MECHANISMS AS MIRACLE MAKERS? THE RISE AND INCONSISTENCIES OF THE “MECHANISTIC APPROACH” IN SOCIAL SCIENCE AND HISTORY

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ABSTRACT

In the increasing body of metatheoretical literature on “causal mechanisms,” definitions of “mechanism” proliferate, and these increasingly divergent definitions reproduce older theoretical and methodological oppositions. The reason for this proliferation is the incompatibility of the various metatheoretical expectations directed to them: (1) to serve as an alternative to the scientific theory of individual behavior (for some social theorists, most notably Jon Elster); (2) to provide solutions for causal inference problems in the quantitative social sciences, in social history, and in the (3) qualitative research context; and (4) to serve as an alternative for narratives (Charles Tilly). Mechanisms can do (1) only as under-specified law-like regularities, deliver (2) as robust generative processes represented by models, and accomplish (3) as fragile generative processes (stories), but these are not all compatible. In particular, the mechanisms promoted by Tilly are bare mechanism-sketches, and their elaboration transforms them into the description of fragile generative processes; as such, they cannot accomplish (4). The extension of the concept of mechanisms to cover stories neglects the unique function of narrative to represent fragile contingent processes, and obscures the peculiarities of human action as the rock-bottom constituent of social and historical reality.

Theorizing about mechanisms in the social sciences and history is a recent trend in how social researchers account for their goals and strategies. Its beginnings can be traced back to the 1980s, when it was introduced by Jon Elster, who has used “search for mechanisms” synonymously with a reductionist strategy in the social sciences, associating it with methodological individualism in general and with the rational-choice approach in particular.1 The last ten years have witnessed an explosion of interest in and writing on mechanisms in the social sciences. The landmark events in this explosion were the publication of the collection edited by Peter Hedström and Richard Swedberg intended to consolidate the “mechanism approach” as a distinctive style of sociological theorizing; the works of Charles Tilly and his co-workers intended to provide substantive his-

1. Jon Elster, Explaining Technical Change (Cambridge, Eng.: Cambridge University Press, 1983). As a matter of historical record, Elster’s feat was anticipated by another Scandinavian sociologist, Georg Karlsson, who used the term “social mechanism” to refer “to a model that covers only a part of a total social process, omitting the relations to the total process except as is shown by the values of the parameters” (Georg Karlsson, Social Mechanisms: Studies in Sociological Theory [Glencoe, Ill.: The Free Press, 1958], 16).
torico-sociological theory-building within the mechanisms framework; and new books by Elster that both explain and exemplify by substantive work his evolving ideas about what mechanisms are.2

As younger researchers jump on the mechanism bandwagon, something like a “mechanism industry” preoccupied with “second-order questions” about the very concept of “mechanism” is emerging.3 This industry has gone international4 and displays the potential for exponential growth, as evidenced by numerous working papers available on the Internet, panels on “causal mechanisms” during the APSA annual meeting in August 2003,5 and the recently published issue of “Philosophy of the Social Sciences” under the editorship of Andreas Pickel, including the contributions by Mario Bunge, Patrick James, Renate Mayntz, R. Keith Sawyer, Colin Wight, and others.6

The “mechanism approach” was also recently introduced into the field of historical studies. During the last few years, many prominent social historians have repeatedly voiced their conviction about the coming renovation of social history, bringing it back into a dominant position in historical studies.7 Different projects


of the “new” or “new new” social history abound. Many social historians set their hopes on the “history of practices.”8 Charles Tilly, who is perhaps the most influential social historian in the U.S., has recently proposed reorienting social historical research by focusing it on the discovery of mechanisms and on explanation by mechanisms and (robust) processes.9

According to Tilly’s account, explanation by mechanisms is one of the five “views” or strategies of social explanation.10 The full list of such strategies includes: (1) skepticism (denial of the possibility of explanation in history and social science); (2) the covering-law view, where researchers seek after “robust empirical generalizations” and try to subject them to higher-order generalizations; (3) the propensity view, in which researchers explain social facts by the inner states and propensities to act of individual actors; (4) the system view, which explains by identifying functions within a social system; and (5) the mechanism view.

Tilly pleads for the “mechanism view,” which consists of decomposing “unique sequences of alterations in relations among connected elements,” called “episodes,” into recurring processes and mechanisms.11 I have already discussed Tilly’s idea of processes and mechanisms elsewhere, tracing back its putative roots in recent developments in the philosophy of science.12 In this paper, I would like to put it into a different context—that of contemporary social science, considering it as part of the broader movement that in recent decades has swept over the various branches of social research.

Tilly’s work on explanation by mechanisms is important as the first intrusion of the mechanism movement into historical studies. As there is no evidence that historical studies will avoid the maelstrom of the mechanism movement, it is critically important for historical scholars to be informed about the assets and liabilities involved in the use of the vocabulary of mechanisms for the articulation of the goals and rules of historical research. My paper is prompted by a fear that the mechanism approach will degenerate from (fruitful) theorizing about mechanisms into (unfruitful) “mechanism talk.” I see this in the proliferation of different definitions of “mechanism” and the increasing divergences among them. James Mahoney, who compiled a list of the available definitions of “mechanism,”13 groups them into four major types: (1) mechanism as synonymous with

10. Tilly, “Historical Analysis of Political Processes”; McAdam et al., Dynamics of Contention.
11. McAdam et al., Dynamics of Contention, 85.
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a cause of an outcome;¹⁴ (2) “mechanism as an intervening process, event or variable”;¹⁵ (3) mechanism as “underspecified causal propositions that can be applied to a fairly wide range of cases”;¹⁶ and (4) “mechanism as an unobserved entity that generates outcome.”¹⁷

This proliferation and divergence invites doubt whether “mechanisms in the social sciences” still remains synonymous with “causal mechanisms” and “social mechanisms,” as was the case in the 1980s and early 1990s. Mahoney differentiates among three types of mechanism theories—“rational-choice,” “functionalist,” and “power” theories. These theories prefer correspondingly “instrumental rationality,” “system needs,” and “exercised capacity” as explanatory mechanisms. McAdam, Tarrow, and Tilly differentiate between “relational” mechanisms (or “social mechanisms” in the strict sense) on the one side, and “environmental” and “cognitive” mechanisms on the other.¹⁸ It is difficult to avoid the impression that the growing body of writings on mechanisms is about to reproduce the entrenched theoretical oppositions in the way described by “fractal division” theory,¹⁹ or simply dress the old theoretical divisions and oppositions in social theory and methodology in the new terminology of “mechanism talk.”

