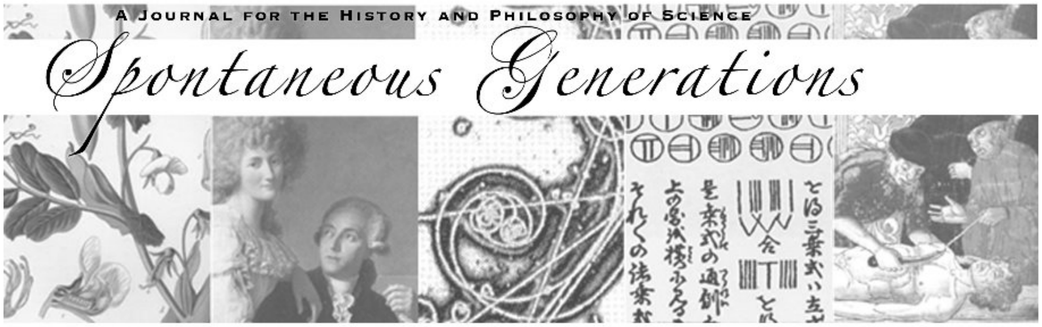

Was Kekulé's Mind Brainbound? The Historiography of Chemistry and the Philosophy of Extended Cognition

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PEER-REVIEWED

Was Kekulé's Mind Brainbound?
The Historiography of Chemistry and the Philosophy of Extended Cognition*†

David Theodore‡

This article examines the revisionist role that current debates and philosophical positions on extended cognition might play for the historian of science, and uses as its case study August Kekulé's formulation of the benzene molecule's structure, including the dreams that Kekulé reported as the origin of his model. It builds on the notion of engaging philosophical positions through the historiography of nineteenth-century chemistry, but also examines some of the implications of the history of science for extended cognition. While an extended cognition approach to Kekulé's use of graphics and visual materials is promising, I argue that there is less usefulness for the idea of collective cognition. Instead I advocate using detailed historical studies to test theories of extended cognition.

This article examines the revisionist role that current debates and philosophical positions on extended cognition might play for the historian of science, and uses as its case study August Kekulé's formulation of the benzene molecule's structure, including the dreams that Kekulé reported as the origin of his model. It builds on the notion of engaging philosophical positions through the historiography of nineteenth-century chemistry (Gay 1976; Gay 1978), but also, more ambitiously, examines some of the implications of the history of science for extended cognition.

I take extended cognition to be the idea that human beings think by opportunistically making two-way links–links that can sustain "systematic causal interactions"–with either non-biological exogenous resources or other human beings or both (Churchland 1984, 22). Andy Clark, whose writing about of extended cognition I will rely on here, has argued

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for the philosophical usefulness of this idea since an article on "The Extended Mind" he co-authored with David Chalmers in 1998 (Clark and Chalmers 1998; the article is included as an appendix to *Supersizing*, Clark 2008). Clark and Chalmers contrast extended cognition with two standard interpretations of cognition. The first position claims that thinking is trapped inside our head, so that what lies outside the body lies outside the mind. Following Clark's terminology, I will refer to this position as "intercranial" or "brainbound." They also acknowledge a second tradition in philosophy of mind, externalism, whose proponents argue that since meaning and language are to some extent dependent on the external world, so must be the mind. Extended cognition goes one step further. Clark and Chalmers argue that external states and processes, material and symbolic, are active, tightly-coupled parts of a cognitive "system." The world external to the individual not only supports cognition, or can be instrumentally manipulated as a tool, but actually "drives cognitive processes" (Clark 2008, 220). If external processes in a cognitive system are indistinguishable from "internal" ones, they argue, then the boundary of the skull is artificial, and we gain explanatory power by extending cognition beyond the brain and into the world. I will refer to this position as "extended."

Clark distinguishes two strong research programs in extended cognition. One would try to explain how minds ("specific cognitive agents") persist. The other would try to understand the components and boundaries of whatever system of agent-plus-environment "underpins" an agent's specific cognitive performance (Clark 2008, 116-18). Clark argues that option two has more interest for the philosophy of mind, and will perhaps be more likely to contribute to and take advantage of developments in cognitive science, whether in further studies of neurobiology or in understanding the coupled interactions between embodied brains and the environment, or even in coming to grips with longer time spans of development and evolution (Clark 2008, 218-19). Histories of particular scientific events might prove good cases for testing the appropriateness of extended accounts of the interaction between scientists and their physical and social environments.

At the same time, however, I also find persuasive Lawrence Shapiro's plea (2008) that if we cannot conceive ways to make claims for an extended mind–and not just for a temporarily spatially extended system–something valuable about the extended mind program gets lost. As Clark and Chalmers claim, "There are obvious consequences for philosophical views of the mind and for the methodology of research in cognitive science, but there will also be effects in the moral and social domains" (Clark 2008, 232). In particular, for the historian of science, I will argue that Kekulé's example shows that there is no outstanding value in renaming certain configurations of research teams and experimental instruments as extended cognitive systems, rather than socially-coordinated collectives.

The caveat, however, is that recent historians of science have already developed quite sophisticated procedures for including the material and the social in the history of science (Knorr Cetina 1999; Pickering 1995; Bijker, Hughes, and Pince 1987; Latour 1987; Shapin and Schaffer 1985). Peter Galison's examination of the material culture of twentieth-century physics, *Image and Logic*, testifies to the power of historical rather than philosophical method. That is, his account of the interaction between cloud chambers, laboratory spaces, and physicists deftly explains how scientific instruments embody both theoretical and practical knowledge, linking scientists and their environment. Galison never suggests, however, that the machine is part of a physicist's mental state; it is something physicists think *with*, or an historical document of knowledge and practice (1997, 75). For recent historians science is indeed spread out among people, places and things; in Andrew Pickering's phrase they are "mangled" together. Extended cognition holds the potential of explaining *why* they are so mangled. If somehow we could see collective systems not merely as distributed but as forming cognitive agents, we would have a potentially powerful conceptual tool to re-think and rewrite much of Western scientific history.

