

## OF NUMBERS, HISTORY, AND OTHER THINGS

Any scholar who has followed the ongoing debates in the profession over the use and misuse of quantitative data for the study of historical phenomena is well aware of the perils inherent in any attempt to use numbers to help explain the past. The controversy surrounding Robert W. Fogel and Stanley L. Engerman's *Time on the Cross* is only the most visible manifestation of the continuing battle over the limits, usefulness, and standards of quantitative history. Closer to home, in the pages of this journal, we have witnessed the exchange between Wilkie, Smith, and Skidmore over the proper way to analyze and interpret Mexican budgetary accounts.

Having observed these encounters with the impartial enthusiasm of a spectator, evaluating arguments and assessing import, it is only just that I too, having published a book of numbers, should bear the wrath of the better trained. At times in the preparation of *People and Places*, when struggling to resolve the dilemmas so truculently underlined in McCaa's review, I would marvel at the folly of trying to construct a reasonable argument out of all those numbers. But then, encouraged by the belief that only through risking a little can we make any progress at all, I would regain my enthusiasm and make the decisions required for the work to proceed. As McCaa indicates, he thinks I made all the wrong decisions. And as an announced demographic historian, maybe he ought to know.

But from my perspective, there seems reasonable justification for making the case I do in *People and Places*. Many of the criticisms, to be sure, are about matters of preference. For example, my tables, it appears, are not extensive enough. This is, of course, true. And in the best of all possible worlds, I should have published every scrap of data available in every possible combination of tables, arranged to suit every scholarly need. Unfortunately, my publisher thought the material I did publish was far too extensive, and several scholars advised leaving all the tables out to protect the data for my private use. In the end, the selection and format of tables to be included were determined by my perception of the utility of the volume. They are not there to help quantifiers construct elaborate indices but for historians looking for information on parishes in the Bishopric of Caracas. And in the second volume of this ongoing inquiry, there will be space for the publication of data not tabulated in this volume. Whatever the deficiencies of the table presentation, it is important to reiterate what is said in the book: the data will be made available to anyone who wants them in machine readable form, thereby permitting any permutation or combination of the file imaginable. The original documents are also available on microfilm for those who think something significant is missing from the published or machine readable data.

It appears that the most serious failure of this book, at least from McCaa's point of view, is that I use less than perfect data to try to talk about the Bishopric of Caracas at the close of the colonial period. Well, I plead guilty. But it should come as no surprise to any reader that the data have their problems, since I spend an entire chapter discussing the likely sources of error, the methods of census taking, the procedures for updating, and the likelihood of fabricated results. And it is true that I did not repeat these caveats at every point throughout the book, under the assumption that my readers could carry over the qualifications about the data in general to the discussions about the population in specific. In this I appear to have been in error. So let me summarize briefly what I say at length in the book. The data are not perfect. The priests undoubtedly filed returns they had not updated by a headcount; but they are the best we have for that period, they are part of a headcount census system, and they provide results that help us understand much about Venezuela's past.

The difficulty with the perspective displayed in this review is that it is too narrow, too cautiously technical, and too unwilling to permit the forwarding of hypotheses. The main point of *People and Places* is to provide hypotheses about the organization and structure of Venezuela's population at the close of the colonial period. The book makes no elaborate demographic propositions, it offers no modification of formal demographic canons. Rather, it simply explains where the people of the Bishopric of Caracas lived, what proportion, more or less, of the people lived in hamlets, villages, towns, and cities, whether there appeared to have been any differences in the distribution of races and sexes among the regions and population centers of the bishopric, and what hypotheses might help explain the conditions outlined. The rationale of this book is less that of formal demography and more that of a *Relación Geográfica*. *People and Places* uses methods scaled to the quality of the data, and its scope is determined by the coverage of that information.

The proposal that because the parish priests of the Bishopric of Caracas failed to present totally accurate returns, that because some priests must have falsified the data, and that because there are gaps in the series we must wait for micro-level parish register studies before hypothesizing about Venezuela's population distribution and composition is unacceptable. As a result of working with the imperfect returns from the Bishopric of Caracas we now know better than before what the range of population centers at the end of the colonial period was, where the various racial groups clustered, what the urban network of Venezuela must have looked like, and what some of the forces that created this population landscape might have been. When more detailed information becomes available from some of the projects in progress at the present time, we may well find cause to modify the broad picture of the Bishopric of Caracas presented in *People and Places*. But to say that we cannot begin with the annual parish returns from the Bishopric of Caracas because they have some defects is a position without merit.

What may complicate the issue is the fact that *People and Places* makes no claim to universal validity, it holds out no statistical nirvana, it promises to resolve no immortal questions. Instead, it proposes to reconstruct, using imper-

fect quantitative data and a wide range of other sources, the map of late colonial Venezuela in the Bishopric of Caracas. It does for the 1800–1810 period what Codazzi tried to do for the early republican period and what Bishop Mariano Martí tried to do for the late 1770s. And it proceeds with the knowledge that the result will be an approximation—closer than earlier estimates, more detailed than other discussions, and more explicitly developed than most previous analyses—but nevertheless an approximation.

