**Colonial Latin American Historical Review**

Volume 2
Issue 3 *Volume 2, Number 3 (Summer 1993)*

Article 3

6-1-1993

# Counting the Encounter: The Pernicious Appeal of Verisimilitude

David Henige

# Counting the Encounter: The Pernicious Appeal of Verisimilitude

DAVID HENIGE[1]

> *Where no authors are cited specifically for the information . . . I simply have made a reasonable guess in the absence of the relevant data.*—Michael B. Schiffer[2]

The encounter that began in 1492 has been a protracted one, generating numberless issues that have resisted resolution even half a millennium later. Both the heat and the light cast on these issues are certain to be more abundant in the 1990s than in any of the previous fifty decades, except perhaps the very first. In no small part this is due to the interest taken by scholars in other fields than history and geography, as the very concept of encounter takes on a virtually transcendental glow. Among the more important new entrants are literary criticism, seeking new texts to conquer; biology, looking for new connections among biota; anthropology, attempting to undo the damage of its earlier years; and historical demography, seeking to compensate for centuries of aspersions on the populousness and sophistication of the American Indian societies. It is to the last of these that this essay is dedicated.

---

[1] The writer thanks Jim O'Neill for much needed and much appreciated tutelage with Lotus 1-2-3 in order to derive a set of population projection tables covering all years and rates between 1520 and 1570. Thanks also to Robert McCaa and Bruce Fetter for their help and encouragement, to William Denevan for alerting me to my propensity to generalize too easily, and to Sandra Guthrie for her usual well-tempered devil's advocacy. An earlier version of this paper was presented at the conference entitled "Discoveries: Meanings, Legitimations, Critiques," at the University of Wisconsin-Madison, September 1992, and the writer has benefited from comments offered there.

[2] Michael B. Schiffer, *Behavioral Archeology* (New York: Academic Press, 1976), 49.

In the abstract, the thrust of recent American Indian historical demography has been a good thing, as are many attempts at historical revisionism. After all, it is no exaggeration to claim that the vast majority of what was once routinely accepted as true in all areas of knowledge has since been found to lack the substance required for more sceptical succeeding generations to embrace. No interpretation of the past should be considered immune to relentless scrutiny and, if necessary, to overturning. So much for the principle; this paper will be concerned with practices engendered by the principle.

The notion that the Americas were largely and unprofitably occupied in 1492 has been commonplace for most of the last five centuries. Nonetheless, it was hardly the earliest impression left by the numerous discoveries that followed on the first. Convinced that he was on the threshold of China, Columbus tried to persuade himself and others that the islands he encountered and settled were exceedingly well peopled—did he not have Marco Polo's assurance on the matter? Many other early chronicles followed suit, or at least appeared to. Cortés, for example, invariably—at least in his own reports—faced huge enemy armies and of course always defeated them. So too the missionaries seemed, at least to judge from their own accounts, to have been remarkably successful in converting and baptizing Indians by the millions and thereby mobilizing their own huge armies for the faith.[3]

After a while, though, the sources changed color and began to speak about how many Indians there were not, rather than how many there were. By the middle of the sixteenth century one Spanish report after another bemoaned a decline in population due to disease, hunger, and mistreatment. Interestingly, these causes, although related in fact, tend to be separated in the sources. Official reports focus almost entirely on disease, mention hunger from time to time, and all but ignore mistreatment. Conversely, missionary accounts emphasized hunger and bad treatment, but often managed not to report on disease, at least until mid-century, when it could hardly be ignored.

---

[3] For the outlandish number of baptisms recorded in some missionary chronicles see Angel Rosenblat, *La población indígena y el mestizaje en América*, 2 vols. (Buenos Aires: Edit. Nova, 1954), 1:96-100; David Henige, "Their Numbers Become Thick: Native American Historical Demography as Expiation," in *The Invented Indian*, ed. James A. Clifton (New Brunswick, NJ: Transaction Publishers, 1990), 173-75.

This dichotomy is made almost grotesquely clear in the writings of Bartolomé de las Casas, particularly in his notorious *Brevísima relación de la destrucción de las Indias* which, when published in 1552, provoked international outrage against Spain, and Spanish outrage against Las Casas. In the *Brevísima relación* Las Casas offered a long series of regional depopulation figures. According to Las Casas, these losses totaled more than 12 million.[4] Significantly, in all his catalog of horrors, Las Casas himself never thought to mention epidemic diseases as a factor in this depopulation, nor did he *calculate* any hemispheric estimates as such, ignoring large parts of the Americas entirely.[5]

It was to be a long time before scholars began to turn seriously to the magnitude of depopulation, as well as its causes and effects. In fact this has been largely a twentieth-century phenomenon and a western hemispheric one at that. It developed from a growing interest on the part of anthropologists in the American Indian. No longer a military threat nor a serious impediment to untrammeled white settlement, the Indian could now serve as the subject or object of anthropological investigation. In this regard Indians were customarily seen as a primitive race, conveniently lodged on the doorstep, so that they could easily be studied and compared with other primitive races

[4] Bartolomé de las Casas, *Brevísima relación de la destrucción de las Indias*, ed. André Saint-Lu (Madrid: Catedra, 1982), 74.

[5] W. George Lovell, "'Heavy Shadows and Black Night': Disease and Depopulation in Colonial Spanish America," *Annals of the Association of American Geographers* 82 (1992):438n1, notices this and claims that "[i]t did not serve [Las Casas's] purpose to mention disease, for the cause he then served called for him to highlight...the undeniable atrocities [he] referred to." N.D. Cook makes a very similiar observation in "Disease and the Depopulation of Hispaniola, 1492-1518," *Colonial Latin American Review* 2 (1993):238-39. This is probably an essentially correct view of the matter, although one that comes only at great cost to the contemporary notion that Las Casas and other early chroniclers can be drawn on at will to support twentieth-century quantitative historiography. Certainly it confers no credit on the reliability of Las Casas's testimony, whether with respect to the atrocities—which are clearly exaggerated in presentation—or, more generally, on Las Casas's interpretative skill. See also note 74.

as handily interchangeable parts in the grand comparative *schemata* which anthropologists were busily constructing.[6]

There were, of course, many ways to measure and classify these new subjects, wherever in the world they might be located, but one of the most popular was the belief that their population densities had always been low. In India, the "Tribals" were distinguished from other groups by their comparatively small numbers, while in Australia the Aborigines complied nicely with the premise by scattering themselves thinly over large parts of the continent. In southern Africa the so-called Bushmen and Hottentots were thin enough on the ground to provide justificatory arguments for European influx into the area. In Canada the Inuit served this purpose, and in the United States and Latin America the American Indians.

It was only in the last case that serious efforts began to be made early in this century to project existing low populations back to the early periods of contact and conquest. Until the 1960s the most extensive effort to estimate the maximum historical populations of various Indian groups was that of A.L. Kroeber. Basing himself on his own work for California and that of James Mooney and others for the rest of the United States, Kroeber concluded that contact (the term was not, however, yet in vogue) populations had been low, perhaps in the neighborhood of one million for the United States and another seven to eight million for the rest of the hemisphere. Nor had they declined much, making it easy to judge contact population by present levels without heroic extrapolation. In fact, Kroeber warned against such exercises, urging "a minimum of the mechanical multiplications which are so likely to conduct to misleading and usually exaggerated results."[7] Most importantly, he operated under the principle that "other things being equal, a higher, richer, or more complex culture is a reasonable index of greater population density, within the same cultural area."[8] Kroeber's statement was less significant as a personal state-

---

[6] For a few examples of this see Don D. Fowler and Catherine S. Fowler, "The Uses of Natural Man in Natural History," in *Columbian Consequences 3: The Spanish Borderlands in Pan-American Perspective*, ed. David Hurst Thomas (Washington, D.C.: Smithsonian Institution Press, 1991), 37-71.

[7] A.L. Kroeber, "Native American Population," *American Anthropologist* 36 (1934):25.

[8] A.L. Kroeber, *Cultural and Natural Areas of Native North America* (Berkeley: University of California Press, 1939), 180.

ment, though he was certainly influential, than because it reflected so well the ethnographic tenor of his times.

The supposed close correlation between numbers and political sophistication did not spring entirely from anthropological work. Angel Rosenblat, no anthropologist, also professed to see a "parallelism" between population densities and cultural levels throughout the Americas as he went about compiling the most extended look to the time at contact and later population levels throughout the Americas.[9]

Eventually those advocating much higher numbers began to bolster their own position—which was considerably more self-consciously advanced and inevitably required a greater array of assumptions—partly by criticizing the motives of the lower counters.[10] Henry Dobyns began his manifesto on the subject by claiming that "[t]he idea that social scientists hold of the size of the aboriginal population of the Americas directly affects their interpretations of New World civilizations and cultures."[11] After decrying the use of this argument by the low counters, Dobyns proceeded to adopt it himself, but in service of much higher population estimates. When challenged, he backtracked by claiming that he "sought merely to suggest that the most accurate figures possible be employed. . ."[12] Nonetheless, he was to return to the motif again and again, particularly in his study of sixteenth-century Timucuan depopulation, where he unblushingly and repeatedly

---

[9] Rosenblat, *Población indígena,* 1:103.

[10] John D. Daniels, "The Indian Population of North America in 1492," *William and Mary Quarterly* 49 (1992):298-320, refers to this group as the "Top Down School." On other occasions the present writer has called them high counters, but this seems to raise hackles for some reason. At any rate, that term is avoided in this paper. But whatever the phrase used or avoided, because it would be so difficult to particularize the changing, but generally growing, numbers emanating from this work, the writer emphasizes that the term is used primarily to describe a state of mind rather than a range of numbers. Prominent exponents of the school include Woodrow Borah, Sherburne Cook, Henry Dobyns, and several others, some of whose work is discussed in the writer's "Native American Population at Contact: Discursive Strategies and Standards of Proof," *Latin American Population History Bulletin*, no. 21 (Fall 1992): 2-23.

