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QUANTIFICATION IN HISTORY, AS SEEN FROM FRANCE

by Charles Tilly

Why should historians worry about numbers? Suppose we take our text from Lawrence Stone:

If these fleeting appearances are to be given historical significance, it is necessary to be sure that they are typical, a thing which only statistics will reveal. Political history is different, and easier. At any one time there is only one Prime Minister—if that—and at most no more than three foreign or economic policies. But a social group consists of a great mass of men, each an individual human being, and as such a partial variant from the norm. Statistical measurement is the only means of extracting a coherent pattern from the chaos of personal behaviour and of discovering which is a typical specimen and which a sport. Failure to apply such controls had led to much wild and implausible generalization about social phenomena, based upon a handful of striking or well-documented examples.[1]

All this has long been true. But before the last decade or so, extraordinarily few historians recognized it. When they wrote of a great mass of men they either brushed them into place with the grand gesture of a Macaulay or plucked a few telling cases from the record in the loving manner of an Eileen Power.

Then collective biography, in its many forms, began to take hold of historians. Often the "masses" under study were simply large elites; the analyses of the House of Commons by Namier, of Roman dignitaries by Syme, and of Chinese bureaucrats by Ping-Ti Ho fall into that class. The study of historical demography through the person-by-person analysis of genealogies or parish registers, however, follows the same general logic. So does reconstitution of patterns of social mobility from city directories, manuscript censuses, and notarial records. In fact, many of the most important innovations in historical work since World War II consist of variants of the same procedure: documenting important historical conditions, changes, or relations which are hard to detect in the experience of any particular individual by accumulating the experiences of many individuals in comparable fashion. The "individuals" may actually be firms, villages, or families, but the overall procedure of cumulating and comparing is pretty much the same.

This sort of research does not absolutely require statistical analysis. Namier's books, for example, contain only the crudest quantification. Nevertheless, once started on this path, a historian almost inevitably adopts quantification as a means of clarifying his thought, representing his argument, and summarizing his findings. The increasing availability of data-processing procedures and machines—including computers—eases and encourages the adoption of quantitative procedures.

Innovations outside of collective biography have drawn historians in the same direction. Social scientists examining political, economic, or demographic change in the contemporary world have asked what the historical record of development reveals about similar processes—some in proud confidence that mysteries of the past will yield to laws discovered in the present, others in the humbler hope that the long and often well-documented experiences of old nations will provide some means of anticipating the experiences of new ones. National income analysis, as shaped by Wesley Mitchell and Simon Kuznets, whetted the appetites of economists for series of data extending back into history. When seeking to generalize about "nation building," sociologist Reinhard Bendix undertook thoughtful qualitative comparisons of the historical experiences of Japan, India, Russia, England, France, Germany, and other western European countries. Such efforts have built up a class of adepts in the analysis of historical materials in such fields as sociology, economics, and political science. They have also stimulated historians to consider some of the same questions. Both the questions and the questioners have helped introduce the quantitative styles of these various social sciences into historical analysis.

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There is, to be sure, more to the recent rise of quantification in history than the adoption of collective biography and the quest of students of developing nations for historical analogies. For other reasons, economists have taken up some old historical problems—whether, for example, slavery was profitable in the United States before the Civil War. Sociologists have scooped up historical evidence for generalizations held to operate regardless of time or place. Aided by new technologies for the collection, storage, and processing of data, some branches of history have been able to pursue their traditional inquiries into statistics of steel production, election results, or literacy on a greatly expanded scale. Nevertheless the greatest impulses to historical quantification since World War II have come (1) from the widespread adoption of collective biography, in the broad sense, as a means of investigating historical conditions affecting large numbers of people, and (2) from the arrival among historians of outsiders trained in the analytic styles and quantitative techniques of the other social sciences and inspired by pressing questions about the long-run changes of whole societies.

In North America, important branches of historical inquiry—notably intellectual history and the history of science—have virtually escaped these influences.[2] What is more, scholars in these areas frequently hold the social sciences in fear and contempt. Within political, diplomatic, and what passes for social history, there are also plenty of historians who treat quantification as a blight. The greatest enthusiasm for, and comfort with, quantitative procedures shows up among political and social historians of Latin America, Asia, and Africa, among economic historians of most sections of the world and among that newest generation which has had the greatest exposure to the recent work of the other social sciences. North American historians are divided on the issue; the frequency with which they issue pronouncements on quantification, pro and con, attests to this. But let us suppose that the exhortation of Lawrence Stone (whose superb combination of qualitative and quantitative investigation is more widely admired than imitated) carries the day. Consider us all converts. There the work only begins.

Once persuaded that, all things considered, it is better to quantify than to remain in doubt, the historian must return to considering all things. If not why, then how and when? When and how is it worth the effort to use materials which are already in numerical form as historical evidence, or the even larger effort to draw numbers from the memoirs, letters, or bureaucratic files which are the ordinary historian's raw materials? My first general answer is: much more often and in far more ways than today's historians think. My second general answer hedges: still, it depends.

On *what* it depends is the subject of this essay. The essay will present some reflections on the kinds of problems which lend themselves to quantitative treatment, on the sources which are worth quantifying, and on the range of procedures available for doing the job. I will not, however, pre-

tend to catalog all the many varieties of quantitative material and quantitative techniques.

Worse still, I shall draw all my observations concerning these exceedingly general problems from the experiences of the modern historians of a single country, France. Let me offer a quadruple defense of that narrowness. First, I have some firsthand knowledge of French archival materials and essentially secondhand knowledge of the rest. Second, modern France has produced and preserved exceptionally rich series of some of the most eminently quantifiable kinds of sources. Third, her historians have been among the world's leaders in several crucial varieties of quantification. Fourth, some of the problems and promises of historical quantification come out most clearly when one can see the interaction of different kinds of sources from the same period and area. There is enough to analyze in France to keep us busy for quite some time.

<div align="center">WHAT ARE QUANTITATIVE PROBLEMS?</div>

Quantitative analysis strikes in unexpected places. Where it strikes depends more on the investigator's genius than on the intrinsic nature of the problem. Before André Siegfried (1. 118),* for example, writers on French politics were well aware that durable differences in voting behavior existed among France's regions and were somehow related to differences in property, religious practice, and so on. Among others, Charles Seignobos, a leading "conventional" historian, had gone on about these matters at considerable length. Siegfried's contribution was both to develop a coherent argument about the connections between social structure and political behavior and to put together three procedures:

1. Identifying and tabulating series of "typical votes" as a means of judging the political tendency of an area
2. Assembling uniform information about the politics and social structure of whole sets of areas, ranging from very small (communes) to very large (departments)
3. Undertaking systematic comparisons of those areas, mainly through the mapping of their characteristics, with the hope of identifying correspondences between political behavior and social structure

Together, the three procedures amounted to a crude but persuasive quantitative analysis.

Half a century later Paul Bois's reexamination of the same problems (1.011) challenged a number of Siegfried's comparisons and conclusions

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* Citations in this form refer to "A Selected Bibliography of Quantitative Sources for French History and French Sources for Quantitative History since 1789," by Louise and Charles Tilly (pp. 157–75 below). In this case, for example, Siegfried's *Tableau politique de la France de l'Ouest* is item 118 under heading 1, "Representative Scholarly Works Using Quantitative Sources," hence it is identified as 1.118.

by employing an essentially Siegfriedian analysis. The argument falls; the logic remains. In its time, Siegfried's *Tableau politique de la France de l'Ouest* inspired a generation or more of French scholars (the names Goguel, Dupeux, Le Bras come to mind) to take up similar crude quantitative procedures. Siegfried had recast a well-recognized problem in quantitative form and invented some workable procedures for dealing with it quantitatively. Before his intervention, there was nothing obviously quantitative about the problem; after his intervention, there was.

That is the usual course of events with quantitative analysis in history. The case of Siegfried should warn us that there is no distinct class of "quantitative problems" only more or less quantitative ways of dealing with problems. Nonetheless, some kinds of inquiry lend themselves to quantification more easily than others. In the present state of our knowledge, an historian is more likely to profit from a quantitative statement of his problem if he is concerned with a considerable number of people than if he is concerned with only one or two, if his basic question or explanation has to do with a change or a difference among groups, if that which he is seeking to explain is already in numerical form (as is the case with strike activity, birth rates, or voting in national elections), or if his argument deals with a complicated but well-identified set of interdependencies. To put it more abstractly, the gain from quantification generally rises with—

1. the number of units involved;
2. the importance of variation to the central argument;
3. the quantifiability of the phenomenon to be explained;
4. the complexity of the principal model.

The ideal case for quantification, therefore, would be the attempt to explain how some obviously quantifiable phenomenon observable for many people, households, or communes (like number of children or wealth) changed or varied during a given historical period, the explanation itself involving a half dozen specifiable features of the people, households, or communes. The worst possible case would be the attempt to account for a single act, trait, or event for a particular individual (or, for that matter, a particular nation) by means of some general characteristic of that individual.

