**Models and Meaning Change: A Brief Introduction to the Work of Mary Hesse**

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Mary Hesse was one of the most significant figures in 20th Century history and philosophy of science, not only because of her academic research, but also for the role she played in further developing and enhancing the field at the institutional level (for a useful biography see ‘Website in Honor of Mary Hesse’: http://www.collodel.org/hesse/#). She was instrumental in the formation of the Division of History and Philosophy of Science at the University of Leeds, where she was a lecturer in mathematics, before she moved to University College, London and from there to the Dept. of History and Philosophy of Science at Cambridge where she was eventually appointed to a Professorship. She was not only Vice-President of the *British Society for the History of Science* and President of the *Philosophy of Science Association*, as well as being elected to the *British Academy*, but more importantly – as far as we are concerned at least! – was Editor of the *British Journal for the Philosophy of Science* during a time of considerable change for the field as a whole.

 Here we have chosen a selection of her papers, together with some notable responses, from the pages of the *British Journal for Philosophy of Science*. We begin with her first ever paper in the philosophy of science, published in the second issue of the journal while she was still in the mathematics department at Leeds. She begins with a critique of Bridgman’s account of the meaning of physical concepts in terms of operational definitions, using the example of Dirac’s formulation of quantum mechanics to argue that such an account cannot capture the nature and role of such concepts in modern physics. In particular she insists that despite the significance given to observables in quantum mechanics, these cannot be straightforwardly related to the kinds of measurements that are typically carried out in practice. Instead, she suggests, the meaning of concepts in quantum physics is grounded in the analogy with the relevant elements of classical physics. Thus, ‘Dirac's discussions about measurement and observability become meaningful if we realise that he has in his mind, not practically possible experimental measurements, but a highly idealized system of particles like those considered in classical dynamics’ (p. 291) – with the crucial difference of course that we cannot assume the particles to possess definite positions ands measurements when not observed. Of course, one might suggest that the role of such analogies is limited to the heuristic development of the theory and they will eventually be eliminated. However, Hesse suggests, not only would the resulting theory be unwieldy, it would also be unable to accommodate new observations, ‘… because one of the main functions of an analogy or model
is to suggest extensions of the theory by considering extensions of the analogy, since more is known about the analogy than is known about the subject matter of the theory itself’ (p. 291).

 Together with her subsequent work on models in physics (Hesse 1953), these papers laid the basis of her classic text on models and analogies in science which remains widely cited and for which she is perhaps most well-known. In her 1953 paper she draws attention to two important points regarding scientific hypotheses: first, ‘Mathematical formalisms, when used as hypotheses in the description of physical phenomena, may function like the mechanical models of an earlier stage in physics, without having in themselves any mechanical or other physical interpretation*.*’ (pp. 198-199). What Hesse meant by this is that if hypotheses are to have heuristic value they need to be capable of being understood independently of the data that can be deduced from them and a good example of this would be the famous billiard-ball model of a gas, the ‘further ramifications’ of which can be used to extend the kinetic theory of gases, thereby generating further questions that can then be answered via experiment.

 The second point is that, ‘…most physicists do not regard models as literal descriptions of nature, but as standing in a relation of analogy to nature.‘ (p.201). Here Hesse goes beyond her previous work in distinguishing two senses of analogy as used in physics: we may say that one branch of physics is analogous to another, and may thus be used as model of the latter, on the basis of identical mathematical formalisms, such as in the case of the theories of heat and electrostatics; or we may say that the billiard balls are analogous to gas molecules, in which case we have identity of mathematical structure between the model and nature, where ultimately the analogy reduces to a correspondence between numerical consequences of the model and numerical experimental results.

 The wide variation in types of models is then illustrated by drawing on the history of science in the form of a survey of 19th century aether models, with Hesse noting that all the ‘real’ work was done by the mathematical model, with the mechanical counterparts added as an afterthought, ‘…in the mistaken belief that it endowed the mathematics with a respectability it would not otherwise possess. ’ (p. 212). This then allows her to identify two sorts of characteristics of models: formal rules, such as the axioms and rules of inference of a mathematical formalism; and what she calls ‘pointers’, which can be found in the ‘haze’ of mathematical and physical associations surrounding the model and by means of which it can be further extended. An example would be the suggestion of new formal rules similar to the original set, as in the case of elasticity in the billiard-ball model, suggesting modifications to the laws of collision for gas molecules. Here we see the roots of what she was later to call the ‘neutral’ analogy and which she insisted was essential to scientific progress.

