physical world as a solid base for their own metaphysical endeavours. *Scientific Representation* should be required reading for contemporary physicalists and analytic metaphysicians. It is an important contribution to a grand tradition of work in philosophy of science by physicists like Boltzmann, Hertz, Mach and Poincaré as well as philosophers including Russell, Reichenbach, Carnap and Putnam willing to engage with science as it is rather than how they imagine it to be.[12]

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Reply to Contessa, Ghins, and Healey

By Bas C. van Fraassen

Gabriele Contessa (2010), Michel Ghins (2010) and Richard Healey (2010) each broach issues concerning realism, and their constructive critique presents a strong challenge, requiring me to specify precisely where I take it that *Scientific Representation* (henceforth SR) lands us. Contessa argues that there I have left not only metaphysical realism but also common sense realism behind. I'll argue that it isn't so, though I reject the metaphysical realism that might be taken to underpin our common sense. But then Ghins and Healey challenge just what it is that is represented, if at all, by scientific models and theories, and I will maintain that in a truly robust sense models do represent the observable phenomena.

1. What is Realism?

Contessa begins, following Stathis Psillos, by depicting scientific realism as consisting of a metaphysical, semantic and epistemic thesis. That goes against my contention that instead, scientific realism is in the first place a view that

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characterizes science in terms of an aim, a criterion of success, different from what empiricists take it to have. Be that as it may, Contessa's main challenge concerns metaphysical realism, and it seems to me that he vacillates on what that is.

That 'the world is (largely) independent from our way of representing or describing it', in his first paragraph, is innocuously true. It is implied by such common sense assertions as that there are, have been, and will be many things and events that are never represented or described at all (cf. SR: 246). That this is so, and that there is no problem of reference to the things, persons and processes that we mention, for example, when discussing scientific practice, I will label 'common sense realism'.

Metaphysicians may contend that common sense realism is unintelligible except as presupposing a metaphysical underpinning, encoded in Plato's phrase in the Phaedrus, 'carving nature at its joints'. It is the latter that bears the burden of Contessa's sally in his Part IV. I reject that contention, taking common sense and its unproblematic reference to children and toboggans to be intelligible without such underpinning. The puzzles that call for that sort of metaphysical realism dissolve into thin air upon scrutiny (cf. my 2009).

Michel Ghins addresses this more clearly in his section 'Appearance and Reality', though I would again insist on a careful distinction between the innocuous and the philosophically laden, as I will explain below. The question of realism is there connected with a correspondence theory of truth. 'Correspondence' can either have an innocuous sense, or be meant to be substantive. Such clearly realist writers as Stephen Leeds, Michael Devitt and more recently Penelope Maddy, have argued that scientific realism does not need a substantive correspondence theory of truth. Outside philosophy of science the point is made sharply by David Lewis 2001.

But Contessa presumably sides with Psillos who originally read my position as agreeing to the metaphysical thesis they see as part of scientific realism: 'Structural empiricism takes scientific theories literally and rests on the notion of truth as correspondence' (Psillos 2004: 566). I'll argue that this is not so, but at the same time, that there is no conflict with common sense realism.

2. *Representation and the 'Bridging Problem'*

The first part of the book intends to bring the discussion of scientific representation down to earth, to place our philosophical feet firmly on the ground. Representations are entities made, taken, or used to represent something – so they are *artefacts*, that is, objects and structures on which there is a bestowed role and function. When deliberately made to serve a specific

purpose, the criteria of adequacy for representational success relate to that purpose.

To present this view at all is to presuppose that reference to such objects or structures, and to the persons using them and the uses to which they are put, is freely available. That presupposition makes one a common sense realist, at the least. So if Contessa could successfully argue that the position presented in the book is not compatible with reference to his children's sleigh, he would not only have convicted the book of leaving common sense realism behind, but also of rank inconsistency.

Ghins, Healey and Contessa all broach the problem of how an abstract structure can represent the phenomena; Contessa refers to it as the 'bridging problem'. This is the problem of coordination in its most general form, and the eventual answer must be the same as in the concrete case: through measurement. But posed in this form, the problem is surrounded by metaphysical questions. Reichenbach put it quite clearly: isomorphism is a relation between mathematical structures, and well-defined only there. So we cannot blithely say that an abstract structure can be used to represent a physical entity because it is isomorphic to that entity.

Reichenbach spoke dismissively of an 'off-hand answer', such as this:

> the model in which PV=rT fits a body of gas if and only if there are physical quantities P*, V*, and T* characterizing that body which stand in the relation P*V*=rT*.

That is just a 'dormative virtue' response. In the abstract, the off-hand response takes the form: the model can be used to represent the body of gas because the model (or a sub-model) is *isomorphic* to a structure 'in' that body. Not only is that a merely verbal response, but it also hijacks a term from mathematics for unwarranted use elsewhere.

