Event Sequence and Event Duration: Colligation and Measurement

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Social science history has often attacked narrative history for preferring the arts of narration to the sciences of analysis. But the old approach had strengths that are too often ignored. Perhaps the most important was its insistence that social life happens in stories, that the order of events influences their ultimate outcome. This view has not been absent from the social sciences themselves; interactionist sociologists have long upheld it. But quantitative social science has never dealt effectively with order in social processes, and social science history acquired this weakness with the methods it borrowed. Sequence regularities are afterthoughts in models aimed primarily at structural and systematic ones.

Generalizing stories effectively is not, however, merely a matter of conceiving and testing sequential regularities, any more than dealing with synchronic structure is merely a matter of conceiving types of structures and testing them. Between conception and test intervene the tasks of defining constructs and finding indicators. In an earlier paper I discussed the conception and testing of sequence regularities. Here I shall concentrate on these two intervening tasks. I shall first discuss the definition of constructs. To make order effects visible, one must section the continuous social process into events and stories, a process I shall follow Whewell, Walsh, and McCullagh in calling colligation. I then turn to the issue of measurement proper, considering indicators for these constructed events and their stability under various data constraints.[1]

An example will both clarify the line I am drawing between colligation and measurement and introduce the source for examples throughout the paper—the development of American medicine. At various times and places one can speak about an *event* called the rise of interest in medical education—in St. Louis, in Massachusetts, in the United States. For each of these *events* we can choose any of a number of *occurrences* (e.g., foundings of medical schools) as indicators that that event has happened. Thus, the foundings of some 500 medical schools in the United States are to be regarded as *occurrences*, rather than as events proper. Events are constructs, just as "concepts" are constructs in the usual approach. The act of thinking up the events and the stories they compose is separate from the act of measuring those events with occurrences. One is colligation; the other, measurement.

The distinction between events and occurrences is relative. There is no absolute level of events, any more than there is an absolute level of constructs. But in any given application, a distinction must be made between the theoretical matters of interest—the events and the story that contains them—and the indicators used to measure them—the occurrence data at hand. In rare cases these may coincide. Generally they do not. The distinction may be reinforced by recollecting an example of the construct/indicator distinction in standard methods. Education, the construct, is not the same thing as years of schooling, the indicator; you can lead a horse to water but you can't make it drink. Yet in many studies reification makes the one into the other. In fact, just as education is not the same as years of schooling, so professional concern for medical education is not the same as founding of medical schools.[2]

This paper, then, discusses two phases of the formal analysis of sequences of social events: colligation and measurement. The discussion of colligation is relevant to both quantitative and qualitative social science history, since both must undertake this task. The measurement discussion is aimed at the quantitative scholar who expects to test data with seriation or some other formal

sequence analysis method. Nonetheless, many of its points apply directly to arguments commonly used in qualitative work.

COLLIGATION OF SEQUENCE HYPOTHESES

The aim of colligation is to create a plausible and followable hypothetical story that generalizes a group of particular events and stories. What are the events in this hypothetical story? Phrased thus, the problem of colligation is deceptively simple. In part its danger lies in the kind of reification just discussed.

> People began to work with certain problems and discovered that they and other practitioners needed certain skills. So they became interested in education and, eventually, founded a school.

It is easy to read this narrative to imply that the important events are full-time employment and schools, rather than work and education. The mistake becomes clear when we consider two alternative narratives.

> The first workers with these problems found themselves at a loss and formed study associations and journal clubs to upgrade their skills. Simultaneously, they created an apprenticeship system.
> Around this time a great wave of organization swept the upstate area. Professionals and businessmen founded societies, lyceums, and schools with wild abandon.

In the first alternative version the same problem situation generates an alternative occurrence. In the second, the occurrence of schooling arises out of a different event altogether.

But the issue is more subtle than reification. The true difference between these narratives lies in the mechanisms propelling them. In the first, workers created schools because they found that skill was necessary for work. In the second, the same goal leads them to create journal clubs and study associations for themselves and apprenticeships for their juniors. In the third, however, schools appear for different reasons, which are less intentional than they are determined, and which seem to come from outside the professionalization story. In thinking about possible stories, one would regard the third as part of a different story than the standard professionalization one, even though it involves the same occurrence (schools) as the first. By contrast, the first two narratives clearly belong under the same conceptual story. Thus, it is the mechanisms, the intentions or determinants that lead from situation to situation through the sequence, not the occurrences realizing those intentions and determinants, that define the story. The third narrative is part of a different story because of two aspects of its mechanism. First, that mechanism's domain of operation is different, embracing much more than merely professions. Professional schools arise because schools are one type of organization and professions are one type of social actor and social actors in general were organizing at that time. Second, the mechanism, actually not given here, is definitely not the kind of purpose or intention evident in the first two narratives. It differs in quality as well as domain. The identity of stories is thus founded on the identity of the domains and qualities of the mechanisms that constitute them.

Thus the problem of defining events is preceded, in colligation, by the problem of defining these domains and qualities. These in turn shape the definition of events, for the central aim in defining events must be to make the mechanisms—and the story they produce—particularly plausible or followable. It is this criterion that makes reification such an issue. If *school foundings* are treated as the event, then the intention of skill enhancement is a less plausible mechanism because it can have only this one realization. If an underlying desire for skill enhancement is truly the driving mechanism, it will undoubtedly have multiple realizations. This means treating a *hypothetical*—concern for education—as the event, and treating school foundings as one of a number of alternative outcomes of the problematic situation of unskilled practice. Events must be defined so that the story is "closed," so that the possible outcomes of the mechanisms are all included in the unfolding story.[3]

There is another difficult problem in defining events; an occurrence can be seen as a part of infinitely many events. The Oswego County Medical Society was founded in 1821. This occurrence can be made part of the story of the growth of medicine in Oswego County, or in New York State, or part of Oswego County history generally, or part of Tocqueville's grand event of voluntarism in America. How then is colligation possible at all? In principle this is a grave difficulty. In practice it is not. The fact that the same objection applies to collecting the very same occurrences into variables has not slowed qualitative social science. For example, the occurrence "40 percent of practitioners of psychiatry are board certified" can be defined as one observation of a variable called "practitioner coverage by specialty licensing." But it is also an instance of a variable called "prevalence of restrictive trade practices" or even of "relative involvement of psychiatrists in varying kinds of activities." We change the meaning of the 40 percent by changing the universe of which it is a part, just as we change the meaning of the founding date of the Oswego County society by colligating it as part of different events.[4]

These criteria and problems place limits on the formal task of colligation. Formally defined, colligation is the combination of pieces into an emergent whole—an event, an actor, a principle. The term was Whewell's inductive alternative to classification, that form of deduction in which discrete items were placed under headings. Classification presumed that reality was already cut up;

the question was what to do with the pieces. For colligation, the question was how to dismember reality in the first place. In bringing the term into the philosophy of history, W. H. Walsh limited it to "the assembly of complex particulars." But C. B. McCullagh quite correctly expanded it to general terms like "revolution." Thus, even though revolution can be seen as a category, which can be used for classification, it is created by colligation, and the best way to classify a set of events as a revolution may be to see if they "naturally" colligate the same way as does revolution in general.[5]

This task of colligation is difficult but not impossible. It has three essential parts. One is the conceptualization of levels of analysis—of domains—within which one can tell coherent stories. These levels may be geographic, temporal, or societal. The second is the substantive theoretical analysis. What, for example, are the crucial events and mechanisms of professionalization—the rise of education? the coming of national organization? the rise of professional status? Should one consider the emergence of professional controls essential? Is self-interest the dominant model for mechanisms? The third part is the qualitative analysis of the events—do they have duration in time, do they have distribution in space, are they sharply definable in principle or practice? This last aspect of colligation leads directly into the problem of measurement. In any practical application, the second of the three would be the most important. But for the purposes of this paper, I shall concentrate on the first and the last.

