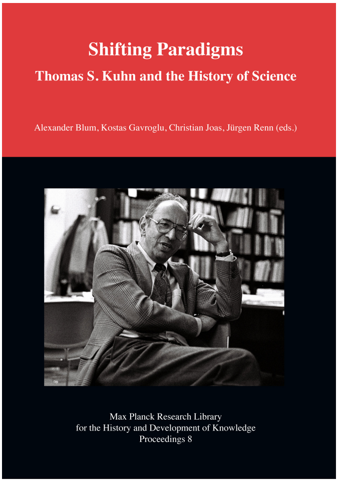
M. Norton Wise:

Goethe Was Right: 'The History of Science Is Science Itself'



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Chapter 26
Goethe Was Right: 'The History of Science Is Science Itself'
M. Norton Wise

In preparing for a recent conference reflecting on the significance of Tom Kuhn's *Structure* I was struck by how forthrightly the organizers stated that "little current work in the history or philosophy of science engages with Kuhn directly. Why and how did his program unravel?" I tend to agree with their assessment and want to engage with their question here. But the MPIWG conference that gave rise to the present volume displayed something that seems contradictory. Several of our most prominent representatives of social studies of science remarked on how deeply *Structure* inspired their own *sociological* work. They included such notable figures as David Bloor, Harry Collins and Martin Rudwick. Now this is strange, as there is no sociology in *Structure* and Tom never wrote what could be called a social history. Harry Collins dismissed this observation with the remark that people never know what's in their own books. I suggested that he and others saw the *need* for sociology in such statements as that paradigms were what a group shares so that they went about supplying it. This did not satisfy David Bloor. Nevertheless, I want to propose that the most basic reason so little current history of science engages with *Structure* is that social history, especially the social history of *practice*, plays such a fundamental role in current work, while it played little role in *Structure*, despite repeated references to practice, especially as exemplary problem solutions in the postscript. Tom, in fact, was rather hostile to the priority of practice in social studies of science and remarked more than once in conversation that he just could not get practice in their sense. Why is that? A first answer is that Tom understood history of science as history of ideas, in the strong sense that ideas were the active agents in history. But that answer has a more general context, which is the theory-dominated character of both science and the history and philosophy of science at the time he was writing (and he had of course been trained as a theoretical physicist himself). By theory-dominated I am referring in the first instance to the theory-ladenness of observation and experiment but also to the priorities of reduction and deduction, if only in a loose sense. In that world, the priority of practice was not quite comprehensible. But the world has changed, both in terms of the sciences and of the history of science.

And it is ultimately that change that I want to get at, with emphasis on narrative explanation.

Work

I begin with a brief characterization of some aspects of this change in historiography and then give an example from my own engagement with Tom and his work. For historians writing today the sociality and historicity of everyday life in the sciences, that is scientific practice, has become an unquestioned assumption. With that we take for granted not only the multiplicity and diversity of the sciences—on which Tom himself insisted—but also their embeddedness in economic, political and cultural contexts—which he severely circumscribed. So when we want to understand a development in one of the sciences we try to give a richly embedded historical development of its practices and representations. The science is presented within what I think of as its "field of interactions." Tom liked to say that he wanted to "get *inside* people's heads," to understand how they were thinking. Today we want to get *outside* people's heads, to understand the tools that make thinking and acting possible. "Distributed cognition" may be ultimately what we want to understand, in the sense that cognition is distributed over our tools and social relations, rather than only taking place in our individual brains. But more immediately, we aim at a narrative of how the field of interaction develops, and that developmental narrative constitutes our explanation of what happens. There are alternative narratives and there are better and worse narratives, depending on how well they incorporate the full range of evidence available. One could say that there remains something deeply Kuhnian in this, namely the essential historicity of science. And it is all too easy to forget how radical that notion was at the time of *Structure*. But the historicity now has a different character.