CHARACTERISTIC QUANTITATIVE PROBLEMS

Some typical problems already faced by historians of France have quantitative edges to them. We might group them roughly under these headings:

1. Composition of particular populations
2. Group differences
3. Trends and shifts in trends
4. Paths
5. Correlations

Quantification in the French historiography of the last few decades has followed approximately this order, with population composition most often studied quantitatively, correlations least often, and the others as listed in between.

Composition

Questions about the composition of particular populations keep coming up in French history because historians necessarily single out certain groups as crucial actors at a given time and account for their acts, at least in part, in terms of the groups' enduring characteristics. The still-unresolved debate over the politics of the men within the Convention who came to be called the Girondins bears less on what they did than on who they were, whence they came, whom they represented. Sydenham's *The Girondins* (1.125) took some gingerly steps toward quantification of the problem by using several proscription lists as the means of identifying the population in question. But these steps did not take Sydenham very far. Perhaps they led him backward: he finally concluded that the population was neither well enough defined nor sufficiently homogeneous in character to justify calling the Girondins anything more definite than the presumed enemies of Robespierre.

I have begun with a dubious example in order to warn against numerical hubris. The quantitative study of populations like the Girondins, which have blurred boundaries—no clear rules of membership, no corporate identity, no obvious distinguishing marks—will always be risky. Identifying the parliamentary "monarchists" of 1791, the "republicans" of 1848, or even the "Gaullists" of 1946 presents political historians with a challenge which numbers alone cannot meet. Nonetheless, Donald Greer's useful statistical work with the official lists of émigrés and persons sentenced under the Terror (1.063; 1.064) shows that with caution and application one can make sense of such elusive populations as political refugees and victims of repression. Duncan MacRae's recent quantitative analyses of the National Assembly under the Fourth Republic (1.091), furthermore, make even the identification and dissection of parliamentary factions seem feasible.

Where the political population is well defined, the task is much easier. By now we have a number of valuable studies of the composition of different groups of French officeholders. Tudesq's analysis of the 3,500 *conseillers généraux* (members of the elected departmental assemblies) from 1840 to 1848, for example, provides information on their ages, occupations, wealth, and so on (1.131). The information comes from the standard non-quantitative sources of the collective biographer: the individual dossiers of the officeholders stored in the archives, notes of the prefects and their staffs on the elections and the elected, minutes of the *conseils,* biographical dictionaries.

The quantification consists very simply of collecting comparable informa-

tion about each councillor, classifying all the data in a standard way, and then preparing maps or frequency distributions to represent the characteristics of councils and councillors. Occasionally Tudesq undertakes a simple cross-tabulation—comparing, for instance, the characteristics of opponents and supporters of the regime in 1840. Most of the time he uses his data as but one more form of description. The data show how little the advent of manhood suffrage shook the hold of small-town notables on these honorific offices. A number of the studies of this variety, in fact, point to a greater stability of French political personnel than the *va-et-vient* of regimes has ordinarily led us to imagine. That is one of the important advantages of collective biography and related quantitative procedures: they provide alternative ways of judging the abruptness and extensiveness of changes which may appear catastrophic (or, for that matter, unimportant) when viewed from the top of the system.

It would be illuminating to extend Tudesq's procedures to communal councils, since a number of standard ideas about French political life attach importance to purely local influences. Of course, there are nearly 40,000 communes in France, as compared with only 90-odd departments, and their officials are frequently obscure enough to leave very little written about themselves. One would have to sample, perhaps using the sample of communes which the Institut National d'Etudes Démographiques has set up for the study of demographic history. Communal and departmental archives are richer in data on local administration than on anything else, so there is a reasonable chance of assembling comparable descriptions of most communal councillors over long periods of time. Again the question of how much the local officeholders changed from 1847 to 1851 (when, in a number of communes in the southeast, republican mayors who had been forced out of office by Louis Napoleon appeared at the heads of troops of rebels) would be worth examining; similar questions concerning 1830, 1870, 1940, and 1944 are still pending.

More important in the long run, studies of this kind will eventually make firmer international comparisons of political life possible. Is the problem set for us by Alexis de Tocqueville and Elie Halévy (why the French political system was less stable than many others) a false one? If it turns out that the notorious instability of the French system occurred mainly at its very top, might that not have something to do with the kinds of people who filled positions below the top? A systematic comparison of the political personnel of France, Germany, Italy, and Great Britain since 1800 would help enormously in examining the even bigger enigmas which lie behind the puzzle of French instability.

Of course, the populations studied through collective biography and related procedures need not be sets of officeholders. Tudesq himself made a far more extensive analysis of the rich men he calls the *grands notables* of France than of the *conseillers généraux*. His thesis concerning these

grands notables (whom he defines for most purposes as men paying at least 1,000 francs a year in property taxes) concentrates on their social lives and political orientations (1.132). But it places them in a framework of comparable, quantified categories. Tudesq's statistical work makes it easy to compare his two inquiries and to discover how much greater the change of political personnel in 1848 was at the national than at the departmental level—and therefore how much greater the shift away from the republicans at the national level was between 1848 and 1851.

A similar sort of quantification has been overturning established ideas about French revolutionary crowds and activists. Georges Lefebvre inspired many of the last few decades' studies of crowds and activists. Albert Soboul's *Sans-culottes parisiens en l'an II* (1.123) has been the single most influential work in this line, but by now George Rudé (1.111; 1.112), Kåre Tønnesson (1.127), Richard Cobb (1.032), David Pinkney (1.100), and Jacques Rougerie (1.109), among others, have made significant contributions to the statistical description of the formerly faceless ordinary participants in the multiple French revolution. Their work has included far more than statistics. (Richard Cobb, in particular, has frequently complained about the substitution of enumeration for understanding.) Their tallies of the occupations, residences, ages, and birthplaces of the revolutionaries have nevertheless been crucial in identifying the men with their milieus and in clearing away old notions about their marginality, criminality, and desperation.

The statistical study of ordinary activists is bound to remain controversial for some time to come, since the sources it requires come into being on two main occasions: (1) when ordinary people form or take over associations which maintain records of their proceedings, as happened in the Parisian *sections* during the early years of the revolution; and (2) when the government defines and lists them as participants in some notable collective action, most often by arresting, prosecuting, wounding, or killing them. Soboul's and Rudé's investigations have shown that the existing records are very rich and that a bright historian can do wonders with them; they have not erased the suspicion that the people who get into the record differ from those who do not. That suspicion can only be confirmed or spiked through much closer comparison of sources generated in different ways (for example, dossiers of arrestees vs. records of persons killed and wounded vs. eyewitness accounts for the June Days or the Commune).

Biases concerning those included in the records are serious when the investigator is attempting to use a statistical summary of the record descriptively—for example, as a characterization of all participants in the Stavisky riots of 1934. That is the way most of the quantifiers have worked. The biases are often less important when the point is to detect whether two groups differed significantly, as in Remi Gossez's comparison of the June Days' rebels and the troops who put them down (1.060). For example, his conclusion that the *garde mobile* came more regularly from the so-called

dangerous classes than did the rebels themselves is likely to hold up even
if the sources describing both groups underrepresent the dregs of Paris.
Likewise, regardless of whether the native Parisians arrested during the
June Days accurately represent all Parisians who somehow took part in the
rebellion, it is worth knowing that native Parisians, if arrested, were much
more likely than outsiders to be convicted and sentenced.

Table 3.1 offers an interesting comparison between arrests in Paris at the
time of the insurrection of December 1851 and at the time of the Com-
mune of 1871. It confirms the more definite working-class character of the
Commune and indicates that within the working class, the industries and
occupations absorbing unskilled workers were exceptionally well represented
among the supporters of the Commune.

Table 3.1

Arrests in Paris during Insurrection of December 1851
and Commune of 1871

Industrial category	Percentage of 2,390 persons arrested in 1851	Percentage of 31,717 persons arrested in 1871
Agricultural workers	1.3	1.3
Wood	10.5	8.8
Textiles, clothing	9.4	4.2
Shoemaking	6.9	4.7
Leathers and hides	1.1	1.2
Luxury crafts	8.3	7.6
Printing and publication	3.0	2.9
Metals	8.2	13.0
Construction	7.5	17.2
Day labor	6.2	16.4
Office workers	7.9	8.8
Domestic help, janitors, etc.	3.9	5.4
Retail trade	9.9	4.8
Professions, finance	15.9	3.7

SOURCE: Extract from table published by Jacques Rougerie (1.109,
p. 127), adapted for easier reading and corrected for a few computa-
tional errors.