Levels of Analysis

The first problem in colligation is the establishment of levels of analysis. On the one hand, these must be "socially real" universes of action. That is, they must be interactive arenas within which a given subject's actions might be expected to have significant effect. The gradual association of doctors in Oswego County may be part of a national story. But it is best understood at a local level, for it is within its own locality that it will find a response—from charlatans, from other professionals, from clients, and so on. At the national level, there is a different story and a different actor—the national medical community that includes the Oswego group and its peers, and *its* story, which transcends the sum of their individual actions. Thus the levels of analysis are also levels within which one can conceive of coherent *subjects* for social stories, what Hull calls "central subjects." This accords with the insistence of the interactionist theorists in sociology that social life is a matter of real agents who make and remake their social world through their activity. Thus one simple and restrictive rule for colligation might be that all the events in a particular colligated story should happen to a single generic subject, for example, the local medical community.[6]

In the case of American medicine, the coherent levels for stories seem to be local, state, and national. The history of medicine clearly "makes sense" at each of these levels; that is, one can tell a plausible and followable story at each level. But the levels can relate in various ways. Occurrences in them may coincide. The founding of the University of Pennsylvania Medical School in 1765 is indeed part of stories at all three levels. It was the first school in Philadelphia by many years and only the city's second medical institution of any kind. One could justifiably take this founding to indicate the event of rising concern for medical education in Philadelphia. However, the school at Penn also signals an important event for the larger state medical community, of which Philadelphia is the dominant center, as well as for the nation, for which it provides an important symbolic beginning. It does not, of course, signal the national event "rise of medical education," which comes much later, but it does signal *something* at the national level, perhaps the event "first stirrings of an organized medical profession." Thus one occurrence is part of three important, but different, events at each of three different levels.

Later on, these levels separate, and the Philadelphia story becomes more its own, and less the nation's as a whole. Indeed, levels may relate differently in different geographical areas at a given time. Long after the upstate medical communities in some states have achieved a separate life of their own, in other states the central cities continue dominant. Finally, a given type of occurrence may be localized to a particular level. A good example of this is ethics codes, which in American medicine were national in scope almost from the start, even while in social work and teaching the early codes were all locally adopted.[7]

The first task of colligation, then, is to decide what are the levels of stories and who their subjects will be. Yet establishing them is not enough; one must analyze how they interact. There are a number of standard answers. The first is that of methodological individualism. This position assumes that meaningful social action can be undertaken only by individuals or, failing data on them, by the smallest possible aggregate.[8] This simplifies the problem by excluding all but one level of analysis. On the other hand, methodological individualism regards the founding of the AMA as part of a local story, when in fact the AMA's place in any local chronology has less to do with local events than with when the local story happened to begin.[9] One may, therefore, accept a part/whole relation between levels *without* positing any direction of reduction. Yet this requires that one formalize the relation of occurrences in different levels. For some occurrences, there is a straightforward part/whole relation. Many local school foundings constitute a larger occurrence, "a flurry of action," at the state or national level. For others, the local occur-

rences create the environment or structure within which the larger occurrence takes place, but do not add up to it. Thus foundings of many local associations do not add up to the founding of a national association, as professions have often discovered. This linkage between levels as structured environments for each other seems to be the most empirically common relation. It allows for coherent, somewhat autonomous, stories of actors at each level, but also recognizes the mutual contingency of these stories.[10]

Substantive Colligation

Given levels within which coherent stories are possible, one must next decide what the substantive story is to be at each of those levels. What *are* the essential constituents of the professionalization story? Growth of knowledge? of association? of control? of education? How are these to be constructed into a hypothetical story of professionalization, and what are mechanisms that propel that story from one situation to the next? Do these mechanisms differ in the local, state, and national professionalization stories? Understanding these mechanisms is clearly the ultimate aim of this kind of theorizing about social events, just as understanding the causal relations of such variables as income and education is the ultimate aim of standard methods. Since this is not a substantive paper, I shall omit the conceptual arguments about professionalization in medicine or elsewhere. Yet in any application this discussion must occupy the central place.

The substantive discussion should yield a series of hypothetical stories. Each of these should be made up of events proper to the relevant level, connected into a sequence by mechanisms that are intentions or determinants.[11] An example might be:

> Early practitioners of a profession find themselves with many clients and many competitors. Their first need is to identify themselves as a coherent social group and so they form associations. The process of association leads immediately to comparisons of good and bad practice and consequently to an attempt by the "good" to dominate the area of professional practice. This leads to overt conflict with other groups, in which the nascent profession is repeatedly accused of not having its own house in order. These problems, involving both professional education and professional control, must therefore be remedied. Having confronted them, and possibly vanquishing its competition, a profession is free at last to dedicate most of its non-practice effort to the development of new knowledge.

The events here are association, dominance, education and control, and finally knowledge building, in that order. The underlying coherence of the story comes from a competitive achievement model, and the story itself is held together by intentional links following from that model, with the possible exception that outside competition (a determinant) is required to make the sequence run to completion.

Qualitative Analysis

Given this kind of story, one must analyze in detail the formal qualities of each event. The first such quality is *duration*. Duration is most obviously an issue in comparing events at different levels. Thus we feel that a national event of a given kind must take longer before we detect it than will a local event of similar kind. It is one thing to say that medical education is coming to New York City, and another to say that it is coming to the United States. Even within a given level, events vary widely in duration. A change in medical paradigm is an event that seems long and fuzzy; the coming of national organization is much shorter and sharper. It is important to confront the issue of duration directly. It is important conceptually because we have difficulty relating long and short events that are partially simultaneous, generally speaking of the long one as a "context" for the short one. It is important practically because our ability to place events in a definite order declines as their duration increases.[12]

Temporal extension in fact covers a multitude of patterns of activity. Behind an event like the coming of medical association, one may envisage a causal, driving force, a pressure to organize that derives from a number of sources. These sources may include outside competition, desire for learning or control of work, desire for status, and even simple demographic factors like the increase of doctors in a locality. This pressure or intensity of the event varies in time and may be imagined as instantaneously measurable.[13] Is there early strength, followed by habituation, apathy, and renewed activity? This seems to be the duration pattern of the association event at the local level; the typical local medical association starts strong, disappears for a while, then finally sputters into permanent life. Is there, on the other hand, a long and ever-growing concern pushed over a threshold by conclusive forces? This is the pattern of the association event at the national level, with the founding of the AMA in 1847 as the decisive threshold occurrence. Is there a flurry of little occurrences that precede or even replace a major one? This seems often to have been the case with medical education, as viewed at the local level. If sequence methods are to be effective in generalizing stories, they must address these differences. A pair of texts may illustrate these differences in narrative terms.

