

# Philosophy of Science in Action\*

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ABSTRACT: The article reviews Christopher Hitchcock's *Contemporary Debates in Philosophy of Science*, which aims to present contemporary issues in philosophy of science through a series of eight debates between leading analytic philosophers in the given specialist field. Each contributor argues for or against a proposed motion of the debate, ranging from issues of metaphysics and epistemology of science to specific philosophical questions in physics, biology and psychology. In that they draw on a wealth of techniques from the practice of philosophy of science – from conceptual clarifications to invocation of examples from scientific practice. Nonetheless, against the background of philosophical work associated with contemporary scientific practice the topics selected for this volume seem limited in both depth (the fundamental metaphysical and epistemological questions are not backed up by the equally fundamental scientific research) and scope (the 'contemporaneity' of scientific questions presented is not always up to scratch). The volume's great worth is in presenting 'philosophy of science in action' by exercising many tricks of the trade in several self-contained chapters.

KEYWORDS: Philosophy of science, contemporary science, epistemology, metaphysics, physics, biology, psychology.

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## Gluing Science and Philosophy Together

Though *Contemporary Debates in Philosophy of Science* is in many ways a synthesising volume, Christopher Hitchcock does not intend this to be an introduction to the (analytical) philosophy of science. As the title suggests, it brings together the authors from different parts of the field, paired against each other according to the topics they specialise in. Having said that, it is not intended to be a treat for a small group of buffs devoted to de-

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\* Review of Christopher Hitchcock (ed.), *Contemporary Debates in Philosophy of Science*, Blackwell Publishing 2004, 348 pp, ISBN 1405101520 (paperback).

lineating the precise boundaries of ‘information’ in genetics or the conceptual origin of time’s arrow in thermodynamics either. Hitchcock believes that philosophy of science chews, in a manner traditionally philosophical, traditionally scientific or both, on a number of well-known philosophical issues that arise time and again through scientific practice.

The value of the reviewed volume is not in that it illustrates them all, but in that both the methods and the issues are dispersed through the debates the editor judges to be the cutting edge of philosophy of science today. In that the ‘standard’ issues are often pushed behind the domain specific titles, but we get to witness the philosophy of science ‘in action’. Nonetheless, a more thorough synthesis (referring to particular issues discussed) than that provided in Hitchcock’s introduction to the volume would have better illustrated the importance of philosophical issues for science, as well as that of philosophy of science in action for understanding the world.

Whilst, in some contributions to the volume, traditional philosophical tools such as clarification of meanings and validation of arguments are at the forefront of the said debates, in others the authors borrow freely from both the strongholds of philosophy and contemporary sciences. Thus a debate may draw on traditional philosophical explorations of the relationship between mind and body (or more restrictively: brain), precise meaning of the terms employed in modelling of the mind; but also on empirical results in psychology, theoretical issues in evolutionary biology or freshly defined mathematical models employed by scientists to describe the phenomena encountered.

This way we get to see the ‘real’ philosophy of science in action, application of the philosophical methods to the issues arising from the scientific practice, but are not simply loaded with a lot of technical jargon and supposedly well-known points about the field. Each contribution to the debate is a reasonably self-contained argument, including the author’s overview of the nature of the problems and the clarification of concepts that the debate hinges on (along with detailed suggestions for further reading). Philosophically inclined readers may take delight in sniffing out just how such radically opposite conclusions can follow from sometimes seemingly identical premises, and authors indeed do not shy from using multiple tricks from the proverbial philosophical book to make that possible. In the course of that, some general points from the philosophy of science, or philosophy in general, are covered or explained, without opening up a debate topic of their own (thus, ‘philosophy of science in action’). Whilst on occasion some formal logical or mathematical tools are employed, the individual topics are generally free of the jargon of specific scientific disciplines, as well.

The issues of ethics, not only paradigmatically philosophical, but of interest to a great many readers (both lay and professional) are deliber-

ately left out of this volume following the assumption that they deserve a whole separate one for themselves. Hence, here we are left with ‘contemporary debates in epistemology and metaphysics of science’. As science itself is not a monolithic practice that the learned contributors could easily philosophise about, a selective approach is once again taken, guided by how *contemporary* the editor judges the topics to be. Those issues that are taught as part of standard undergraduate courses in philosophy of science (or have been taught last time I checked) are left to those courses and considered sufficiently (though by no means conclusively) explored not to be illustrative of the contemporary debates. In face of the possible dismay of buffs and interested novices alike, the volume has a purpose clearly indicated in the title and a necessary limit on the number of pages.

