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## COMMENTARIES

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### Neither Science nor History?

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To a historian of the social sciences, Simonton's appeal is likely to evoke strong echoes of 19th-century proposals for a marriage between history and statistics. The most influential of these proposals was that of T. H. Buckle (1865), who thought that history could be turned into a science by showing that it depended on "social laws" conceived as statistical generalizations. Although that notion was kept afloat for a time by a wave of 19th-century positivism, it soon ran into difficulties (Porter, 1986).

That led to various attempts at sorting out the difference between science and history. Among these, Wilhelm Windelband's (1904) is the most relevant in the present context because Simonton uses the nomothetic–idiographic distinction, which goes back to this philosopher. But, between Windelband in 1894 and Simonton a century later, a conceptual shift occurred that enables the latter to return to a position more characteristic of an earlier philosophy.

Simonton, in common with other behavioral scientists, identifies the idiographic with unique facts—"names, dates, and places." Historical discourse then consists of two kinds of statements—those that refer to unique facts and those that imply generalizations, nomothetic or historical. But, this conceptual scheme has no place for what Windelband's term *idiographic* was meant to characterize—namely, the distinctive features of historical explanation. Windelband (1904) introduced his famous polarity in order to distinguish between two kinds of science, not between science and nonscience. The two kinds of science were what he called "sciences of law" (*Gesetzeswissenschaften*) and "sciences of events" (*Ereigniswissenschaften*). The former were "nomothetic", the latter "idiographic," but they were both sciences, both explanatory, although in different ways. The historical or idiographic sciences accomplish their explanatory task by demonstrating the interconnectedness of "a series of deeds" or of the life of an individual or a people or by showing "the character and development of a language, a religion, a system of jurisprudence" (Windelband, 1904, p. 11). Idiographic sciences look for a "historically determined

pattern" (p. 12). (The term that Windelband used is *Gestalt*.) Historical explanation deals in part-whole relations, not in logical subsumption under superordinate "laws," historical or otherwise.

With the rise of 20th-century positivism, there was a renewed attempt to annex historical explanation to scientific explanation through an appeal to the ubiquity of the "covering law" model (Hempel, 1959). But, after about 1965, the topic ceased to arouse much interest, partly because of the generally recognized inadequacies of the covering-law model, even in the case of the physical sciences (Kitcher & Salmon, 1989), and partly because historians did not find covering-law explanations in the least helpful (Novick, 1988). Because a rather extensive treatment of many of the issues raised by Simonton's proposals is to be found in these earlier discussions, I mention only one point that seems to be of special relevance—a point concerning the role of specific hypotheses in historical and in scientific discourse. In the latter, specific hypotheses can be considered in isolation and judged by the logic of empirical confirmation. But the general statements that form part of historical discourse are not detachable in this way. They do not function as potential covering laws but as textual components in the "synoptic" description of very complex situations. As one contributor to this discussion put it, one should not insist that historians "hand out piecemeal, like the slips in fortune cookies, tested hypotheses as 'what history teaches'" (Mink, 1966, p. 189). To do that is to pervert the purpose of historical inquiry.

In the usual reading of a historical text, the meaning of each statement depends on its contribution to the description of what Windelband would have called a "historically determined Gestalt." What Simonton proposes is that, in the case of the subgenre of psychologists' texts on the history of their subject, we substitute for this usual reading a new reading that treats these texts as though they were not historical but scientific. This is an essentially hermeneutic project of textual-meaning interpretation that is made possible by the ambiguity of many discursive elements when isolated

from the conventions of their textual genre. At best, this yields a possible reading, based on the acceptance of a particular interpretive framework.

Thus, when a text states that “many intellectual citizens” of Greek city-states torn by civil revolt “ceased to participate in public affairs and turned to a search for permanent and enduring qualities,” Simonton reads this as “a broad causal proposition that civil disorders deflect intellectuals away from politics and toward philosophy.” An alternative reading might interpret this as an observation about a characteristic of Greek city-states. Similarly, when an author states that a scientist’s commitment “*can lead to the malfunction of ‘single-mindedness’*” (italics added), Simonton suggests that we interpret this as “applicable to *all* scientists, whether great or small, academic or nonacademic” (again, italics added).

Such a procedure is perhaps justifiable on the grounds that the texts analyzed by Simonton are not historians’ texts at all but psychologists’ texts in the double sense that they are written by and for psychologists. Moreover, with one exception, these are texts the primary purpose of which is not the publication of historical research but of pedagogical persuasion. Their style is likely to reflect this function as well as the background of the authors. They constitute a hybrid genre for which the label *psychologized history* may be appropriate and that might be read either as weak behavioral science or as weak history. Simonton chooses the former alternative, whereas professional historians have tended to choose the latter (Ash, 1983; Young, 1966).