[11] Henry F. Dobyns, "Estimating Aboriginal American Population: An Appraisal of Techniques with a New Hemispheric Estimate," *Current Anthropology* 7 (1966): 395.

[12] Ibid., 444.

correlated population density with social stratification and complex societies.[13]

Meanwhile Woodrow Borah took up the cudgels and indicted many of his predecessors by tying their low estimates to such motives as "exaltation of the European," and pointed out that he and others holding similar views supported "the existence of highly complex societies having at their disposal a huge surplus from small surpluses created individually by a horde of peasant families."[14] Others followed, and continue to follow suit, in correlating dense populations and complex societies.[15]

Even William Sanders, who is on record as disagreeing with both Borah's and Cook's population estimates for central Mexico and their methodological premises, joins in espousing a close, in fact a deterministic, relationship between density and complexity. "We can conclude," he writes, "that when a society increases in size over time and this expansion is ongoing, locally contained, and adequately supported, then that society *must* develop more complex features of subsistence, economic exchange, and political integration."[16] Although slightly transplanted and more categorically stated, this sentiment bears close resemblance to the sometimes implicit, but seldom absent, predicates of this school of thought in the matter.[17]

As it has become elaborated over the past several decades, this so-called Disease Model of American Indian historical demography is underlain by a fairly consistent set of premises. The most important of

---

[13] Dobyns, *Their Number Become Thinned: Native American Population Dynamics in Eastern North America* (Knoxville: University of Tennessee Press, 1983), 34-45, 131-32 and passim.

[14] Woodrow W. Borah, "The Historical Demography of Latin America: Sources, Tendencies, Controversies, Yields," in *Population and Economics*, ed. Paul Deprez (Winnipeg: University of Manitoba Press, 1970), 184-87.

[15] Daniels, "Indian Population," 317-18, discusses a few of these.

[16] Michael H. Logan and William T. Sanders, "The Model," in *The Valley of Mexico: Studies in Pre-Hispanic Ecology and Society,* ed. Eric R. Wolf (Albuquerque: University of New Mexico Press, 1976), 32, with emphasis added.

[17] In the early days of African studies, some scholars found it difficult to accept the notion that such densely-populated groups as the Ibo of southeastern Nigeria had what appeared to be "'stateless' societies," while more thinly-populated groups seemed to have developed more complex political structures. The dichotomy between "state" and "stateless" and "simple" and "complex" has gradually disappeared in this field, being seen as an unnecessarily simplistic and ethnocentric concept.

these are that, first, newly-introduced diseases from the Eurasian disease pool were by far the greatest contribution to American Indian depopulation; second, although the documentary evidence usually (and sometimes unavoidably) mentions these diseases as occurring only after the arrival of Europeans in a particular area, one may assume that they had spread to most areas well in advance of the arrival of these Europeans; third, accepting the early and widespread onset of these diseases virtually requires one to assume much larger precontact populations than would the absence of such a premise; and fourth, it is possible to estimate fairly reliably the magnitude of contact populations by applying various arithmetical formulae, in particular by devising depopulation ratios by working backwards from presumed nadir populations to presumed maximum populations—these latter invariably occurring on the eve of the arrival of the first European diseases in any particular area.

The argument of this paper is that the probability that each of these premises is true declines from virtually 100% in the first case to 0% in the fourth. Elsewhere the writer has discussed in more general terms the recent course of this debate; here he wishes only to concentrate on the implications of the fourth proposition for historical method.[18]

Much of this criticism concerning the ideological impulses of the low counters was and is correct, of course, but its impact is blunted when the critics hoist themselves with their own petards and turn the law-like low population=low complexity=low population reasoning loop on its head. In each case we see a facile correlation emerging from respective ideologies, which is then reified in the easiest way possible, by devising estimating procedures and associated assumptions best designed to present the particular case—in short, not only by enlisting numbers, but by deploying them. But if ideologies are reflected in the numbers mobilized, there are also ideologies latent in mobilizing the numbers. The rest of this essay is devoted to discussing how many of those estimating contact populations in the Americas rely on the graphic *display* of numbers to make their case when other means

[18] Henige, "Native American Population at Contact," 2-23, and other writings cited there.

fall short.[19] It concludes that efforts to demonstrate the fourth proposition noted above have—to put matter paradoxically—succeeded only in showing that they must fail.

William Playfair, the founder of modern graphic display, frequently commented on the ability of pictorial representation to guide and persuade users.[20] When the inchoate field of statistics began to develop early in the nineteenth century, its apologists waxed eloquent about how their field had already acquired "the dignity of a science," in no small measure, they held, because statisticians were able to present diverse materials in a "tabular form," which helped to foster an appearance of objectivity.[21] This lesson of course has hardly been lost on succeeding generations of quantitative historians, among them those interested in the contact population of the Americas.

The ultimate distillate of this increasingly quantitative approach is the depopulation table. This expedient at once denies and transcends other rhetorical strategies and is designed to seal their quantitative arguments. At the same time the depopulation table lays bare the irrelevance of problematic sources and obviates the need for the impedimentum of doubt. Whatever their individual variations, depopulation tables share common features. Each contains several columns, the first of which is a listing of epidemics (and sometimes famines) suspected for a particular area, together with their dates. Next comes the level of mortality allocated to each of these, sometimes expressed in ranges (e.g., 10%-30%, 25%-50%), sometimes expressed in more descriptive terms ("innumerable people died"), sometimes expressed geographically ("across [the] present United States").[22] The

---

[19] This lack of data—although not usually quite the same complete lack—is a problem elsewhere as well. For one important example see Tim G. Parkin, *Demography and Roman Society* (Baltimore: Johns Hopkins University Press, 1989), 58-91 and passim.

[20] William Playfair, *The Commercial and Political Atlas* (London J. Debrett, 1786).

[21] [William Cooke Taylor], "Objects and Advantages of Statistical Science," *Foreign Quarterly Review* 16 (1835):205-207.

[22] Suzanne Austin Alchon, *Native Society and Disease in Colonial Ecuador* (Cambridge: Cambridge University Press, 1991), 37; Michael K. Trimble, "Infectious Disease and the Northern Plains Horticulturists," in *Plains Indian Historical Demography and Health: Perspectives, Interpretations, and Critiques*, ed. Gregory R. Campbell (*Memoir* 23) *Plains Anthropologist*, 1989: 49.

last column in these tables is one of descending population, serving as a kind of function of the second column. The last figure in this last column is usually a so-called nadir figure, culled from a source dating, typically, to the seventeenth, eighteenth, or nineteenth centuries.

Although appearing last in such tables, these nadir figures are the progenitors of all that precedes them and serves as their point of departure, with the rest of the journey being engrossed by the number of epidemics multiplied by their purported mortality rates. The destination is the contact population figure which heads the last column. The degree of difference between this figure and the nadir population figure is directly and solely contingent on the number of epidemics and on the multiplier assigned to each. Generally speaking, the first figure in this column has recently tended to range from ten times to fifty times higher than the last figure.

This columnar form of argument tolerates no doubt, no ambiguity, no imprecision. Although a few question marks occasionally jar the serenity of these tables, particularly in terms of the specific identity of certain diseases, this is allowed to have no effect on the relentless arithmetic. In effect these question marks displace an entire universe of doubtful sources, doubtful diagnoses, and doubtful spread. Their purpose, however, is not to infect the final arithmetic with their own doubt, but to anesthetize the table by quarantining the uncertainty and thereby preventing it from spreading to the numbers themselves.

The depopulation table enjoys another, if perhaps unintended, benefit. Its streamlined design inhibits including such processes as population recovery between epidemics, since to include such complications would destroy the tables' effectively stark appearance and make murky the mathematics. Some, notably Thornton et al., have pointed this out.[23] Just by doing so, however, critics ineluctably subscribe to the ground rules set by the compilers of these tables; that is, they argue for modifying the tables rather than for dispensing with them entirely.

Besides eliminating population recovery, depopulation tables oversimplify mortality patterns by forcing disease to serve as synecdoche for every other form of mortality. Were such tables to be taken literally, then, one would be required to believe that no American

---

[23] Russell Thornton, Tim Miller, and Jonathan Warren, "American Indian Population Recovery Following Smallpox Epidemics," *American Anthropologist* 93 (1991): 28-45.

Indians died of old age, injuries and accidents, natural disasters, and the like. These, of course, the simplistic depopulation ratio technique cannot take into account, but attempts (one presumes) to factor them into the ratio itself in some untold and arbitrary fashion. Nonetheless, at their face, depopulation tables as used in this way inevitably account for every death entirely by disease, underscoring yet again their incapacity for synthesizing complex demographic processes.

About these tables' catalyst, the purportedly nadir figures, something further must be said. As the first step in an extended mathematical process, even slight differences in these result in much greater differences in the projected contact figures. The surest way to arrive at very high contact populations is simply to use the highest possible nadir figure. Henry Dobyns adopted this recourse when postulating a contact population of 18 million for North America, and was duly taken to task.[24] As a result he devised an appreciably higher depopulation ratio (i.e., more epidemics and higher mortalities) in its place.

