Nancy Cartwright's Philosophy of Science

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16 Getting the Causal Story Right
Hermeneutic Moments in Nancy Cartwright's Philosophy of Science

Alfred Nordmann

For all I know, the term "hermeneutics" appears nowhere in Nancy Cartwright's books and articles. Any attempt to appreciate hermeneutic moments in her work therefore requires special justification.[1]

Even though there is by now a long tradition of studies and reflections on the hermeneutics of science, it has not been able to dispel serious reservations about the transfer of a textual, if not literary mode of analysis to the domain of science and nature. First, even though it has been acknowledged that in scientific experience we do not encounter things in themselves but something that is structured by conceptual, instrumental, and sensory modalities, reality is not therefore inert and fabricated like a text. Second, the hermeneutic process is said to consist in the integration of a text within a horizon of meaning, and as this integration is never seamless it requires adjustments such that the reader of the text emerges as a different person (Gadamer 1975; Ricoeur 1981). This presupposes an individualistic conception that is hardly suitable for the collective work of science. Third, though one can say that scientific data require interpretation, this kind of "interpretation" is surely much more constrained than, say, the interpretation of a literary work.[2] Fourth, while in the paradigmatic case of literature hermeneutics generally refers to the relation between reader and text, the hermeneutics of science follows Kuhn in that it is less interested in the reader of a scientific text and rather more in the scientific community as a community of interpreters that reads nature in a certain way. The hermeneutics of science thus appears stuck between a rock and a hard place: It needs to either consider nature as a text and encounter the first objection above, or it must account for the curious fact that scientific texts defy hermeneutics in that they do not require exegesis but disclose themselves immediately. Indeed, it is a hallmark of membership in a scientific community that the texts of one's peers can be taken literally and are rarely subject to interpretation. Science and nature and scientific texts and their readers have thus appeared to be the moving targets of hermeneutic equivocation. Fifth and finally, the hermeneutic process is said to lead into a hermeneutic circle according to which there is no outside to the activity of interpretation. Bas van Fraassen elaborated how the scientific enterprise moves within such a hermeneutic

circle: Because the empirical content of a theory is specified by the theory itself, theories can only save their phenomena and have no further-reaching claim to truth (van Fraassen 1980: 56–9; see Cartwright 1983: 88). Nancy Cartwright's work, however, is an attempt to meet van Fraassen's challenge and to show a way out of the circle at least for causal explanations.

> Van Fraassen [. . .] offers more of a challenge than an argument: show exactly what about the explanatory relationship tends to guarantee that if *x* explains *y* and *y* is true, then *x* should be true as well. This challenge has an answer in the case of *causal* explanation but *only* in the case of causal explanation. [. . .] In causal explanations truth is essential to explanatory success.
>
> (Cartwright 1983: 4, 10, see 89–99, 159)

For the most part the scientific enterprise may well be caught up in van Fraassen's nonvicious hermeneutic circle. However, we should not underestimate 'the very special case of causal explanation' (Cartwright 1983: 10). Empiricists like van Fraassen have tended to discount it; realists take it to be the paradigm of successful ordinary science. Cartwright seeks a middle ground: The very special case of causal explanation can teach us about the work that is required for scientists to achieve this peculiar kind of success. By showing that causal explanation results from a felicitous alignment of phenomena, models, and theories, she introduces her readers to the toolbox and resources of science. Cartwright thereby presents scientific work as a hermeneutic process of sorts and, along the way, counters the various objections to the very idea of a hermeneutics of science.

MIDDLE GROUND

In other respects, too, Cartwright locates her own position between that of various received views. Only a few instances of this need to be mentioned here. They help define *ex negativo* where Cartwright stands, allowing us to then appreciate the centrality of the hermeneutic moments in each of her three main works.

Cartwright explicitly claims for herself a 'middle ground in the dispute' between realist and constructivist accounts of the success of science (Cartwright 1999: 47). According to the first of these, science 'reveals [. . .] directly the language in which the Book of Nature is written' (Cartwright 1999: 46). This direct revelation issues in statements that are straightforwardly true or false, that can therefore be taken literally and require no mediation by a hermeneutic process of interpretation or negotiation. According to the second account, the success of science is trivial in that one cannot first construct a world and then act surprised that certain constitutive principles apply to

it. This constructivism posits a realm of human practice that is hermetically self-enclosed and is not measured against anything outside it.[3] In contrast, Cartwright emphasizes that science requires work, that is, practical human engagement with a world of immensely varied concrete situations. Whether scientific work succeeds is no matter of simple procedure, methodology, or routine. The success of science consists in the establishment of a more or less local, more or less robust alignment of phenomena, models, and theories. Indeed, as will be shown below, this success coincides with the achievement of literalness: Once everything fits together, the hermeneutic mediations of scientific work give way to straightforward truth or falsity.

Cartwright also seeks a middle ground regarding "modalization" (Cartwright 1989: 158–170). She is sympathetic to empiricist attempts to "modalize away" causal laws, that is, to refer to the formal mode of mere linguistic representation what, as a manner of speaking, is misleadingly cast in the material mode. According to Cartwright, laws 'are generally pieces of science fiction, and where they do exist they are usually the objects of human construction, objects to be explained, and not ones to serve as the source of explanation' (Cartwright 1989: 218, see 229). Cartwright is also sympathetic, however, to the attempts by scientific realists to distinguish causal laws from merely accidental generalizations (Cartwright 1989: 7, 36, 131–136). Here, Cartwright claims as middle ground that one cannot modalize away capacities and their power to productively bring things about (i.e. singular causation). Those who wish to distinguish between laws and generalizations are onto something, namely capacities, even as they are wrong about causation, truth, explanation, and law.[4] They tend to be confused, in particular, about the relation between the formal mode of theoretical representation and the material exhibition of the capacity in the model. While they think of this relation as one of inclusion, Cartwright argues against the notion that the materially concrete is an instance of something like a general fact. In her view, properties like "being subject to a force" or "doing work" do not exist in the abstract but can exist only when, by way of models, they are referred to concrete situations like "being located at some distance to a charge" or "washing dishes" (Cartwright 1999: 40–46).