If anything is to be said concerning the decisions influencing the selection, it would not be that the debates included are not contemporary enough, but that they exclude some ‘very contemporary and very important’ issues that may be seen as rooted firmly in a single scientific discipline and thus mistakenly thought of as not sufficiently generally scientific enough. Fundamental research in science carries with it enormous philosophical implications, but to understand those a lengthier technical exposition (or alternatively a more rigorous synthesis) is required, demanding more from the reader as well as from the printing press. But in many cases in philosophy and science the devil is in the detail.

By focusing on the specific issues stemming from the contemporary scientific practice, the volume does not devote any debates to the wider philosophical issues related to overall scientific practice which should most certainly be addressed by philosophers, and philosophers of science in particular: the answer to sociological critique of the production of knowledge in science and the general legitimation of scientific knowledge (in contemporary issues of climate change or teaching of evolution in schools). These are sometimes mentioned in passing in some closely related debates and some pointers to further reading are provided. Though an institutionalised philosopher of science may shy away from grappling with them, the ‘uninitiated’ audiences may like to hear more about such problems from ‘contemporary experts’.

Finally, though each of the contributors provides an extensive bibliography and instructions for further reading, the editor uses the introduction to list a range of introductory texts to the philosophy of sciences (including its not so contemporary debates) as well as the ‘classics of the genre’ that all buffs should no doubt be familiar with. Should a novice chance upon Hitchcock’s volume first, it would do them well to consult the volumes in this list before grappling with the details of the contemporary issues. However, as such a studious approach may prove to be a

laborious task, potentially killing off any desire to ever form one's own opinion about e.g. the puzzling asymmetry of time, it is possible to work this way round and first get a taste for what is really contemporary at the intersection of philosophy and science and then slowly work one's way through the fundamentals (of both philosophy and science) behind. This, indeed, may be the greatest worth of Hitchcock's volume, a didactic shortcut, carved out by no less than true masters of their fields, to what is a gleaming tip of a largely submerged massive, structurally complex and in places dull iceberg. To scale it successfully one may need to be focused on the gleaming prize that sits at the top.

### **Are There Laws in the Social Sciences? Roberts vs. Kincaid<sup>1</sup>**

John T. Roberts and Harry Kincaid agree that an important segment of science is the potential to explain the phenomena observed and to predict the course of as yet unobserved phenomena. In Roberts' view this is achieved chiefly (though not solely, and he gives a good overview of the literature on this point) by subsuming individual phenomena under universal laws 'discovered' by scientists. Whether there are laws in nature waiting to be discovered, and whether scientific practice indeed is in the business of searching for those laws is to be a hot topic in this debate.

Analytically, Roberts 'reduces'<sup>2</sup> the concept of law to regularity plus the mystery ingredient X to be identified, and names the conditions the mystery ingredient has to satisfy. Needless to say, laws of physics satisfy these conditions and the ones of social sciences don't. When asking whether physics has laws Roberts disambiguates the question into three separate but related questions. There is a question of whether in the course of 'scientific practice' the physicists are actually in the business of discovering laws. Then there is also the question of whether there are any laws 'out there', in reality, waiting to be discovered. This is an important metaphysical issue: whether nature is such as to 'contain' any laws, or whether they are superimposed on it by human perspective (Humean supervenience).<sup>3</sup> Finally there is the question of simply looking for law-

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<sup>1</sup> John T. Roberts, "There are no Laws of the Social Sciences" (pp. 151–167); Harold Kincaid, "There are Laws in the Social Sciences" (pp. 168–185). Debates are not presented here in the order that they follow in Hitchcock's volume.

<sup>2</sup> To be precise, it is not a case of a clear-cut reduction to regularity + X, as regularity may be only entailed by a law, not necessarily contained in it as a constituent part.

<sup>3</sup> David Lewis describes his own view on natural laws and chance in nature as Humean supervenience; in Lewis, 1986. See also Loewer, 2001. For a contrary view see "Why be Humean?" in Maudlin (in print).

like segments of the contemporary successful theories of a given scientific discipline and declaring them to be the laws of a given science. Starting with a positive answer to the final question, Roberts answers the other two for the case of physics.

But in the case of social sciences he seems to take a different tack and attack the second question, the one of the metaphysical existence of laws in reality, first (thus disregarding what the contemporary successful social science theories may call laws). From this perspective (and with physics in mind) he believes that every concept of the social sciences, such as the social class or the economic demand, can in the end be reduced to concepts of physics and psychology, such as human bodies composed of atoms, or individual human motivations as derived from the individual's psychology. Even Humean supervenience seems to go out the window here, theoretical concepts of social sciences are just not fundamental enough, regardless of whether they are successful at the 'predict and explain' game.

