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by Larry J. Griffin

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Introduction

Out of fashion for decades in American sociology, historical sociology in the U.S. gained force and then took off in the 1980s. Inspired by sociology's holy trinity – Marx, Durkheim, and Weber – and by latter practitioners of the craft, particularly Reinhard Bendix, Barrington Moore, and Immanuel Wallerstein, a handful of sociologists began in that decade to forge a methodologically self-conscious historical sociology, one often with a strong comparative dimension. Most responsible for this resurrection were Charles Tilly and Theda Skocpol. Tilly, of course, had been labouring in the vineyard of history for years, but in two books, *As Sociology Meets History*, and, more conspicuously and successfully, *Big Structures, Large Processes, Huge Comparisons* [Tilly 1981; 1984], he issued assertive programmatic visions of comparative historical sociology. For her part, Skocpol had already begun to reorient sociological inquiry with her hugely influential comparative-historical study, *States and Social Revolutions*, an analysis of revolutionary processes in three *ancien regime* states, pre-1789 France, czarist Russia, and Imperial China. But it was the her 1984 edited volume *Vision and Method in Historical Sociology*, and, for my money, at least, her introduction and conclusion [Skcopol 1984a; 1984b] to that book that fuelled and cemented her authority in the practice of historical sociology [see also Bonnell 1980; Skocpol and Somers 1980].

Skocpol and Tilly, of course, were not the only ones who, at about the same time, were pushing the frontiers of the discipline by urging us to emphasize comparisons and/or to historicize sociological inquiry. In 1987, Charles Ragin's *The Compar-*

ative Method – both a plea for and, with "qualitative comparative analysis" (QCA), a formal methodology for systematic comparative analysis – came out. Because QCA is rooted in a static logic (Boolean algebra) and is not intrinsically "historical" in any sense of the term, its impact on historical sociology has been limited, albeit quite powerful in some circles. A few years earlier, both Arthur Stinchcombe and British sociologist Philip Abrams [1980; 1982] published works extolling the promise of historical sociology, and, in the case of Abrams, offering a rough outline of how to go about doing it. Stinchcombe's *Theoretical Methods in Social History* [Stinchcombe 1978] is a quirky, highly idiosyncratic, and smart treatment of the topic. Abrams's *Historical Sociology* [Abrams 1982], still a woefully underappreciated gem, argues that there is no essential difference between history and sociology – only a matter of detail and scale separate the two – and remains the most analytically "radical" of the entire bunch. Neither of these, though, had the impact of those, substantive as well as methodological, by Skocpol and Tilly.

So by the end of 1980s, historical sociology was well established – indeed, it was practiced by some of the most distinguished and important scholars in all of American sociology – and the stage was set for further innovation. And the innovations did come. In 1990-91, I began experimenting with event-structure analysis [Griffin 1993], which I discuss in detail below.

Also, in the late 1980s and early 1990s, Roberto Franzosi [1989; 1994] began to analyze textual or "narrative" data using semantic text grammars, and Andy Abbott [1990] began publishing a series of articles on the importance of time and narrative in the journal *Historical Methods*. Putting some of these ideas to work, he and Alexandria Hrycak [1990] also adapted optimal matching analysis – a mathematical procedure designed to analyze sequence data – to social inquiry. The impact of Abbott's conceptual explorations of temporality was modest initially, but he did reach and shape the thinking of those of us (myself included) who had begun to explore time and its role in historical sociology.[1] So, too, did Philip Abrams. I noted above that his book *Historical Sociology* was the most "radical" – here meaning "historical" – intervention in the ongoing debate about the whys and hows of historical sociology. A quarter of a century later, it can still jar, can still unleash and stoke creative fires: image "explaining" V.I. Lenin, as he said a good historical sociology could accomplish,

[1] With Larry Isaac [Isaac and Griffin 1989], I had been experimenting with a form of time-series regression known as time-varying parameter estimation (TVPE). Unlike conventional times-series analysis, TVPE does not assume that time conveys a seamlessly and a historically general and temporally homogeneous causal process. Rather, it historicizes time-series analysis by showing how causal inferences about historical processes change with the addition and deletion of specific chunks of time.

as one would go about explaining any other unfolding happening! [Laslett 1991, for one, took him at his word].