To the extent that these texts still include some elements of historical explanation, Simonton’s reading will be inappropriate. We seldom come across components of historical events that can be defined independently of other components, and the interaction among these components is never merely additive. Furthermore, the nature of the interactions frequently changes over time. Because the empirical methods advocated by Simonton are based on the assumptions of independence of variables—and additivity as well as temporal invariance of causal interaction—they appear to be singularly inappropriate as a vehicle for furthering historical understanding.

Of course, there can be no objection to making use of psychological knowledge in the course of historical inquiry, just as there can be no objection to making use of sociological, economic, or geological knowledge. But, if Simonton’s proposal were to be followed through systematically, it would seem to lead to a virtual replacement of historical judgment by a particular version of behavioral science. One reason I am not attracted by this prospect is that the model of science that Simonton presents appears to be so remote from the way that scientific inquiry usually operates. The

goal of Simonton’s science is stated to be “a huge collection of general principles,” “a huge stockpile of behavioral laws,” and an “ever expanding catalogue of behavioral laws.” This is strange, because in no established science could the structure of general principles be reasonably described as a collection, a stockpile, or a catalogue. Rather, we expect that a genuine science will offer us theoretical models that link its generalizations in a logically coherent system. The picture of behavioral science that Simonton gives us looks much more like a radical Baconian caricature of science than the real thing.

Before such a science could provide us with genuinely nomothetic laws, there are both empirical and conceptual problems to be addressed. Empirically, one would require a demonstration that the generalizations in question hold across a large range of times and places. But, the empirical evidence cited by Simonton is too often based on data from a single country and from historically minute time periods. At the very least, it seems premature to invest such findings with the authority of nomothetic generalizations. Similarly, there seems little reason at present to promote to the status of scientific “laws” the kinds of statistical relations cited by Simonton. Not only have these relations generally been established by applying the weakest of criteria, the null hypothesis, but they do not in themselves establish the existence of a causal effect. There is an immense distance between (a) finding that under certain conditions a better-than-chance relation appears between two variables and (b) establishing a scientific law. That distance cannot be covered by the mere heaping up of such findings. Statistical associations among separate variables are poor candidates for the status of nomothetic law in the absence of theoretical models relating to the pattern and conditions of their interaction.

Actually, there is an implicit theoretical model built into the methodology favored by Simonton. It is a model that assumes that the interaction of effects is additive. For many practical tasks faced by behavioral scientists, such an assumption can be justified on pragmatic grounds, but, for the kinds of events the historian tries to make more understandable, the additive model is almost certainly wrong. Even if that were not so, it would be contrary to good scientific practice to prejudge the issue by investing one specific methodological model with the authority of science as such.

A fundamental conceptual problem concerns the nature of the categories in terms of which generalizations claiming trans-historical validity are to be expressed. Such categories should be historically stable. To be able to claim that the relations of attribute X do not change between times P and Q, we must be confident that the meaning of X has not changed between those times.

## Note

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One cannot feel such confidence with regard to many of the categories that Simonton cites. For example, is the meaning of "a higher degree" the same today as it was a century ago, and do such things as disciplines and scientific careers mean the same thing before and after their institutionalization? The problematic nature of such generalizations is precisely the kind of thing that professional historians are interested in.

As Cronbach (1986) pointed out, even in experimental situations, "the so-called independent variable is the conjunction of the manipulated variable with all the other features of the situation" (p. 91). In situations that are not experimental, definition takes the place of manipulation. We construct variables by opting for specific definitions of categories. Strictly speaking, any effects we observe are attributable only to a particular category as it manifests itself in a particular situation. In ordinary research, it is often safe to abstract across situations because the range of situations is so limited. But, if we go to situations separated by centuries of time and vast cultural discrepancies, it becomes implausible to assume that the components of these situations can be captured by the same unvarying set of categories.

Ultimately, these questions relate to the function of historical inquiry as a vehicle for critical reflection. Historians of science have been able to make their unique contribution by analyzing science itself as a historically constituted formation of institutions, practices, and beliefs. To do that, they have had to forego allegiance to the shibboleths of scientific schools and to look at science as one historical product among others (Forman, 1991). Psychologist historians are gradually following in their footsteps and casting off an older tradition that assigned them the role of celebratory chroniclers. Simonton's proposals are unlikely to assist this process.

## What Is the Social Impact of Behavioral Generalizations?

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Simonton's target article, dealing with behavioral laws in the history of psychology, is a well-conceptualized and thoughtful presentation. Its exceptional documentation from widely diversified sources is one of its particular strengths. However, the essay is essentially descriptive and epistemological, as reflected in the questions it poses. It addresses the nature, uses, and sources of the generalizations; the degrees to which

they have been validated; and their proper status in psychology.

Aside from these questions, another group of somewhat more empirical questions might be addressed relating to their proper status in society. These relate to the impact of such generalizations on public policy, on society at large, and on the behavior of individuals in society. In formulating questions dealing with the im-