In the first three parts of my paper I will show that this first impression is the correct one, and I will identify the centrifugal forces that are driving the views of the “social mechanicists” apart. The most important of these forces are the differences in the problems for which mechanisms are expected to provide the solution. These differences generate contradictory expectations about the goods that mechanisms should deliver. Some researchers (most notably, Elster) consider “mechanisms” as the solution to the problem of the lack of a universally accepted explanatory theory of individual behavior. For him and the like-minded,²⁰ mechanisms represent the alternative to rational-choice theory and other psychological theories that have promised but failed to provide satisfactory “covering-law” type explanations and predictions of human behavior. This idea of mechanism and its difficulties is discussed in the first section of the paper. Some quantitative researchers expect “mechanisms” to solve the “black box” problem that allegedly is necessary for valid causal inferences in the non-experimental “large N” context.²¹ Besides this, there are qualitative researchers who are eager to use

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mechanisms for the solution of the “many variables, few cases” problem that threatens to establish a logical blockade for causal inferences in the “small N” context. These quantitative and qualitative hopes directed toward the mechanism approach are discussed in the second and third sections, respectively.

My argument in these three sections goes as follows: if mechanisms are conceived in a way that provides them with the capability to deliver one of these services, they cannot be useful for other jobs, and vice versa. There is no idea of “causal mechanisms” that would make them scientific “miracle makers” for all three problems at once. The more varied are the services expected from mechanisms, the more divergent and incompatible are their definitions. After establishing this basic antinomy of “mechanist thinking” in the social sciences, I will introduce (in the fourth section) Tilly’s version of the “mechanism approach” in more detail and will ask whether Tilly can avoid this antinomy. I will close with some general considerations about the differences between mechanisms and stories, and the costs involved in the obliteration of the differences between them.

I. PROVIDING THE ALTERNATIVE TO NOMOTHETICAL THEORY FOR SOCIAL THEORISTS

Jon Elster has contributed perhaps the most to the popularization of mechanism thinking and talk in the social sciences. He made his name in the 1970s and 1980s as a distinguished exponent of the now already extinct Rational Choice Marxism (or “Analytical Marxism”) movement. During this time, he used “explanation by mechanisms” as shorthand for reductionist, individualist microexplanation grounded in Rational Choice Theory (RCT); this was conceived as antonymic to purely “macrocausal,” “structural,” and “functional” explanatory hypotheses. He described the search for microfoundations as “a pervasive and omnipresent feature of science,” identical with the reduction to a more fundamental level of theories describing and explaining the behavior of objects at a higher level:

Generally speaking, the scientific practice is to seek an explanation at a lower level than the explanandum. . . . To explain is to provide a causal mechanism, to open up the black box and show the nuts and bolts. . . . The role of mechanisms is two-fold. First, they enable us to go from the larger to the smaller: from molecules to atoms, from societies to individuals. Secondly, and more fundamentally, they reduce the time lag between the explanans and explanandum. A mechanism provides a continuous and contiguous chain of causal or intentional links; a black box is a gap in the chain.

Beginning with his widely translated book *Nuts and Bolts for the Social Sciences*, Elster associates “mechanisms” with a new and distinctive philoso-

22. For example, Bennett and George, “Process Tracing and Case Study Research.”
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phy of social explanation. According to Elster’s new definition, mechanisms are “frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with indeterminate consequences.”28

Different from law-like generalizations, which are described by statements of the form “if conditions $C_1$, $C_2$, …, $C_n$ obtain, then always $E$,” mechanisms can be described by the statement “if conditions $C_1$, $C_2$, …, $C_n$ obtain, then sometimes $E$.” This new conception of mechanism was prompted by Elster’s disappointment with the prospects of general explanatory theories in social science: “the aim of such theories—to establish general and invariable propositions—is and will always remain an illusory dream. Despite a widespread belief to the contrary, the alternative to nomological thinking is not a mere description or narrative ideographic method. Between the two extremes there is a place and need for the study of mechanisms.”29

Among many theories that have disappointed Elster, RCT was perhaps the most important. Because of his long-standing interest in the intricacies of human psychological make-up,30 he was deeply impressed by the mounting “anomalies of rational choice,” the experimental research of which has established itself as a thriving business in psychology since the 1970s.31 However, the successes of behavioral researchers in undermining the pretense of RCT as an empirically adequate theory of human behavior were not matched by an equal success in developing an alternative positive theory with the scope and promise comparable to that of RCT. So Elster hoped to find in “mechanisms” a comfortable retreat area from the overly-ambitious and unfulfilled promise of general explanatory and predictive theory of human behavior, and he encouraged social researchers to follow the example set by Alexis de Tocqueville and other authors of ancient times by grounding their analyses of social life in the generalizations of folk psychology that are used by human agents in their everyday interactions to provide accounts of their own behavior and that of their peers.

This body of pre-scientific knowledge finds its most easily accessible expression in the proverbs that recur in the lore of widely varied peoples and cultures. This can be considered as evidence of both the universality of the “mechanisms” referred to and of the empirical adequacy of their descriptions in the proverbs. The important feature of this proverbial folk-psychological knowledge is that proverbs occur in pairs—for any proverb there exists its opposite: like attracts like and opposites attract; out of sight, out of mind and absence makes the heart grow fonder; forbidden fruit is sweet and sour grapes.32 For Elster, this feature of proverbs is evidence of an empirically interesting and important peculiarity of human psychological make-up: mechanisms themselves (as nuts and bolts, cogs

and wheels of the human mind) occur in pairs. That’s why the prediction of human behavior is much more problematic than its explanation.

For every past episode of human behavior, it is a relatively simple task to provide an *ex post* account of the observed behavior, by subsuming it to one of the opposite proverbial generalizations. It is much more difficult to predict it, because for a given or realistically accessible set of an agent’s beliefs and desires we can expect opposite courses of action. In situations of a given type, a pair of exclusive mechanisms is operative, but not sufficiently determinative so that one can predict which of the different courses of behavior will be triggered: will an animal in fear fight, flee, or freeze? Elster classifies mechanisms with this kind of indeterminacy as “type A mechanisms . . . which arise when the indeterminacy concerns which (if any) of several causal chains will be triggered.” He differentiates them from “type B mechanisms” that can operate together but can have opposite net effects in terms of observable behavior. This unpredictability can be due to the simultaneous triggering of the two opposing mechanisms by the same cause (for example, the increase in the marginal tax rate both lowers the opportunity cost of leisure [making it more likely that people will choose it], and lowers their income [thereby inducing them to work harder]), or to the triggering of one mechanism by another (for example, the “contrast effect” mechanism: termination of pleasure triggers feelings of discomfort or pain, and termination of pain triggers feelings of relief, with net effects of both sequences on the welfare of an agent remaining indeterminate). Elster classifies B-type mechanisms of the first kind as B₁-type, and those of the second kind as B₂-type.