Let me illustrate this shift with respect to one of Tom's well-known papers. When he wrote "Energy Conservation as an Example of Simultaneous Discovery" (1959), he drew heavily on "the engineering concept of work" as one of three crucial intellectual constellations that fed into the early expressions of what would be identified as energy and its conservation. This paper remains a classic in the history of science, admirable for the clarity with which it brought into view issues that had remained largely outside history of physics. And it is characteristic of the best intellectual histories that Tom wrote. But it would not do today, nor even in the 1970s, simply because "work" remained largely a disembodied idea, an idea extracted from concerns with engines and "applied in deriving the abstract scientific conservation law" (Kuhn 1977, 92). The engineers and their interests thus disappear in the "abstract scientific" object of the analysis. When I began working with Tom in 1971, this kind of history had already begun to call out for something more, just as his discussion of paradigms did in *Structure* as read by sociologists of knowledge. Reference to the energy paper in *Structure* occurs in the Preface in a footnote attached to his acknowledgment that he had said nothing about “the role of technological advance or of external social, economic and intellectual conditions in the development of the sciences” but contending that, although they might affect the timing of a crisis or the range of revolutionary reforms available, their explicit consideration “would not, I think, modify the main theses developed in this essay” (Kuhn 2012, xliv). The footnote seems to back off a bit from this position with respect to energy conservation, but not with any specificity or consequence.

In fact a rather strict formulation of the internal-external distinction is essential for the “esoteric” and “professional” character of a mature science governed by the paradigms presented in *Structure*. Their “very special efficiency” depends on “the unparalleled insulation of mature scientific communities from the demands of the laity and of everyday life.” Even in conditions of crisis for a paradigm, “technical breakdown would still remain the core of the crisis” so that “external factors” were secondary (Kuhn 2012, 164, 69). It is just this insulation that had come into question from many directions in the 1970s.

In reworking the story of “work,” here with reference to both William Thomson in Britain and Hermann von Helmholtz in Germany, it seemed necessary to put the actual engines producing work at the center of attention, as active agents, especially in their role within the factories of industrializing economies (Wise 1988; Brain and Wise 1994). Where machines replaced humans, “work” replaced labor value as the source of the value of commodities and of the wealth of the industrial nation. This process of revaluation was intimately bound up with the emergence of energy conservation. But even writing at that level was too general. To understand “work” required figuring out what it meant to particular people in particular places and how it was valued and measured, not simply as force times distance but as embodied in engineering practice, for example as registered visually using indicator diagrams. In this way the study of “work” becomes an intensely local enterprise. The explanation of how energy, measured as work, became the most fundamental concept of physics in the nineteenth century, then becomes a story of detailed local histories and their interrelation. The concept of “work” does not generate these histories; *they* generate *it*. As such, they *explain* it, or so I propose to say.

I am sensitive to the fact that some people, especially scientists, may still see Tom’s history of ideas as more satisfying as an explanation because of its simplicity and conceptual clarity. It also conforms more nearly to the way in which physicists at the time he was writing preferred to explain things within a theory-driven enterprise. But that situation has changed rather dramatically for many scientists, particularly those dealing with complex systems, where it is common to use simulations to gain understanding, indeed to provide "explanations."

Snowflakes

The designation "complex" in this usage implies that the organization of the system is not subject to either reduction to a lower level of constitutive elements or to deduction from general laws. In this situation investigators typically explore the developmental dynamics of the system either experimentally or by using simulations as an alternative. I have been particularly interested in the way in which the simulations take on a historical character. To make clear what I mean, I will borrow my favorite example from a previous discussion (Wise 2011). It concerns snowflakes. Perhaps most of us will think of the typical snowflake as exhibiting an intricate geometrical pattern, highly symmetric, with six identical arms. This is the idealist image that Kepler, Descartes and Hook all presented in the seventeenth century. Their familiar assumption of mathematical regularity as the foundation for order and beauty in nature continued to govern studies of snowflakes through much of the twentieth century. Despite the fact that full hexagonal symmetry was very rarely observed, the asymmetry was ascribed to accidental disturbances of various kinds.