Especially in his audaciously brilliant chapter "Explaining Events," Abrams hit on and propagated an event-centered mode of inquiry of immense potential power, one privileging and exploiting time, understood here as the temporally ordered, interconnected sequences of actions that constitute an event. His approach is at some analytical distance from that of Tilly and Skocpol. The former had long used time-series analysis and other quantitative techniques, and the latter deployed what Sewell [2005, 95] called "experimental" time, a form of time in which historical temporality is viewed as an ahistorical "congealed block" which is then sliced into "artificially interchangeable units" suitable for conventional scientific and causal analysis, here Millsean-type logical comparisons. For Skocpol, then, her use of temporality was inferentially limited to what I have elsewhere called "time-as-context" [Griffin 1992].[2] In contradistinction to Skocpol and other formal comparativists, such as Ragin, Abbott [1990; 1991; 1992] also persuasively argued that the processual nature of social life – whether the life of an individual, an institution, or a revolution – requires thinking temporally and narratively. In particular, narrative, in focusing on temporality and social action, promises deep theoretical knowledge about the mutually constitutive interplay of agency and social structure, a dynamic continuously occurring *in* time and *through* time [Giddens 1979; Abrams 1982; Sewell 2005]. Events are the "indispensable prism" [Abrams 1982, 192] through which this process – labeled "structuring" by Abrams [1982, 3] – may be observed. As "portentous outcomes (...) transformation device(s) between past and present" [Abrams 1982, 191], events are imbued with sociological import because it is *in* and *through* their unfolding that we see the collision of social structure and social action. Narratives are crucial here because they are how we analytically reconstitute, describe and comprehend events.

Sociological explanation of how and why an event unfolds as it does requires a type of causal logic that is grounded in "time" and in distinctively temporal processes [Abrams 1982, 302; Aminzade 1992]. Most sociological explanations are comparative and generalizing, not temporal, in their logic [Lieberson 1985; Abell 1987]. They rely on logical comparisons across a few cases – Skocpol's [1979] preferred method – or analysis of statistical regularities across many cases, or logical subsumption of

[2] By this, I mean that Skocpol and other "contextualists" use the temporal context of an event as an indicator of institutional or structural arrangements, possibilities or contradictions. Occasionally, it is tapped for evidence of the modal interests, thoughts or other mental processes of the "typical" actor in the time period studied [Beer 1963]. Contextual differences across cases indicate differences in these arrangements/processes, and it is these differences that are compared, contrasted, and analyzed logically and statistically to make causal and statistical generalizations. See also Burawoy [1989].

particular cases under broader historical generalizations and theoretically general laws [Abbott 1991; Griffin 1992]. Narratives, on the other hand, are intrinsically temporal – that is to say, historical – in both construction and explanatory logic. They portend a very different sort of sociology from the sort practiced by scholars uninterested in history, even, for that matter, from the sort of historical sociology practiced by Skocpol, Tilly, and Ragin.

Narrative explanation takes the form of an unfolding, open-ended story fraught with conjunctures and contingency, where what happens, an action, in fact happens because of its order and position in the story. Narrative therefore permits a form of sequential causation that allows for twisting, varied and heterogeneous time paths to a particular outcome. In narratives, we can see how the cumulative consequences of past actions increasingly constrain and limit future action. This notion of "path dependency" [Aminzade 1992] can be used to examine the determinants of key actions at any given historical moment, counterfactually explore actions and choices not taken, and help explain why sequential paths are sustained through time. We also see the "emergence of novelty" in narrative [Porter 1981, 34], those contingent, unpredictable acts often having big consequences that are nonetheless explicable in light of temporal ordering and connectedness. Thus through how it organizes information and fosters understanding of sequentially unfolding action, narrative encourages, even coerces, far more explicit deployment of the sort of temporal causation envisioned by Abbott, Abrams, and others than do other analytical procedures. How, though, to implement narrative analysis of events in a rigorous, replicable fashion? How, that is, can a sociologist simultaneously use and transcend an event's temporality to understand the reason's for an event's unfolding, and how to do so in a way that permits critics to understand and evaluate the soundness and fecundity of the explanation without losing narrative, the unfolding story? For me, the answer was event-structure analysis.[3]

What ESA Does and How It Works[4]

Event-structure analysis (ESA) is a member of a family of formal analytic procedures designed to analyze and interpret text, in particular the temporal sequences constituting the narrative of an event. It was developed by David Heise [1988; 1989;

[3] Some material in this and the preceding two paragraphs first appeared in Griffin [1992].

