Computers in Historical Analysis

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A Computational Memory

An image of my early days as a graduate student sticks in my mind after more than twenty years. Half a dozen of us are standing around a clanking, whirring machine in a harshly lighted basement room. There in the middle is sociologist Samuel Stouffer, talking fast, cigarette swinging from his mouth, ashes showering his vest. Stouffer grabs a deck of punched cards, shoves them into the hopper at one end of the machine, pushes a button, and watches the cards sort themselves into glass-topped bins. He peers at the size of the various piles. Then he says, "OK. Now let's try breaking on religion." He whips each stack of cards from its bin and slaps it onto the glass above the bin. Then he grips the first stack, fans it out, drops it into the hopper again, taps to straighten the cards, sets a weight on top of the stack in the hopper, turns a crank, and pushes the button again.

We, the graduate students, were learning a crude but serviceable way of analyzing data, i.e., translating arguments about social phenomena into statements about variables and units of analysis. (The units were most often individual survey respondents and the variables their responses to standardized questions, but neither was essential to the logic we absorbed with the cigarette smoke.) Then we were supposed to identify the variable to be explained, cast our explanations in terms of differences with respect to other variables, represent obvious alternative explanations as "control" variables, and then carry out a cross-classification of the units which would reveal whether they did, indeed, vary as expected.

We might, for example, have tried to determine whether people living on farms actually said they wanted more children than people in big cities, once we "controlled," or held constant, the present family positions of respondents in the two populations. The machine was not indispensable to this sort of analysis. In principle, one could do it with pencilled tallies or with slips of paper sorted on a large tabletop. (I have done both things myself in emergencies.) Compared with the other alternatives we knew, however, only the machine made the analysis quick and practical.

The clanking old machine was not, of course, an electronic computer. It was a counter-sorter, a contraption in which electrified metal brushes responding to the presence or absence of holes at different positions in a Hollerith card activated gates along a belt on which the cards were moving, thus shunting the cards into one bin or another, and counted the cards shunted into each bin. Although people who work with punched cards still use the quicker, smoother descendants of our old basement machine in getting their data ready for the computer, the counter-sorter has practically disappeared as a tool of analysis in the social sciences.

Still the memory is useful. It sums up the predominant experience of social scientists with analytic machines until very recent years: the machine has greatly

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increased the feasibility of procedures which were actually invented without reference to machines, or at least without any necessary connection to them. The analytic inventions occurred more or less independently of the existence of counter-sorters or computers. The diffusion of those inventions, however, depended very closely on the availability of the machines.

Something else about that basement scene needs attention. Although Sam Stouffer taught us to admire the classic deductive progression from general theory to specific hypothesis to empirical test against real-life observations, he was a wizard of *post hoc* interpretation of findings. In the jargon of the time, his "let's try breaking on religion" meant dividing the sample into Protestants, Jews, and so on, in order to see if any new differences, or any new explanations of the old differences, showed up. We learned that part, too. The nature of the data and the implicit or explicit theories the investigator is working with will set limits on how many *ad hoc* explanatory variables he will introduce in this way. Nevertheless, the vaguer and the more variegated the arguments at the investigator's disposal, and the easier the introduction of one more explanatory variable, the greater the likelihood that he will fashion and attach importance to spurious explanations.

The counter-sorter and the tabulating equipment that came with it heightened this risk somewhat by comparison with the hand tallying and other such primitive procedures they replaced. Their net effect may still have been to reduce the number of spurious interpretations being seriously entertained by social scientists, because they came into wide use when theories were superabundant, determinate findings relevant to those theories quite rare, and cross-checking of doubtful explanations extremely hard to do.

Computers and Social Scientists

The computer compounds the risk. The capacity to absorb, store, and manipulate large bodies of data, the easy introduction of complex, prepackaged statistical routines, the speedy efficiency of the machine (at least on its good days), and the tiny incremental effort ordinarily required to introduce one more analysis, one more variable or one more observation all reduce the cost of running vast exploratory analyses. If the self-discipline of social scientists and the determinacy of their theories do not increase at the same rate as the expansion in computing facilities available to them, the net effect is likely to be a growth of the number of spurious explanations of social phenomena having currency.

Depending on one's view of the intrinsic possibility of reliable knowledge about human social behavior, this worry may sound cynical, or it may sound irrelevant. Yet it follows pretty closely from the characteristic relationship of social scientists to their data. The directors of most university computing centers have, for example, had to deal with it one way or another.