Kekulé's story is a suitable test case for three reasons. First, Kekulé's dreams are often described as the paradigmatic example of science being done by someone thinking through entirely internal processes. His dreams are part of the history of science, but also of psychological studies of creativity, psychotherapy, and pop culture. This literature primarily sees Kekulé's creativity as an heroic activity; extended cognition can allow us to test whether a non-heroic non-individualist account is appropriate here. Second, historians have treated him as an exemplar of scientific creativity, meaning that his mind, his thinking, was brainbound. Yet there are important parts of those historical accounts that might be better explicated through a philosophy of extended cognition: his emphasis on graphical and pictorial tools constitutes an attempt to make a case for the usefulness of pictures and symbolisms. Third and, more broadly, the benzene theory depends on a distributed community of scientists. Overall, I wish to show that while an extended cognition approach to Kekulé's use of graphics and visual materials is promising, there is less usefulness for the idea of collective cognition. Instead historians' accounts of group science may provide empirical cases to test theories of extended cognition.

I. Kekulé's dreams

The case of scientific creativity I have in mind is the oft-told story of German chemist August Kekulé's dreams (Brock 1996; Anschutz 1929). In 1865 and 1866, Kekulé published three papers that established the first viable theory of the benzene molecule's structure, opening up the whole field of aromatic compounds to scientific investigation. Indeed, in 1898 Francis Japp claimed that three quarters of "modern" organic chemistry could be traced to Kekulé's theory (Japp 1898; Rocke 1992, 145). It was not, however, until 1890 that Kekulé had publicly told the stories reporting how he came up with the theory while dreaming, stories that have become a mainstay in discussions of scientific creativity. Kekulé published them in a written version of the speech he gave at the *Benzolfest*, a twenty-fifth anniversary celebration of his first paper on the theory hosted by the German Chemical Society in Berlin:

> During my stay in London I resided for a considerable time in Clapham Road in the neighborhood of the Common. I frequently, however, spent my evenings with my friend Hugo Müller at Islington, at the opposite end of the giant town. We talked of many things, but oftenest of our beloved chemistry. One fine summer evening I was returning by the last omnibus, "outside," as usual, through the deserted streets of the metropolis, which are at other times so full of life. I fell into a reverie (Träumerei), and lo, the atoms were gamboling before my eyes! Whenever, hitherto, these diminutive beings had appeared to me, they had always been in motion; but up to that time I had never been able to discern the nature of their motion. Now, however, I saw how, frequently, two smaller atoms united to form a pair; how a larger one embraced two smaller ones; how two still larger ones kept hold of three or even four of the smaller; whilst the whole kept whirling in a giddy dance. I saw how the larger ones formed a chain, dragging the smaller ones after them, but only at the ends of the chain. I saw what our Past Master, Kopp, my highly honoured teacher and friend, has depicted with such charm as his "Molekularwelt"; but I saw it long before him. The cry of the conductor: "Clapham Road," awakened me from my dreaming; but I spent part of the night in putting on paper at least the sketches of those dream forms. This was the origin of the *Structurtheorie*. (Rothenberg 1995, 423-24)

Kekulé stressed in the speech that dream-concepts should not be given credence until examined by someone fully awake. Note, also, that although he designates this moment as the origin of his theory, this is not the first or last time that similar *Träumerei* and similar visions occurred. Apparently, such thinking methods were habitual for him. One story, his crucial vision of dancing snakes has burst out of the history of chemistry into psychology and even pop culture:

> During my sojourn in Ghent in Belgium I occupied an elegant bachelor apartment on the main street. My study, however, was located in a narrow side lane and during the day had no light. For a chemist, who spends daylight hours in the laboratory, this was no disadvantage. There I sat, writing on my textbook; but it wasn't going right; my mind was on other things. I turned the chair to face the fireplace and slipped into a languorous state. Again atoms fluttered before my eyes. Smaller groups stayed mostly in the background this time. My mind's eye, sharpened by repeated visions of this sort, now distinguished larger figures in manifold shapes. Long rows, frequently linked more densely; everything in motion, winding and turning like snakes. And lo, what was that? One of the snakes grabbed its own tail and the image whirled mockingly before my eyes. I came to my senses as though struck by lightning; this time, too, I spent the rest of the night working out the results of my hypothesis. (Rothenberg 1995, 424-25)

There have been several sustained scholarly attacks on the veracity of Kekulé's account, arguing that he was guilty even of outright lying and falsification (Wotiz 1993; Vanderbilt 1975; Wotiz and Rudofsky 1984). In *The Kekulé Riddle*, John H. Wotiz and Susanna Rudofsky argue: "The content of this, and most of the other chapters in this book, should correct and end the many outrageously erroneous stories originating from the Kekulé dream anecdote. . . and thus dispel the *Kekulé Myth* and the *Kekulé Dream-Paradigm* once and for all. The chapters also show that there is no justification for using the *Kekulé dream anecdotes* for the understanding of the *creative process* of the human mind" (1993, 267). Historian Alan J. Rocke, however, who has carried out the most detailed study to date of the early (i.e. pre-1866) elaboration of the theory, has responded to most of the criticisms, arguing that "the amount of detail included in [the anecdotes] suggests that they happened pretty much as Kekulé described them, nor do we have any persuasive grounds to accuse him of deliberate falsification" (Rocke 1985, 356). We can assume, then, as do most students of scientific creativity, that the incidents actually occurred much as Kekulé reports. In other words, I do not take on the additional burden of trying to argue that Kekulé made up the idea of thinking while dreaming, or that cognition in Kekulé's instance took place *unconsciously*. I instead concentrate on the connections between the extended cognition program and the historiography of science.

In the rest of this short paper I will deal with the details of Kekulé's science only as need arises. What I want to do next is look at three linked issues: a) Kekulé as exemplar of scientific creativity, b) Kekulé's work with diagrams and paper, and c) Kekulé's theory as a collective achievement.

