Structures and contingencies in historical research

The theme of this book contains two elements which go badly together. At first sight we might be inclined to oppose ‘structures’ to ‘contingencies’. On second thoughts, however, they appear as the two main modalities of reality in time, alternative to each other and yet consocial. We may better refer to the frictions between contingency and historical computing, i.e. any type of computerized research in history. There exist, of course, statistically organized contingencies which fit well into a structural approach. But, in daily life as well as in philosophy, the meaning of ‘contingency’ is opposed to systematization. Contingency appears as a random event, the structure of which depends on strictly individual or once-only factors. That’s why contingencies seem to break away from any quantitative approach. The question is to discover whether and until what point contingencies really prevent a quantitative approach or a structural analysis. Or is there some hidden logic that would permit us to turn contingencies into structural elements? I am not qualified to give definite answers to these major questions. But, on reflection, the theme of this book thus immediately raises three major questions:

- Can contingent historical events contribute to the creation of quantifiable sources with a common structure?
- Is the researcher entitled to plot a structure into a collection of contingent sources?
- What happens to contingencies when we begin to put them into structures, and conversely?

The scope of this introduction does not make it feasible to cover these questions completely. Besides, the editors have asked me to reflect freely upon the theme, using as far as necessary my own research, which basically is about different forms and levels of education and cultural transfer in early modern history. In fact, though a believer in historical computing, I am not really a practising computer historian. I have been for a long time and still occasionally am addicted to quantitative research. But just like other historians of my generation, I had adopted cliometrics, quantitative history and structural analysis as a methodological creed before discovering that the truth has many faces and that even the most beautiful creed can conceal other very legitimate expressions of truth. Hence, to a certain extent, our return to the contingencies of history, to hermeneutics, biography and narrativism, not to forget that ‘thick description’ celebrated by
the anthropologist Clifford Geertz. My reflections will be partly based upon my own research, partly of a more general nature.

In this respect, it might be important to know something about my own background. I shall give you this personal testimony on purpose, not for the sake of idleness, but because the very first interrogation about structures and contingencies has to begin with the researcher’s own contingent history, both as a personality and as an historian. Why does the researcher do in that way what he actually does? What about his particular goals? Why opt for computer based research when other methods may be available, perhaps less time-consuming, and often more easily legitimated by historiographical tradition? The computer is never the only solution, although it might be the best solution for a given configuration of problems, and quantification is never the final answer, although it might be the most adequate method to reach it. Besides, the recourse to computer-mediated history, though imposing strict rules, does not enslave the historian to the software or the machine. Even in big research programs, the individual has his or her share of responsibility for the definition of the research themes, for selection, capture and processing of the data, and for the methodology employed, if not for the interpretation of the results. The more so as the researcher works on an individual project.

Looking through the table of contents of this book, I noticed the almost complete absence of the historian himself (or herself) in the titles of the papers announced. Nevertheless, few historians seem more research-addicted and indeed workaholic than computer historians. The apparently unlimited potentialities of computerized research may act as a real personal drug and create Prometheic illusions of total data coverage in space and time. Huge computerized research programs, initiated for the simple sake of data storage and altruistically designed for others’ research, can in the long run water down into endless databases, never finished, never complete, and never ready for the appropriate treatment. Scanning all the notarial records of a whole province, all the pictures of Dutch art, all the demographic movements of a whole country, and so on. There is no doubt about the benefit of such immense operations for future historians, but they are only a beginning. They are not real history yet, only the very first steps on the pathway of collecting material, information management, documentation. And one may well wonder whether in the long run their conception will survive the inevitable changes in historical methodology.

The reality will, of course, be not as blatantly bloodless as I am sketching it now, but there is a methodological ground for my concern. Ever since a decade, philosophy of history reminds us of the fact that objective, clean, self-evident history does not exist. That sources do not speak out of themselves, but only when properly interrogated, not only with the help of interrogation techniques but with a hypothesis in mind, a narrative structure permitting to conclude to meaning when data are linked, or cross each other. Evidence is always constructed evidence, either from the outset or as a result of the historian’s interpretation.
of his selection of the 'facts', but mostly both at the same time. History is always embedded in the heuristic, methodological and narrative choices of the scholar, and no choice is innocent. That is not to say that it is the researcher who brings contingency into pre-existent or pre-established historical structures, the one who makes history casual, and human. But the preference for either structures or contingencies as the focus of research depends mainly on the choices of the scholar, and these choices are partly guided by the very contingencies of the scholar's career (his position in time, space and society), partly by his own, or his group's, preconceptions.

