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THE EMERGENCE OF THE MODELLING ATTITUDE

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1. A History of the Modelling Attitude

An 'attitude', or a 'stance', is a set of loose methodological and heuristic commitments, a style of doing science. It is not a thesis or a set of propositions explicitly defining the nature of science or its aim (Chakravartty 2004; Rowbottom 2011). The modelling attitude is the mode of scientific work that relies on the construction, development, and application of models; it does so to achieve the plurality of aims pursued by science. It need not be defined as a thesis about scientific knowledge: it is merely a methodological stance, a commitment to a mode of work.

Philosophical discussions about stances or attitudes are by now, of course, rather entrenched, and postulating a stance, or attitude, in the study of the nature and aims of science is a respected view. Arthur Fine (1984/1987) proposed a natural ontological attitude, and Bas Van Fraassen (2002) advanced an empirical stance. Both intended their views as viable hermeneutics in a project of understanding science. The aim of this chapter is more modest: it aims to defend that a large part of the present-day scientific work in the physical sciences answers to a 'modelling attitude'. It does not claim that this is the (only) hermeneutics suitable for natural science, or science in general; in fact, it makes no claims regarding the appropriate interpretational stance on science, taken as a whole. Rather, it approaches stances and attitudes as primarily part of the scientists' own methodological practices and only derivatively sees them as informing philosophical debates and narratives. Just as philosophical realism is born out of internal scientific disputes regarding the atomic hypothesis, so is the modelling attitude born out of scientific modelling methodology. Moreover, both are interconnected fin-de-siècle developments.

Indeed, the modelling attitude has a history, (Suárez, 2014, 2024) which sees it emerge in full force in the nineteenth century, in the wake of both British Victorian physics and the German theory of models or *Bildtheorie*. The main contention of this chapter is that there are interesting insights in this history that are relevant to the contemporary debate regarding modelling and the nature of representation. The story commences at a perhaps unsuspected place and time, the Scottish Enlightenment at the beginning of the nineteenth century.

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2. The 'Relativity of Knowledge' in the Scottish Enlightenment

The roots of 'analogy' and its use in British Victorian nineteenth-century science lie in the Scottish Enlightenment (see Davie 1961; Olson 1975; Harman 1998; Siegel 1991; Smith and Wise 1989). They can be located more precisely in some common-sense philosophical views regarding the nature of knowledge that derive from the practice of mathematical abstraction. Outstanding amongst this is the so-called relativity of knowledge, a thesis regarding the comparative nature of knowledge (hence in no way related to our contemporary forms of epistemic relativism).

The Scottish abstract school of mathematics was in many ways shaped over the generations by Robert Simson's (1756) commentary on Euclid's Elements – a book that went through many editions and was in print in the US until the end of the nineteenth century. In a much-discussed passage in the book, Simson develops the concept of a surface by abstraction, a process carried out entirely in the mind. First, consider a solid geometric object in physical space shaped as a rectangular block. Then, imagine the solid block divided into two halves, right down the middle. Had the surface in between any thickness, it would belong to either half. Yet, it cannot be part of either half because, if we imagine that half being removed, the surface still exists in the remaining half. By reduction, it follows that the surface has no thickness and belongs to neither half – it is rather an abstraction. We apprehend the nature of a plane, or surface, only when we split the real block in our mind, into two imaginary situations, and compare them. We can continue this process of abstraction to generate cognate results regarding one-dimensional lines as the intersection of planes and non-dimensional points as the intersection of lines.

While the nature of mathematical abstraction is involved and has roots in medieval concepts and doctrines that cannot be discussed here (see Davie, 1961, 127–149), one feature stands out for our purposes. The method of abstraction is a way to infer a result about a real physical object and its properties based on a piece of reasoning that is carried out in some imaginary situations involving this object. The analogy, or comparison, between such imaginary situations yields knowledge of the nature of the object or its properties. While this is a method envisaged for abstract mathematical (geometrical) properties, it is not hard to see how it could be implemented to obtain empirical knowledge regarding physical properties too.

