

### Quantitative analysis of historical material as the basis for a new cooperation between history and sociology

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## Quantitative Analysis of Historical Material as the Basis for a New Cooperation Between History and Sociology

### I. Programmatic Cooperation Versus Quantitative Analysis

There is no shortage of programmatic statements on systematic cooperation between historians and sociologists, but actual joint work has rather been impeded by just such programs. This is not a unique experience, as programs for interdisciplinary work between other disciplines often fared no better<sup>1</sup>. Interdisciplinarity as a sustained activity requires certain conditions quite different from those emphasized in many of the programmatic statements. It is the contention of this paper that these conditions now exist for the quantitative analysis of historical materials.

So far, the most important contributions to sociology were the work of scholars who as individuals were able to synthesize knowledge from sociology and history. Among the several scholars from the founding period of sociology — such as Lorenz v. Stein, Robert v. Mohl, Gustav Schmoller, Werner Sombart, Joseph Schumpeter —, Max Weber stands out as a scholar with a universal knowledge by the standards of his time who translated historical material into a basis for a systematic sociology<sup>2</sup>. Contrary to Weber's reception in the USA and from there subsequently in other countries, his colleagues in Germany saw in him more of a social historian than of a sociologist; von Wiese's reference to Max Weber in his short „History of Sociology“ as a promising empiricist and economic historian is representative<sup>3</sup>. As the knowledge of historical detail accumulates such a synthesis as an individual accomplishment

<sup>1</sup> Examples are the attempts to institutionalize interdisciplinarity between sociology and medicine, jurisprudence, and economics. Cf. Scheuch, Erwin K., *Interdisziplinäre Zusammenarbeit — aus der Sicht des Soziologen*, in: Langenbeck's Archiv der Chirurgie, No. 337, München 1974.

<sup>2</sup> Max Weber as re-imported from the United States and interpreted by Talcott Parsons is primarily the author of part of his incompleted „Wirtschaft und Gesellschaft“. Prior to this „Parsonification“ the work considered central was his sociological analysis of world religions, *Gesammelte Aufsätze zur Religionssoziologie*, 3 vols., Tübingen 1920–1921.

<sup>3</sup> Cf. von Wiese, Leopold, *Soziologie, Geschichte und Hauptprobleme*, 5th edit., Berlin 1954, p. 129 and elsewhere. As is true for many of his contemporaries, von Wiese treated the cultural philosopher Alfred Weber as the more prominent of the brothers.

becomes an obvious impossibility — safe for a selectivity and level of abstraction from details that earn such attempts the epithet „tour de force“. In such uses of universal history as by Herbert Spencer, or Oswald Spengler, or Pitirim Sorokin historical material is not really the object of an analysis but illustration for a systematic point, characteristically some form of either evolutionary or cyclical perspective of human existence<sup>4</sup>.

Actually this was the prevailing use made of history even earlier by some of the founding fathers of sociology, such Georges Sorel, or Gaetano Mosca, or Vilfredo Pareto — and certainly also Karl Marx, excepting his „18th of Brumaire of Louis Napoleon“<sup>5</sup>. This use of historical material as illustration in the guise of „proof“ contributed to the hostility of historians against sociology, which in the tradition of German historicism was expressed by such influential historians as Johann Gustav Droysen or Heinrich v. Treitschke<sup>6</sup>. In retrospect, it is specifically the use of historical material from a single systematic viewpoint — be it the eternal circulation of elites, or the oscillation between materialistic and idealistic orientation of cultures, or the trend from simple to ever increasing complexity, or history as a succession of class struggles — that makes out of „great books“ very perishable products. As knowledge of historical detail increases these great books suffer the fate that has been characterized for the natural sciences as the greatest tragedy in the life of a scholar: A beautiful idea slain by a brute fact. Courses on the history of sociology have as their main subject matter such systematic uses of historical material by universalistically educated scholars that are now merely of historical interest, and not as contributions of substantive knowledge<sup>7</sup>.

<sup>4</sup> The most important evolutionary writer for sociology has been Spencer, Herbert, *The Principles of Sociology*, 3 vols., New York 1876–1896. A very characteristic recent example of cyclical theories is Sorokin's attempt to interpret history as an oscillation between materialistic and spiritual orientation; Sorokin, Pitirim A., *Society, Culture, and Personality*, New York 1947, especially Part VI.

<sup>5</sup> The evolutionary orientation in nearly all of Marx's works is obvious, although it is not always recognized to which degree Marx chose his references to actual facts and events to fit his evolutionary scheme. In some of his comments on events of his own time, however, Marx is a historiographer — specifically in his analyses of the various uprisings in France.

<sup>6</sup> In reaction to this the school of historicism in Germany emphasized the need to understand each time by itself as a unique configuration. This historical approach had for a considerable time the function of an alternative social science to sociology. Compare Droysen, Johann Gustav, *Grundriß der Historik*, Leipzig 1868 and von Treitschke, Heinrich, *Die Gesellschaftswissenschaft. Ein kritischer Versuch*, Leipzig 1859.

<sup>7</sup> An example of this are theories on the development of the family from a presumed „natural“ condition to its current form. Cases were cited to argue for the primacy of just one form of the family, such as the primacy of group marriage by Friedrich Engels („Vom Ursprung der Familie. . .“), or of the matriarchal family by Johann Jakob Bachofen, or of the patriarchal family as argued by Edward Westermarck. As systematic information about the past and of development these „great books“ are useless.

This tendency to premature high level generalization by many of the founding fathers of sociology aside — although it is still with us and rewarded with reputation —, the synthesis as an individual accomplishment is obviously only fruitful in the early development of a discipline. This has been no different in the cooperation between other disciplines that are rich in material and conceptual apparatus. There is no way around the need for cooperation between scholars from different disciplines who contribute to this cooperation through their distinct competence, such as the specific competence of the historian in judging documents or being able to place them into context, or the skill of sociologists in data analysis. It is more problematic to which degree and especially in which way sociologists and historians can combine their respective problem formulations and conceptual apparatus — a point to which it will be necessary to return.

In view of this, the various programmatic statements for cooperation between the two disciplines are understandable — and yet they have resulted in more damage than good if they were phrased as exclusive programs rather than as one new possibility in addition to other programs (or paradigms, as it now has become fashionable to say). Examples of such exclusive programs are the demand to rewrite history as social history (at least for purposes of instruction in secondary schools), or the blanket demand for history to be practiced as an applied social science<sup>8</sup>. Add to this such ideological formulations as the request that history should from now on spotlight the downtrodden, the victims of events rather than the actors, and the call for cooperation between historians and sociologists becomes a political issue<sup>9</sup>. However, historians may be reminded that this politicization of disciplinary issues is not a consequence of „sociologisation“ but due to a more general trend that produced

<sup>8</sup> Compare Wehler, Hans-Ulrich (ed.), *Geschichte und Soziologie*, Köln 1972; Tilly, Charles, *Clio and Minerva*, in: McKinney, J. C., and Tiryakian, E. A. (eds.), *Theoretical Sociology*, New York 1970, pp. 434–466; Hobsbawm, E. J., *From Social History to the History of Society*, in: *Daedalus*, No. 100 (1971); Benson, L., *Toward the Scientific Study of History*, New York 1972. For problems resulting from such an approach see Sherif, Mustapher and Sherif, Caroline (eds.), *Interdisciplinary Relationships in the Social Sciences*, Chicago 1969. For a more pragmatic approach see the program of the International Association for Historical Social Research, *QUANTUM* (1975).

