THE AMPLE MODELLING MIND

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Scientific Models in Philosophy of Science
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> *Out, out, brief candle.*
> *Life's but a walking shadow, a poor player,*
> *That struts and frets his hour upon the stage,*
> *And then is heard no more.*
> (Macbeth, 5.5)

Pierre Duhem's Two Minds

Duhem famously distinguished two 'minds': the deep but narrow mind of the French, and the shallow but ample mind of the English. [1] The French mind is strong, precise, axiomatic and deductive; it neither dwells on, nor tolerates, exception. In explaining a natural phenomenon, it develops an axiomatic theory that allows for its logical deduction. It is a vigorous 'manly' mind. (Duhem did not use gender terms, but the connotations are apparent.) By contrast, the English mind is quick, but weak and playful; it is given to consider the exceptional character in everything, and it often falls into contradiction. It can describe a great many happenings at once, but it is unable to hold firmly onto any underlying logical principles with any strength. Any fancy assortment of mechanical models and half-baked metaphors that just about fits the phenomenon will satisfy the weak English mind.

Duhem had a clear sense of hierarchy. The French mind answers to the stringent dictates of the faculty of reason; the English mind by contrast is diverted by the siren songs of the lowlier faculty of the imagination. When reason reigns, imagination is rendered superfluous, and this is something that all minds (French or English) can recognise as "an innate feeling of ours which we cannot justify" (p. 102). An English mind cannot be changed ("do not compel the English to think in the French manner", p. 99), but it can certainly be put it in its place. The English mind only gets a role in science when the French mind falters, so it should go about its business with the appropriate sense of constriction. It knows itself an usurper, having an ephemeral part in the play, owing its time entirely to the unforeseen and unpredictable absence of the leading actor from the stage: "Those very physicists who have developed theories whose various parts cannot be fitted together […] have only done so reluctantly and with regret". (p. 102). Once reason makes its luminous return, imagination (the mere shadow, or understudy, of reason) is heard or seen no more. Brief is the candle.

Duhem seemed to be in no doubt that his own mind was French, and commentators have often tried to downplay what they perceive as the embarrassingly nationalistic tone in his comments, or to otherwise take the sting out of them. For instance, the Stanford Encyclopedia of Philosophy article devoted to Duhem states that his "categories are analytic ones, not mere epithets to be used rhetorically against his adversaries". In other words it assumes that Duhem's distinctions are drawn in the same 'esprit géométrique' that characterises the French mind. [2] Certainly the definitions are not meant to apply to nationality by birth (nor gender by birth). Some of Duhem's main exemplars of the English mind are Napoleon Bonaparte and Honoré de Balzac, while the French mind is apparently well exemplified in Isaac Newton. But these particular choices seem to express Duhem's idiosyncratic political and intellectual conservatism. He disliked Bonaparte's strategic thinking and Balzac's detailed descriptions of custom as much as he was attracted to Pascal's abstract principles and dictums. Far from being merely analytical, his categories are meant to reflect social habit and practice. They are "noticeable in every manifestation of social life" (p. 67) including, prominently, the French legal code and the English common law. The former is, according to Duhem, based upon abstract principle while the latter is a mere motley collection of contradictory local customs and habits.

In other words, the distinction between the French and English minds *was* meant to capture some of the central features of the respective cultures in their historical development: "Ample minds are to be found in every nation. But only among the English is ampleness of mind found so frequently as an endemic, traditional habit" (p. 77). Moreover Duhem perceived ethical implications that we nowadays would find chillingly familiar. At the height of British imperialism, he saw the weak English mind colonising every aspect of his own country's science and culture. The main battleground then, like now, was in education: "The evil has not only touched the texts and courses intended for future engineers. It has penetrated everywhere, propagated by the hatreds and prejudices of the multitude of people who confuse science with industry" (p. 93). To sum up, it is debatable whether Duhem's definitions are appropriate; but the use that they are put to is indisputably interested and partisan in his advocating a particular view of science.