A) Scientific creativity as cognition

Psychologists and historians assume that Kekulé's work was creative, and that they can use the benzene episodes as test cases for their definitions and theories about creativity. The whirling-snake dream, in particular, is an epitome in the psychology of scientific creativity (Gruber 1981; Gruber 2005; Rothenberg 1995, 419). For instance, the anecdotes constitute an important conceptual position in Arthur Koestler's impressionistic but influential 1964 book *The Act of Creation.* Koestler named the whirling-snake vision "the most important dream in history since Joseph's seven fat and seven lean cows" (1964, 118, 170). Margaret Boden claims that there is widespread agreement, at least among psychologists, that discussing creativity means attending to how novel ideas are "generated" in "individuals' heads" through "novel combinations (or extensions) of familiar ideas" (Boden 2005, 484). "The key question," she adds, "is how it is possible for new ideas to arise in someone's mind." That Kekulé's creative moment was a dream reinforces this version of creativity, in that dreaming is a mental activity that one person does alone, and that it seems to be a private, brainbound experience. For Boden, though, studies in neuroscience and psychology show that creativity is not a special capacity innate to only certain human beings, but rather arises from everyday cognitive practices such as memory and perception (Boden 2005, 480). Creativity then, is paradoxical, involving a kind of thinking that derives the unusual from the usual, the novel from the common. In considering the role of education in forming scientific creativity, Thomas Kuhn emphasized just this continuity between innovative and traditional thinking (Kuhn 1963). Thus scientific creativity is studied as a both a subset of creativity and as a way of understanding what science is and how science proceeds, progresses, and is taught (Taylor and Barron, 1963).

This description of creativity helps explain why Kekulé's dreams are so widely cited as a case study. First, his creative experiences–his dreams–are self-reported, so that we obtain insight into his internal mental processes, into his "individual head." In part the dreams' reputation is based on the scarcity of such self-reported accounts; there are remarkably few documented claims of dream-generated creative ideas in all of artistic and scientific history reported "by the creative people themselves" (Rothenberg 1995, 420-21).[1] This scarcity makes the popularity and tenacity of Kekulé's dreams all the more striking. That is, rather than being seen as extraordinary and atypical, Kekulé's self-reported experience is taken as exemplary and typical for "creativity" in general, in science, art, and elsewhere. Second, Kekulé's structure theory and benzene ring dreams have persistently been characterized as creative because they are speculative hypotheses: there was no contemporaneous experimental evidence that could have supported Kekulé's guess. As Rocke writes, "the dearth of appropriate evidence [ca. 1865-66] prevented him from establishing his views in anything like definitive form" (1985, 379). In this sense his proposals were mental representations, rather than material artifacts. Historians, including Rocke, describe the dreams as a mental rather than physical scientific representation–the elaboration of benzene theory is presented as a story of theoretical achievement, and not experimental material culture.

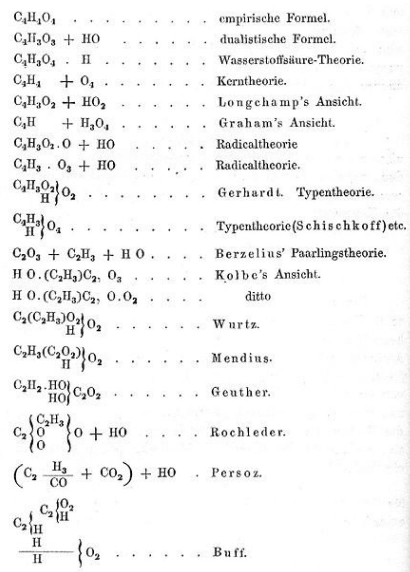
Clark's thinking on extended cognition helps us understand the material dimensions of Kekué's theory because a) it foregrounds the role of extraorganismic representations and b) it helps us to understand creativity *qua* cognition as a transformative link to external conditions, in which cognition is embedded, rather than as an ex-nihilo internal invention. The two concepts are closely coupled. Psychotherapist Carl Jung, for example, categorized the dream-vision as an instance of the mystic marriage; through the continuity of this mental imagery, Jung argued that traditional alchemical thought provided the ground for modern scientific chemistry.[2] For Jung, Kekulé did not create the image in a vision; the image exists, *a priori*, in all human beings. Jung writes: "Thus Kekulé's vision of the dancing couples, which first put him on the track of the structure of certain carbon compounds, namely the benzene ring, was surely a vision of the *coniunctio*, the mating that had preoccupied the minds of the alchemists for seventeen centuries" (Jung 1954, 168). Jung argues that dreaming provided access to shared mental imagery that, by focusing and fixing the

[1] My personal favourites among self-reported instances of creative dreaming come from pop music: a) Paul McCartney's report of having written "Yesterday," the most covered song of the twentieth century, while sleeping, and b) Johnny Cash's report of first "hearing" his breakthrough arrangement of "Ring of Fire," ornamented with Mexican trumpets, while dreaming.

[2] Images have histories (even if not all those histories can be traced to Jungian archetypes!). James Elkins (1999, 156) makes a similar point about images in twentieth-century science in a discussion of Galison's *Image and Logic* (1997): "It turns out," Elkins writes, "that some of the most purely 'logical' and 'homologous' (in Galison's sense) contemporary scientific diagrams are closely indebted to medieval and Renaissance mystical schemata."

scientist's attention, works "effectively in the material world" (Jung 1954, 169).

There is another way in which mental imagery may be shared among a community. In her book *The Creative Mind*, now in its second edition (2004), Margaret A. Boden argues that the computational versions of cognition developed in artificial intelligence can shed light on how creativity works, and, conversely, that looking closely at the mechanisms of creativity in specific instances can also advance computing models of cognition. Using Kekulé as an important example, Boden claims that creative ideas simply do not make sense without a "sociocultural context": they cannot be recognized as important or developed without being embedded in institutional and cultural communities. She adds that this interdependence between individual creativity and culture is particularly true in the case of science: "The sociocultural context (ranging from the creator's personal friends and acquaintances to the wider society) provides the styles of thought and most of the specific ideas required for an individual to think creatively. . . . In general, an individual's peer group is much more important in the generation of his or her ideas than is recognized by heroic, neo-Romantic accounts of creativity" (Boden 2004, 483). For Boden, a creative idea is embedded socially, but moreover it also *changes* the social setting in which it is embedded. This claim moves towards Clark's more radical insistence on our manifold ability to perform "self-engineering" (2008, 59-60) on both our cognitive tools and our environment. In Kekulé's case, the dream arose from familiarity with contemporary problems in chemistry (how atoms might explain chemical phenomena), and the vision in turn engineered a new tool that solved and then re-oriented those problems (towards manipulating carbon-chain compounds). This notion of embeddedness seems to serve the historian well, providing a serviceable version of the constitutive relations of environment and cognition (i.e. one that is more useful than the Kuhnian notion of an "essential tension" between tradition and innovation precisely *to the extent* the latter preserves a heroic, neo-Romantic attitude to scientific achievement; Kuhn 1963).