That way of putting it, however, raises a question which has rarely been
adequately answered in French studies of revolutionary crowds: how do the
rebels differ from the general population? Since there were censuses in
1851 and 1866, one can attempt a comparison of the arrestees with the
Parisian labor force of the time. Table 3.2 gives numbers of workers ar-
rested per 10,000 workers in each field. The table's weaknesses illustrate the
problems of this type of quantification. To make the comparison properly,
we would want to arrive at a closer fit between Rougerie's categories and
those of the census. The brackets show cases in which we had to combine

census categories in order to match arrestees with the labor force; the dashes indicate cases in which even combining categories produced too risky a match. Some of the people Rougerie calls *journaliers* (day laborers) in 1871 almost certainly appeared in another category in the 1866 census, since his figures imply that almost half the city's day laborers were arrested. During the years from 1866 to 1871, the character of the Parisian labor force undoubtedly changed to some extent, and during the revolutionary years of 1870 and 1871 an unknown number of workers left the city.

Table 3.2

Arrests in Paris during Insurrection of
December 1851 and Commune of 1871
(Per 10,000 workers)

Industrial category	1851	1871
Agricultural workers	11	256
Textiles, clothing	85 ⎫	74
Shoemaking	7 ⎭	
Luxury crafts	48	368
Printing and publication	24	171
Metals	109	527
Leather and hides	312 ⎰	191
Wood	⎱	1143
Construction	20	248
Day labor	100	—
Office workers	—	167
Domestic help, janitors, etc.	6	134
Retail trade	437	293
Professions, finance	18	61

SOURCE: Reworking of Rougerie's data (1.109) plus labor-force data from censuses (5.003; 5.006).

Even these crude figures, however, indicate some features of the two insurrections left unidentified by the percentage distributions: the generally higher involvement of the whole range of industries in the Commune; the waning but still substantial contribution of the shopkeepers to the insurrections; the heavy participation of wood and metal workers, considering their numbers, in both 1851 and 1871; the exceptional rise in arrests within those industries employing the least skilled labor. The calculations provide a first link, if no more than that, between the insurgents and their milieus.

The trick is to compare the participants in an action with the population "at risk," as the epidemiologists would say. Calculating participation in rebellion as a rate (just as labor statisticians have long calculated participation in strikes as a series of rates by industry or locality) opens a simple path from an essentially descriptive to a somewhat more analytical use of the same quantitative data.

The quantitative analysis of population composition reappears in a far wider range of studies than we have discussed. I have just mentioned one application: the study of industrial disputes. Louis Chevalier used compositional analysis widely in his studies of migration to Paris, which are models of ingenuity and thoroughness in the matching of sources with problems (1.029). Studies of the French labor force, too, have ordinarily gone on against the backdrop of compositional analysis. It is true that most writers referring to labor force characteristics have dipped into compendia like the *Annuaire statistique;* they have not faced the problems of quantification directly. But the more detailed historical studies of workers have ordinarily required data not available in published census tables and the like. Quantitative analyses of population composition, used mainly for descriptive purposes, are a standard fixture of French historical works.

Group Differences

The systematic quantitative study of group differences is not quite so widespread as that of population composition. In French historical works it most frequently takes the form of spot maps displaying the variation of some characteristic—religious practice, literacy, cholera, leftist voting, industrial production—among the departments of France or within Paris. Since the time of Siegfried, such maps have recurred persistently in French political, social, and economic histories. The delightful little *Atlas historique de la France contemporaine 1800–1965,* constructed by MM. Bouju, Dupeux, Gérard, Lancelot, Lesourd, and Rémond, is stuffed with them. Rarely have the makers of such maps gone beyond the crudest nonquantitative attempts to get at the covariation of such characteristics. (For methods of doing so, see 2.033.) Although most French scholars are aware, for instance, of the general correspondence among the distributions of modern industry, transportation lines, and literacy, I have not found a single statistical study of the extent and form of their interdependence.

The comparison of major segments of the population defined in terms of occupation or wealth has also concerned a number of quantifiers in French history. Relying especially on records of notarized transactions and of death-duty declarations, Adeline Daumard, François Furet, and their associates in the Sixième Section of the Ecole Pratique des Hautes Etudes have been attempting to piece together the social structure of Paris during the eighteenth and nineteenth centuries. Much of their analysis has been geographic: an investigation of variations from arrondissement to arrondissement, from *quartier* to *quartier.* When they have not been mapping wealth as revealed by marriage contracts or the sale prices of dwellings as reported in the *Petites Affiches,* they have typically been examining the differences in those characteristics among the major occupational groups within the city. This procedure has brought them difficulties and criticism; no one can under-

take such comparisons without some a priori decisions as to which occupations fit together, yet nothing is more likely to excite debate than the grouping of the population into presumably homogeneous classes.

Not that French social historians abhor the idea of class; each one simply has his own idea of where and when to draw the dividing lines. It would be possible to use conventional statistical procedures like analysis of variance to examine whether variation within the categories in one classification or another was so great as to cast doubt on the assumption of homogeneity. Or it would be possible to use some single criterion like endogamy to establish the solidarity of one presumed class or another, and then open to investigation how heterogeneous the class was in other respects. So far neither of these procedures has received much serious attention.

Other forms of systematic group comparison are exceedingly rare in French historical writing. Few researchers have attempted to compare special groups like officeholders, rebels, entrepreneurs, artists, and vagabonds with the general population. No one has dealt seriously with the apparent retreat of women from French public life after the revolution, a problem which male-female comparisons would help clarify. Even such much-mooted questions as the differences between large and small industrial firms, the contrasts among the various socialist factions (Guesdistes, Allemanistes, etc.), and the variable impoverishment and mobility of agricultural workers in different sorts of communities have received almost no quantitative treatment. In the present state of technique and documentation, these questions elude pursuit at the national level. They are, nevertheless, quite promising for study at the level of the commune, department, or region.

Trends

The statistical study of trends and shifts in trends is often the first thing that comes to mind when French quantitative history is mentioned. Analysis of "la conjoncture" à la Simiand, Labrousse, and Braudel permeates French historical work. In sheer volume, reconstructions of trends in prices, fertility, crime, or living standards have been much rarer than examinations of population composition and group differences. However, a few such trend studies have had an exceptional impact.

The substantial theses of André Armengaud (on Tarn-et-Garonne, Tarn, Haute-Garonne, and Ariège) (1.005) and Georges Dupeux (on Loir-et-Cher) (1.044) illustrate that influence very well. Each man is seeking to account for the political evolution of his region from about the middle of the nineteenth century. Both books contain three main sections: an area-by-area examination of the region under study at the beginning of the period, a substantial analysis of the *conjoncture* over the entire period, and a discussion of changes in political life over the same period. Obviously the first two are supposed to provide the basic explanations for the third. While in

Armengaud's thesis demographic changes get more attention than in Dupeux's, and while Dupeux works harder to assemble the continuous series of prices, incomes, and production dear to the economic historian, the midsections of both works consist largely of attempts to fashion time series, or, at least, comparable quantitative observations spread over considerable stretches of time) from sources ranging from the census and the *Travaux statistiques de l'Administration des Mines* to mercurials, conscription registers, and tax rolls.

The reader who approaches either of these books from the viewpoint of the social sciences notices two things about the uses of the time series. First, there is no attempt to apply statistical analysis to the covariation of the different series, even in so simple a form as the correlation coefficient. Second, their point is to establish a nonquantitative proposition (most classes of the peasantry of Loir-et-Cher prospered between 1850 and 1870; property was fractionating in the Southwest after mid-century) which will later serve as an explanation of social and political life. Both writers stop short of quantitative analysis as it is ordinarily carried on in the social sciences. This may, of course, reflect no more than the prudence of historians well aware of the flaws in their data or the roughness of their arguments. But I suspect that the example set by such masters as Labrousse has stayed the hands of those who might be capable of more extensive quantitative analyses of trends.

Over the last few decades, the most striking innovations in the quantitative analysis of trends in French history have, in fact, come from outside the school of *conjoncture* and *longue durée*. The controversial national income analyses of François Perroux, Jean Marczewski, and their collaborators are one major example. The family reconstitution approach to demographic history developed by Louis Henry and his co-workers is the other.

Although less frequently debated, the second example is more of a break with past procedures than the first. It involves deliberately employing sampling procedures, squeezing numbers from apparently unquantitative sources like genealogies and parish registers, and aggregating from very small units like households and parishes to large ones like regions and the nation as a whole. The procedure has some obvious difficulties: the use of the families which stay in place to represent all families, the underenumeration of the destitute, drifters, and derelicts, and so on. Nevertheless, once the method has proved itself as a way of establishing national trends in fertility and mortality, it is likely to serve as a model for studies of social mobility, wealth, or even political participation.

