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Maxwell's contrived analogy: An early version of the methodology of modeling

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ABSTRACT

The term "analogy" stands for a variety of methodological practices all related in one way or another to the idea of proportionality. We claim that in his first substantial contribution to electromagnetism James Clerk Maxwell developed a methodology of analogy which was completely new at the time or, to borrow John North's expression, Maxwell's methodology was a "newly contrived analogue". In his initial response to Michael Faraday's experimental researches in electromagnetism, Maxwell did not seek an analogy with some physical system in a domain different from electromagnetism as advocated by William Thomson; rather, he constructed an entirely artificial one to suit his needs. Following North, we claim that the modification which Maxwell introduced to the methodology of analogy has not been properly appreciated. In view of our examination of the evidence, we argue that Maxwell gave a new meaning to analogy; in fact, it comes close to modeling in current usage.

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To the memory of John D. North (1934-2008).

> The method pursued in this paper [see Maxwell, (1858/1890) 1952] is a modification of that mode of viewing electrical phenomena in relation to the theory of the uniform conduction of heat, which was first pointed out by Thomson [(1842/1854) 1872].... Instead of using the analogy of heat, a fluid, the properties of which are entirely at our disposal, is assumed as the vehicle of mathematical reasoning.... It was shown... that electrical and magnetic phaenomena present a mathematical analogy to the case of a fluid whose steady motion is affected by certain moving forces and resistances. [The purely imaginary nature of this fluid has been already insisted upon.]
>
> Maxwell, 1856, 11, 404, and 12, 316–317. The bracketed sentence is bracketed in the original.

> If an analogy is an explanation of the unfamiliar by the more or less completely familiar, then this [Maxwell's system of an incompressible fluid] is not a case of analogy. Perhaps we should distinguish between *established analogues* and *newly contrived analogues*.
>
> North, 1981, p. 129, italics in the original.

1. Introduction

Modeling has become a characteristic feature of modern science; hence, any history of recent scientific methodologies has to address the key role which model plays in many current scientific domains. One difficulty which is immediately encountered in developing such an historical account is the fact that the term "model" has been invoked for a variety of concepts, and it is therefore important to recognize that usages of the term have changed over time. The concept stands for concrete objects as well as abstract thoughts; for scaling; and for representing phenomena and data. It can also function as a thought experiment, a simulation, or an idealization of a general theory. Moreover, it may consist of set-theoretic structures, descriptions, as well as equations, and can offer physical interpretations of a mathematical structure (Frigg & Hartmann, 2009). Thus, although the term remains, the underlying concepts have changed. It is no wonder that, when asked what they mean by model, scientists give a remarkable variety of responses (Bailer-Jones, 2002). The attempt to sort out all such usages – historically and philosophically – is a project for a monograph; here we restrict our attention to a prominent physicist who, we argue, pioneered the scientific methodology of modeling, albeit not calling it so.

The figure of James Clerk Maxwell (1831–1879) looms large in the world of physics in the latter half of the nineteenth century. His contributions are comparable to those of Isaac Newton

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(1643–1727) in that both provided the fundamental theories that govern major domains in physics. Moreover, both were deeply concerned with methodology—the application of heuristic rules as well as the epistemic grounding of scientific knowledge. The making of a new methodology in the scientific realm is a rare event; it takes a measure of imagination and an acuteness of mind – notably in the likes of Newton and Maxwell – to conceive of a novel methodology and to put it to good use.

In this paper we are concerned with the methodology of analogy which was, as we will show, the foundation for the development of the methodology of modeling. As a methodology, analogy has been practiced throughout the ages and in many scientific domains, beginning in Greek antiquity; indeed, it is still pursued today with a great measure of success, following the traditional scheme of analogy, that is, proportionality, *a* is to A, as *b* is to B (see, e.g., Lahav et al., 2010). There has recently been a renewed interest in this methodology in the context of history and philosophy of science. A variety of usages of the methodology of analogy designed for different purposes have been identified, the applications of the methodology in specific domains have been studied, and an in-depth philosophical analysis has appeared (see, e.g., Gingras & Guay, 2011; Joas & Katzir, 2011; Darrigol, 2010, 2009; and Bartha, 2010). Undoubtedly, the term "analogy" stands for a variety of methodological practices all related in one way or another to the idea of proportionality. In this framework, we claim that in his first substantial contribution to electromagnetism Maxwell developed a variant of analogy which was completely new at the time or, to borrow North's expression, Maxwell's methodology was a "newly contrived analogue". Simply put, in his initial response to the experimental researches of Michael Faraday (1791–1867) in electromagnetism, Maxwell did not seek an analogy with some physical system in a domain different from electromagnetism; rather, he constructed an entirely artificial one to suit his needs. As hinted by North in an essay that has largely been neglected by recent commentators on Maxwell, the modification which Maxwell introduced to the methodology of analogy has not been properly appreciated. In view of our analysis of this modification, we argue that Maxwell gave a new meaning to analogy; in fact, it comes close to modeling in current usage. Although Maxwell retained the term "analogy" – he called his methodology "mathematical analogy" (Maxwell, 1856, 12, 316) – we claim that it functioned more like modeling. The analogy is contrived and in that sense it is not intended to illustrate anything in nature and certainly it does not represent a physical system. Thus, we take issue with Tweney's claim – based on earlier literature – that "for Maxwell, analogies across domains were of central importance (Hesse, 1973; Nersessian, 1984a, 2008), and he explored them intensively" (Tweney, 2009, p. 765). As we will see, in Maxwell's hands mathematical analogy is not "across domains"; rather, it relates fiction to physics.