This way one could reduce all other natural sciences to physics just as Roberts does with the social sciences. For the concepts of biology, chemistry or geology (or even psychology)<sup>4</sup> may in the extreme be reduced to physical concepts of fundamental material existents and their properties. On the other hand, if physics were approached starting with the question whether there 'really' are laws in nature it would be possible to deny that there are really any laws in physics either (some of the literature Roberts briefly reviews and suggests for further reading argues in fact for such a view, though Roberts disagrees). For it would be difficult to point to a feature of physical reality (especially if contemporary 'unobservable' physical entities were to feature in it, cf. the debate on unobservables, pp. 115–148), and be able to call it a natural law with confidence that extends beyond that of a well 'entrenched' empirical generalisation (cf. the probability debate, pp. 67–114).<sup>5</sup>

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<sup>4</sup> Something that neither of the sides in the debate on the structure of the mind in Hitchcock's volume ("Is the Mind a System of Modules Shaped by Natural Selection?", pp. 291–334) would argue for.

<sup>5</sup> This point may be further complicated by 'paradigm shifts' even in the exemplary scientific discipline – physics (Roberts does not call upon Kuhn, but his idea of scientific revolutions may be taken as a common cultural locus by now), including the changes of concepts and the laws they partake in. Of course, Roberts may try to invoke the stated counterfactual robustness of the natural science laws, claiming that the core of the law-like regularity does not change with the change in theory, but is merely 'refined'. Be that as it may, this seriously diminishes the pedestal that he places physics on in relation to natural sciences. For a metaphysics (of physics) that takes a non-Humean perspective on laws in fundamental physics seriously see Maudlin (in print).

Roberts concludes that social sciences are successful in the business of providing predictions and explanations (and thus might be called sciences), but do not achieve this by relying on laws (as laws are perceived from the paradigm of physics). Kincaid, on the other hand, argues that there are laws in social sciences which *function just like* the more familiar laws of physics (though they don't necessarily have the same structure), and that therefore social sciences are sciences in the same way as physics is. Moreover, he claims they share other similar features such as reliance on abstraction and the charge that all observation is theory-laden. However, even though Roberts' reduction may damage some traditional disciplines of the natural sciences, Kincaid's function based approach, if taken too lightly, can admit too much into science. One may argue, for example, that astrological 'laws' can also fulfil the function taken from the paradigm instances.

The function of the laws in the paradigm cases, according to Kincaid, is to explain and predict the observed phenomena. Skipping over the metaphysics, Kincaid says that when this function is fulfilled in an 'organised-enough' way then we have a scientific discipline and its law-like generalisations are the laws of science. This is in effect a perversion of Robert's approach to physics above, for it looks at the contemporary institutional practice first and uses it as guide to answering deeper normative and metaphysical questions. His case is certainly helped by expanding the scientific paradigm away from pure physics to include other natural sciences, such as biology (through examples of natural selection).

When he finally tackles metaphysics, Kincaid is keen to tack the existence of laws firmly to the issue of causation (which again takes us back to David Hume).<sup>6</sup> In that he neglects powerful criticism from Philip Kitcher that in science 'causal relatedness' is dependent on 'explanatory success' and not vice-versa (Kitcher, 1989). That is to say that the account of causal forces does not provide an explanatory success of a theory, and thus fulfil at least the desired function of providing an explanation, but the explanatory success (judged to be a success by some other means) makes the account of causal forces stick as a discovery of some true fact about nature/reality. On the other hand, Kitcher's ideal solution of explanatory unification seems to be heading in Roberts' direction of reduction of multiple sciences to a basic set of fundamental laws. It is regrettable that Kincaid does not take up this issue.

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<sup>6</sup> For a contemporary account of causation in scientific explanation, see Lipton, 2004.

### **Placing Trust in Unobservable Entities: Leplin vs. Kukla and Walmsley<sup>7</sup>**

For Jarrett Leplin the issue with unobservables is not whether they exist, but which of them do. Like most realists he is not swayed by the arguments from historical change of scientific theories, as most of the superseded theories' unobservables were not as rigorously tested as the current ones are. That, in his view, is the proof of a progress in science, not some major fault in its methodology. He undertakes to answer two major lines of anti-realist criticism of belief in the existence of the unobservable entities: (1) that all theories are underdetermined by observable data alone (and thus can't offer strong enough warrants for their theoretical entities as opposed to the competing theories' theoretical entities); and (2) that many unobservable theoretical entities have been abandoned by science in the past (e.g. phlogiston, crystal heavenly spheres and the like) so the current ones may be abandoned in the same way as well. The latter is answered along the lines given above. Though André Kukla and Joel Walmsley acknowledge such an answer, they will aim to show that a circularity is hidden in its reasoning such that a realist interpretation is presupposed from the beginning.