 In his wide ranging paper (Achinstein 1965), Achinstein criticizes Hesse’s identification between models and analogies noting first, that with regard to analogies between different branches of physics, some of the laws are similar or identical in such cases, but others are not; and that when it comes to models as analogies themselves, many models of systems are not analogues of those systems and neither is an analogue always a theoretical model. In defence of this last claim he deploys the example of the similarity between the laws of heat and those of electrostatics but of course, Hesse would insist that this falls under her first sense of analogy, not the second, which is what Achinstein was criticizing. And he does acknowledge, in a footnote (p. 108), that in her 1963 book she refines her account, drawing the distinction between those models that are analogies and those that might more broadly be called ‘theoretical’. The paper concludes with another review of models of the aether, which is taken to support the overall claim that although some might be described as analogies, others were not and are better understood as ‘theoretical’ models, whereas still others have an intermediate nature.

 Granted that some of the details of Hesse’s approach required further refinement (for a review of her 1963 book, see Ackermann 1965), nevertheless her consideration of certain kinds of models as analogies was hugely influential and it can still be found referred to in works from Kroes 1989, who cites her book as ‘one of the best studies in the field’, to Weisberg 2007 and Dardashti, Thébault and Winsberg 2015, just to give a few examples from the pages of the *BJPS*; for an example of ‘failure to cite Hesse’ used as a criticism, see Bailer-Jones 2004).

* Hesse also made significant contributions to the developing critique of what came to be called the ‘Received View’ of theories, which took them to be sets of logico-linguistic statements, divisible into ‘theoretical’ and ‘observational’. In her 1958 paper (Hesse 1958) she focuses on the core claim that theoretical statements and observation statements are related via some kind of ‘dictionary’ and argues that if the latter are to be understood as tests of the former, then the distinction cannot in fact be maintained. Interestingly, she begins by insisting on calling such statements ‘phenomenal’, rather than ‘observable’, to avoid confusion with the use of the latter term in quantum mechanics. Phenomenal statements, then, ‘… they do not describe what happened on a particular occasion to a particular observer but what always happens and will happen on sufficiently similar occasions to all normal observers.’ (p. 15). However, insofar as such statements are intended to be involved in tests of a theory, their meaning cannot be independent of the statements of that theory, since if they are to have ‘scientific significance’, there must be appropriate connections between them at a level of meaning higher than that of so-called ‘common sense’ or non-scientific concepts. Here she not only gives the well-known ‘bent stick in water’ example but, noting that it may be objected that this could be understood in terms that are not theory laden, she turns to a more interesting and modern example – that of a scintillation screen recording the detection of neutrinos produced in a nuclear reactor. As she argues, the mere report of such scintillations cannot be related as it stands to any relevant theory; it is only when the observation is interpreted in the language of elementary particle physics that the associated statement can count as confirming the theory.

 As she goes on to say, ‘…To interpret an experiment directly in theoretical terms so that it can be a test of the theory is always to say more than t he corresponding phenomenal statements would say, because such interpretation carries with it natural expectations about possible but so far unobserved behaviour which the scientist has to *learn*, just as the child learns the contextual overtones of ordinary language. ’ (p.20). Given this difference, the correct analogy is not that of a dictionary, translating sentences from English to French say, but that of the translation of poetry into pedestrian prose and thus Hesse suggests, evocatively, that ‘… the phenomenal description of an experiment has a relation to the scientist’s theoretical description which is similar to that between Holingshed and Shakespeare.’ (p. 21) The hierarchy that extends from phenomenal statements to the theoretical may then be likened to the gradation in degrees of imagination from prose to poetry!

 With this in hand, we can understand that it is misleading to call theoretical concepts ‘unobservable’ since all observation involved in testing theories involves some interpretation. In a claim that resonates with subsequent well-known discussions in the field, she insists that the scattering of electrons by gas atoms should be characterised as ‘observed’, although the phenomenal description ‘… would mention only white streaks in a cloud chamber.’ (p. 23). And she continues by criticizing attempts to draw a distinction between ‘direct’ and ‘indirect’ observation in this regard, arguing that such distinctions do not correspond to any important difference in the way the relevant terms function within the given theory (p. 27). Instead, she insists, we should take each kind of entity to be observed ‘… in the ways appropriate to it.’ (ibid.) Any difficulties associated with the idea of ‘unobservable entities’ are instead peculiar to quantum physics as here what is actually ‘unobservable’ is not the entity itself but the state it is in when unobserved but this feature, albeit fundamental, has no bearing on the epistemological issues she is concerned with in this work.