Cartwright finally detaches models both from phenomena and from theory in the sense that there are no determinative relations among them. On the one hand, this opens an indeterminate space for a wide-range of models (and this, in turn, has prompted wide-ranging discussions): She considers phenomenological or representative as well as theoretical or interpretive models; she allows for experimental situations, schematic and block diagrams, equations, conceptualizations, and simulations to serve as models. Models can have various degrees of idealization and abstraction, and some models are models of models. If there is a significant shared feature of interest in Cartwright's discussion of models, it is that they can exhibit the causal structure in which capacities come alive and manifest their productivity (Cartwright 1989: 223). Models figure prominently in the story of how one

moves from phenomena all the way up to theory, and equally prominently in top-down accounts that take us from theories to the phenomena. While they stand at an intersection of the roads that lead from phenomena to theory and from theory to the phenomena, there are no antecedent guarantees that they will successfully coordinate theory and phenomena. Indeed, phenomenological or representative models may fail to concretize or realize theoretical concepts, and it may require a rather tenuous process to relate theoretical or interpretive models to the phenomena (compare Cartwright et al. 1995). However, it is also possible for phenomenological and theoretical models to be aligned or even to coincide. In those instances, it becomes possible for scientists to routinely traverse in both directions between the abstract and the concrete. Cartwright rejects any philosophy of science that takes those cases as its paradigms and thereby ignores the work that is involved in relating phenomena, models, and theories to one another (Cartwright 1983: 17, 162; 1999: 43, 47). At the same time, whenever Cartwright considers in her own terms the movements back and forth between the abstract and the concrete, she arrives at what I here call "hermeneutic moments". At these moments, the models are the stage on which the negotiations take place and on which the top-down and bottom-up approaches become calibrated to each other. Moreover, her hermeneutic characterizations treat the model not only as the site at which those negotiations converge, but in an interesting sense they turn the model into a protagonist of sorts, namely into a device that interprets, measures, or reads phenomena and theory and that promotes the attunement of concrete and abstract properties.

MIXED METHOD: HOW TO READ MARX, SCHRÖDINGER (AND MILL)

In *Nature's Capacities and their Measurement*, Nancy Cartwright endorses not only Mill's discussion of tendencies but, along with it, his mixed method and its proposed middle road between inductivism and hypothetico-deductivism. To the extent that both methodologies take laws to be exceptionless statements about what things regularly do, neither does justice to her and Mill's view that laws are about the tendencies or capacities of things even where these are manifested only in highly irregular circumstances. To show how one arrives at knowledge of these capacities, Cartwright quotes the following passage in which Mill contrasts the inductivism of the so-called "practicals" with the mixed method that is adopted by the "theorists." As Cartwright emphasises, Mills' theorist does not conjecture a theory in order to deduce a testable prediction. Instead, he draws on his knowledge of capacities and extrapolates from this knowledge:

> Suppose, for example, that the question were, whether absolute kings were likely to employ the powers of governments for the welfare of or

> for the oppression of their subjects. The practicals would endeavour to determine this question by a direct induction from the conduct of particular despotic monarchs, as testified by history. The theorists would refer the question to be decided by the test not solely of our experience of kings, but of our experience of men. They would contend that an observation of the tendencies which nature has manifested in the variety of situations in which human beings have been placed, and especially observations of what passes in our own minds, warrants us inferring that a human being in the situation of a despotic king will make a bad use of power; and that this conclusion would lose nothing of its certainty even if absolute kings had never existed or if history furnished us with no information of the manner in which they had conducted themselves. (Cartwright 1989: 171)[5]

The theorist's mixed method here refers on the one hand to the experience of men and thus to knowledge of our tendency to exercise power over others—a kind of self-knowledge—and on the other hand it refers to the experience of kings by way of the conjecture that kings are men like other men. Together, introspective acquaintance with a tendency and the deductive consequence of a hypothetical generalization yield the conclusion about the despotic king's abuse of power. This is how Nancy Cartwright goes on to generalize Mill's example:

> [O]ne looks for what is true in an ideal model in order to establish an abstract law. But there is a difference between what is true in the model and the abstract law itself. For the ideal model does not separate the factors under study from reality but rather sets them into a concrete situation. The situation may be counterfactual; still it is realistic in one sense: all the other relevant factors appear as well, so that an actual effect can be calculated. What is ideal about the model is that these factors are assigned especially convenient values to make the calculation easy. (Cartwright 1989: 191)

By the observations that pass in his own mind, Mill's theorist has experience of how men use their powers generically in regard to the factor of social standing, that is, for any situation where someone has power over another. An ideal model represents such an experience. From the truth contained in the model one may then advance to an abstract law which states something about the uses or abuses of power over others, and this law does not need to refer to social standing at all. The ideal model thus sets the factor of social standing in a way that "makes the calculation easy"; one can concretize it by adding the factors back in and assigning them more definite values, for example by considering the case of an absolute king.

Idealizations thus remain realistic in the sense that, in principle at least, they afford a way back to the phenomena. All one can ever do in a

concretization, however, is to give definite value ("absolute monarch") to a factor that remained generic in the idealized model ("power over others"): One can fill in causal structure (Cartwright 1989: 223). However, one cannot undo the abstraction from the materiality of all situations that takes place in the abstraction to laws and in models of theories: In a concretization from theory, "theory gives out" sooner or later (Cartwright 1989: 211, see 207, 226). As distinct from idealization, abstraction from factors in the material world is no longer realistic in that it subtracts the factors altogether, rather than merely assigns them an idealized, convenient value in a counterfactual, yet concrete situation. Indeed, the terms of a theory implicitly provide a list of all those factors that can be concretized. As this list always abbreviates the total number of factors involved in any concrete situation, 'this kind of process will never result in an even approximately correct description of any concrete thing. For the end-point of theory-licensed concretization is always a statement true just in a model' (Cartwright 1989: 207; compare Suárez 1999: 180–182).[6]

In the case of Mill's example, any materially concrete historical situation contains more than what is contained in our historical experience of kings and in the experience of our tendencies in exercising power over others. In particular, it may contain factors that counteract our tendencies in the exercise of power. Even if it is true that a certain social structure which concentrates power in an absolute monarch produces a lack of social justice, this truth does not serve to describe the concrete situation of a religious state or of the enlightened despot who lets fairness rule by his grace or whim: The truth doesn't explain much.