Let me show you some steps of my own career in order to make my point clear. I started my historical studies at the university of Paris, in the late sixties. For some of the younger historians I might therefore appear as a brontosaurus of the pre-computer era. Let us rather say that I acquired my research habits during the period - some might call it the heroic period - when researchers were supposed to make themselves acquainted with one of the current computer languages in order to write their own programs. In the mid-seventies, at the École des Hautes Études en Sciences Sociales, we learned Fortran and tried to apply it. For two reasons I quickly deserted the course, together with other historians of culture. Firstly, because it seemed impossible in those days to apply computerized data analysis to other than statistical, or at least numerically arranged, historical sources. No images, no symbols other than the current ones, virtually no texts. Secondly, because all the money available was given to cliometrics, i.e. quantitative social and economic history in the narrower sense of the word. One needed a lot of money in those days for collecting data and preparing it for computer use, not to speak of the very problematic chance to get the necessary computer time and to gain access to one of the very scarce computers themselves.

We therefore had recourse to the calculator. But what type of calculator? Exactly 25 years ago a socio-economic study on the Fugger family of Augsburg was published by a famous French social historian, the late Robert Mandrou. In his introduction, he thanked the École des Hautes Études for having lent to him an electric calculator once a week (Mandrou 1969, 8). Not yet an electronic one, but such a noisy and terribly slow electric apparatus that quickly got on your nerves. Twenty-five years ago – less than a generation! By that time, being students, we still did all our calculations by hand. In 1971/2, I wrote with a colleague one of my first scientific articles, on food rations in 18th-century French boarding schools, based upon the account books and other sources from the school archives (Frijhoff and Julia 1979). We were obliged to convert an enormous variety of very different local, nondecimal measures into decimal values. By chance we discovered an unused and forgotten electric calculator in a cellar of the French ministry of Education. The endless noise made by the machine overnight must have terrorized our neighbours. Some months after we had finished our article, in the Autumn of 1972, the first manageable electronic calculator came out on the annual French electronics fair, the SICOB. Its cost was about twice what I earned in a month. Hence, before the stormy development of the computer
took place, our basic choice was made. Eight years later I finished my PhD, which was a quantitative study about the 23,000-odd graduates of Dutch universities from the 16th to the beginning of the 19th century, including an important prosopographical sample. Computerized data collection and processing would have been welcome here, but there was no money for it at the time, and at any rate, I had not yet got the 'habitus' (to use Pierre Bourdieu's key notion) of considering data collection and processing in terms of computer programs. More generally speaking, the lack of such an habitus may well be the reason why in the second half of the eighties many historians of my generation stepped over so quickly to more narrative forms of history, including the French guru of computerized historical research himself, Emmanuel Le Roy Ladurie. This is not to say that narrative history would be easier, although it often may seem less time-consuming than the exacting systematics of historical computing, especially as far as the pre-statistical era is concerned. But besides the fact that we already had invested too much time in hand- or typewritten card files, the reason is that we were beginning to consider the very structure of our research basically in a different way. Isn't it true that computerized research requires an attention for formalization of research that is often absent from the cultural historian's mind? In fact, the famous 'linguistic turn' in history has induced cultural historians not so much to the formalized structures of linguistics as to the aesthetic contingencies of textual analysis, literature and narrativism.