So far as I can tell from personal experiences, the university-wide computing facilities which sprang up in the 1950s tended to organize around several interesting assumptions concerning their clientele: 1) that the users were primarily interested in computation as such, rather than tabulation, compilation, reordering of large files, preparation of descriptive maps and graphs, content analysis of texts, and a variety of other uses to which computers have sometimes been put; 2) that the users would arrive with step-by-step descriptions of the computations they wished to perform, and would want to translate each step into a command the machine could follow; 3) that the work involved extensive manipulations of relatively small volumes of input data. These assumptions appeared, among other places, in the early emphasis on short courses in FORTRAN (=FORmula TRANslation) as the all-purpose preparation for computing, in the physical arrangement of input and output areas to accommodate clients who would ordinarily submit thirty or forty cards of data and programs, and in the great shortage of facilities for plotting, mapping, reordering files, reading of texts, and the like.

Computation, formula translation, and low ratios of input to analysis do not describe the usual situation of the social scientists who were coming to the computer in the 1950s and 1960s. At the beginning, most of us were interested mainly in the sort of reordering and tabulation of data we had previously done with machines like counter-sorters. We knew little mathematics, and found it difficult to transcribe the simple statistical operations we had learned to do with pencil and paper, slide rule or desk calculator into logical sequences comprehensible to machines. Not much computation there, nor much preparation for programming an entire analysis.

What is more, the computer-bound social scientist most often wanted to do something simple but cumbersome to a large body of data: a rank ordering of all counties in the United States on per capita income, and then on the proportion of the labor force engaged in agriculture, perhaps, or a cross-tabulation by occupation and region of the stated party preferences of 2,171 persons in a national sample survey. In short, a high ratio of input to output and a very high ratio of input to computation. A good deal of the extensive development of computing facilities for the social sciences during the 1960s went into redressing the balance: extending the noncomputational capacities of the computer, packaging and simplifying the commands required to get the computer to carry out big but standard routines, and increasing the capacity of computing systems for input, storage, output, and transmission of large bodies of data.

No doubt my description applies least well to the economists of the 1950s and 1960s; although their analyses of national income and related phenomena required simplification of large masses of data, they were already quite accustomed to applying complex models to small numbers of observations. At the other extreme stood the historians. Even the minority who identified themselves with the social sciences, even the smaller minority who were working with material which could, in principle, be placed in machine-readable form, wanted lists, catalogs, tabulations, and other convenient descriptions of their data rather than any systematic analysis of patterns, relationships, and correspondence to models. The first textbooks of quantification, statistical analysis and computing for historians only began to appear, after all, around 1970. My description probably holds best for sociology and political science, and loses accuracy as we move away from them.

Now, I am not naive enough to think that the puny demands of university social scientists alone produced the changes in computing which occurred after 1960. Some of them resulted from the increased use of the computer by branches of the natural and physical sciences like ecology, whose main need was for the simplification and ordering of large bodies of observational data. More, I think, resulted from business and military applications, which are often quite similar in form (if not in content) to social scientific analyses. In any case, during the later 1960s American social scientists in big centers of research finally began to have at their disposal computing facilities that were well suited to the characteristic approach of social scientists to their data.

The Historical Social Sciences

Let us look more closely at the experience of the historical social sciences. I mean the conglomeration of specialities which have three things in common: 1) they concentrate on human social relationships, 2) they deal with change over a substantial succession of particular times, 3) their procedures yield conclusions which are generalizable, at least in principle, beyond the particular cases observed. Those specialities don't group together in any one major discipline. They exclude most of sociology, even more of anthropology, almost all of psychology, much of economics, perhaps less—surprisingly enough—of geography, demography, and political science. Demographic history, econometric history, historical studies of social mobility, and long-term analyses of the determinants of political participation illustrate what I have in mind.

Note some common features of this brand of inquiry. First, it commonly involves the systematic accumulation of numerous more or less uniform individual observations into a general portrayal of the phenomenon under study. Where the observations concern individual people we call the procedure collective biography or prosopography; where they concern firms, families, communities, or other units, we have no standard term; but we might call the whole approach collective history. While many American historians have been willing to let the observations of a Tocqueville or the life histories of a few entrepreneurs stand as their evidence of the opportunities for mobility in the nineteenth century, a number of younger historians have recently been insisting on the study of occupational mobility for the entire population of whole communities. The most ambitious venture of this kind so far has been Stephan Thernstrom's person-by-person examination of Boston's adult male population from 1880 to 1970 (Thernstrom 1973). (Among other things, Thernstrom discovers great stability in the rates of movement from manual to white-collar jobs and in the propensity of new migrants to stay in the city, despite large changes in the rate of in-migration, and an important shift from greater long-distance mobility among manual workers in the nineteenth century to greater long-distance mobility among white-collar workers today.)