> Early practitioners often tried to build associations for common interests, but a variety of forces dissipated their efforts many times. Eventually one of these abortive organizations managed to survive.
>
> After extensive planning, the early practitioners built an organization, at first informal, that gradually came to involve the county as a whole, leading to the formal founding of the county society in 1821.

In each case, the event of association has temporal extension, but the *substantive* qualities of those durations differ sharply.

Duration has powerful implications for measurement. Suppose there are two events—a movement for association that produces immediate results followed by declining interest and a movement for education that slowly builds to the successful founding of a medical school. These conceptualized patterns of duration dictate how one measures and orders the two events. Even if their limits of temporal extension *exactly coincided*, one would regard association as coming first and would want to choose a conceptualization and indicators that would make this so.

A second quality of events, beyond their duration, is the property of *dispersion* or location. Some events propagate from one geographic or social subsection of a given level of analysis to another. Certainly this is true of the urge to associate in American medicine, which is contagious through emulation and a variety of other mechanisms. Such "diffusion events" may intervene in otherwise autonomous sequences of development within units of analysis, and should probably be analyzed at a higher level. They induce external effects similar to those (period) effects created when higher level events are treated as if they were local ones.[14] Such contagion can also be of a negative variety, suppressing standard connections between events and occurrences. The fact that there are no medical schools in Westchester County does not signify that concern for medical education never arose there, but rather that New York City is nearby. One might argue in this case either that the occurrences of school foundings in New York City were to some extent indicators of events upstate, or again that the state is the proper level of analysis.

Colligation as a formal process thus begins by isolating a level of analysis and the subjects within it. It then creates an abstract story, telling the events at each level and connecting them by intentions and determinants that are its ultimate objects of analysis. Finally, it carefully reflects on the quality of the events it is colligating—their duration, their dispersion. The colligated story must be coherent both in its limitation to a particular subject and, within the bounds set forth above, to a particular level of analysis. Its pattern of links must be closed, although external antecedent events may be allowed. Period events must be treated as such. Diffusion events should be treated separately and at a level consolidating their zone of diffusion into one unit of analysis. Events with duration must be carefully conceptualized and the meaning of their duration understood.

MEASUREMENT

The measure question in sequence data arises directly out of the separation of events and occurrences. As I have presented them, individual occurrences are instantaneous and there is no problem specifying their order. But *events* may overlap. This overlap has several motivations. The events may have duration in principle. They may be aggregates of events diffusing across lower level units of analysis, and hence have duration de facto. Or they may occur instantaneously, but be measurable only by indicators that follow some distribution in time. Of these three motivations, true event overlap is the most important and will be assumed throughout the following discussion.[15]

At the substantive level, it is clear that a followable and plausible story may include overlapping events. In the sequence hypothesis given earlier, it is not necessary that there be no overt intergroup conflict until the association event is over, or that there be no interest in knowledge until everything else is done. Rather the dominant portions of the events must fall in the order given, or the focal attention of the profession must fall on areas in the order of the story. Some such conceptual order must obtain for the story to seem properly ordered; but the overlap of colligated events implies the possible overlap of occurrence distributions, which in turn implies that estimating the order of the events is problematic. The central problem of sequence measurement is therefore that of inferring the true conceptual event order from the problematic empirical occurrence order.[16]

There are several steps to this inference. The first step is implicit in the above discussion of event duration. Events may be represented by continuous curves graphing "intensity of activity" as time progresses. The second step involves deciding what parameters are to be used to order these curves. The third step involves the relation between these curves and curves describing the time-density of event-generated occurrences. Since occurrences are our only means of knowing hypothetical events, any inference of the conceptual order rests on them. A fourth step involves the gathering of actual occurrence data, a step that includes several different kinds of selection. Finally a fifth step involves choosing indicators from the occurrence distributions that will do a good job, however that is defined, of indicating the event order parameters. Each of these steps creates a serious risk of error and so each must be carefully examined. Since the duration discussion has already examined the representation of events by curves of activity, I shall begin here with the problem of parameters.

The Choice of Order Parameters

Indication begins with the representation of events by curves showing event "intensity" in time. The central problems of choosing order parameters for events are (1) that these intensity functions may be of several different general shapes and (2) that they are subject to

censoring in their right-hand tails. The first of these problems, although by far the more profound, is the more easily dealt with. Suppose event A has an "exponential" intensity function while event B has a "bell-shaped" one. The order of the events seems unambiguous if the peak of the bell lies to the left of the exponential's first departure from the axis.[17] But as the peak moves to the right, the order is less and less sure. Yet the issue remains purely a conceptual one. The order of the two events should be decided only on theoretical grounds; one chooses parameters for this order that will reveal the most about the mechanisms thought to link them. Is there a threshold mechanism whereby a certain amount of A triggers B? Does B respond to a decline in A? Each of these cases dictates a certain choice of parameters for ordering the two events. There is tremendous complexity here, but it is complexity proper to the theoretical issues of the analysis and decidable within them.[18]

The more thorny problem in choosing parameters is the right-hand censoring. The events may have beginnings but no endings yet. This makes most parameters unstable. Thus concern for knowledge is something that rises but then perhaps continues to the present day. There are several ways of handling this problem, all of which depend on the characteristic shape of the event intensity functions. If those functions are single peaked functions in which the peak has already occurred (however one defines this), then the two modes can be used to order the events, even though neither event is "finished." An alternative strategy would use the maximum rates of positive increase in event intensity, the peaks of the first derivatives of the intensity functions. In practical terms, this approach assumes fairly smooth intensity functions. In theoretical terms, it redefines the event of interest as a rate-of-change event, rather than a level event; so profound a change would need justification on conceptual grounds.

A particularly difficult case of this "conceptual right-hand censoring" occurs when the curves are exponential, as is often assumed to be the case with concern for knowledge. One strategy is to impose a rate criterion here as well, ordering the event by the mode of the exponential's rate parameter. This procedure assumes, quite realistically, that that rate is not constant but follows a smooth, peaked function of some sort as time progresses. An alternative would be to employ a formal order statistic, ordering two such events by their first measurable (i.e., non-zero) intensity. However, there are great difficulties in defining such "measurability." Also, this measure places heavy reliance on the extreme of the distribution, when in many cases the mode is a much clearer guide to when the event "really happened."

Thus, irrespective of occurrences, there are serious problems in deciding what parameters to use to order overlapping events. This is, of course, true whether the conceptual right-hand censoring is present or not. The choice of a criterion for placing events in a particular order relation is an important conceptual issue, and one properly decided by the theoretical interests at hand. Censoring, however, makes the matter much worse. But it is of little use to decide, in the abstract, which of these parameters to employ. Ultimately they must all be estimated by occurrence data and these may place impossible constraints on the parameters of theoretical choice.