One major exception to this rule appeared in work of Ukichiro Nakaya, first published in English in 1954. Trained as a nuclear physicist, but lacking nuclear facilities at Hokkaido University in the north of Japan, Nakaya turned to taking photomicrographs of both natural and artificial snow crystals, which he grew in a cold chamber. Figure 1 shows his first artificial flake. Finding that "a perfectly symmetric snowflake is very rarely observed," Nakaya studied asymmetries and irregularities of many kinds, which focused attention on the normal processes of growth rather than on supposed states of perfection (Nakaya 1954). The result was effectively a natural history of snowflakes, published as a "museum" of micrographs, including both stages of growth and diversity of form. Such a museum was just not the sort of thing that interested most physicists in the 1950s, particularly not those who sought their explanations in elegant mathematical models. Nakaya's snowflakes gained little recognition.

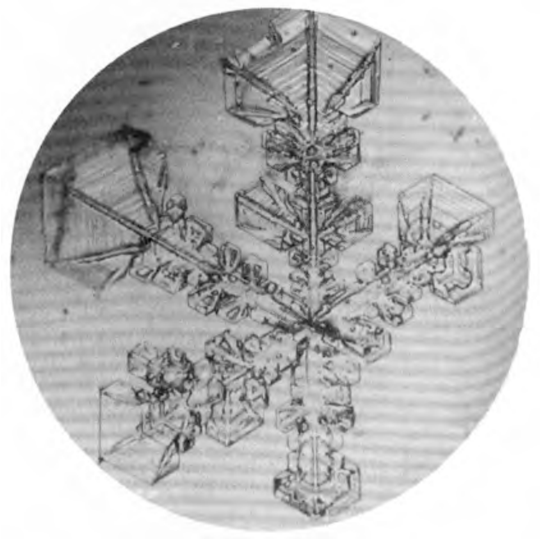

Figure 26.1: The first artificial snowflake (Nakaya 1954, 152).

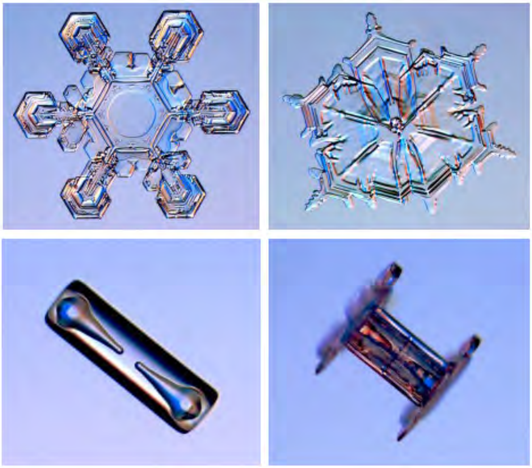

Figure 26.2: Photomicrographs of snowflakes (Libbrecht 2011).

The climate for work like his changed dramatically during the 1970s and 1980s as problems of complexity became ever more important in mainstream physics research. Only very recently, however, has a physicist at California Institute of Technology taken up snowflakes as part of his work on pattern formation in nonlinear, nonequilibrium systems. Kenneth Libbrecht has extended Nakaya's natural and artificial crystals with much higher resolution equipment (figure 26.2), yielding in 2006 what he called a *Field Guide to Snowflakes* (Libbrecht 2006). I take the term "field guide" to be explicit recognition that natural history and the study of nonlinear dynamical systems have much in common. Indeed, Libbrecht writes about snowflakes in terms of their "life history." The life history yields a lesson: "Complex history [produces] complex crystal shape" (though, ironically, even he harbors the idealist aesthetic preference: "I always select their most symmetrical crystals to display") (Libbrecht 2013).