Even those intent on choosing an accurate nadir figure, however, must find themselves constrained by the exiguity and ambiguity of the available evidence. The aims of such counts in the historical record were entirely expedient and the sources naturally reflect no attempt to identify any figures in them as "nadir," a modern contrivance. Nor was there any notion of an upward turn in the depopulation curve or, for that matter, of a depopulation curve at all. In all cases, actual nadir population would have been reached at some point between two recorded counts, both of which would be higher than actual, but undetectable, nadir. To what degree this is true would vary from case to case, but some proportionate deduction will normally be required before adopting any nadir figure. Such a deduction, say of 10%, while perhaps amounting to only a few thousand people at that point, would have a ten- to fifty-fold effect on contact population projections.[25]

---

[24] Harold E. Driver, "On the Population Nadir of Indians in the United States," *Current Anthropology* 9 (1968):330; Russell Thornton and Joan Marsh-Thornton, "Estimating Prehistoric American Indian Population Size for United States Area," *American Journal of Physical Anthropology* 55 (1981):47-53.

[25] And whether the hypothetical nadir occurred before or after the lowest recorded count would have implications, possibly serious, for posited depopulation rates as well.

Another formidable task in dealing with nadir population figures—and with all other population figures as well—is to know just who were being counted. In this century the census figures for American Indians have fluctuated wildly, primarily because the definition of the category has not remained constant. The more generous definition applied in the 1980 census, for instance, resulted in a manifold increase in the number of "Native Americans," a wholly artificial increase that has entered the historical record.[26] To what degree this elevated figure will be used and abused by future generations of historians can at the moment only be contemplated.

It would be presumptuous to believe that categories did not change through time, space, and circumstance during the colonial period, even though records to demonstrate this no longer exist—may never have existed. Just the same, when projecting into the unknown past, those using this approach overlook this and treat the extant estimates as if they were the very models of consistency, whereas seldom is anything known about the criteria defining the conditions of these estimates. This problem of inconsonance is only aggravated by the high degree of internal migration that characterized Spanish America during the colonial period.[27] In an era when there were no true censuses (that is, simultaneous universal counts) migration is a disquieting wild card.

The first two columns of the typical depopulation table—the type of disease and the intuited mortality—must be treated integrally. Retrospective diagnosis has proven a popular but inescapably protean exercise for virtually all those who have written on the history of disease. The classic case is the famous Plague of Athens (430-429 B.C.). Its appearance in the historical mainstream, classical Greece; its ultimate effect of the victory of militaristic autocracy (Sparta) at the expense of democracy (Athens); and the fact that one of historiography's heroes, Thucydides, described it so copiously, have all led to intense modern interest in the episode. Inevitably the resulting studies

---

[26] C. Matthew Snipp, "Who Are American Indians? Some Observations About the Perils and Pitfalls of Data for Race and Identity," *Population Research and Policy Review* 5 (1986):237-52; Russell Thornton, *The Cherokees: A Population History* (Lincoln: University of Nebraska Press, 1990), 178-203.

[27] See *Migration in Spanish Colonial America*, ed. David J. Robinson (Cambridge: Cambridge University Press, 1990).

have been based on Thucydides's description of the etiology, symptoms, and effects of the epidemic. Unfortunately, the trouble he took seems not to have helped modern students. Paradoxically, the very detail in his account has fostered a concomitant measure of disagreement. To date as many as ten modern diagnoses have been made, all professing to have captured Thucydides's essence, or at least his intended meaning.[28]

No account of disease among American Indians during the colonial period is nearly as detailed as that of Thucydides for Athens, but this has hardly deflected the diagnosticians from their task. Three features typically mark the historical record in these cases: sometimes a disease is mentioned by a name that is still in use today (e.g., *viruelas* or smallpox); occasionally a name is given (e.g., *modorra*) that has no identified modern equivalent; finally, on occasion a disease has entered the historical record only under its indigenous name (e.g., *matlazahuatl* in New Spain in 1545). For the earliest period (say before ca. 1540), two other patterns share the record. Sometimes a generic symptom or two (often just "fever") are mentioned. More often only terms like *enfermedad* (sickness or illness) or *peste* (plague and a lot of other things) are found in the sources.

The instances where the sources mention a disease by a still-existing name would appear to be the least problematic. The earliest known instance of this is the mention that smallpox (*viruelas*) struck Hispaniola and Cuba in 1518/19 and then spread to Mexico just in time to assist a beleaguered Cortés in overcoming Mexica resistance.[29] Because of their well-known symptoms, smallpox and plague are

---

[28] Two recent studies are Karl-Heinz Leven, "Thukydides und die 'Pest'in Athen," *Medizinhistorisches Journal* 26 (1991):128-60 (with full bibliography); and James Longrigg, "Epidemic, Ideas, and Classical Athenian Society" in *Epidemics and Ideas: Essays on the Historical Perception of Pestilence*, ed. Terence Ranger and Paul Slack (Cambridge: Cambridge University Press, 1992), 21-44. Both conclude, among other things, that no demonstrably successful diagnosis is possible. An even more vigorous argument against the practice is made in Lee T. Pearcy, "Diagnosis as Narrative in Ancient Literature," *American Journal of Philology* 113 (1992):595-616.

[29] However, a recent article argues, on epidemiological and text-critical grounds, that the epidemic of smallpox that is recorded for central Mexico in 1520 was at best of modest consequence: see Francis J. Brooks, "Revising the Conquest of Mexico: Smallpox, Sources, and Populations," *Journal of Interdisciplinary History* 24 (1993): 1-29.

probably the most likely to be identified correctly in the sources, which are the products of laymen, however observant or learned some of them were. The other crowd diseases attributed to the sixteenth century are less distinctive in their appearance. Even though modern medicine has been able to distinguish measles, typhus, typhoid, influenza, etc. from each other, the degree of differentation is more clinical than it is demotic.[30]

We must wonder, of course, how well the casual observers of the sixteenth century were able—or inclined—to appreciate symptomatic differences and to speak reliably on matters of disease identification. It is true that by this time there existed some published work on the diagnosis and treatment of particular diseases, but is it likely that observers in the New World would have had, or desired to have, access to this literature? For them there was less need to make these distinctions than there seems to be for us. It was often a matter between man and God, especially in the minds of the clerical chroniclers, our best sources for this early period. In the circumstances it becomes somewhat risky business for modern scholarship to accept at face value—and even more to infer—ascriptions in sixteenth-century texts.

This has been a cursory look at various ways in which tables and other graphic representations of data can serve as rhetoric, but in order to study more effectively the nature and the effects of such tables in support of the Disease Model, it makes sense to carry the analysis to a single work in order to catch a whiff of the cumulative effects the process can have. Virtually all studies that employ the Disease Model as a guiding premise include one or more depopulation tables. However, Noble David Cook's study of demographic collapse in early colonial Peru,[31] by far the most extended treatment of its subject, is also the most salient example of the use of such reasoning. The work stands as the culmination of several earlier, shorter, and more localized studies. It is certainly, as David Stannard has described it, "the most

[30] Several articles in *"Secret Judgments of God": Old World Disease in Colonial Spanish America*, ed. N.D. Cook and W. George Lovell (Norman: University of Oklahoma Press, 1991), raise this issue, in particular, Hanns J. Prem, "Disease Outbreaks in Central Mexico During the Sixteenth Century," ibid., 20-48.

[31] Noble David Cook, *Demographic Collapse: Indian Peru 1520-1620* (Cambridge: Cambridge University Press, 1981).

detailed and exhaustive of Peru's population histories to date."[32] As much hard-core quantitative history as narrative analysis, the work is laden with pyramid age-distribution charts and age-sex models, as well as the more conventional depopulation tables. In all there are fifty-three tables and charts, as well as another sixteen figures. Clearly Cook intended these to carry a heavy load in conveying his arguments persuasively. Many of the tables present information from the late sixteenth and early seventeenth centuries and/or deal with small locales in what is now Peru. The present study ignores these in favor of concentrating on the tabular presentation of data that Cook uses to establish and support his estimates for the precontact population of the area.

Before this, however, a quick look at the work's broader conclusions. Cook begins by noting that "[r]ecent estimates of the 1520 [*sic*] population of Peru have ranged from less than 3 million to approximately 32 million." Correctly arguing that the true number lies somewhere between these extremes, Cook adds that his hope is "to narrow the gap" by devising an estimate that takes advantage of "new evidence and methods developed in recent decades." As will be seen, his emphasis is very much on the latter. Cook concludes that he expects his own estimates to be "carefully scrutinized."[33] To date, however, his invitation has not been accepted by other American Indian historical demographers, who instead have endorsed both Cook's methodology and his results, without subjecting them to much analysis. While this perhaps is only to be expected, even outside their circles his results are seen as corroborating and reinforcing similar approaches and conclusions for Hispaniola, Mexico, and elsewhere.[34]

Cook devotes roughly the first half of *Demographic Collapse* to discussing various ways by which unknown population levels are determined from known population levels—in other words, to various extrapolative techniques. Among these are archeology and paleodemography, depopulation ratios (which he sagely discounts as giving us "the results we expect"), "estimates from social organization," disease

---

[32] David E. Stannard, *American Holocaust: Columbus and the Conquest of the New World* (New York: Oxford University Press, 1992), 44.

[33] Cook, *Demographic Collapse*, 13.

[34] E.g., Benjamin Keen, "U.S. Writings on Colonial Spanish America," *Hispanic American Historical Review* 65 (1985):671n.

mortality models, and finally "census projections."[35] The last are the converse of depopulation ratios in that they are projected from later, more accurate, counts onto earlier populations, normally by using standard projection formulas in which certain assumptions are made numerical and then plugged in. While Cook goes in turn through each of these techniques in his exercise, in the end only the last two play a consequential role in devising his estimates. He concludes that in 1520 there were most probably about nine million Indians in the area now called Peru.[36]

Throughout the process he is careful to discuss the possible effects of various assumptions, particularly those regarding the impact of epidemic disease in the area. For this in particular he establishes two timelines—the first extending fifty years back from 1570, the second extending another fifty years forward from the same date. Read as *an assemblage of words,* Cook's arguments are generally put forward modestly and almost never in isolation. Several of his tables, however, afford quite a different picture. In looking at these, the present writer will have little to say about the probable reliability of any of the numbers in them, whether the backward projections or the counts on which they are based. This is not necessarily because he views them as reliable, although some of them may well be, but because, for the present, reliability is less a concern than method.