The Relation of Events and Occurrences

Events as I have here defined them are hypotheticals. They become concrete in occurrences. Indeed, occurrences may be defined as those outcomes of events that acquire a kind of independent, objective quality. The constellation of forces and mechanisms that brought about an event may change. The event has in turn its effect on the social world about it, then swirls downstream. But an occurrence—a new medical school, for example—may well remain after the disappearance of the underlying forces and the event of rising interest in education that created it in the first place. It takes on its own qualities as a fixture of the social environment, a determinant of possible future events.

Events generate an infinite number of occurrences. Some we take more seriously than others. A medical school founding is more important to us than the sponsorship, two or three times, of a six-week proprietary lectureship in methods of surgery. But both are outcomes of the event "rising interest in education" (and possibly of other events, of course). Thus, disregarding importance to us, an event clearly generates infinitely many occurrences. These follow some distribution in time, a distribution that will be more or less homologous to the event intensity function discussed before. It is useful to inquire to what extent the two curves may be regarded as interchangeable.

There are a number of problems with this interchange. Most of the occurrences I have discussed are recorded organizational outcomes of events—the outcome of a rising concern for education in schools, a rising concern for knowledge in journals, and so on. Such occurrences tend to be drastic—foundings, disbandings, splittings, joinings. The relation of these drastic occurrences to the event we conceptualize is, as any historian knows, inherently ambiguous. The founding of a school may arise out of a new interest in education, out of minor political squabbles in an old school, out of profit motives, out of the extrusion of charlatanism. The founding of an association may anticipate or it may follow a real necessity for organization. There is thus often an important but unknown bias in the indication of events by occurrences. The danger is that this bias may systematically vary from event to event. Some events

may produce occurrences that we take as important early in their duration; others, late. The early occurrences may be hollow shells—personal journals rather than true expressions of a group's thirst for knowledge, small clubs rather than professional societies. Later occurrences may be mere culminations of processes whose most important activities—the mobilization that led to the occurrence—may have long preceded them. Depending on our definitions of important occurrences, these hollow or culminating occurrences may bias our ultimate ordering of the events.

The obverse of this problem is that truly important occurrences may be invisible. Thus one important class of occurrences, which may be called transformations, produce no recorded organizational outcomes. There is a familiar truism that Johns Hopkins is the first "real" medical school in America and that Harvard pre-Hopkins is one school and Harvard post-Hopkins quite another. Yet there are few data available (other than the founding date of Hopkins, of course) to date such transformations, even though these would be excellent indicators of the event we call the rise (or reform) of medical education in the United States. Worse still, sometimes such transformations are implicit, occurring simply because a continuous process is progressing. The AMA means something very different in substantive terms when it includes 80 percent of the profession than when it included 20 percent. Yet it is clearly fallacious to talk about this transformation as exactly locatable in time.[19]

Thus, while the infinite occurrence distribution and the intensity function are homologous, the empirical occurrence data sets may take a very different shape. The empirical data sets are samples, and possibly biased samples, of the true, infinite occurrence distribution. Even if there is no such bias, however, there remains an important problem. Many methods of indication assume that the recorded occurrences will be "equally important" from one event to another. Such methods compare early subsets of two occurrence samples to see which event is earlier, and early occurrences will more likely be from whichever occurrence sample is larger. Other things being equal, events that involve large numbers of occurrences that we take seriously will appear earlier than *exactly coincident* events that involve few.[20] Use of such statistics requires exacting attention to equalizing levels of importance between events, since otherwise what is really a difference in relative level of importance will be measured as priority in order.

Another vagary of the event/occurrence relation is that different types of occurrences may derive from the same event in different localities. This is a central and difficult issue. Most stories of professionalization (like most action/response narratives) employ a functional causal logic. Associations and schools arise as structures that serve a function. But since one function can be served in several ways, one may easily find substitutable occurrences, either one of which serves the function involved.[21] To serve the function of control of work, for example, American professions customarily rely on exclusive, monopolistic licensure, while British ones rely on enrolling practitioners on a protected list. These two occurrences are both produced by the same event, a movement towards control of work. To say, then, that licenses come late in Britain, which they do when they come at all, is to miss the conceptual point. The desire to control work arises at the same point, but in Britain the American means of doing it is invoked only latterly if at all.

A final issue is the changing quality of the event/occurrence relation. There is a time when founding a journal indicates interest in local medical politics and perhaps local exchange of ideas. Later on, journals may indicate specialty interests, or national political, or knowledge concerns. Early associations indicate need for professional identity. Later ones often indicate dissatisfaction with that identity. Founding of medical libraries may signal concern for knowledge early on, but simple inertia later. Most important, all of these occurrence distributions run to the present even though we can safely assume that professionalization as a story (i.e., the events) is over at all three levels of analysis. It is thus clear that the difference between the occurrence distribution and the event intensity function increases as time passes. Curiously, one may also expect a similar divergence on the other end of the two. There the search for honorably ancient precursors leads chroniclers of occurrences to include in their data occurrences only tangentially related to the events of interest. (For example, does one regard the Wharton School as the first university-level school of accounting, given that accounting was only a minimal part of its early curriculum?) This early divergence proves especially problematic if order statistics are used for actual indication.

Actual Data Constraints

The issue of precursors raises the least controllable and hence most difficult source of error between the conceptual event order and the empirical indicators we choose for it. This is the method and quality of the occurrence data themselves.

Such data are usually assembled from different sources, occurrence by occurrence. These sources vary tremendously. Some are excellent and detailed, others careless and spotty. It is thus unlikely that an equal chance exists for occurrences of conceptually equal importance to enter the data. These problems are confounded by the shapes of the occurrence samples, which may be a function of how the data are gathered. In addition, occurrence data always have cutoff dates and these often differ from event to event. Conceptual censoring is thus worsened by direct censoring. Further, most data

sources give survival data; they report the organizations surviving on a given date ordered by their dates of foundation. Survival data strongly favor recent organizations, which do not have to be as durable to enter the data set. Even with exact data, the chance of discovering an occurrence of a given conceptual importance declines with its distance in the past.

The most practically dangerous of these data constraints are the cutoff dates. Occurrence data with uniform cutoff dates are rare. Normally occurrence data sets are assembled from lists published by various antiquaries at diverse times. Each followed his occurrence of interest up to his own present. One seeks to use all of these data. Yet the disparity of cutoffs makes the censoring problems insurmountable. Most often, the data are survival data with unequal cutoffs, which cannot even be "equalized" by rejecting all data after the earliest cutoff. The original, later cutoffs remain implicitly present in the contents (more important in the omissions) of the later data sets. These data constraints mean that any indicating strategy based on modes or moments must be sharply restricted.

The Choice of Formal Indication

Notwithstanding these sources of error, it is important to consider the practical possibilities for indication with occurrence data. The aim is to estimate the population parameters that are used to order the events. Given the sources of error just discussed, the determining quality of these estimators is not their bias, consistency, and efficiency, but rather a looser quality of robustness. Can they survive certain kinds of error? More important, are there aspects to the colligated models and occurrence data that rule out their use entirely?