What sort of assertion about a body implies the existence (not only of that body but also) of a structure 'in' that body? Not any ordinary predication. Recall Quine's retort to the fictional McX in his 'On what there is'. That there are red houses does not imply the existence of an attribute *redness* – it does not imply the existence of anything except houses. McX insists to the contrary that 'there are red houses' cannot be true if all that exists are houses. 'There are red houses' is true if and only if there are red houses. But to McX it looks as if that implies just what he wants to say!

As a more pertinent example (see my 2008: 242–43 for elaboration), suppose I draw a rectangle to represent the top of my desk. My drawing represents the desk top as rectangular. Neither my presentation of the drawing nor an assertion of 'The desk top is rectangular' implies the existence of anything beyond the desk top. Suppose I do not draw the rectangle, but refer to a geometric object, the Euclidean rectangle, and say that this is what the desk top is like. Again I have represented the desktop as rectangular. The same point applies. I *have not* thereby *asserted* that there is a structure, that the

desk top has or instantiates or participates in, of the same kind as the mathematical structure, so that there exists an isomorphism between elements of that structure and elements of the Euclidean rectangle.

Asked to back up my representation of this physical desktop by means of a Euclidean rectangle, I will make measurements. For at first blush, the question is just a practical one: how closely do the measurement results align the desk top with the Euclidean rectangle? The practical worries are about possible unreliability and the inevitable degree of approximation involved, and I can respond to them.

The dialogue could now just repeat itself: do the assertions, that report the measurement results on parts of the desk top, imply the existence of something beyond those parts? I see absolutely no need to say so.

3. *Contessa: the Deerstalker Tale*

The general question posed was this:

> [Q] how, or in what sense, can such an abstract entity as a model 'save' or fail to 'save' [a] concrete phenomenon? (SR: 245).

A theoretic model's relation to the phenomenon may be mediated by a data model; but this just pushes the question one step back. So let's look at a case in which all the quantities are directly measurable. I divided the question into two, for phenomena that respectively are or are not encountered in human experience actually subjected to measurement. Contessa's Deerstalker tale relates to the former, so let's stay with that.

The Deerstalker tale is essentially my example (SR: 254–57), moved from Princeton to Red Deer, Alberta. Professor Deerstalker presents his theory about factors affecting deer population growth, and displays a graph of that growth in a recent period, constructed from measurement results, which bears out his theory. Now an audience member challenges Deerstalker:

> Yes, T fits well with this graph, your representation S, but does T fit the actual deer population growth in [Red Deer]? (SR: 254)

This challenge can be taken in various ways; for clarity we can imagine two such challengers, one concerned with practical issues, the other philosophical. In response to the former, Deerstalker will account for how the graph was constructed, how the measurements were made, and so forth. That could satisfy a challenger with practical concerns. But for the philosophical challenger, that is all irrelevant, and she replies:

> Yes, I understand, I can see that you carried out those procedures diligently and responsibly, and that the outcomes are summarized properly in your graph. But the question remains: theory T fits the summarized outcomes of your measurement procedures, but does T fit the actual deer population growth in [Red Deer]?

At this point, in my version of the example, the professor realizes that of course in general, there may be a mismatch between a representation and what it represents. But since he is presenting the graph as his representation of the deer population growth, the following questions are equivalent *for him*:

(a) Does T fit the graph?
(b) Does T fit the actual deer population growth?

The 'for him' signals that the equivalence is not semantic – not a necessary sameness of truth value of two propositions – but on the level of pragmatics. In this situation, unless the professor takes back his graph, for him to say 'yes' to (a) and 'maybe not' to (b) would involve him in the sort of incoherence we find in Moore's paradox.

Clearly recognized in Michel Ghins's discussion, but I think absent from Contessa's, is the distinction between two senses of 'representation'. Deerstalker's graph, a *thing*, is a representation of the deer population growth, and as Ghins rightly says, such a thing does not by itself have any illocutionary force. However, Deerstalker's presenting the graph is a representation too. It is an *act*, and it involves the claim that the deer population was thus and so.

Appreciating this, we will not be tempted to think that treating (a) and (b) as equivalent, Deerstalker must be implying that there is a structure in nature – that there exists a relevant entity other than the deer in this area – to which his graph instantiates the special correspondence relation, such as is postulated between representation and the represented by traditional metaphysical correspondence theories of truth. Whatever puzzles appeared to demand such postulates dissolve once we recognize the pragmatic incoherence he would land in by simultaneously (i) not withdrawing his graph as adequately representing the deer population growth, and (ii) not treating the two questions as equivalent.