<sup>9</sup> A very pointed advocate for a new history whose heroes would be the silent masses, a history that would view events from the bottom up instead of replicating the view of the „makers“ of history, is Modell, John, *Die ‚Neue Sozialgeschichte‘ in Amerika*, in: *Geschichte und Gesellschaft*, Vol.1 (1975), pp. 155 passim. One of the most influential sociologists-plus-historians, Richard Tilly, is not free of the claim that this new history is at last real history. Cf. Tilly, Richard, and Hohorst, Gerd, *Sozialer Protest in Deutschland im 19. Jahrhundert*, in: Jarausch, Konrad (ed.), *Quantifizierung in der Geschichtswissenschaft*, Düsseldorf 1976, pp. 232–278; also Tilly, Richard, *Zum Thema*, in: *Geschichte und Gesellschaft*, 3 (1977), pp. 151–152. The ideological use of history has invaded the class rooms of secondary education and journalism. Cf. Rudolph, Hermann, *Was ist Geschichte?*, in: *Frankfurter Allgemeine Zeitung*, June 9, 1978, p. 25.

also such sects as „revisionistic“ history<sup>10</sup>. There is supreme irony in this reideologization of history and sociology alike, as it is largely based on a defunct historiography.

The currently virulent ideologies apart, programmatic requests for cooperation of the type quoted above tend to block sustained work for reasons of principle. In all these cases a follow-up of the programmatic request assumes that from now on a particular perspective, a paradigm, is shared, up to and often including a common teleology. In this day and age this may be the binding element for a sect but cannot be the universal orientation for an empirical discipline that every so often happens upon new knowledge.

The quantitative analysis of historical material may sometimes be advocated in a manner that sounds like the programs referred to earlier, and yet it is a completely different basis for cooperation. All that is required here is an agreement on a common material, and a common technology in data handling. From traditional points of view in the discipline this may presuppose both substantive and methodological decisions that are considered alien. One may object that trivial objects of trivial people are not the observational base for a history that reveals purposes to mankind or is able to provide lessons to the present. As a sociologist, these arguments are outside my proper realm of interest. The methodological argument that this quantitative history presupposes a deterministic view, however, is not; this was the central issue in one of the great methodological controversies in sociology, namely the historicism controversy. As is usual for such sweeping issues it did not get resolved but was largely forgotten — and rightly so. In order to perform quantitative analysis a deterministic view is unnecessary; it is only necessary to expect that there are also regularities in human existence which are not apparent to the actors themselves but have to be inferred. Whether this is indeed so and what strength these factors have relative to unique influences, is an empirical question. The experience so far suggests that it is worthwhile to continue this search, and be it only as a form of description that transcends any observers ability. And if some sociologists argue that with industrialization man's conditions are changed in a way that reference to previous experience is an obstacle to what is really needed, namely utopian phantasy, then again this is properly an empirical issue and not a decision immune to it. Anyway, so far the predictions of the non-utopians have been better than utopian scenarios of the immediate future.

<sup>10</sup> A central figure for the evolution of an ideologically committed history is Moore, Barrington, *Social Origins of Dictatorship and Democracy*, Boston 1966. Compare also Rothman, Stanley, Barrington Moore and the *Dialectics of Revolution*, in: *American Political Science Review*, Vol. 64 (1970), pp. 61–82, and Fogel, R. W., and Engerman, S. L. (eds.), *The Reinterpretation of American Economic History*, New York 1971. French ‚structuralism‘ is another intellectual fashion that encouraged an ideologically committed history; compare Schiwy, Günter (ed.), *Der französische Strukturalismus*, Reinbek 1969. A German variant of this plea for ideological commitment is Schmidt, Alfred, *Geschichte und Struktur*, München 1971.

Quantitative analysis of historical material provides a common empirical base for many diverse interpretations — in this way similar to such a tool as time and money budgets<sup>11</sup>. It's particular contribution is the description of diversity and the detection of regularities in so far as both transcend the observational powers of contemporaries — and this is a direct analogy to the most fruitful applications of quantitative techniques in sociology. This empirical base is open to a variety of paradigms — and emphasizing this may help to overcome some of the reservations of historians that as yet view this trend with reservations.

For sociologists a different explanation is necessary to stimulate their attention. There are, however, two traditions that impede the full use of a new, vast empirical base for their discipline.

## II. On the Evolutionary Tradition in Sociology

The topic should be unnecessary as we have it on the eminent authority of Talcott Parsons that „Herbert Spencer is dead!“<sup>12</sup> However, Herbert Spencer under different names is very much alive, indeed, and kicking for the same reasons that produced Herbert Spencers in the first place. For a while it seemed that Herbert Spencer was dead, as the motivations for the evolutionary canvasses in our disciplines had paled. Now, the interest in the course of development is high once again as the confidence in the acceptability of the future is low.

Sociology — in the form that has become professionalized — is indeed a „crisis discipline“. In the 19th century there was a wide-spread agreement that the current situation, the current societal condition, could not last. This was not to be a new form of human existence to continue but a transitory period<sup>13</sup>. Conservative observers, such as Wilhelm Heinrich v. Riehl or Lorenz v. Stein, might emphasize the features of dissolution that they saw at their time, and would accordingly choose

<sup>11</sup> This was the perspective from which Lenin advocated the collection of time and money budgets, namely as reflections of reality. This led to a specific version of empirical research in countries that officially follow Leninist principles. The most representative presentation of this research is Szalai, Alexander (ed.), *The Use of Time*, The Hague 1972.

<sup>12</sup> Cf. Parsons, Talcott, *The Structure of Social Action*, New York 1937. Later, Parsons is less certain, as is evident in the *Point of View of the Author*, in: Black, Max (ed.), *The Social Theories of Talcott Parsons*, Englewood Cliffs 1961, pp. 311–363.

<sup>13</sup> That in spite of all the protestations about his „scientism“ in charting the course of history, Karl Marx is really driven by apocalyptic vision was recognized early by Sorel, George, *La décomposition du marxisme*, Paris 1907. René König revived this understanding in *Soziologie heute*, Zürich 1949, pp. 30 *passim*, and he stimulated the work of Jakob Taubes, *Abendländische Eschatologie*, Bern 1947.

topics and perspective in empirical work. From a more radical perspective one might emphasize the direction of development and opt for a teleology, which is obvious in the works of Marx, Spencer, Comte, but also characteristic in such concepts as Ferdinand Tönnies' „Gemeinschaft“ and „Gesellschaft“<sup>14</sup>.

For sociology, the empirical basis for the construction of these teleological schemes, answering „whither are we going?“, shifted over time with material from other disciplines being dominant then. We are now used to sociologists being their own data gatherers but during this ‚heroic‘ period they relied on historical material, sometimes ethnographic material. There was a preference for historical material until about the 1870s, and as subsequently ambitious ethnographic reports became available this was the preferred material. The differences between Marx and the elder Friedrich Engels are a case in point<sup>15</sup>. To a degree both types of material were used in the same way: one would look at the past or at „primitive“ cultures as a description of origins. Hopefully, one would find a few examples of intervening conditions, and from there constructed a picture of the future.

A specific interest was the search for zero-points of human developments, those elementary forms behind which human existence did not go back, the bases from where human existence progressed. And if one had found the zero-base, one could then speculate to which degree human history was in error, impeded possibilities; not only would one be able to predict the future but to create a better one by knowing from history not only the direction but also man's unused potential. Seen from today it may be baffling to read the arguments about the originality either of the nuclear family or of group-marriages, the arguments resting on exceedingly few cases. However, this was primarily not a discussion with scholarly intentions, *sine ira et studio*, but one which had immediate ideological consequences. The writer to whom we are obliged for the very term „sociology“, Auguste Comte, consequently proceeded from an apparent concern with scholarship to the founding of an elitist sect.