The importance of indicator choice when events overlap can be graphically illustrated by considering what happens when one chooses at random. Suppose a body of sequence data has four events with known and finite duration and suppose that a conceptual order exists for the events. Let each event be represented by one indicator drawn at random from its duration. The starting and ending points of the four durations create at least one and at most seven separate intervals, depending on the pattern of overlap. Given the chance that an indicator of an event lies within one of these intervals, one may calculate the likelihood of observing one of the 24 possible sequences. (Some of these may, of course, be a priori impossible given the overlap pattern.) To investigate the behavior of observed sequences under various assumptions, assign the probabilities in these intervals arbitrarily. Table 1 shows, for a variety of possible patterns of overlap, the likelihood of observing certain sequences. Each four-row matrix gives the probability that an indicator for events 1, 2, 3, and 4 will be found in one of the seven (column) possible intervals. At the right are listed the sequences observed with probability greater than .10. This table shows how murky a situation event duration creates. Even if only one of the events overlaps others (case #1), there is a substantial problem. But when all four do, there is little chance of turning up any underlying sequence in particular without serious consideration of the issues of measurement. Examples in the table also show how mixing of different shapes and cutoff dates affects observed sequences under random measurement. It is clear that exponential distributions (cases #6-8) are particularly problematic.

TABLE 1
Probable Sequences Given Random Indication

Event	Pattern of Probabilities							Sequence	Probability
1	1.0	.0	.0	.0	.0	.0	.0	1234	.5500
2	.0	1.0	.0	.0	.0	.0	.0	1243	.2500
3	.0	.0	1.0	.0	.0	.0	.0	1423	.1500
4	.1	.2	.3	.4	.0	.0	.0		
1	.2	.2	.2	.4	.0	.0	.0	1234	.1413
2	.0	.2	.2	.6	.0	.0	.0	1243	.1200
3	.0	.0	.2	.8	.0	.0	.0		
4	.0	.0	.0	1.0	.0	.0	.0		
1	.1	.8	.1	.0	.0	.0	.0	1234	.7407
2	.0	.1	.8	.1	.0	.0	.0		
3	.0	.0	.1	.8	.1	.0	.0		
4	.0	.0	.0	.1	.8	.1	.0		
1	.2	.6	.2	.0	.0	.0	.0	1234	.5536
2	.0	.2	.6	.2	.0	.0	.0	1243	.1224
3	.0	.0	.2.	.6	.2	.0	.0	1324	.1364
4	.0	.0	.0	.2	.6	.2	.0	2134	.1224
1	.3	.4	.3	.0	.0	.0	.0	1234	.4261
2	.0	.3	.4	.3	.0	.0	.0	1243	.1404
3	.0	.0	.3	.4	.3	.0	.0	1324	.1610
4	.0	.0	.0	.3	.4	.3	.0	2134	.1404
1	.1	.2	.3	.4	.0	.0	.0	1234	.3424
2	.0	.1	.2	.3	.4	.0	.0	1243	.1207
3	.0	.0	.1	.2	.3	.4	.0	1324	.1489
4	.0	.0	.0	.1	.2	.3	.4	2134	.1467
1	.1	.2	.3	.4	.0	.0	.0	1234	.2072
2	.0	.1	.2	.3	.4	.0	.0	1324	.2243
3	.0	.0	.1	.6	.2	.1	.0	2134	.1231
4	.0	.0	.0	.1	.6	.2	.1		
1	.1	.2	.6	.1	.0	.0	.0	1234	.2195
2	.0	.1	.2	.3	.4	.0	.0	1324	.2525
3	.0	.0	.2	.5	.2	.1	.0	1342	.1026
4	.0	.0	.0	.2	.5	.2	.1	2134	.1018

Pattern of Probabilities = For each event (row), the likelihood of an indicator of that event falling in one of seven possible intervals.

Sequence = An observed order of events (rows) 1-4

Probability = The likelihood of observing a given sequence. Only sequences with $p > .10$ are reported.

Before discussing indicators in detail, it is wise to recall the overall data structure. Throughout the following discussion I assume the following general structure. The data comprise lists of several kinds of occurrences (for simplicity, one per event) for any number of cases. Thus, one might have lists of all the medical schools, all the medical journals, all the licensing laws, and so on ever founded or surviving until given dates that vary by occurrence. These are arranged by occurrence within case, each case (locality) having from zero to possibly several hundred occurrences of each of the several occurrence types. Depending on the method to be used to calculate the final estimated order, this data will be assembled in various ways. The most general assumption is that indicators will be chosen for each event from the relevant occurrence sample *within* each case, and these indicators will then be used to generate, case by case, information to be used to estimate the overall order of events. One might choose indicators, rank order them within the case, then analyze all the rank orders. Alternatively, one might choose indicators, calculate distances between them, and apply seriation methods to generate the overall order. Whatever the ordering algorithm, the assumption throughout the following is that indicators are chosen for each occurrence and case, and then combined within the case before aggregate analysis. There will be, for each case, as many indicators as there are non-zero occurrence samples.

The first choice about indication is whether to choose a uniform strategy or to tailor indication to the event, the associated occurrences, and the data at hand. Existing work on such sequences has chosen uniformity, using the first-order statistic (first occurrence) to indicate any event.[22] The justification for uniform indication is that it will presumably randomize errors of measurement. This presumption rests on assumptions about two of the error sources so far considered. The first source discussed, the impact of conceptual right-hand censoring on the theoretical choice of order parameters, generates purely conceptual difficulties; these problems in the theoretical parameters cannot be affected (or salvaged) by the choice of indicator. But assumptions are made about the second two sources of error. First, it is assumed that there is no bias due to hollow or invisible occurrences, or that there is no information that could allow the individual adjustment of indicators to offset this bias. It is also assumed that the event/occurrence relation does not change in quality or, if it does, that available information is not sufficient to calculate corrections for it. These are the only distinguishing assumptions made by uniform measurement about error arising from the event/occurrence relation. Assumptions about uniform levels of importance are proper to particular indicators, and both uniform and individualized measurement assume that substitutable occurrences have been lumped together into single occurrence samples.

But the assumptions of uniform measurement about the actual data constraints are quite strong. It assumes (1) that actual right-hand censoring is either unspecifiable or uniform from occurrence to occurrence and (2) that all data are survival or all data are exact. In the case of survival data, uniform measure must assume that survival rates are equal from occurrence to occurrence or their differences unspecifiable with existing information. As I shall show below, certain indicator choices can help withstand the violation of these assumptions, but they nonetheless set a very stringent standard for an occurrence data set. In general the use of uniform measure when these assumptions are violated will lead to artifactual results. The estimated conceptual order turns out to be a function of the various data set structures. The apparent rigor of uniform measure may thus obscure a dangerous situation.