Here lies, of course, a methodological trap. But the trap also lies in wait for computer-minded researchers themselves. Formalization may easily be seen as the supreme target, and quantification as proof. Of course, the textbooks on history and computing firmly assert that computer use is not the final goal of historical research, and that computing techniques constitute only one of the methods to attain the historian's aim, which is: writing history, or, if one prefers: representing history, either in writing, or visually, or perhaps along still other lines, such as living history. Psychologically, however, the status of a quantified analysis in a proof procedure is very high, at least in most Western societies. Quantified research resulting in structural analysis benefits from the prestige of hard science. It derives its value from the social recognition of statistics as a valuable tool and from the emotional status of representativity, which gives the illusion that one holds a true reflection of reality. A contingent, not quantifiable source not only seems unfit for the computer, but there is a suspicion about its usefulness for the writing of history as such. Of course, new computer techniques make the exploration of such isolated sources easier, but as a rule, quantities are preferred to unities and structures seem to have more weight than contingencies. Quantified social history seems more 'true' than micro-history or symbolic anthropology because it has been tested by the machine, and the machine is a ruthless critic indeed.

Let us now return to the questions asked above. First question: Can contingent historical events contribute to the creation of quantifiable sources with a common
structure? In other terms: can historical sources have an autonomous structure? I do not believe that any historian is as naive as to think that historical sources do exist outside of the 'social memory', as it is called nowadays, or the 'collective memory', as it was called some fifty years ago by Maurice Halbwachs and Sir Frederic Bartlett (Fentress and Wickham 1992; Hutton 1993). Historical sources are selective forms of conservation of the past. Conservation is organized, as is amnesia. A recent, rather violent public discussion on the occasion of a new Bill on the public archives proposed to the Dutch Parliament, has shown that social memory is endangered if it is left to the sole responsibility of the public authorities, i.e. the people who are the authors and the first keepers of the public records. From the very beginning, a proper historical memory has to scan the archives, to select, and to organize gathering and conservation. But is that possible without a firm and structured idea on the past in mind? Even the conservation of purely quantitative sources supposes a hierarchy of research goals, and a vision of what we really do want to know, projected into the depth of historical time.

However, quantitative sources often seem to have a quality that other source material hasn't got. The serial aspect of such sources easily gives us the illusion of a truthful reflection of evolution in time. The possibility to construct a complete but finite database makes us readily believe that nothing more can be said than the collected material is able to reveal. But except for the potentialities for virtually unlimited retrieval, does computerized data processing really reveal anything else than our working hypotheses make us put into it – just like traditional research methods? Computers answer the questions you ask. To bad questions, trivial answers. To good questions, the promise of an exalting answer, provided that the software and the data be of the same quality. I wonder whether the frustration about the poor conceptual quality of many computer research is not the very reason why after the explosion of quantitative approaches and structural analyses in past decades many historians have returned to more contingent forms of history. The fate of the term 'representative', once so crucial in quantified research, is a clear mark of this volte-face. In some sectors of the social sciences, it has virtually become a dirty word. It has been replaced by 'representation'. But whereas 'representative' refers essentially to a structured collection of data, 'representation' refers substantially to individuality and contingency. Anything and anybody can be a 'representation' provided that it be according to the social conventions which enable us to recognize it as a meaningful representation. But symbolic conventions remain fundamentally of a contingent nature.

Hence the second question: under what conditions is the researcher entitled to plot a structure into a collection of contingent sources? I should like to refer here to my own PhD, defended thirteen years ago at Tilburg University, because it has the advantage of a concrete quantified research having come to an end, and by now sufficiently far away in time to discover freely its shortcomings (Frijhoff 1981). Let me explain what it was about. In the seventies, I worked in France at the History department of the École des Hautes Études en Sciences Sociales. We
had a common project on the history of universities which started from quantitati-
ve research principles. Abandoning the old continental history of universities,
which had been mainly a history of medieval institutions and pre-modern ideas,
without any link with social structure and social development, we tried to lay
new foundations for a social history of the university, in fact mostly the early
modern university because the modern university had already been colonized
by sociology. I looked for quantifiable data enabling me to construct a social
history of the early modern Dutch university, bottom-up, that is, seen as a
breeding-place of future intellectuals.