Nor is Duhem's language devoid of metaphor or rhetoric. His main historic illustration of an English mind in the arts is William Shakespeare, whose (often female or feminine) lead roles, such as Lady Macbeth and Hamlet, display "confused, imperfect thoughts, with vague, incoherent outlines" (p. 64). By way of contrast Duhem invites us to consider the clear-headedness and conceptual precision of Pierre Corneille's (male and virile) lead characters, Rodrigue and Auguste: [3] "what a perfect order there is in their discussion" (p. 64) Similarly, René Descartes' "very precise and rigorous method" is superior to Francis Bacon's "childishly simple division" (p. 66). Within the science of his time – Duhem's proper target - the fiercest diatribes are reserved for James Clerk Maxwell and William Thomson (Lord Kelvin). [4] Their capital sin: the introduction of models into scientific theorising, and the ensuing replacement of the lofty faculty of reason by the unconstrained exercise of the imagination. It is the imagination that allows us to picture complex systems of concrete objects endowed with multiple and often

contradictory mechanical properties – and these are abject to reason alone. Duhem disparages most strongly against Thomson's development of Maxwell's mechanical vortex model of the aether, consisting of "rigid boxes, each containing a gyrostat animated by a movement of rapid rotation around an axis fixed to the sidewalls" (p. 84). Models are lowly and deformed creatures, which owe their ephemeral existence entirely to the regretful absence of their high-level abstract theory master. They are devoid of any virtue, the sinful result of an all too human weakness of the will.

In this view the only virtue a model may possess is as an effective vehicle for the ulterior workings of theoretical reason. Maybe models lead heuristically towards better high-level theories. Yet, Duhem denies models even such a minor heuristic value. Instead he claims that all discoveries supposedly provided for by models have in fact been the fruit of reason acting upon the underlying theories. Models are mere ornament with the only function to satisfy the desire for visual representations characteristic of the weak English mind. They are genuinely the understudies of theories – and just as ephemeral in their existence.

A History of Philosophy of Science

The writings of Pierre Duhem are the starting point in the history of the philosophy of science recounted in *Scientific Models in Philosophy of Science*. The book takes the modelling tradition to essentially begin with Duhem's adversaries, and to run all the way to the present day. It is essentially a book in the history of the philosophy of science, and even those sections that are more explicitly directed at strictly philosophical issues never completely lose sight of its essentially historical mission and dimension. The author, Daniela Bailer-Jones, was born in Germany in 1969, educated briefly at Freiburg and then at Cambridge. She earned her first degree in Astrophysics under the guidance of Malcolm Longair, and she developed an interest in scientific modelling, which has of course a long tradition in Cambridge. Mary Hesse, in particular, was to a large extent responsible for the brief flourishing of the philosophical interest on models in the late 1960's, and her *Models and Analogies in Science* (1963/6) continues to be a classic reference in the field. Bailer-Jones felt proudly part of the Cambridge modelling tradition, and in 1993 she enrolled as a graduate student in its History and Philosophy of Science Department.

The focus on modelling had in the meantime somehow shifted elsewhere, and in particular the LSE had just started running a seminar and research project on models, which eventually gave rise to the 'mediating models' movement. [5] This movement aimed to liberate models from what it perceived as the shackles of theory imposed by the then dominant conceptions of scientific theory. As a result it promoted a return to, and reassessment of, the modelling tradition of the 1960's and 1970's. So, for a few years Bailer-Jones became a regular commuter on the Cambridge to London train. Her PhD Thesis was defended in 1997, and *Scientific Models in the Philosophy of Science* is a version of it, developing more fully some of its most important notions, while taking in and responding to later developments.

Bailer-Jones' untimely death in 2006 unfortunately cut short its final elaboration, and as a result the book is a little incomplete in places.

There are eight substantial chapters entitled "Scientific Models", "Mechanical Models", "Analogy", "Theories", "Paradigms and Metaphors", "The Semantic View and the Study of Scientific Practice", "Phenomena, Data and Models", "Representation". There is also a preface by Coryn Bailer-Jones and Peter Machamer, and a concluding chapter that summarises the contents of the book. The first chapter is essentially descriptive and reviews Bailer-Jones's fieldwork in interviewing a range of scientists regarding their views on models. On the whole her findings are roughly in agreement with the features that Duhem ascribes to his 19th century modelling opponents – thus vindicating his fears regarding the 'colonisation of science by the English mind'. Modellers are often aware of the tentative, incomplete and even fictitious nature of their models – but will find them acceptable whether or not they regard them as pointers towards more complete and accurate theoretical accounts. The modelling spirit seems to be everywhere in science nowadays.