Second, this sort of investigation ordinarily includes systematic comparison of standard units—populations, areas, periods, or something else—with respect both to the phenomena to be explained and the explanations proposed for them. Thus in their analysis of the rural uprising of 1830 in England, Eric Hobsbawm and George Rudé tabulate villages which rioted or did not riot by whether land had recently been enclosed, how much of the village population was non-agricultural, and so on. (As it turns out, recently enclosed and semi-industrial villages appear to have had the higher propensity to riot; altogether, the findings lend weight to explanations of the revolt in terms of the defense of particular local rights to land and work, and cast doubt on explanations in terms of spontaneous reaction to short-run economic crisis.)

Third, the kind of inquiry I am identifying with the "historical social sciences" tends to rely on the explicit statement of concepts, hypotheses and models, as well as the self-conscious matching of the observations to them. In his influential study of marketing in rural China, G. W. Skinner begins by laying out the logic of the central-place theory often employed by economic geographers, proceeds to show that the timing, interdependence, and geographic distribution of local and regional markets in pre-communist China fall into the patterns anticipated by central-place theory, argues that the market system provided the framework for a wide range of other activities not obviously related to marketing—for example, the choice of marriage partners and the gathering of peasants to recreation—and finally points out the persistence of the fundamental patterns past the revolution of 1949. Skinner's studies launched a whole fleet of studies of Chinese marketing patterns and their correlates.

Finally, quantification. Neither the aggregation from individual to total population, the reliance on systematic comparison nor the explicit confrontation of fact and theory is intrinsically quantitative. Yet all three are obviously hospitable to quantification in ways that many approaches to history are not. A case in point: David Herlihy seeks to learn whether, as some theories would lead us to expect, prosperity encouraged the Tuscan population of the fifteenth century to marry younger, have more children, and form larger households. He has enormous documentation at his disposal, notably the *catasto* of 1427, a document resulting from the effort to enumerate and describe every single one of the 50,000 to 60,000 persons eligible to pay taxes in the territory then controlled by Florence. We are not the least surprised to find him casting the crucial questions in quantitative form, by calculating fertility rates, mean household sizes, and the like. (Nor are we surprised to find that computers are doing a major part of the routine work, but let's save that for later.) Once he is committed to checking theory against fact by aggre-

gating thousands of individual observations into comparisons over space, time, and social category, it is hard to imagine how Herlihy could proceed without quantifying a number of his main arguments. Yet dozens of historians before him have written histories of Tuscany and of Florentine families without a trace of quantification.

Current Counter-Currents

My reliance on positive examples may obscure this important fact. Most inquiry into history does not fit my description of the historical social sciences. Most historians are not doing collective history, are not making systematic comparisons of standard units, are not self-consciously building models and confronting them with historical fact, are not casting their arguments in quantitative form. Nor do they want to. They are, on the whole, proud to be doing something else. They are often distressed that anyone should be building models, quantifying, and so on. Since the computer is quite unlikely to spread into those areas of history in which investigators lack or reject the habit of putting part of their work into quantitative form, the prospects that the computer will revolutionize historical analysis as a whole in the near future are slight indeed.

The faint possibility of such a revolution has nevertheless called forth indignant roars from some of the profession's strongest voices. The statements on the subject tend to confound computerization with quantification, to trot out examples (real or imagined) of trivial, illegitimate and/or misleading quantitative analysis, and then to call for a common defense against the Huns. The enduring objections, however, do not appear to center on existing misuses of computers and quantification. Instead, they concern the possibility that quantitative historians will abandon the humane depiction of real, whole persons, mistake their statistical results for the reality and—worst of all—communicate their delusions to other historians.

Statements of this view are often written with passion and brilliance. One of my favorite specimens of the genre comes from Richard Cobb, the superb portraitist of cops, spies, criminals, rioters, revolutionaries, and ordinary people of the revolutionary era in France. At one point in a recent essay called "Historians in White Coats" he describes an investigation now proceeding in the United States as:

> the computerization of 516 urban riots, turbulences, disturbances, *fracas, prises-de-barbe*, semi-riots, *revolvérisations*, lynchings, stabbings, slaughters, massacres, protests, collective threats, abusive slogans, provocative songs, in France, for the whole period 1815–1914. The end product will no doubt reveal some highly interesting pattern: that, for instance, market riots occur on market days, on or near the market, that marriage riots take place after weddings, that funeral riots take place either outside the church or near the cemetery or along the course of a funeral procession, that Saturday riots take place on Saturday evenings, between 10 and 12 o'clock in the winter and between 11 and 1 o'clock in the summer, that is after the wineshops and *bals* have closed, that Sunday riots take place after Mass, that rent riots take place on rent days and that they are commoner in April and July than they are in January and October, that port riots take place on or near ports, that recruitment riots converge on railway stations or on barracks, that prison riots take place inside or opposite the prison, or both, that religious riots, especially in towns or *bourgs* in which there exist two or more antagonistic religious communities, favour Sundays, Catholic feast days, or St. Bartholomew's Day, or the Passover. Perhaps we thought we knew already; but now we *really* know; we have a Model. Riot has been tamed, dehumanized and scientificated (Cobb 1971: 1528).