Paths

As we turn to the analysis of paths, we enter historical terrain little explored by quantifiers. This includes all those phenomena which can be

represented usefully by directed graphs such as that shown in figure 3.1, showing links from A to B, B to C, B to D, D to E, and E to A, not from C to D, and so on. The spread of epidemics, crazes, rebellions, or innovations are obvious cases. Less obviously, patterns of occupational mobility, trade, intermarriage, political influence, or migration also form paths. Not long before his death, André Siegfried wrote a provocative little book called *Itinéraires de contagions, épidémies, et idéologies* (1.117). In it he elaborated the notion that population movements, epidemics, and new ideas follow the same paths around the world and are propagated in similar ways. A related, if somewhat less ambitious, idea had appeared in Georges Lefebvre's famous *Grande Peur* some thirty years before (1.081). It recurs, at least by implication, in George Rudé's *Crowd in History* (1.112). All these books include detailed maps, but none of them goes beyond maps to quantitative analysis of the correspondences among them.
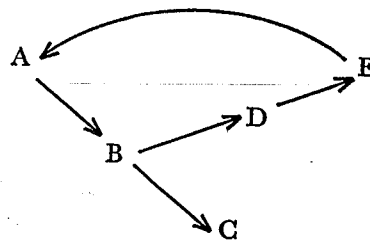

Figure 3.1

The quantitative analysis of paths has three possible applications to French history: (1) the identification of the most common, powerful, and/or persistent links in a complicated set of relations; (2) the investigation of similarities and dissimilarities among different sets of relations; (3) the study of the interdependence between the form of linkage and the behavior of the units linked. One could usefully do the first in attempting to discern the principal channels and most important nodes in a complex set of trading relations like France's grain trade or domestic textile industry. Fernand Braudel's *La Méditerranée* (1.017, e.g. 1:557–75) provides a model for the first stages of such an analysis, as it does for many other forms of quantification. The second type of application would be appropriate as a means of asking whether something like the same pattern of movement among regional cities, trading towns, smaller villages, and open countryside appeared in normal marketing arrangements and in such widespread conflicts as the grain riots of 1789, the 45-Centime Revolt of 1848, or the soi-disant rural uprisings of 1961. The third application would help in assessing the impact of the railroads on local political organization after 1840. Although writers like Georges Duveau, André Siegfried, and Maurice Agulhon have given a number of hints along these lines, no one has converted those hints into quantitative inquiry.

Correlations

The quantitative study of correlation is in about the same condition as that of paths. That comes as a bit of a surprise, since Braudel himself has declared that "to explain, then, is to locate, to imagine correlations between the metabolism of material life and the other multiple fluctuations in the life of men" (1.017, 2:213). Innumerable writers, to be sure, have detected connections between pairs of variables in French society, but almost no one has ever attempted to measure the connections themselves. For example, it is a commonplace of French political history that the electoral strength of the Radicals under the Third Republic rested especially with the small property holders. Yet we have no serious quantitative investigation of the correlation between Radical vote and fragmentation of property from time to time or from area to area. The necessary materials exist and could be analyzed for small units like cantons selected through a sampling procedure.

Most efforts to deal with such problems have been through the crude and deceptive procedure of comparing maps representing the distributions of the two phenomena in question—comparing them by eye, at that. Gordon Marker's little-known work on interregional migration in France is an exception (1.093), as is Robert Goetz-Girey's preliminary attempt to correlate fluctuations in strike activity with variations in wages and business activity (1.058). Few more exceptions are likely until (and unless) French historians develop greater statistical expertise and a stronger interest in explicit verification of statements of relationship. In this case, the main innovations are likely to come from outside history; economists, sociologists, and political scientists have the appropriate questions and technical preparation for the study of correlations—especially when it becomes a matter of relating three or more variables, instead of just a pair.

This is a pity. Much historical argument is relational: A goes with B, A causes B, A and B jointly produce C. A good many relational arguments would benefit from being brought out into the open and subjected to quantitative verification. Albert Soboul's vigorous discussion of 1848 is a case in point:

> While the peasant proprietors, the rural bourgeoisie, the great noble and bourgeois landlords, after a moment of fright, increased their political control and economic domination from 1848 to 1851, the poor peasants responded violently with a true class reflex which led them in 1851 to a genuine *prise de conscience* in favor of that republic which they had formerly misunderstood or maligned. [1.122, p. 55]

The ideas about class consciousness resist quantification, and any division of the rural population into major categories will start an agitated debate. Yet the general assertion of a relationship among wealth, property holding,

political alignment, and protest from 1848 to 1851 invites quantitative comparison. Enough records of taxation, voting, and repression exist for the period (as the theses of André Armengaud, Louis Chevalier, and Philippe Vigier have shown) to make such a comparison feasible and profitable.

This survey has focused on the quantification of questions historians are already asking themselves; the typical problems are population composition, group differences, trends, paths, and correlations. If we strayed outside our fence, we would soon stumble across the sorts of problems the "new economic historians" have claimed and have sought to treat with quantitative procedures rather more sophisticated than those mentioned: the French equivalents of the economics of slavery or the contributions of railroads to economic development. The recurring thought that declining fertility slowed economic growth in nineteenth- and twentieth-century France, for instance, might yield to cliometric examination. Still farther away we would come upon the application of complicated models of population growth, industrialization, or political development to the French experience. Does Kaldor's model produce a satisfactory approximation of French economic development? At the very end of the stroll, we might find ourselves quite outside history, in that timeless realm in which situations, persons, or events plucked from the past or the present serve as tests of general statements about social life.

Abundant materials for quantitative analyses of each of these varieties exist in and out of French archives. Whether they are actually undertaken does not depend very heavily on the availability of data or of statistical techniques; it depends on whether French historians become concerned about a different range of questions than those on which they have traditionally fixed their attention.

WHAT ARE QUANTITATIVE SOURCES?

Quantitative problems do not form a distinct class of inquiries in history. Nor do quantitative sources stand out from all others. Historians have quantified the oddest things: funerary inscriptions, baptismal certificates, parliamentary careers, blast furnaces. These phenomena have no intrinsic quantitative character. Indeed, their uses have only five important things in common:

1. They were relevant to questions concerning population composition, group differences, trends, paths, or correlations already being raised by historians.
2. There were several, or many, instances to deal with.
3. It was possible to document those instances in a somewhat comparable fashion.
4. The record appeared sufficiently complete and reliable to make the quantifying effort worthwhile.

5. The historians were able to summon up the will and the abstractions to
   treat the instances as similar in some important respects.

Only two of these conditions have to do with the sources themselves. Even
then the kind of document matters less than the character of the collection
of documents.

All these conditions vary over time and space. I have already said that
the gain from quantification of any particular historical question rises
with the number of units involved, the importance of variation to the
central argument, the quantifiability of the phenomena to be explained,
and the complexity of the principal model. Now I should add that the
questions themselves change. At present the analysis of collective biog-
raphies of participants in the great annual bicycle race, the Tour de France,
might be diverting, but it would not resolve any currently pressing his-
torical question. Fifty years ago, before Richard Cobb's writing (1.032),
who cared about the individual recruits to the militias which marched out
from Paris and Lyon in 1793? Now historians do care, and they are well
informed by Cobb's work. Questions change.

Likewise, the availability of multiple instances changes. The opening up
of the Archives Historiques de la Guerre, in the Fort de Vincennes, has
placed within historical reach the dossiers of thousands of nineteenth-
century victims of military repression. Thus Jacques Rougerie, Rémi Gos-
sez, and dozens of Parisian candidates for diplomas in modern history have
been enabled to carry out statistical studies of rebels and rebellions (cf.
2.050). Yet such studies are at present much harder to conduct for the rebels
and rebellions of small-town France, for lack of accessible documentation.
The availability of multiple instances not only changes but varies from
region to region.

The ability to document the instances in a comparable fashion also varies.
Computer technology makes it simpler to keep control of vast files in-
cluding standard sets of notations for each individual or unit (this is the
main use to which students of history have put them). But computer or no
computer, it is much easier to assemble uniform documentation when the
people who did the original recording also did some standardizing. The ef-
forts of the Office du Travail to organize the reporting of strikes—which
are by no means identical events—imposed a relatively constant form on
the materials available for the analysis of industrial conflict after 1889. No
such standardization occurred in the reporting of electoral campaigns. Every
standardization, to be sure, twists events somehow. Real strikes are subtler
and more diverse than the *Statistique des Grèves*. And practically no
standardizing procedure picks up all instances of the phenomenon it pur-
ports to deal with, or even an unbiased sample of the phenomenon.

Historians face a difficulty encountered by almost all consumers of quanti-
tative information generated by other people. The measurement arises
from the working of an institution whose activity overlaps but does not

coincide completely with the phenomenon measured. In fact, what appears to be an index of the phenomenon is more directly an index of the institution's activity. Crime statistics offer the best-known example of this difficulty. Reporting procedures have an enormous effect on the quantity of crimes recorded. If a serious theft is defined in terms of the value of the object stolen, a statistical increase in crimes of that category will occur as a consequence of an increase in wealth.

The setting of definitions is the weakest form of organizational impact on statistics. The organization can have a stronger influence on the numbers by changing the definitions so as to produce an apparent change in the phenomenon measured. But its own activity shows up most clearly in figures which actually represent the frequency with which its members carried out certain formal procedures: filing of complaints, arrests, bookings, convictions, and so on. Students of crime statistics have often noted the pressure on a patrolman to produce the "right" number of arrests for his particular assignment; they have also observed the production of "crime waves" through the temporary or permanent stepping up of the incentives for reporting infractions previously ignored or handled informally.[3] Jack Douglas (2.030; 2.031) has identified similar difficulties in the reporting of suicide; we might more confidently read suicide statistics as indexes of the willingness of the authorities to label acts of self-destruction publicly than as evidence of the frequency of self-destruction.