Elster’s new conception of “mechanism” remains within the framework of the “correlational” (or Humean) analysis of causation and the “covering-law” model of scientific explanation. Elster’s “atomic” mechanisms are “brute” (albeit vague and almost tautological) generalities, correlating types of mental states and actions. Are there “black boxes” in between them? On the one hand, by designating them as “atomic” mechanisms, he suggests that they have “rock-bottom” status in the explanation of human behavior. On the other hand, by interspersing his descriptions of “mechanisms” with information about their neurophysiology, he suggests the possibility of their reductive explanations in terms of neuroscience. However, such explanations would evidently represent the kind of reduction that is called “eliminative reduction” by philosophers of science³⁵ with uncertain implications for all social sciences dependent on the description of human behavior in the mentalist idiom.³⁶ Elster himself simply does not discuss this prospect of neurophysiologically-grounded social science.

Because Elster’s mechanisms are attenuated and under-specified law-like generalizations, his theory of “explanation by mechanisms” can be considered to be a watered-down version of the “covering-law” model of scientific explanation. In its “unwatered” version, the explanation of E means its derivation from the

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nomological premise “if conditions $C_1$, $C_2$, $C_n$ obtain, then always $E$,” and singular statements that assert that conditions $C_1$, $C_2$, $C_n$ obtain as matter of fact in the given case:

$$\text{If conditions } C_1, C_2, \ldots, C_n \text{ obtain, then always } E$$

$$C_1, C_2, \ldots, C_n \text{ obtain}$$

$$E$$

The “covering-law” model of explanation is transformed into the “explanation by mechanisms” model when the nomological statement in the covering-law explanation scheme is replaced by a general statement containing the consequent that consists of the disjunctive list of all the possible consequences of the conditions $C_1$, $C_2$, $\ldots$, $C_n$: “if conditions $C_1$, $C_2$, $C_n$ obtain, then always $E_1$, or $E_2$, or… or $E_3$.” Some examples of such mechanism statements are produced from Elster’s own examples of opposite mechanisms, supplementing them with descriptions of the possible consequences of behavior that lie “in between” the outcomes of the opposite paired mechanisms: if people cannot satisfy their desires, then they adjust by changing their beliefs (engaging in wishful thinking) or changing their desires (“sour grapes”), or they persist in their beliefs and desires; if a society has a democratic political organization, then people either become more strongly religious to satisfy their need for authority (compensation effect), or they extend their critical attitude to religious authorities (spillover effect), or their attitudes to religion do not change. These statements describe what Elster calls “molecular” mechanisms that in the simplest cases consist of paired opposite mechanisms, and in the more complex cases include more extended “concatenations” of “atomic” mechanisms.

It can be seen immediately how mechanisms in Elster’s sense “allow us to explain, but not to predict.” When mechanisms are used for predictive purposes, the knowledge that $C_1$, $C_2$, $\ldots$, $C_n$ obtains provides the ground only to expect that one from the set of their possible outcomes will occur. However, in the explanatory (ex post) situation it is already known which of them obtains as a matter of fact. The statements that report which of the possible consequences do not obtain are part of the explanans. So Elster’s explanation by mechanisms can be represented by the following (valid) form of argument:

$$\text{If conditions } C_1, C_2, \ldots, C_n \text{ obtain, then always } E_1, \text{ or } E_2 \text{ or… or } E_n$$

$$C_1, C_2, \ldots, C_n \text{ obtain}$$

$$E_2, E_3, \ldots, E_n \text{ do not obtain}$$

$$E_1 \text{ obtains}$$

II. OPENING BLACK BOXES FOR QUANTITATIVE RESEARCHERS

The service expected from the mechanism approach by its partisans in quantitative social research is to provide the solution for causal inference or the “common cause” problem. Two aspects of this problem can be distinguished. The first

38. Cf. Ibid., 19-20.
39. Ibid., 1.
is, how to define the very concept of “causal relation” (that is, the exact meaning of causal claims). The second is, how can the existence of the hypothesized causal relation be proven, that is, how is it possible to know the truth of the causal claim from the observational (non-experimental) data? To begin with the second, if the variables $X$ and $Y$ are causally related, then the data should reveal a statistical dependence between them. However, a statistical dependence between $X$ and $Y$ does not imply that $X$ and $Y$ are causally related—the apparent causal connection can be spurious, being produced by $Z$ as the common cause of both $X$ and $Y$. Given this, a researcher is thus well advised to measure all potential common causes of $X$ and $Y$. However, even if all such causes are measured, the statistical analysis of the evidence still may fail to identify the causal structure. So, for example, the same statistical evidence regarding conditional dependencies and independencies is compatible with two different hypotheses about causal structure represented by the causal graphs (a) and (b) in Figure 1:

![Causal Graphs (a) and (b)](image)

**Figure 1**

According to the mechanist “solution”\(^{40}\) of the statistically indistinguishable causal graphs problem, one knows that $X$ is the cause of $Y$ if one can identify the causal mechanism $M$ from $X$ to $Y$.\(^{41}\) For the case involving more than two variables (for example, that in Figure 1), researchers should accept hypothesis (a) if they know the set mechanisms corresponding to graph (a) but know no mechanisms that correspond to graph (b). In negative terms, researchers are not allowed to accept the causal hypothesis “$X$ causes $Y$” if they cannot specify the mechanism $M$ that relates $X$ to $Y$—even if the correlation between $X$ and $Y$ stands for all the controls for non-spuriousness, and the time order in the changes of the variables is well established (changes in $X$ precede those in $Y$). There can be no knowledge of a causal relation between $X$ and $Y$ (only that of their non-spurious correlation) until the “black box” containing the mechanism that produces $Y$ is opened.

This mechanist “solution” of the causal inference problem presupposes a particular concept of causation, one that can best be expressed by the following definition: “$X$ is a cause of $Y$ if and only if there exists mechanism $M$ that has as

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40. I am putting the word “solution” in quotation marks because this solution boils down to the proposal to drop the restriction that only observational data are used. From the standpoint of the original problem this is equivalent to the statement that the problem is not soluble. I discuss this matter below in more detail.

its input X and has as its output Y.”42 The causal mechanism M itself is conceived as a “generative process”43 that produces Y as output from X as input.44 The mechanicist analysis of causation opposes the conception of causation as a robust statistical correlation that identifies “causal relation” with non-spurious statistical dependence. This idea of causation is an adaptation of the Humean analysis of causal relation as regular association, and was prevalent in quantitative social research until the 1980s. This correlational account can also be described as an “informational” one, because it treats causation in terms of predictive power.45 This feature of the correlational account is quite explicit in the elaboration of the correlational account known as “Granger causation” that is influential in econometric analysis: a variable X causes Y if the knowledge of the values of X improves the researcher’s ability to predict future values of Y, after all information apart from the values of X is taken into consideration.46