The method of abstraction was one central ingredient in the intellectual milieu that saw William Thomson (Lord Kelvin, 1824–1907) and James Clerk Maxwell (1831–1879) develop analogy as a method for scientific discovery. The other key ingredient was the cognate thesis in Scottish common-sense philosophy that all knowledge is the result of apt comparison, the so-called "relativity thesis" (Davie 1961; Olson 1975, chap. 12; see also related discussions in Harman 1991, chap. 2). This opposed atomistic theories of knowledge, according to which knowledge can be exclusively of a given object. The Scottish common-sense tradition emphasised the way all knowledge of an object is the product of a comparison of that object with something else. Thus, the only means to achieve genuine knowledge of the world necessarily involves likeness, comparison, or analogy. The word 'analogy' was common in the nineteenth century, and its use is widespread even now (as a simple Google search shows) but, as we shall see, its meaning shifts into what we nowadays refer to as 'model'. By the time Boltzmann writes his 1902 entry for the Encyclopaedia Britannica, he has no need for 'analogy' and employs 'model' instead. A genealogical study of 'model' thus turns out what was considered a method, and an activity, involving analogical reasoning. It is a contention of this chapter that this genealogy is not a mere accident, but the history of the modelling attitude informs our current modelling methodologies (as well as, arguably, other features of our contemporary scientific culture) and merits philosophical attention.

3. Kelvin, Maxwell, and the Uses of Analogy

James Clerk Maxwell (1831–1879), Edinburgh-born and educated at its Academy and University, completed three full courses until he left for Cambridge in 1850. William Thomson (1824–1907) grew up in Glasgow and was linked to the city throughout his life. In 1892, he was elevated Baron Kelvin after the river that runs through the city and university. Maxwell was mentored by the physicist James David Forbes and the philosopher Sir William Hamilton, within the broad-based liberal Scottish educational system. Thomson was taught by his father, the reformist mathematician James Thomson, and the radical professor of astronomy John Pringle Nichol, who in turn had been trained at Aberdeen's King's College. All of these are habitual localities in the history of Scottish common-sense philosophy and abstract mathematics, and all mentors and tutees were willing partakers in both traditions.

Maxwell, in particular, was strongly imbued with the relativity thesis, including Thomas Reid's tenet that analogical reasoning was an unavoidable – however regrettable, in Reid's view – component of scientific reasoning (Olson 1975, chaps. 2 and 3). He went on to develop his own philosophical views in a paper delivered in 1856 at the Apostles in Cambridge (Maxwell 1856b/1890). The paper is a disquisition on the nature of analogy, and it shows that the term had a somewhat more general meaning than we ascribe it today, rather closer to our current generic notion of 'model' (see Cat 2001, for an insightful account of analogy and metaphor in Maxwell's thought). His central question concerns whether analogies are in mind or nature. This would nowadays be rendered as a question regarding whether models are realistic renditions of their targets or not. His response is revealing. Maxwell acknowledges that there exist objects endowed with properties and holding an array of properties and relations to each other. In our conventional contemporary terms, he is thus a kind of metaphysical realist. Yet, he also claims there to be a distinct kind of necessity that applies to thoughts – there are laws amongst thoughts that can only be said to apply to objects by means of some comparison or likeness. This induces a method for surrogative reasoning, which, according to Maxwell, is a typical inclination of any student of analogy ('modeller'): "Whenever they [men] see a relation between two things they know well, and think they see there must be a similar relation between things well known, they reason from the one to the other" (Maxwell 1856b/1890, 382).

We can take this to be a statement for the modelling attitude in the Victorian era. The mechanical models of the aether so dear to 'the Maxwellians' (Hunt 1991) are fine examples of Maxwell's view of analogy as reasoning via the perceived shared relations amongst distinct systems of objects. Mechanical models were taken to bear informative likenesses to the electromagnetic aether, and they were thus employed by Victorian physicists such as George Francis Fitzgerald, Oliver Heaviside, or Oliver Lodge to infer a diverse range of properties of electromagnetic radiation. Maxwell even took care to fill in the concept of reasoning employed as follows: "A reason or argument is a conductor by which the mind is led from a proposition to a necessary consequence of that proposition" (1856/1890, 379). As we shall see, the notion of a 'conductor' (itself a useful analogy) turns out to be critical to the development of a modelling attitude in the nineteenth century.

Maxwell himself famously put all these ideas to use in his development of Faraday's experimental findings on electromagnetic induction in a full electromagnetic theory, culminating in his celebrated *Treatise on Electricity and Magnetism* (Maxwell 1873). This development essentially took place at Cambridge, where Maxwell moved in 1850 for further studies, graduating in 1854, as a Trinity College fellow. He would return to Cambridge in 1871 as the new Cavendish professor, after short hiatuses at Aberdeen and King's College London. Thomson had also been a graduate student at Cambridge a decade earlier, and Cambridge provided both men with formidable formal skills through its mathematics tripos.[1] It was exposure to Cambridge that turned them into what Siegel (1991) calls 'deep theory modellers'. In Scottish common-sense philosophy, analogy is essentially a heuristic for research and discovery: however, one that could mislead if taken at face value. Analogy is to be employed, but not to be trusted too much, and Reid in particular disparaged against any realist interpretation. Under the influence of John Herschel's theory of errors and William Whewell's consilience of induction, both Maxwell and particularly Thomson became wedded to a more realistic form of analogy relying on classical mechanics.