In the remaining chapters in the book, alongside the more historically descriptive material, substantial philosophical theses are debated. In particular chapters 2, 5, 7 and 8 contain Bailer-Jones' own reflections and reactions to current philosophical debate. She and I agreed on many of the larger issues – in Duhemian terms, which we both enjoyed, we were aware of the 'Englishness' of our own minds. But there are also disagreements, and differences in emphasis here and there. Even without the benefit of her responses, it seems to me that the best homage one can pay her is to continue the conversation by focusing on such disagreements and differences.

For instance in the second chapter Bailer-Jones reviews the mechanical approach to models defended by William Thomson and tries to develop it further by expanding on the notion of mechanism. The main problem here is that for 19th - century physicists the notion of mechanism is linked to classical mechanics, so Bailer-Jones seeks for an extension of the concept that is appropriate to 20th century science. She reviews a few well-known attempts in the literature to define a more general concept of mechanism, namely those due to Glennan (1996), and to Machamer, Darden and Craver (2000). She finds difficulties with both (particularly with the latter's focus on "activities") but she does not provide an alternative conception. Instead she acknowledges in the concluding chapter that "this is an area that most definitely requires further study" (p. 207). The development of the appropriate notion of mechanism is maybe the main task that Bailer-Jones felt she did not have enough time to complete. But is it really such an important omission? Certainly Thomson and Maxwell sought mechanical models in their explanations, but it is not clear that the modelling tradition as a whole is per se committed to them. Rather what seems of the essence for the modelling tradition is the use of analogies, metaphors, and fictions in model-based science.

Analogies and Metaphors

The next three chapters focus precisely on the essential role of analogies and metaphors in models, and their comparative absence in theories. These chapters are central to the book's endeavour, just as their ancestors were central to Bailer-Jones' PhD thesis project. Chapter 3, entitled "Analogy", is an essentially historical chapter reviewing some uses and theories of analogical thinking in science, beginning with the writings of James Clerk Maxwell. This is followed by a discussion of two philosophical theories of analogy in science, developed by Norman Campbell and Mary Hesse. In Campbell's view a theory is divided into a hypothesis and what he notoriously called a 'dictionary' (1920, particularly chapter 6). The hypothesis contains abstract propositions that are implicitly defined by the axioms of the theory, while the dictionary translates some of the terms in the theory into some previously understood vocabulary. In addition a proper and mature scientific theory displays analogies with what Campbell calls 'empirical laws'; [6] and these analogies enforce the main translation rules in the dictionary. In modern parlance, it is tempting to conclude that the dictionary induces what the logical empiricists called "bridge principles" connecting the theoretical and observational vocabularies. And indeed Bailer-Jones follows Mellor (1968) in supposing that Campbell's use of analogy is intended to close the gap between theory and observation. [7]

By contrast, Mary Hesse (1963/6) understood analogy as a relation between model-objects and the object systems modelled by them. Let us refer to the object that constitutes the model as the source, and the one that constitutes the system as the target. Then Hesse claims the positive analogy is whatever they share in common; the negative analogy refers to what is distinct; while the neutral analogy comprises all those properties of the source for which it is not known whether or not they obtain in the target. For example billiard balls are like gas molecules in some respects: they are individual objects that may collide virtually elastically. There are some respects in which billiard balls are definitely not like gas molecules: they are coloured all over and have a number printed on them. And then there are those aspects for which it is not known whether or not they are like gas molecules, such as dynamical behaviour, or inertia. The neutral analogy is essential to scientific research since it provides a heuristic for it: It is by exploring the neutral analogy that we can aim at discovering new aspects of targets on the basis of the properties of the sources in our models.