Another object of mechanicist criticisms is the idea of causation as consequential manipulation, an idea that has a home in the experimental context: X is a cause of Y if, by changing the values of X, it is possible to change the values of Y. This idea was systematically developed by Donald B. Rubin and Paul Holland; in social research, it became widely known and influential since its adoption in the famous book by Gary King, Robert O. Keohane, and Sidney Verba.47 According to their elaboration, the causal effect Y of the explanatory variable X “is the difference between the systematic component of observations made when the explanatory variable takes one value and the systematic component of comparable observations when the explanatory variable takes on another value.”48 Another version of the manipulationist or control notion of causation is represented by the definition of causation as a relation of statistical dependence that is invariant under intervention.49 Among other applications of this idea, James Woodward, who is a leading exponent of this view in the philosophical analysis of causation and scientific explanation, provides a causal interpretation of structural equation models that is different from both a correlational and a mechanicist account.50

The incompatibility of the “mechanicist” and “control” view of causation is demonstrated by the well-known criticisms of the mechanicist account that expose the fallacy of the infinite regress involved in the former view. King, Keohane, and Verba state that:

43. Goldthorpe, On Sociology, 149.
44. Little, Varieties of Social Explanation, 14.
46. Goldthorpe, On Sociology, 139.
50. Ibid., 315-349.
the causal mechanism generating the output Y from the input X is constituted by the number of intermediate causal links building the chain between them. However, once the mechanist definition of causation is accepted, there is no sufficient reason to accept any single intermediate link as “causal” until the generative process between the input and output in each particular link is identified and so on ad infinitum. This regress can be avoided only if we end with the level where the causal relation between the relata in causal links can be identified without recourse to intermediating mechanism—by experiment or statistical controls. In doing this, the researcher is bound to use some non-mechanicist concept of causation, thus demonstrating the logical primacy of such a concept.51

Because their book is written for qualitative researchers, this statement can be understood as referring to the concerns of qualitative researchers and the problems that “mechanisms” are expected to help solve in qualitative research. I will discuss in the next section whether or not this is the case. Meanwhile, I want to focus on the question of whether this procedure is of any use for the solution of the problem of identifying a causal model for the quantitative researcher. Can the “procedure of identifying causal mechanisms” help the researcher working with quantitative data to infer causal structure?

It is common knowledge that basic criteria for establishing a causal relation include association, time-order, non-spuriousness, and rationale (background assumptions).52 The problem that the “procedure of identifying causal mechanisms” is supposed to solve is that the data on relations about statistical association, time-order, and controls of non-spuriousness under-determine which of many possible alternative causal hypotheses provide a true explanation of the data. In terms of this standard formulation of the problem of causal inference, the plea for mechanisms as scientific “miracle makers” boils down to the assertion that a true hypothesis about causal structure is not possible without sufficiently strong background assumptions. For data involving even a modest number of variables, there exist huge numbers of logically possible causal explanations. The background assumptions help to reduce them to a tractable set. The data on relations of statistical association, time-order, and non-spuriousness do the remaining elimination work. If the “plea for mechanisms” is understood as advice to make the additional background assumptions when these data are not sufficient to eliminate all remaining causal hypotheses except one, it is reasonable if it is understood as the recommendation to re-design the research project to make it determinate. However, if the plea for mechanisms means that a causal hypothesis cannot be accepted if it is not a part of background knowledge, the mechanicist “solution” for the causal inference problem either commits the fallacy of dogmatism, or conflates the discovery of a causal relation with its explanation. Let me explain why.

The fallacy of dogmatism consists in trivializing the task of statistical inference by maintaining that it can identify the true causal hypothesis if its truth is not already known. Clark Glymour describes nicely where the crux of the matter is:

Skepticism is trivial: without some assumptions limiting the possibilities, any interesting inferences—whether about causal relations or about the existence of other minds—are

52. See, for example, S. Goldenberg, “Rediscovering and Confronting Critical Ambiguities in the Determination of Causality,” Quality and Quantity 32 (1998), 181.
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underdetermined by data, and no reliable, informative inference procedures are possible. Dogmatism is easy too; make the background assumptions strong enough and reliable (under background assumptions) inference is trivial. Real work in methodology investigates the trade-off between the strength of background assumptions and the existence of reliable, informative, and feasible inference procedures.53

Of course, I am not insinuating that John Goldthorpe, or somebody else with much less experience and skill in quantitative social research, has committed such an elementary blunder (this blunder can be attributed, however, to some over-enthusiastic propagandists of causal mechanisms among philosophers). The close reading of the procedure that Goldthorpe proposes for the identification of mechanisms suggests rather that he conflates the discovery of the causal relation and its explanation by the deduction from more fundamental theory (in his case, this is RCT). This reading is suggested by his statement that

the whole statistical technology that has underpinned the sociological reception of the idea of causation as robust dependence, from Lazarsfeldian elaboration through to causal-path analysis, should be radically re-evaluated. That is to say, instead of being regarded as a means of inferring causation directly from data, its primary use should rather be seen as descriptive, involving the analysis of joint and conditional distributions in order to determine no more than patterns of association (or correlation).54

In his view, real causal explanation that discloses “generative processes” is achieved by the RCT-powered models describing the actions of individual actors. For the testing of these models, he advises using the hypothetical-deductive method applied according to Popperian rules: the hypothesis about the “generative process” is at least provisionally acceptable, if it not only is compatible with statistical dependences in the initial data set (and thus has not been falsified), but also has new observational implications that withstand the controls of further research.55 In Goldthorpe’s opinion, the “whole statistical technology” is useful only for “establishing the phenomena” that should be causally explained, and suggesting (as heuristics) causal accounts, but not for causal inference.

However, there are some strong reasons not to accept the rules for causal language proposed by Goldthorpe. The acceptance of these rules commits one to the view that there are no macro-social causal relations, because all “generative processes” work at the level of individual action. This is perfectly acceptable for the methodological individualist, but certainly will be rejected by many social researchers who are committed to explanations in terms of macro-social causal propositions. Even among individualists themselves, the interpretation of individualist explanation in the sense of “eliminative reduction” is not generally accepted. On an alternative view, the explanation of robust macro-regularity in terms of the entities and processes at the lower level does not deprive it of causal status.56 Otherwise, there is no brake to prevent the slide down the slippery slope to elementary particles as the only entities in terms of which “real” causal expla-

nations can proceed. To avoid these and other difficulties, it is sufficient to assume that a causal relation can be discovered, and knowledge about it can be used to guide practical action, without the actors being able to explain how it operates. For example, it is not necessary to have a firmly established theory of money that reveals in detail the “mechanisms” involved in order to discover that financing the budget deficit by issuing paper money causes inflation.

This is the hardly disputable matter of fact from which the instrumentalist notion of causality draws its strength. The deduction from corroborated theory of the statements that describe the regularities that are invariant under intervention does not provide them with the status of causal statements, but changes their epistemic status in terms of their place in the web of our beliefs. They move from peripheral locations occupied by causal truths with less certain status (accepted only until contradicting evidence is found) to the more central areas occupied by causal truths that are not rejected even in the face of cumulating contradicting evidence—in the hope that further research will discredit this evidence. Goldthorpe’s proposal to classify as “causal” only statements that are supported not only by statistical evidence, but also by established theories from the body of background knowledge, is not fair to the capacity of statistical analysis to augment the web of our beliefs at its periphery, by supplementing it with new elements that are causal, even if of uncertain status because they lack the backing of established theories. Because some social-scientific disciplines lack such theories (for example, sociology), Goldthorpe’s view implies that to this point they have failed to discover even a single causal truth.