[4] Much of this section is a slight modification of material first appearing in Griffin [1993] and Griffin and Korstad [1998]. See http://www.indiana.edu/~socpsy/ESA/ for more information about ESA, about downloading ETHNO, citations to works that have used ESA, etc.

Corsaro and Heise 1990], a social psychologist at Indiana University. Influenced by intellectual developments in cognitive anthropology as well as by rational-choice theory, Heise developed ESA to study cultural routines and the subjective representations of reality, presenting it as a tool both to impute causality and to interpret meaning in ethnographic data. Its basic purpose is to aid the analyst in "unpacking" an event – that is, in breaking it into its constituent parts, which are sequences of actions – and analytically reconstituting it as a causal interpretation of what happened and why it happened as it did.[5] It differs therefore from much conventional narrative history in that it forces researchers to develop an *accounting for* the event rather than simply an *account of* it [Abrams 1982]. ESA focuses on and exploits an event's "narrativity" – its temporal orderliness, connectedness, and unfolding – thereby helping historians and social scientists infer causal links between actions in an event, identify its contingencies and follow their consequences, and explore its myriad sequential patterns. Unlike most other formal analytical techniques, it is completely nonnumeric and non-statistical, and it most decidedly does not answer questions analysts might wish to ask of their data ("does the presence of strong trade unions in a nation result in the transfer of income from rich to poor"?; "do unions impede or stimulate strike activity?"). Just about all it does is ask researchers questions about the actions making up the event and then diagram their responses. ESA's value, then, is largely heuristic, centered on how it relentlessly probes the analyst's construction, comprehension and interpretation of the event.

To analyze an event one first prepares a chronology of actions which, in the analyst's mind, define the event. While ETHNO offers no direct assistance at this point, preliminary event-structure analysis often helps the analyst to detect weaknesses in the chronology and to refine it in various ways. The chronology is entered as input into ETHNO, where it is then reformulated as a series of questions about the causal connections among actions constituting the chronology. ESA thus forces the analyst to replace temporal order as the basis for attributing causality with her or his expert judgement or knowledge about potential causal ties among these actions, judgements limited by an action's temporal position in a sequence but not determined by it. It does this by quite literally transforming the chronology into a series of "yes/no" questions where the analyst/expert is asked if a temporal antecedent ("or a similar event") is required for the occurrence of a subsequent action. Responses to ETHNO's questions are displayed as a "directed" or causal diagram of the logical

[5] On "causal interpretation," the best source remains Max Weber, *The Methodology of the Social Sciences* [Weber 1905], pp. 113-188, especially p. 169. Clifford Geertz [1980], who was greatly influenced by Weber, uses a similar phrase, "interpretive explanation."

structure of action underlying the event's narrative or chronology. The diagram, finally, is the event structure and represents the analyst's interpretation of the causal connections among the unfolding actions constituting the event's chronology.

ESA also allows explicit generalization of the initial (or "concrete") configuration. Here the analyst abstracts from the concrete event structure in two ways. First, actions which, in the expert's judgement, are embellishments on or otherwise incidental to the main paths of action or are without imputed cultural or historical significance can be dropped from the "abstracted" chronology. Second, actions which are retained for further analysis are conceptualized as instances of theoretically general sequential actions. ETHNO then interrogates the expert about the causal relations between these generalized actions just as it did about the concrete actions. As the analyst answers these questions, ETHNO, again as before, constructs a diagram of action, this time structuring the theoretically abstracted actions at a higher level of generality and parallel to the concrete event structure. The general event structure is subsequently assessed for logical inconsistencies both against ETHNO's internal logic about how action must unfold (e.g., for an action to occur it must be "primed" by a previous action) and the causal imputations embedded in concrete event structure. Logical contradictions between the general and the concrete event structures may necessitate altering the imputed causality in either or both to make each logically consistent with the other.