In a similar but less obvious fashion, production figures based on the volume of materials inspected and/or taxed by representatives of the central government (as in the case of eighteenth-century cloth manufacturing in France) respond as directly to changes in the punctiliousness of local officials as to changes in the vigor of local industry. Perhaps the hardest cases of all in which to sift out this effect of the specialists in control of an activity on the volume of activity reported are those in which measurement depends on the entry of goods or services into a market. Even if good data were available on the amount of prostitution in a country, for example, no one would presume to estimate the total level of sexual activity, because a high but variable proportion of such activity goes on outside the market. Nevertheless, we do attempt to measure variations in personal services, energy production, or field crops via the part which passes through the market. In this case, the standardizers influence the measurement at least twice: when merchants initiate and record the transactions, and when tax collectors set up procedures for dipping into the record. The standardizers, in short, enslave historians with their largesse. What we need to combat that servitude is a kind of historiography as yet ill developed: the investigation of how organizational conditions themselves affect the character of the documentation produced and available to the historian.

As a result of these processes, the completeness and reliability of the sources vary. Oskar Morgenstern's little book on errors in economic data

(2.073) will quickly sober up any historian who has drunk too deeply of numerical Nepenthe. Even in contemporary, deliberately assembled estimates of production, consumption, or income, Morgenstern points out, errors of 10 percent or more often occur. A fortiori for the estimates of Necker or of nineteenth-century prefects. The numerophilic historian often has one advantage over the economist who also works with numbers: he knows a great deal about the original sources. The classic cautions apply. For the years 1790 and 1791, we can attach greater confidence to the records of church property sales than to estimates of the revenues of the clergy, and greater confidence to those estimates than to the reports of poverty sent to the Constituent Assembly's Committee on Mendicity (see, e.g., 1.050). The first came out of a public occasion involving crucial government funds and a number of parties with an interest in a reliable public record. The second involved a sort of adversary proceeding and considerable public review. In the third case, there were few checks and a potential advantage to the liars. The historian can, of course, use the poverty reports for purposes of comparison if he is willing to assume fairly constant mendacity about mendicity, or if he is able to use the voluminous supporting notes they contained for verification. He will nevertheless want to use them with even greater delicacy than the data on the property sales.

Should we therefore forget numbers? Listen to Oskar Morgenstern:

> The weakness of econometric, mathematical models, when subjected to numerical application is *not* due to the fact that they are mathematical or that a numerical application is made. Rather we are confronted with a property of our reasoning and of our ability to observe and to measure the phenomena which we want to explain. The properly handled mathematical formulation has the virtue of showing us clearly where the limitations of our knowledge are. [2.073, p. 14]

Let that be our text. The sources vary greatly in reliability and completeness; the quantitative historian takes on a special obligation to examine and report that reliability and completeness.

Finally, the acceptable abstractions change. Gradually, if not without growling and baring of fangs, French economic historians are moving toward some sort of agreement on the possibility of, and the means of establishing, cost-of-living estimates for widely spaced points in the nineteenth and twentieth centuries as a step toward the identification of fluctuations in real wages over the entire period. The real wage is a controversial abstraction.

Again, the very first question Annie Kriegel (1.071) and Antoine Prost (1.074) take up in their separate studies of the Confédération Général du Travail after World War I and in the 1930s is: "Qu'est-ce qu'un syndiqué?" What, indeed, *was* a union member during those turbulent years? What sense can the labor historian make of times when union affiliations changed

sharply from month to month; when a system of stamps, half-stamps, and quarter-stamps made it easy for workers to "join" part-way or part-time; and when union leaders often disguised the information they did have for tactical purposes? The definition—the abstraction—turns out to affect the numbers seriously. Prost chooses to deal with persons formally enrolled and paying dues; he uses records of dues payments themselves to establish estimates of membership. (That was, in fact, close to what the officials of the CGT did when they were not manufacturing numbers for the sake of propaganda.) Very likely Prost's way of dealing with the problem, and the numbers that go with it, will serve as a standard for other examinations of union strength in France.

All this amounts to an important qualification of my initial statement. "Quantitative sources" still do not form a separate realm. A series of important conditions, however, affects whether quantitative documentation of any particular phenomenon is available or even feasible at a given time. The nature of the phenomenon probably matters less than the way its initial recording occurred and the way its historian goes about his inquiry. If there is a large set of events which the participants or the observers considered to have something in common, and especially if some organization established a routine which defined, treated, and recorded those events uniformly (as parish priests recorded marriages, registry offices recorded deeds, and city officials recorded the price of grain), the quantifier usually has an easier time of it. Indeed, under these conditions some of the material in the archives is quite likely to be in statistical form already, quantified by bureaucrats. For France since 1789, the extreme examples are census data, reports on industrial activity, vital statistics, information on crime and repression, strike materials, conscription records, and tax rolls.

QUANTITATIVE ANALYSIS AS SUCH

Suppose an historian finds some of these sources and their quantification of probable value to his work. What should he do? He cannot stick his hand into the toolbox and come up with a universal statistical wrench. An historian always faces the problem of fitting the sources to his questions, and vice versa.

Fitting them together means finding a quantitative operation which corresponds to the logic of his inquiry. That much is obvious. I feel no compulsion to lay out the full set of quantitative tools likely to help an historian—a useful task, but one which would run on to dictionary length. I wish instead to mention some of the large alternatives the quantifier has open to him, in order to emphasize how wide the range of choice is. It would be a pity if any reader who has borne with me this far came to the conclusion that he could quantify profitably only if his sources resembled those of Louis Henry, André Tudesq, or Duncan MacRae—or that in the

event that his sources did resemble those of Henry, Tudesq, or MacRae, his only choice was to use their techniques.

LEVELS OF MEASUREMENT

The first large choice has to do with levels of measurement. Teachers of elementary statistics commonly distinguish among nominal, ordinal, and interval measurement. Nominal measurement consists of placing units in mutually exclusive categories which do not form any particular order (from high to low, large to small, and so on): years, departments, industrial/commercial/administrative, invaded/not invaded. Ordinal measurement consists of placing units of ordered categories whose distance from each other is unspecified: high/medium/low, heavily industrial/moderately industrial/nonindustrial, destroyed/heavily damaged/lightly damaged/undamaged. At the extreme, there may be one category per unit, which means that they are ranked individually. Interval measurement actually states the distances between units along a continuum: prices of 2 sous, 2.5 sous, 4 sous, 8 sous; populations of 350, 650, 1,200, 850,000. Only with the third variety of measurement does it begin to make sense to calculate means, to speak of one unit as being three times as large as another, and so on.

There is nothing esoteric about these three levels of measurement. Historians have been using all of them, without the labels, for generations. But they have usually turned away from ordinal measurement, and they have almost never realized they could perform statistical analyses of data unsuitable for interval treatment but quite amenable to nominal or ordinal measurement. Even in the admirable, highly statistical work of Antoine Prost, there is a reluctance to exploit these lower levels of measurement. His data, for example, make possible a rough test of his speculation that the Communists gained strength mostly in the industrial unions into which workers rushed in response to the Popular Front excitement of the mid-1930s—that they were riding on a "syndicalisme de crise et non de longue

Table 3.3

Tendency of Union Départementale in France, 1939

Ratio of union membership in November 1938 to membership in March 1936	Communist	Balanced/ Undecided	Non-communist	Total
Less than 4	9	6	21	36
4 to 6.5	11	5	8	24
Over 6.5	7	3	12	22
Unknown	2	1	6	9
Total	29	15	47	91 [a]

SOURCE: Data taken from Antoine Prost (1.103, p. 138, maps 5, 13).
[a] Includes Territory of Belfort.

organisation" (1.103, p. 138). Table 3.3 shows the political orientation of these unions. There is some tendency for departments experiencing rapid increases in union membership to opt for Communist leadership, as Prost's argument would lead us to expect. But how strong is the relationship?

Prost's usual statistic, the product-moment correlation coefficient, will not work in this case; it requires interval measurement. The variable "tendency of Union Départementale" is measured only ordinally. There are, however, several statistics appropriate to this problem. One of them is Gamma, a measure of the similarity or dissimilarity in the orderings of two variables, which goes from −1 to 0 to +1. In this case, Gamma $=+.14$, indicating a weak relationship between the two factors but leaving a great deal of room for the operation of other variables. (In fact, a chi-squared test indicates that a relationship at least this strong could occur about one time in four by chance alone.) Clearly Prost's argument needs further scrutiny. It would be easy and useful to apply the same treatment to a number of the other nominal or ordinal variables which slip into his discussion.