This is perhaps best exemplified in Maxwell's two most important contributions on the road to a comprehensive electromagnetic theory. In the earlier 'On Faraday's Lines of Force' (Maxwell 1856a/1890), Maxwell exhibits a characteristically 'Scottish' attitude: he compares electrical and magnetic phenomena with the flow of an incompressible fluid through a porous medium, and he uses the comparison merely as a provisional template for investigating such phenomena. Anticipating a role for fictional assumptions in science, Maxwell even claims that the incompressible fluid is 'imaginary':

> The substance here treated of must not be assumed to possess any of the properties of ordinary fluids except those of freedom of motion and resistance to compression. It is not even a hypothetical fluid […] It is merely a collection of imaginary properties […]. The use of the word 'Fluid' will not lead us into error, if we remember that it denotes a purely imaginary substance.
>
> *(Maxwell, 1856a/1890, 160)*

Partly inspired by Thomson's (1847) and Rankine's (1855) molecular vortices theory of elasticity, Maxwell's attitude changed in the years leading up to 1861. Analogy became more than merely a useful heuristic. It developed into a magnifying glass for probing into the world, a window on the underlying laws of apparently detached and distinct phenomena. By the time he published 'On Physical Lines of Force' (Maxwell 1860/1890), the analogical source itself had changed: rather than modelling the induction in currents as a flow, the aether was then represented as molecular vortices in rotational motion, in terms of the famous vortices and idle-wheels model. The tiny counter-rotating 'idle-wheels' were introduced to account coherently within mechanics for such rotational motion (see the famous figure 2 in plate VIII in Maxwell's 1860/1890). This model is a mixture of heuristically useful assumptions, such as the idle-wheels, and what Maxwell called 'real' analogies, namely the molecular vortices themselves. The 'relativity of knowledge' drives all these attempts to illustrate electric and magnetic phenomena by means of mechanical models made up of elastic solids or fluids (Harman 1998, 71–80; Siegel 1991, chaps 2 and 3).

The turn away from useful mechanical models towards deep theory was to be completed in Maxwell's *Treatise*, in 1873, where Maxwell finally developed the theory of electromagnetism that bears his name. The so-called Maxwell equations were a somewhat later development arising out of mainly the work of Oliver Heaviside, but essentially all the main empirical results and theoretical concepts employed by 'the Maxwellians' (Hunt 1991) were already formulated in *Treatise*. This includes the radical insight that light is a transverse wave in the electromagnetic aether, as well as the famous equivalence of the speed of light with the inverse of the square of the ratio of electrostatic and electrodynamic units. The full history of the development is rich, and there is no space to broach it in detail here (Hunt 1991; Siegel 1991; Harman 1998; Cat 2001; Nersessian 2008). The main lesson for our purposes concerns the use of mechanical models in arriving at these theoretical developments. While there is some debate regarding how necessary the analogies are methodologically to arrive at the full electromagnetic theory (Hon and Goldstein 2020), it is undeniable that in Maxwell's own reasoning, the vortices and idle-wheels model plays a key role, particularly in the derivation of the displacement current (see Harman 1998; and particularly Siegel 1991, chap. 4).

4. Helmholtz and the Origins of *Bildtheorie*

Roughly at the same time as Thomson and Maxwell developed an English-speaking modelling attitude, Hermann von Helmholtz (1821–1894) established his 'Berlin school' of physics and in so doing set up a distinct German-speaking variant of the nineteenth-century modelling attitude. Helmholtz's account of *Bilder* was essentially driven by his sign theory. The 'Bildtheorie' – literally the 'theory of images' – is not merely an account of scientific representation: it is also the name of a movement in scientific modelling practice that emerged in fin-de-siècle Austria and Germany. While it is expressly inspired by the English-speaking modellers – most prominently by Thomson and Maxwell's analogies between fluid mechanics, heat, and electricity – it also has its own roots in Neo-Kantian empiricism. Thus, although the Bildtheorie emerges most explicitly in the writings of Heinrich Hertz (1857–1894) and Ludwig Boltzmann (1844–1906) towards the end of the century, it is really to their mentor Hermann von Helmholtz (1821–1894) that we must look to searching for its intellectual and historical sources.[2]