On looking at Cook's tables in aggregate, several simple arithmetical errors emerge, usually in subtraction and addition.[37] Sometimes these are minuscule, sometimes more important, and in some cases they are ramified by ensuing calculations. Two errors in Table 12 result in an error of about 3% in the postulated net nadir figure that concludes the column.[38] While this might be insignificant since the figures themselves are both inferences and projections, it is cause for concern when the stakes are higher and when behind-the-scenes calculations are not available for perusal.[39]

---

[35] Cook, *Demographic Collapse*, 54.

[36] Ibid., 108-14.

[37] E.g., ibid., 50, 66, and 222, in addition to the cases mentioned below.

[38] Ibid., 70.

[39] And even this has the effect of increasing Cook's projected population from 8,090,421 to 8,393,813. This is a change of about 5% from an error of 3%—another illustration of the exponential effect discussed below.

To simplify matters, let us look only at three of Cook's tables. The first of these is Table 10, "Peruvian epidemics, 1524-1635."[40] Here he lists twenty-three epidemics for the period in question, but the list is padded in several ways. First, epidemics in areas outside Cook's self-imposed limits (i.e., the area "delineated by Peru's contemporary boundaries") are included—Potosí (several times), Popayán, and Quito.[41] Then too he occasionally treats different accounts of the same epidemic as separate episodes. For instance, he lists two epidemics for Potosí, one for 1560 and the other for 1560-61, calling the first *mortifera* and the second one *peste*. He gives different sources for each, but one of these is simply a twentieth-century account quoting the same eighteenth-century source Cook cites for the other purported incident.[42] Later in the table he lists an epidemic in Quito in 1588-89, immediately after having subsumed it in a portmanteau entry covering the years 1585 to 1591. As a result, this table is considerably longer than it should be.[43]

The second table in question is the aforesaid Table 12, with the title of "Possible population loss due to epidemics, 1524-1619, with 'maximum' and 'minimum' mortalities (base population of 1,000)."[44] In company with many others who have attempted to measure epidemic mortality in the Americas, Cook believes that this "can usually be predicted on the basis of the known experiences of past populations."[45] As he notes, this table stems from three "premises:" that "the vast majority" of Indians died from disease, that mortality levels for these diseases can be determined "with relative accuracy," and that there was little population recovery between disease episodes.[46] Each of these suppositions is necessarily speculative and they are very unlikely all to be true. Cook's Table 12 is as follows:

---

[40] Cook, *Demographic Collapse*, 60-61.

[41] Ibid., 116.

[42] Cf. José Toribio Polo, "Apuntes sobre las epidemias en el Peru," *Revista Histórica* 5 (1913):58, and Bartolomé Martínez y Vela, *Anales de la Villa Imperial de Potosí* (La Paz, Bolivia: Artistica, 1939), 23-24.

[43] Cook needlessly complicates matters by quoting and citing early sources and modern studies indiscriminately in his "Mortality, comments" column.

[44] Cook, *Demographic Collapse*, 70.

[45] Ibid., 6.

[46] Ibid., 69-71.

| | | MAXIMUM MODEL | | | MINIMUM MODEL | | |
|---|---|---|---|---|---|---|---|
| Epidemic | Dates | Rate | Deaths | New Pop. | Rate | Deaths | New Pop. |
| smallpox | 1524-26 | 50 | 500 | 500 | 33 | 333 | 667 |
| smallpox/measles | 1530-32 | 30 | 150 | 350 | 25 | 167 | 500 |
| plague or typhus | 1546 | 20 | 70 | 280 | 20 | 100 | 400 |
| influenza/smallpox | 1558-60 | 20 | 56 | 234 | 18 | 72 | 328 |
| smallpox/measles/etc. | 1585-91 | 60 | 140 | 94 | 30 | 98 | 230 |
| diphtheria | 1614-15 | 10 | 9 | 83 | 10 | 23 | 207 |
| | | | 83:1,000 | | | 207:1,000 | |

(courtesy of Cambridge University Press)

The result is a classic epidemic *cum* depopulation table, in which various Old World diseases are listed in such a way that, the longer the list, the greater the depopulation.[47] Since Table 12 is central to supplying the lineaments of Cook's disease mortality model, as well as being one of the legs supporting his general conclusions, it is well to look at it closely to see whether it is as unexceptionable as it first appears.

Even though Cook elsewhere speaks of the likelihood of "rapid recuperation" from disease among affected populations under certain conditions, in this table such considerations have been sacrificed to the mathematical straight line.[48] As is evident, he does not allow for any retrenchment or growth whatever between epidemics, but offers a scenario in which population levels remain fixed from the end of one occurrence of disease to the beginning of the next. Thus, between 1561 and 1585, the hypothetical remnant population of 234(224)/1,000 remains constant, only to fall to 94(90)/1,000 between 1585 and 1591. For the next twenty-three years this number remains fixed in its turn, finally to drop to 83(81)/1,000 in 1614/15.

[47] Ibid., 62. Cook notes the problem, mentioned above, in identifying diseases on the basis of the symptoms mentioned in the records, but is impressed by "the general agreement" as to which diseases struck Peru in the sixteenth century. Actually, though, this agreement is just that—several modern studies agreeing with each other rather than arriving at the same conclusions independently.

[48] Ibid., 71.

While the particular figures in the accompanying "maximum model" are of course different, the principle of no population change between epidemics remains firmly in place. As Thornton et al. have pointed out, such a notion, while easy on the mathematics, is a bit hard on the empirical data.[49] More likely, there was some population recovery during these intervals. Less likely, but still more so than absolute stasis, is that there continued to be net losses between epidemics for reasons only remotely connected with them, if at all. Thus Cook's hypothesized population declines of 91.7% and 79.3% respectively are phantoms, both on arithmetical and on epidemiological grounds.

The difference (207/1,000-83/1,000=124/1,000) between the minimum and maximum models in Table 12 is the cumulated sum of the ranges Cook provides for the six disease episodes. In their turn these are based on "comparable" examples from times and places that are better documented. He is correct in trying to provide a *range* of figures here (although there is no range in two cases and only a slight range in a third), but it is dangerous to seek the parameters for such ranges elsewhere, a point implied in his text, but inevitably missing from Table 12, where the very use of the terms "minimum" and "maximum" leads readers to conclude that no other possibilities exist beyond the ranges' self-defined limits. To drive the point home, Cook reminds readers that in the "epidemic mortality model [i.e., Table 12] the range of total population is narrowed considerably" compared with archeological or ecological approaches, which of course allow no estimates at all beyond vague orders of magnitude. In fact, though, in tabular form, and presented and labeled as they are, these extremes circumscribe as well as narrow the choices.

On the basis of extrapolating backwards from an estimated population of 671,505 in 1620, the range for 1520, based on Cook's chosen ratios, is between 3.2 million and 8.1 million.[50] He leaves the matter there temporarily, since there were not enough epidemics to proceed, even by rejecting any interepidemic population recovery, without bringing other models into play. He is later forced to abandon his evident and appropriate inclination for mid-range figures and argue on behalf of the highest possible figure his disease mortality model can

[49] Thornton, Miller, and Warren, "American Indian Population Recovery," 28-45.
[50] Cook, *Demographic Collapse*, 71.

tolerate. The reasons for this become apparent on turning to his chapter on census projection models.

First, though, note that Cook's opening date, both in Table 12 itself and its caption, is 1524. By treating recorded and inferred epidemics as episodes, that is, *discontinuous* agents of depopulation, he eliminates any qualitative difference between 1524 and any earlier date in this respect. That is, using any particular starting date has no consequences unless he were to posit an epidemic before 1524. In his next chapter, however, where his census projection model presupposes *continuous* year-by-year decline rather than intermittent depopulation, he abandons 1524 in favor of 1520. At first glance this change seems unimportant, even trivializing. In fact it is no exaggeration to describe this superficially minor change as methodologically momentous.

Cook expresses a high opinion of the capabilities of census projection models to "provide one of the most promising avenues of approach to answering the problem of the size of the aboriginal population" of Peru, and in the present instance he unwittingly shows how effective it can be.[51] In previous estimates of the contact population of Peru in whole or part, he chose 1530 as the baseline for initiating depopulation.[52] In *Demographic Collapse* he neither alludes to his former practice nor explains his departure from it, a departure that is routinely employed throughout.[53]

Although the present writer is interested in one particular table (Table 19) in this chapter, it must first be pointed out that this table encapsulates only one of five possible models that Cook adumbrates along the way.[54] Each model is scrupulously laid out and each is accompanied by a synthesizing table, and the model discussed here is not in fact that which he seems to favor most. Notwithstanding this, there is good reason for concentrating on this particular table, although it would be unfair to do so without also mentioning briefly Cook's other models.

---

[51] Ibid., 109.

[52] Noble David Cook, "Estimación sobre la población del Peru en el momento de la conquista," *Histórica* 1 (1977):37-60; also see his *The People of the Colca Valley: a Population Study* (Boulder, CO: Westview Press, 1982).

[53] E.g., Cook, *Demographic Collapse*, 113, where "the actual 1520 population" of Peru is mentioned.

[54] Cook, *Demographic Collapse*, 93-107.