There are basically three series of such quantitative data available: matriculati-
on (i.e. entrance registers), recension (i.e. the annual registration of the students
really present – badly kept and only partially conserved registers, however), and
graduation lists (i.e. the university’s administration of its own output). But
quantitative capture of matriculation registers without a previous prosopographi-
cal treatment is hardly recommendable. In the early modern matriculation regis-
ters people appear often as many times as they apply for a student’s tax reduc-
tion or for an examination. They do so not always under the same name, not even
under a recognizable equivalent. Patronymic names may in the meantime be
amplified with a formal family name (e.g. Jan Pietersz. becomes Jan [Pietersz.]
Brouwer or Johannes Petri Brouerius); Christian or family names are sometimes
translated into other current languages, such as Willem into Guillielmus, or Derk
into Theodoricus, and d’Outreleau [French] into Overwater [Dutch] or Transangu-
 anus [Latin]. Besides, in variable proportions names in matriculation registers
apply to non-students who are for some reason (status, tax reduction, or other-
wise) interested in and entitled to formal matriculation. Prosopography is impossible
for more than one hundred thousand students, and sampling or random procedu-
res suffer from the very low degree of identification and from the high proportion
of foreign students difficult to locate.

Hence I decided to attack the question from the other end: not from the
university’s input but from its output. I quantified the numbers of graduates
coming from all the early modern Dutch universities, adding also, in as much
as I was able to trace them down in foreign archives, the rather high numbers
of Dutch graduates at foreign universities. Besides, as an in-depth sample I
compiled a complete prosopography of about 1500 students born in the town of
Zutphen or having come to live there with their parents. I crossed my university
data with other time series like admission registers to the bar, to medical societies,
to pastoral functions, to political positions and to learned societies. The quantitati-
ve evolution of university graduates was related to population figures, tax regis-
ters, patents of inventions and other parameters of social, economic and cultural
development. What I got at the end, was a kind of a university business cycle,
completed by a set of other quantified parameters over time which permitted me
to sketch an overall picture, to give it social meaning and to favour a structural
interpretation of the evolution. I had, of course, from the very beginning an idea
in mind, and that idea was what I got in the end: I mean, quite simply, the
gradual dissociation of two elites in the Netherlands in the period under review, i.e. an academic, intellectual and political elite on the one hand, and a vernacular, industrial and commercial elite on the other hand. The Revolution would give its historical chance to this last elite.

Ever since I completed this work, I have deplored that I had not been in the position to computerize my data either for storage and retrieval or for processing and analysis. My electronic calculator was not more than a first-degree brain-help. Crucial operations of a more complex nature on the collected data had to be cancelled. In fact, for many operations there was no decent database available because cross-tabulation was executed immediately on the collected material and the previously calculated figures, as we did it in those days. Since most databases are closely dependent from the research aims, one may wonder whether, after all, such a database would have had much utility in the future. There is however reason to believe so. More than ten years later, I am indeed more and more aware of the constructed character of the results of my research. Elites are more than isolated social groups: they have ideas which separate them from their peers, whereas they live closely together with other social groups in the same country. The more I look at their everyday life, the more the formal dissociation of these two elites seems to me replaced by more informal ways of conviviality which put the socially separated groups culturally on the same footing. At the same time, major political cleavages appear within the same culturally coherent groups and gain a crucial significance for social change.

Besides, my Dutch university business cycle has only a virtual reality, that is, it exists only in a research perspective. Just like all cycles, it is, in fact, a composite one, resulting from the amalgamation of the smaller cycles of the individual provinces or towns, very different between each other. But whereas fifteen years ago Dutch historical research still considered the Dutch Republic as a cultural unity and my overall cycle was the normal outcome, the tendency is nowadays towards a dissociation of the early modern provinces and a reappraisal of their particular rhythms in matters of culture. In a recent version of my book, I would have stressed the disparities within the Dutch Republic and the composite character of my cycle. This nascent historiographic shift does not invalidate my research as such, but it asks for a different interpretation of the data. In order to do so, I should start with putting my basic questions into another structure, and I should certainly look for the help of other contingencies. Thus I still have the main question on my mind, which is to know whether the construction of a structural model of cultural evolution has or has not, purposely or perhaps unconsciously, eliminated crucial contingencies which might have altered the outcome of my research.