According to Buchwald (1993), 'Helmholtzianism' is an open-ended set of methodological maxims for the practice of experimental science. At the core of this practice is the requirement to actively intervene experimental setups to obtain anomalous results or effects. These would be described in terms of the ascription of dynamic states to systems, together with functions operating on these states representing interaction potentials. The evolution of the states is therefore the key to the result of the interaction, and Helmholtz assumed everything else was essentially redundant or derivative, including charges, currents, or forces. Thus, contrary to what is sometimes supposed, Helmholtz was never entirely at ease with action-at-a-distance theories such as those of Wilhelm Weber and Gustav Fechner (or their equivalent over in Britain, such as those taught by Rouch and the other Cambridge coaches until well into the 1890s, as described in Warwick (2003)). Rather, he followed Franz Neumann in not presupposing any account of charges or currents, or the forces supposedly acting on them at a distance. Thus, Helmholtz – and Neumann – postulate a potential function between any two charges whose shape depends on their distance. The energy of the system is thereby determined without making any further assumptions regarding the nature of the system of charges itself, or the forces operating, other than the system that can be ascribed a 'state', which figures in a potential 'function' that fully describes its interaction properties. As Buchwald (1994, 15) puts it: "It could […] be said of Helmholtz that after the early 1870s nothing was clear to him until it could be formulated in terms of interaction energies".

This is relevant to our present purposes for three reasons. First, it belies the thought that Helmholtz was initially resistant to field theories such as Maxwell's. On the contrary, Helmholtzianism is essentially neutral on whether fields mediating inductive currents on conductors exist, or instead forces acting at a distance displacing charges and thereby setting up such currents. There was no transition in Helmholtz from an action-at-a-distance to a field-theoretic account of electromagnetism because Helmholtz was never wedded to an action-at-a-distance theory to begin with.[3] The models Helmholtzians and Maxwellians countenanced were in fact similar from the start. This is perhaps not surprising, since Helmholtz and Thomson corresponded regularly and read each other's work avidly (Smith and Wise 1987, 1989). Furthermore, Helmholtz's (1870) proof that Fechner–Weber theories entail the predictions of the Maxwell displacement current model was a noted milestone on both sides of the channel (Buchwald 1993).

Second, Helmholtz's initial training was in medicine, and he started as a sort of Neo-Kantian, committed to the principle of causality and a style of causal realist explanation (Heidelberger 1993; Turner 1993). Yet, starting with his work on the physiology of perception in the 1860s, he progressively veered off towards a generic form of empiricism (Eckert 2006, 19; Patton 2010). Thus, Helmholtz moved away from the idea that perceptions are 'copies' of the objects perceived towards the view that they are signs instead, standing in the same conventional relation a name stands to its bearer. Helmholtz's 'sign theory' is a direct predecessor of the *Bildtheorie*: it identifies perceptions with representational signs, which can be operated upon in accordance with certain rules of inference. And indeed, at roughly this time, Helmholtz begins to employ the term 'Bild' to refer to the discovered laws of science (Schiemann 1998, 25). Hertz and Boltzmann inherited the insight that models are sign systems endowed with internal rules of inference.[4]

Third, and finally, Helmholtz's characteristic neutrality on issues of ontology is inherited by both Hertz and Boltzmann and turns out to be at the heart of the German-speaking modelling school. The principal lesson that Hertz and Boltzmann derived from their work in Helmholtz's laboratory is that the most appropriate representations must abstract away from the concrete material details of systems and instead focus on dynamic states and their potential and interaction functions. Once the appropriate dynamic models are adopted, ontological disputes will prove beside the point. Are there really forces in nature, or just masses? Do atoms exist, or are they just packets of energy? These are ontological disputes that are beyond the purview of scientific models per se but rather belong to the domain of interpretation. Hertz's attempt to derive a representation of mechanics devoid of forces and Boltzmann's attachment to atoms do not have the dogmatic character of a believer (in potentials and atoms, respectively) so much as that of a sceptic regarding forces and energentism, respectively. In both cases, they are attempts at justifying introducing alternative scientific models.[5]

5. Hertz and Boltzmann: Conformity and Information

There is one critical difference between the mentor and mentees, though: where Helmholtz upheld the principle of 'sign constancy' (Schiemann 1998), Hertz and Boltzmann allowed for multiple alternative representations. Hertz (1894) puts it with characteristic clarity:

> The images which we may form of things are not determined without ambiguity by the requirement that the consequents of the images must be the images of the consequents. Various images of the same objects are possible, and these images may differ in various respects. (1894, 3)

Ultimately, it is this multiplicity of models of phenomena – and their underdetermination by both experimental evidence and dynamic presuppositions – that gives rise to both Hertz's and Boltzmann's unusual scientific views at the time (D'Agostino 1990; De Regt 1999, 2005).