Maxwell's paper at the center of our discussion, "On Faraday's lines of force," (Maxwell, [1858/1890] 1952) was read on 10 December 1855 and 11 February 1856, and these are often the only dates given in citations of it: shortly after it was read, an abstract of it appeared, which we designate Maxwell (1856). The paper was first published in 1858 in *Transactions of the Cambridge Philosophical Society*, 10, Part I, pp. 27–83, and then bound with vol. 10, Part II, which appeared in 1864, with an added title page for the entire volume.[1]

It is worth noting that for Maxwell Faraday's "processes of reasoning" are methods (Maxwell, [1858/1890] 1952, pp. 157–158). However, at the same time, he characterizes the usage of analogy as a "method of investigation" (Maxwell, [1858/1890] 1952, pp. 156, 157). In the abstract of the paper (published separately) Maxwell (1856, 11, 404) begins by stating the "method" he intends the pursue (see the motto, above). Evidently, Maxwell did not distinguish between "method" and "methodology", although the term "methodology" was available at the time.[2] Given current usages, we wish to introduce this distinction. Thus, we consider the way Maxwell used analogy, which he adopted from William Thomson (1824–1907; known as Lord Kelvin after 1892), a methodology, for it is a procedure for attaining knowledge, that is, it is employed (more or less systematically) in a field of study as a mode of investigation and inquiry. By contrast, a method is a plan of action or an ordered systematic arrangement. Indeed, the concept of lines of force, conveying in Maxwell's view processes of reasoning, is an ordered systematic arrangement of phenomena and thus a method.

At the time of Maxwell "model" did not have the relevant meaning we ascribe to it today. In the Appendix (see Section 7), we show that the meanings of "model" were restricted in the mid-nineteenth century. However, this evidence has not been taken into account by historians and philosophers of science. For example, Cat (2001, p. 432) claims that "the intended and manifest heuristic value of the models is not completely undermined by the fact – acknowledged repeatedly by Maxwell himself – that their success was also limited". In our view the historical record has to be reconsidered and the account put aright: at the time of Maxwell model was not yet formulated as a distinct methodology. It was just about to be introduced. In fact, one of Cat's arguments would be enhanced had he carefully distinguished various usages of the term "model". He places himself on a "slippery slope" (pun intended):

> Maxwell traced the source of error in Mr. Sang's *model* to the slipping added to the rolling contact between two parts ([Maxwell, [1890] 1952] SP 1, pp. 231–233). Then, in his 1862 presentation of the mechanical vortex *model*, Maxwell discussed slipping between cells and idle-wheels—illustrating electricity. There, slipping, added to rolling contact, is the source of energy wasted and dissipated in the form of heat ([Maxwell, [1890] 1952] SP 1, p. 486). (Cat, 2001, p. 432, italics added).

Notice the resulting confusion. The first use of model is appropriate for the design of a machine or an instrument (see Section 7.1), but the second usage is applied to Maxwell's vortex "hypothesis", which Maxwell did not call a "model". So here we have a mixing of categories. In the first case, Maxwell's view was that he had a new "design" (Maxwell ([1856/1890] 1952), p. 237) – not a new model – for this instrument, which he illustrated with

[1] There is a bibliographical difficulty here: the problem is acute since many historians and philosophers of science refer to Maxwell ([1858/1890] 1952) as Maxwell (1856). Moreover, we have found internal evidence, previously unnoticed, that Maxwell modified his paper after it was read in February 1856. Maxwell ([1858/1890] 1952, p. 180) alludes to a paper by Quincke, only citing Verdet's

(*footnote continued*)
French summary that appeared in June 1856 (offering an English version of the title, but not mentioning the date) of Quincke's German original that appeared in March 1856 (see Verdet, 1856, espec. p. 203, where the German version of Quincke's paper is dated March 1856; and Quincke 1856). Maxwell may have made other modifications as well. W. D. Niven, the editor of *The scientific papers of James Clerk* Maxwell ([1890] 1952), did not usually give dates of publication; rather, he cited the date when the paper was read (and in some cases he offered no date at all). It is true that many papers in the nineteenth century circulated as preprints to the author's close friends and associates but, in the absence of access to such preprints, we prefer the date of publication of the paper. However, where appropriate, we will also cite the date(s) when a paper was read.

[2] The term "methodology" was available in the literature in 1855. See *OED* sub "methodology"; Whewell ([1837] 1858, 3, 327) invokes "methodology" in discussing Linnaeus's *Systema naturae*.