Leplin's general answer to (1) is (though, his is logically more precise, and indeed a fine example of command of contemporary analytical philosophical technicalities) that the supposed competing theories, underdetermined by the available observations as much as the original theory, share its fallible ontological and nomological commitments. The observations do not logically support the denial of existence of the theoretical unobservables of the original theory (and such denial cannot be a part of the competing theories), but merely support the theories with similar (though, not identical in every respect) commitments. This is where a more detailed discussion (including perhaps some quantification of the credential support provided by the observations to the theoretical entities) of the relevant/applicable/key properties those unobservables must display would have been more illuminating for those seeking philosophical support for belief in quarks, excitations of the different force fields, superstrings or genes.

Leplin's positive argument advocates selective confirmation of belief in unobservables, especially in the case of successful prediction of 'novel'

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<sup>7</sup> Jarrett Leplin, "A Theory's Predictive Success can Warrant Belief in the Unobservable Entities it Postulates" (pp. 117–132); André Kukla and Joel Walmsley, "A Theory's Predictive Success does not Warrant Belief in the Unobservable Entities it Postulates" (pp. 133–148).



phenomena,<sup>8</sup> some entities are more essential for the ‘functioning’ (cf. the debate on laws in social sciences, pp. 149–186) of scientific theories than others and those should be given special credence. The essential entities are those that serve a function in the operations of the theory rather than just providing assistance in visualising the related processes. Ether, absolute space-time manifold or igneous fluid, superseded theoretical entities, were in fact metaphysically superfluous as their function was easily fulfilled by the bare mathematical constructs from the formalism of the theory (though at least one of them keeps coming back time and again). Sadly, again, we are not given detailed examples of such essential entities (one is left to wonder what such entities would be in the case of quantum mechanics), but mere examples of successful historical predictions of novel phenomena (not necessarily based on the positive account of metaphysical construction) such as Einstein’s prediction of the gravitational deflection of starlight. According to Leplin, a theory’s sustained record of novel predictive success is only explainable by supposing that the theory has correctly identified and described the entities and processes responsible for the observations predicted.

Kukla and Walmsley have a fine-tuned logical bone to pick with such an argument, and this is the ground that the novice better avoid. They argue that to presume that explanatoriness (this is a term they introduce, meaning ‘the ability to provide an explanation, or even the best explanation’) is a good reason for belief in the entities and processes supposedly responsible for the observations predicted is a circular move in an argument that seeks to establish what are good reasons for belief in the very same entities (namely, the truth of their existence and ‘action’). They want to force the realists, such as Leplin, to show that explanatoriness of a theory’s entities and processes is a virtuous reason to believe in their existence. A novice, if he can stomach even this digested argument, as well as some buffs may wonder why the worth of explanatoriness isn’t patently obvious to Kukla and Walmsley, the anti-realists. But in the context of the rest of their essay, this may not be so surprising.

First of all, they force even the realists to concede a certain approximation to truth as applicable to scientific theories, rather than the straightforward truth of each of the theories’ statements. They then argue that it is possible to maintain a good approximation to truth whilst getting the ontological segment of a theory wrong, so that despite a successful predic-

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<sup>8</sup> He requires two general conditions of an observation for its predictions to be justified as novel: independence (the observation must not be an automatic a priori outcome of the logical structure of the theory) and uniqueness (viable competitors of the theory must not provide an alternative basis for predicting it).



tion, the explanatoriness of the entities is just not a good enough guide to truth. Yet, in doing so they never directly address Leplin's issue of novelty nor go head to head with him to show whether supposed essential entities can be so wrongly postulated. Secondly, they aim to show, following van Fraassen (1980), that the explanatory virtues of a theory are pragmatic virtues (such as ease of calculation may be) and not epistemic virtues (empirical virtue of confirming more hypotheses than not) that are needed for belief justification.

But where do we draw the line between what is real and directly observable and what is theoretically postulated and unobservable?<sup>9</sup> Kukla and Walmsley admit some arbitrariness in their answer but claim that is not a decisive point against a philosophical position. Be that as it may, the issue of realism in science seems to be too large for debate of this scope to handle, and after a lot of fine philosophical trickery which teaches us about the meaning of success of scientific theories and different types of entities and processes they may postulate, we are left with no clear guide whether predictive success (an important function in science) is sufficient to warrant a belief in electrons and quarks. The ball seems to be in the explanation's court and that is a whole other kettle of fish (intelligibility of theories is a matter of 'psychological acclimation' according to Cushing, 1991). *Philosophy* of science, indeed.

### **Do Thought Experiments Transcend Empiricism? Brown vs. Norton<sup>10</sup>**

Though focused fully on the thought experiments (thus being perhaps one of the few debates in the volume that stays true to its title), this is essentially the age-old debate between rationalism and empiricism, especially in the way they are applied to science. John Norton dismisses a number of proposed justifications for the 'trans-empiricist' view of thought experiments though he makes James R. Brown's the hardest to get rid of.<sup>11</sup> That is because it is a metaphysical issue that makes its way into an essentially epistemological topic. If one accepts Brown's Platonic metaphysics, then

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<sup>9</sup> Is, for example, what we can see through the microscope a directly observable entity or a mere optical illusion that is a product of instruments constructed in accordance with some theory rather than another?