Where, now, lies the hermeneutic moment in this negotiation of the abstract and the concrete by way of Mill's mixed method? A first clue is provided by Cartwright's reliance in her account of abstraction and concretization on Leszek Nowak's *The Structure of Idealization: Towards a Systematic Interpretation of the Marxian Idea of Science*, i.e. a hermeneutic exercise par excellence.[7]

> Nowak's story involves the obvious idea that one must add corrections and additions as one moves from the abstract to the concrete. It is critical to the account that these corrections should not be ad hoc addenda just to get the final results to come out right: they must be appropriately motivated. I take it that means they must genuinely describe other causes, interferences, impediments, and the like. But it follows from that that the scheme can only work if we are already in control of a rich set of non-Humean, capacity-related concepts. (Cartwright 1989: 202, see 206)

In the case of Marx's *Das Kapital*, this requires an interpretive reconstruction such that 'a more detailed account of the nature of the corrective factors vis-à-vis the principal ones can be given'. Once this account is obtained, 'it is Marx's theory that tells what kinds of factor have been

eliminated in arriving at the law of value' and what sequence of corrective steps will take us closer to a concrete historical situation until 'theoretical corrections run out and the process must be carried on case by case' (Cartwright 1989: 206, 209):

> The same is true for quantum mechanics. The Hamiltonian for a "real" hydrogen atom is supposed to be arrived at by adding correction terms to the ideal Hamiltonian, where the correction terms come from the theory itself, from the list of other acceptable Hamiltonians. (Cartwright 1989: 207, see 205)

Cartwright and Nowak thus show that this widely, perhaps standardly used scientific method gives rise as a matter of course to the perceived explanatory weakness of Marx's economic theory in particular and of social or political science in general (Cartwright 1989: 204). In terms of explanatory weakness or strength, quantum mechanics fares no better than Marx's theory: In both cases, if we want to move from abstract theory toward concrete phenomena, the theory has to be read or interpreted such that it tells us not only what is true in certain idealized circumstances but also what factors have been eliminated by it.

Here, the reader or interpreter need not and perhaps should not be an individual scientist who subjects the theory to some sort of exegesis. Nor is it an abstract entity like the scientific community as a whole or "science itself" that provides such a reading of the theory. Instead, just as abstract properties exist only in models, the abstract scientific reader and interpreter of theories also exists in the model.[8]

It holds for both, Marx's *Das Kapital* and the Schrödinger equations, that 'it needs to be made clear that in this or that concrete situation the designated factors are indeed correctives or preventatives, as required for the reconstruction, and also why that is true' (Cartwright 1989: 206). However, only in the first case someone like Leszek Nowak is required to interpretively tease apart the corrective factors vis-à-vis the principal ones. In the case of Schrödinger's equations, the principal factors are identified by the theory itself and the ongoing work of quantum physics adds to the list of other acceptable Hamiltonians that can serve the corrective purposes. The subjective or personalized reader thus drops out in quantum physics. Similarly, the place that was occupied in Mill's account by the theorist himself is taken over in Cartwright's account by the model. Mill's theorist begins with introspective self-knowledge of his tendencies in regard to the exercise of power over others.[9] In science more generally, this knowledge of tendencies is exteriorized and instead of a person, the model provides the causal structure in which tendencies manifest themselves, capacities do their work, and abstract properties come alive.

Although this idea of "the model as reader" needs to be substantiated further, it is already apparent how it addresses a central problem for any

hermeneutics of science.[10] The hermeneutic process is typically said to involve the disclosure of self and world in the interpretive encounter of a reader with a text. As Gyorgy Markus pointed out, the place of the reader in this encounter has traditionally been occupied by a solitary, individual nineteenth-century subject confronted by a poetic text (Markus 1987). Hermeneutics has thus been ill-equipped to acknowledge the depersonalized knowing subject of science. To the extent that Nancy Cartwright's nonsubjective models can take the place of the personalized reader, she has met one of our initial objections against hermeneutic approaches to science.[11]

Cartwright's account of the abstract and the concrete addresses another major difficulty of any hermeneutics of science, namely the problem of literalness. It always appeared to be a hallmark of successful science that it requires no interpretation but issues statements that are straightforwardly true or false.[12] To the extent, however, that hermeneutics denies literalness, it will have to explain how the appearance of a transparency of meaning can come about in the case of science. Nancy Cartwright offers such an explanation by showing literalness to be a specific accomplishment of science. Accordingly, just as she rejects any philosophy of science that takes successful causal explanation to be its paradigm rather than an important special case, she rejects any approach that presupposes rather than explains literalness. Instead of taking literalness for granted, Cartwright begins by pointing out that theoretical laws cannot be literally true because their very purpose is to consider causal processes in isolation (Cartwright 1983: 12). An abstract law about the use and abuse of power might not refer to social standing, let alone the mitigating or aggravating influence of prevailing religious sentiments. The abstract properties it identifies have no given literal referent but will only exist in a model which provides at least an idealized situation such as our own tendencies when we imagine to have power over others. Only once the antecedent of the abstract law has thus been filled in with the relevant detail, one gains a concrete law 'that can be read as literally true or false in the most straightforward sense' (Cartwright 1989: 199). This concrete law assigns phenomenal content to the abstract law, and only the collection of all such concretizations would provide the more or less homogeneous phenomenal content of the abstract law in its entirety.[13] As Cartwright's discussion of Nowak has shown, concretizations stop short of the phenomena and cannot fully undo the material abstraction of the theoretical law. Therefore it is the particular concretized laws that "lie" about concrete situations such as that of the Enlightened despot (compare Cartwright 1989: 199–212). In contrast, the abstract laws cannot be literally true because, taken by themselves, they have no literal meanings that could be judged true or false.[14] Since only their interpretation by an ideal model creates empirical truth-conditions, the interpretive model constitutes meaning and Nancy Cartwright has described a hermeneutic process that yields literalness.[15]

To be sure, just like "the model as reader," this "hermeneutic construction of literalness" requires further substantiation. The hermeneutic moments of *How the Laws of Physics Lie* and *The Dappled World* provide this.

FITTING TO: PREPARED DESCRIPTIONS IN THE THEATRE OF PHYSICS

As we have seen, Cartwright argues that the process of concretization can rarely be completed "once theory runs out". In *Nature's Capacities and their Measurement* she suggests that this is the point where science stops and only engineers can bridge the remaining gap between concretized models and real-life situations (Cartwright 1989: 211). However, both her earlier and her later work have more to say on how to bridge that gap, namely from the bottom up in *How the Laws of Physics Lie*, and from the top down in *The Dappled World*. Both invoke the metaphor of "fitting"—fitting facts *to* theory and fitting *out* theories by dressing them up as statements of fact.