$C_4H_4O_4$	empirische Formel.
$C_4H_3O_3 + HO$	dualistische Formel.
$C_4H_3O_4 . H$	Wasserstoffsäure-Theorie.
$C_4H_4 + O_4$	Kerntheorie.
$C_4H_3O_2 + HO_2$	Longchamp's Ansicht.
$C_4H + H_3O_4$	Graham's Ansicht.
$C_4H_3O_2 . O + HO$	Radicaltheorie
$C_4H_3 . O_3 + HO$	Radicaltheorie.
$\left.\begin{matrix}C_4H_3O_2\\H\end{matrix}\right\}O_2$	Gerhardt. Typentheorie.
$\left.\begin{matrix}C_4H_3\\H\end{matrix}\right\}O_4$	Typentheorie (Schischkoff) etc.
$C_2O_3 + C_2H_3 + HO$	Berzelius' Paarlingstheorie.
$HO.(C_2H_3)C_2, O_3$	Kolbe's Ansicht.
$HO.(C_2H_3)C_2, O.O_2$	ditto
$\left.\begin{matrix}C_2(C_2H_3)O_2\\H\end{matrix}\right\}O_2$	Wurtz.
$\left.\begin{matrix}C_2H_3(C_2O_2)\\H\end{matrix}\right\}O_2$	Mendius.
$C_2H_2.\left.\begin{matrix}HO\\HO\end{matrix}\right\}C_2O_2$	Geuther.
$C_2\left\{\begin{matrix}C_2H_3\\O\\O\end{matrix}\right\}O + HO$	Rochleder.
$\left(C_2\frac{H_3}{CO} + CO_2\right) + HO$	Persoz.
$\left.\begin{matrix}C_2\left\{\begin{matrix}C_2\left\{\begin{matrix}O_2\\H\end{matrix}\right.\\H\end{matrix}\right.\\\frac{H}{H}\end{matrix}\right\}O_2$	Buff.

Figure 1: Kekulé's list of nineteen formulas for acetic acid published in Lehrbuch der Organischen Chemie (1861).

B) Benzene theory's material symbols

Kekulé's work is immersed in problems of symbolic representations (Francoeur 1997; Ramsay 1974; Wieninger 1998). Historian Pierre Laszio claims even that the chemist's life was dominated by how to represent chemical reactions by formulas (1979, 1019-20). Kekulé himself argued that his early studies in architecture gave him an "irresistible need" for visualization (Anschutz 1929, 994).[3] In 1867 Kekulé published an article in the London-based journal *Laboratory* on the conceptual usefulness of the atom. The article was meant to have two follow-ups, including one on graphic formulae (Brock 1996, 121; article rpt. in Anschutz 1929, 364-69). The link, for Kekulé, was clear: thinking about chemistry meant visualization, which in turn meant worrying about how chemistry *looks* when written down. This in turn provides a connection to Clark's argument, based on research in a variety of disciplines, that the materiality of symbols provides "simple affect-reduced, perceptual targets" (2008, 45) that reduce the "descriptive complexity of the scene" (2008, 46; see also Griesemer 2000, 3). Kekulé's interest in graphic imagery, then, might be better told as a story grounded in Clark's insistence that we see how representational symbolic systems, especially language, give us the "capacity to control and guide the shape and contents of our own thinking" (2008, 44).

In Kekulé's tale, thought clearly depends on language, writing, and paper as means for developing and exploring–elaborating and clarifying–the dream-visions. Recall his recollections: "The cry of the conductor: 'Clapham Road,' awakened me from my dreaming; but I spent part of the night in putting on paper at least the sketches of those dream forms," and "this time, too, I spent the rest of the night working out the results of my hypothesis." The eureka! moment consists of a vision. As Rothenberg puts it, "the breakthrough conception of the snake with its tail in its mouth was an organized mental image" (1995, 434). It's an image that in Boden's words led to "a historically new conceptual space" (2004, 71).

There are two strands to Kekulé's interest in graphics germane here, the development of the hexagonal benzene ring and the development of symbolism for chemical structures. They are somewhat difficult to separate, because these symbol systems act and interact as both abstract, conceptual devices and as representations of physical systems. Graphic visualizations allow chemists to work out chemical problems without physical experimentation, a result derided by Hermann Kolbe as "paper

[3]Kekulé wrote: "ein unwiderstehliches Bedürfniss nach Anschaulichkei." Rocke (1985, 377) translates this as "irresistible need for graphic imagery."

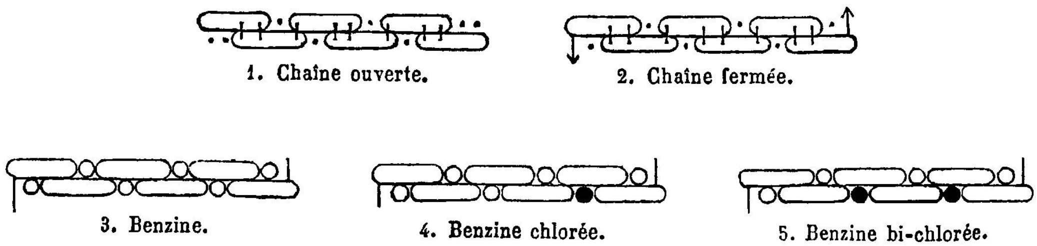


Figure 2: Kekulé's "sausage models" from August Kekulé, Bulletin de la Société Chimique de France 3, no. 98 (1865).

and pencil chemistry" (Nye 1996, 131). Nye adds that paper chemistry "allowed a chemist to predict both derivatives and isomers, based on the equivalent atomicities or valences of atoms, without leaving the desk. The graphical possibilities were endless and they provided innumerable hypotheses for laboratory investigations" (Nye 1996, 131). This debate about the value of paperwork is not unique to the debate about aromatics. Cambrosio, Keating and Jacobi, for example, show that immunologists Paul Ehrlich faced similar derision (and acceptance) in using graphical representations to develop and explain his theories (Cambrosio, Jacobi, Keating 1993, 682).

