of the Commission data with marriage register signatures (presumably the more reliable source) reveals percentages signing in excess (by 7 and 8 percent: male and female) of the percentage of readers and writers, while petition evidence of signers varies from three to 27 percent in excess of readers and writers. In all cases there are more signers than writers, but in four cases, the petition signers even exceed the number of readers. This pattern of results will *not* "confirm the value of the Buller literacy rates" (p. 313), as Greer asserts; his first inference is more apt. The Buller Commission's data are interesting, but their absolute and comparative value remains to be determined. If his comments on their geographic representativeness are well-taken, they cannot be used to estimate province-wide patterns (pp. 314-15).

The gender differences in literacy are also noteworthy. The female dominance in fourteen places and relatively slight difference overall are important, and they are not as exceptional or unique as Greer indicates (pp. 317-18, 331-32). First, I found similar patterns in rural Ontario; second, Egil Johansson's pioneering Swedish researches have stressed the importance of female literacy, especially in contexts in which home and

church (and *not* formal schools) are primary agencies for the transmission of literacy.¹⁹ Female literacy could be quite significant and highly valued. In circumstances in which literacy was not regularly demanded for daily usage, a woman's skills could well suffice for economic, social, and cultural requirements. The implicit issues raised require direct and sustained attention. Finally the determinants of school attendance (*and* their relationship to literacy) are much more complex than Greer's discussion (321-23) reveals. Parental motivations cannot be deduced directly from rates of attendance, nor can motivations be interpreted apart from the opportunities and constraints within which they function.²⁰

The major criticism should now be clear.²¹ Though comment has focused necessarily upon work of one scholar, whose research is prodigious and intelligent, I have attempted throughout to speak to issues which transcend this one essay, and not to denigrate Allan Greer's original efforts in the history of literacy in Quebec. Literacy, past or present, is a stubborn and resistant subject; its history is a complex one, its parameters difficult to unravel, and its meanings obscure. Progress comes only with intensive research, clear and consistent conceptualization, and due attention to definitions. Literacy can only be understood when its meaning(s) has (have) been interpreted, in a manner appropriate and fitting the context; it cannot be measured sensitively or reliably without its meanings clarified. In other words, epistemologically and methodologically, measurement, explanation, and interpretation are neither independent from one another nor logically separate aspects of a research strategy. They are dialectically related; that is the way in which historiography advances.

¹⁹ GRAFF, "Elgin County," and "Literacy and Social Structure in the Nineteenth-Century City" (unpubl. diss., University of Toronto, 1975), pp. 515-18; JOHANSSON, *op. cit.* See also, Greer's remarks on school attendance, pp. 321-23.

²⁰ For detailed and important discussions, see Ian DAVEY, "Educational Reform and the Working Class: School Attendance in Hamilton, Ontario, 1851-1891" (unpubl. Ph.D. diss., University of Toronto, 1975); MAYNES, *op. cit.*; Carl KAESTLE and Maris A. VINOVSIS, *Education and Social Change in Nineteenth-Century Massachusetts* (Cambridge, 1979).

²¹ I will not extend this comment with a discussion of the census as a source for historical studies of literacy; this I have done previously, as the notes indicated. I wish to add that the 1891 Census aggregates cannot be brought to bear on the major issues which Greer addresses. First, he cannot differentiate urban from rural, Francophone from Anglophone or Irish, or Protestant from Catholic. Second, census categories and channels for obtaining their information vary from census to census complicating the issues. Finally, although I have used this technique myself, I grow increasingly wary about static, cross-sectional cohort especially over long periods of time; I refer readers to Robert WELLS, "On the Dangers of Constructing Artificial Cohorts in Times of Rapid Social Change," *Journal of Interdisciplinary History*, 9 (1978): 103-10. Note the literacy rates of Greer's oldest cohorts (Table 10; p. 327); they are much higher than those indicated by his more contemporary examinations. This may result from either the variant measures or from mortality differentials. The mechanisms and engines of the changes in literacy patterns, in sum, remain obscure.