<sup>14</sup> The apparent dichotomies prevalent in sociology early in this century, were frequently teleological in an extremely reduced form. This is true for Emile Durkheim's juxtaposition of mechanical vs. organic solidarity, for Ferdinand Tönnies' dichotomy „Gemeinschaft“ vs. „Gesellschaft“, and for Charles Cooley's distinction primary vs. secondary groups. The very basic concepts of sociology until the recent past implied teleologies.

<sup>15</sup> Friedrich Engels was the consumer of ethnographic material, as he was attentive to empirical material that came into his view. However, there are significant blind spots, the most important being an ignorance of the quantitative history already available at that time, such as Graunt, John, Natural and Political Observations Mentioned in a Following Index and Made Upon the Bills of Mortality, London 1662. Neither did they pay attention to quantitative research at their time, such as Le Play, M. F., *La réforme sociale en France*, Paris 1864, and Morselli, Henry, *Suicide – An Essay on Comparative Moral Statistics*, New York 1882 (Durkheim's „Suicide“ was not published until 1897!). In addition there were many more good statistical sources available than were used – as is evident from Weiss, Hilda P., *Les enquêtes ouvrières en France entre 1830 et 1848*, Paris 1936. The social sciences – or at least the part that was handed-on to the past as important – could have been far more empirical than they actually were.

Of course, the empirical base available to social scientists of this time was extremely thin. Each time when a significant new contribution of ethnographers became available the evolutionary constructs needed to be rearranged. Equally, a single case, the presumed conditions in an individual tribe, had a sensational impact provided the case fitted the preconceptions of the social scientist. An example is the publication by Henry Morgan about the Iroquois. Morgan was employed as an engineer in building a railroad to Lake Erie, and he became fascinated by the life of Indians as he was able to record it at the end of the 19th century; there are now some arguments that this was a non-typical situation for the tribe itself. Even though this was a contribution by an amateur, it was immediately used by the evolutionists such as Friedrich Engels, and even today the presumed case of the Iroquois as proof of the primacy of matriarchalism was cited uncritically by ideologists such as Ernest Bornemann.

The empirical material was in truth not an empirical base for the theory but mere illustration for preconceptions. Thus, when the cultural revolution of the sixties erupted with the dusting off of 19th century thought, the example of the earlier use of the Iroquois had a contemporary parallel. Some deservedly forgotten student-sociologist thought he had found an African tribe, the Amba, who lacked any stratification in power or authority. And significantly, a fully grown German university professor, Ralf Dahrendorf, argued the case as though it would be decisive for the question whether stratification in power is a necessary part of a developed social structure<sup>16</sup>. Contrary to the situation at the time of Henry Morgan and his report on the Iroquois, there was now ample ethnological material on the stratification in power of tribal societies, but the neo-evolutionists were not interested in this. Even if the Amba had indeed lacked any stratification in power and authority: So what?

The use of historical material by evolutionaries and neo-evolutionaries was of the same character. This was not really an interest in history as a characterization in each case of past conditions as they really were, and there was accordingly no immersion in sources. The characteristic evolutionist was and is in search of building blocks to fit his architectural design of human development. Contrasting the use of ethnographic and historical material by Johann Jakob Bachofen and Edward Westermarck with the work of Karl Wittfogel or Max Weber exemplifies the difference between using other disciplines for illustration rather than as providing an extension of the empirical base for sociology.

When Bachofen and Westermarck argued for one „original“ form of the family, then „original“ was to imply „natural“. Human history was then a formation of this natural state as a deformation, until it would be possible now to regain the natural state at a higher level of civilization. The mystical theologian Bachofen cited historical and ethnographic material, but also used legends and fairy tales, to demonstrate the primacy of matriarchalism; the historically known forms of the family, such as the classical Roman family (or rather what at that time was believed to have

<sup>16</sup> Cf. Dahrendorf, Ralf, *Amba, Amerikaner und Kommunisten – zur These der Universalität von Herrschaft*, in: Dahrendorf, R. (ed.), *Pfade aus Utopia*, München 1967, pp. 315–336.

been the Roman family) or the family of Judaism were seen as suppression of a natural state<sup>17</sup>. Parallels were maintained between the presumed suppression of women in patriarchalism and the political organisation of countries. Westermarck, too attributed a paradigmatic quality to the forms of the familial distribution of authority, although he cited historical sources for the primacy of the patriarchal family. Even though this controversy surfaced again as part of the intellectual imitations that were characteristic for the „cultural revolution“, family sociologists generally agree that searching ethnographic and historical records for a „natural“ state of human existence is futile. Evidence from research on primates makes it more likely that there was „originally“ more than one form of the family. It is characteristic for sociological evolutionism that it is clandestinely anti-historical, namely the search for non-historical conditions and the perspective of history as deformation.

In analyzing historical material on the great river-valley civilizations, Karl Wittfogel also had an ideological motivation, namely to develop a scheme for the necessary development of state socialism into a bureaucratic oligarchy<sup>18</sup>. The centrality of the single source of wealth, the river water, and the need for regulation of this resource, leads to the development of a central bureaucracy – and according to Wittfogel it does so with inevitability. Wittfogel's writings resemble classical evolutionism in his use of history in so far that historical instances of what Wittfogel calls „hydraulic civilizations“ are presented with the intent to demonstrate an inevitable development, in this case the dominance of a bureaucratic class. However, Wittfogel does attempt to work as an historian, and above all history is treated as a normal state of human existence instead of a transitory condition.

<sup>17</sup> Our understanding of the Roman family is largely a reflection of the construction of ideal types by legal historians. Even if we leave aside the question whether these legal constructs had much to do with reality – and among other indications sculptures and inscriptions on cemeteries suggest otherwise –, there were two legal forms for marriage among which the spouses could choose. The patriarchal family was the marriage „cum manu“, the essence of which was the transfer of the wife from her kin to that of her husband's, as against the marriage *sine manu* which was a contract between individuals including the right to divorce for both parties. This latter form was usual and disapproved by the Caesars – which may be the reason for historians to be largely silent about it. Even the family of Ancient Judaism was probably not an institution of despotic power as it appeared in official descriptions and in several spectacular cases in the Old Testament. At the Institute of Applied Social Research of the University of Cologne we reanalyzed the conflicts within the family that are described in the Old Testament. These descriptions were read as an indication of what they implied about the operative norms in daily life. As a result we concluded that the usual picture of Patriarchalism in Ancient Judaism referred primarily to the family as a religious unit, and in official transactions with the outside world – but not in other fields of behavior. Cf. Wurmnest, Karl Friedrich, Die Rolle des Individuums innerhalb von Familie und Ehe im Alten Testament anhand relevanter Texte unter Berücksichtigung welt- und kulturgeschichtlich bedingter Raum-Zeitstrukturen, unpublished dissertation at the Philosophische Fakultät, University of Cologne, 1978.

<sup>18</sup> Cf. Wittfogel, Karl A., *Oriental Despotism*, New York 1957. Wittfogel presents his monograph as comparative research with total societies as a unit.