According to Ludwig Wittgenstein, causal accounts establish a fit between causes and effects, and what he had in mind is a kind of mechanical "fit": The machinery of causal interpretation must not idle and will only work if its various parts engage properly (Wittgenstein 1993). Cartwright, in contrast, takes a realistic view of causation: Capacities productively bring things about and need not be fitted to effects. Accordingly, she does not restrict herself to a mechanical notion of "fit" when she requires that many levels of description need to be fitted together in order for abstract laws, models, and materially concrete situations to work together in a scientific explanation. Instead, in Cartwright's case the judgement of proper fit concerns appropriateness and how the different levels are attuned to one another. Beyond that, however, there are important difference between "fitting to" and "fitting out."[16] While "fitting out" is the subject of the next section, facts are "fitted to" theory by being prepared properly:

> At the first stage of theory entry we prepare the description: we present the phenomenon in a way that will bring it into the theory. The most apparent need is to write down a description to which the theory matches an equation. But to solve the equations we will have to know what boundary conditions can be used, what approximation procedures are valid, and the like. So the prepared descriptions must give information that specifies these as well. [. . .] The first stage of theory entry is informal. There may be better and worse attempts and a good deal of practical wisdom helps, but no principles of the theory tell us how we are to prepare the description. We do not look to a bridge principle to tell us what is the right way to take the facts from our antecedent, unprepared description, and to express them in a way that will meet the mathematical needs of the theory. The check on correctness at this stage is not how

> well we have represented in the theory the facts we know outside the theory, but only how successful the ultimate mathematical treatment will be. (Cartwright 1983: 133–134)

There is a certain ambiguity in this passage regarding the notion of "prepared description", an ambiguity that will go away once "fitting out" is considered along with "fitting to". By extending the talk of "prepared descriptions" beyond the realm of quantum mechanics to science more generally, Cartwright invites the analogy to the preparation of a sample, say in microscopy. Now, this is preparation for scientific observation and eventually for theoretical treatment, and surely it is always constrained by disciplinary or theoretical interests (compare Cartwright 1989: 209). However, to prepare a sample in microscopy is not necessarily a preparation for a particular theoretical, let alone mathematical, treatment. The sample is not normally prepared specifically for the theory which is expected to deliver the explanation of the phenomena. In the preparation of samples, "theory" enters only in a generic fashion, it sets the parameters of the stage which the prepared description enters as an actor and on which it will eventually become a well-defined character.

> Imagine that we want to stage a given historical episode. We are primarily interested in teaching a moral about the motives and behaviour of the participants. But we would also like the drama to be as realistic as possible. In general we will not be able simply to "rerun" the episode over again, but this time on the stage. The original episode would have to have a remarkable unity of time and space to make that possible. There are plenty of other constraints as well. These will force us to make first one distortion, then another to compensate. Here is a trivial example. Imagine that two of the participants had a secret conversation in the corner of the room. If the actors whisper together, the audience will not be able to hear them. So the other characters must be moved off the stage, and then back on again. But in reality everyone stayed in the same place throughout. [. . .] We cannot replicate what the characters actually said and did. Nor is it essential that we do so. We need only adhere "as closely as possible to the general sense of what was actually said".
>
> Physics is like that. It is important that the models we construct allow us to draw the right conclusions about the behaviour of the phenomena and their causes. But it is not essential that the models accurately describe everything that actually happens; and in general it will not be possible for them to do so, and for much the same reasons. The requirements of the theory constrain what can be literally represented. This does not mean that the right lessons cannot be drawn. Adjustments are made where literal correctness does not matter very much in order to

> get the correct effects where we want them; and very often, as in the staging example, one distortion is put right by another. That is why it often seems misleading to say that a particular aspect of a model is false to reality: given the other constraints that is just the way to restore the representation. (Cartwright 1983: 140)

"The requirements of the theory constrain what can be *literally* represented", and indeed, nonliteralness increases representational salience as one tries to remain "as realistic as possible". By transforming the phenomena into actors fit for a morality tale, physicists create a setting in which new conditions for literalness are set. The model is the stage where the productivity of capacities can be witnessed. It thus institutes a hypothetical "as if" condition where the "as if" does not signify fictitiousness but uses theory to create a perfectly real situation that is counterfactual only in respect to the ordinary course of natural events.

When Cartwright thus discusses 'Physics as theatre' (Cartwright 1983: 139), she refers to the theatre specifically in order to distinguish the "as if" of the novel from the "as if" of the stage where perfectly real events unfold in space and time. The difference between these two uses (and placements) of the "as if" operator makes for different hermeneutic processes.[17] A historical novel may refer to real agents and say of them that they behaved *as if* they were in rage, that is, it may treat their mental states as if these were accessible to us. At the same time, the meaning of the novel can be recovered only by means of interpretation (what is literally true of the novel is limited to the appearance of signs on the page). In contrast, the performance in the theatre of a historical play puts the "as if"-operator "all the way up front" (see Cartwright 1983: 129). A person appears on stage *as if* he were someone who acts in rage. Here, it may well be literally true that the stage action is a manifestation of rage.

This difference between novel or script on the one hand and the theatre on the other is due to the fact that the theatre is already a reader of the script (compare Nordmann 1996). The performance renders the text of the play as a score for the public exhibition of certain movements and events. Similarly, the model takes the theory as an occasion to exhibit certain physical occurrences. Performance and model are thus impersonal readers of a text (the script, abstract theory) by creating representationally salient (though not descriptively true) conditions of literalness: Theories cannot be literally true about the phenomena, but they can be true and false in the models, that is, in the setting in which these phenomena are prepared for the stage like actors. While this once again presents the "model as reader" and takes the hermeneutic situation of the theatre to exemplify the "construction of literalness," theatre and model are not just readers of script and theory but also of the world. Indeed, they mediate between the abstract and the concrete precisely in that, as readers of both, they establish their commensurability.

Theatre and model are readers of the world in the sense that they mobilize or prepare phenomena for the performance, that is, by making them speak. The model turns phenomena into stage-actors by giving them a setting in which they can perform and become eloquent. In *Nature's Capacities and their Measurement* Cartwright describes this setting as the causal structure which renders capacities salient. This setting is hermetic in that, like a text or theatrical performance, it offers no way out and is no longer transparent to the conditions of its creation. Just as the theatrical performance by an actress allows no direct inference to her private character, one cannot recover raw data from prepared descriptions or a biological cell from a slide. Knowing how samples or descriptions are prepared may give us some tools for reconstructing the original phenomenon but this reconstruction will remain speculative or must draw on circumstantial evidence in order to subtract the various effects of preparation.[18]

Like a theatrical performance, therefore, the model has more reality than what it ostensibly refers to—the reality "behind" it is just as derivative as the laws that are prompted by or extracted from it (compare Morrison 1999).