The lesson of complex history is apparent also in the work of two mathematicians who do simulations. Even a decade ago it was not practicable to simulate the evolution of a snowflake at high resolution. But Janko Gravner and David Griffeath, have produced a three-dimensional, mesoscopic, computational model that replicates many of the basic forms or "habits" of snowflakes—dendrites, needles, prisms—along with their more intricate "traits"—sidebranches, sandwich plates, hollow columns (figure 26.3) (Gravner and Griffeath 2009, 1, 17).

Figure 26.3: Simulated snowflakes (Gravner and Griffeath 2009, 13).

Gravner and Griffeath employ a conceptually simple computational model, which grows a virtual snowflake from a small seed of ice surrounded by water vapor and governed by only three mechanisms: diffusion of water vapor from the crystal; freezing and melting in a narrow boundary layer; and attachment rates at the boundary that favor concavities. Despite this conceptual simplicity, however, implementation of the model in a continually updating cellular automaton requires many parameters and large amounts of computing time, even for a fully symmetric snowflake (about 24 hours on desktop computer). Gravner and Griffeath forthrightly acknowledge that it is not very clear just how their intuitively plausible parameters correlate with physical processes and that their simulations do not treat important issues of non-symmetry, randomness, singularities and instabilities. They nevertheless believe that the evolutionary simulations provide "explanations" of many of the characteristics of natural snowflakes, both in general morphology and in the details of their traits. Run many times over, with varying parameters, the simulations explore the space of possible snowflakes. These explorations discover previously unknown properties in natural snowflakes and suggest new kinds of observations.

The explanations and discoveries obtained in this work are natural historical in kind. Key terms are *trait*, *habit*, *morphology*, *seed*, *evolution*, *field guide*. The simulations not only generate a museum of snowflakes, but explain their characteristics by the conditions of their development, read as evolution. The algorithms governing the evolution may not be the Darwinian principles of variation and selection but they are nevertheless generative algorithms capable of explaining how the entire phylogeny derives from something like a common ancestor developing under varying environmental conditions. *That is, the simulations generate an evolutionary narrative which explains the natural order of snowflakes as an essentially historical order*. Every individual snowflake is a unique product of its history, full of contingencies and accidents. The virtual history of a snowflake, then, is its explanation. This is a long way from the traditional reductive and deductive explanations in physics, or indeed from anything Tom contemplated.

Finally, the role of visualization requires comment. Visual images have always been crucial in physics to guide intuition and to illustrate solutions. But the role of visualization in many simulations is qualitatively different, for it typically serves as the only effective means for understanding the growth process and its intricate results. The snowflake simulation, for example, must incorporate a technology for converting its calculations into visually legible images comparable with photomicrographs. Slide shows and movies naturally result. Inevitably these visual media enhance the sense that the simulation is productive of a narrative, a narrative that describes how a complex history generates a complex system.

Museums and History

The old question arises of whether historical narratives really explain. I am struck by the degree to which the literature on this question has been shaped by Carl Hempel's articles of 1942 and 1963 on explanation (Hempel 1965, 2001). His view that explanation requires subsumption under general laws reflected the assumption that theoretical physics supplies the model for all natural science. Narratives as such do not explain. The social sciences explain to the degree that they find principles of rational action for typical situations. Noretta Koertge, drawing on Popper and Hempel, gave a succinct formalization of this kind of explanation (Koertge 1975). The only point I would like to make is that such explanations seek to find something in the social sciences that would be analogous to deductions from general laws. The same remark applies to Arthur Danto's effort to defend narrative explanation, arguing that one could inscribe tiny historical micro-changes into the Hempelian mold, while nevertheless insisting that for macro-narratives describing long-term developments "no general law need be found to cover the *entire change*" (Danto 1985, 255). The problem of course is that all of this loses its point if, as in the snowflake example, deduction from general laws is not at issue.

The major alternative accounts of narrative, from more literary figures like Hayden White and Paul Ricœur, focus on its fictional character and largely dismiss the relation to natural science. This seems equally unhelpful. So I propose to throw out both traditions and to start over by returning to the early nineteenth century when history was gaining newfound prestige as a form of knowledge, especially in Germany, and when Goethe published his *Zur Farbenlehre* (*On the Theory of Colors*) in 1810.