The overemphasis on the role of background assumptions in causal inference in the mechanistic “solution” to the problem of causal inference from observational data is not only misguided but also potentially counterproductive. As a matter of fact, there are no methodologists who would assert that it is possible to discover causal relations in a purely inductive way, making no background assumptions. In this respect, there is no difference between causal inferences from experimental or from observational data. The real problem in methodological work on the problem of causal inference is to find the procedures that (1) work with minimal background assumptions in general, and (2) are maximally independent of domain-specific assumptions. (Domain-specific assumptions are represented by the substantive knowledge of the field in question, including what social mechanists call “mechanisms.” These domain-specific assumptions should be distinguished from general or formal assumptions that describe the formal properties of the causal structures and statistical associations.)

Typical formal assumptions include: the Markov condition, which assumes that each variable in the causal graph is probabilistically independent of its non-effects, and conditional on its direct causes; minimality (no subgraph of the

causal graph also satisfies the causal Markov condition; faithfulness (all independencies in the data are implied via the causal Markov condition); linearity (all variables are linear functions of their direct causes and uncorrelated error variables); causal sufficiency (all common causes of measured variables are measured); context generality (every individual possesses the causal relations of the population); no side effects (one can intervene to fix the value of a variable without changing the value of any non-effects of the variable); and determinism. The fewer formal assumptions a statistical technique makes, the broader its potential field of application—although, on the other side, additional assumptions facilitate the inductive procedures based on the causal Markov condition, either by helping to provide some justification for the inductive procedure or by increasing the efficiency or efficacy of data-processing.60

The minimization of the dependence of causal inference on domain-specific background assumptions is a necessary condition for the construction of algorithms for discovering the causal structure or automated causal inference programs that could usefully supplement SPSS or other packages of programs for analysis of statistical data. Researchers currently working on algorithms for the discovery of causal structure (or causal learning) include groups in Pittsburgh,61 Los Angeles,62 and other places.63 Their work has proved that under certain favorable circumstances it is possible to infer causal structure without significant assumed knowledge of underlying causal relationships, and even without the assurance that all common causes have been measured. The plea for mechanisms as a solution to the problem of causal inference in the statistical context may occasionally serve as a useful reminder of the limits of this work,64 but it can also discourage efforts to exhaust the possibilities of the development of statistical techniques with minimal domain-specific background assumptions, because the output of the algorithm that is supposed to discover causal structure does not correspond to the mechanistic idea of causal relation as something that is irreducibly domain-specific.

III. SUBSTITUTING DEGREES OF FREEDOM FOR QUALITATIVE RESEARCHERS

Arguments on behalf of causal mechanisms have found limited acceptance on the quantitative side of social research. Among the quantitativists, the mechanists include only those few who maintain that there cannot be causation with “black

boxes” (with the exception of the most fundamental physical processes), and that there is no knowledge of causal relation (only that of robust correlation) until the black box between the relata of causal relation is opened. Describing their standpoint in the old-fashioned terms of John Stuart Mill, no causal truth can be proved by means of induction only. An inductively established regularity can be considered as causal only after it is also deduced from the “axioms” of an established theory. Because of the inductivist bias of quantitative researchers and methodologists, the denial of the possibility of knowing the truth of a causal claim without opening the “black box” does not find widespread acclaim. The idea of causation as robust association, or some version of its “control” or “con-sequential manipulation” (adapted for the observational context), provides the conceptual basis for the predominantly inductive approach to the solution of the causal inference problem that still prevails in quantitative research and its methodology.

On the qualitative side, interest in mechanisms derives from the hope that they can do the job of scientific “miracle makers” by helping to solve the “small N” or “degrees of freedom” problem that is specific to, maybe even definitive of, qualitative research. The “small N” problem is the research design problem that surfaces when researchers have too little variation or diversity in their evidence to eliminate all but one competing hypotheses about the candidate causes of their target variables. In properly designed quantitative research (with determinate research design), the general and domain-specific background assumptions limit the range of these hypotheses to a manageable size, and the check of statistical associations does the elimination job. But this won’t work in the case of qualitative research. Because of their limited number of cases, qualitative researchers are bound to assume deterministic causality and zero measurement error in their data to apply Mill’s classical canons of eliminative induction or their generalized version—Boolean methods of qualitative comparative analysis.

Another condition for the application of these methods of causal inference is the identification of all causally relevant variables. In the experimental context, this condition can be approximately satisfied by randomization procedures that enable researchers to neutralize the impact of unidentified causally relevant variables on their causal conclusions. As a matter of definition, this is not the case in qualitative inquiry. Even if researchers make the (hardly justifiable) assumption that their list of candidate independent variables is exhaustive, they have to struggle with the lack of sufficient diversity in their population of cases to let the data reduce the set of the candidate hypotheses to only one. This problem recurs in the attempts to apply Boolean algorithms of qualitative causal inference designed by Charles Ragin. It is here that qualitative researchers look to mechanisms; indeed “mechanisms” is the keyword in the solution that some qualitative

methodologists propose for these difficulties involved in “small N” and case-study research.67

In extracting this service from “mechanisms,” qualitative methodologists exploit the ambiguity in the very concept of “case” that is at the root of the qualitative–quantitative divide. In the context of quantitative research, the “case” is understood as simply a row entry in the data set, column entries being reserved for the variables. In qualitative research, cases are understood as spatially and temporally extended wholes “over there in the world” that can be considered by researchers from different perspectives and on changing scales, with researchers free to “zoom in” and “zoom out” on the aspects or temporal phases that they consider particularly interesting or challenging. In “zooming in,” researchers change the spatio-temporal scale of their investigation, going “into the depths of the case.” They search after the evidence of the hypothesized causal relation between the variables that describes the aspects or temporal phases of their “big cases.” This is the evidence about the causal process connecting these variables, and the whole procedure is described as “process tracing” or “narrative analysis.”68 For example, a researcher can substantiate the causal hypothesis that military defeat and state breakdown are causes of social revolution by tracing the temporal sequences of the causally chained events that, in the cases under consideration (for example, France in the late eighteenth century, Russia and China in the twentieth century), led from military defeat and state breakdown to social revolutions as “rapid, basic transformations of a society’s state and class structure, accompanied and in part carried through by class-based revolts from below.”69

Andrew Bennett and Alexander George, who have provided the most elaborate discussion of this procedure, take their point of departure from the description of how historians prove their causal claims provided by Clayton Roberts.70 According to Roberts’s description of this procedure (he calls it “microcolligation”), if a historian’s causal statement “Y was caused by X” is challenged, the historian meets the challenge by telling in more detail about the events that were involved in the problematic episode, that is, by expanding the problematic part of the overall story into an elaborate sub-story that purports to represent the prob-