FITTING OUT: FABLES AND MODELS

In *The Dappled World* Cartwright shows that the model does more than fit the phenomena to a causal structure such that their capacities can perform and bring things about. The model also assimilates theory into its setting. Only in this setting, she argues, does the theory or do things like "force" concretely exist. In doing so, the model effects a further transformation. After the phenomenon has been prepared to act in the causal structure provided by the model, the phenomenon-qua-stage-actor now becomes a character in a play. After all, for the purposes of teaching a moral it is not enough that the phenomena are fitted to the task of displaying their capacities. The actors also have to be fitted out such that they are sufficiently stereotyped characters to convey the moral of a fable.

This final hermeneutic moment draws on the work of the eighteenth-century playwright, critic, and philosopher Gotthold Ephraim Lessing and his 1759 *Abhandlungen über die Fabel* (Lessing 1854; see Cartwright 1999: 37–44). Lessing is best known in the theory of the arts for his essay *Laocoön: On the Boundaries of Poetry and Painting*. By determining these boundaries, Lessing shows what is suitable for each medium of representation. For example, while the expression of Laocoön's pain is suitable to poetry and any art-form that develops its subject in time, only a sublimated attitude of suffering is suitable to sculpture and any art-form that freezes a moment for all time. Consequently, whether or not the historical Laocoön really wailed in anguish or suffered his pain with stoic nobility cannot be inferred from its representations in poetry and sculpture. Since these representations may sacrifice descriptive accuracy for the sake of realism, an inference from

representation to concrete historical situation would confuse formal and material modes (Lessing 1962).

Similarly, Lessing's treatments of the fable determine its constitutive boundaries in contrast primarily to allegory (see Cartwright 1999: 39): The fable's moral is not disguised or expressed by the fable, nor is the moral inferred from the similarity of concrete character in the animal or human world and certain abstract properties like strength of weakness. The grouse in the fable of grouse, marten, fox, and wolf is not merely similar to the weakest but *is* the weakest. Accordingly, the fable provides a story that instantiates the moral: The moral is couched in the story or the story fits out (*einkleiden*) the moral (Lessing 1854: 243, 255; see Cartwright 1999: 39). Of course, the grouse *is* the weakest only in the concrete situation provided by an ideal model, namely a situation that brings together only wolf, fox, marten, and grouse. And yet, though this situation provides a concrete instance of what it means to be the weakest, the meaning of weakness as an abstract property can be articulated also on the level of theory, for example by saying that the weaker are always prey to the stronger.

In her *Study of the Boundaries of Science*, Cartwright considers the relation between theoretical law and concrete model in Lessing's terms.[19] The notions of "work" or "force" do not exist on the level of theory, but theory can articulate the meaning of these terms, for example by opposing work and leisure on the one hand and by associating force with acceleration and mass on the other (Cartwright 1989: 40). To the extent that these are linguistic representations, it would be a categorical mistake to speak of the action in a model as being similar or dissimilar to the relation of terms in a theory[20]: Meaning is produced differently in the formal mode of theory (e.g., by way of definition or location in an axiomatic structure) and the material mode of the model (e.g., by instantiation, preparation, or mediation).

> Turn now from the Gascon and the fox to the stereotypical characters of the models which "fit out" the laws of physics. Consider F = ma. I claim this is an abstract truth relative to claims about positions, motions, masses and extensions, in the same way that Lessing's moral "The weaker are always prey to the stronger" is abstract relative to the more concrete descriptions which fit it out. To be subject to a force of a certain size, say F, is an abstract property, like being weaker than. Newton's law tells that whatever has this property has another, namely having a mass and an acceleration which, when multiplied together, give the already mentioned numerical value, F. That is like claiming that whoever is weaker will also be prey to the stronger.
>
> In the fable Lessing proposes, the grouse is the stereotypical character exhibiting weakness; the wolf, exhibiting strength. According to Lessing we use animals like the grouse and the wolf because their characters are so well known. We only need to say their names to bring to mind

> what general features they have—boastfulness, weakness, stubbornness, pride, or the like. In physics it is more difficult. It is not generally well known what the stereotypical situations are in which various functional forms of the force are exhibited. That is what the working physicist has to figure out, and what the aspiring physicist has to learn. (Cartwright 1999: 43)

Cartwright's and Lessing's dappled world is a product of work that is performed in a piecemeal fashion by exhibiting capacities in models, by rendering particular models as stereotypical situations that can teach a general lesson, and by sometimes managing to do both at once. Just as poetry and sculpture set their own rules of representation for the achievement of realism and thus claim Laocoon's suffering differently, each scientific discipline will constitute its domain by seeing what phenomena it can claim in the terms of its theories (compare Cartwright 1983: 13, and Cartwright 1989: 209). The success of science therefore cannot consist in the reduction of complexity or the unification of domains. Instead, it owes to the rightness or appropriate fit of particular causal accounts. If we are interested in descriptive adequacy, Cartwright argues, we are better off not caring 'about the tidy organization of phenomena'. Instead, we should be interested in how scientists are 'getting the causal story right. This interest 'is new for philosophers of science' (Cartwright 1983: 160, 162), as analytic philosophers have traditionally distinguished the goodness of stories from the rightness of knowledge. By asking what it takes to get a story right and thus to successfully mediate in particular cases the formal relations among abstract concepts and causal processes in the world, Cartwright confronts 'Physics as theatre' (Cartwright 1983: 139–142), the reconstruction of *Das Kapital* and Schrödinger's equations in terms of 'Abstraction and concretization' (Cartwright 1989: 202–212), and 'Fables and models' (Cartwright 1999: 35–48).