First (Model 1) Cook takes the "unadjusted" 1570 population of 1,290,718 and projects a population decline from 1520 using the same regional rates of change that he has extrapolated from the 1570-1620 period. This furnishes a population of some 3.3 million for 1520. Again, using the 1570-1620 rates of change (in Cook's Model 5a), but this time adjusting the 1570 population upwards to 2,116,780, he gets a 1520 population of about 5.0 million. Other of Cook's models (in particular, 3 and 4) and other assumptions provide at one extreme a postulated 1520 population of 3.1 million and at the other a range between 67.6 million and 211.1 million. He rejects both as implausible.

Cook's most complex model (Model 5) is uniquely the result of a number of interrelated assumptions and yields a range of figures between 3.9 million and 14.2 million for 1520. Despite the fact that he characterizes this model as "highly speculative," he believes its results to represent "a reliable range for the true preconquest population."[55] In fact, though, neither Cook, in his synthetic conclusion, nor his interpreters have been much enamored of ranges, and this justifies concentrating on his Model 2, which provides not a range, but a very specific figure, and which is distilled in his Table 19, following:

| Region | Rate of change 1570s-[1620s] | Estimated 1520-70 rate (2x) | Population 1570 | Estimated 1520 |
|---|---|---|---|---|
| North coast | 2.2 | 4.4 | 80,123 | 723,111 |
| Central coast | 3.1 | 6.2 | 128,820 | 2,859,540 |
| South coast | 3.8 | 7.6 | 36,587 | 1,635,480 |
| North sierra | 1.2 | 2.4 | 209,057 | 694,094 |
| Central sierra | 1.4 | 2.8 | 240,604 | 975,697 |
| South sierra | 1.2 | 2.4 | 595,528 | 1,977,220 |
| TOTAL | | | | 8,865,142 |

(courtesy of Cambridge University Press)

As Table 19 indicates, in developing his census projection models Cook divided his area of analysis into six regions on the grounds that depopulation differed among them due to varying economic and epidemiological circumstances. As a result, the inferred

[55] Ibid., 106.

rate of decrease for the coastal regions is much higher than that of the highland or sierra areas of the interior. The data that he collected for the 1570-1620 period suggest that the depopulation rate varied from a high of 3.8% per annum on the south coast to a low of 1.2% for both the north and south sierra. For Cook to retroject beyond 1570 he needed to decide whether these rates prevailed for this earlier period as well. For purposes of Model 2 he concludes that they did not, but lacking information for the earlier period, he is forced to decide arbitrarily just to what degree they did not. In doing so he gives the appearance of conservatism. Although "the initial [1520-1570] mortality rate could have been easily double that which followed," or even higher, in the end he decides only to double the later depopulation rate for each of the six regions.[56]

Even though this is essentially meaningless—that is, incapable of being tested—Cook candidly admits that it is the "key assumption" in his operation because it allows him to "calculate the total population of each of the six major geographical sectors *in 1520* by using the standard formula [*sic*]."[57] Table 19 distills, quantifies, and arrays the results of this decision. Here it is necessary to look at three aspects of Cook's procedure—the beginning date of 1520, the end date of 1570, and the rate of depopulation—in order to show how great an effect changing *any one* of these has on determining contact population.

As noted, he uses 1520 as a *terminus a quo* in all five of his census projection models—a departure from his earlier work. Presumably (he does not account for the change) he modified his previous position in light of his belief that epidemics struck the Inca empire before 1530. But how much before? For the first of these epidemics, and the universal *point d'appui* for those inferring high populations for the area, we must depend on the tradition that Wayna Qhapac, the last ruler of a united Inca state, died of smallpox along with many of his subjects. This might well have been the case, but so far no one has undertaken the kind of thoroughgoing exegesis of the extant sources, all of them retrospective, for Wayna Qhapac's reign needed to determine the validity of this claim, which does not occur in all of them. The major written sources span about eighty years and many of them are interdependent. Moreover, the circumstances under which various

[56] Ibid., 96.

[57] Ibid., 96, with emphasis added.

traditions, including this one, were collected and codified are little known but must often have been problematic, at least by modern standards.[58]

However, although this point must be made, of rather greater concern here is Cook's assertion that "the years *around 1520*, 1546, and 1558 were all marked by high epidemic mortality."[59] Elsewhere, he concedes that Wayna Qhapac died between 1524 and 1526, and in his table of epidemics discussed above he uses 1524 for his first, inferred, epidemic.[60] In short, by his own admission (as well as the weight of all the evidence) his backward depopulation projection should end *no earlier than 1524.*

Cook's extrapolation by census projection is exponential; decreasing its length by 10% (from fifty years to forty-five years) results in a decrease of some 20% in the results, and so on.

| | |
|---|---|
| 1570-1520 | 8,865,142 |
| 1570-1525 | 7,013,552 |
| 1570-1530 | 5,607,591 |
| 1570-1532 | 5,142,472 |

In this case, simply by using 1520 instead of 1525 (for 1524/26), Cook adds nearly 2 million Indians to his contact estimates. Were he to bring the beginning date forward to 1530, the latest proposed date for Wayna Qhapac's death, he would have to reduce this figure by another 1.4 million.[61] And if he were to accept, for the sake

---

[58] Suggestive studies are the essays in *From Oral to Written Expression: Native Andean Chronicles of the Early Colonial Period*, ed. Rolena Adorno (Syracuse: Maxwell School of Citizenship and Public Affairs, Syracuse University, 1982), and Susan Paulson, "Double-Talk in the Andes: Ambiguous Discourse as a Means of Surviving Contact," *Journal of Folklore Research* 27 (1990):51-65. Cook has recently addressed the problem of the sources but not with the exegetical rigor that will be required; see his "Impact of Disease in the Sixteenth-Century Andean World" in *Disease and Demography in the Americas*, ed. John W. Verano and Douglas H. Ubelaker (Washington, D.C.: Smithsonian Institution Press, 1992), 208-209.

[59] Cook, *Demographic Collapse*, 95, with emphasis added.

[60] Ibid., 60, 62, 70.

[61] María Concepción Bravo Guerreira, "La muerte de Huayna Capac, 1530: precisiones cronológicas," *Revista de Indias* 37 (1977):7-22. Of course it cannot be ruled out that this association is in the nature of a symbolic *propter hoc ergo post hoc* rationalization. Accepting a date as late as 1530 would do great damage to schemes

of argument, that no epidemics preceded Pizarro's arrival in 1532, his figure of 8.9 million would be reduced again, this time to only about 5.1 million.

Of these four dates, it is impossible to choose between the two most likely, 1525 and 1530. Using both dates to establish a range would probably encompass the most probable case, which then would average out to about 6.3 million, or 30% less than Cook's most likely suggested figure.[62] The year 1532 is possible if one accepts that epidemics did not precede Pizarro, and this date would become the most probable of the four alternatives if it could be shown that the traditions of Wayna Qhapac's death by smallpox or other introduced disease were tainted. On the other hand, it is easy to show that the date he chose—1520—has no standing at all except to inflate the proposed contact population by extending the length of the projection.[63]

The projection path would be shortened at the other end as well if we were to push back the date at which Cook began to double the depopulation rate. He does not specify why he chose 1570 to represent this watershed. Since his disease table shows that there were no major epidemics anywhere in Peru between 1560/61 and 1585, it is more

---

like this since it would have the effect of removing one epidemic from their equation.

[62] Even though an expedient—as all averages are—this approach could have a historical footing since there is good evidence that Wayna Qhapac was alive at the time of the first serious Spanish reconnaissance of Peru in late 1527. See Pedro de Cieza de León, ed. Carmelo Sáenz de Santa María, *Obras Completas*, 3 vols (Madrid: Consejo Superior de Investigaciones Científicas, 1984-85), 1:219-20. Efforts to date his death any earlier are likely to be unpersuasive, although admittedly there is a range of opinion offered in the various chronicles.

[63] Let it be said, though, that in using the words "no standing" the writer does not mean to argue that it is *impossible* that newly-introduced epidemic disease could have spread to the Incan realm by 1520, only that it is highly unlikely logistically; is not supported by a shred of evidence of any kind; is contradicted by what evidence, however exiguous, we do have; and has not to the writer's knowledge been suggested by any historian or demographer. Recently Cook has mentioned possible means by which disease might have been introduced before the mid-1520s. See his "Impact of Disease," 207-208. Among other expedients he suggests that an episode of disease posited for Hispaniola in late 1493 might have found its way to the Inca heartlands. Unfortunately, this episode is a phantom created by the modern misuse of sources: see Henige, "Native American Population at Contact," 12-13.

realistic to use 1561 rather than 1570.[64] Even this seemingly minor adjustment produces a marked effect.

| | |
|---|---|
| 1570/1561/1520 | 7,176,377 |
| 1570/1561/1525 | 5,725,775 |
| 1570/1561/1530 | 4,625,469 |
| 1570/1561/1532 | 4,257,148 |

Finally, there is the question of the depopulation rate itself. Cook does not go to great lengths to justify doubling the rate before 1570, beyond arguing that "the historical record does suggest" that the earlier rate of decline was "higher."[65] With this most scholars would be inclined to agree (especially if the Wayna Qhapac epidemic is accepted), but it is harder to concede that doubling the depopulation rate is the best or most reasonable choice. He does remark that raising the later depopulation rate by *more* than a factor of two would have created a level of depopulation that was "much more devastating" than the historical record seems to indicate. Unquestionably this is so, since raising it to just two and a half times the later rate would increase the projected 1520 population to more than 16 million. But he does not consider the effects that reducing the multiplier would have. This can be done by taking rates from 1.5 to 1.9 times the suggested post-1570 rate, using Cook's figure of 1,290,719 as Peru's total Indian population in that year, although noting that this too is a projection of sorts.

| | **1570-1520** | **1570-1525** | **1570-1530** | **1570-1532** |
|---|---|---|---|---|
| 1.5 x | 5,066,980 | 4,324,921 | 3,712,149 | 2,805,701 |
| 1.6 x | 5,606,210 | 4,718,129 | 3,997,034 | 3,747,157 |
| 1.7 x | 6,262,404 | 5,191,600 | 4,336,813 | 4,044,056 |
| 1.8 x | 7,063,626 | 5,772,517 | 4,756,105 | 4,411,363 |
| 1.9 x | 7,923,693 | 6,373,198 | 5,173,441 | 4,771,526 |

Again it is evident that, because of the cumulative effects of projecting over a fifty-year period, even the slightest change in one

[64] Or even 1559 or 1560, since the epidemics listed for 1560 and 1561 apparently took place (see note 29 above) only in Potosí, outside Cook's self-imposed area of study.