In most applications, cautious application of individualized measure will be best. The great danger of individualized measure is the arbitrary finding. It is indeed difficult to specify the hollow occurrence bias or changes in the event/occurrence relation, however desirable it may be to do so. Knowing that one has survival data with cutoffs that vary from occurrence to occurrence does not tell one what to do about it. The best rule seems to be that departures from uniform measure should be justified conceptually and practically if possible. There should be, first, a conceptual reason for applying the particular type of indicator to the particular type of occurrence, with case study evidence brought to bear on the choice. Second, there should be empirical evidence that artifactual results are unlikely. Generally these will be tests applying a variety of ordering algorithms to the data under various types of uniform measure. Some data sets are alike in structure; they share a common cutoff date, they are both survival while all of the others are exact, or have some other similarities. If these comparable occurrence data always remain together in the final estimated order, irrespective of the type of uniform measure used and the ordering algorithm employed, then there is clear danger of artifactual results, results based on data set structure alone. If they do not, but rather shift around depending on how the data are analyzed, then there is some evidence that individualized measure is justifiable. If they always fall together, then artifactual results cannot be ruled out under either uniform or individual measure.

The uniform/individualized choice is followed by the choice of indicator itself. Indicators vary in the amount of information they use. For each event, a given case has from zero to possibly several hundred occurrences. (Consider the contrasting numbers of medical journals in Oswego and New York City, for example.) Some indicators use all of this information, others very little. At the very little end are the order statistics, which use the

complete occurrence listings only to identify, for a given observed occurrence sample and case, one particular occurrence as indicator. Quantiles use the same information, but with a slightly different purpose. They are obviously more flexible than order statistics if the observed number of occurrences of a given type varies widely from case to case; there is always a median medical school in a given locality, but not always a tenth, or even a second. (Of course, where there is none, any indicator is undefined.) Some indicators use the same ordering approach, then calculate a more complex function of the occurrence sample—the maximum rate of occurrences, the mean date of all occurrences in a "zone" defined by quantiles or order statistics. These are considerably more refined, but may demand substantial quantities of data. Since typically only a few cases have much data and most have little, such indicators require fallback definitions to enable them to function when data are insufficient for their normal definition. They are more useful when data are aggregated into larger units of analysis, as at the state level in studies of American medicine. Finally, some indicators make direct use of a large amount of the information in the observed occurrence samples of a given case. These would be standard parameters based on moments, such as the mean. Again, while these might be defined under sparse data (e.g., there is a mean date of medical society founding in Oswego), they would in effect be equal to first order statistics in the many cases where there was only one occurrence for a given event.

This conditioning of indicator choice by the relative distribution of data in different cases is, in fact, a conceptual issue. If maximum rate of medical school founding is the indicator, it will return the first and only medical school founding in the many localities with one school. The indicated date will be "later in the history of the local profession" in places like New York and Philadelphia, because the event indicated there will be different. The maximum rate of founding in Oswego may indicate the cresting of a drive for medical education there, while the maximum rate in New York will indicate New York's recognition of itself as a major regional center for medical education. These are clearly different events. Thus, a clear assumption of any analysis of this type is that units of analysis are comparable in character. Otherwise, complex indicators may, in fact, indicate different events.

Broadly speaking an indicator should be chosen to minimize the impact of the known errors on the final estimated orders. As I have noted, certain kinds of error cannot be affected by indicator choice. These include errors induced by the choice of conceptual order parameter that the indicator will estimate (including conceptual right-hand censoring) and by the failure to lump substitutable occurrences. All other errors are relevant to the choice.

The strengths and weaknesses of the various indicators can be shortly summarized. Order statistics, particularly the early ones, have real strengths. They enable one to solve the actual right-hand censoring problems, including the messy difficulties of cutoffs that vary from occurrence to occurrence. They are also useful if the event/occurrence relation degenerates as time passes. On the other hand they have serious difficulties. They are subject to strong precursor effects if gatherers of occurrence data sets have gone beyond the real beginnings of their field to marginally related occurrences. They are also more likely than the middle range quantiles and rate-based or moment-based indicators to be affected by variation in survival rates from one occurrence type to another in survival data. Most important, however, they are massively affected by the differences in the relative importance of the occurrences from event to event, as discussed above. Indeed, they assume that a unit occurrence of each type is of exactly equivalent importance.

The fundamental tradeoff in moving from order statistics to virtually any other form of indicator is to shed the vulnerability on importance, on precursors, and, to some extent, on survival rates, but to gain in return the vulnerability to right-hand censoring and changing event/occurrence relations. The decision thus rests on which set of problems is worse. The quantiles are a good example. These familiar statistics, from 10 percent to 50 percent to 99 percent, are not affected at all by variations in occurrence importance, except to the extent that hollow or invisible occurrences are involved. (These are, of course, a problem for order statistics as well.) Quantiles are also more stable under variation in occurrence survival than are the extreme order statistics (which are the best for handling right-hand censoring) and are similarly less influenced by the single dubiously relevant precursor. But they are deformed terribly by uneven cutoff dates. These can easily end up determining the observed order in cases where there is wide variation in the structure of data for the different occurrence types. These same problems hold for means and other indicators based on moments.

Indicators based on rates are perhaps less vulnerable. Examples are "first time two medical schools are founded within ten years of one another" (a threshold rate) or "maximum rate of observed foundings of medical schools" (a true rate measure). The difficulty with these measures is that they require substantial data; indeed, in the second case, effectively continuous data. Otherwise, they interact with the variation in occurrences per case to produce the artifactual results noted above. They indicate one thing in small localities and another in large ones. Where there are substantial data on all occurrences for most or all cases, they will work well in most applications. A caution is necessary in occurrences with exponential shape, however, particularly with survival

data. There the exponential growth curve is exacerbated by the greater survival of the more recent foundations. The result is to locate the maximum rate of foundation, arbitrarily, immediately before the date of cutoff.

It is also possible to use zone indicators, defining the edges of the zone with either order statistics or quantiles. Thus one could define a zone as all occurrences between the first and the fifth of a given occurrence type, or all occurrences lying between the quartile and the median. It is again clear that such measures are only appropriate when data are extensive. However, given such zones, a composite indicator can then be based on all the dates in the zone or, with such ordering algorithms as standard seriation, the data may be directly entered. These zone indicators would have the strengths and weaknesses of the means used to define them—order statistics or quantiles—and require no further discussion.