Chapter 4 deals with theories and aims to understand why for so long models were outside the focus of philosophical attention. Bailer-Jones' answer is that for most of the 20th century the dominant conception of scientific knowledge was that of the logical empiricists, according to whom theories articulate and organise all scientific knowledge. Thus models become otiose. The history of the axiomatic method in the formulation of theories is briefly reviewed from Hilbert onwards. It is in particular noted that the introduction of the distinction between the context of discovery and the context of justification conspired to place models in a marginal position by placing them outside the purview of the rational reconstruction of knowledge. The chapter ends with a brief consideration of those who in the British context continued to uphold the significance of modelling for science in the 1950's and 60's. The writings of Campbell (1920), Braithwaite (1953/1968), Harré (1960), Hutten (1954), Hesse (1963/6) stand out in this

respect. These authors disagreed among themselves on important matters of detail, but they all agreed on the heuristic importance of models in the development of theories. Their contributions are interpreted as visionary. Yet, it is hard for me to see how exactly their view, as explained by Bailer-Jones, differs significantly from that of their predecessors in logical empiricism. According to Bailer-Jones, they all seem to regard models as belonging to the context of discovery. It is clear that the modellers have a wider conception of the role of models in scientific practice than their logical empiricist predecessors, but that is just because they have a wider conception of the context of discovery.

Chapter 5 is interesting in raising the very important issue of metaphor. The idea that models are either a variety of, or otherwise intimately linked to metaphor is of course very old and runs through different traditions. But how precisely are models and metaphors related? And how, in turn, are these related to analogies? Bailer-Jones' basic view is that "analogy deals with similar attributes, relations, or processes in different domains; exploited in models and highlighted by metaphors" (p. 121). This is slightly vague but it boils down to the idea that analogies are monadic or relational properties of things; while models and metaphors are descriptions of things. The object of these descriptions is precisely to highlight certain analogies for the usual scientific purposes of prediction, explanation and control.

Nothing so far distinguishes models from metaphors – they seem to have the same descriptive function. Yet, Bailer-Jones does not seem inclined to identify them outright. Undoubtedly this is a difficult issue which calls for much detailed discussion. The contemporary literature on metaphor is large and begins with the seminal contributions by Max Black (1954, 1962). Black argued for the importance of metaphorical thinking across the board and particularly in science. His 'interaction' view is in turn a philosophical development of Richards' (1936) account of metaphor. Richards was critical of static accounts of metaphor that understand metaphor as a mere comparison between a 'primary' and a 'secondary' subject. Metaphor instead has a fundamental dynamical aspect in bringing entire new regimes of significance and meaning into being. Hence Macbeth's "Life's but a walking shadow" does not just highlight some features that are shared between life and dramatic production. It reveals entirely new aspects of life and drama that were opaque ahead of the consideration of the metaphor. Life reveals itself sadly ineffectual and futile in ways that we would not be able to express by means of any literal use of language. Metaphor is a cognitive achievement whereby 'life' and 'shadow' take on new meanings. In the appreciation of the metaphor we learn something new about both subjects, as well as about our relation to both.

Black followed up Richards' view by criticising in analytical detail the alternative 'comparison' views. It is obvious that metaphors like metonyms cannot be reduced to statements of similarity. For a metaphor to work the source and the target have to be remarkably dissimilar – and the lack of similarity is at the heart of what makes the statement a metaphor and not a mere simile. To return to Macbeth: Life is patently nothing like a shadow or an understudy. That they are unlike each other is precisely what makes the metaphor formidably powerful. More generally, the metaphorical assertion that 'x is y' implies the literal assertion that 'x is dissimilar

from y'. Yet, there must be something about y that makes it relevant to our gaining a better understanding of x, and there lies the very cognitive content of the metaphor. [8] Hence 'x is y' metaphorically asserted implies both that x is dissimilar from y and that it is in some way like y. According to Black and Richards this likeness is not a conventional similarity, in that it is not there waiting to be discovered, but is brought into existence by the metaphor itself.