Heinrich Hertz's full formulation of the *Bildtheorie* came in early enough in his astonishingly deep *Principles of Mechanics*, where he famously wrote:

> We form for ourselves images or symbols of external objects; and the form which we give them is such that the necessary consequents of the images in thought are always the images of the necessary consequents in nature of the things pictured. (1894, 3)

This is through and through a Helmholtzian insight. There is first the idea that models are symbolic representations endowed with certain rules of inference (symbolic or logical necessity). There is then the thought that such models are related to the systems represented not by standing as copies of them, but only in the way in which conventional signs stand for their bearers – merely, at best, by exhibiting correlations between their consequents. The laws of nature and the rules of *Bilder* answer to different sorts of necessity (natural or physical; and logical or symbolic, respectively), but the consequences of rules and laws must correspond to each other. Thus, Hertz goes on to write: "The images that we here speak of are our conceptions of things. With the things themselves they are in conformity in one important respect, namely, in satisfying the above-mentioned requirement" (1894, 3).

It can then be argued, following Hertz, that 'conformity' is the only necessary condition on *Bilder*, the only defining condition on a scientific model or representation. It is not, however, the only virtue that a model can have. Hertz lists another four desirable properties in a *Bild*, namely *permissibility*, *correctness*, *distinctness*, and *appropriateness*. These conditions, Hertz argues, are not always fulfilled in every model. In fact, they often militate against each other, so that they must be traded wisely within their context of use. Thus, in practice, no model possesses them all, and most models struggle to possess one of them at all. Hertz's introduction of these conditions is interesting for what it lets in as desirable virtues of a model, but even more so for what it leaves out: 'conformity' is not taken to be an optional virtue, but the only necessary condition on any model.[6]

Thus, 'permissibility' is coherence "with the laws of our thought" (Hertz 1894, 2), which on the face of it appears to be a requirement of consistency or non-contradiction. Yet, Hertz is clear that a model may be contradictory, yet conform. And if a model conforms, it remains indeed a model. This makes room for models of fictional or impossible worlds, which may be 'impermissible' in this terminology, but are nonetheless allowed if they conform. 'Correctness' is the requirement of consistency with the properties of the target system, since an incorrect model, according to Hertz, is one whose "essential relations contradict the relations of external things" (1894, 2). Again, a model may be grossly 'incorrect', or inaccurate, or even an artefact, in the sense of being built to purpose but not necessarily truth-apt, yet conform and hence remain a model. According to Hertz, 'distinctness' is the requirement that a *Bild* provides an accurate rendition of every aspect of the target; we would nowadays refer to this roughly as 'completeness', and obviously, it is not a plausible requirement on any model. Thus, many models are highly streamlined, idealised, abstract, or 'indistinct' in Hertz's terminology, yet of course, they remain models if they conform to their targets. Finally, 'appropriateness', according to Hertz, is a measure of simplicity. A minimal model is 'appropriate' if it does not make or contain superfluous claims regarding its target system. Another way to put Hertz's thought is that an appropriate model lacks any properties that have no role at all in the sorts of inferences that the model promotes with respect to its target. Hertz most clearly does not think that every scientific representation is appropriate: his *Principle of Mechanics* is a forceful argument to the effect that the standard representation of mechanics in terms of forces acting at a distance is inappropriate, at least when compared to his own much more streamlined and scarce representation in terms merely of mass and potentials. More generally, it seems indeed clear that the conformity of a scientific model in no way requires its appropriateness: most models are far from minimal, and they contain elements that are extraneous to their representational tasks.

Hertz's discussion, I argue, is a *tour de force* and sets the stage for the ensuing modelling attitude. Nevertheless, Hertz's *Principles of Mechanics* remained controversial, and Hertz's untimely death in 1894 curtailed this work. So it was down to a devoted admirer, Ludwig Boltzmann, working in Vienna, to promote the *Bildtheorie* most firmly. The high peak of the German-speaking school of modelling may well be signalled by the publication of Boltzmann's *Popularen Schriften* in 1905.[7] Boltzmann's goals for modelling are also arguably less lofty than Hertz's, imbued instead with characteristic Viennese pragmatism and empiricism. The modelling attitude is, in Boltzmann's hands, what results from the application of principles of economy of thought to scientific theorising: "As the facts of science increase in number, the greatest effort had to be observed in comprehending them [models] and in conveying them to others" (Boltzmann 1902, 2).