CONCLUSION

The tasks of colligation and measurement discussed in this paper are the beginning steps in the analysis of a particular sequence hypothesis. Such an analysis would, of course, be completed by application of a procedure testing the hypothesis on the data. But both colligation and measurement look beyond themselves. Colligation, by necessity, refers back to a discourse or metatheory for sequence hypotheses. It requires general models for mechanisms in sequences, for different types of order effects. Measurement, on the other hand, refers forward to the algorithms that will transform the information of the indicators into effective tests of hypotheses. In my previous work I analyzed these problems of metatheory and method. Taken together, this paper and its predecessor are meant to raise the major questions of sequence analysis and to answer as many of them as I can.[23]

ACKNOWLEDGMENTS

Like other papers in this issue, this article was presented at the Sloan Foundation Conference on Statistics, Epistemology, and History at MIT, 10–12 November 1983. I thank Peter Smith for the invitation to speak there and my colleagues for an unusually productive meeting. In its MIT incarnation, this paper included a full analysis of the American medical data discussed throughout the present article. Dire needs for space and clarity dictated its removal. I have a few personal acknowledgments. Most important, some of the central ideas of this paper came out of conversations with my erstwhile research assistant, Bruce Carruthers, presently at the University of Chicago. It was while we lazily argued, one summer afternoon under the oaks by the "passion puddle" on Rutgers's Cook Campus, that the concept of indication bubbled to the surface. Second, I must thank Harry Bredemeier, who asked, in his typically gracious manner, a number of devastating questions about an earlier draft. Finally, I must thank my wife, Dr. Susan Schlough, whose real-money job at Bell Labs provided the cash that bought the IBM PC that did the calculating and word processing.

NOTES

1. My earlier discussion of conceptualizing and testing sequence regularities is "Sequences of Social Events," *Historical Methods* 16(1983):129–147. Beyond the matters discussed there and in the present paper, there remain other difficult issues. Thus the sequence approach derives an occurrence's meaning from its past, the standard one from a fixed, atemporal scale. I am pursuing such problems in further work. On colligation, see W. Whewell, *The Philosophy of Inductive Science*, (London, 1847); W. H. Walsh, *An Introduction to the Philosophy of History*, (London, 1958); and C. B. McCullagh, "Colligation and Classification in History," *History and Theory*, 17(1978):267–284. The exact meaning of the term is considered below.
2. In insisting on this distinction, I am diverging sharply from at least one important body of events research, that of event study in international relations. There the two tasks are usually run together, although sometimes colligation has been performed, ex post facto, by factor analysis (see examples in E. E. Azar and J. D. Ben-Dak [eds.], *The Theory and Practice of Events Research* [New York, 1975]).
3. The idea of a followable narrative is taken from W. B. Gallie, *History and the Philosophical Understanding* (New York, 1968). The criteria of plausibility and followability that Gallie sets for particular historical narratives are equally valid for the general (universal) narratives discussed here. Also, the veiled implication of the examples above that a given story link cannot have more than one motivation (intention or determinant) should not be taken as given. In first creating a generic narrative one ought to aim for single motivations. But one must ultimately allow multiple intentions and determinants. Certainly in particular narratives, multiple motivations are standard historical practice. However, I have written most of this analysis assuming single motivations in order to make the rest of the exposition clear. Further, one should not assume that all intentions are internal to the subject of the story and all determinants external. I am grateful to Harry Bredemeier for demanding this clarification. The mathematically oriented reader will note an underlying metaphor here. I am viewing the mechanism of each story link as a mapping whose range is the set of possible events and whose domain is the set of outcomes of the prior mechanism (mapping), i.e., prior events. Events are in each case collections whose elements are occurrences, the individual outcomes. The original domain is the subject of the story. The criterion of closure is equivalent to requiring that the range of each mapping be a subset of the domain of the next. "Outside" antecedents may be allowed, but in actual storytelling one ignores consequents that have no future role in the story, and tries to define events so that there are few of these external consequents. While this metaphor is not exact, it may make the underlying concept clearer.
4. A fine discussion of these issues is found in A. C. Danto, *The Analytical Philosophy of History*, (Cambridge, Eng., 1965). The rules governing these changes of meaning are clear in the case of the standard approach; an occurrence's location within a variable must be consistent within a given research project, and the variables must themselves refer to mutually coherent theoretical entities. These rules are less clear in the colligation of occurrences into events. In part this is simply because colligation lacks the conventions that have arisen in several decades of positivist social science, linking certain indicators definitively with certain variable constructs (i.e., years of schooling and education). In part it is because the focus of traditional narrative history on particulars permits more recolligation than the focus of social science on universals does reconceptualization. Even though such colligations as "The Progressive Era" and "Reconstruction" seem as unchallengeable as the connection of education and years of schooling, more normal historical work recolligates occurrences as part of new stories than routine social science redefines variable/indicator links. Normal social science is more concerned with redefining model relations.
5. The concept of "naturally colligating the same way" is, of course, dangerously vague. My position is merely that some colligations work with given occurrences better than others. For a pointed discussion of this issue in a closely related context, see W. C.

Booth, "M. H. Abrams: Historian as Critic, Critic as Pluralist," *Critical Inquiry* 2(1976):411-445 and "Preserving the Exemplar," *Critical Inquiry* 3(1977):407-423; M. H. Abrams, "Rationality and Imagination in Cultural History," *Critical Inquiry* 2(1976):447-464 and "The Deconstructive Angel," *Critical Inquiry* 3(1977):425-438; and J. H. Miller, "The Critic as Host," *Critical Inquiry* 3(1977):439-477.