Kekulé was clear that his graphics did not represent the spatial positions of molecules, but rather described how the atomicity (valency) structures worked. The advantage, in Clarkian terms, is quite clear; the notation's features–"arbitrary amodal nature, extreme compactness and abstraction, compositional structure"–allowed chemists to develop "otherwise unattainable kinds of expertise" (2008, 55, 44). But it should not overshadow the standard and less radical story of terminological clarification. Kekulé published a famous list (see Figure 1, page 165) of the number of different ways chemists wrote out the formula for acetic acid, which made communication (and theoretical manipulation) difficult.

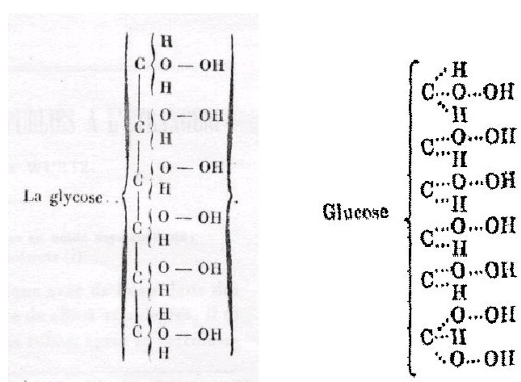


Figure 3: Diagrams of glucose from French and English versions of Archibald Couper, "On a New Chemical Theory," Philosophical Magazine [4] 16 (1858): 104-16.

Kekulé advanced thinking on two major graphic symbolic problems. The first, associated with the omnibus dream, was the question of how to represent chemical formulas and molecular constitution (Nye 128-33; Thuillier 1986; David, 1945). Kekulé's structural theory argued that atoms link up to from molecules on the basis of their atomicity values, and that in particular carbon is "tetratomic" and can form links with itself. How then to represent the links? Kekulé proposed a "sausage" formula, where rounded masses graphically bulge at the link points (see Figure 2, page 167).

A few weeks later, Scots chemist Archibald Scott Couper proposed using dotted lines and then straight lines (see Figure 3, page 167), a visual device that lead to the verbal terminology of "chemical bonds." By 1864 Alexander Crum Brown had worked out the notation still used today (see Figure 4, page 169).[4]

Kekulé's second visual contribution, associated with the fireside dream, was the idea of a hexagonal benzene ring (Rocke 19; Brush 1999). In Kekulé's 1866 paper "On the Constitution of Aromatic Substances," he proposed that aromatics, of which benzene (or "benzol") is the simplest, were formed of a nucleus of six carbon atoms in a closed chain with alternating double and single bonds (see Figure 5, page 171).

Kekulé's students experimented with a hexagonal sausage-like model of the interlocking nucleus, but the standard hexagon representation was invented by Adolf Claus, who claimed to get it from Kekulé (Brock 1993, 264-67).

C) Benzene theory and collective cognition

I argued in section A that extended cognition helps clarify Kekulé's work as a sociocultural phenomenon, rather than as a bounded intercranial event. The next step is to see whether extended cognition actually contributes to understanding that phenomenon. The previous section discussed the integration of the individual scientist with graphics and symbols; in this section I will look at the ways the individual scientist might be part of a collective system. The question then arises whether a group of scientists working on a common topic can have a collective mental state. Can cognition, in this case, the elaboration of a scientific theory, be socially distributed and collectively organized? Again, there are two closely coupled questions: a) can a mind extend across individuals and artifacts? and b) can a cognitive system extend across individuals and artifacts?

The turn here is from a consideration of how a scientist thinks to consideration of the question of whether scientists can think together. If one finds compelling any of Clark's examples of integrated systems (i.e. examples where non-biological systems and biological components

[4]For more diagrams and other models from the 1860s, see www.chem.yale.edu/ chem125/125/history/models/models.html.

together form a "cognitive profile"; Clark 2008, 99), then one begins to accept the position that cognition, whatever it is *exactly*, involves the body in an extracranial social and physical environment (Wheeler 2005, 11). But there are also defenders of the "orthodoxy" who claim that "there are principled reasons for believing that the kind of cognitive processing cognitive psychologists care about is, essentially without real-world exception, intracranial" (Adams and Aizawa 2008, 9; see also Rupert 2004). There does not (yet) seem to be a resolution to this debate. Arguments against distributed cognition claim that there is no value to positing collective mental states that are something other than the mental states of the individuals who make up the collective (see Rupert 2005; Bennett and Hacker 2003). But Bryce Huebner shows that such arguments against understanding cognition as a system constituted, if only temporarily, by a heterogeneous hybrid of brains, bodies, artifacts and environments are too strong, since they also constitute arguments against individual mental states (Huebner 2008, 109-10).

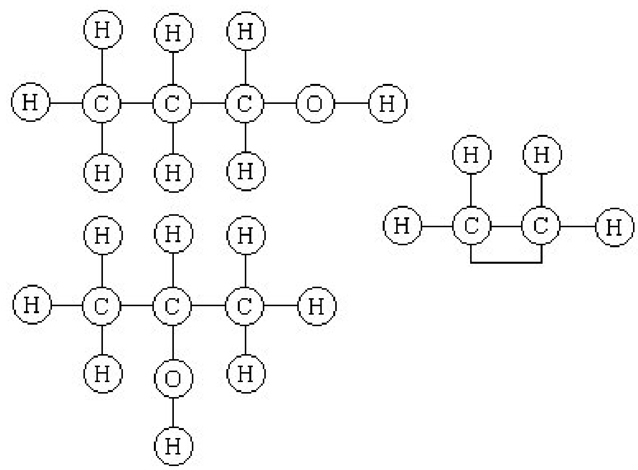


Figure 4: Alexander Crum Brown, chemical structural diagrams, from "On the Classification of Chemical Substances, by Means of Generic Radicals," Transactions of the Royal Society of Edinburgh 24 (1867): 331-9.