DESCRIPTION AND ANALYSIS

The second large choice is between descriptive and analytic uses of quantification. Here the distinction is a bit looser than in the case of nominal, ordinal, and interval measurement, but it is no less important. Users of statistical methods commonly make a three-way division among procedures for directly describing a body of data (for example, mean, standard deviation, proportion), procedures for measuring the relationship of separate variables to each other (for example, the correlation coefficient), and procedures for inferring from a given body of data to the phenomena the data represent (t-tests, chi-squared). The latter suggests what I mean by analytic uses of quantification. The scanner of French quantitative history sees no more than wisps of inferential statistics and statistics of relationship amid the mounds of descriptive statistics. By and large, he finds the numbers used to buttress statements like "There were many Ps in Poitiers," "Q went up in Quercy from 1910 to 1920," "R was lower in Reims than in Rouen." He will have to search strenuously for quantification of statements like "S varied as a function of T" or "There is a high likelihood that U and V were negatively related to each other in France as a whole," although he will frequently encounter the qualitative versions of these statements. Normally the French quantifier lines up numerical descriptions on the way to conducting an essentially nonquantitative analysis.

An excellent case in point comes from a book I have already hailed for its thoroughness and ingenuity in quantification: Paul Bois's *Paysans de l'Ouest* (1.011, p. 349). In a section which is crucial to his argument, Bois carefully assembles data on eighteenth-century property holding in a sample of ten communes of the Sarthe. The calculations took a large effort and a

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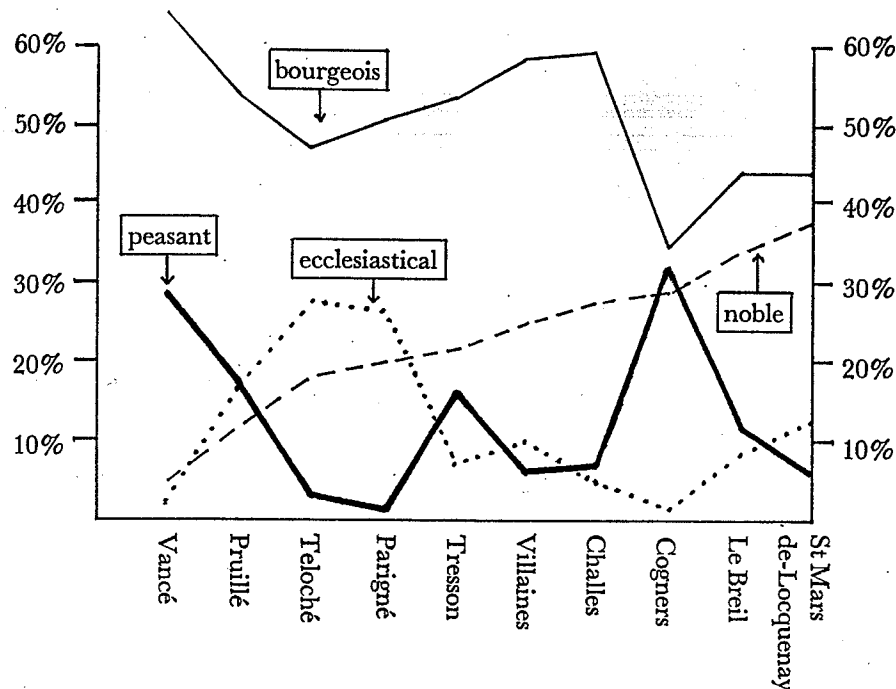lding in a sample
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Figure 3.2

great deal of knowledge of the locales. Bois sums them up in the graph in
figure 3.2. Then he turns from quantification back to a qualitative inspec-
tion of the graph:

> One can see by examining the graph that peasant property is in opposi-
> tion above all to that of the bourgeoisie. As it happens, the highest point
> for the bourgeois (Villaines, Challes) corresponds to one of the lowest
> points for the peasants. Moreover, the bourgeoisie's lowest point (Cogners)
> corresponds to the peasantry's high point, the two swings in the curve
> having the same amplitude, as if the peasantry had retained that which
> the bourgeoisie could not grab. None of the other swings is so striking.
>   One can see, next, that there is also a fairly strong opposition between
> peasant and ecclesiastical property, although not so strong as between
> peasant and bourgeois. [1.011, p. 349]

These interpretations of the graph are, to say the least, misleading. If we
calculate a rank-order correlation coefficient comparing peasant property
with each of the other three, we get the following result:

peasant: bourgeois	.04
peasant: noble	−.11
peasant: clergy	−.62

In fact, there is no association between peasant and bourgeois property, positive or negative. There is a trivial negative association between noble and peasant property. And there is quite a strong tendency for ecclesiastical property to rise as peasant property falls. By the logic of the argument, we should conclude that the real rivalry for the land was between peasants and priests. A more deliberate analytic use of statistics would have helped Bois correct and clarify this tangled section of his argument.

UNITS OF ANALYSIS

The third large choice comes with the designation of the units to be analyzed. Quantitative analysis almost always requires identifying multiple, comparable, mutually exclusive units which can be documented in a similar fashion. In our survey of French quantitative work, the units we have most frequently encountered are individuals, departments, and France as a whole (the "comparable units" in this case being different years of France's history). Such units as communes, cantons, and arrondissements come into play in detailed regional studies. And on occasion a political analyst will collect data for different departmental councils, different sessions of the Chamber of Deputies, and the like. Quantitative analysis rarely takes firms, families, associations, unions, parties, and the like as the basic units. When the units are not individuals, they tend to be geographic.

The selection of the unit emerges from a transaction between the analyst's argument and the way the relevant data are stored. The preeminence of the geographic principle in French administration, and consequently in French archives, has encouraged historians to assemble their data in terms of departments and arrondissements. This concentration on a relatively small repertoire of units has some advantages: it assures the transfer of expertise, procedures, and data from one study to the next. Its disadvantage, clearly enough, is sometimes to misalign the logic of the statistical analysis and the logic of the argument which contains it.

Agricultural historians seeking to trace the evolution of a particular natural region have perennially contended with this misalignment of administrative and agricultural areas. In his examination of the modernization of farming in the vicinity of Semur in Burgundy, Gérard Martin warns repeatedly of the approximations he has been forced to adopt:

> The study of the specialization of agriculture in the Auxois is made difficult by the lack of continuity of the documents. Often the statistics are prepared for the department or the arrondissement. But our region does not correspond to an arrondissement. It has often happened that we had a usable statistic for the whole region at a given date and could not find a comparable statistic for a later date. . . . Since the Revolution, the name Auxois stands only for an economic and geographic area, and does not correspond to any administrative unit. In his talk given in 1949, titled

> "L'Auxois, entité historique, ethnique, géographique et géologique," Louis Bourrhier defined it as a quadrilateral whose sides are made up by the valleys of the Serein on the southwest, of the Oze, continued by those of the Brenne and the Armançon on the northeast, by a hypothetical line connecting Sainte-Magnance with Buffon. But it is practically impossible to carry out an economic study of a region laid out this way; we must find the appropriate administrative divisions and, in our case, the cantonal division is crucial. The rural economy is tied directly to the earth. The Auxois is the geographical system of the Lias. We shall therefore include in our region the cantons in which Liassic terrains occupy a significant part of the territory. [1.095, pp. 87–88]

He then proceeds to list the cantons. Here we witness the transaction between argument and data right out in the open.

The problem is by no means peculiar to agricultural historians. Louis Chevalier has to stretch rather far to connect his data on suicide with the presumed disorganization of Paris under the impact of rapid population increase during the July Monarchy: his data on differentials in suicide among occupational groups deal with France as a whole, and his data on overall suicide rates deal with the department of the Seine as a whole (1.028, pp. 345–46). During the period under study, Paris's share of the Seine's population was decreasing rapidly as the suburbs grew faster than the central city. As a result, the rise in the Seine's reported rate of attempted and completed suicides from 43 per 100,000 in around 1820 to 56 per 100,000 in around 1845 would be consistent with any of the hypothetical situations given in table 3.4. These are very different circumstances. Common sense

Table 3.4

Hypothetical Breakdown of Suicide Rate in
the Seine in 1820 and 1845
(Per 100,000)

	Hypothesis A		Hypothesis B		Hypothesis C	
	Paris	Rest of Seine	Paris	Rest of Seine	Paris	Rest of Seine
1820	40	63	45	30	45	30
1845	40	110	45	93	60	42

and Louis Chevalier tell us to pick hypothesis C over the other two. Yet the risk is there. And here the fit is fairly good: Paris had about 87 percent of the Seine's population in 1820, about 77 percent in 1845. The worse the fit between units in data and argument, the greater the risk.

Sometimes there is no choice: no matter how the historian pushes and hauls, the gap between data and argument remains. I have no new solution for that difficulty. I wish only to insist that the selection of units of analysis is a major problem, which becomes more obvious and more acute with extensive quantification. For a large-scale quantitative analysis often

means long accumulation and preparation of the data, unit by unit. Initial selection of the wrong unit can cost a great deal. Nonhistorical social scientists faced with the same problem tend to assemble their data in terms of the smallest unit possible (the person, the block, the firm), tag each unit according to the larger groupings into which it falls, and thus retain the ability to aggregate and disaggregate as the analysis demands. Historians have something to learn from that procedure.