CONCLUSION

This survey of the three hermeneutic moments in Cartwright's books prompts again the opening question of how it can be justified to treat Cartwright's contribution in the terms of hermeneutics at all. It can only be part of the answer that this treatment afforded a reconstruction in Cartwright's own terms of her approach as a whole and that it thereby helped to clarify the broad outlines of this approach. As opposed to traditional philosophy of science, she does not provide formal reconstructions of causal stories but asks just what it takes to get the causal story right in the first place. Instead of taking as her paradigm of science just those cases where scientists traverse easily and successfully between abstract theories and concrete phenomena, she shows how these are the very special cases that are the hardest to understand. Similarly, she presupposes neither the impersonal knowing subject

of science nor the literalness of scientific language but shows how these are constituted only as phenomena are fitted to models and theories fitted out by models.[21]

This clarification of her project also indicates what further work may need to be done. In particular, to the extent that Cartwright helps undermine the notion that phenomena are constituted by theories or paradigms, the preparation of phenomena for scientific or disciplinary treatment ought to be distinguishable from their calibration to a particular theory with its particular formalism. In the "physics-as-theatre"-analogy this is the difference between training concrete individuals to become actors on stage and then fitting them out as stereotypical characters that can convey a moral. This distinction might be clear enough conceptually or programmatically, but it remains to be seen whether it can be used to tease apart what has become amalgamated at least since the time of Kuhn, namely the demands of a discipline and the demands of a central theory.

A second critical opportunity arises from Cartwright's reticence to distinguish between physically instantiated models (e.g., experiments) and conventionally formalized models (e.g., schematic and block diagrams). It needs to be shown how a block diagram, too, can provide the causal structure in which capacities can bring things about and in which we can see, for example, what lasers tend to do or how they tend to behave (Cartwright 1989: 226). This project gets help from two very different corners. On the one hand, it can be advanced by attention to the intermediate case of simulations in which schematizations take the place of experiments. On the other hand, one can now draw on hermeneutic conceptions of a "text": When Paul Ricoeur, for example, considers actions as a text, he does not take texts to be inert but appreciates their power to bring things about, and in particular to bring about a changed alignment of self and world (Ricoeur 1981). Even our ordinary language can be more and less finely attuned to concrete situations and the resulting, more or less conventional, verbalizations can afford or resist a seamless integration into a larger horizon of expectation and meaning. Just like Mill's theorist we can learn about causal capacities from the stories we tend to tell ourselves and not just from experiments. How we learn this, in each case, requires more detailed study.

These are the various heuristic benefits of taking Cartwright to suggest that scientific modeling corresponds to a hermeneutic process, and the approach can be justified further: In the course of reconstructing this process, the five initial objections toward any hermeneutics of science have become insubstantial. As for the first objection that the object of scientific inquiry surely must not be likened to a text, matters are obviously not as simple as that. Whether nature can be considered as a text depends on whether texts are thought to be inert and fabricated in the first place. In Cartwright's case, however, and in regard to the general discussion of the mediations in modeling, it is not so clear that "nature" is the immediate object of scientific inquiry at all. Instead, the model takes the place of the phenomenon—its

reading of the world provides the text for the general lesson that is to be developed.

As we have seen, Cartwright meets the second objection regarding the individualism inherent in the relation of reader and text by having the model stand in for the impersonal knowing subject of science. If the hermeneutic process consists in the integration of a text into a horizon of meaning, and if this integration requires a new alignment of reader, text, and world and thus changes how the reader relates meaningfully to the world, this process is now transposed into the model as the site at which these mediations take place. And in the literary as well as scientific case, "interpretation" is neither more nor less than attending to concrete situations and abstract concepts and fitting them to one another. This fairly inconspicuous empiricist notion of interpretation meets the third objection according to which the term should have a fundamentally different use in the contexts of science and literature.

The fourth objection maintained that by equivocating between the book of nature and the texts that are produced by scientists, hermeneutics fails to address the appearance of literalness and thus the decidedly antihermeneutic self-presentation of science. Nancy Cartwright shoulders this explanatory demand by showing how literalness emerges from a hermeneutic process.[22]

This leaves the final and perhaps most difficult question, namely whether scientific inquiry leads into a hermeneutic circle. Again, any answer depends on what, precisely, this notion is taken to mean. In van Fraassen's account, the hermeneutic circle appears nonviciously in the context of justification. Perfectly capable of absorbing into it our experience of an outside physical world, the circle merely indicates that observational content cannot be specified independently of theory and that the truth of a theory cannot be claimed on top of its ability to save the phenomena (van Fraassen 1980: Ch. 3, 5). As we have seen, Cartwright contradicts van Fraassen for the special case of causal explanation. It is here, perhaps, where her distinction between phenomenological or representative models and theoretical or interpretative models is most significant. As we have seen, each type of model constitutes a hermeneutic process of its own (fitting to and fitting out), and it is entirely nontrivial how these are fitted together in order to allow for scientists to traverse by way of these models back and forth between concrete situations and abstract theories. This suggests that Cartwright breaks out of the hermeneutic circle by positing various circles of interpretation that are in some measure external to each other.[23]

While appreciating central hermeneutic moments in Cartwright's philosophy of science, I have nowhere suggested that her account of science is derived from or even similar to any extant position in the hermeneutic tradition. Instead, I am suggesting that, without trying, she succeeds where most hermeneutic accounts have failed, namely in making sense of scientific activity as a hermeneutic process.[24]

NOTES

1. All the more so, as the author of this chapter would not describe his own interests or background as that of hermeneutics, either. The choice of the label reflects the difficulties not of understanding Nancy Cartwright's work but of accounting for its originality. The various contributions to the Konstanz workshop on 'Nancy Cartwright's philosophy of science' identified salient issues, but most treated these in terms of similarities and differences to a "received view" (as Cartwright herself tends to do, for example Cartwright 1999: 183). This chapter adopts the heuristic of setting her work quite apart by focusing on central passages in her three main works, which state her position in a germane and idiosyncratic fashion. As it happens, all these passages consider clearly identifiable hermeneutic situations.
2. This is the weakest of the various objections against a hermeneutics of science. It sets up as a straw man the hermeneutic notion of interpretation as if somehow it *must* mean more than an appropriate fitting of some input into a given context.
3. According to the constructivists, scientists 'do not take laws they have established in the laboratory and try to apply them outside. Rather, they take the whole laboratory outside, in miniature. They construct small constrained environments totally under their control. They then wrap them in very thick coats so that nothing can disturb the order within' (Cartwright 1999: 46).
4. Just like her concern with *ceteris paribus* conditions in *How the Laws of Physics Lie*, her discussion of the distinction between causal laws and mere generalizations serves Cartwright as 'a kind of ladder to climb out of the modalization programme, a ladder to be kicked away at the end' (Cartwright 1999: 169). This chapter attempts to characterize where she ends up after the ladders are kicked away.
5. Cartwright is quoting (1985) *The Economics of John Stuart Mill*, Oxford: Blackwell: 325.
6. For a non-theory-licensed concretization from a schematic diagram compare (Cartwright 1989: 225).
7. While Nowak's title promises a book on "idealization", Cartwright points out that, according to her terminology, he deals with abstraction (Cartwright 1989: 202; see Nowak 1980).
8. Also, just as models provide for the measurement of capacities, they measure or judge theory. Moreover—as remains to be shown—the two measures become commensurable in the model. The model can take on this productive task of producing commensurability because of the tension between the various functions of the model coupled with the aim of science to overcome this tension and establish as direct a link as possible between theory and the phenomena. The characterization of Mill's mixed method identified two different functions of models. To the extent that the model provides an idealized setting in which one gains acquaintance with a tendency, it supports the causal explanation of concrete phenomena. To the extent that the model results from the concretization of a materially abstract theory until the theory runs out, it instantiates an abstract relation that supports theoretical explanation. Already in *How the Laws of Physics Lie* Cartwright speaks of this 'tension between causal explanation and theoretical explanation. *Physics aims to give both*, but the needs of the two are at odds with one another. One of the important tasks of a causal explanation is to show how various causes combine to produce the phenomenon under study. Theoretical laws are essential in calculating just