<sup>10</sup> James Robert Brown, "Why Thought Experiments Transcend Empiricism" (pp. 23–43); John D. Norton, "Why Thought Experiments do not Transcend Empiricism" (pp. 44–66).

<sup>11</sup> Due to the longevity of the debate on the topic between the two contributors (each thanks the other one for the constructive opposition over the past 20 years) this is also perhaps the most head-to-head debate in the volume.

it is rationally appealing to accept his view of thought experiments as going over and above the principle of empiricism. Having said that it is still not straightforward, as Norton places some serious objections: the whole metaphysical baggage of Platonism is not really explicitly required by any thought experiment, and even if we accept that we have a special 'sensory' organ for perceiving things in the Platonic realm (where the laws of nature, as relationships between universals, essentially lie according to Brown) how can we tell when we 'misperceive' things Platonically.

And this possibility of 'misperception' is crucial for Norton's argument. Though he admits that thought experiments have proven more than useful in the history of science, he also warns that they have been used for all intents and purposes and cannot historically be said to have been a sure-fire guide to true theoretical insight (Brown, too, is aware of as much, and demands that only some thought experiments are insightful, and among them only some transcend empiricism). He presents three pairs of thought experiments with contradictory (i.e. opposing) conclusions, claiming that precisely through searching for clues of misrepresentation among them we can investigate more thoroughly the epistemology behind thought experiments.

In his view thought experiments are nothing more than logical rearrangements of what we already know (though are not always directly consciously aware of) from empirical observations (even regular everyday ones that make up a large part of the common sense). As the only consistent truth-preserving transformation available is to follow the rules of logic, thought experiments are in fact arguments with premises rooted in some empirical generalisation. If they are to be of use in the natural sciences they must operate on the existing scientific concepts that come from empirical findings. Norton refuses to acknowledge that there could be any other source of knowledge resulting from thought experiments.

He has, in fact, placed a great many years of practice into turning every thought experiment presented to him into an explicit argument structure, and is thus confident in claiming that he could do so with any thought experiment ever. Brown, on the other hand, claims to have shown at least one example from mathematics where the thought experiment provides something more akin to *seeing*, rather than deducing, as the source of believing (an epiphany of sorts) in at least half of the expert audience.

Norton proceeds to show that any attempted addition to 'memory of empirical findings + logic' is either falsely attributed to the given thought experiment, is included in the arguments already, is epistemically irrelevant (though may provide a 'picturesque' addition to the argument) or is epistemically unreliable as source of knowledge. Thus, he concludes

there is no possibility for *a priori* knowledge of the natural world, all our knowledge of it comes from the testimony of our senses.

Brown agrees with Norton's criticism of all other possible additions to the simple formula above, save for his own Platonism about universals.<sup>12</sup> For how could we have placed our trust in the outcomes of even the most profound thought experiments if we can't show them to be insights about something, and in some cases can't ever hope to be able to conduct them in a traditional experimental setting (thus providing a potential for eventual empirical corroboration). Thus Brown concludes that thought experiments incorporate the possibility of *a priori* knowledge of nature, they conjure up experimental situations that transcend experience and allow us the insight into hitherto unknown natural regularities without import of any further empirical data.

Brown wants Platonism to provide a metaphysical foundation for distinguishing the laws of nature (which Brown is a naïve realist about, unlike Roberts and Kincaid in the debate on the laws in social sciences) from the mere accidental generalisations and to make such laws independent of individual human subjects who discover (and/or use) them. Laws of nature then become expressions of real relationships holding between the abstract universals (such as numbers are in Platonist mathematics) that exist in the Platonic realm and are open to direct 'intuitive' perception. Thought experiments provide 'a mental setting' for such perception.

But Brown avoids confronting the objection of possible intuitive 'misperception', sticking firm to his claim that at least some thought experiments are not just arguments from purely empirical premises and that real-life examples show that, at least in some cases, they provide non-sensory insight into the laws of nature. Nonetheless, the overall debate does a great job of taking the extensive topic of rationalism vs. empiricism and narrowing it down, for the purposes of philosophy of science, to the crucial point of contention: does the use of thought experiments in science provide a counterexample to the wide ranging principle of empiricism, i.e. is it an example of use of rationalism in an otherwise empiricism dominated practice?

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<sup>12</sup> This is one aspect of the realism/antirealism dichotomy in science that is not mentioned in the "Can a Theory's Predictive Success Warrant a Belief in the Unobservable Entities It Postulates?" debate. It is also an issue that goes beyond the scope of science alone, and provides an example where scientific practice has to turn to philosophy for justification and clarification.