ESA, finally, permits strict comparisons of events. Events' general structures can be directly compared in numbers of ways, and numbers of discrete particular events – after comparison – can subsumed under a one theoretically *and* historically general logic of action. Events' general logic of action, additionally, can be strictly compared to a purely theoretical representation of a general process to permit theory testing and theory development.

What ETHNO does not do is answer its own questions, and, as I noted above, causality is not "discovered" through its use. The analyst, not the software, possesses the knowledge needed to structure and interpret the event. By forcing the user to be precise and meticulous about the construction of historical narratives, to reason causally about their sequences, and to be clear about the bases of causal judgements, ESA lays bare the investigator's understanding so lucidly – indeed starkly, as a diagram of the logic of action – that insights into causal significance are intensely sharpened and problems of causal interpretation are prominently displayed.

More firmly and self-consciously than do most formal analytical procedures, ESA thus bridges the often damaging methodological chasm separating narrative history from generalizing social science. On the one hand, it borrows from formal social science methodology several features, including the

a) explicit deployment of theoretical concepts and hypotheses about social life,

b) application, validation, and development of theoretical, historical, and causal generalizations, and

c) use of replicable procedures of analysis.

On the other hand, ESA also mimics in important ways how many historians and historically-oriented social scientists, although themselves drawing on less formal analytical strategies, actually reason to infer causality and meaning from an event. Thus, it requires analysts to

a) situate events in their historical and cultural contexts and then capitalize on contextual knowledge for explanatory and interpretive purposes,

b) focus on actors and on social action, thereby fostering appreciation and comprehension of agency; that is, of how women and men actively mold, if in ways they do not always foresee or necessarily wish, their world,

c) evoke "imaginative reconstruction" of the actor's world and her/his motives, strategies, and understandings,

d) view the precise unfolding of an event as of cardinal importance to its interpretation, including its interpretation as an instance of a general phenomenon,

e) maintain, through the question-and-answer routine discussed above, fidelity with the interrogatory spirit undergirding much historical reasoning by requiring the analyst to "interrogate" and "cross-examine" events for evidence of causal significance,[6]

f) adopt the understanding of historical events as configurational, contingent happenings characterized by what philosopher-historian Porter [1981] labels "the emergence of novelty," and

g) rely on their substantive judgement and interpretive skill rather than on prefigured (and thus historically "inflexible" or "static") theoretical generalizations, or logical or statistical algorithms or rules.

ESA's unique analytical efficacy, then, resides in the fact that its very logic of operation – that is, in what it does and what it demands of the analyst – synthesizes social science and historical methodologies while also empowering each to speak (as it were) with its own "voice." This synthesis is seen in research practice both through 1) ESA's integration of the theoretically/ empirically general and the historically particular so thoroughly as to render their differences largely moot, and 2) its merger of two modes of inquiry often juxtaposed against each other, explanation and interpretation; with ESA, analysts explain *as and because* they are compelled to interpret, and they interpret *as and because* causal explanation is demanded. ESA demands what

[6] "Interrogate" is from E.P. Thompson [1978], "cross-examine" is from Marc Bloch [1953].

E.P. Thompson [1978] has called a "disciplined dialogue" between theory and evidence. In particular, it mandates near constant "particularization" of the theoretically general ("what bearing does this generalization have for this particular action? for this particular event?") and the "generalization" of the historically particular ("what is the general meaning and significance of this action? what generally induces action of this sort?"). Narrative is buttressed by ESA's analytical self-consciousness, then, just as social science methodology is historicized in purpose and practice. Due to this methodological dualism, finally, inferences reached with ESA, though generally interpretive in nature, are strictly replicable. Critics know exactly the causal interpretations, and (often) the logical, empirical and theoretical reasons for them, and can directly challenge any aspect of the analysis, from the selection and written description of actions to be analyzed to their imputed significance and causal connectedness.