I had some trouble recognizing my creature in motley. But once I realized that I was the originator of the investigation in question, I read Cobb's account with fascination. It proves him a master of historical fiction. Every detail is invented: the time-span, the number of events, the kinds of action covered, the questions asked, the answers given,

the whole point of the study. (In fact, the study runs from 1830 to 1960, deals mainly with large-scale collective violence, and consists of a number of different efforts to determine the impact of urbanization, industrialization, and political centralization on patterns of collective action and struggles for power in France, not to mention related work concerning other West European countries.)

Yet some resemblances to the original are interesting, even disquieting. It is true, for example, that a number of the results of such an inquiry are bound to be trivial, and others more or less self-evident after the fact. No one is stunned to discover that labor unions became more heavily involved in major French conflicts toward the end of the nineteenth century than they had been fifty years before. Unions had, after all, only existed in the shadows until their legalization in the 1880s.

The main reason for pursuing results which will appear obvious in retrospect is that not all of them are obvious in prospect. Some fly in the face of widely held opinions. In the investigation at hand, the widespread small-town participation in the rebellion of 1851 against Louis Napoleon's coup d'état (when provincial France as a whole, sickened or disappointed by the course of the 1848 revolution, is supposed to have lapsed into apathy or conservatism) makes us rethink the whole process of political mobilization and demobilization in that period. Other findings help discriminate among several alternative readings of a process, each of which is plausible, and therefore obvious in retrospect. Should we, for instance, expect crime against persons and collective violence to vary together or to follow distinctive patterns in time and space? The latter is the case in modern France. But if we had found the former there would have been plenty of common-sense rationalizations and sociological theories to make the findings self-evident.

It is also true (as Cobb indicates elsewhere in his essay) that this sort of inquiry is expensive, requires the organization of a research team, and relies relatively little on the traditional lonely encounter of one man with one document. It is true (as Cobb's reasoning suggests, despite the fact that his blurred vision of computing doesn't allow him to pick out the details) that the combination of high initial investment and low marginal cost of additional items encourages the builder of a data file, once begun, to pack in all sorts of apparently useless information. It is true that the moving of some of these large machine-readable historical files into the public domain (which is beginning to happen now) facilitates the pursuit of bad hypotheses and meaningless correlations as well as sound hypotheses and meaningful correlations. It is true, finally, that the scale and complexity of such an investigation produce important periods when the researchers are so preoccupied with problems of coding, file construction, statistical procedure, computer techniques, and coordination of the whole effort that they practically lose contact with the people, events, places, and times they are studying.

These are genuine costs. Yet it strikes me as perverse to count the costs alone without considering the benefits. Fortunately, working historians pay little attention to exhortations on one side or the other. They respond instead to concrete examples of procedures for getting answers to questions they are already pursuing. The problem is simply to understand why spokesmen for the profession should so regularly emphasize the costs of computing without mentioning the benefits. The answer, I suppose, is that the critics consider the accumulation of systematic knowledge about human behavior either impossible, dangerous, of little value, or a serious diversion from other more worthy ventures.

Is History Computable?

It is not just that historians are usually impressionistic, belletristic or just plain cantankerous, although each of these is often the case. A number of traditional and legitimate historical problems simply do not lend themselves to quantification; they therefore remain unlikely prospects for work with computers. The intellectual gain from

quantification in history generally rises with 1) the complexity of the models employed, 2) the importance of variation to the arguments at hand, 3) the number of units involved, 4) the ease with which the phenomenon to be explained can itself be put into quantities. Historians, however, often find themselves trying to account for a single act of a single person by means of some general assertion about the person's character or situation. For my part, I wonder whether there is any means at all of verifying or falsifying statements of that variety. In any case, quantification is not likely to be the means.