 Her claim that positivistic philosophy of science does not adequately accommodate the practice of science is further pursued in a number of pieces, including an illuminating review of Rom Harré’s book, *Theories and Things* (1961). Harré argues that by means of ‘ontological experiments’ the domain of things that may be said to exist can be extended from the observable to the unobservable. Underpinning such an extension is the notion of ‘family continuity’, presented as a linear sequence of partially overlapping events, taking us from, in the case of optical continuity, an okapi (!) to a virus, such that any two adjacent objects in the sequence can be observed via the same mode of observation – such as the eye, the optical microscope, the electron microscope and so on – and any one such object can be identified as the same such object via any two such modes. Harré then claims that if any one member in such a sequence exists, then any other does also, from which he concludes that okapis and viruses exist, but not electrons, the wave function or the unconscious.

 Here of course we have a foreshadowing of the later debate between the likes of Hacking, van Fraassen and others over the use of instruments such as the microscope and the observable-unobservable distinction in general. And Hesse raises some useful concerns in her review (Hesse 1962). As she points out, a plausible form of positivism will not be undermined by Harré’s argument, since family continuity does not extend across ontological classes but only shows that new objects can be placed in the same class as ‘ordinary’ physical objects by means of a series of ‘ontological experiments’, namely observations via a magnifying glass through to an electron microscope. Thus, although this might be argued to undermine that kind of positivism that rules out observation via instruments such as microscopes, it leaves untouched what she considers to be a more plausible form that maintains that only one ontological kind of thing exists, taking us from okapi, say, to those things that can be observed with instrumental aid, such as viruses.

 Furthermore, the realist may have reason to be unhappy with Harré’s argument as well, since it rules out electrons for example. Hesse finds this to be very odd, since it would seem that there is another family continuity in this case, taking us from molecules to atoms to electrons, and for Harré to deny this seems to imply no connection between electrons and observables, thereby undermining the former’s status as a kosher theoretical entity to begin with. Harré’s response is that only the effects of the electron are observed, not the thing itself. Hesse is dismissive: ‘What is this distinction between the *effects* of a thing and the thing itself? Might it not be said equally that the electron micrograph shows the *effects* of a virus, namely its deflections of fast electrons?’ (p. 238) Furthermore, it would seem that we can put electrons into the kind of sequence covered by ‘family continuity’, simply by taking into account any of the experiments by which we observe them and working our way ‘up’ to physical objects. But then as she says, we may have proved too much, since this could be done for any theoretical entity in an acceptable theory. Although she admits that Harré might have been on to something here, it clearly stands in need of further development.

 More interestingly, perhaps, is Hesse’s point that Harré conflates the issue of *what exists* with that of *what are objects*, drawing on Strawson’s definition of an individual object as that for which well defined identity conditions hold. But as she notes, such conditions arguably do not hold for electrons and those of us who care about such things will recall her later use of the ‘money in the bank’ metaphor in this context: electrons are like € in a bank account, I can say that I have €300 in my current account but I can’t go in, as it were, and point to a particular € and say, ‘that’s mine’. Indeed, she argues, fundamental particles in general ‘…are only just admitted as entities because of the short-term invariance of rest-mass, charge, and spin. ’ (p. 243). And she goes on to take a pop at Quine by insisting that it is useless to expect any light to be cast on this issue from the definition that an entity is one of the values over which variables of the theory range, ‘because it is precisely what this domain of values is that is often a matter of dispute within physics. ’ (ibid.) Indeed, she continues, the very act of axiomatising a theory in order to answer the question ‘what are the values of its variables?’ implies the adoption of a certain interpretation which in turn is equivalent to the decisions involved in answering the question ‘what are entities?’.

* This critique of the Received View continues in her pointed review of Scheffler’s book, *Science and Subjectivity* (1967). Responding to the likes of Feyerabend, Hanson and Kuhn, who he viewed as advocating a pernicious form of subjectivism, Scheffler attempted to ground scientific objectivity in a Fregean analysis of meaning. Thus, we may grant that the *sense* of a theoretical term is determined by the relevant theoretical context and so, to use a hackneyed example, the meaning of ‘mass’ differs between Newtonian and relativistic mechanics but the *reference* may remain the same, and hence we can maintain that the theory of relativity represents an advance over Newton’s theory. However, Hesse argues (1968a), sameness of reference is neither necessary nor sufficient for the comparability of two theories. It is not sufficient because scientific properties are intensional rather than extensional; and it is not necessary because two different theories may deploy different categorizations of objects – and here she gives the examples of Dalton’s and Cannizzaro’s theories of atoms – and yet we still want to say that statements of the one imply or contradict those of the other. (Hesse also went on to criticize Fine’s criterion of meaning change in her (1968b) but Leplin subsequently argued that it missed the mark (Leplin 1969)).