Contessa's reaction refuses to distinguish the practical from the philosophical question. He takes Deerstalker's response to indicate that he is not willing to recognize or admit any possible defects or deficiencies in his graph.

But we could easily add an intermediate stage in the dialogue, with Deerstalker agreeing that the graph represents the deer population growth partially, imperfectly, and approximately due to averaging and the limits of sampling. It would have been a perfectly reasonable addition if Deerstalker had done so. But it would make no final difference; the philosophical challenger would just come back with

> Yes, I understand, you are presenting the graph only as a smoothed summary of data pertaining to a measurements made just once a year, and that these are data from relevant but not perfectly reliable

> measurements made during the period at issue. But the question remains: theory T fits the graph to the relevant degree of approximation, but does T fit the actual deer population growth to that degree of approximation?

Deerstalker must say yes, unless he decides to give up his – now more clearly specified – presentation of the graph as depicting the deer population growth to that degree of approximation.

I am not suggesting that this is merely a confusion on Contessa's part. His point is that in responding in this 'Wittgensteinian' way, my solution 'to the bridging problem seems to come at a hefty philosophical price – that of rejecting metaphysical realism.'

That would be bad news if the rejected realism was the common sense realism that was involved in constructive empiricism as well. But Deerstalker has said nothing to imply that there were no actual deer, or that they did not have a particular (though perhaps only vaguely known) number of offspring. What is rejected is the more properly so-called metaphysical realism encoded in the phrase 'carving nature at the joints'. To conclude then, let me just revise very simply the last statement in Contessa's section III:

> An empiricist structuralist … would not deny that there is such a fact of the matter [as to the number of deer in a well-defined area], for … she does concede that there is in general a difference between the theory fitting the graph and the theory fitting the world. But she adds that *for the representor offering the graph as depicting the world*, whether *the theory fits the graph* amounts to the same question as whether *the theory fits the world*.

4. *Ghins: the Crucial Move Revisited*

In Michel Ghins's discussion it seems to me that, at the most crucial point, he makes the same kind of move to counter the 'loss of reality' objection, or equivalently, to dissolve rather than solve the 'bridging problem'.

Ghins opts explicitly for a correspondence theory of truth, and writes

> we don't need to posit the existence of a property such as 'whiteness' that exists in the snow in order to account for the truth of the statement 'snow is white'. Yet, if in presence of snow we assert that it is white, our judgement is true or false in virtue of something which is in some sense independent of us. […] In this sense, I accept that true statements, in which our correct judgements are expressed, have truthmakers, that is, corresponding real external facts that make them true.

But on close reading, Ghins seems far from a naïve postulate of 'truthmakers' as an additional specific ontological category of entities. The only structures that appear in his account are those of the theoretical models and data

models. When Ghins turns to the relation between the appearances and the phenomena, he explicitly rejects what Reichenbach calls the 'off-hand' response that postulates a corresponding structure in the phenomenon. He insists: 'To repeat, phenomena are not structures.' The contents of a measurement outcome can be conveyed in a simple statement, such as 'the temperature is 37.3°C', a predicative statement. This is the point where representation stops:

> Predicative statements of this kind ... do not trade on representation [... they do] *not* state a representative relationship ... there is no chasm between a representation and a thing, simply because there is no representation, period.

What about physical quantities then? In models, the terms 'temperature', 'mass', 'force', etc. do have denotations: they denote functions that assign numbers, vectors,... to other elements of the model. But in describing the bodies measured or represented, they provide a convenient *façon de parler* for predication:

The patient has a high temperature	The patient is hot
The patient's temperature is 37.3°C	The patient is 37.3°C-hot

The predicate '37.3°C-hot' is related to 'hot' as 'scarlet' is to 'red'. Applying the predicate to a subject is simple attribution or predication, and does not bring along any extraneous existential commitment.

Now I want to make a distinction: while I agree strongly with this point about the predicative statement, I do want to say, for example, that the Euclidean rectangle can be used to represent *the desk top*. I do not see a need to go further, with Ghins, to his contention that 'What is represented is what we decide to abstract from the target, not the target itself.' May I surmise that there is lingering sense in Ghins that the one could represent the other only if there were two isomorphic structures in play? The move about the role of predicative statements, at the point where the appearance is said to be 'of' the phenomenon, already gets him out of the problem. No need for extra steps! It is the phenomena, even *stricto sensu* that are represented by our models.

5. *Healey: the Appearance from Reality Criterion*

Part Four of *Scientific Representation* is devoted to the venerable problem of how the scientific image relates to experience, sub specie the development of physics in the modern era. Richard Healey advocates a pragmatist turn, in view of his radical conclusion: 'a physical theory – even a fundamental theory – may be completely successful in all its applications without offering a representation of reality at all.'