Kekulé's case allows us to approach these questions at an angle. The new approach asks: *if we accept* the premise of collective (or distributed) cognition (or mind), in what ways should we (or could we) then re-write the history of benzene theory? We can clearly give a sociological account of Kekulé's dreams in terms of his biography, institutional affiliations, and intellectual traditions; but can we give a *non*-sociological account in which the cognitive process of creating *Structurtheorie* is distributed across multiple individuals and artifacts?

Answering these questions is to a large degree historical, not philosophical work. Indeed it is not, in 2009, controversial to state that Kekulé's theories are actually the result of interactions among a number of chemists working on a number of problems related to atomicity and aromatics. Russell D. Larsen (1993), for instance, argues that structure theory clearly represents an example of multiple discovery. In the previous section I argued that a number of scientists worked out simultaneously symbols for the benzene ring and chemical formulae. In both these instances, the idea of collective work is strong. The term "chemical structure," for instance, seems to be the invention of Russian chemist Mikhailovich Butlerov. Overall, historians have identified a number of chemists and students whose contributions are critical to what are commonly known as Kekulé's ideas (Rocke 1984, 261-86).[5] Alan J. Rocke argues for a "gradualist" interpretation, noting (in detail) that the theory was developed only gradually, in continuity with previous theories, and only through "subtle reciprocal interactions of hypothesis and experiment" (Rocke 1985, 357). Kekulé himself realized that some kind of group contribution was necessary and desirable. He wrote in a letter: "A great deal is in the works; the plans are unlimited, for the aromatic theory is an inexhaustible treasure-trove. Now when 'German youths' need dissertation topics, they will find plenty of them here" (quoted in Rocke 1985, 370).[6] In short, even without the thesis of extended cognition, it is necessary to step back from the singularity of Kekulé's reverie on that omnibus from Islington to Clapham Road, and instead recognize the historical evidence for collective formulation of benzene's structure.

Still collective activity is neither evidence nor argument for the stronger claim that scientific creativity is collective *cognition*. Indeed, Rocke's analysis works *only* by imagining individual, brainbound scientists who work alone, sharing information through publications, demonstrations and conferences (Brush 1999, 23). The question of collective activity is most

[5]Two quotes from Rocke can serve as summaries of the social connections I have in mind: "The rise of structure theory is as convoluted a subject as the discovery of energy conservation; in both cases a number of scientists simultaneously contributed bits and pieces of the picture that eventually emerged, and in both cases it was not apparent that many of the contributors were even talking about the same set of concepts until after the dust had settled" (1992, 273). "Even when one steps back from the heat of battle and soberly considers relative merits, an unequivocal decision as to who was the originator of valence cannot be made. Frankland was correct in saying that his law requires little modification to convert it into a modern statement of valence. Nevertheless, Kekulé was correct in asserting that the modern concept derives directly from the Williamson-Kekulé school, Frankland's work being essentially without influence, and this in fact explains the chemical world's neglect of Frankland. Valence was discovered independently and in different contexts by two theoretical schools; Frankland's statement has technical priority, but Williamson's and Kekulé's proved more fertile and influential" (1992, 280).

[6]Kekulé to Baeyer, 10 April 1865, Kekulé Sammlung, Darmstadt.

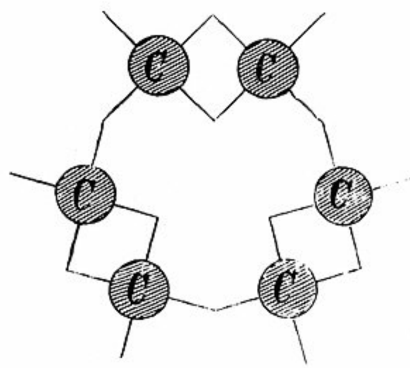


Figure 5: Benzene ring from Kekulé's *Lehrbuch der organischen Chemie.* Note that the hexagon is not regular, and that the carbon atoms are depicted as round atoms (i.e. not simply reduced to letters), and that they sit in the middle of the sides of the hexagon, not at the corners as in Alexander Crum Brown's version (and in typical "Kekulé diagrams" today).

often poised as a controversy of priority (Gay 1978; Merton 1957). Rocke, for instance, even though he notes that structure theory "was approached nearly simultaneously by several scientists from different directions," wants the historian to "reach impartial judgements on the relative merits of the protagonists," not to imagine that simultaneous discovery implies a shared cognitive system (Rocke 1981, 28). Historians are clearly aware that the development and working out–the "elaboration"–of Kekulé's theory involved a community of scientists, but working together does not mean thinking together, but simply conjugating multiple instances of brainbound thinking plus social communication.

At issue is whether designating such collective work as cognition would only amount to what M. Sheets-Johnstone calls (in a different context) a "lexical band-aid" (quoted in Clark 2008, 217). Whether writing the story of sociological cooperation or a cognitive system, the historian must search out and trace out the actual connections, links, communication, and so on between the parts of the collective. There is no obvious advantage to saying "cognition is distributed across the collective" when it can be treated by purely orthodox ideas of brainbound cognition plus communication.

One stumbling block is that for historians, there is a lingering need to identify agency, not simply to describe collective systems. Take, for example the recent discussion of high-energy particle physics in Karin Knorr Cetina's book *Epistemic Cultures* (1999). Ronald N. Giere (2002) explores the idea that research in big science centers on the experiment itself, not the individual scientists in the group. Giere acknowledges that there is distributed cognition in such situations, distributed heterogeneously among people and artifacts, but argues that there is no need to image the collective as an epistemic agent: "Individuals cannot *produce* the knowledge in question, but they can in a completely ordinary sense consciously come to *know* the final result" (2002, 7; this point is elaborated in detail in Giere, 2006.) Another way of putting it is that we can only ask the collective certain kinds of questions; otherwise, we get responses that are identifiable strictly and clearly with an individual, or no response at all.