 The issue of meaning change crops up again in her analysis of the problem of ‘Grue’ (Hesse 1969). As we all recall, Goodman posed the following problem: consider the predicate ‘Grue’ which applies to all things examined before time *t* just in case they are green and all things after *t* just in case they are blue. The two hypotheses ‘all emeralds are green’ and ‘all emeralds are grue’ are thus supported by the same evidence before *t* but make different predictions after *t* and intuitively we prefer the former to the latter. How then to capture that intuition, captured by Goodman’s characterization of ‘green’ being more projectible than ‘grue’? Goodman himself argued for a pragmatic account, according to which the predicate ‘green’ is historically more entrenched in our language than ‘grue’. While agreeing that this is correct in principle, Hesse argues that in most actual cases, certain relevant asymmetries other than entrenchment can always be found to justify our choice of the competing predictions. First she insists that any satisfactory solution of the puzzle must adhere to the following principles: (A) ‘the language describing the present evidence must not be merely verbally different, but must yield predictions which are both genuinely different, and diff- erent in respects which the 'green' and 'grue' speakers (who will be called 'Green' and 'Grue' respectively) can explain to each other and agree to be different. ’ (p. 14); (B) ‘the problem should be shown to be soluble in its *strongest* form, and that since the solution is to be sought by finding asymmetries in the predictions of Green and Grue, it should not be set up in such a way as to introduce needless asymmetries into the definition or interpretation of the problematic predicates.’ (p. 15)

 Considering what ‘Green’ and ‘Grue’ understand each other to be asserting in their respective predictions, principle (B) rules out such obvious suggestions as ‘Green understands Grue to be predicting that emeralds will change colour at time *t*’. In that case, symmetry would clearly be violated but that can’t be a viable solution. But if both Green and Grue are committed to emeralds remaining the same colour, how can principle (A) be satisfied? The asymmetry must be sought elsewhere, in some ‘real difference’ between Green and Grue. One option would be to shift away from what objects look like and focus on some ‘objective’ property such as wavelength. Now of course, as Hesse shows by means of a nice piece of dialogue between Green and Grue, Grue can always respond to Green’s insistence that his prediction does not predict a change in wavelength while Grue’s does and hence his (Green’s) is simpler, thus establishing an asymmetry, by introducing further ‘grue-like’ predicates at this more fundamental level in terms of which the symmetry is restored. That is, symmetry can be restored by substantially modifying the relevant theory held by Green, but that, Hesse maintains, would ultimately involve the construction of a new theory entirely – one that is entirely different from currently accepted physics. It is then the absence of the latter that accounts for our inductive expectation that ‘all emeralds are green’ will continue to hold after *t*. In other words, once we pay attention to the relevant network of laws in terms of which we understand predicates such as ‘wavelength’ or ‘green’, we can see that non-trivial cases of the puzzle are going to be few and far between.

 Of course, as she then concludes, genuine examples might be found in the context of the debate regarding meaning variance. Whether there are any theories that are symmetrical in the requisite manner is a question for the historians but as she says, ‘What Goodman has shown, and it is a fundamentally important insight, is that if there were such pairs of conflicting theories, they would be confirmationally incommensurable. ’ (p. 23) Thus Kuhnian incommensurability is identical with the non-trivial form of Goodman’s paradox. In that regard, then, the puzzle is insoluble but Hesse insists, in practice we rarely encounter such fundamentally conflicting theories (and even more so, theories that are appropriately symmetrical). And so focusing on entrenchment is misleading, since paying due attention to the history of science will show that in all cases, other considerations are always given priority, generating the asymmetry that is needed to break the paradox.

* This paper then formed a chapter in her highly regarded book on scientific inference (Hesse 1974), praised by Bloor as ‘…an important and challenging book [that] deserves the closest study not only by philosophers of science, but also by historians and sociologists of science. ’ (Bloor 1975) Here she brings together the various components of her rejection of the Received View and offers instead her ‘network model’ of scientific knowledge (sometimes called the ‘Hesse-net’), touched on above, according to which all scientific predicates are to be understood in the context of a law-informed network. As a result, the circumstances under which a so-called ‘observation’ predicate may be correctly applied may change as these laws change and hence there is no fundamental distinction between theoretical and observational languages. The structure of the network is then best described via Bayesian probability theory and we’ve chosen to include Dorling’s critical review of this aspect of the book, which focuses on the core problem of how we can be justified in taking the evidence for some theory as increasing the probability that other predictions of that theory will also be confirmed.
* Hesse’s response is to draw on the notion of analogy once again, and to argue that such an analogy holds between the relevant instances of prediction, such that we are justified in inferring from the presence of the relevant properties in one such observed instance, to their presence in the unobserved ones. This is formally accommodated by requiring that the relevant *apriori* probabilities satisfy a ‘clustering’ rule according to which properties that go together in one case, stay together in other cases. As Dorling notes, this is taken to lie at the heart of scientific inference. Unfortunately, he goes on to argue, Hesse’s principle fails in the context of actual historical examples of rival theories, such as Tycho’s and Copernicus’ or Einstein’s and Ritz’s, which agree on some predictions but not on others. In these cases, Dorling insists, we cannot find the relevant analogy in one case but not the other, but yet we cannot have transitivity of confirmation in both cases as the theories are incompatible (Dorling 1975)