Suppose then that there is good reason to think that in many areas of natural science explanations at one level of phenomena, say snowflakes, cannot be reduced to a lower level under general laws, say the dynamics of the water molecules that make up the snowflakes. What options are available if explanation has to rest on things and their relations all at one level? Basically, I think, we are left with two avenues: museums and histories. By "museum" I mean a collection that displays the diversity of generically similar things, ordered in an illuminating manner. That is one part of what the new snowflake people give us, whether as the natural snowflakes in Libbrecht's *Field Guide* or the simulated ones of Gravner and Griffeath. Under "history" I include two aspects: context and development in time. This second part is what the simulations of snowflakes provide: a context for water vapor under particular conditions of pressure, temperature, density and other parameters, and a developmental history of how a seed of ice grows in time as it falls for an hour or so through this continually changing environment, or context. The two parts, museum and history, are presented together as a natural history museum.

This combination is what Goethe prescribed in his *Farbenlehre* when he famously remarked: "The history of science is science itself." He meant this in two senses, the first concerning light itself, "we attempt in vain to express the inner nature of a thing. We experience only effects, and a full history of these effects comprises the essence of the thing." Here a "full history" is effectively a natural history museum, and he devoted Part I to comprehensively collecting and to showing how to produce such a history of the various effects of light as color, the "acts of light" as he put it (von Goethe 1890, ix). On this reading, the snowflake museums collect the diverse *acts* of freezing water vapor. The emphasis on the need simply to find out what kinds of things are in the world and how they come to be there is quite common among complexity people. In their manifesto for the twenty-first century, the condensed matter physicists Robert Laughlin (Nobel laureate) and David Pines said in 2000 that "The central task of theoretical physics in our time is no longer to write down the ultimate equations but rather to catalogue and understand [i.e., collect and organize] emergent behavior in its many guises, including life itself" (Laughlin and Pines 2000, 30).[1] Here the natural history imperative turns into an attack on pretentious theory, namely on quantum mechanics as a grand unifying theory, a theory of everything.

This attack mode captures the second sense of Goethe's dictum, the more infamous one. Just as *things* should be understood in terms of their histories, he insisted, so also with the *sciences* of things. The science of light should be understood in terms of its history of development. But this history had been marred over the last 100 years by a seriously distorting accident, namely the prominence of Newton's theory of light and colors with its mathematical reduction to rays of various refrangibility. So Goethe felt the need to rid the history of arrogant Newtonian reduction in his Part II, the polemical part, before going on to recovering the positive history in Part III.

Goethe's sense of contingency in history here, of wrong paths and alternative paths, is quite radical. I am reminded of the chauvinistic Berkeley professor who responds to the arrogance of his Harvard colleague with the observation: "If the Puritans had landed in San Francisco, Boston would never have been discovered." Since about 1970 there has been plenty of this kind of polemic coming from condensed matter physicists contesting the reductive and deductive pretensions of elementary particle people. Laughlin and Pines, for example, in their own appeal to the need to understand physics in terms of its history, say: "Indeed, one could ask whether the laws of quantum mechanics would ever have

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[1] See also Laughlin, Pines, et al. (2000), where they invoke evolution, growth, aging and adaptation to capture the analogy of physical to biological processes.

been discovered if there had been no hydrogen atom" (Laughlin and Pines 2000, 30). My snowflake people are not given to polemics, but if they were they would attack the 300 year prejudice for mathematical idealism that insisted on reducing the rich and complex history of snowflakes to simple, perhaps simple-minded, hexagonal symmetry.

Narrative—Suggestive Directions

Leaving Goethe and polemics aside, I want to make the wholly unoriginal point that the great explanatory power of museums and histories lies in analogy. And it is the power of analogy that is exploited in a number of related methods of investigation that employ non-reductive methods of understanding. Here I will just mention a few that figure in a volume on *Science without Laws* that developed from a two-year workshop at Princeton University (Creager, Lunbeck, and Wise 2007). They are model systems, cases and exemplary narratives. Simulations also appear but I will not say more about them.