Boltzmann also added a requirement of informational gain to Hertz's minimal condition of conformity. In discussing the models in thermodynamics that he was so instrumental in establishing, he wrote: "If for one of the elements [in the model] a quantity which occurs in calorimetry be chosen – for example, entropy – information is also gained about the behaviour of the body when heat is taken in or abstracted" (1902, 2). A model must show conformity to its target, but not any conformity will do: the model must provide us with relevant new information about that target. It is this combination of minimal conformity and informational gain that makes a model scientific – and a valuable instrument for surrogate reasoning regarding its target. Together, these two requirements bring into relief Maxwell's notion of a 'conductor' as an instrument for reasoning, which was reviewed in the first part of this chapter. It is not a coincidence: Boltzmann was arguably led to the informational gain requirement through Maxwell's analogies, which he had studied very closely (Klein 1973). Furthermore, it is through these two conditions that we can ultimately understand the work that analogy and metaphor can do for us in scientific inquiry. Reasoning by analogy requires both a degree of conformity (to make it possible to inquire into the nature of an object or system by means of a comparison to other systems or objects) and a measure of informativeness, the capacity of the source of the analogy to enlighten us regarding aspects of the target that had not been considered before.

In setting such a minimal bar on acceptable *Bilder*, Hertz paved the way for the underdetermination of theoretical models – and hence for pluralism, as the thesis that more than one model is often available for any phenomena, effect, or process of interest. And in the insistence that the logical necessity in a *Bild* is distinct from the natural necessity in the phenomena pictured or in their represented causes, he opened up models to the normative practices that sanction the rules of reasoning within *Bilder*, beyond those of logical consequence or necessity. Hertz's 'conformity' appears similar in this regard to the cognate notion of 'conformation' in Helen Longino's celebrated *The Fate of Knowledge* (Longino 2001). Both notions are attempts to set a lower bar for scientific representation, thus widening its scope and generating room for underdetermination and genuine pluralism. Moreover, they both seek to do this by grounding the activity of modelling in our socially sanctioned surrogate inferential practices, thus placing greater emphasis on the communal sets of norms required to functionally set and maintain representations. Yet, 'conformation' is not 'conformity'. According to Longino (2001, 117) 'conformation' is "a general term for a family of epistemological success concepts, including truth, but also isomorphism, homomorphism, similarity, fit, alignment and other such notions". In terms of the recent debates over representation, Longino advances a general noun for the variety of conditions of accuracy or adequacy of scientific representation, not the conditions for representation *per se*. By contrast, I shall argue, Hertz's 'conformity', like Maxwell's analogy, is a minimal requirement on the conceptually prior obtaining of representation, however erroneous, false, or inaccurate.

6. The Philosophical Reception of the Modelling Attitude

The modelling attitude in science reached a high peak at the turn of the century, as signalled by Boltzmann's entry in the Encyclopaedia Britannica (Boltzmann 1902). It is a *fin-de-siècle* development that changes the character of scientific work and inquiry, and it continues to the present day. Whereas modern science had taken inspiration from the ancients to base indubitable knowledge upon the twin sources of demonstrative proof and empirical observation, the modelling attitude adds a third prominent layer involving the construction of figurative, idealised, fictional, or artefactual scenarios within scientific models. In practice, models often mediate between the lofty realms of high explanatory theory, on the one hand, and low-level renditions and records of data and phenomena, on the other (Morgan and Morrison 1999). As such, models continue to take place of pride in scientific work throughout the natural and social sciences – including the physical, chemical, earth, and life sciences, as well as in economics, psychology, or sociology.

Yet, the fortunes of the modelling attitude in the philosophy of science and amongst philosophers have been varied, experiencing ups and downs, and always subject to a measure of controversy. The object of some fierce criticism in the work of Pierre Duhem (1861–1916), the modelling attitude nonetheless experienced much philosophical attention and influence in the early decades of the twentieth century, in the wake of formidable endorsements by the likes of Boltzmann, Norman Campbell (1880–1949), Henri Poincaré (1854–1912), and Hans Vaihinger (1852–1933). However, with the ascent of logical positivism, particularly its North American version from the 1930s onwards, the modelling attitude went into a period of relative philosophical decline. There was for many years scant regard for modelling generally amongst philosophers, and a return to the dismissive cautionary warnings so acutely voiced by Pierre Duhem (Bailer-Jones 1999). A renaissance of philosophical interest began in the 1960s, and the modelling attitude as a philosophical object of inquiry slowly surged back in the wake of pioneering work by authors such as Max Black (1909–1988), Mary Hesse (1924–2016), and Stephen Toulmin (1922–2009). The last years of the twentieth century finally saw the modelling attitude gain centre stage in the philosophy of science once more, with the publication of the celebrated *Models as Mediators* collected volume (Morgan and Morrison 1999), signalling the start of an entire movement that endures to the present day. Philosophical discussions of the nature, role, and practice of modelling are now very prominent and are an absolutely central piece in contemporary philosophy of science, as is shown by even a cursory look at the major philosophy of science journals and publishing houses.