69. Theda Skocpol, States and Social Revolutions: A Comparative Analysis of France, Russia, and China (Cambridge, Eng.: Cambridge University Press, 1979), 33. This is how some methodologists (Mahoney “Nominal, Ordinal, and Narrative Appraisal,” 1164-1169; Mahoney, “Strategies of Causal Inference,” 413, 416) reconstruct the “logic-in-use” of Skocpol’s famous causal argument, criticizing her account of this logic as inadequate. According to the “reconstructed logic” of the theory provided by Skocpol herself, it is a straightforward application of Mill’s canons (Skocpol, States and Social Revolutions, 36-40).

lematic causal link in more detail. What is the difference between such narratives and process-tracing explanations? Bennett and George provide the following answer: “A process-tracing explanation differs from a historical narrative, as it requires converting a purely historical account that implies or asserts a causal sequence into an analytical explanation couched in theoretical variables that have been identified in the research design.”

They provide no further details about the technical side of this “conversion” except the approving citation of Roberts’s statement that the conjoined events at which process-tracers stop their descent into the depths of the case should be redescribed in terms that open the possibility of subsuming them under some accepted generalization. One thus cannot avoid the conclusion that “process-tracing” is simply storytelling, perhaps expressed in social researcher’s jargon as distinct from the historian’s more commonsensical idiom. On this account, “mechanism” turns out to be what the story represents. If we accept the distinction between “narrative discourse” (the order in which the story is told) and the “story” (the narrative content or spatio-temporal process that is represented by the narrative discourse) that is usual in the branch of literary studies known as “narratology,” “mechanism” is simply a “story.”

IV. . . . AND DISPENSING WITH “STANDARD STORIES”?

This finding can shock because another prominent exponent of the “mechanism approach”—Charles Tilly—promotes explanation by mechanisms as the alternative to storytelling. “Mechanism-based accounts select salient features of episodes, or significant differences among episodes, and explain them by identifying robust mechanisms of relatively general scope within those episodes.” Episodes are chunks of social reality that are sorted out and bounded by special processes that are called in contemporary social theory “social constructions.” Participants or observers in social life sort out myriads of events. However, only part of events acquire social significance because the participants or observers give them names, draw boundaries, and most importantly, tell stories about them. “As conventional or arbitrary entities, events we call revolutions, social movements, wars, and even strikes take shape as retrospective constructions by observers, participants, and analysts.” In Tilly’s view, storytelling and negotiating which story is “true” is the most important mechanism of the social construction of events or episodes. Names like “revolution,” “strike,” “war,” and so on are designations of different kinds of “episodes,” which are “unique sequences of alterations in relations among connected elements.”

71. Roberts, The Logic of Historical Explanation, 105-118.
73. Ibid., 13.
75. Tilly, “Historical Analysis of Political Processes,” 569.
76. McAdam et al., Dynamics of Contention, 308.
77. Ibid., 85.
The goal of explanation by mechanisms is to decompose such episodes into processes and mechanisms, with mechanism itself defined as “a delimited class of events that alter relations among specified sets of elements in identical or closely similar ways over a variety of situations.” 78 Tilly differentiates between “cognitive,” “environmental,” and “relational” mechanisms. “Cognitive mechanisms operate through alterations of individual and collective perception; words like ‘recognize,’ ‘understand,’ ‘reinterpret,’ and ‘classify’ characterize such mechanisms.” 79 Environmental mechanisms “mean externally generated influences on conditions affecting social life,” 80 and are exemplified by resource depletion or enhancement. Relational mechanisms “alter connections among people, groups, and interpersonal networks,” 81 and include brokerage, exploitation, opportunity hoarding, and emulation, among other examples.

The concept of “robust process” is the third and last in the list of core concepts in Tilly’s metatheory of social-historical explanation. Robust processes are regular concatenations of mechanisms—“regular sequences of such mechanisms that produce similar (generally more complex and contingent) transformations of those elements.” 82 The difference between robust processes and mechanisms is a relative one, depending on the scale of analysis: an item that is a mechanism in some large-scale process can be resolved into a robust process in smaller-scale analysis. “We may call an invariant and widely applicable cause a mechanism when at the current level of observation its components are invisible and its immediate effects indistinguishable.” 83

The recent substantive work of Tilly and his collaborators applies this view of social explanation. “Explanation entails identifying problematic features of episodes or classes of episodes, then discovering what mechanisms and processes produce these problematic features.” 84 In the collective work Dynamics of Contention, Tilly and his collaborators pursue the goal of showing that “episodes” that in standard stories and received social-scientific explanations are conceived as exemplifying social phenomena of different kinds—nationalist mobilization, democratization, and revolution—can be partially explained by the same robust processes. They discuss three such processes. First is “actor constitution”; it involves mechanisms of social appropriation, innovative action, attribution of threat/opportunity, identity shift, decertification, and category formation. They find this process at work in two such apparently dissimilar episodes of what they call “contention” as the Civil Rights Movement in the U.S. in the 1950s and the Cultural Revolution in China in 1966. The second robust process is “polarization”; it combines the mechanisms of opportunity/threat spirals, competition, brokerage, and category formation. Tilly and his team show in detail how this process can help to explain the split between Jacobins and Girondists during the French Revolution, and the Christian–Moslem conflict in Indonesia in

78. Ibid., 24.
80. McAdam et al., Dynamics of Contention, 25.
81. Ibid., 26.
82. Ibid., 24.
84. Ibid.
the late 1990s. The third process is that of “scale shift”; it involves localized action, brokerage, diffusion, attribution of similarity, and emulation as its constituent mechanisms. Tilly and his co-authors believe they can detect this process in the spread of the Mau Mau rebellion in colonial Kenya in the 1950s, in the propagation of the student movement in 1968 in Italy, and in the Rwandan genocide of 1994.

Tilly contrasts and opposes his account of explanation to the “covering-law” model, which still serves as the standard point of reference in discussions concerning the logic of historical explanation. In Tilly’s view, explanation by mechanisms involves not the derivation of the description of the explanandum from statements describing initial conditions together with general laws, but its decomposition into constituent mechanisms and processes. A successful explanation by mechanisms dissolves the petrified classifications of social episodes into different types like “war,” “terrorism,” “nationalism,” “democratization,” and other categorial distinctions. “These types will turn out to represent different combinations of settings and causal processes but not distinct species of social interaction. The types will continue to guide comparisons and searches for causes, but they will not require separate kinds of explanation.”