A serious critique would focus on the way in which *People and Places* views late colonial towns and cities; on the hypotheses of racial and sexual distribution; on the identification, location, and classification of parishes; and on the structure of the Bishopric's urban network. Because the materials from Venezuela's parishes have not yet been fully exploited, and because the micro-level data permitting age and sex analysis have yet to be analyzed, discussions of Venezuela's late colonial population in full, formal demographic detail are still premature. But that does not mean that we must refrain from constructing general hypotheses out of the material available to us, especially when that effort is preceded by elaborate, detailed discussions of the likely sources of error in the data and when the assumptions and the data from which these hypotheses are constructed are fully displayed. These, then, are the major questions that ought to be discussed in a forum of this kind; but, in the interests of encouraging constructive criticism, let me respond to the minor technical points in the review.

McCaa appears especially unhappy about my use of a model life table and about the inclusion of some correlation coefficients. He thinks I have done my math wrong on the one and misused the summary statistics on the other. But as he so eloquently points out, the use of the model life table is fraught with dangers, not the least of which is determining which model life table corresponds most closely to reality. He makes a number of assumptions that give him what he regards as a better fit, although the rationale behind those assumptions is unclear. But whether you take his assumptions or mine, the results are hardly enough to warrant any important conclusions, as I point out in the text. The purpose of the life table exercise was simply to see what would happen if we tried out the census data against a theoretical model. The exercise showed mostly that the data could not be rejected on the basis of the life table model; it did not prove much else. Perhaps the exercise could have been omitted, but here, as elsewhere in the book, I tried to err on the side of inclusion rather than exclusion. The review indicates that my judgment was correct, for it provided an opportunity for a discussion of the difficulties of using model life tables for retrospective demographic analysis. As I point out, and as McCaa agrees, any argument based on the use of model life tables constructed in accordance with one set of fertility and mortality assumptions to approximate conditions in the real world that may differ greatly, is very tenuous indeed. His calculations, based on different assumptions than mine, produce slightly different results, as we would expect them to do, but these results contribute very little to the evaluation of the data displayed in the book.

The argument that unless the data are perfect or close to it we should avoid correlations and scatterplots does not convince me. After all, in *People and Places* the quality of the data is fully discussed, the data are there, the scatter-

plots are there, the regression lines are there, and the interpretations of the data are there, all discussed fully within the historical context of the time and place. This is done with careful attention to the limitations of both the data and the method. As a result of the procedure, any reasonably sophisticated historian can and should do what this reviewer has done—accept or reject the argument on the basis of all the evidence available to the author. And that, I think, justifies the method. If McCaa sees too many outliers to make the regression analysis convincing, fine; if he thinks the data are too flimsy to support any analysis at all, fine; if he prefers to study tables instead of scatterplots or graphs, fine. But the point here is that all the evidence I use is available to the reader; if my explanation does not convince, then let the critics construct a more convincing one. That, after all, is one of the better ways of making progress. But I find it a bit disingenuous to be criticized for including enough data to permit a reasoned critique.

In any case, the book has justified its primary mission, which is to present data and explanation in such a way that others may take the same material and rework it in accord with other perspectives and other assumptions. Such a procedure permitted McCaa to catch an error for me in the discussion of Caracas' role as a primate city. In the text I gave the city's population as about 24,000, inadvertently leaving out one of the city's parishes included in the tables. It should, of course, be about 30,000, a number that strengthens my hypothesis about the capital's primacy. If that is the only significant error in the volume, I am delighted.

Now that a demographic historian has given me the benefit of some technical advice, I hope other scholars will focus their critical insight on the description and analysis of the Bishopric of Caracas presented in *People and Places*. Are the hamlets, villages, towns, and cities reasonably defined, located, and analyzed? How can the hypotheses about the urban network and the analysis of residential preferences be tested or improved? In what ways can my understanding of the process of urban network formation be revised to reflect better the data and the nonquantitative information available? What other approach will permit a better understanding of the tremendous impact of the independence wars on Venezuelan population centers like San Carlos de Austria? Because this volume is, as I mention time and again in the text, a set of preliminary hypotheses about the material conditions of Venezuela's late colonial past, I am especially interested in receiving useful criticism that will permit the kind of refinement of method and hypothesis that comes from the best scholarly conversations.

Although the publication of monographs like *People and Places* is an invitation to the scorn of the specialist, it is, I think, the only way to begin. None of us controls such overwhelming expertise that criticism and constructive controversy are unnecessary. And from such criticism, I would hope, will come a history of late colonial Latin America based on better data, more skillfully handled, than the analyses available today.

JOHN V. LOMBARDI  
*Indiana University, Bloomington*