The most striking episode in this remarkable history (gracefully told in Bailer-Jones 1999) is perhaps that unusual, slow, and gradual upsurge in interest in models during the 1960s. Where did authors like Max Black and Mary Hesse gain inspiration from? Not entirely surprisingly, they were mostly inspired by the originators of the modelling attitude, by Hertz and Boltzmann, and, most prominently, by James Clerk Maxwell. Black and Hesse, in particular, both went back to Maxwell to the point of restoring the focus on the sort of analogical thinking practised by Maxwell.[8] ‘Analogy’ as a form of reasoning thus took the stage again, with ‘model’ consigned to the secondary role of its main product.

Black focused on analogies that turn fully into metaphors, which he argued required a realist reading distinct from Maxwell’s early typically Scottish attitude. As he writes (Black 1962, 228): “One approach uses a detached comparison reminiscent of simile and argument from analogy; the other requires an identification typical of metaphor”. The nature of metaphor is debated to this day, and its application to science remains controversial (see Suárez 2024, chap. 3, for an assessment). By contrast, Hesse’s nuanced analysis of analogical reasoning caught on quickly and is widely regarded to be central to any understanding of modelling. It informs the sort of philosophy that focuses on scientists’ inferential processes and practices at the expense of just analysing their product in ready-made models. In her highly influential *Models and Analogies in Science* (Hesse 1966), Hesse distinguished between three parts in any analogy or model: the positive, negative, and neutral analogies. The first includes those properties and relations shared between the source and the target; the second, those properties denied in the target; the third, those properties about which it is unknown whether they are shared between the source and target. She also helpfully distinguished vertical and horizontal relations in analogical thinking, thus emphasising the fact that model sources are dynamic structured entities endowed with parts and often dynamically evolving in time (see Bartha 2019 for further development). The vertical relations thus capture some causal principles at work. As Hesse puts it (Hesse 1966, 87; quoted in Bartha 2019, 28):

> The vertical relations in the model [source] are causal relations in some acceptable scientific sense, where there are no compelling a priori reasons for denying that causal relations of the same kind may hold between terms of the explanandum [target].

This is exactly in line with Hertz’s ‘conformity’ when suitably extended to capture all kinds of dynamic relations within the model source that may not be ruled out to have correlates in the target. Whether it actually corresponds to existing causal relations in nature is rather a question for the further Hertzian ‘correctness’ of the model.

7. Lessons for Contemporary Debates

The modelling attitude has Scottish and Cantabrigian origins in Victorian science and is deeply enmeshed in James Clerk Maxwell's work and thought. Yet, it developed most firmly in Berlin, Bonn, and Vienna, as the German-speaking *Bildtheorie* took hold. This key development in the emergence of a modelling attitude characterises much of the twentieth-century science. In the proficient hands of Maxwell, Hertz, and Boltzmann, the modelling attitude gained weight and developed into a formidably precise tool for mathematical and quantitative prediction and understanding. Nevertheless, Maxwell's insights regarding analogy (especially his apt metaphor of a model as a 'conductor' of surrogative reasoning) are deeply embedded in *Bildtheorie*'s conception. The outlines of a twofold conception of scientific representation emerged around two minimal conditions of conformity and informational gain, which every scientific model minimally complies with. These two requirements, as they appear in Hertz's and Boltzmann's work, lead naturally to a deflationary, functionalist, and pragmatist conception of representation.

The dominant accounts of representation in recent literature fall into one of two kinds: substantive and deflationary. The ostensive thought in a substantive account is that every case of representation is the instantiation of a particular type of relation between what we may call the representational source and its target. Thus, there is a substantive relation r of type R, $\{r \in R\}$, such that for any pair of objects or systems {x, y}, x is the source S, and y is the target T of a representation if and only if x and y stand in that relation: r (x, y). It is important to get the order of the quantifiers right in this expression: $\exists r \in R : \forall \{x,y\} : \left(S(x)\ \&\ T\ (y)\right) \leftrightarrow r(x, y)$. That is, the quantifier that determines the domain of the universal substantive relation of representation ranges over all source–target pairs. In other words, a substantive account of representation assumes that a certain type of relation (similarity, isomorphism, or some variety thereof) is invariably instantiated in every case of representation by models in science. Model building is then essentially all about finding out that relation as it applies to each {source, target} pair. The Victorian models of the aether, for example, are attempts to characterise the main properties of the aether through the similarities or isomorphisms that the aether (or its 'structure', whatever that may mean) holds to the mechanical models advanced to represent it, such as the vortex model. If there is no substantive relation to speak of, or none that actually holds, then there is no actual representation. Since the aether is nowadays not a recognised real entity, it seems to follow that Maxwell's model was never a representation in the first place. This seems farfetched to say the least and does poor justice to the historical record, which does not contain any indication that the model worked as anything other than a model and invited the sorts of inferences in practice that any model would. A metaphysical distinction without any practical consequence is arguably, on a pragmatist maxim at least, an idle posit lacking any content.