Historical work in general has a large component of description, interpretation of texts, reconstitution of sequences, imputation of motives to actors, making of single connections, offering of judgments, drawing of moral or political conclusions. In principle, machines could do some of these things well. In practice, these capacities of computers are developing only very slowly, and historians, even historical social scientists, are doing little to encourage their development. Harry Hanham said a couple of years ago that photocopying machines had to that point exerted a far larger influence on historical practice as a whole than had computers. If he said it now, he would still be right. Photocopying machines do quickly and cheaply something that most historians are already much involved in doing: transcribing and collating texts. The everyday capacities of run-of-the-mill computer installations meet the existing needs of a far smaller group of historians. No substantial increase in the use of computers by historians is therefore likely to occur unless a) the kinds of problems and explanations with which ordinary historians concern themselves change substantially and/or b) the practical capacity of local computer installations to deal with textual analysis, cataloging indexing, sorting, sequencing, summarizing, and retrieving simply and cheaply expands to a large degree.

I consider the probabilities that either one will happen in the next ten years or so very small. There, of course, I could be egregiously wrong. The sorts of computer-based information systems now in use at Bell Telephone Laboratories could, in principle, give historians fast access to each item in a whole archive or a large library. With unlimited funds, it would not be hard to automate a good deal of the searching, storing, and retrieving which consume such a large part of the average historian's time. It won't happen soon, in my opinion, because historians lack the power, the funds, and the inclination to make it happen.

So we should distinguish between the computer in history as practiced by historians and in the historical social sciences as practiced by people from a wide variety of disciplines. It is in the historical social sciences that we should expect to find rapid increases, and some innovations, in the use of computers. Why? Because it is there that the incentives to quantification are strong, some of the essential facilities, resources and technical expertise are already available, and the attractiveness of anything that reduces the time, effort, and unit cost involved in dealing with complex analyses and large pools of data is great.

Development and History in the Social Sciences

The historical social sciences provide an important object lesson in the responsiveness of scholars to changes in the world about them. The multiplication of new states at the end of World War II turned the interests of a wide variety of Western scholars toward the elaboration of schemes intended to anticipate, and perhaps even to guide, the political, economic, and demographic changes which would take place in the non-Western world. The most popular of those schemes postulated standard paths and processes of "development." We had theories and programs for economic development, of course; but political development, demographic development, educational development, social development, urban development, and still other purported standard processes also came in for a great deal of attention in the social sciences. The models for the developmental schemes came most often from readings of Western history; Rostow's scheme for stages of eco-

nomic growth, for instance, began explicitly with an interpretation of English experience. The developmental schemes proposed in the 1940s and 1950s all turned out to have great weaknesses, both in their own terms and as tools for the analysis, anticipation, or guidance of changes in the non-Western world.

The nature of those weaknesses need not detain us here. For present purposes, the important thing is that dissatisfaction with them drove a number of social scientists back to look more closely at the conceptions of the Western experience which had, implicitly or explicitly, inserted themselves into available theories of development. This happened somewhat independently, and at different points in time, in economics, demography, political science, and other fields; but it happened very widely. The net effect of the two moves–first toward formulating theories of long-run development, then toward reexamining the fit between such theories and the historical record–reintroduced long time spans into disciplines which had been concentrating rather heavily on the short run. As Julius Rubin describes the situation in economics while discussing Ester Boserup's *Conditions of Agricultural Growth*:

> It has been a very long time since the last simple, centuries-spanning theory of economic-demographic relations was proposed. After Malthus, Ricardo and John Stuart Mill, economists turned–many with a sense of relief, no doubt– from the problems of the ages to the down-to-earth, short-term analysis of a market economy whose success and stability could be taken for granted. The problems of the long-term, of analytic history, sank into the underground of economics, with rare exceptions neglected in the universities until the Second World War War. And though after the war the renewed perception of economic development as a major social problem produced an immense amount of research and generalization, the great economic-demographic framework remained the same: neo-Malthusians are hard to distinguish from paleo-Malthusians. Mrs. Boserup has taken advantage of that research, particularly of the recent advances in our knowledge of agricultural systems, to suggest a modification of the classical framework and has thereby irritated some economists, who are sceptical of all long-term theories and large-scale frameworks, while she has given hope to historically-oriented social scientists, who are badly in need of a new Ariadne's thread (Rubin 1972: 35).

Between John Stuart Mill and Ester Boserup, to be sure, a good deal of economic history got written. Yet economists (with the important exception of Marx and his followers) generally avoided economic history and treated it as an inferior good. Only after World War II, with the new urgency and respectability acquired by the analysis of economic growth, did any substantial number of people trained primarily in economics turn back to the serious analysis of historical sequences, problems, and materials. But then it happened in a big way. It happened in such a big way, indeed, that by the early 1960s economic historians who received most of their training in history found themselves pressed hard by youngsters who spoke a mathematical language, built models, tried to unearth the buried economic assumptions in older arguments concerning such phenomena as slavery, the building of railroads, or technological change in agriculture, and employed considerably different standards of evidence from their elders. What is more, the youngsters frequently used computers to collate their data or to perform their computations.