They also have something to learn from the sociologists' discussions of the problem of "ecological correlation." Sociologists have learned through hard experience that correlations which obtain at the level of a given unit do not necessarily hold for smaller units contained within it or for larger units of which it is a part. The fact that average education and income vary together closely among the neighborhoods of a metropolitan area gives us no warrant to conclude that they are just as closely related for individuals or for municipalities within the metropolis.

Although in general the quantitative historian should match argument, units analyzed, and statistical procedure as closely as possible, there are some occasions for inferring characteristics of units not observed from units observed. One is the estimate of values for the units missing from a set, as in the interpolation of labor-force figures for intercensal years or in the use of a regression equation of income on education to estimate income where the investigator has education data for an entire set of arrondissements but lacks income data for a few of them. The other is the deliberate employment of sampling to expand the range and reduce the cost of an analysis involving many units. Historians tend to be properly wary about interpolation and improperly fearful about sampling. Indeed, the only notable use of sampling procedures I have encountered in my survey of sources is INED's establishment of a national sample of communes for the exploration of demographic history. French historians could gain a great deal by tying other long-range inquiries requiring both local data and national findings to the same sample.

HANDLING THE DATA

The quantitative historian has a final large choice among ways of recording, storing, and processing his data. The choice sequence is shown in figure 3.3. Actually it is quite likely, even desirable, that the choice of quantitative procedure come first. The point of the diagram is to show that the choice of machine data-processing sets important limits on how the data can be reduced, the choice of early data reduction sets limits on the quantitative procedure, and so on.

Some of these terms may be mystifying. For most purposes, at the present time, the historian's choice between machine data processing and work by hand amounts to a decision whether to punch his information onto machine-readable cards via a standard coding procedure. If his pool of in-