Model Systems are surely one of the most powerful tools of twentieth-century biology. While eschewing any reduction of phenotype to genotype they offer strong heuristics for relating such things as tumors in nematode worms to human cancers, or just jet lag in humans to that in rats, based on conservation of evolutionary genetic acquisitions. Even physicists have been learning how to use the analogies of model systems in understanding complex phenomena. And historians of science have taken them up in a big way both as subjects of investigation and as tools for investigation. Angela Creager is one of them with her important book on tobacco mosaic virus (Creager 2001; also Kohler 1994). Model systems attain their great strength precisely because they are used in the first instance effectively to generate museums of the diverse "acts" of the system and secondly because of the developmental histories that they produce. Indeed this strength depends on a whole community of workers who develop the natural (or unnatural) history of the model system: the mice people, worm people and fly people. So following the model system provides a means of unpacking an interconnected network of materials, instruments, institutions and people.

In some ways similar to model systems are cases. The study of cases, and of case histories as narratives, has of course long been a standard means of using analogy in medicine, law and the social sciences. Again, the museums of cases and their histories are crucial. Excellent examples for the history of science appear in the work of Mary Morgan, both in her contribution to the *Laws* volume and in a new book on models in economics, *The World in the Model* (Morgan 2012). She looks in detail at a series of cases of economists employing models from Ricardo to the present. The result is a history of how economists have come to use and think about models over two hundred years. She analyzes also the function that a wide variety of specific narratives play in allowing economists to attach very simple models, like the 2x2 matrix of the prisoner's dilemma, to diverse situations in the world. This appeal to narratives operates quite widely in other analogical methods, as I have indicated for simulations. The important feature of Morgan's use of narratives is that she studies them as a tool for exploring the functionality of the models.

Exemplary narratives offer yet a third means of pursuing museological understanding. One of the best discussions I know is by Carlo Ginzburg in a paper in *Science without Laws*. He narrates the history of a particular individual acting in a richly described eighteenth-century context in order to explore the dynamics of the period in which the individual lives (Ginzburg 2007). The particular narrative is exemplary not in the sense of its being typical but in that it is representative for the situation. Thus Ginzburg sharply differentiates his generic approach from idealized models like Weberian ideal types. Here again, we can learn a great deal from Ginzburg's historiography about the function of narrative as an investigative tool.

To model systems, cases and exemplary narratives I would add one further area of contemporary history of science that belongs to my story of natural histories and narratives. I claimed for the snowflake simulations that "they generate an evolutionary narrative which explains the natural order of snowflakes as an essentially historical order." This generative aspect has a clear analogue in Hans-Jörg Rheinberger's analysis of "experimental systems" as "generators of the future." They are the laboratory systems that give life to what he calls "epistemic things," those not yet understood objects of investigation that the experimental system may or may not convert into an object of knowledge. The system will support a variety of narratives about what is going on and the outcome cannot be predicted. But it is generated by a historical process that can be explored and understood in retrospect. This is the task of history, to explain how the object comes to be known through as full an account as possible of the dynamical operation of the experimental system. Explorations of such systems provide the empirical base for what Rheinberger calls "historicizing epistemology" (Rheinberger 1997, 2010).

To conclude, I would return to the original question of the fate of Thomas Kuhn's *Structure*. Clearly it was a great inspiration for many people, including me, to pursue what we thought the book implied needed to be pursued: sociology, practice, materiality, political economy, culture. But none of those things were actually engaged in *Structure*. I have argued that they could not have been, not only because of who Tom was but because the world was rapidly moving out from under both science and history of science as he knew it. But these developments only enhance what was very much in *Structure*, namely the historicity of science. In the end, that has been both its most radical and its most lasting import.

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