ESA's Impact, Or the Lack Thereof

One would think – at least *I* thought at the time – that a methodology with so much going for it would be embraced by a wide range of scholars, from historical sociologists and historians to those political scientists interested in the temporally-grounded, less formal narrative procedure known as "process tracing" [Bennett and Elman 2006]. Well... I was flat wrong: ESA's impact, at least as assessed in the Social Science Citation Index (SSCI), has been small indeed.[7] True, my 1993 *AJS* article, in which I introduced ESA to historical sociologists, has been cited a bit more than 100 times (as of mid-December 2007), a respectable number, I suppose, but way, way short of Ragin's *The Comparative Method*, which by my estimation has been cited *at least* 700 times in journals alone. His various expository articles on QCA have accrued still more citations. Few of the works citing my article, moreover, delved into or used ESA per se; most gave a it a passing nod or referred to my general brief for the importance of time, sequence, and narrative. In fact, other than myself and my co-authors [Griffin and Korstad 1995; Griffin, Clark, and Sandberg 1997], few historical sociologists have placed ESA at the core of their research [e.g., Isaac, Street, and Knapp 1994; Brown and Brueggemann 1997; Brueggemann and Boswell

[7] Among other things, the SSCI tallies the number of times a scholarly piece – book, article, or chapter, published or unpublished – has been cited in articles published in hundreds of journals. For numbers of reasons – citations in books are not counted, for instance, and some journals are not included in the count (but very few, if any, "high impact" journals are excluded) – the SSCI count is only one approximation of a scholarly production's impact. Still, it is an important one, often evoked to denote influence of a piece of work, a scholar, even entire departments, and often used, also, in the U.S. in hiring, promotion, and tenure decisions.

1998; Brown 2000; Brueggemann and Brown 2003; Biggert 2004; Richardson 2004]. The procedure has been used in other disciplines – medical anthropology (U2001), organizational behaviour [Stevenson and Greenberg 1998, 2000], and human ecology [O' Neill *et al.* 2007] – but very infrequently. Heise's pioneering articles on ESA [Heise 1988; 1989; Corsaro and Heise 2000] have also received only a modest numbers of citations (a total for the three articles of 80 or so), and my only other exposition of ESA, co-written with historian Bob Korstad, almost never appears in the SSCI [Griffin and Korstad 1998]. Moreover, I've no knowledge of a historian or political scientist ever using ESA, and very few have cited the basic references to it.[8]

Why So Few Applications

Beyond whatever personal disappointment all of the above suggests – and there is some, no doubt about it – the real question is why has ESA been used so infrequently. Let's take its unimpressive presence in sociology first.

Possibly that condition is due to the fact that ESA is hard work – not learning the software, which can be mastered in a few hours – but hard in the sense that it demands so much from those who use it. To use it productively, one must know an immense amount about one's case: cursory information – "background" information, if you will – is not sufficient to answer intelligently ETHNO's questions. As I said above, very different kinds of knowledge and modes of reasoning are essential – and remember, they must be synthesized – because of the nature of these questions: does action B require action A for it to occur? Sounds simple? It's not, not in the least. I know from personal experience that it is a whole lot easier to use very sophisticated time-series analysis or procedures to model a system of structural equations containing latent variables, correlated measurement errors, and non-recursive causation. To use these procedures and software packages (e.g. EVIEWS, Mplus), one does not need to know the math behind them, or really know how they work or what they do. With ESA, though, one can not answer these "simple" questions adequately without being somewhat of an "expert." There is, I think, a bit of truth to the claim that sociologists do not use ESA because it is too demanding; a bit, but no more. I trust that most of my colleagues would do the hard work if they believed it would pay off: look, for example, at what Skocpol had to learn to write *States and Social Revolutions*.

[8] On historians, see, e.g, Brundage [1994], Cha-Jua [2000]; on political scientists, see Rihoux [2006].