We mentioned already that to his German contemporaries, Max Weber was rather an economic historian than the theorist of the first part of *Economy and Society*<sup>19</sup>. At the beginning of his career, German economic historians were analyzing their material in order to show necessary „stages“ in the development of civilizations, and to demonstrate a close relationship between an economic and a social order. This was a far cry from the evolutionism of Auguste Comte or Herbert Spencer who maintained a continuity of evolution from simple inanimated conditions to the complexity of society, an evolution that presumably was inevitable, monodirectional and monocausal. Yet Weber differed from those contemporary economic historians still further into the direction of an historian *strictu sensu*. In the central part of his work, the volumes on the sociology of religion, Weber deliberately varies civilizations in order to refute monocausal notions about the relation between „base“ (economy) and „superstructure“ (religion): Each of these civilizations has to be understood via its own „Sinn“ (approximately „meaning“), has a „Gestalt“ (approximately „shape“) of its own<sup>20</sup>. Yet Weber was also a sociologist using concepts without specific time-space meanings, and in this context he was a modified evolutionist. Weber's writings on music, on authority (an unfortunate translation of his „Herrschaft“), on science, and on bureaucracy all have one „Leitmotiv“: Why did a specific type of rationality develop only in Europe?<sup>21</sup> In pursuing these two main lines of work — their relation cannot be discussed here — Weber did not work with the conclusions of historians but with the source material itself.

<sup>19</sup> In the American reception of Max Weber the conditions under which Weber approached his monumental *Economy and Society* are largely forgotten. It is no longer possible to reconstruct a definitive version of this posthumous work, as it is likely that Weber changed his original notions several times as the work progressed. *Economy and Society* was to be in a way a contrast to his work so far as it was to present his concepts in a systematic way. This proved to be more difficult than expected as indeed the concepts were developed at different times in response to different tasks. Thus, it is simply not possible to establish a systematic relation between the taxonomies for forms of legitimate authority („reine Typen der Herrschaft“) and the taxonomy for types of action orientation („Typen des Handelns“), without creating confusion — as is indeed sometimes the case in *Economy and Society*. Cf. Scheuch, Erwin K., and Kutsch, Thomas, *Grundbegriffe der Soziologie*, 2nd edit., Stuttgart 1975, Chapter 9, Sections 1 and 2. For a new way to look at *Economy and Society* see Tenbruck, Friedrich H., *Abschied von Wirtschaft und Gesellschaft*, in: *Staatswissenschaft*, Tübingen 1978, pp. 1–34.

<sup>20</sup> The notion of „Sinn“ is central for Weber's analysis of a total system: its the attribution of ‚Leitmotivs‘ to the actions in a society. Cf. Girndt, Helmut, *Das soziale Handeln als Grundkategorie erfahrungswissenschaftlicher Soziologie*, Tübingen 1967.

<sup>21</sup> The notion of „rationality“ as a ‚Leitmotiv‘ of systems is explored in Münch, Richard, Max Webers ‚Anatomie des okzidentalen Rationalismus‘ — eine systemtheoretische Lektüre, in: *Soziale Welt*, 29 (1978), pp. 217–246. There are two more sides to Weber's work, the second of which is largely unknown today. It is better known that Weber was interested in methodological issues, as is evident e. g. in Weber, Max, *Methodologische Schriften*, Frankfurt 1968 (a collection), but he was also a passionate commentator on political developments. Cf. Weber, Max, *Gesammelte politische Schriften*, 2nd edit., Tübingen 1958.

Even the second accent of Weber's work could not be replicated today. There may be a revival of evolutionism in intellectual life, but only in the sense of a philosophical exploitation of historical generalizations and not in the sense of a use of historical sources to construct laws of development. None of the grand conclusions of the evolutionists stood the test of time, and it is unlikely that the neo-evolutionists will fare better. There is now such a wealth of empirical evidence, and the movement of quantitative history increases the volume still further, that a simple ordering whether in „stages of development“ or in cycles is no longer feasible. The publications of Shmuel Eisenstadt demonstrate that historiography and social science can still be combined in the grand style, but the accent is on comparativism and definitely not on evolution<sup>22</sup>. A sociology that hopes to regain the courage to sweeping theories of the 19th century, a sociology that looks upon history as an opportunity to revive evolutionism, misses the specific usefulness of the current meeting of sociology and history. The description of everyday life and mass events in the past that now becomes possible, definitely does not lend itself to a type of theoretizing in the evolutionary tradition. Although at first sight the assertion may seem paradoxical, it nevertheless can be argued that structural-functional theoretizing is more compatible with the data from quantitative history.

### III. Is Functionalism Necessary Anti-Historical?

This is only in part a rhetorical question, as there is no unequivocal answer: there is no necessary conflict between a structural-functional kind of theoretizing and history, but in practice this is so. This is probably due to the development of structural-functionalism in the United States. Be that as it may, structural-functionalism has been so dominant a mode of theoretizing since the middle forties until the middle sixties that it became synonymous with general sociological theory. In practice, this kind of theory prided itself in formulating general sentences without time-space referents, was general theory in line with the introductory part of Weber's *Economy and Society* and not with his other writing. It was usual amongst sociologists to understand this mode of formulating as following the example of the successful natural sciences — and that meant largely physics.

This is, however, a misunderstanding. The discipline in the natural sciences closest to structural-functionalism in sociology is, at least in the case of Parsons, rather biology. Biology, that is a discipline which has to do with reactive systems, and in

<sup>22</sup> A good introduction to his approach is Eisenstadt, S. N. (ed.), *The Decline of Empires*, Englewood Cliffs 1967. Much more ambitious is Eisenstadt, S. N. (ed.), *Political Sociology*, New York 1971, which in spite of its title is predominantly a book with historical comparisons, albeit of a non-quantitative character.

this sense it is contrary to some of the classical sciences. Here, the object is not „cause“ and „effect“ but „effect and counter-effect“. At any given time, an object or process may serve more than one function, or the same object or process may serve different functions at different times. A biological organism as an object of explanation is a vastly more complicated thing than the inanimate nature. Society as well, if structural-functionalism is properly practiced, is treated as a reactive system and not in an analogy to inanimate nature.

This would be complicated enough, but in addition there is an unnecessary problem in the functionalism as it is actually practiced. It becomes most apparent in what is called „Systems Analysis“. In this approach it is assumed or implied that basically all parts of a system are necessarily cooperating and that they react tightly together. This is completely unnecessary to assume since there are parts in the body too, which are unnecessary, not everything is directed to the same purpose. There are countervailing processes, functional substitutes in addition to fixed organs, and a lot of give-and-take, i. e. looseness between organs and parts of a body. However, systems theory as a specific form of structural functionalism in its actual practice assumed a direct reaction of all parts of a system to each other<sup>23</sup>.

The conceptual apparatus, the research problems and the empirical research connected with these approaches found its purest expression in small-group research<sup>24</sup>. Indeed, small-group research has as an object something that does not really exist but is constituted as an construct — and yet this research was to a degree successful in finding universals that eluded sociologists in many other areas. Yes, sociology has developed universal sentences about human behavior that can be applied in a variety of contexts. This copy of physics was not a story of complete failure, unfortunately it is also not a story of a large scale success. As sociologists moved beyond the micro level it became much harder to justify time-space free sentences in terms of „X“ being a function of „Y“. What stood sociologists in good stead, namely the type of conceptual apparatus, the type of methodology and specifically the type of interpretation when they worked with the immediately observable, was much less successful when they had to work with indicators and the proof had to be inferential. Most macro phenomena are of an inferential nature. This became even more important and more obvious when cross-cultural research became important.