It would seem to follow from this that models are metaphors in some but not all respects. An equation on a piece of paper, a graph, a toy figure, may have very little in common with the physical processes and objects that they may represent. The dissimilarity is there in the comparison between some of the fictional entities described in models and their real world counterparts. Yet if they are good models, they must also be informative in specific ways. We would be hard pressed to say that in those cases what makes the model appropriate is the cognitive content of the related metaphor, if there is any. And we would be even more hard-pressed, I think, to say that in these cases the likenesses are brought into existence by the model. Perhaps this is why Bailer-Jones did not accept the claim that models are metaphors – not literally. She instead writes that the claim is *itself a metaphor* – a 'second order' one. The account of metaphor that I have just given makes Bailer-Jones's claim counterintuitive – for there is nothing surprising about models that metaphors reveal. The discussion is unfortunately imprecise at this point, but I suspect this indicates that Bailer-Jones did not fully accept the interactive view of metaphor. [9] What view of metaphor exactly she wished to defend is unclear, but we can hardly blame her – this is an exceedingly difficult and unexplored topic, and she deserves credit for putting it on the table again.

Theories, Models and Representations

Chapter 6 reviews the literature on the semantic conception of theories, including criticisms raised by the 'mediating models' school (Morgan and Morrison (ed.), 1999). There isn't anything particularly new here, and Bailer-Jones seems to go along with the critics in rejecting the view that theory is just a bunch of models. She raises some interesting considerations regarding the independence of models from data and phenomena and elaborates on the view that theories are abstract and general while models are concrete and tailored to the specifics of the case at hand (pp. 144-148). I found myself in sympathy although I missed a greater emphasis upon the fact that these distinctions can only be made 'in use': they are essentially pragmatic.

Chapter 7 turns to Bogen and Woodward's (1988, 1989) well-known distinction between data and phenomena. The chapter is a nice and concise review of different claims made in relation with this distinction, or similar ones by philosophers such as Suppes (1962), Hacking (1983), Kroes (1995), and others. It relates to models in the sense that all these authors roughly accept that phenomena must be described in a model before they can be accounted for theoretically.

Chapter 8 addresses the issue of representation, which has been the object of considerable attention in recent years. Resemblance views of representation were criticised by Goodman (1976): he insisted that representational sources and targets do not always resemble each other. Instead Goodman defended an understanding of representation as denotation. Recent theories of scientific representation as similarity or isomorphism have come under similar criticism for failing to account for misrepresentation. Models are often inaccurate and imperfect descriptions but this does not in any way imply that they cannot serve their functions as representations of their targets. Hence a functional understanding of representation does not sit well with similarity or isomorphism accounts.

I have developed some of these arguments as part of a defence of the inferential conception (Suárez, 2003). On my view a model may or not have a truth-value but in any case it serves as a generator of surrogative inferences, [10] allowing us to infer claims about the target. The mode of generation is open-ended – in the sense that a model may just act as prop for the right kind of inference in the appropriate context without it being itself constituted by a fixed set of propositions. This may be because the propositional description of the model is not unique or because such a decision is lacking altogether. In those cases, which are rather typical, the relationship between the model and the conclusions of the inference that it promotes is certainly not one of logical entailment. Bailer-Jones's account also differs from the similarity and isomorphism accounts, which she criticises for very similar reasons. But she instead appeals to the idea that models, which in her view are not truth-valued, nonetheless *entail* propositions, which must be true or false (pp. 185-193). This is one of the aspects of her work that seemed so promising but appears a little incomplete. For suppose that a non-truth valued model M entails a truth-valued proposition P, and suppose that P turns out e.g. false. If the notion of entailment involved is anything like logical entailment, it follows that M is false and hence –contrary to the hypothesis – that it is truth-valued. And what other notion of entailment is there that is sufficiently restrictive to do justice to the fact that scientific models are informative?

The comparison with the inferential view is once again instructive. Even in those rare cases where it makes sense to speak of a model as constituted by a fixed set of propositions, the inferential conception makes no assumptions regarding the relationship between M and P other than that M allows cognitively functional and informed agents to infer P – and inference of course may be allowed or promoted by forms of reasoning that go beyond logical consequence (Suárez, 2004).

Another interesting claim in chapter 8 is the role that intention plays in representation. The sort of mental models analysed by cognitive scientists are found relevant both for the study of analogy and the study of modelling practice in general (see particularly pp. 59-68). Representation is consequentially linked to the intentions of agents (p. 195). I disagree here too, and the inferential conception instead links representation to the representational practices of communities of modellers, so the key concept is intended use rather than intention per se.