One might also be tempted to conclude that as a formal, but non-numeric, non-QCA-type method it is shunned by a hegemonically quantiphilic sociology, at least in the U.S. Possibly so, but I'm personally dubious. Several facts militate against this explanation as the primary one. First, I am not at all sure that such a description of American sociology is accurate. Clearly, articles in the most widely read journals in the States are more often statistical – and fancy statistics at that – than not, but plenty of non-quantitative, historically-grounded pieces appear in those journals, too. U.S. sociology's most prized books, additionally, are, with some exceptions, qualitative of one sort or another, not quantitative. Second, to the extent that ESA is known by European sociologists, and I do know that a handful, at least, are familiar with it, it has, to my knowledge, never been used there, despite the fact, or so I understand, that European sociology is considerably more methodologically diverse, specifically more historical, than the flavour we Americans tend to practice.[9] Finally, we've a comparison case in Abbott's optimal matching analysis, a technique that is very much statistical. Abbott's and Hrycak [1990] introduced this methodology to the sociology community in 1990, in *AJS*. Seeing print a few years earlier than my 1993 *AJS*, that article seems to have been cited around 125 times, a bit more than mine but not by much, despite the fact that Abbott is much more visible and centrally positioned in American sociology than am I. (Heise, too, is an important figure in American sociology; again, his early ESA articles are cited with only modest frequency.) And, like ESA, optimal matching analysis appears to be seldom deployed in sociology. So, if Abbott's statistically innovative approach to sequence analysis remains about as little used as ESA, the latter procedure's near-invisibility is not due, alone or even predominantly, to the discipline's quantitative bias. But ESA's lack of descriptive and inferential statistics is suggestive of what (I believe) is going on. My untested, quite possibly inchoate explanation for its poor showing in sociology is as follows.

Event-structure analysis, I suspect, appears to many, especially the hard-core positivists and generalizers, as 1) too "historical," too "particularistic" and possibly too "subjective," and 2) to prize complexity at the expense of parsimony. (There is rich irony here: few, if any, sociologists have more accomplished positivist creden-

[9]. To dramatize this point, let me recount a brief story. On behalf of the European Consortium for Political Research (ECPR), Benoît Rihoux graciously asked if I would offer a course for European graduate students on sequence analysis – we agreed on both ESA and optimal matching analysis – at ECPR's 2007 Summer School in Methods and Techniques. I agreed, developed what I believed was a clear, inviting course description/syllabus, and sat back to see how many students the course would attract. The answer: two. And, if memory serves, one of those was not quite sure if s/he would could actually enroll. Needless to say, that class was cancelled, and, understandably, I've not been invited back.

tials than David Heise.) Then, too, there is the way that ESA goes about comparing cases: unlike Mill's methods of agreement and difference [Skocpol 1979] or QCA, it does not isolate a few attributes of cases and then compare and contrast.[10] Rather, ESA compares the entire imputed "concrete" or "general" event structures; each and every path, sequence, historical blind alleys, contingencies, and so on are compared in a way similar to the logic of analytical induction. Even more basic is how ESA construes a case – as a narrative of an event. Such cases need not have set boundaries (as in both quantitative analysis and QCA), need not be independent from other cases, and may be nested, so that one narrative is enveloped by another one, and the second one, in turn, subsumed by a third narrative. Cases, to ESA, may consist of a narrative of a strike lasting days, of a funeral lasting hours, of the Vietnam War lasting years, of the history of a labor union lasting decades, or of the expansion of the capitalist world economy over the last four centuries. Each of these "cases" is, or can be understood to be, an example of what Philip McMichael [1990] calls "incorporated comparisons," whereby a case is anything but a bounded, fixed entity but is instead constructed by conceptualizing different historical instances as interdependent which, when cumulated and connected through time and space, "form" the whole as a general but empirically diverse historical process. Thus the very definition and selection of "cases" in ESA becomes the object of theorizing and research, not their point of departure.

Each of these characteristics of ESA – its intellectual demands, its lack of fealty to statistics or logical algorithms, the unusual way it compares and generalizes, its historical thrust, its construction of cases – is, I believe, challenging to many in the field. Taken together, all of these probably imply, to some (many?), that ESA is not even "sociological" at all. They are wrong, however: it is as "sociological" as any methodological procedure we collectively use; its just sociological in a novel way, in a way that would require jettisoning scientistic definitions of what we sociologists are to do and how we are to do what we do.