6. D. L. Hull, "Central Subjects and Historical Narratives," *History and Theory* 14(1975):253-274. I am of course assuming that social reality can in fact be sectioned into such levels. A recent theoretical analysis holds that many or most interesting natural processes display the same regularity (or more important, irregularity) at any level of analysis. See B. B. Mandelbrot, *Fractals: Form, Chance, and Dimension* (San Francisco, 1977). The symbolic interactionist tradition, however, holds that interactional (i.e., story) reality takes the shape that actors impose on it. Since actors believe in and impose a set of ordered levels on their interactions, those levels of interaction *become* part of the interactional world itself. For a general exposition combining this view with the similar one of phenomenological sociology, see P. Berger and T. Luckmann, *The Social Construction of Reality* (Garden City, New York, 1967).
7. For a general discussion of the history of ethics codes see A. Abbott, "Professional Ethics," *American Journal of Sociology* 88(1983):855-885. Medicine in fact had a short prehistory of local codes, see D. E. Konold, *A History of American Medical Ethics* (Madison WI, 1962). This distinction in level of codes seems related to the type of code origin. The codes that arrived at the national level first were codes imposed by professional elites to control wayward and often lower status practitioners on the professional periphery. The codes that arose locally often began first in intense debates among local professionals concerning fundamental ethical issues. This interaction between level of analysis and motivation for codes emphasizes the importance of working with hypothetical events rather than occurrences directly. If the ethics codes are the events, the issue of their meaning and motivation is hidden in the idea of the event itself. If concern for control or concern for ethics is the event, then the choice of ethics codes as indicators for one or the other at the national or state or local level becomes a conscious choice.
8. The position of methodological individualism is analyzed in detail by A. C. Danto, *Analytical Philosophy*, and by a number of essays in P. Gardiner (ed.), *Theories of History* (New York, 1959). It is anathema in the social sciences other than microeconomics, which operate on a mixed social realism/nominalism (situations or actors real in their consequences are real in fact) or a strict social realism (e.g., Durkheim).
9. Formally the founding of the AMA is a period event that appears at an arbitrary age in any given local history. Assembling the local histories into a synthetic cohort of histories will scramble the period event unrecognizably because of this arbitrary location.
10. One body of literature addressing this problem of multiple, mutually contingent levels is the levels of selection literature in population biology. See M. J. Wade, "A Critical Review of the Models of Group Selection," *The Quarterly Review of Biology* 53(1978):101-114.
11. The requirements of a hypothetical story and some of its properties are analyzed in my earlier paper, "Sequences of Social Events."
12. A classic analysis of the duration/levels interaction problem is found in Danto, *Analytical Philosophy*. Danto gives a particularly nice illustration. Referring to Petrarch's celebrated essay about Mont Ventoux, he remarks that Petrarch's brother saw him climb the mountain but could not have seen him open the Renaissance. The methodological individualist handles the problem of these long but fuzzy events by breaking down, for example, the coming of a new paradigm into a simple sequence of observed percentages of doctors accepting the new paradigm. This acceptance, of course, is just as fuzzy at the individual level as the general event was at the national one. Finally on the context issue, see my paper on "Sequences," particularly n. 10 and 30.
13. This concept of intensity functions that graph activity in time will prove essential in the later measurement discussion. I envision here a simple cartesian plane with time as abscissa and intensity of activity as ordinate. Note that the sources for this intensity are the mechanisms for links in the story, discussed above. I have thus given here a multiple motivation for this link (see also n. 3).
14. The effect here is somewhat similar to that discussed in n. 9. Once again the problem arises when one aggregates the various cases into a synthetic cohort. A fundamental assumption of that aggregation is casewise independence. Diffusion events violate that assumption and cast doubt on any subsequent results. The assumption of independence is both very important and, often, very false. One school of sociological theorists, the mathematical structuralists led by Harrison White, have argued that most interesting social stories are in fact shaped completely by their interdependence. See, e.g., H. C. White, *Chains of Opportunity* (Cambridge, 1970) and "Where Do Markets Come From?" *American Journal of Sociology* 87(1981):517-547.
15. The formal representations of these three motivations are essentially equivalent at the aggregate level. Diffusion processes can of course be represented by formal diffusion models at the lower level. The case of instantaneous events with distributed indicators is interesting, but seems ultimately whimsical. Nonetheless, this model is implicit in Foucault's view of the change of *mentalities*; see, e.g., *The Archaeology of Knowledge* (New York, 1972).
16. There is real ambiguity about what this "conceptual order" means. For the methodological individualist, events themselves are arbitrary aggregations of occurrences (observed and unobserved), which follow distributions that may overlap in time. Conceptual order then refers to some simplification of the relation of these distributions that is at best an analytic convenience, and at worst a complete misrepresentation. Again the answer to this argument is the interactionist assumption that since social actors organize their perception and performance of behavior in terms of stories, consequential events are themselves ordered. Social life is ordered because people live it that way. On this assumption, the conceptual or substantive order of overlapping events is a real property of the story concerned, unconditioned by the event overlap. It is thus still a valid target of analysis. Without taking the extreme ethnomethodological position that social life is completely organized by scripts, one can still hold that this assumption is sufficiently plausible to justify seeing what one can get with it.
17. This departure from the axis is necessarily discontinuous, in a technical sense. I am using terms like exponential loosely here, to refer to shapes, rather than technically. I hope to avoid technicalities. Thus, I shall use the common but technically erroneous equation of "distribution" with "density function." I have, however, tried to be consistent, particularly about the distinction below between the infinite occurrence distribution and the observed occurrence samples.
18. I am implicitly assuming a ternary order relation here. A is before, simultaneous with, or after B. The aim in choosing parameters is to find ones that will return this information. More complex order relations are possible, with correspondingly more complex parameterizations.
19. From a substantive point of view, it is equally fallacious to make the alluring assumption that the change in meaning is described by a well-behaved, perhaps linear function of the percentage of membership. As Foucault and his school argue, the actual happening of such transformations, even in profound, general structures, may be so sudden as to seem discontinuous. Interactionist theory provides a mechanism for this. Violations and problems in the most abstract, framing assumptions of a discourse have to pile up for some time before interacting parties recognize that different framing assumptions might enable a freer or more effective discourse. Once this recognition occurs, however, its implications for concrete discourse are both immediate and great. Kuhn argued a similar point about scientific revolutions.
20. These important indicators are called order statistics, and they all depend on sample size. Since any occurrence distribution is a sample, its order statistics are a function of our definition of importance. A simple calculation shows why this is so. Suppose that we have two events which generate identical occurrence distributions F and G, and consider samples f and g of occurrences from each distribution. We may arbitrarily regard one sample, say $f = [f(1), f(2), \ldots f(n)]$ as fixed in size and order, with $f(1)$ as its first element. If $g(f)$ is the probability that $g(x) < f(1)$, then the probability that there is no $g(i)$ in $g = [g(1), g(2), \ldots g(m)]$ such that $g(i) < f(1)$

is equal to $[1 - g(f)]^m$. Clearly this probability decreases monotonically as m increases. The "first" occurrence comes more and more certainly from event G merely because of the sample size. Sample size, of course, may be a function of many things. Some of these relate to the investigator, such as the amount of time spent researching one particular event relative to that spent on others, the stringency of the definitional requirements for occurrences, and so on. Others are theoretical constraints—there are only 50 states that are available to have licensing laws. Others, and these are the issue here, pertain to the nature of the event and the kinds of occurrences it generates. The urge to associate in medicine tends to create one organization in the locality and then strengthen it. The urge to develop knowledge, on the other hand, leads to a nearly infinite proliferation of journals. If one association equals one journal in importance, then first order statistics will inevitably make journals first.

21. Functional links are particularly difficult, and particularly important, in story models. Useful references on functional arguments are A. L. Stinchcombe, *Constructing Social Theories* (New York, 1968), and W. Wimsatt, "The Logical Structure of Functional Statements," *Studies in History and Philosophy of Science* 3(1972):1:1-80.
22. A classic paper attempting to analyze professionalization sequences is H. Wilensky, "The Professionalization of Everyone?" *American Journal of Sociology*, 70(1964):137-158. Wilensky's paper was a stimulus for this work and, for its time, a definitive analysis. But its chief finding—that there is a clear sequence of professionalization in America—seemed to rest on a mixture of errors of aggregation and measurement. I have here tried to disentangle those errors. I am presently completing an analysis of a 2000 occurrence data set on American medicine following the principles outlined here, aiming to replace Wilensky's work with an adequate conceptualization and test of professionalization.
23. Although my ideas have changed somewhat since the earlier paper ("Sequences") was completed in early 1982, that paper still makes a good pair with the present one. The major change I would make in the earlier paper would be to emphasize two new methodological approaches. One is a group of sequence analytic methods based on "distance" between sequences proper, rather than events in them. D. Sankoff and J. B. Kruskal's collection (*Time Warps, String Edits, and Macromolecules*, [Reading, MA, 1983]), then only in press, has now come out as an excellent collection of these methods. The other approach I would now emphasize is the potentially fruitful application of artificial intelligence models to sequence analysis, for which one may consult the references of Hayward Alker's "Historical Argumentation and Statistical Inference: Towards More Appropriate Logic for Historical Research." *Historical Methods*, 17 (1984) 164-173.