Hence the third of my initial questions: what happens to contingencies when we begin to put them into structures? To make myself clear, I take here the case of early modern literacy. The study of literacy was one of the very first forms of quantitative history, and probably the very first in cultural history (Compère 1995,
chapter 2). Exactly a century before Carlo Cipolla’s seminal work Literacy and Development in the West (1969), the French recteur Louis Maggiolo had, with the help of numerous Republican school masters, counted the number of signatures in marriage registers of different periods all over the country, and calculated literacy rates, in order to get a picture of the cultural development of the masses – of course with a little idea on education policy in mind. This first quantitative research on literacy and its imitators tried to define overall levels of development and to differentiate nations, regions or social groups according to their degree of achievement in the global society. Subsequently, many historians have adopted the measure of the signature – i.e. the ability to sign one’s own name –, as a reflection of literacy rates. On this simple quantitative basis, cultural entities, levels and oppositions have been constructed: between rural and urban, agrarian and industrial, male and female, Catholic and Protestant, unskilled and skilled, lower classes versus merchants and professionals, and so on. Literacy studies became an instrument for the cartography of power, development and modernization, the more reliable as the signature seemed a perfectly objective, ‘neutral’ measure. A society seen from above, from the perspective of literate elites. At a second stage of literacy research, formal policies of literacy, mostly by means of compulsory school attendance, have been identified: among the lower classes, among religious dissenters, for social discipline, for political or ideological reasons, among girls or ethnic minorities. Socialization and nationalization both took a literate outlook.

At a third research stage, however, the link between literacy and modernization is not any longer taken for granted or automatically derived from literacy rates, but is questioned again and becomes itself a subject of research. One of the editors of this volume has devoted his PhD to the interrelation of literacy, between 1800 and 1920, and three other variables: social mobility, migration and demographical behaviour (Boonstra 1993). One of his major suppositions is, that literacy permits an individual adaptation to modernization patterns. Hence a virtual return to the contingencies behind the structures. If literacy is structurally linked up to modernization, the access to literacy and still more the active use of literacy in society is due to individual decisions and initiatives, embedded in a huge variety of factors which remain to be determined. Some of those factors may again prove to pertain to structural aspects of history, either economic, social (such as family structures), or cultural (such as religious push-factors). But fundamentally, contingency is now back again.

For the early modern period, when cultural state policies remained mostly at a low level of enforcement, things are yet a bit more complicated: the status of the written word in those societies was as questionable as is the value of our present-day measuring. What does literacy exactly mean in a not forcibly literate society? And what is the value of its early modern measure, the signature on the marriage register or underneath some notarial record? In the Netherlands at least, the painful reconstruction of 17th-century life histories show enough examples of people using successively a signature, a hand- or a housemark, and a simple
cross. A good case is Hendrickje Stoffels, the last woman in Rembrandt's life: having signed for the first time with her full name, she marked all the latter records with the cross of the illiterate.

Both questions, the one about the meaning of literacy and the one on the value of the measure, put us in front of the question mentioned above: what happens to contingencies when we begin to put them into structures, to quantify and to reduce irregularities, in order to get regularities? There is no point in trying to solve those difficult questions here, but it is important to ask them and to keep asking them. Is it not true that the treatment of literacy as a social value in itself applies a 19th-century social grid to, say, 17th-century cultural realities? Does this not lead the historian to the perception of structural developments where from another, not quantitative point of view, no structure may be perceived at all but mainly contingencies? More concretely: Is the meaning of the ability to read and write the same in all societies and in the whole society? What exactly is the use of literacy? Reading only? Or reading and writing together? And then: reading what exactly? Bible and catechism, i.e. religious socialization? Or does literacy entail the crossing of a border, give access to a secular culture and structurally permit a modernization of the patterns of behaviour? Does the ability to sign one's name point to the emergence of a real writing culture as expressed in bookkeeping, private correspondence, private diaries? To the transition from an essentially oral and local culture to a basically written and supra-local culture, with its own formal logic and its particular modalities of apprehending reality? Reversely, does illiteracy, the incapacity to read and/or write, really prevent the spread of culture and the transition to modernization in space and time? Or is there a fundamental disjunction between ability and practice, the capacity to read or to write turning only into a systematic practice under the pressure of other factors? It is only after having answered such preliminary questions that we may return to the quantification of early modern literacy. Again, there is a lesson we might learn from recent studies on illiteracy among schooled populations: abilities are never definitely acquired, modernization is never the automatic consequence of literacy. There may always arise a distortion between intentions and realities, a gap between abilities and practices, between norms and uses. As the Americans have discovered first, cultural abilities may get lost when there is no structural incentive to put them into practice - that is what the French historians, those very first champions of literacy studies, call by now 'illettrisme', by contrast to 'analphabétisme' (Fraenkel 1993). Structurally organized literacy may quite well be accompanied by the contingencies of 'illettrisme', not to speak of cultural illiteracy (E.D. Hirsch), computer illiteracy and other forms of functional inadequacies...