Now, what about the historians, why have they failed to pick up on ESA? Most, no doubt, have no knowledge of it (this is true, of course, for sociologists as well). The only ESA application in a history journal of which I am aware is my article with Korstad in *Social Science History* [Griffin and Korstad 1995], hardly a "regular" history journal likely to be read by many narrative historians. Also, ESA had the bad luck to have been developed and publicized just when a great many historians were making the "linguistic turn." In leaving analytical history behind, historians felt less interest in, or a need for, a rigorous methodology devoted to causal analysis. Those

[10] See Griffin and Ragin [1994] and Rihoux [2006] for a brief comparison of QCA and ESA.

historians who do know about ESA, finally, may believe it excessively formal and "model-driven." This belief, too, is unfounded: event structures are not theoretical models imposed on the historical record; they are instead nothing more than explicit depictions of the analyst's interpretations. As I have repeatedly stated, ESA does not mechanically spit out answers to pressing historical questions, and causality, significance and meaning are not "discovered" through its use. It assumes that the historian or the historical sociologist, not the algorithm, possesses the requisite knowledge to anticipate possibilities in a sequence of unfolding action, counterfactualize questions and conditions, explain what happened, and interpret meaning. Thus the really hard work of interpreting causality and extracting meaning from the event falls, as always, to the individual scholar.

Nor should historians fret that ESA destroys the tension and drama of narrative flow, reduces "real" persons to theoretical stick-figures, or weighs the story down with needless technicalities and incomprehensible jargon. Because ESA is, first and foremost, for scholarly self-edification – that is, to sharpen, deepen and broaden the analyst's own thinking – the fruits of an ESA analysis need not be explicitly present as such in the text nor impede stylistic facility [e.g. Griffin and Korstad 1995]. The research product would look and read much like any conventional narrative history: it would simply be better history.

Perhaps other factors are at work as well. I'm not much of a publicist for anything, do not enjoy intellectual arm-wrestling, and did not relentlessly pursue study after study with ESA. In the mid-1990s, I turned, as did many historians, from a concern about causality to one about meaning and from an interest in what happened in the past and why it happened as it did to a concern how the past is used in present-day cultural and political struggles. By and large, then, I had my say and sat down. I'll be the first to admit that the potential of ESA in social-historical inquiry very well could have benefitted from a better spokesperson.

But perhaps I'm wrong about all of this; maybe scholars don't use ESA because it tells them nothing new. Maybe Heise and I overestimated ESA's potential from the very beginning; maybe, indeed, it is a waste of time. Possibly... but I don't think so. I've seen social scientists and historians who thought they knew their cases pretty well utterly stymied after encountering just a few of ETHNO's questions. What they learned was how little they knew. Still, I doubt we will ever plumb the depths of ESA's prowess because it is unlikely to be applied enough to gauge how useful it really is. This is a shame; in my opinion, it will be a communal lost both for historically-oriented social scientists and for historians as well. Few methodologies, formal or informal, either demand or offer as much in terms of sheer intellectual rigor and learning-by-doing as does event-structure analysis.

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Historical Sociology, Narrative and Event-Structure Analysis: Fifteen Years Later

Abstract: In this paper I first describe event-structure analysis (ESA) and its promise as a tool to aid socio-historical inquiry, then discuss why researchers have been reluctant to employ it more frequently. On the first issue, ESA is a member of a family of formal analytic procedures designed to analyze and interpret text, in particular the temporal sequences constituting the narrative of an event. Its basic purpose is to aid the analyst in "unpacking" an event – that is, in breaking it into its constituent parts, which are sequences of actions – and analytically reconstituting it as a causal interpretation of what happened and why it happened as it did. ESA focuses on and exploits an event's "narrativity" – its temporal orderliness, connectedness, and unfolding – thereby helping analyst infer causal links between actions in an event, identify its contingencies and follow their consequences, and explore its myriad sequential patterns. It dos this by asking the analyst a series of questions about the causal connections among actions constituting the event's chronology. ESA thus forces the analyst to replace temporal order as the basis for attributing causality with her or his expert judgement or knowledge about potential causal ties among these actions, judgements limited by an action's temporal position in a sequence but not determined by it. Unlike most other formal analytical techniques, it is completely nonnumeric and non-statistical, and it does not answer its own questions. SEA's utility centers on how it relentlessly probes the analyst's construction, comprehension and interpretation of the event. On the second issue, I argue that SEA's intellectual demands, its lack of fealty to statistics or logical algorithms, the unusual way it constructs and compares cases (as narratives, not variables) and generalizes across them, and its decidedly historical thrust challenges many in sociology, especially those committed to scientistic paradigms.

Keywords: event-structure analysis, historical sociology, narrative, temporality, sequence.

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