## **Are Causes Physically Connected with their Effects? Dowe vs. Schaffer<sup>13</sup>**

Phil Dowe argues for a conception of causation in which causes are physically connected with their effects. The issue in the debate eventually turns on how one envisages the basic concept of causation out of which the more technical accounts are constructed. Yet, the validity of technical accounts is often backed up by the ‘intuitive’ conception of causation as it occurs in numerous examples from various disciplines (from scientific disciplines to examples from legal practice). The basic concepts differ crucially in whether they admit negative causation or not, which in turn can be seen as a reformulation of a question whether causes are physically connected with their effects or not. So for good part of the debate the reader can’t be blamed for feeling this is merely ‘the chicken or the egg’ problem.

Negative causation is the observed correlation between an event not occurring and its ‘usual’ consequence not occurring either. So not throwing a stone at it ‘causes’ the window not to break. But in this kind of (seemingly perverse) causation we can hardly say that ‘not throwing a stone is physically connected with ‘window not breaking’. Though at first sight perverse, Goodman’s lessons with induction and projectibility teach us that such philosophical exercises can have serious consequences. And as Dowe himself warns there is an ‘epistemic blur’ between positive and negative causes: “‘Smoking causes heart disease’, but perhaps the actual effect of smoke is to prevent normal processes from impacting certain cells in a certain way, so that, in the absence of those processes, diseased cells prosper (causation by omission)” (p. 94).

So Dowe argues for a basic conception of causation in terms of conserved quantities linking a cause with its effect. Upon this conception negative causation (as straightforwardly illustrated in the window and stone example above, though, rest assured there are more complicated examples) is not an instance of causation, but something epistemically and practically (but, crucially, not metaphysically) similar: quasi-causation. But on such account of things causation is the primitive out of which the quasi-causation accounts are constructed. As it is usually difficult to distinguish between causation and quasi-causation (for we often have problems defining what is positive and what is negative among causes) people mix them up even in scientific practice, but Dowe thinks this should not be an excuse to do so in philosophy.

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<sup>13</sup> Phil Dowe, “Causes are Physically Connected to their Effects: Why Preventers and Omissions are not Causes” (pp. 189–196); Jonathan Schaffer, “Causes need not be Physically Connected to their Effects: The Case for Negative Causation” (pp. 197–216).

On the other hand Jonathan Schaffer argues that if negative causation is an instance of causation proper, and if it works as an explanation of the observed effects, then causes do not have to be (meta)physically connected with their effects. For his more pragmatic account to work he relies heavily on political and legal philosophy (or even the current legal practice) and a conception based on counterfactuals. But he also enlists a heap of negative causation accounts (which mostly work through removal of blockages to a more 'positive' view of a given situation) ranging from paradigmatic scenarios of positive causation to more technical philosophical constructions relying on causation (e.g. Kripke's account of naming [1980], Goldman's account of perception [1977]). In sciences, Schaffer argues, physical connection accounts require some physically expressible persistence to be connecting cause and effect, which is a conception that may work for billiard balls but not for biology (where privation is often seen as an important cause, e.g. in starvation or suffocation).

Schaffer duly admits his reliance on examples from practice rather than arguing for negative causation from first principles (as might be expected in philosophy). But even *a posteriori* observation of what actually exists in nature will not help in this issue as 'cause' is not a natural kind term (cf. Humean supervenience above), but a nomic one. He accuses Dowe of smuggling in two different senses of 'intuition' in his intuitive conception applied to adjudication of examples, as well as the physical connection view *ab ovo* by demanding that an effect have a single cause which is of necessity positive. Though he never offers a finalised and complete account of causation, Schaffer aims to sketch the outlines of an account which fits negative causation as well as positive (through examples of multiple physical connections which make no difference to the causal process).

Thus it transpires that negative-causation supporters start from 'meaning is use' principle to defend the claim that examples of causation without a positive physical connection are also instances of causation proper, and not some additional super-structure (quasi-causation). Their opponents, on the other hand, look for a metaphysical foundation first (that often being some satisfactory physical connection) and from then on build an account of causation that tries to explain many of the 'meaning is use' examples, whilst consigning others to epistemically deceptive quasi-causation. If in that, at least in the case of physics, they take some scientific theories (such as quantum mechanics) seriously as the support for the chosen metaphysical foundation they can step over the charge of Humean supervenience and take causal processes as building blocks of reality. But such conception of 'causation' can probably not be applied to 'less fundamental' sciences at all. Thus, the debate above calls for a reinvention of our common

language concepts on the basis of contemporary scientific theories. Now we just have to achieve a consensus on those (cf. Dummett, 1979 for general instructions; MacKinnon, 1975 for a structured attempt Maudlin, [in print] for a contemporary approach).