Thus important parts of economic history became econometric history, or "cliometrics." Economists were precocious in all these regards. But similar processes created new specialties at the meeting points of history with demography, sociology, geography, and other social sciences. In all of them there was at least one moment of sharp confrontation between the oldsters who were accustomed to offering comprehensive, sympathetic, narrative accounts of their material and newcomers with their models, their jargon, their numbers, their computation–and, many oldsters said, their arrogance. Their ideas and procedures crystallized into new specialties: the historical social sciences.

Historical Demography as an Illustration

Historical demography illustrates the current situation of the historical social sciences. Demography has a reasonable claim to have been the first of the social sciences to take something like its contemporary western form. From its seventeenth-century emergence in England as Political Arithmetic, demography has recurrently dealt with historical materials and long spans of time. In the nineteenth century, however, the development of censuses and related means of collecting detailed data at particular points in time shifted the study of population away from historical concerns. As the standard data, procedures, and theories of demography crystallized, they converged on short-run processes and on the comparison of different populations at the same point in time. Even today, two-thirds or more of the average demographic textbook deals with those ahistorical matters. The substantive chapters of Barclay's standard *Techniques of Population Analysis*, for instance, cover:

- rates and ratios
- accuracy and error
- the life table
- the study of mortality
- measurement of fertility
- growth of population
- migration and the distribution of population
- manpower and working activities

The longest span of continuous observation for any particular population discussed in the book, furthermore, is ten years. Demography crystallized as a non-historical social science.

Nevertheless, a nice dialectic was working. The very accumulation of censuses in the nineteenth and twentieth centuries and the very improvement in the measurement of fertility, morality, and related processes made it increasingly clear that Western countries were undergoing long-run demographic transformations which could be plausibly related to the industrialization and urbanization of the West after 1750. In the 1920s and 1930s Western demographers formulated the idea of a standard "demographic transition" occurring country by country throughout the world. In David Eversley's neat summary:

> This "theory" shows that countries go through various stages of population change: beginning with high birth and death rates allowing a low fluctuating rate of increase (if any), they pass through a phase of increasing death control which leads to very high rates of growth, and finally into the last stage where the pressures set up by fast growth produce some control of the birth-rate which results in a considerable slowing down of the increase. Though this is exactly what happened in all Western countries some time between 1800 and 1900, and in Japan rather later, and is beginning to happen in some of the more affluent third world countries, it really tells us very little. The "transition" may last 100 years, and indeed in some countries we do not yet know whether it will ever occur at all, or whether their problems will not after all be "solved" by Malthusian disasters (Eversley 1971: 1151).

The last two or three decades of effort to refine this argument, check its applicability to the actual patterns of change in particular Western countries, verify the alternative explanations conventionally given for the declines in fertility and mortality, and judge whether and how such a transition is likely to occur in the rest of the world have shaken demographers' confidence in all simple versions of the theory. They have not yet produced an acceptable substitute. But they have stimulated an important series of investigations in the demographic history of England, France, and a half-dozen other countries, mainly in western Europe. These investigations have created a new discipline: historical demography.

The computer played no important part in the creation of the new discipline. (As we shall see later, however, it is playing an important part in the discipline's current work.) Historical demography grew apart from demography, economics, and history by refashioning elements drawn from each of them. The most significant elements, as I see them, were 1) basic descriptive schemes and models of "stable populations" drawn from demography, 2) econometric tools adopted from economics for the purpose of testing the applicability of alternative models to actual historical observations, and 3) creation of new procedures for extracting demographic measurements from registers of births, deaths, and marriages, old enumerations of population and other such bulky sources long known to historians but long neglected for lack of any effective way of exploiting them.

The invention of the procedure called "family reconstitution" probably made the largest difference. The most important contributions came from Louis Henry, a demographer at the Institut National d'Etudes Démographiques in Paris, who began with relatively little interest in history as such, but a strong desire to get at long-run population dynamics. "Family reconstitution" is one of those bright ideas which is perfectly obvious once stated. It consists of accumulating the individual, scattered records of births, deaths, and marriages occurring in a locality into family dossiers relating the events to each other. If the registration is fairly complete, if the population doesn't move too much, and if it is usually possible to match a person mentioned in a given record with a family and with other mentions of the same person, the dossiers will yield tolerably good estimates of the vital rates prevailing in the population as a whole. The record of a wedding, for example, may not include the ages of the spouses. But if we also have birth records for them, we can calculate their ages at marriage. Again, if the bride has her first child two years later, we can calculate her age at first birth without difficulty.