So is big science a cognitive change or a sociological development? Knorr Cetina wants to use the extended view to differentiate two ways of making knowledge, one used by molecular biologists and a second used by high-energy physicists. But the outcome of accepting an extended view is that *both types of scientists would be using extended processes.* Therefore, there is no way to use an extended account to differentiate. As Giere writes, "The cognitive powers of both fields depend upon distinctive distributed cognitive systems," a formulation that still differentiates between intra-human cognitive systems and human-artifact systems (2002, 8). Likewise, it seems that sociological criteria are better than cognitive ones for historical periodization. Kekulé and his peers simply did not believe in extended cognition; it would seem unscholarly to re-write their interactions in a way so strange to their self-understanding, without some hope that we have therefore better–and not merely different–version of the story. In short, even if distributed cognition turns out to be the *best* description of how thinking happens in group science, the Kekulé example hints that such descriptions confer no advantage to the historian. Reciprocally, the thesis of extended collective cognition must be decided on grounds other than historical examples.

II. From Islington to. . .

I started this brief paper by saying I wanted to know whether the notion of scientific creativity would provide an appropriate if tentative proving ground for confronting the methods and concepts of extended cognition with practices in the history of science. By way of conclusion, I would like to briefly highlight some of the results of that confrontation. They include:

Questions of terminology acerbate the problem of bridging work on the psychology of scientific creativity, the philosophy of extended cognition, and the history of chemistry. If there is no robust and useful definition of cognition, then it is difficult to know the limits of cognition. Under certain definitions–such as Clark's–*all* cognition is by definition extended: "The intelligent process just *is* the spatially and temporally extended one which zigzags between brain, body, and world" (Clark 2002, 132). My account of Kekulé's use of graphics demonstrates the potential usefulness to historians in Clark's position but perhaps only by reducing "cognitive extension" from a movement to a lexical band-aid.

In Kekulé's case, the use of the term cognition is misleading because there is no simple identifiable cognitive task. So with a historical development–the elaboration of *Structurtheorie*, the concept of the benzene ring–what is needed is a more broad conception of intelligence and perhaps even consciousness (certainly of agency) to come to terms with the complexity of *thinking.* As Huebner, puts it: "making sense of a system as fulfilling a particular cognitive function is only possible by making reference to a particular task and a contrast class" (2008, 107). On these terms it is difficult to make sense of Kekulé's dreams, which "self-engineered" their own tasks. That is, Kekulé's dreams have no more agency than Kekulé possesses. So we could use extended cog as our means to *test* whether a non-heroic non-individualist account of creativity is appropriate here, although doing so will require historical research into the "system": the ineliminable, tightly coupled elements cannot be identified merely through inspection of the argument but rather require knowledge of social networks, communication, material cultures and chronologies.

The advances in understanding extended cognition may best come through empirical, not conceptual research programs. New definitions of cognition derived from studies in cognitive science approached from embedded, situated perspectives might make the category of "creativity" obsolete. Indeed, one infectious facet of Clark's project is the idea that a whole range of terminology that unreflectively prioritizes brainbound vocabularies might be dropped or significantly modified once the domain of cognition is re-conceptualized (i.e. cognitive systems studied as heterogeneous hybrids that, however temporarily, cross brain, body, and environment). Historians can use extended cognition to test whether concepts such as "scientific creativity" are appropriate.

Finally, and most importantly, even though Kekulé had a self-identified predilection for thinking with images, the existing scholarship still makes visualization and the examination of graphic material as material symbol only a small part of the story. Kekulé himself gives us the clue *not* to think of the dream visions as primordial, but rather his use of paper and quest for symbolic manipulation. Extended cognition, then, alerts us to the materiality of Kekulé's dream, and offers insight into how to make a close examination of his graphical practices.

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References

Adams, Frederick and Kenneth Aizawa. 2008. *The Bounds of Cognition*. Oxford: Wiley-Blackwell.

Anschütz, Richard. 1929. *August Kekulé*. Berlin: Verlag Chemie.

Bennett, Max R. and Peter M.S. Hacker. 2003. *Philosophical Foundations of Neuroscience*. London: Blackwell Publishing.

Bijker, Wiebe, Thomas Hughes, and Trevor Pinch, eds. 1987. *The Social Construction of Technological Systems.* Cambridge, MA: MIT Press.

Boden, Margaret A. 2005. Creativity. In *The Routledge Companion to Aesthetics*, 2nd ed., ed. Berys Gaut and Dominic McIver Lopes, 477-88. New York: Routledge.

Boden, Margaret A. 2004. *The Creative Mind: Myths and Mechanisms*, 2nd ed. New York: Routledge.

Brock, William H. 1993. *The Norton History of Chemistry*. New York: W.W. Norton.

Brock, William H. 1996. August Kekulé: Theoretical Chemist. *Endeavour* 20(3): 121-25.

Brush, Stephen G. 1999. Dynamics of Theory Change in Chemistry: Part 1. The Benzene Problem 1865-1945. *Studies in the History and Philosophy of Science* 30(1): 21-79.

Cambrosio, Alberto, Daniel Jacobi, and Peter Keating. 1993. Ehrlich's 'Beautiful Pictures' and the Controversial Beginnings of Immunological Imagery. *Isis* 84(4): 662-99.

Churchland, Paul. 1984. *Matter and Consciousness*. Cambridge, MA: MIT Press.

Clark, Andy and David Chambers. 1998. The Extended Mind. *Analysis* 58:7-19.

Clark, Andy. 2002. Reasons, Robots, and the Extended Mind. *Mind and Language* 16(2): 121-45.

Clark, Andy. 2008. *Supersizing the Mind: Embodiment, Action, and Cognitive Extension*. Oxford: Oxford University Press.

Elkins, James. 1999. Logic and Images in Art History. *Perspectives on Science* 7(2): 151-80.

Fierz-David, Hans E. 1945. *Die Entwicklungsgeschichte der Chemie: Eine Studie.* Basel: Birkhäuser, 1945: 233-40.

Francoeur, Eric. 1997. The Forgotten Tool: the Design and Use of Molecular Models. *Social Studies of Science* 27: 7-40.

Galison, Peter. 1997. *Image and Logic: A Material Culture of Microphysics.* Chicago: University of Chicago Press.

Gay, Hannah. 1976. Radicals and Types: A Critical Comparison of the Methodologies of Popper and Lakatos and Their Use in the Reconstruction of Some 19th century Chemistry. *Studies in History and Philosophy of Science Part A* 7(1): 1-51.