<sup>23</sup> The most prominent representative of this kind of systems theory in Germany is today Niklas Luhmann; cf. Luhmann, Niklas (ed.), *Soziologische Aufklärung — Aufsätze zur Theorie sozialer Systeme*, Opladen 1970; also *Zweckbegriff und Systemrationalität*, Tübingen 1968; also *Zur systemtheoretischen Konstruktion von Evolution*, in: Lepsius, Rainer (ed.), *Zwischenbilanz der Soziologie*, Stuttgart 1976, pp. 37–48; also *Generalized Media and the Problem of Contingency*, in: Loubser, Jan J., et al. (eds.), *Explorations in General Theory in the Social Sciences*, New York 1974.

<sup>24</sup> A recent overview of the whole field is Schneider, H. D., *Kleingruppenforschung*, Stuttgart 1975. For the self understanding of this approach see Bales, Robert Fred, *Personality and Interpersonal Behavior*, New York 1970. The artificial character of the whole field is criticized by Sorokin, Pitirim A., *Fads and Foibles in Modern Sociology and Related Sciences*, Chicago 1956.

As structural functionalism has been a part-success, as there are areas which can be shown as models to other disciplines, as the methodology works very fine, this partial success tends to somewhat impede the openness in turning to such a vast new area of material as becomes available to us in quantitative history. Especially, the part-success tends to inhibit a re-examination whether structural-functionalism needs to be practiced in the way that prevailed up to now.

#### IV. Empirical Sociology Experiences Limits

At the end of a period of more than thirty years of development in empirical sociology, there is now some soul searching and attempts at stock taking<sup>25</sup>. This was in many ways a most successful period: in some fields general „laws“ akin to those of physics were identified; the methodology for the social sciences in general was furthered and became an export article even to those who voiced programmatic reservations against „positivistic“ sociology; and a vast amount of descriptive knowledge was accumulated. Methodology and social description could be so standardised that they could be the base for a service industry that now produces vast quantities of facts. Increasingly, social scientists begin to tap the additional vast data resources that come into being as a side-product of public and private bureaucracies<sup>26</sup>. Now that we are relatively data rich, we begin to feel just as those rich in other proper-

<sup>25</sup> Examples of this self doubt, coupled with the desire to retain the claim to be at the same time a science and a tool of the Enlightenment are Birnbaum, Norman, *The Crisis of Industrial Society*, London 1969; Gouldner, Alvin W., *The Coming Crisis of Western Sociology*, New York 1970; Dahrendorf, Ralf, *Die Soziologie und der Soziologe*, in: Hess, Gerhard (ed.), *Konstanzer Universitätsreden*, no year. It is instructive to compare these diagnoses with the actual work presented at the 17th German „Soziologentag“ in 1974, presumably a crisis year if judged by public appearances: During the sociological convention routinized science (in the sense of Th. Kuhn) prevailed. See Lepsius, *Zwischenbilanz*. See also Scheuch, Erwin K., *Die wechselnde Datenbasis der Soziologie. Zur Interaktion zwischen Theorie und Empirie*, in: Müller, Paul J. (ed.), *Die Analyse prozeß-produzierter Daten*, Stuttgart 1977, pp. 5–41.

<sup>26</sup> An over-all view of this vast area is Wilcox, Lesly D., et al. (eds.), *Social Indicators and Societal Monitoring*, Amsterdam 1972. So far social scientists use only fractions of the material existing, as can be inferred from Statistisches Bundesamt, *Das Arbeitsgebiet der Bundesstatistik 1976*, Stuttgart 1976. Currently, the chief interest in using these process-produced or officially collected data is their appropriateness for societal monitoring, as explained in Zapf, Wolfgang (ed.), *Sozialberichterstattung – Möglichkeiten und Probleme*, Göttingen 1976. See also Krupp, Hans-Jürgen, and Zapf, Wolfgang, *Sozialpolitik und Sozialberichterstattung*, Frankfurt 1977, for a characterization of the most important research unit in this field in Germany, SPES: Zapf, Wolfgang (ed.), *Soziale Indikatoren*, 3 vols., Frankfurt 1974–1975. This field is internationalizing fast, as can be derived from international data collections such as EUROSTAT: Social

ties presumably do: it is great to be rich but it satisfies a lot less than expected. Many of the facts and figures are suspected to be less informative than we thought at a time when each new fact or figure possessed a novelty value.

To give one example of considerable personal importance. During the fifties it was empirically demonstrated again and again that one large difference between mass opinion in Europe and in the United States was what political scientists conceptualized as „system trust“. Europeans were shown to be highly sceptical about their politicians, their political parties, and sometimes also of all of the political system. In contrast, respondents in the United States expressed an unshakable respect for the office of the president and the institution of the two-party-system, even when they detested a particular president or found their two political parties at a given time to be in terrible shape. Just as they were reputedly cynical about morals, these Europeans were called political cynics, and American political scientists concluded that this was not a condition in which a meaningful democracy could flourish. Now that we count the year five post-Watergate the trust of Americans in their political institutions is below that which opinion researchers now report for European countries<sup>27</sup>. What did we measure some thirty years ago: Was it really an aspect of a distinct political structure, or merely a mood? And do changes in mood matter very much in the operation of a political system?<sup>28</sup>

In looking back at over thirty years of data collection we can observe both high stability of differences between countries and groups within a country for some subject matter, and great changes up to fickleness of figures in other areas. In the field of leisure we have witnessed a high instability of behavior, and this is currently especially true in research on tourism<sup>29</sup>. Research on sexual matters has shown a tremendous instability in beliefs and opinions, and far more stability than instability in behavior. Currently, there is in Germany a debate whether we witness a major change in values amongst youth – the school of „post industrialism“ believes that this is so –, and whether the traditional work ethic is falling apart; it is by no means clear what the figures really do indicate. Where do we measure a structural property, where do we record a mere transitional state? Sociologists are becoming – albeit a bit too slow – more careful in interpreting numbers.

Indicators for the European Community, Luxemburg 1977; and there is also now an international newsheet: Social Indicators Newsletter, Social Science Research Council, New York. While much of this work is pure induction, there are attempts to develop a rationale as in Fox, Karl A., *Social Indicators and Social Theory*, New York 1974; OECD, *Measuring Social Well-Being*, Paris 1976. Decisive for the expansion of basic research using these resources will be the development of an appropriate infrastructure of data services, as reported by Rokkan, Stein, *Data Services in Western Europe – Reflections on Variations in the Conditions of Academic Institution-Building*, in: *American Behavioral Scientist*, 19 (1976), pp. 443–454.

<sup>27</sup> Cf. Huntington, Samuel, et al., *The Crisis of Western Democracy*, New York 1976.

<sup>28</sup> Critical of the literature on the loss of governability is Scheuch, Erwin K., *Wird die Bundesrepublik unregierbar?*, AGV Metall, Köln 1976.

<sup>29</sup> See Scheuch, Erwin K., and Scherhorn, Gerhard, *Soziologie der Freizeit und des Konsums*, in: König, Rene (ed.), *Handbuch der empirischen Sozialforschung*, Vol. 11, Stuttgart 1977.

Empiricism was quite successful in providing a basis for micro sociology. Macro sociology, however, did not progress in the way it was hoped. In Germany it was especially a group of sociologists sometimes called by others the „Cologne School“ that had advocated cross-level analysis and corresponding data collection as the methodology appropriate for macro sociology<sup>30</sup>. However, the pay-off of this theoretically sound notion has been far less than hoped for<sup>31</sup>. Whether this is due to the empirical research, or the conceptualization of it, or the far greater complexity of an empirically founded macro sociology is an open question.