To the extent that registration is incomplete, the population mobile, and the identification of individuals uncertain, the job gets harder and the estimates become less trustworthy. The procedure is possible in important parts of Europe from the seventeenth century onward only because almost everyone invoked religious ceremonies for births, marriages, and deaths, and the parish clergy kept comprehensive registers of the baptisms, weddings, and burials at which they officiated. In those rarer places where civil registration was equally complete before the nineteenth century, it is of course possible to follow a similar procedure.

Family reconstitution by hand is tedious. It takes a long time. It requires numerous small judgments. It produces large files. And calculating vital rates from those large files is a fairly complicated operation. This is where the computer could come in. In fact, the extensive use of the computer in family reconstitution is just beginning. Although by now a few dozen scholars, especially in France, have reconstituted the populations of individual parishes over substantial periods of time, none of them seems to have done the major part of this work by machine. The two research teams which have gotten the farthest with the analysis of multiple communities are the one directed by Louis Henry at the Institut National d'Etudes Démographiques and the collaborative venture of Peter Laslett, E. A. Wrigley, and R. S. Schofield at Cambridge University. The INED group does not employ computers for any of its main tasks. The Cambridge group is setting up its work for the computer; at this point, however, it has not completed the reconstitution of a single parish by machine. A few other research teams doing related work in Tuscany, Québec, Iceland, and Normandy have all reached about the same state: having experimental runs or successful programs for part of the whole inquiry complete, but not having put into operation a true computer-based system for family reconstitution.

One of the most interesting difficulties in setting up such a system results from the uncertainty involved in matching different records with the "same" person. The difficulty appears in all sorts of collective history, not just in family reconstitution. In his study of Boston, for example, Stephan Thernstrom calls it the Michael Murphy problem. When

dozens of Michael Murphys are born every year, and the supplementary information supplied with birth or marriage certificates is sparse, how do you decide *which* Michael Murphy got married twenty years later? What about misspellings, or variant spellings, of the same name: are Michael Murphy born in 1874 (birth record) and Michal Murphey, born in 1874 (marriage record) and Michael Murphey, 60 years old in 1935, the same person? Every one of the simple and obvious solutions is susceptible of introducing systematic errors into the analysis: dealing only with uncommon names, throwing all uncertain matches, matching with the first plausible fit, matching randomly, and so forth. Every investigator faced with the problem so far has adopted some sort of hand solution which requires subjective judgment. Here is a problem worth giving to the computer: applying an explicit set of decision rules to all such matches, tagging each completed record as to the degree of certainty in its matching, identifying all unmatchable observations and their characteristics, calculating the possible effects of different kinds of matching errors on the demographic parameters being estimated from the whole body of data. In fact, most of the research teams using computers for family reconstitution or related operations are also working seriously on computer-based solutions to exactly this set of problems.

Does all this mean that the use of the computer for historical demography is all promise and no accomplishment? No. At this moment historical demographers are using computers to collate the material from huge sources like the Florentine *catasto* of 1427, to perform a wide range of time-series analyses for the purpose of detecting the relationships between demographic and economic fluctuations, testing models proposed for the explanation of regional differences in fertility, and dozens of other purposes. Few students now learning the specialty will enter their professional lives without some competence in computing.

Perhaps this will do as a sign of the times: in 1969 appeared the first major thesis in French history for which the basic quantitative work was done by electronic computer. The book is Marcel Couturier's study of Châteaudun (a city of 5,000 to 10,000 persons southwest of Paris) from 1525 to 1789. For that study Couturier punched about 16,000 cards, each representing a single registration of a marriage, a birth, an act of apprenticeship, or some other crucial event in the life of a particular individual. He built the book along two dimensions: 1) fluctuations over time in the composition and dynamics of the entire city's population, 2) differences among major segments of the labor force in wealth, marriage patterns, and a few other characteristics at particular points in time. He did not carry on family reconstitutions, calculate differential fertility, prepare life tables or any of the other more complex demographic operations for which one might, in principle, employ the computers.

Couturier asked the computer for two main kinds of operation, one corresponding to each of the book's major dimensions. First, he asked for series of births, deaths, and other observations for the city as a whole, or for particular parts of it, over long periods of time. Second, he asked for simple two-variable cross-tabulations: occupational group of bride's father by occupational group of groom, and so on. The operations could have been performed—more laboriously, to be sure—on the old counter-sorter. On a smaller scale, French scholars have often performed the very same operations by hand. In short, Couturier employed his IBM computer as a large tabulator; that choice eased his statistical labors and probably made them more accurate, but it did not introduce any significant innovations into the actual structure of his work. Only with the current round of theses will we begin to see work in historical demography which only the computer makes possible.