That is truly a challenge. I see two major agreements between us, on issues important to philosophy of science and beyond, and will detail those. But even if the displayed difference between a pragmatist and a constructive empiricist position is in the end, as Healey says, 'a mere family squabble compared to the gulf' separating us from a metaphysical alternative, I'll argue that I need not go that far.

The focus is the Appearance from Reality Criterion, a putative criterion of completeness for science: that the appearances should be explained as deriving from, produced by, the theoretically postulated reality. Healey and I agree that a lesson from the history of quantum mechanics is that satisfying it turns out not to be incumbent on scientific practice. But we have different reasons.

The criterion applies at all only if the appearances (contents of measurement outcomes) are represented within the theory's postulated underlying reality. In the discussion of quantum mechanics that means: if the measurement outcomes, as physical final states of the instrument, are represented as quantum states. That is certainly not *outré*; it is part of von Neumann's description of the measurement as projecting both the measured object and the instrument into eigenstates of, respectively, the measured observable and the 'pointer' observable. But it has been a long time since von Neumann presented the matter, and Healey is right to refer to the plethora of new approaches.

So Healey contends that it is just because of assuming that the quantum states are used to represent reality (i.e. presumably, what really happens at the end of a measurement) that I had not yet succeeded in dismissing the quantum measurement problem.

Let me admit at once that if that assumption is dropped, the problem disappears. But then also the question as to the Appearance from Reality Criterion disappears, and so my main conclusion stands. So I can gratefully see Healey's point of view as supporting that conclusion. But I want to dispute the implication that this is the only way to support it. With a relevant footnote attached, Healey writes:

> It is true that one could continue to represent each of the various outcomes of a laboratory measurement by a corresponding quantum state, simply ignoring the problem of how this state could have evolved in any quantum-mechanically describable measurement interaction. But once the link to quantum dynamics has been cut no significance attaches to the fact that the theoretical probabilities of these quantum states match those of the surface model: the representation of a measurement outcome by a quantum state of the apparatus has been rendered idle.

While Healey's text does not go so far as to suggest that it would be inconsistent to represent each of the various outcomes of a laboratory measurement by a corresponding quantum state, he dismisses that option. The reason

is, I think, precisely that quantum dynamics does not, in that case, govern the evolution of those assigned states. And so the discussion here goes back to the feasibility of a modal interpretation of quantum mechanics in which the dynamical state is separated from the 'value state' – both represented by the same sort of operators on Hilbert space – which represents the physical measurement outcome (cf. my 1997 reply to Leeds and Healey). While neither the von Neumann eigenstate–eigenlink option nor the various modal interpretations are much in fashion these days, the option of representing the final measurement state by a Hilbert space vector or operator, despite the disconnect with the dynamics, is certainly not absent from today's scene (cf. further my 2010).

So let us put it this way: both options, representing the physical measurement outcomes with the same mathematical entities for – thus locating them in the same logical space as – the dynamical states, or refusing to thus locate them at all, point to the same lesson to be drawn from the history of the philosophy of quantum mechanics. That is: the Appearance from Reality Criterion is after all not incumbent on scientific practice. Scientific realist views must have some difficulty with this – perhaps they may be maintained in the way Healey develops here.

Healey proposes here a thoroughly anti-representationalist stance with respect to science, while I conceive of science as very much in the business of creating representations, properly understood along empiricist lines. But there is, so to speak, a 'higher level' where a new agreement appears. Healey discusses this when he briefly turns to the question of how we are to view the probabilities that quantum theory provides.

Probabilities in physics, if irreducible to measures of ignorance, are putatively physical objective chances, and thus a real challenge to empiricist views of science. What I proposed in *Laws and Symmetry*, in a correction to *The Scientific Image*, is that to accept such a theory involves (not believing in objective chance but) appointing the theory as one's expert in certain respects, rigorously formulated in terms of Gaifman's concept of an expert function. Briefly put, such acceptance puts the theory in the role of constraining one's subjective expectations – it makes the theory's probabilities *authoritative*, in Healey's apt terminology.

Reading Healey's comments, I was initially tempted to say that we join forces with, so to speak, anti-representationalism in a higher key. But in the end I do not see the above move concerning what it is to accept a probabilistic scientific theory as eliminating a view of such theories as creating representations of the phenomena. We may need to think hard about what representation amounts to in this case. There is certainly a twist. Accepting such a theory does not involve an opinion that could be summed up in the form 'This is what things are like!' Paradigm examples of representation may not shed much light on this case. But I would finally emphasize the part that Healey mentions a bit dismissively: frequencies in repeated or diverse

measurement outcomes will allow a backward inference to the character of the source systems that locates them in the theory's logical space. And that is certainly representation.

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