Research tools are proliferating at a very rapid rate. Techniques that were known for a long time but little used, such as complicated sampling techniques for subgroups of large populations, or techniques of content analysis — are now being actually used. The machinery of large scale electronic data processing is important in turning esoteric knowledge into practical procedures. There are many original ideas in developing so-called unobtrusive techniques i. e. highly inferential measures independent of verbal statements<sup>32</sup>. And in general, there is a greater willingness to combine measurements from several sources: Sociologists may become as critical of their data as historians reputedly are of their sources. On the other hand with the explosive growth of analysis opportunities there has been a tendency to overanalyze some data. The debate about weak versus strong measurement indicates that there has been an unthinking preference for the most powerful statistical techniques regardless of the level of measurement and the reliability of a figure<sup>33</sup>. The latter is a tendency that quantitative historians should better watch.

<sup>30</sup> An overview is given in H. J. Hummel, an adherent of the „Cologne School“, *Probleme der Mehrebenen Analyse*, Stuttgart 1972. A very optimistic expectation was formulated during the sixteenth German „Soziologentag“ in Frankfurt 1968 by Scheuch, Erwin K., *Methodische Probleme gesamtgesellschaftlicher Analysen*, in: Adorno, Theodor W. (ed.), *Spätkapitalismus oder Industriegesellschaft?*, Stuttgart 1969, pp. 153–182.

<sup>31</sup> Massive secondary analysis of data on voting behavior by Franz U. Pappi found in the end that including contextual variables added very little to the explanatory power of the routine individual variables; cf. Pappi, Franz Urban, *Sozialstruktur und politische Konflikte in der Bundesrepublik. Individual- und Kontextanalysen der Wahlenentscheidung* (in publication).

<sup>32</sup> The „classical“ source on unobtrusive techniques is Webb, Eugene, et al., *Unobtrusive Measures*, Chicago 1966. A prerequisite for the large scale use of quantitative content analysis is their combination with sampling techniques; see Kops, Manfred, *Auswahlverfahren in der Inhaltsanalyse*, Meisenheim a. G. 1977. Important contributions to methodology that are especially useful for quantifying and analyzing historical material are Steinhausen, Detlef, and Langer, Klaus, *Clusteranalyse*, Berlin 1977, and Sodeur, Wolfgang, *Empirische Verfahren zur Klassifikation*, Stuttgart 1974. An overview of research techniques that includes advanced methods relevant to quantitative history yet accessible to the non-specialist in methodology is van Koolwijk, Jürgen, and Wicken-Mayser, Maria (eds.), *Techniken der empirischen Sozialforschung*, vols. 2–7, München 1974–1977.

<sup>33</sup> For the debate on the level of measurement appropriate to the data see Scheuch, Erwin K., *Forschungstechniken als Teil der Soziologie heute*, in: Lepsius, *Zwischenbilanz*, especially pp. 94 *passim*. Compare also Acock, Alan C., and Martin, David, *The Undermeasurement Controversy*, in: *Sociology and Social Research*, 58 (1974), pp. 427 *passim*.

There is now some better understanding of what John Stuart Mill meant when he argued that the social sciences were „observational“, and why Emile Durkheim was anti-experimental. One does not have to reject the experiment as a tool of research in order to sympathize with the notions about the character of social systems that lead to the anti-experimentalism of Mills and Durkheim. Many social phenomena have meaning depending on contexts, are interconnected and multifunctional. Even elementary activities such as eating or sexual intercourse carry several meanings! Social processes are both over- and underdetermined. Relating single variables to each other does usually not do justice to the structure of social phenomena, and with the realization of this condition, analysis techniques are being developed that are more appropriate to the interconnectedness and multidimensionality of social phenomena. Path analysis, causal analysis and LISREL are examples for this trend<sup>34</sup>.

It is doubtful that the limits in explanation which empirical sociologists now sometimes encounter can be overcome solely by further analysis techniques, and a more systematic combination of data. For many problems longer periods of observation are required, and an extension of conditions under which behavior is observed. Quantitative history can provide this extension of the data base for sociology — not so much in quantity but more importantly in quality. In turn, the response of sociology to the multicollineality of relations between variables, the reaction to the multidimensionality of social phenomena, means that today sociologists can offer much more adequate techniques of data handling than would have been possible only ten years ago.

The recent meeting of historians with sociologists, in Germany connected with such names as Hans-Ulrich Wehler and Hans Mommsen, has not necessarily been the most helpful experience<sup>35</sup>. These historians hoped to borrow concepts and generalizations from sociology to regain a larger scope for the discipline of history that appeared to be bogged down into historiographic details. This was an inopportune time to do so, leaving aside the question whether there was ever an opportune time for this.

It was a time when many of us realized that our concepts were more time-and-space-bound than we had so far suspected. For some sociologists it was also a time for a „paradigm change“ — away from systems analysis with its harmonistic view of biology. We now understand biology in a very different way, namely as the discipline of imperfectly constructed beings, as of organisms that side-by-side are characterized by surpluses and deficiencies. Real social systems are evidently imperfectly integra-

<sup>34</sup> Cf. Ziegler, Rolf, *Theorie und Modell*, München 1972; also Blalock, Hubert M., *Causal Inferences in Nonexperimental Research*, Chapel Hill 1964; also Weede, Erich, *Hypothesen, Gleichungen und Daten*, Kronberg Ts. 1977.

<sup>35</sup> As a source for this approach that could be called the sociologization of history instead of the shared use of historical data, consult Wehler, *Geschichte und Soziologie*. By now there are side-by-side several forms of cooperation between sociology and history, as is evident from the range of contributions in Ludz, Peter Christian (ed.), *Soziologie und Sozialgeschichte*, Special issue No. 16, *Kölner Zeitschrift für Soziologie und Sozialpsychologie* (1972) — especially the introduction by the editor.

ted, and by now it is no longer very easy to say what the boundary of the systems is that we are analyzing. We cannot simply use national boundaries as being also system boundaries, as the nation state is coming apart as the highest level of integration. Devolution within nation states and international connectedness make the nation state level just one of several levels that indicated system boundaries. This is an intellectually richer and more flexible sociology, but it is certainly not one from which one could easily borrow ready concepts and generalizations.

## V. Quantitative Analysis of Historical Material as an Extension of Comparativism

A more fruitful orientation in seeking a cooperation between sociology and history is cooperation in exploiting a new data base. Time budget research offers an example for the character of such a research. The use of time is a social indicator lending itself to several interpretations, an indicator that can be put to many uses<sup>36</sup>. In some of the socialist countries, time budget data are employed for such engineering purposes as the calculation of waste times, while the very same data are used by Western social scientists to identify the networks of daily intercourse. Many of the data of quantitative history have the same indeterminate character as time budgets have. Viewed methodologically, most analyses of quantitative history have the character rather of secondary analysis than of primary analysis<sup>37</sup>. This may often cause problems in interpretation, but it does also facilitate cooperation between scholars from different disciplines and with different approaches: They do not need to agree on problem formulations, or concepts. Thus, in looking at quantitative history as an opportunity for secondary analyses of vast quantities of data about previously inaccessible topics and subjects, the pitfalls of the above mentioned approach — the Wehler-Mommsen problem — is avoided.