##  This criticism draws on earlier work (Dorling 1974) and Hesse’s response (Hesse 1975) is illustrative: first, she maintains that she does not assume that we are always justified in making the inference above; rather she is interested in establishing when we are and when we are not. And secondly, there are different criteria of confirmation in play here – indeed, at the end of his *BJPS* review, Dorling accuses her of being closer to a neo-Carnapian than a true Bayesian personalist, which, Life of Brian resonances aside, perhaps makes her account even more interesting from today’s perspective!

* + Finally, returning to her earlier work, we’ve also included her paper (Hesse 1960) not only because it serves as a reminder of the close ties the *BSPS* had, at its inception, with the *BSHS*, but also Hesse’s own historical interests, which, as noted above, nicely mesh with today’s revival of an ‘integrated’ approach to HPS, at both the national and international levels. In this paper, Hesse considers the different views of historians of science about Gilbert’s *De Magnete*. Some took him to be one of the first Baconian’s in his emphasis on experimental work; others focused on his metaphysical predilections. Hesse articulates a third view by bringing *De Magnete* under the lens of the (then) recently published (in English) *Logic of Scientific Discovery*. In particular, she suggests that ‘… Popper's thesis, namely, that there are no privileged observation statements upon which scientific theory can be inductively based, is amply illustrated by the details of Gilbert's work. ’ (p. 130). More generally, from the Popperian perspective, Hesse argues that the above historians’ distinction should be replaced by another: between Gilbert the experimental scientist who adopts a form of ‘conjectures and refutations’ and Gilbert the metaphysician who proposes views that are unfalsifiable but heuristically suggestive. Interestingly, in the context of a detailed analysis of Gilbert’s research into the nature of electricity and magnetism, and in particular the sense in which his notion of a magnetic ‘form’ might be viewed as a cause of the relevant phenomena, she makes the distinction between what is now known as the D-N account of explanation and the alternative view of explanation as ‘making plain’ in the sense of capturing some puzzling phenomenon via a familiar model. Here the role of analogy is again crucial and Hesse clearly sets out the positive and negative analogies that Gilbert draws between magnetism and the soul, thereby adding further nuance to his animism. And of course, from the Popperian perspective, this analogy is metaphysical and unfalsifiable.

 The paper ends with Hesse’s reflections on Bacon’s dismissal of Gilbert’s work, in which she suggests that the ‘subsequent practice of science’ (p. 141) justifies the latter’s approach rather than the former’s, in that although Gilbert also used a method of ‘crucial experiment then elimination’, what was being eliminated were entire prior *theories* rather than mere ‘qualities’ (Hesse went on to consider Bacon’s method, as refracted through the work of Hooke in Hesse 1966). And, she argues, this elimination does not proceed with the aim of showing there can be only one, but rather ‘merely replaces a few refuted theories with another’ (p. 142), thereby following a method that is ‘nearer the pattern of later physics’ (ibid.).

 Now of course, one could object to the details of her historical analysis of Gilbert’s work (cited in Freudenthal 1983) and one could certainly balk at this early imposition of a Popperian framework upon it but what is ultimately of lasting significance in this paper is her view, as expressed in another of her books, that ‘[i]n writing the history of science there will always be present, either implicitly or explicitly some philosophical view of the nature of science’ (Hesse 1961; preface). This integrative attitude runs throughout her work, from these early papers to her later forays into the sociology of scientific knowledge (Arbib and Hesse op. cit.; Hesse 1980). But as indicated at the beginning of this introductory essay, her importance for the history and philosophy of science as a whole goes beyond this academic work, to embrace her active roles in professional organizations on both sides of the Atlantic and, not least, her editorship of this journal. The papers reproduced here are only a small sample of her extensive and varied output over many years but hopefully they are indicative of both the range of her interests and her philosophical insight.

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