In Sum

If we were to direct the same sort of survey to recent work in economic history, we would discover a routine use of more complex models, smaller data files, less emphasis on

description, and a higher ratio of computation to input and output. If we turned instead to the systematic historical study of elites, class structure, and social mobility, the prevalence of essentially descriptive work would increase; we would find computers being used primarily for collating, sorting, aggregating, and providing statistical descriptions of a large number of individual observations. *Mutatis his mutandis*, the other historical social sciences have reached roughly the same point as historical demography.

In these fields, the computer's chief impact so far has been to decrease the effort and unit cost involved in procedures for which hand procedures were already well established, and thus to increase the scale of analysis to which those procedures could be applied. The critical innovations (like family reconstitution) have so far occurred more or less independently of the computer, but the computer is making their diffusion easier. In each field, a few investigators are demanding more of the computer: building complex files with extensive cross-referencing, testing complex mathematical models against large bodies of data, simulating social processes in order to rule out assumptions which produce implausible results, performing content analyses of texts. In each field, no more than a handful of investigators learn much about computing itself; the great majority content themselves with a knowledge of prepackaged programs, with perhaps a rudimentary competence in FORTRAN, COBOL, or some other standard language for emergencies. And many are willing to let others handle their access to the computer.

There lies a danger. In these days of the computer it is easy, tempting, and relatively cheap to run large statistical analyses which are appropriate neither for the data at hand nor for the arguments which the investigator is really prepared to make. So long as the computer is being used simply to collate and describe large bodies of data, the main costs of freehandedness are likely to be wasted effort, boredom, and excessively long manuscripts.

When it comes to fitting statistical models to data, however, the potential costs are much more serious. The ease with which historical social scientists can run a hundred multiple regressions, carry out a large factor analysis, or compare every vote in a given legislature to every other one makes it easy to coax striking pseudo-results from almost any substantial collection of data. The danger is that investigators will use the easy procedures to explore the whole terrain instead of to follow a map already prepared on the basis of well-reasoned theory and previous findings. If that were to happen without a compensating strengthening of our theories of social processes and without a complementary increase in our ability to articulate and test the more complicated notions we have about those social processes, then, alas, the many critics who see the computer as the harbinger of mindless empiricism would be right.

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News and Notes

THE UNIVERSITY OF NOTRE DAME has received a grant of $220,000 from the Alfred P. Sloan Foundation to design and evaluate computer-assisted instruction (CAI) programs for two undergraduate economics courses and an undergraduate history course. The CAI programs, which will supplement lectures and replace discussion groups, will be compared to traditional instruction methods to determine if CAI is less expensive and superior in instructional quality. Dr. Robert E. Burns, associate dean of the College of Arts and Letters, and the Rev. Marvin R. O'Connell, associate professor of history, will oversee the history program.

Students receiving CAI will use computer terminals to interact with the computer in a variety of instructional exercises. In the History of Western Civilization, a two-semester freshman course, students participating in weekly computer tutorial sessions will be asked a series of questions based on course material. Depending on the correctness of each answer, the computer will reply with a clarification, correction, or explanation of why the answer was correct.

Students will be divided into CAI and non-CAI groups; the history course will include a third group receiving CAI plus videotaped lectures. The various groups will be tested and the results compared to determine which form of instruction is superior in quality. Student satisfaction with each teaching method will be evaluated and the cost of each method compared. The projects will be completed in September 1975. Further information is available from the Department of Information Services, University of Notre Dame, Notre Dame, IN 46556; phone 219/283-7367.

COMPUTERIZED INDEXING TECHNIQUES are being employed for the first time to create a single reliable catalog of feature films. *Feature Films 1921–1930,* the first volume of *The American Film Institute Catalog* (New York: Bowker, 1971), lists 6606 films with cast lists, production credits, and synopses. Exhaustive credit and subject indexes permit the user to identify all the films associated with a particular production company, actor, director, costume designer, or writer. A separate literary and dramatic source index identifies all films based on novels or plays. Since the information is all computer-accessible, the social scientist interested in pursuing specific thematic content may obtain specialized date groupings. For information on present and projected activities, write to Richard P. Krafsur, Executive Editor, American Film Institute Catalog, Kennedy Center for the Performing Arts, Washington, DC 20566 (tel. 202/833-9300).

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