### **How Probable is Your Theory? Maher vs. Kelly and Glymour<sup>14</sup>**

Science is a disappointingly messy business, and a lot of the research results resemble the pronouncements of ancient oracles: they resist ready-made precise answers to our questions. In such climate many believe that empirical evidence can at least probabilistically justify the belief in the scientific theories. Based on the messy evidence available, some theories are at least more likely than others (lacking the outright confirmation due to underdetermination of theories by data). Bayesianism is a popular formal account useful for dealing with this claim. But Bayesianism is largely subjective (it formalises the rational reasoning of individuals in the face of uncertain evidence), and to be used for confirmation in (objective) science, this subjectivity has to be removed or circumnavigated. Alternatively, one may abandon any hope of fishing out remnants of certainty from the data and stick firm and hard only to those theories (if there are any as yet) unshakably proven by the data available, whilst respecting that future is always uncertain and that that should not pose worries for induction. According to Kevin T. Kelly and Clark Glymour “high confirmation provides no guarantee, or partial guarantee, of a smooth inductive future” (p. 112; for modifications of Bayesianism to serve epistemological purposes see also Williamson, ch. 10).

At its core, Bayesianism is a rationale that degrees of belief of an ideally rational person conform to the mathematical principles of probability theory. Respecting and explicating on this can resolve many problems of confirmation. However, an essentially individualistic nature of a single rational person poses a problem for a collective activity as science, or even more collective widespread belief in the epistemological outcomes of scientific practice. Furthermore, in conjunction with similar probabilistic accounts, it will also encounter (purely?) philosophical problems with Goodman projectibility and similar contraptions (Goodman, 1983). Maher approaches this problem in his defence of probabilistic justification from a strongly scientific perspective: instead of dealing with pros and cons of

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<sup>14</sup> Patrick Maher, “Probability Captures the Logic of Scientific Confirmation” (pp. 69–93); Kevin T. Kelly and Clark Glymour, “Why Probability does not Capture the Logic of Scientific Confirmation” (pp. 94–114).

Bayesianism he sets out to establish a new probabilistic account of justification (the predicate 'C'). Following a lengthy definition he collides his now formal concept head on with some traditional problems of confirmation and our intuitions about probability, at times bruising the new concept slightly and at others declaring the intuitions inadequate (for example he declares Hempel's paradox of the ravens [Hempel, 1965] misguided).

Yet, however robust the new concept may prove in dealing with exemplary confirmation issues, it still seems to only skirt the deeper philosophical issues of justification of the very concepts we project onto the natural world (such as natural kinds). Maher simply pushes aside as foolish any questioning of the familiar concepts (such as 'green' as opposed to 'grue') and ignores any bias our existing conceptual repertoire may place before selection of concepts with which to address the hitherto uncharted epistemological territory, such as is contained in new scientific theories. Thus a probabilistic confirmation of some hypothesis will have with it an inbuilt likeness for hypotheses with 'entrenched' concepts, complicating conceptual innovation. Maher readily admits this about his 'predicate C' account as well. As a final word of warning, the 'formally faint-hearted' should stick to introduction and conclusion of his exposition for it is almost mathematical in structure.

Kelly and Glymour do not object to Maher's approach to scientific confirmation but rather his (supposed) claim that it is *the* approach to scientific confirmation: confirmation and justification in science cannot be modelled by probability alone (and such technical justification formally provided is not justification enough). In fact the probabilistic approach (most commonly some form of Bayesianism) isn't even the first choice to approach science with as science aims at truth and the best confirmation approach for science is one that smoothly converges towards that aim through accumulation of evidence. Bayesianism on the other hand fluctuates wildly in assigned probabilities with progressive evidence accumulation. So Kelly and Glymour's money is on the slow and sure progress towards truth rather than the wild fluctuations Bayesianism brings with it. As their method for achieving the truth-aim they refuse to prescribe any preferred method; confirmation, they say, stands to science like proof stands to mathematics: it focuses the philosophical attention to a tractable subdomain of the overall practice, it is a call for philosophical scrutiny in relevant areas of individual problems and not some universal underwriter of scientific validity. Be that as it may, Kelly and Glymour still don't tell us anything about the conceptual apparatus behind the practice of confirmation (save for the implication that it may be as broad as the methods employed), or about the general issue of subjectivity in Bayesianism. But they tell us a lot about what the real practice of science is like, and why it



should remain so, despite proposals for universal formalisation of the approach to uncertainties of inductive future.