[65] Cook, *Demographic Collapse*, 96.

variable has substantial resonating effects. In the circumstances, Cook's estimate of ca. 8.9 million within a range of 3.2 million to 14.4 million is entirely the outgrowth of a few reasonable, yet at the same time peremptory, decisions, each made at the beginning of the process and each having exponential effects on the results. Change any of these even slightly—and in particular change the beginning of the projection path to 1525 or 1530 instead of 1520—and the result is a much lower number of Indians. Such slight changes in no way diminish the likelihood that any of the new packages is any closer to reality than Cook's. On the contrary, any new model which uses 1525 instead of 1520 is bound to be *historically* more plausible than Cook's ahistorical assumption that uses 1520 as the starting point.

By denying such alternatives a place in the estimating process, and in the tables that represent it, however, and by massaging those data and assumptions that he accepts until they are fat-free, Cook renders Table 19 canonical. Its every facet is entwined with every other facet, so that *in toto* it appears to be more coherent and internally consistent, and therefore more reliable, than it is. Conceivably, a similar impression could be conveyed by words, if at a much greater expenditure of readers' energy and at the risk of rousing their critical instincts by a semantic misstep. The chances of this occurring are immeasurably reduced in configuratons like Table 19 since, like any graphic synthesis of an involved narrative argument, it finds itself wrenched from its context in form, in style, and in argument.

Besides disguising its assumptions, Table 19 also masks its own implacably exponential character. By offering only the first and last figures of a fifty-year projection, it fails to alert readers to the dire implications of the length of time involved. Here it is evident again how this is the direct result of using tables to exemplify words. Cook does tell readers that "[t]he curve of population growth, positive or negative, is an exponential curve, not a straight line."[66] No doubt for historical demographers no more need be said, but for generic historians and others seeking to absorb Cook's arguments, more *is* necessary—either a graph showing how rapidly such projections climb after the first few years or a more elaborate table that shows details of

[66] Ibid., 75.

this year-by-year growth, even if only in five-year increments.[67] In aid of this, the figure illustrates these effects, if somewhat crudely.

Perhaps the depopulation formula Cook uses could stand more scrutiny. It may be "standard" and "widely used," but again only in the rarefied world of the demographer.[68] While displaying the formula $P_2 = P_1e^{rt}$ Cook does not explain that $e$ is a logarithm with a constant value of 2.71828, and without such knowledge no researcher will be able to test his calculations or understand their implications, particularly when it involves a matter so utterly central to Cook's general conclusions.[69]

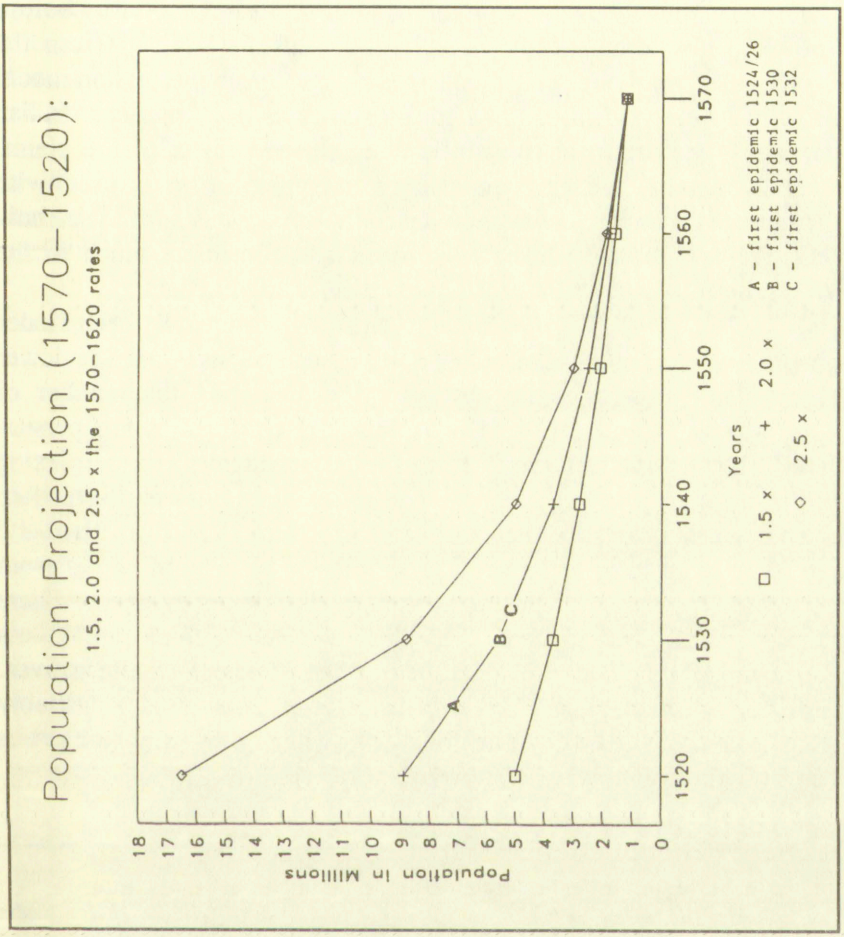
It is plain enough how different mixes of perfectly reasonable mathematical and historical assumptions can reduce Cook's hypothesized most likely population by as much as two-thirds. Moreover, this can be achieved while still accepting for the sake of the argument his premise that mathematical models can overcome the absence of data. Finally, it can be done by incorporating assumptions that fit the evidence better than those in Cook's models—specifically his choice of 1520 and 1570 as the termini for his census projections. Using dates of 1525-30 and 1561 not only reduces the projected numbers inordinately but lends them credence *as a model* by improving their historicity. If one accepts the premise that mathematical models can mimic lost data, and even the assumption that before 1561 mortality rates were twice what they were later, one may arrive at a suggested contact figure (for 1525) of 5.7 million, or a reduction from his figures of about 36%.

---

[67] Back projection as an expedient has met with criticism lately, even when applied to modern Europe, with infinitely greater data available: see R.D. Lee, "Inverse Projection and Back Projection: A Critical Appraisal, and Comparative Results for England, 1539 to 1871," *Population Studies* 39 (1985):233-48; H.A.W. van Vianen, "Past Population: a Critique of Back Projection" in *Profession Demographer: Ten Population Studies in Honor of F.H.A.G. Zwart,* ed. B. van Norren and H.A.W. van Vianen (Groningen: Geo Pers, 1988), 117-25. William F. Keegan, *The People Who Discovered Columbus: the Prehistory of the Bahamas* (Gainesville: University Press of Florida, 1992), 162, speaks of "the mindless projection of annual rates of decline" for Hispaniola as proposed by Woodrow Borah and Sherburne Cook.

[68] Cook, *Demographic Collapse*, 90.

[69] Cf. Thomas M. Whitmore, *Disease and Death in Early Colonial Mexico: Simulating Amerindian Depopulation* (Boulder: Westview Press, 1992), 15, for a similarly reticent discussion of this formula.


Population Projection (1570–1520):
1.5, 2.0 and 2.5 x the 1570–1620 rates
Population in Millions
0
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
1520
1530
1540
1550
1560
1570
Years
A
B–C
□ 1.5 x
+ 2.0 x
◇ 2.5 x
A – first epidemic 1524/26
B – first epidemic 1530
C – first epidemic 1532

However, while this figure conforms much better to the data available to plug into it, there is no reason to assume that it is anymore probable than Cook's own figure, or than *any other* figures contrived by modeling, because too many assumptions, whether or not demonstrably improbable, comprise the model, reminding one that as possibilities are multiplied, probabilities decrease. To make this point it has been shown how varying the presumed rates of mortality before 1561 can also dramatically affect the conclusions derived from the exercise. As has been seen, Cook regards the census projection model as a particularly "promising" means to measure American Indian decline.[70] His use of it, however, does not add luster to the claim. Even when this ordering impulse of the imagination is used with greater sensitivity to historical evidence, however, it is hard to see how such a homogenized banker's approach can contribute much in the virtual or complete absence of requisite demographic data.

Reviewers of *Demographic Collapse* have generally been kinder about Cook's results than about his assumptions. One reviewer encapsulated this when he observed that "[d]espite the number of questionable assumptions and problems, the conclusions are persuasive."[71] Many are certain to find this an intriguing and perplexing rationale. It may spring from the fact that many reviewers have, like the present writer, been impressed by Cook's carefully constructed and modestly expressed narrative analysis. There he seldom shirks from conceding the exiguity and problematic nature of his evidence. There he is generally candid about what his own assumptions are, if less forthcoming about why he holds them to the exclusion of alternatives. Still, the notion that defensible conclusions can somehow demonstrably transcend the indefensible methods by which they are reached is certainly the triumph of hope over experience.[72]

---

[70] Cook, *Demographic Collapse*, 109.