It is dangerous when sociologists by themselves quantify and analyze historical data, as they usually lack the familiarity with the contexts of these data; and it is no less hazardous if historians feel confident to order high powered statistics from the now easy-to-use packages. But cooperation between sociologists and historians properly goes beyond such a symbiosis in research technology. Historians rightfully expect that quantitative history will give new impetus to history as a generalizing

<sup>36</sup> Cf. Szalai, *The Use of Time*.

<sup>37</sup> „Secondary“ does not imply „second class“ but denotes a use of data different from the intentions with which the data were collected. The classical source on the methodological issues in secondary analysis is Hyman, Herbert, *Secondary Analysis of Sample Surveys — Principles, Procedures, and Potentialities*, New York 1972.

discipline, and sociologists hope for a vast extension of their empirical base. In this latter sense the use of historical data is a form of comparativism, is observation under varying conditions in the sense that John Stuart Mills argued for „observational“ social sciences. This form of comparativism complements and extends significantly what currently is being done in comparative social research.

One of the important resources for sociological comparativism has always been — earlier more so than during the last decades — ethnology, and here a development analogous to that now in quantitative history occurred much earlier. A group of ethnologists around John Peter Murdock from Yale translated the ethnographic reports of their time into a common scheme<sup>38</sup>. This meant among other things that checklists had to be developed for institutions and fields of behavior as a prerequisite for the coding of ethnographic descriptions. Methodologically, this implied the translation of descriptive accounts into configurations of variables. Only through this „translation“ becomes it possible to develop a quantitative ethnology on a world scale as though the descriptive accounts had been questionnaires about cultures: Frequencies are identified, correlations are computed, factor analyses are meant to show hidden communalities. By now the „Human Relations Area File“ (HRAF) is in part machine readable, and available in several countries. While this increases its accessibility, and makes comparative ethnography something every graduate student can practice, the decisive step was not the machinery but the „translation“ of the narratives. The organization of data sets from projects in quantitative history could do the same for historical data<sup>39</sup>.

It would be of considerable greater consequence. The Human Relations Area File has data from more than 500 cultures, and while there are greater variations between, the hundreds of simpler cultures, they remain simple cultures that are of limited relevance for the understanding of a complex modern society. Even though the volume of quantitative history has been limited, at least as compared to quantitative ethnology, its impact for social science has been far greater<sup>40</sup>. The conditions

<sup>38</sup> Cf. Murdock, George Peter, *Social Structure*, New York 1949; also *World Ethnographic Sample*, in: Moore, F. W. (ed.), *Readings in Cross-Cultural Methodology*, New Haven 1961.

<sup>39</sup> A survey in Germany showed that in 1977 there were more than two hundred machine readable data sets with quantified historical information; compare Bick, Wolfgang, et al., *Quantitative historische Forschung 1977*, Stuttgart 1977. (= *Historisch Sozialwissenschaftliche Forschungen*, Vol. 1). See also in the same series which is issued in cooperation with the International Association for Historical Social Research, *QUANTUM: Best, Heinrich, und Mann, Reinhard* (eds.), *Quantitative Methoden in der historisch-sozialwissenschaftlichen Forschung*, Stuttgart 1977, and Müller, *Die Analyse prozeß-produzierter Daten*.

<sup>40</sup> The development has gone furthest in the United States, and the best source to follow is the journal *Historical Methods Newsletter*, between 1968 and 1977 ten volumes. An example is Volume 9, Nos. 2 and 3 on one of the massive cases of quantitative history, the Philadelphia Social History Project. There is a very long tradition of a social science orientation with attention to quantitative data in France, the school of the *Annales*; cf. Iggers, Georg, *Die ‚Annales‘ und ihre Kritiker*, in: *Historische Zeitschrift*, 219 (1974), pp. 579–608. The most direct impact on sociologists in English and German speaking countries can be attributed to the works

and the impact of social differentiation can only be studied by looking at other complex civilizations. It is indeed quite necessary to use historical complex civilizations for purposes of comparison in order to avoid a tendency in sociology to argue *post hoc propter hoc*. Bureaucracies, corporate associations, formalization of procedures are part of our daily life — but does that make them distinctive features of industrial societies? There is no other way to establish what is unique about industrial civilizations, and what is a feature of many complex societies, than to engage in historical comparisons. In this perspective the quantitative analysis of conditions during the Roman Empire at the time of the principat may contribute more to our understanding of contemporary industrial societies than yet another survey.

An example may help. In working on the sociology of vacations and tourism it is usual to assume that long-distance travel, weekend excursions, and the desertion of cities during the holiday season are phenomena unique to the very different industrial societies<sup>41</sup>. However, weekend traffic problems were part of life in the richer Greek cities, holiday desertion of cities was common amongst the bourgeoisie of classical Rome, and long distance travel institutionalized in several high civilizations such as Sumer, Persia, and Moghul India<sup>42</sup>. Several high civilizations even developed some infrastructure for travel, such as the road networks of ancient China or Persia or Rome, complete with a system of accommodations. However, at least one phenomenon appears to be unique to a modern civilization, namely the regular travel for pleasure only, while other travel such as the „Bildungsreise“ have been developed in other high civilizations.

Economic historians now inform us that production for markets is nothing unique to our industrial civilization<sup>43</sup>, nor is occupational specialization nor are election campaigns<sup>44</sup>. However, the differentiating out of economic activities appears to be a feature of our industrial societies, are a characteristic that to someone from a non-Western society gives our civilization a commercial flavor. In most cultures economic matters are subservient to political considerations, and political power is deemed a central goal and not economic well being. And in all other cultures econo-

of the brothers Tilly; Tilly, Charles, et al., *The Rebellious Century 1880–1930*, Cambridge/Mass. 1975; Shorter, Edward, and Tilly, Charles, *Strikes in France 1830–1968*, Cambridge/England 1974; Tilly, Charles, *The Vendée*, Cambridge/Mass. 1964; Tilly, Richard, *Popular Disorders in Nineteenth Century Germany*, in: *Journal of Social History*, 4 (1970). In Europe, Stein Rokkan in his many publications on nation-building has done more than any other individual scholar to further quantification of historical material for sociological analyses. An overview of the breadth of this development can be found in Flora, Peter, *Quantitative Historical Sociology*, in: *Current Sociology*, 23, No. 2 (1975).

<sup>41</sup> This is maintained e. g. in Scheuch, Erwin K., and Meyersohn, Rolf (eds.), *Soziologie der Freizeit*, Köln 1972, pp. 304–317. Some years later this opinion is revised in Scheuch and Scherhorn, *Soziologie der Freizeit und des Konsums*, pp. 115–147.

<sup>42</sup> Many details can be found in Casson, Lionel, *Reisen in der alten Welt*, London 1974.

<sup>43</sup> For Germany, urban history is the chief corrective for the previous inclination to mistake ideals for reality. See Kellenbenz, Hermann (ed.), *Zwei Jahrtausende Kölner Wirtschaft*, 2 vols., Köln 1975.

<sup>44</sup> Cf. Etienne, Robert, *La vie quotidienne à Pompéi*, Book no. 2, Chapter 2, Paris 1966.

mic relations between people who know each other are subservient to requirements and considerations of the social fabric<sup>45</sup>. Beyond economics, it may be possible that the generally distinguishing character of Western industrial societies is the sectorial rationality, the differentiating out of sector after sector from diffuse and multifunctional roles<sup>46</sup>.

However, such a statement may not last long in view of the many surprising findings of the history of our early industrial periods. Now we learn that not even early capitalism lived up to its reputation of mindless exploitation of helpless proletarians. Undoubtedly this occurred in the large industrial agglomerations, but in production and in living conditions on a smaller scale the employers cared not only for profit but also for their local reputation as human beings<sup>47</sup>. In addition to the comparison with other high civilization, the quantification of European data both of the late medieval period and of early industrialization are likely to be important contributions to our understanding of the distinguishing features of modern industrial societies.