As an historian of culture, I should like to end these reflections with a challenge. I have taken my literacy case on purpose, because it is a good example of a research contingency unconsciously determined by the structure of the sources: in this case we have a ready textual source with an apparently high degree of formal and functional uniformity, and so frequently used as to permit easy
quantification. Until quite recently, the main sources of the historian were written sources, either textual or numerical, conserved for administrative use or because of their perceived value. Objects with a high aesthetic value pertained to the proper domain of art history, meaningful texts were the hunting-ground of the historian of ideas. As far as I can see, most present-day computerized research relies heavily upon those 'old', traditional sources: quantifiable data about social, economic or demographical behaviour, textual documents, graphical representations and valuable images are the most commonly used objects for computerized data analysis.

Ever since two or three decades, however, the historian's potential sources have increased greatly, and his methodology has been enriched. Not because there would be more sources as such, but because new directions in historiography, new historical objectives and new research methods have made us recognize ever more aspects of reality as potential sources of historical analysis: the landscape and its transformations, town-planning, material culture, oral history, symbols and rituals, the history of the body, of emotions and feelings, of visual stimulants and drugs, and so on. All involve either new sources or new uses of sources, and have been incorporated into the new canon of historical research. Yet, in many cases, historical analysis still follows old patterns. Since many of those new potential sources are not easily quantifiable, and have got in themselves a contingent nature (or may seem to have so), computerized research might tend to a conservative position and favour big, traditional programs monopolizing virtually all the time and money available. Not every new source is easily fit for computer-based treatment. However, in the long run computerized techniques of analysis may well present unheard-of possibilities for creating, linking and interpreting data of a very different nature. Please think of what happened ever since the quotation from Robert Mandrou, 25 years ago! In the meantime, we may begin with the methodological expansion of what we know already. Why, for example, should we study so extensively the literacy of a population, and not – to quote a problem raised some years ago by Sir Keith Thomas – its numeracy, i.e. its ability to deal correctly with figures? What are the implications and meanings of counting deficiencies in daily life and in historical societies? And why shouldn't we be able to add to the study of literacy and numeracy the third panel of the triptych: I mean that what correspondingly might be called the imaginery of a group, i.e. its ability to form or to seize and transform images, giving them a meaning which is recognizable not only for the group itself but also for outsiders?

Does historical computing follow historical research? Does it indeed stand in its forefront? There may be reasons to raise some doubts. One of those reasons is structural and pertains to the proper logic of the public administrations everywhere in the present-day world: ever faster the cost of research programs has to be justified by results. Whereas historians have to publish within three or four years, the elaboration of computer techniques for processing and analysis takes time, and new sources present us with new methodological problems. But then, new techniques may also lead to new methods of research, because new questions
may be asked. A good example is image-processing, almost simultaneously undertaken and quickly brought to an ever more sophisticated level in several countries. Most forms of image-storage still concern art history and apparently do not serve other aims than the act of collecting and retrieving itself. Yet images of everyday life are purposely collected and studied, for example at the Institute of Krems in Austria. Similarly, the Museum of Education at Rouen (France) soon will begin to scan all the images contained in French primers and textbooks, starting from the hypothesis that images are more vital to cultural transfer than texts. Experience proves indeed that adults keep a much clearer memory of history through the images in the textbooks of their youth than through the study of the texts themselves.

As a cultural historian, I really do wish that computerized research should take more and more into consideration the cultural dimension of social history. Not only by creating more databases, and not even particularly so, but first of all through the development of techniques of analysis which will prepare us for a more thorough and multifaced interrogation of the sources and make us better understand the interrelations between the multiple dimensions of reality. That is, I think, the methodological challenge of the next years. At the same time, it is, of course, a challenge of the realm of contingency to the republic of structure.