Where does Tilly’s version of the “mechanistic approach” stand in relation to the three approaches discussed above? Tilly seems to share his goals with Elster—to provide an alternative to the “covering-law” model, but one that remains rigorously scientific. Like Elster, Tilly considers mechanisms as the middle ground between the positivist search for law-like generalizations to be used as premises in covering-law explanations, and “interpretivist” satisfaction with narratives and descriptions. However, for Elster the commitment to explanation by mechanisms implies methodological individualism; this is rejected by Tilly in favor of “relational realism,” with transactions, interactions, or social ties serving as starting points of social analysis.” Elster’s “mechanisms” are identical with the items that Tilly describes as “cognitive mechanisms;” in other words, they are only a subclass of mechanisms. Tilly’s plea for “mechanistic” social explanation includes the thesis of the primacy of relational mechanisms in social-historical explanation.

Tilly’s ideas about mechanistic explanation resemble most closely those available in the rapidly growing body of literature known as mechanistic or mechanismic theory of scientific explanation. These authors pursue the goal of

85. Ibid., 23.
86. Tilly, “Means and Ends of Comparison in Macrosociology,” 47.
MECHANISMS AS MIRACLE MAKERS?

explicating or defining in a precise way the concept of “mechanism” involved in the explanation of biological and other complex systems, and of describing the research procedures involved in the discovery of such mechanisms. According to one influential definition, “mechanisms are entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions.” This is what Tilly describes as “robust processes.”

A mechanism is identified by some function, performance, or behavior as an output that it accomplishes or displays (for example, DNA replication, protein synthesis or regulation). This output is explained by the description or depiction of a set of “fine-grain” activities at the lower level, which are organized in a specific way, described in terms of temporal order, rate, and duration, and localized in some component entities arranged in a specific way. The standard and perhaps most adequate device to represent a mechanism is by a flow chart. “Mechanisms are often represented this way. Such diagrams exhibit spatial relations and structural features of the entities in mechanism. Labeled arrows often represent the activities that produce changes. In these ways, diagrams represent features of mechanisms that could be described verbally but are more easily apprehended in visual form.” Characteristically, Tilly also typically represents his mechanisms (robust processes) by diagrams. If “covering-law” explanations are explanations by arguments, mechanistic explanations are explanations by diagrams.

These diagrams are technically called “mechanism schemata” in the recent literature in the philosophy of scientific explanation: “a mechanism schema is a truncated abstract description of a mechanism that can be filled with descriptions of known component parts and activities.” The degrees of abstraction in a mechanism schema should be distinguished from those of the scope of the mechanism. The more or less broad scope of a mechanism depends on how large the domain is in which the mechanism is instantiated. The degree of abstraction is measured by the amount of detail in the description of a mechanism. Special cases of mechanism schemata are “mechanism sketches”; these are abstract representations of mechanisms for which some intermediate entities or activities are not known.

In the case of a mechanism sketch, the abstraction from details is not deliberately chosen (for example, for didactic purposes), but is forced by the lack of knowledge. The discovery of a mechanism can proceed by schema instantiation—making a schema that is a mechanism sketch less abstract and applicable to a given case. If researchers proceed by schema instantiation, their task “is to


89. Ibid., 8.
90. Ibid., 15.
find the entities and activities, or modular groups of them, that play the roles outlined in the abstract schema. A schema has placeholders, variables, black boxes, that may be filled piecemeal as empirical evidence is found for the various components. The lack of an entity or activity or module to fill a role in a schema points to the need for further work.”

With this background in the recent philosophy of science, I would like to make two points about Tilly’s version of explanation by mechanisms. First, it does not seem that Tilly’s diagrams are sufficiently specific to be classified as complete mechanism schemata.

Complete descriptions of mechanisms exhibit productive continuity without gaps from the set-up to termination conditions. Productive continuities are what make the connections between stages intelligible. If a mechanism is represented schematically by $A \rightarrow B \rightarrow C$, then the continuity lies in the arrows and their explication is in terms of the activities that the arrows represent. A missing arrow, namely, the inability to specify an activity, leaves an explanatory gap in the productive continuity of the mechanism.

The criterion of completeness of the mechanism schema implies the possibility of converting this schema into an algorithm or computer program that allows one to simulate the “productive continuity” in question. Tilly’s diagrams of his “robust processes” do not seem to be specific enough to provide useful instructions for how to engineer polarization or identity formation, or how to make successful interventions to manipulate the overall outcome. They seem more like mechanism sketches. As a matter of fact, Tilly and his collaborators frankly concede this point: “nowhere have we demonstrated the empirical reality of any of these mechanisms. A formidable challenge therefore lies in the operationalization and systemic empirical interrogation of these, and other, candidate mechanisms.”

The question is, then, whether they can be elaborated in sufficient detail. Here is my second point: as one proceeds with elaborations of Tilly’s mechanism sketches, one gets involved in the difficulty that I would like to call the “dilemma of specification”: if Tilly’s diagrams really are those of robust processes of actor constitution, scale shift, or polarization, they cannot be specific enough to explain particular processes of actor constitution, mobilization, or polarization. Insofar as they are specific enough to account for these processes, they cease to be depictions or descriptions of robust processes. A sequence of stages leading from some input to output can be called a “mechanism,” if this sequence is a robust one—the same in all cases where the mechanism in question is working. The specification of a mechanism sketch into a complete mechanism schema should preserve this robustness, providing a diagram that has the same scope but more details about the components and activities involved in the mechanism. Most importantly, these components and activities should be the same in all cases instantiating this mechanism.

However, as one makes the attempt to elaborate Tilly’s diagrammatic “mechanism sketches” into descriptions of complete “mechanism schemata,” these sketches dissolve into many different diagrams of lesser scope that are applica-

93. McAdam et al., Dynamics of Contention.
ble only for some (or only one) episode from the initial set of the episodes that instantiated the “mechanism sketch.” Not only the degree of abstraction, but also the scope of the diagram, changes, so that it is not possible to consider them as depicting the same mechanism or robust process. The detailed account of how the robust process of “actor constitution” took place, for example, in the Civil Rights Movement in the U.S. in the 1950s, will show that the “micro-sequences” in the particular episodes instantiating the putative mechanisms were contingent on some specific circumstances that were not present in other episodes that were putatively considered as the instantiations of the same mechanisms (for example, those during the Cultural Revolution in China in 1966). In other words, more circumstantial research into the details of how some target outcome was produced typically reveals that the process was a highly contingent or fragile one. The effort to transform a mechanism sketch into a mechanism schema leads to process tracing, and the “mechanism” is no longer distinguishable from a “mere” story. Evidently, therefore, mechanisms cannot supplant stories if they do not work without stories or turn out to be stories in the “process tracing” used to elaborate a mechanism sketch into a complete mechanism schema.