This does not mean to just wait for the conclusions of historians, this requires data to be handled in ways that are usual in sociology, and for problem formulations that are sociological. This should be evident for what we believe to be characteristic for modern civilizations of the Western variety, namely the prevalence of sectorial rationality. It was already mentioned that in other civilizations our economic rationality is practiced in exchanges with outsiders, and this behavior is considered unfriendly. However, even with Western industrial societies there are limits to the extension of a specific economic rationality: We do not accept economic rationality between spouses, and between parents and children. For us, a really functioning family is based on communist sentiments, namely to each according to his needs and from each according to his abilities. In some areas such as sports, there is both sectorial rationality and diffuse standards, distinguishing the professional with a specific sectorial rationality from the amateur for whom sports has a diffuse meaning. Good research with the intention to specify sectorial rationalities requires the manipulation of historical material such as diaries or personal letters, looking for indications of value conflicts and for justifications of behavior. There is little hope that a historian would systematically look for indications of such aspects of behavior that are not part of the problem understanding of his discipline or of people themselves. In this sense there are many problems where sociologists cannot be consumers of conclusions from quantitative history but have to reexamine quantified historical material.

<sup>45</sup> This is a central theme in the research of Raymond Firth about the tribal cultures in the South Pacific, *Elements of Social Organization*, London 1951.

<sup>46</sup> This is the central notion in Scheuch, Erwin, K., *The Relationship of Government and Business to the Individual in Democratic and Totalitarian Systems*, in: *International Conference on the Unity of Science*, Vol. 4, International Cultural Foundation, New York 1978.

<sup>47</sup> See Stearns, Peter N., *Die Herausbildung einer sozialen Gesinnung im Frühindustrialismus – ein Vergleich der Auffassungen französischer, britischer und deutscher Unternehmer*, in: *Ludz, Soziologie und Sozialgeschichte*, pp. 320–342.

## VI. The Importance of Descriptive Knowledge

However, quantitative history is of tremendous importance to sociologists in so far as it is an extension and more often a correction of social history. Students are still being tested by asking them to explain the *loi de contraction* by Emile Durkheim, and yet quantitative historical research shows that in all likelihood this *loi de contraction* is simply in error, is repeating what were the erroneous perceptions of eloquent contemporaries<sup>48</sup>. Provided we would hand on to our successors as the condition of public safety what our newspapers write, this would amount to a massive handing-on of misinformation; provided we were to hand on what magazines write about family life today, our successors would be better off without that information. However, many, many of the statements about daily life in the past are based on reports that are no more reliable than newspaper reports or the impression of contemporary intellectual gurus about our own industrial societies. Even if the guru or newspaper were correct about a condition or a change, they would be incompetent to characterize the diversity existing at this time and earning our societies the label „pluralistic systems“. Now that some preliterate cultures have been studied by more than one ethnologists we understand that even those relatively simple cultures have diversity, and that past ethnography reduced that diversity to an ideal type. It is reasonable that in historical societies there was no less diversity, that differences between actual behavior and official norms were common-place, and that an informal system paralleled official structure much as this is the case for our societies. Most social history is simply hopeless in these respects, and the only hope is the systematic analysis of large quantities of evidences of daily life in the past.

Was the exploitation of colonies a major cause for the economic development of France, or Germany? Was the French revolution caused by an intolerable pauperization of ordinary people? Was the middle of the 19th century in Germany a time when in economic controversies capitalists stood against labor? By now we know through quantitative history that the answer to all three questions is „no“<sup>49</sup> – and

<sup>48</sup> This is the conclusion of a number of quantitative studies in urban history, such as Hubbard, William H., *Der Wachstumsprozeß in den österreichischen Gross-Städten 1868–1910*, in: *Kölner Zeitschrift für Soziologie und Sozialpsychologie*, Special issue 16, pp. 386–418, and *Forschungen zu städtischer Haushaltsstruktur am Ende des 19. Jahrhunderts – Das GRAZHAUS-Projekt*, in: Conze, Werner (ed.), *Sozialgeschichte der Familie in der Neuzeit Europas*, Stuttgart 1977, pp. 283–291. Compare also Thernstrom, St., *The Other Bostonians*, Cambridge/Mass. 1976; Katz, M. B., *The People of Hamilton*, Canada West, Cambridge/Mass. 1975.

<sup>49</sup> For question no. 1 see Hochheimer, Albert, *Abschied von den Kolonien*, Zürich 1972; for question no. 2 compare Tilly, Charles, *Vendée*; for question no. 3 see Best, Heinrich, *Interessen-*

that is by no means unimportant for sociology. It will become even more evident to which degree we have based sociological statements on a social history that is becoming defunct.

For this author descriptive knowledge about the economy of the Roman Empire as it was furthered especially by historians in Oxford and in Princeton, became of great importance. My understanding of Rome was very much colored by the German historical tradition which concentrates on the turmoil period of the Roman Republic, and in the tradition of Theodor Mommsen understands this period as the corruption of republican ideals – which I now see as a perspective that is very much beside the point. By way of contrast British economic history has always emphasized the empire during its successful time – which after all is several hundred years. During this time the economic order was a variant, from a partial market economy to the centralized state socialism of Diocletian. During the whole time of the Roman Empire, the governments were unable to cure inflation and to establish a sound currency for any extended period – which, by the way, the Chinese Empire failed to do as well. Whether detailed regulations or market mechanisms: Nothing really worked.

And yet the Roman Empire failed to decay, while undoubtedly our systems would be mortally threatened if there would be inflation on the Roman scale over many decades. Being raised on Parsons I had believed that when interchanges are seriously upset there will be countervailing processes until the disturbances are corrected; the Roman Empire demonstrates that social systems can live with unsolved problems on a massive scale – provided there are redeeming features. In the case of the Roman Empire its performance as a political and legal order was obviously so impressive in comparison to other contemporary systems that the „Roman Way of Life“ was as successful an export article as the American Way of Life was after World War II.

Perhaps this is a general feature of highly differentiated societies: that they have „central problems“ but are at the same time able to live with them via redeeming features. More descriptive knowledge would help in translating this still very vague notion into a researchable question. However vague this notion, the descriptive material was already sufficient to correct the conventional wisdom in American sociology. Perhaps there will be reports based on quantitative history about other advanced civilizations that lasted hundreds of years without solving some central problem; perhaps these reports will inform us what the character of countervailing forces was.

It is especially quantitative history as the rewriting of conventional historiography, and as the extension of knowledge about forgotten eras that will have an important impact on sociology. One last example: We can expect important insights into the change of systems when the fifth century in Western Europe is being

analyzed. The notion of a Roman empire being overrun by screaming Barbarian hords bent on destruction is stark nonsense; Roman power did not collapse or was broken — it simply seeped away<sup>50</sup>. It is a story of desintegration and not of forceful destruction.

This is an exciting time both for historians and for social scientists. There is more than one way in which the discipline will benefit from the renewed encounter. Programmatic debates will have little utility in starting the development. Much the best way to aid this development is simply more empirical work.

<sup>50</sup> Sterzl, Anton, *Der Untergang Roms an Rhein und Mosel*, Köln 1978; also Ternes, Charles-Marie, *La vie quotidienne en Rhénanie Romaine (Ier–IVieme siecle)*, Paris 1972.