[71] Robert V. Morey in *American Ethnologist* 11 (1984):205-206. For a recent favorable discussion of *Demographic Collapse* see Lovell, "'Heavy Shadows and Black Night'," 435-37. Conversely, in his review of the work, Rudolph Zambardino, in *Journal of Interdisciplinary History* 14 (1984):719-22, is critical of Cook, although on statistical rather than historical or epidemiological grounds.

[72] Of course this quixotic view of the nature of proof is widely shared. Cf. the comment of the Africanist structuralist anthropologist Luc de Heusch that "[w]hatever attacks can be made on his methodology, [Marcel] Griaule was the first to reveal to us the African cosmological vision." Pierre de Maret, interview with Luc de Heusch,

But a closer look at Cook's tables—not only elements in his rhetorical strategy but elements that play a crucial mediating role—allows us to sense some of the problems lurking beyond the text. Of these, clearly the most important is his decision to extend his depopulation projection as far back as 1520, as though disease-based decline had begun by that year. At first glance this decision might appear innocuous—it did to the writer—yet it alone adds almost two million people to his final estimate of contact population. More than that, it shows clearly how mathematical imperatives can override other considerations.

In providing readers with a welcome synthesis of his first several chapters, Cook—perhaps inevitably—feels obliged to abandon the expedient of population ranges in favor of arguing for a particular preferred single, if composite, estimate. As he puts it:

> My final estimate of 9 million. . . may appear to be arbitrary [but] it is made after careful weighing of the evidence, rather than being merely an act of faith.[73]

This, as he points out, is roughly the midpoint of the range he considers most probable. More to the point, perhaps, it is also very close to the two figures at which he arrived through the exercises carried out in Tables 12 and 19, and in these circumstances it is appropriate to ask the degree to which these exercises were done independently. Cook admits that he prefers to accept the maximum decline countenanced by his epidemic model, but, as noted, is somewhat more vague about why he decided to double the rate of decline for the period from 1570 and 1620 and apply it bodily to the preceding fifty years.

This close relationship between the results of the epidemic and census projection models brings forth the issue of the symbiotic relationship among quantitative analyses, especially within a single work with a single overarching argument. It is a question that can be answered only by those carrying out the exercises, but on the face of it it seems reasonable to wonder whenever two apparently independent lines of arithmetical reasoning coincide so closely in their results. In the case of Tables 12 and 19 of *Demographic Collapse,* we see how in

---

[73] Cook, *Demographic Collapse*, 114.

each case the particular scenario Cook chooses is only one of several equally probable alternatives. There is no indication whether these choices were the culmination of a testing process involving such alternatives, whether the ranges and values were adopted instinctively, or whether there was interplay between the process underlying Table 12 and that behind Table 19, so that the coincidence of conclusion is contrived rather than derived, since the congruence is man-made, that is, imposed.[74]

Mathematical arguments presented in tabular form are designed to appeal to the eye rather than to reason. They become graphic as a means of "aiding" readers and this helps render them all the more estranged from any reality beyond their underlying assumptions. Paradoxically, for *Demographic Collapse* the contrast is especially marked—paradoxically because Cook's narrative argument is much more scrupulously laid out than is typically the case for this field; but this state of affairs changes in his tables, many of which are destined solely for historical demographers. Even so, the tables discussed here do not require much statistical sophistication. In fact they are so streamlined that they divorce themselves from their accompanying texts and create an impression of exactitude and correctness that is not supported either by the text or by the balance of relevant historical evidence. Yet—and this is admittedly surmise—Cook's success in selling his overall argument about contact population size depends more on these tables than on all the rest of *Demographic Collapse*.

Referring to a different, but not dissimilar, set of circumstances Grant Jones points out well the problems facing historians of this time and place. In face of a new document that came to light and that forced him to reinterpret a series of events in the Yucatán late in the seventeenth century, Jones recognizes

> the methodological dilemma that all colonial ethnohistorians must find both most frustrating and most tantalizing—that missing or unavailable information is

[74] This approach could be considered an example of the "realism to a coherence" theory in sociology, in which hypotheses are validated by matching them with each other rather than with an independent reality. For this see Peter Halfpenny, "Talking of Talking, Writing of Writing: Some Reflections on Gilbert and Mulkay's Discourse Analysis," *Social Studies of Science* 18 (1988):169-82.

> likely [if discovered] to transform understanding *and that ignorance is as powerful a factor in ethnohistorical analysis as is knowledge.*[75]

Our virtual lack of knowledge about the demographic and epidemiological dynamics of Peru (and of course of most other areas of the New World) before the mid-sixteenth century ineluctably means that almost any model that is internally coherent can gain plausibility. In this case the writer has offered alternatives to Cook's preferred scenarios which change his results significantly without sacrificing their own plausibility. If the matter of contact population is simply to be a game characterized by competing plausibilities—and what else can it be at this stage of the proceedings?—then perhaps it is a game that need not yet be contested.

Having criticized the conclusions of the Disease Model of depopulation both here and elsewhere, the writer should in all fairness comment as well on alternative approaches. This can be briefly done. As has been implied, the best alternative—that is, the alternative that can most easily bear the weight of falsification—is simply to decline the temptation to posit contact population figures anywhere in the New World. Doing so in no way diminishes the value of recognizing the central role of new diseases in depopulating the Americas. In fact, it avoids the opportunity for critics—such as the present writer—to dispose of the baby with the bath water by denying them the occasion to point to problematic use of sources and arithmetical methods. One unavoidable and debilitating fact remains: no depopulation table ever constructed for the post-contact Americas can be verified by applying independent evidence, whereas most of them can be readily falsified. In effect they are—for they must be—mere simulacra dragooned into service to impersonate a reality that is quite beyond recovery.

Graphics are so central to the communication process that the display of information graphically has become a recognized subfield of communications theory. One of its tenets is that the effectiveness of such displays is in no way bound to the accuracy with which they distill the larger body of evidence. Instead, it lies in the ways they organize

---

[75] D. Grant Jones, "The Canek Manuscript in Ethnohistorical Perspective," *Ancient Mesoamerica* 3 (1992):249, with emphasis added.

and sell themselves as accessible microcosms of whatever it is they purport to represent.[76]

In scholarly communication, such graphics, whether they be maps, graphs, tables, or pictures, often spell relief for readers from the surrounding dense foliage of argument, that is, from verbal display. More specifically, in the area of American Indian historical demography they conveniently encapsulate quantitative analysis; close textual study, sometimes in foreign languages; unexciting if necessary historiographical exegesis, and the like. All things considered, it probably is fair to suggest that they have had an influence well out of proportion to the space they occupy in the literature. In effect they are windows allowing quick, if grimy, glimpses at the demographic, particularly the numerical, conclusions minus the arguments. While it is impossible to demonstrate the effect these epidemic/depopulation tables have had and will have, it is certainly possible to illustrate how estranged they can become from the nature of the evidence that purports to support them.

Besides looking at depopulation tables and allied weapons from these demographers' arsenal as semiotic devices, it is useful to think of them as examples of another rhetorical category—lists. Organizing like and unlike items into lists constitutes a popular way to homogenize and synthesize their value, since in many lists the good, the bad, and the

---

[76] Some brief studies of aspects of this issue are M.D. Vernon, "The Visual Display of Factual Data," *British Journal of Educational Psychology* 20 (1950):174-85; Michael Macdonald-Ross, "How Numbers are Shown," *AV Communication Review* 25 (1977):359-409; P. Wright, "The Comprehension of Tabulated Information: Some Similarities Between Reading Prose and Reading Tables," *NSPI Journal* 19 (1980):25-29; Howard Wainer and David Thissen, "Graphical Data Analysis," *Annual Review of Psychology* 32 (1981):191-241; and Stephen M. Kosslyn, "Graphics and Human Information Processing," *Journal of the American Statistical Association* 80 (1985):499-512; Edward R. Tufte, *Envisioning Information* (Cheshire, CT.: Graphics Press, 1990); Alan S. Gross, "Extending the Expressive Power of Language: Tables, Graphs, and Diagrams," *Journal of Technical Writing and Communication* 20 (1990):221-35. For graphics as applied specifically to the arrayal of historical information see the series of articles culminating in Rudolf G. Andelt, "Die grafische Darstellung. Ein Hilfsmittel des Unterrichts. Ein methodisches Instrument der Geschichtswissenschaft?" *Zeitgesch-ichte* [Vienna] 14 (1986-87):198-210.

indifferent mingle indistinguishably.[77] The results of this are the opposite of the weakest-link effect, since any individual entries on a list that are inherently dubious actually gather strength from being in the mixed company of stronger elements. In particular, depopulation and epidemic tables can be compared to chronological tables. Of these, the most common are king lists, episcopal lists, and lists of other types of officeholders. In the typical list of this nature, all incumbents appear looking very much alike, regardless of their credentials. Presenting the course of succession to an office over time in list format thereby exonerates the compilers from the requirement of defending the inclusion (or exclusion) of particular names.

Lists also make it simple to confer legitimacy in cases of doubt. Take the list of the popes. In 1947 the Vatican promulgated and sanctioned a newly-minted official list of the bishops of Rome.[78] In this the number of so-called antipopes increased, partly to fix the official list more securely. One way in which these antipopes have been banished was by having later, officially recognized, popes recycle their ordinal numbers. Thus the antipope John XXIII (1410-15) had his name and number reused by the pope who held office from 1958 to 1963, only one of more than twenty such cases.

The officially accepted list of Japanese emperors is carefully numbered from 1 to 125. Most of the first twenty or so of these never existed, certainly not as emperors, but for our purposes it is more pertinent that a sequence of six fourteenth-century emperors who ruled contemporaneously (as the "northern court") with other emperors (at the "southern court") are either omitted from modern official lists or carefully sequestered by being labeled N1 to N6.[79] The very act of listing turns the northern emperors into usurpers.