what each cause contributes. But they cannot do this if they are literally true; for they must ignore the action of laws from other theories to do the job' (Cartwright 1983: 12) emphasis added.

9. In light of Cartwright's analysis one should say more precisely that Mill's theorist constructs a model in his own mind in order to manifest a tendency which he then observes.
10. It would appear that this idea assimilates Cartwright's view ever more closely to Margaret Morrison's notion of models as mediators or instruments, that is, of physical models that '*can* take on a life of their own as a way of mediating between technology, theory, and phenomena;' (Morrison 1998: 70). Mauricio Suárez identified three features of mediating models and added a fourth: (1) they are not derivable from theory, (2) they are not necessitated by empirical data, (3) they can replace the phenomena themselves as the focus of scientific research and thus become a quasi-autonomous source of knowledge, (4) they fix the criteria used to refine theoretical descriptions of the phenomena (Suárez 1999: 169–171). Cartwright doesn't speak of mediating models but distinguishes, instead, between representative and interpretive models. However, the *modelling* that on her account is done with these two kinds of models satisfies all four criteria of mediation.
11. From the point of view of the philosophy of science there is another way of formulating this achievement: Like Karl Popper's 'Epistemology without a knowing subject' (Popper 1972), Cartwright provides us with a depersonalized epistemology according to which '"p" says that p' (Wittgenstein 1922: 5.542). Unlike Popper, Cartwright develops the tools with which to analyze the mediations between theory, model, and world, i.e. with which to appreciate hermeneutic processes in science.
12. It is said that the hermeneutic approach equivocates between the scientific interpretation of nature and scientists' interpretations of scientific texts. However, this equivocation is part already of the decidedly antihermeneutic self-understanding of science. First and foremost, theories and hypotheses and descriptions and predictions are to be literally true of their object—they do not allude to, evoke, or illuminate nature; they do not enter into a dialogue with the world. Even when it is said that science reads the book of nature, this is not to be the kind of reading which effects a change in the reader who attempts to constitute symbolic meaning in the encounter with the text. Secondly and by the same token, the claims of science, including its so-called interpretations of data, are to be taken literally—they do not require interpretation by those who have learned to read them. Therefore science appears to be most successful where it manages to become entirely unselfconscious about its means of representation and where it establishes conditions under which nature itself appears merely to produce imprints, traces, or effects, i.e. where it leaves its mark and inscribes itself into our representations. This is the view according to which science 'reveals [. . .] directly the language in which the Book of Nature is written' (Cartwright 1999: 46). While analytically distinct, the two notions of literalness mutually support each other and only together achieve the ideal of unselfconscious immediacy of agreement between mind and world and among minds.
13. Compare Cartwright's discussion of the requirement of "contextual unanimity" in (Cartwright 1989: 143–148).
14. Cartwright endorses, for example, Leszek Nowak's claim that Marx's law of value applies to an economic system that 'resembles ideal gases, perfectly rigid bodies', that is, an empirical domain in which it is 'satisfied vacuously' (Cartwright 1989: 203).

15. Again, from the point of view of the philosophy of science there is another way of formulating this achievement: Following Wittgenstein, Thomas Kuhn offered a generic account of literalness as a construction that to the members of the scientific community does not appear to be constructed. According to Kuhn, membership in a scientific community requires the acquisition of a shared language. By learning to speak the same language, scientists become socialized into an interpretive community where agreement and disagreement about empirical matters no longer appears to involve interpretation at all (Wittgenstein 1958: remark 241). Again, Cartwright improves on this generic account by showing that this interpretive community should not be presupposed in our accounts of normal science but that the acquisition of a shared language and the training of scientists go hand in hand with concrete knowledge of the conditions of literalness for abstract theories (Cartwright 1999: 43).
16. "Fitting out" is introduced by Cartwright as a translation of the German word "einkleiden," whereas "fitting to" corresponds to the German "an-" or "einpassen" or "annähern".
17. Cartwright first discussed these differences in a seminar with Paul Grice on metaphysics in which 'we talked about pretences, fictions, surrogates, and the like' (Cartwright 1983: 129).
18. Of this reconstruction, Cartwright says it is an engineering task rather than scientific (Cartwright 1989: 211.)
19. 'Lessing said about his examples, "I do not want to say that moral teaching is expressed (*ausgedrückt*) through the actions in the fable, but rather . . . through the fable the general sentence is led back (*zurückgeführt*) to an individual case." In the two-body system [. . .] Newton's law is "led back" to the individual case' (Cartwright 1989: 44).
20. This is the point of Cartwright's *simulacrum* account of explanation: Theory is applied to the construction of the models and the similarity of the models with concrete situations is then determined or established (Cartwright 1983: 143–162).
21. See notes 11 and 15.
22. Moreover, the equivocation was seen to be endemic not to hermeneutics but to the scientific claim to literalness (see notes 12 and 15 above).
23. Such an investigation would probably show up differences between Cartwright's argument against van Fraassen in *How the Laws of Physics Lie* and her account of representative models in *The Dappled World*. By relying in the latter work on R. I. G. Hughes's notion of representation, Cartwright attributes to the representative model more clearly the characteristics of a hermeneutic circle (see Cartwright 1999: 192; and Hughes 1998: 128).
24. It would go beyond the scope of this chapter and the expertise of its author to relate Cartwright to the considerable variety of positions in the hermeneutic tradition (e.g., Gadamer, Heelan, Bubner, Ihde, or—to the extent that he wishes to be counted in—Hacking). A fairly general understanding of the hermeneutic project allowed me to identify the five obstacles to its applicability in the case of science and nature. Like the majority of Cartwright precursors and readers I am perhaps falsely assuming that I would have heard of a hermeneutic account that overcomes these obstacles and has yet something to say about the peculiar dynamics of scientific inquiry. Unlike most of her precursors and readers I am neither shocked nor surprised that someone firmly rooted in the "analytic tradition" of the philosophy of science has managed to do so. —I thank various critical readers of earlier drafts, especially Davis Baird and Jan Schmidt.