formation is very small or very irregular, he may well choose to do his work by hand instead. In that case, he still faces a data-reduction problem. Often a student working in the archives takes a dossier of acts like the sales of church properties in 1791 or 1792, prepares a list of major occupational titles likely to appear in the documents, and tallies the titles as they appear. If the occupational classification turns out to be unsatisfactory, or if it turns out to make a big difference whether the buyer bid singly or with a group, the student must return to the original source for another tally. That

```
                              start
          use machine                       work
          data-                             by hand
          processing


    reduce          reduce          establish         new file
    data early      data late       reusable          for each
                                    file              operation


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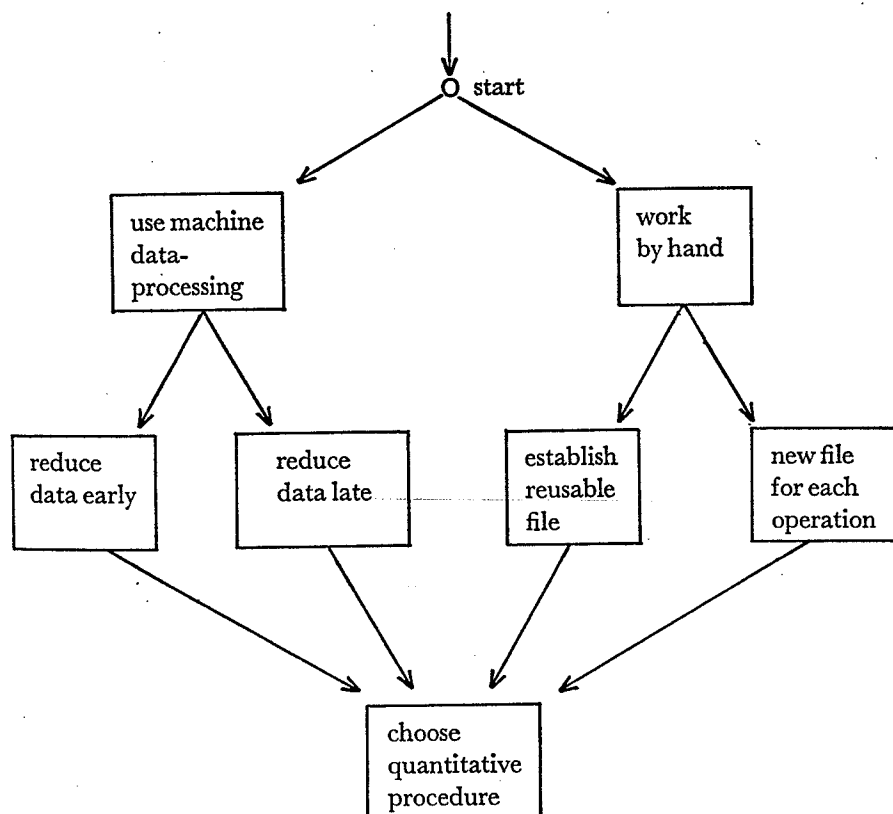
Figure 3.3

is what I mean by establishing a new file for each operation. Preparing a single sheet for each person encountered in the source and then recording each transaction involving that person on the same sheet—a fairly common procedure—has some of the same inflexibility. Even if the description of each transaction is quite detailed, the student who decides to analyze, say, the change in the average value of purchases from one period to the next will have to do much of the work over again.

A reusable file, on the other hand, commonly consists of a separate, detailed sheet or card for each of the smallest units consistently represented in the file. In the case of sales of church properties, parcels or transactions

are the likely units. The decision as to how much to record the first time is essentially an economic one, balancing the cost of transcription, the cost of returning to the source, and the probability that one will change his mind about what to ask from the data. In our own painful work, my group has found the marginal costs of recording additional items once one is already working with a source, and of maintaining the larger files required by the selection of small units, modest compared with the costs of changing one's mind later on.

If his pool of data is large, or if he plans an analysis using a number of separate items of information at once, the historian will most likely find it profitable to turn from hand to machine data processing. That presents him with a decision about the timing and character of data reduction. In the present state of technology, it is rarely practical to put an entire source into machine-readable form. The historian must sort out the parts he needs. Sometimes that means transcribing texts or part of texts onto cards or tape. In his enormous project of coding the *cahiers de doléances* of 1789, for example, Gilbert Shapiro actually places key words and abbreviated phrases on punched cards for machine interpretation.

Much more often this reduction of data means another kind of coding: placing the particular unit within a numerically tagged set of categories. Here, for example, are two excerpts from a codebook used for the recording of political disturbances in France:

CARD 82: Formation
A Note on Classifying the Industrial Group of the Formation
A large minority of our formations consist of persons from a single fairly well defined industry—masons, railroad workers, winegrowers or something else. When that is the case, find the appropriate category and code it in columns 18 and 19. If it is a combination of distinct industrial groups, code 02 ("Mixture of industrial categories") and COMMENT if possible. If there is nothing distinctive about its industrial composition (for example, if the formation is a casual crowd or a group of women and children), code 01 ("No distinct industrial category"). Only code 00 ("Insufficient information") when there is too little information in the account to make a choice among the other alternatives reasonable.

The code for columns 18 and 19 is an expanded version of the industrial classification used in the *Statistique des Grèves*. The summary which follows gives all the headings used in 1935, and places all the occupations for which strikes were reported in that year. That means a number of occupations are not on the list; but they are rare, and in any case not usually too hard to match with occupations already shown. This classification emphasizes the industry rather than the job. The code in column 20 ("Detailed occupation") will offer another chance to place the occupation, whether you know the industry or not.

cols. 18–19
NOTE: Code detailed occupations in col. 20. If there are

INDUSTRIAL GROUP OF FORMATION
00 Insufficient information
01 No distinct industrial category
02 Mixture of industrial categories: COMMENT ENCOURAGED

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11	Farm workers
12	Winegrowers
13	Forest workers
14	Fishermen
20	Extractive industries
21	Mines
22	Quarries
30	Textiles and clothing
40	Wood-working industries
41	Wood-working
42	Construction in wood
50	Metal-producing and metal-working
60	Commerce
61	Traders, wholesale merchants
62	Tradesmen
63	Retail Merchants
64	Shopkeepers
65	Peddlers, etc.
70	Liberal professions
71	Sciences, letters, arts
72	Students
73	Teachers, professors
74	Priests, monks, nuns
80	Public Services
81	Government service
82	Tax collectors, customs officers, etc.
83	Police
84	Military
90	Other industries
91	Food
92	Chemistry, rubber
93	Paper and cardboard
94	Printing and publication
95	Leathers and hides
96	Working of stones and earths
97	Transport and maintenance
99	Other: MANDATORY COMMENT

CODING IMMEDIATE BACKGROUND

Remember that this formation's participation in the disturbance begins at the moment when it starts to interact either with another formation taking part or with the object of its attack. Determine whether the formation was engaging in any collective activity back to the point of a break of six hours or more. The time from the beginning of that continuous activity to the moment of first participation in the disturbance is the "immediate background."

Out of that immediate background, code the collective activity you judge to be most relevant to the disturbance itself in columns 24–25, the next most relevant in columns 26–27. Remember that you can generalize two or more activities with the same first digit by placing a zero in the second column: picketing (32) plus petitioning (31) plus sending a deputation (33) can be coded 30. If there are three or more important activities, and devising combination codes will not solve the problem, place 90 in columns 26–27, and COMMENT.

cols. 24–27   *IMMEDIATE   BACKGROUND   OF   THIS   FORMATION'S*
              PARTICIPATION IN THE DISTURBANCE
     00   Insufficient information
     01   This formation did not exist before the disturbance began
     02   This formation existed, but was not acting collectively just
          before the disturbance began
     03   This formation existed, and was acting collectively in a way
          not directly relevant to the disturbance
     10   Peaceful meeting
       11   Harangue by a speaker
     20   Strike
       21   Sit-down, occupation of premises
     30   Presentation of demands
       31   Petitioning
       32   Picketing
       33   Sending a deputation
     40   Preparations for violence
       41   Arming
       42   Occupation of positions
       43   Show of force
       44   Awaiting arrival of forces/preparing ambush
       45   General security measures
       46   Planning insurrection
       49   Other specific preparation: MANDATORY COMMENT
     50   Obstructive measures
       51   Blocking of streets, entrances, passages
       52   Closing off a public area
       59   Other specific obstructive measures: MANDATORY
          COMMENT
     60   Organizational activity of formation
       61   Election of leaders or officers
       62   Strategy meeting
     70   Parade, ceremony, celebration, fete involving this formation
          and appealing to only a segment of the community
       71   May Day demonstration
       72   Belligerent march
       73   Demonstration
       74   Counter-demonstration
     80   Community activity
       81   Community election
       82   Patriotic holiday festivities
     90   Uncodable combinations: MANDATORY COMMENT
     99   Other uncodable collective activity: MANDATORY COM-
          MENT

When the information concerning the disturbance is adequate, the first of
these codes arouses no great controversy. It adapts one version of the
standard industrial classification used in French censuses, strike statistics,
and other official publications. It therefore lends itself to comparison with
the data in those sources. The subheadings are not exhaustive or even
mutually exclusive, as a glance at the headings under "commerce"—which
are, in the French of the actual codebook, *négociants, commerçants,*

marchands, boutiquiers, colporteurs—will show. Instead, they permit the coder to record the most common labels which appear in the sources. The whole coding scheme is a compromise among the mixed industrial-occupational codes employed in various official publications, the vocabularies of the sources with which the code works, and the needs of the analysis. But it seems to be a viable compromise. Aside from the frequent absence of information and appearance of occupationally mixed crowds, its main weakness is its failure to reflect the changes in the major divisions within the French labor force; the classification applies more neatly to 1930 than it does to 1800 or to 1960.

Many of the judgments made in accordance with the code for "immediate background," on the other hand, are bound to be contestable. That is one reason for having a COMMENT option; it permits (and sometimes requires) the coder to place verbal qualifications, explanations, and specifications on punched cards which form part of the machine record and can be recalled along with the numerical codes. Still, we cannot hope to make all these fine distinctions reliably. Nor do we use all these categories in any one analysis. They serve three purposes:

1. To make coding easier (The strain of choosing among large categories like "preparations for violence" and "obstructive measures" is rather great, and the coder who can match the situation at hand with a descriptive phrase finds the task less arduous.)
2. To make possible a wide variety of recombination of these small items, as further inquiry dictates
3. To make it feasible to recapture the detail when the analysis produces confusing or unexpected results

In short, this codebook rests on the assumption of a very late reduction of the data to the final categories to be used in the quantitative analysis. The coding of political disturbances is so novel and so risky that we could not afford to reduce all the data to stark, simple, immutable categories early in the investigation.

If the fortunate analyst knows exactly what output he will eventually demand from the computer, he can feed just the relevant items and distinctions into the machine record and forget the rest. But rare is the man who knows exactly what he will want. That is why the choice between early and late data reduction is crucial and precarious. Early data reduction is cheap, quick, neat; it tends to hide mistakes. Late data reduction is costly, cumbersome, and untidy, but it is also safer. The choice is not trivial.

CONCLUSIONS

However the data are reduced, the quantitative historian finally comes to the application of a statistical or mathematical procedure. On this weighty matter, I have only a pregnant platitude to offer: the logic of the

procedure should correspond to the logic of the argument in which it is embedded. Even in elementary descriptive statistics, each procedure has its own logic and limitations. One extreme value can easily produce an apparently important difference between the means of two distributions which are otherwise substantially the same; the median behaves rather differently. And even the simpler statistics of inference and relationship incorporate varying models of the world as well as varying conceptions of agreement, chance, or causality. There is no way the historian can saunter up to the computer, plug in his file, and walk away with an all-purpose statistical analysis in his fist. Statistics do not supplant thought; they sharpen it.

Not everyone believes that. Louis Chevalier, on whose earlier work I have relied so heavily in this survey of quantitative sources and quantification, has recently issued a series of warnings against numerology in history. "The intrusion into history," he says, "of disciplines and especially of quantitative techniques alien to history is generally accompanied by a pseudo-scientific presentation, with references to concepts borrowed from elsewhere, which cannot even render history the service rendered in the fable by the blind man to the paralytic, and by a ponderous, codified text, whose contents no one will ever check and which testifies, in the absence of scholarship, to the author's determination" (1.031, p. 797). In his recent, brilliant book, *Les parisiens*, Chevalier goes on to reject even those forms of quantification which grow from within the discipline itself. The following passage refers to a survey done in Aubervilliers:

> Finally, instead of displaying our cumbersome measurements and proofs, instead of dissecting our patients in the usual way, in horizontal slices—demographic, economic, and other kinds—and weighing the pieces on properly verified scales, we shall be satisfied to tell the tale and to choose the facts, the figures or just the responses which best summarize the conclusions of our survey on which all the researchers were agreed. It sometimes happens (for periods before our own and also at times for our own time) that the novel, especially the Parisian novel, takes the place of history and of sociological description, nothing being less fictional than some works of fiction. Likewise, contemporary social research has no chance to survive or even to exist unless it borrows its procedures from the novel or, at least, goes up to the point at which the novel begins. [1.030, pp. 43–44]

Indeed, Chevalier's *Parisiens* presents a startling contrast to his number-jammed early writings on the city; Balzac and Zola elbow out the census.

For my part, I love Balzac and Zola. *Le père Goriot* and *Germinal* will far outlive the statistical scribblings of the nineteenth century. They are both works of art and stores of acute observation of social life—thank goodness the two are not incompatible! One of the great merits of Chevalier's earlier *Classes laborieuses et classes dangereuses* (1.028), however, was to show how much inspiration Balzac, Zola, and other nineteenth-century chron-

iclers of the city drew from the statistical inquiries of their contemporaries. The inspiration is still there.

Today's historians have no doubt engaged in a certain amount of heedless, and even erroneous, quantification; my review of French quantitative work has touched on a few such extravagances. Quantification couples its benefits with large risks, and this survey has identified some of them. Yet the French experience of the last few decades points to the capacity of intelligent quantification to renew, clarify, and enrich history. We could have drawn the same lesson from recent exploits in Chinese history, where numerical description and analysis are helping students reconstruct population and social structure over vaster expanses of space and time than French historians ever dream of. Or Latin American history, where questions about racial composition, urbanization, population growth, and economic structure are compelling historians both to quantify and to broaden their conceptions of history itself, could have illustrated the promise of quantification. Although many historians remain dubious, few fields of history are now untouched by quantitative procedures.

The fundamental division of tactics and opinion among today's students of French history is not between quantifiers and qualifiers; it is between those whose inquiries begin with the questions of economics, demography, or some other generalizing discipline outside history and those who draw their quantitative questions from the logic of historical inquiry itself. I imagine that within a generation the same will be true through most of the historical profession. We must hope for a synthesis and fear a schism. In any case, we can be sure that disagreements among the proponents of both procedures will rage for years to come, reshaping historiography in France and elsewhere. The debaters will rarely debate *whether* to quantify; they will, instead, argue over what, when, how, and to what end.

NOTES

1 Lawrence Stone, *The Crisis of the Aristocracy, 1558–1641* (Oxford: Oxford University Press, Clarendon Press, 1965), pp. 3–4.
2 These bald assertions rest on data from a survey of about 600 members of twenty-nine departments of history in distinguished United States colleges and universities. The survey was conducted in 1968 under my direction for the Behavioral and Social Science Survey of the National Academy of Sciences and the Social Science Research Council. See David S. Landes and Charles Tilly, eds., *History as Social Science* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1971).
3 These difficulties of reporting, although part of the lore of criminologists, have apparently failed to get much attention from French historians of crime, judging by the work of such authors as Bercé (1.008).

Note: Bibliography for this chapter is included in "A Selected Bibliography of Quantitative Sources for French History and French Sources for Quantitative History since 1789," by Louise and Charles Tilly, pp. 157–75 below.