Gay, Hannah. 1978. The Asymmetric Carbon Atom: (a) A Case Study of Independent Discovery: (b) An Inductivist Model for Scientific Method. *Studies In History and Philosophy of Science Part A* 9(3): 207-38.

Giere, Ronald N. 2002. Discussion Note: Distributed Cognition in Epistemic Cultures. *Philosophy of Science* 69: 1-8.

Giere, Ronald N. 2006. The Role of Agency in Distributed Cognitive Systems. *Philosophy of Science* 73: 710-19.

Griesemer, James. 2000. Development, Culture and the Units of Inheritance. *Philosophy of Science* 67 (Proceedings): S348-S368.

Gruber, Howard E. 1981. On the Relation between "Aha Experiences" and the Construction of Ideas. *History of Science* 19: 41-59. Revised version in Howard E. Gruber and Katja Bödeker, eds. 2005. *Creativity, Psychology and the History of Science*. Dordrecht: Springer: 201-16.

Hein, George E. 1966. Kekulé and the Architecture of Molecules. *Advances in Chemistry: Kekulé Centennial* 61:1-12.

Huebner, Bryce. 2008. Do You See What We See? An Investigation of an Argument Against Collective Representation. *Philosophical Psychology* 21(1): 91-112.

Japp, Francis. 1898. Kekulé Memorial Lecture. *Journal of the Chemical Society* 78: 97-138.

Jung, Carl G. 1954. *The Collected Works of C.G. Jung*, Vol. 16. New York: Pantheon.

Kekulé, August. 1890. Rede. *Berichte der Deutschen Chemischen Gesellschaft* 23: 1302-11.

Knorr Cetina, Karin. 1999. *Epistemic Cultures: How the Sciences Make Knowledge*. Cambridge, MA: Harvard University Press.

Koestler, Arthur. 1964. *The Act of Creation*. London: Hutchison and Co.

Kuhn, Thomas. 1963. The Essential Tension: Tradition and Innovation in Scientific Research. In *Scientific Creativity: Its Recognition and Development*, ed. Taylor and Barron, 341-54. New York: John Wiley & Sons.

Larsen, Russell D. 1993. Kekulé's *Benzolfest* Speech: A Fertile Resource for the Sociology of Science. In *The Kekulé Riddle: A Challenge for Chemists and Psychologists*, ed. John H. Wotiz, 177-93. Vienna, IL: Cache River Press.

Laszio, Pierre. 1979. Kekulé ou l'éloge de l'équivoque. *La Recherche* 104: 1019-20.

Latour, Bruno. 1987. *Science in Action: How to Follow Scientists and Engineers Through Society*. Milton Keynes: Open University Press.

Merton, Robert K. 1957. Priorities in Scientific Discovery: A Chapter in the Sociology of Science. *American Sociological Review* 22: 635-59.

Nye, Mary Jo. 1996. *Before Big Science: The Pursuit of Modern Chemistry and Physics 1800-1940*. New York: Twayne.

Pickering, Andrew. 1995. *The Mangle of Practice: Time, Agency, and Science*. Chicago: University of Chicago Press.

Ramsay, O. Bertrand. 1974. Molecular Models in the Early Development of Stereochemistry: I. The van't Hoff Model, and II. The Kekulé Models and the Baeyer Strain Theory. In *Van't Hoff Le Bel Centennial*, ed. O. Bertrand Ramsey, 74-96. Washington, DC: American Chemical Society.

Rocke, Alan J. 1992. Kekulé's Benzene Theory and the Appraisal of Scientific Theories. In *Scrutinizing Science: Empirical Studies of Scientific Change*, ed. Arthur Donovan, Larry Laudan and Rachel Laudan. London: Kluwer Academic Publishers.

Rocke, Alan J. 1985. Hypothesis and Experiment in the Early Development of Kekulé's Benzene Theory. *Annals of Science* 42: 355-81.

Rocke, Alan J. 1981. Kekulé, Butlerov, and the Historiography of the Theory of Chemical Structure. *The British Journal for the History of Science* 14(1): 27-57.

Rocke, Alan J. 1984. *Chemical Atomism in the Nineteenth Century*. Columbus: Ohio State University Press.

Rothenberg, Albert. 1995. Creative Cognitive Processes in Kekulé's discovery of the structure of the Benzene Molecule. *The American Journal of Psychology* 108(3): 419-38.

Rupert, Robert. 2004. Challenges to the Hypothesis of Extended Cognition. *Journal of Philosophy* 101: 389–428.

Rupert, Robert. 2005. Minding one's cognitive systems: When does a group of minds constitute a single cognitive unit? *Episteme: A Journal of Social Epistemology* 1: 177–88.

Russell, Colin A. 1965. Kekulé and Benzene. *Chemistry in Britain* 1: 141-42.

Shapin, Steven and Simon Schaffer. 1985. *Leviathan and the Air Pump: Hobbes, Boyle and the Experimental Life.* Princeton, NJ: Princeton University Press.

Shapiro, Larry. 2008. Functionalism and Mental Boundaries. *Cognitive Systems Research* 9(1-2): 5-14.

Taylor, Calvin W. and Frank Barron, eds. 1963. *Scientific Creativity: Its Recognition and Development*. New York: John Wiley & Sons.

Thuillier, Pierre. 1996. Du rêve à la science: le serpent de Kekulé. *La Recherche* 17(175): 386-90.

Vanderbilt, Byron. 1975. Kekulé's Whirling Snake: Fact or Fiction? *Journal of Chemical Education* 52(11): 709.

Weininger, Stephen J. 1998. Contemplating The Finger: Visuality and the Semiotics of Chemistry. *HYLE–International Journal for Philosophy of Chemistry* 4(1): 3-27.

Wheeler, Michael. 2005. *Reconstructing the Cognitive World: The Next Step*. Cambridge, MA: MIT Press.

Wotiz, John H. and Susanna Rudofsky. 1984. Kekulé's Dreams: Fact or Fiction? *Chemistry in Britain* (20 August 1984): 7203.

Wotiz, John H. and Susanna Rudofsky. 1993. Herr Professor Doktor Kekulé: Why Dreams? In *The Kekulé Riddle: A Challenge for Chemists and Psychologists*, ed. John H. Wotiz, 247-75. Vienna, IL: Cache River Press.