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Reply to Alfred Nordmann

Alfred Nordmann offers a hermeneutic reading of my accounts of theories, models and empirical success that I much welcome and for one special reason that I shall explain. Often the question arises, am I a scientific realist. It arises not least because I claim in *The Dappled World* that I had earlier wanted to attack realism—particularly the claim that our best scientific laws are approximately true. By contrast, in *The Dappled World* I take many of the laws as true—so long as we affix the right kind of *ceteris paribus* clause to them: The laws are true so long as the right kind of arrangement and interaction of capacities to generate them is in place and operates without interference.

Stuart Hampshire criticized me for this. Not for the case studies and the detailed lessons I draw from them. Nor for the strictures about warrant and trusting in what some one or another scientific group takes to be the dictates of "well-established" theory for a concrete case without a very great deal of different kinds of corroborating evidence. Rather he criticized me for indulging in questions of "realism" and for supposing it to be worthwhile to ask whether and how theory really describes the world. This is just the kind of metaphysics that he thought he and his colleagues at Oxford—Ayer, Ryle, Austen, Berlin, and others—in league, but naturally not in total agreement, with those elsewhere had left behind. Anglophone philosophy, he had believed, could never turn to them again, just as he thought that the ideas and commitments of the government of "the good Mr Attlee" were a turning point for Britain from which we would never turn back. Perhaps it is because of the too-close association of the political and the philosophical histories, and of my own work with Thomas Uebel and Jordi Cat on the linked shifts in political and philosophical thought in the Vienna Circle, and Peter Galison's work on *Aufbau-Bauhaus* that I have felt particularly shaken by Hampshire's criticisms.

Hampshire himself was no special friend of hermeneutics. Nevertheless I think that the hermeneutic reading that Nordmann proposes of my views show them in a light far more acceptable to the kind of in-the-world empiricism and particularism that we might ascribe to Hampshire, and that I would wish to emulate, than does the framing in terms of realism,

universalism, unification, simplicity, and the like familiar in contemporary philosophy of science.

Nordmann says that models, like performances, become impersonal readers of the text of abstract theory, and they create the conditions for literal truth and falsity of theory. But they are not just readers of the text of theory; they are also readers of the text of the world. As readers of both, models establish the commensurability of theory and "the world"—or better, the world as read through what I call "unprepared descriptions" but which Nordmann points out are already prepared 'for scientific or disciplinary treatment' though not yet calibrated to a particular theory, as are what I call "prepared descriptions" (Nordmann this volume: 383).

So why is talk of an "impersonal reader" better than talk of "realism", "fundamentalism", and "unity of nature"? Because it allows a description like the following from Nordmann:

> . . . there are no antecedent guarantees that they [models] will successfully coordinate theory and phenomena. Indeed . . . models may fail to concretize or realize theoretical concepts, and it may require a rather tenuous process to relate . . . models to the phenomena. . . . However, it is also possible for . . . models to be aligned or even coincide. In those instances, it becomes possible for scientists to routinely traverse in both directions between the abstract and the concrete. Cartwright rejects any philosophy of science that takes those cases as its paradigms and thereby ignores the work that is involved in relating phenomena, models, and theories to one another. . . . At the same time, whenever Cartwright considers in her own terms the movements back and forth between the abstract and the concrete, she arrives at what I here call 'hermeneutic moments'. At these moments the models are the stage on which the negotiations take place and on which the top-down and bottom-up approaches become calibrated to each other. Moreover, her hermeneutic characterizations . . . turn the model . . . into a device that interprets, measures, or reads phenomena and theory and that promotes the attunement of concrete and abstract properties.
>
> (Nordmann this volume: 372)

This seems to me an entirely apt and accurate description of what is going on and without any references to the world that are probably, on closer inspection, nonsense, as Hampshire suspected and Neurath certainly believed. We align theory and the world often through the process of simultaneously building the model, building the system it models—literally building, or shielding or substituting a different system with more agreeable characteristics (as in Gähde's account in this volume of Halley who took Jupiter to act only when on one side of the sun and not the other), as well as making the theory say what we need it to by exploiting the flexibility of the

mathematical representations and the looseness of the constraints for fixing physics descriptions. The models are the centre point at which the processes get aligned—as best they can.

Other views on models too can be happily rid of any metaphysical overtones they might have been ascribed and read with Nordmann's hermeneutical interpretation. For instance, his claim that 'it is not so clear that "nature" is the immediate object of scientific enquiry' echoes Mary Morgan's idea that in many cases models themselves have become the object of experimental enquiry (Nordmann this volume: 383). This is patent in the case of model organisms, like fruit flies and laboratory rats, and prepared systems, on slides and in test tubes. But it is equally true of the kind of fictional models that we make up and write down.

Nordmann highlights the hermeneutic elements in my story of how models become the objects that theory can describe and make predictions about; Morgan tells of how they become the objects of experiment. We experiment on the models and not on reality; indeed, it is hard to learn from models except by experimenting on them. Morgan's chief examples are from her own field of economics and from biology. But, it is true in spades of much of our contemporary mathematical physics where, Peter Galison tells us, mathematics is the new laboratory.

So I am happy to adopt the description of models as impersonal readers of both theory and the world, both for my own views and those of many others. And I especially embrace Nordmann's descriptions of science—really good science—that take us away from discussions of Truth, Unity, and Beauty, which I ought to have had no truck to begin with, to something far more modest: 'The success of science,' Nordmann tells us, 'consists in the establishment of a more or less local, more or less robust alignment of phenomena, models, and theories' (Nordmann this volume: 371).