Substantive accounts of representation suffer from additional problems, canvassed thoroughly in the literature (including Suárez 2003; 2010; 2024). These need not detain us here, though. The historical observation above regarding the representational use of Maxwell's vortex model already ought to prompt a search for an alternative account of representation, one that stays resolutely close to the practice, while avoiding reifying the diversity of representational means and relations into any essential constitutive element in all scientific representations. These accounts are deflationary because they skip any substantive constitutive relation. Thus, another way to characterise the difference is that in a deflationary pragmatic account, the quantifiers appear inverted relative to the statement of a substantive account. Hence, there is in fact no constitutive relation that universally applies to all representations. Rather, for all {x, y} pairs where x is the representational source, S(x), and y is the representational target, T (y), there may be some functional relation $\{r \in R\}$ that applies to that {source, target} pair: $\forall\{x,y\}:(S(x)\ \&\ T(y)) \leftrightarrow \exists r \in R: r(x, y)$. Here, the quantifier ranges over all the various relations that instantiate representations; it merely affirms that there is one such relation for every source–target pair. Since the relation is merely a function and the set may contain the null relation, this definition, significantly, does not require all representational sources to have targets. Nor does it require that all those representational sources that do have targets be related to them via the same universally applicable relation. Thus, Maxwell's vortices and idle-wheel model is a representation of the aether, properly speaking, even if it lacks a target. And if we were to insist that Maxwell's model represents not the aether but properties of the electromagnetic field (such as the displacement current), it would not need to be related to it by means of the same type of relation as, say, Maxwell's equations hold to the electromagnetic field. The former ones may be related by similarity, while the latter ones are by convention, or through a statement of some structural morphism in their phase spaces.

There are a number of deflationary accounts in the recent literature, including RIG Hughes' DDI model (Hughes 1998), the artefactual approach (Knuuttila 2011; Carrillo and Knuuttila 2022), and a variety of inferential approaches (Kuorikoski and Ylikoski 2015; De Donato and Zamora-Bonilla 2012; Khalifa et al, 2022). They all have considerable merits and are apt in confronting a large variety of modelling cases. The original inferential conception [inf] (Suárez 2004; 2010; 2024) has the additional virtue to accord with the history of the modelling attitude reviewed in this chapter. The only two necessary conditions on representation, according to [inf], are what I refer to as the 'representational force' of a source, and its 'inferential capacities' with respect to the (real or fictitious) target. Each of these conditions describes, properly speaking, an aspect of the normative practice of reasoning by analogy and is not to be conceived as a relation in any metaphysical sense. Thus [inf] is anticipated by the twofold requirements adumbrated by Hertz and Boltzmann in the wake of Maxwell's innovations: Hertz's conformity requirement anticipates [inf]'s 'representational force', while Boltzmann's information requirement informs [inf]'s 'inferential capacities'.

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Notes

1 Warwick (2003) is an unsurpassed account of the Cambridge tripos system in the nineteenth century, while Buchwald (1985) and Darrigol (2000) are key historiographical references.

2 The literature on Helmholtz is large, and the account that follows leans heavily on Darrigol (2000), Eckert (2006), Patton (2010), as well as the superb essays in Cahan (1993). In addition, Hatfield (1991), Patton (2009), and Schiemann (1998) are insightful accounts of Helmholtz's work on perception and his 'sign theory'.

3 This is curiously in contrast with the sorts of pedagogical resistance that field theories encountered initially precisely in Cambridge, where they were first adumbrated – see Warwick (2003, 306–56).

4 There are essentially two kinds of rules, referred to in Suárez (2024) as horizontal and vertical rules of inference, which mirror Mary Hesse's (1966) similar distinctions reviewed later in the chapter.

5 For Hertz's views regarding the underdetermination of ontology, see the essays in Baird et al. (1998). For Boltzmann's epistemology, see Blackmore (1995) and de Regt (1999, 2005).

6 Hertz is uncharacteristically not entirely clear in his presentation of the relation between correctness and conformity. I follow the reconstruction in (Suárez, 2024, pp. 38–41).

7 Boltzmann's entry on 'models' in the Encyclopaedia Britannica in 1902 is also climatic for the *Bildtheorie*, but it had less of an impact on the public and the modelling community in the German-speaking world.

8 A revival of interest in Aristotelian analogy at Cambridge may have been involved too – Lloyd's seminal *Polarity and Analogy* (Lloyd, 1966) was published in the same year as the revised version of Hesse's book.

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