Occasionally, Bennett and George see this problem and attempt to come to terms with it by using the concept of “equifinality” (different sets of initial conditions can lead to the same outcome), and by pleading for a “typological theory” that represents all possible paths leading to the same outcome.94 However, this idea fails to take into account the possibility that not only different sets of conditions can produce an identical outcome, but also that identical types of conditions can produce identical outcomes by different “mechanisms” (ways).95 This circumstance is the reason why ever-new (and interesting) stories can be told that involve the same initial constellation (for example, a “love triangle” or murder) and the same outcome (marriage; apprehending and punishing the perpetrator). The idea of “typological theory” does not solve this problem of the fragility and variability of the intermediate links (“mechanisms”), because this problem repeats itself within each “path” (type), if the “path” is identified by the set of identical initial conditions and outcome. To return to the famous example: although the historical situations that led to social revolutions in France, Russia, and China can be described as involving similar sets of initial conditions, this is not the case with the “intermediate links” that are “process-traced” by markedly different narratives. If this were not the case, there would be no point in reading the history of the Russian or the Chinese revolution after reading about the revolution in France.

V. CONCLUDING DISCUSSION

I started my argument with the statement that the current proliferation of divergent concepts of “mechanism” and “social mechanism” derives from the hopes for them as scientific “miracle makers” that could be useful for solving a broad

95. An exception is the possibility that each type is exemplified by only one case. However, in this case there is no sense in designating the whole enterprise as “typological theory.”
range of problems in different contexts: providing a middle ground between the “positivist” search for law-like generalizations to be used as premises in “covering-law explanations” and the “interpretivist” search for narratives and descriptions; framing an alternative concept of causation to the Humean view of it as constant association, and describing the kind of rationale needed for causal inference in quantitative (“large N”) contexts; warranting causal conclusions in qualitative comparative research working with a small number of cases; and supplanting standard (story-like) accounts of social episodes.

I have argued that these various services expected from mechanisms entail different and incompatible definitions of the concept of mechanism, precluding the possibility that theorizing in terms of mechanisms can bring about the integration of social theory and research, and bridge the qualitative–quantitative divide in social research. Elster’s view of mechanisms as under-specified “brute” folk-psychological generalizations about individual behavior presupposes the Humean view of causation and the “covering-law” theory of scientific explanation, but as such it is incompatible with that of some mechanicists who view mechanisms as robust generative processes that generate relations of statistical dependences between variables. Mechanicist quantitative researchers and methodologists consider knowledge about such processes to be an indispensable part of domain-specific assumptions, necessary to justify causal inferences from observational data. But this view of mechanisms contradicts the conception of some mechanicists who conceive mechanisms as contingent and fragile processes. This conception follows from the descriptions of narrative “process-tracing” by qualitative methodologists (who expect this procedure can help to solve or circumvent the famous “many variables, too few cases” problem). Tilly’s mechanisms are conceived as alternatives to the “standard stories,” but they remain of uncertain explanatory value insofar as they are not elaborated into detailed accounts or transformed into fully-fleshed stories.

Because of Tilly’s stature and influence both in sociology and historical studies, I have focused on the assessment of his contribution to “mechanism talk.” Insofar as Tilly conceives explanation by mechanism as the alternative not only to stories but also to covering-law explanations, he shares his goals with Elster. However, the difficulties that his approach meets make him a bedfellow with Elster. He shares with them also a dependence on the old tradition in the social sciences that mistrusts “stories” and “storytelling” as pre-scientific or para-scientific, and that uses these words as pejorative terms. The burden of this tradition accounts for the euphemistic preference for the term “process-tracing” by qualitative methodologists.

However, critics of storytelling may be overstating their case by failing to pay due attention to the difference between fictional and non-fictional narratives. The production of non-fictional narratives is constitutive of those historical studies that provide old, well-established causal accounts of the past, and are thus an indispensable collective epistemic enterprise. These narratives that represent the causal links between target outcome and the initial state satisfy all the tradition-
al requirements for historical scholarship, solid news reporting, and court investigation. Such stories that are acceptable include only singular statements that report the facts that are established by available evidence; such solid grounding in evidence provides the non-fictional story with informational content and epistemic value that holds even if asserted or implied causal connections are dubious or disputable.

Why one should reject the subsumption or replacement of stories by mechanisms?

First, I doubt whether the use of the same name for both stories and the items that are designed to supplant them can be fruitful. Because the different services expected from mechanisms as scientific “miracle makers” dictate different and incompatible construals of “mechanism,” the mechanisms that can be narrated cannot be identical to those that are supposed to bear the burden of justifying causal inferences in the quantitative context. To do this job, the generative processes that link independent (input) and target (output) variables must be robust, involving the same intermediate links and being invariant under some range of interventions. However, this cannot be expected from the mechanisms present in stories.

The causal processes that are traced by narratives are fragile; that is, they happen only because of the confluence of many contingent circumstances. These causally relevant circumstances may be links that produce a target outcome, but they are produced themselves by the accidental confluence of circumstances, and they play their causal role only as parts of causal conjunctures that are extremely unstable. It is the variability and fragility in the event-sequences that connect the initial conditions and outcome that make narrative an indispensable—maybe even the optimal—“cognitive instrument” for their representation.96 Because of their fragility and instability it does not make much sense to designate narrated sequences as “mechanisms.” Would we call a “mechanism” a complex tool (including “social tools”—institutionalized procedures) that does not display some minimal reliability or robustness in its workings?

Second, the dressing of the storytelling narrative as “process tracing” in mechanism talk has an unhappy consequence in that it impedes clarity about the real issue that is involved here. This is the question whether narrative explanations are the cognitive form _sui generis_ that is peculiar to the explanation of the events described in the “agency” framework, with “action,” “belief,” and “desire” as its fundamental terms.97 (Note that such explanations cannot be simply eliminated: they are the means whereby the “infinite regress” problem discussed above can be solved. In terms of the qualitative conception of mechanism, this “regress” means telling an ever more detailed or finely-grained story about what really has happened between the two events that are the initial and the end point of the problematic causal link. The danger of the infinite regress arises when we ask,


how far can and should such detailization go [provided the available evidence allows it]?

There is the long tradition of the theory of action and its explanation that maintains that this rock-bottom level is distinguished by the possibility of grounding singular causal statements in actors’ sincere testimony about the motives for their actions.98

One way of putting this is as follows: When an actor offers testimony to the effect that he or she did B because of A, and one wishes to establish a causal link “A caused B,” then the validity of the inference is not comparative but, shall we say, internal.99 The standard (statistical) model of causality enjoins on us an inflexible trinity: no causal explanation without comparative method and generalization. That is, comparison and generalization are both necessary conditions for a (causal) explanation. However, in human affairs, we can (with due circumspection) offer evidence (perhaps only probabilistic) for a causal explanation without invoking any comparison or generalization. Explanation on this reading is thus logically prior to either comparison or generalization.100

Is this the case? If so, then the danger of “infinite regress” in the explanation by mechanisms is pre-empted by the ontological peculiarity of the human world that finds its expression in epistemological features specific to the explanation of human action. However, no useful service is done by obliterating this real, or only alleged, feature of intentional or narrative explanation by the “mechanism” talk that makes the explanation of human action of a piece with the explanation of the working of clocks or other mechanical devices.

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