The chapter, and the book, ends with Bailer-Jones' request for an answer to what she calls the 'burning question' (p. 197): "How is it that there is something about

the model that allows us to demonstrate something that then, after appropriate interpretation, becomes applicable to and insightful about real-world phenomena?" Her own answer is that "there is something that the phenomenon and the model share that allows us to treat them in parallel". This is a question worthy of a highly passionate intellect in search of a deep understanding of science. Nonetheless one may disagree that the question is a burning one. From the deflationary point of view of the inferential conception, it is even doubtful that the explanatory request is well placed in the first place. This is why I could never bring myself to share Bailer-Jones' motivation; for her part she clearly found deflationism unsatisfying. She wanted to go beyond the surface features of representation towards a more substantial account. Deflationists doubt that such an account is forthcoming, but that just makes Bailer-Jones's determination and high spirits in pursuing it even more admirable and awesome in our mind.

Daniela Bailer-Jones' life was brief; she died at her prime. But her philosophical thought lives on, and her voice is heard loud and clear in this excellent posthumous book.

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[1] In Chapter 5 of (1914/54), entitled "Abstract Theories and Mechanical Models". All references are to the 1954 Princeton edition.

[2] See also the introduction to Duhem's book by Louis de Broglie (p. xi). As it should be evident to the reader, this part of the essay review is an attempt to contest this judgment. What I find most interesting about Duhem's distinction is precisely the cultural rhetoric – which displays a very 'English' turn of mind.

[3] Rodigue in *Le Cid* is Rodrigo Díaz de Vivar, the legendary Spanish mediaeval commander of the early Christian crusades against the Moors. Auguste in *Cinna ou la Clémence d'Auguste* is Octavius Augustus, first Emperor of Rome.
[4] Maxwell was a Scot, born and bred in Edinburgh; and Kelvin was so named after the Glasgow River that he lived close to during the last 50 years of his life. In a provocative synecdoche (or is it a 'confused thought with vague outlines'?), Duhem seems to be referring to all British people as "the English".
[5] The project culminated in Morgan and Morrison, ed., (1999).
[6] The main example described by Campbell is the dynamical theory of gases, where the main hypothesis displays an analogy with the dynamics of a system of elastic particles (1920, pp. 126-128).
[7] This interpretation of Campbell seems debatable, since Campbell's formal analogies do not take the form of definitions or mixed postulates. Moreover, he is clear that the connection with experience is provided for by the dictionary itself, and that the analogy is a pervasive and permanent feature of the theory, which continues to hold even after the dictionary gets firmly established. Mellor also argues that Duhem and Campbell did not actually differ substantially on the role of models, since they both only meant to emphasise the heuristic value of analogies in the development of theories. In this view their quarrel was just basically about terminology, and Mellor claims that Duhem was misunderstood. I disagree with this. I believe that, on the contrary, it is Mellor's interpretation of Duhem that misses the point. It both undervalues and ignores Duhem's very considerable rhetorical powers.
[8] Pace Davidson (1978) of course; since on his view metaphor lacks any cognitive content. (According to Davidson 'x is y' metaphorically stated says nothing over and above 'x is y' literally stated – the only purpose of the metaphor is to call attention to an unsuspected but pre-existent similarity).
[9] Black (1979) asserts roughly the converse: all metaphors sit at the tip of a model – thus essentially inverting the direction of analysis. This seems to me the right way to go, not only in being a natural consequence of the interaction view, but also because it sits nicely with the view that representational models are fundamentally conducive to inference. There is unfortunately not enough space in this review to explore the idea fully.
[10] The term "surrogative reasoning" was introduced by Swoyer (1991) and is by now a term of art. It is used to refer to the form of reasoning about some *x* that is grounded on considering a distinct *y* that, at least for the purposes of the reasoning at hand, is taken to stand for *x*. This is the form of reasoning that underwrites analogical and other forms of model-based reasoning. While Swoyer was perhaps the first to understand the importance of surrogative reasoning for scientific representation, he did not think of the subclass of surrogative inference as the basic hallmark of representation, and he did not defend a deflationary view.