List formats also permit and encourage notional continuity. The canonicity of the official list of popes is invigorated by the fact that it reaches back in an unbroken chain to the apostle Peter. In the Middle

---

[77] For examples from the Norse Edda literature see Elizabeth Jackson, "Some Contexts of Old Norse Ordering Lists," *Saga-Book of the Viking Society* 23 (1991): 111-40.

[78] Angelo Mercati, "The New List of the Popes," *Medieval Studies* 9 (1947):71-80.

[79] Kitabatake Chikafusa, trans. H. Paul Varley, *A Chronicle of Gods and Sovereigns* (New York: Columbia University Press, 1980), 1-41; *Cambridge History of Japan*, 6 vols. (Cambridge: Cambridge University Press, 1988-1990), 3:187, 456-57.

Ages most other sees in western Europe did their best to emulate this prized antiquity by inventing whatever number of bishops were required to span the three centuries left embarrassingly vacant by "missing" documentation. Only in the nineteenth and twentieth centuries were these invented bishops—many hundreds of them—removed, and it is certain that some yet remain.

Closer to home, the most fully developed list of the Inca rulers, and the one that has since found greatest favor among scholars, was devised by Inca Garcilaso de la Vega when in his European Humanist persona. Here one may find ten Incas ruling in uninterrupted father-to-son succession, very much the contemporary European ideal. But how European, after all, was pre-conquest Inca society? Recent efforts to understand indigenous Andean norms appear to show a much more complex situation—at worst the early names on Garcilaso's list never ruled, never even existed; at best they ruled in contemporaneous parallel lines, rather like the ancient Spartan rulers.[80] Perhaps there were even three Inca rulers at a time, in the vein of the rulers of Elam in the second millennium B.C.[81] At any rate what one may find in this instance is the deconstruction of a simplified solution to the Inca past and its gradual replacement by concepts that are at once more challenging and less doctrinaire than the conventional wisdom.

Statements in lists are usually affirmative; doubt is handled by elimination, and users of lists are forced to accept their contents as representing—and symbolizing—only one facet of whatever universe they attempt to encapsulate. In the circumstances such lists are residues and therefore synchronic. For example, the periodic table of elements reveals only that at a particular point in time—the date at which the table was prepared—a certain number of elements were recognized as canonical. Missing is any hint that over the period of a couple of centuries or so, other elements were considered, sometimes temporarily

[80] Most of this new thinking emanates from the work of R.T. Zuidema, but for historians the key discussion is that of Pierre Duviols, who shows that the early chronicles themselves—most compiled by Europeans, although of course based on Andean informants—present an ambiguous and discrepant picture in this regard. See his "La dinastía de los Incas: ¿monarquía o diarquía? Argumentos heurísticos a favor de una tesis estructuralista," *Journal de la Société des Américanistes* 66 (1979):67-83.

[81] For this notion see Martti Pärssinen, *Tawantinsuyu. The Inca State and Its Political Organization* (Helsinki: SHS, 1992), 207-27.

welcomed, but eventually cast out. The dynamics are entirely absent; stasis is paramount.

Depopulation tables, lists of epidemic, and, most obviously, population projections faithfully carry on this tradition. Thus when Cook takes his census projections all the way back to 1520, he does so without historical license, but does have the warrant provided by the very act of list-making. His depopulation formula emulates continuous compounding, and despite the fact that it is virtually impossible that epidemics affected the population of Peru as early as 1520, 1521, 1522, or 1523, the numbers for these years retain, as constituents in an undifferentiated list, the same apparent value as those for later years when it is known that there were epidemics.

Finally, lists are among the easiest records to change. If orally transmitted, nothing is required but to incorporate the desired changes in the next transmission. In written lists, the changes are slightly more difficult but probably still easier than making similar changes in a narrative text.[82] And if the lists are generated electronically, as is now so often the case, any number of changes can be made with almost no effort—and certainly no trace—at all.[83]

Despite the intervention of nearly five centuries and any number of epistemological sea changes, there are surprisingly striking similarities between much recent work in the field of American Indian historical demography and that of Bartolomé de las Casas. The most obvious of course is the enchantment with large numbers and the inability of either camp to defend these with unequivocal evidence.[84] To compensate for this Las Casas relied on numerous grisly, if possibly

---

[82] Mark Monmonier, *How to Lie With Maps* (Chicago: University of Chicago Press, 1991), 1-4, expresses very well the points being made here about the dangers of verisimilitude.

[83] For a somewhat different look at lists and their implications see Jack Goody, *The Domestication of the Savage Mind* (Cambridge: Cambridge University Press, 1977), 74-111, and, on Goody's interpretation, John Halverson, "Goody and the Implosion of the Literacy Thesis," *Man* 27 (1992):301-17.

[84] Thus, elsewhere in his work Las Casas did offer the throwaway estimate that "more than 40 million" Indians had perished by 1560. Las Casas, *Historia de las Indias*, ed. Agustín Millares Carlo, 3 vols. (Mexico City: Fondo de Cultura Económica, 1951), 3:363. This number is very much at odds with the figures advanced in his *Brevísima relación*, but Las Casas offered no justification for using it. In any case, any use of the number forty, especially in ecclesiastical circles, is *prima facie* suspect, given its heavy Biblical symbolism.

historical, anecdotes to make his case. These he juxtaposed with numbers, but without tying the two together—by claiming, for instance, that the incidents he included by themselves accounted for the larger depopulation he implied. In this way he let the stories themselves—mere anecdotes— stand surrogate for absent argument.

In much of this work there is no lack of argument, of course, nor of appeal to sources, but the numbers achieved can come *only* as the result of massive mathematical manipulations, which are as divorced from the testimony of the sources as Las Casas's horror stories are from his numbers. In place of direct evidence for particular numbers, those advocating higher numbers of people at contact substitute epidemic episodes, for which they must frequently sacrifice supporting detail, but are rewarded with the opportunity to wield numbers far greater than Las Casas ever imagined.[85]

Las Casas's *Brevísima relación* acquired a certain kind of credibility almost immediately and underwent a phenomenal number of editions and translations, as Spain's enemies sought to use his arguments on their own behalf instead of on behalf of the Indians.[86] It is no surprise then that acceptance or rejection of Las Casas's version of things broke down very much on political and ideological party lines and had little if anything to do with the plausibility of his case or the ways he presented it. Spain's competitors in both the Old and New Worlds found little difficulty in accepting the gist of Las Casas's account, and his decontextualized numbers, simply on the ground that they ought to be true—or at least ought to be believed.

Even today there is a strange eagerness to accept much of the *Brevísima relación* at face value and as a tract for our times, in at least one instance with sinister overtones. In 1974 an English translation appeared, which was graced by an introduction by Hans Magnus Enzensberger, who used the occasion to clothe Las Casas with some

---

[85] As Lovell, "'Heavy Shadows and Black Night'," 437, points out: "...controversy is generated not so much by the numbers themselves as by the divergent views of history that any particular choice of numbers represents."

[86] Isacio Pérez Fernández, *Inventario documentado de los escritos de Fray Bartolomé de las Casas* (Bayamón, P.R.: Centro de los Estudios de los Dominicos del Caribe, 1981), 323-32; Jan Lechner, "En torno a la *Brevíssima relación de la destruyción de las Indias* de Fray Bartolomé de las Casas" in *España, teatro y mujeres: Estudios dedicados a Henk Oosterdorp,* ed. Martin Gosman and Hubert Hermans (Amsterdam: Rodopi, 1990), 217-26.

contemporary relevance. Enzensberger was alarmed by those who were prepared to dispute Las Casas's figures, or at least the goodwill behind them: "[s]uch an approach has something repulsive about it from the very outset. It would like to prove Las Casas a liar but let murderers go scot-free because they only killed 8, 5 or 3 million Indians instead of 20 million." Enzensberger then clinched his argument by proclaiming that "[t]hat is the way the *National Zeitung* protects the German fascists, claiming that not 6 million Jews were killed but at most 5."[87]

In 1992 a reprint of this edition appeared, virtually unchanged except that it has been mercifully purged of Enzensberger's outrageous comments, along with the rest of his introduction. Nonetheless, the way in which he bound together ancient numbers and modern guilt and contrition is instructive. For him, it is clear, numbers are the simplest embodiment of both the depopulation of the New World and of the Holocaust. While this writer knows of no such sentiments ever having been expressed by any advocate of the Disease Model, it is apparent nonetheless that the arithmetic of contact population is being played out against an ideological background, in which the numbers act as badges by which parties identify—and identify with—each other. *Plus ça change, plus c'est la même chose.*

[87] Bartolomé de las Casas, *The Devastation of the Indies: a Brief Account*, trans. Herma Briffault (New York: Seabury Press, 1974), 12-13.

# estudios fronterizos

REVISTA DEL INSTITUTO DE INVESTIGACIONES SOCIALES
No. 26 septiembre-diciembre de 1991

## ARTÍCULOS

Síntesis histórica del poblamiento de la región fronteriza
Manuel Ceballos y Lawrence Douglas Taylor

Dinámica de los componentes demográficos de Baja California
Gabriel Estrella Valenzuela

Migración y derechos humanos
María Eugenia Anguiano

México: estimación de la migración internacional
Jorge Castro Martignoni

*Estudios fronterizos* (revista cuatrimestral). Para mayor información comunicarse en México a revista *Estudios fronterizos*, Instituto de Investigaciones Sociales-UABC. Apdo. postal 3-136, 21100 Mexicali, B.C. En el extranjero al P.O. Box 3280, Calexico, CA 92232-1426 U.S.A.