### **Explaining the Beginning of Time: Price vs. Callender<sup>15</sup>**

This, the most physics-related of all debates, perhaps the one most focused on fundamental metaphysical and epistemological issues in contemporary sciences, is emblematic of the volume as a whole. It takes a well contained issue in contemporary physics, probably not the most pressing one but one that wraps up nicely into a self-contained article (or in this case: debate) whilst touching lightly upon a host of challenging general philosophical issues. It also occasionally calls upon the contemporary physical theories, but does not engage with the philosophical issues associated with those.

Both sides of the debate agree that there is something special about the 2<sup>nd</sup> law of thermodynamics, as it is the only dynamical law that is time-asymmetric. It has a clearly inbuilt distinction between past and future, most popularly expressed as the increase of entropy from past to future. That would make the entropy of the beginning of the universe the lowest entropy (at least in this universe), and Huw Price thinks we have to ask why this is so. Craig Callender thinks this question can't be meaningfully asked as we can't hope to understand through science the *reasons* for conditions at the beginning of the universe (if universe is understood literally as *everything*).

Price starts off (rightly) with an exposition of Boltzmann's explanation of entropy increase in the future through sheer overwhelming likelihood of higher entropy microstates being responsible for the future macrostate.<sup>16</sup> But, from the perspective of just our current state, according to the same calculation/explanation it is also vastly more likely that the past microstates were also ones of high entropy (making all our memories of low entropy false records in our current state). Or, if flow of time is associated with entropy increase, it is vastly more likely that time flows forward from this very point (and our memories of it flowing in the past are just false records). In contemporary physics (relativity theory) the argument carries over to space as well, predicting disorder on a large scale.

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<sup>15</sup> Huw Price, "On the Origins of the Arrow of Time: Why there is Still a Puzzle about the Low-Entropy Past?" (pp. 219–239); Craig Callender, "There is no Puzzle about the Low-Entropy Past" (pp. 240–255).

<sup>16</sup> Don't worry about terminology here: the point is that higher entropy values are vastly more likely in the future than are lower entropy ones. A good introduction is given in Albert, 2000.

Yet, empirical discoveries of the 20<sup>th</sup> century point to the conclusion that the early universe (cf. Smoot, 1993) has a smooth matter distribution which is itself a very low entropy state given that the predominant force is gravitational attraction (clumping matter distribution into clusters), and mandating that our memories of yesterday are not false after all. The question can now be rephrased: why was universe initially so smooth? And there should be nothing mysterious about asking this question, as had it been about the ‘end’ rather than the ‘beginning’ of the universe there would be no debate at all. If it was just luck, how come luck stretches so far (whole of space) and continues for so long? Price dismisses a host of objections to attempts to provide a scientific explanation of this issue, most of them based on a Humean premise that such explanations only deal with regularities and there are no regularities of (the) everything (as there is only one such whole).

He dismisses inflation and anthropic strategies as satisfactory explanations for observed smoothness across both space and time (or whole of space-time) and opts for a metaphysically ‘heavy’ ‘Penrose’s Weyl hypothesis’ that effectively makes it a consequence of a fundamental law<sup>17</sup> of nature that (among other things) entropy be so low at one extremity of the universe, and that we call this extremity ‘past’. Now, Callender’s own solution is not far removed from this one, though the order of priorities is reversed. Callender wants to make the so-called Past Hypothesis<sup>18</sup> (cf. Albert, 2000) itself a fundamental axiom of science, requiring no further explanation but providing explanation for other (more complex) phenomena. He wants to add the Past Hypothesis as a new non-dynamical law of nature (in general agreement with Lewis’ Best System Approach; Lewis, 1994) rather than trying to explain it through more convoluted theories, or removing the argument that made it an abnormality in the first place (cf. Albert’s developments of the GRW version of quantum mechanics in Albert, 2000).

Thus, it seems both debaters agree once more, that without some addition to existing physics we cannot make sense of the low entropy past. We either need to expand the current physical theories and search for a more fundamental principle whose direct consequence would be a low entropy past, or we need to include the low entropy past among the fundamentals of our understanding of the world. Despite brilliant philosophical trickery once again put into practice (Callender opens his contribution with a thought-experiment-like story unrelated to past or physics) the verdict is

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<sup>17</sup> But as an as yet obscure law, requiring that Weyl curvature of space-time approaches zero at one extremity of the universe.

<sup>18</sup> ‘Past Hypothesis’ is simply a supposition that the early universe has low entropy.

once more with the reader. Do we (like Callender) find it objectionable to push the physics so far in searching for explanations to ask why there is something rather than nothing; or do we (like Price) find it cowardly to simply settle for brute facts about temporal asymmetry without at least trying (albeit trying hard and altogether in the future) to explain it in terms of something physically more fundamental and testable (Weyl curvature reinterpreted in terms of quantum cosmology)?

### **Is there Information in the Genes? Sarkar vs. Godfrey-Smith<sup>19</sup>**

There is a recurrent metaphor in biology (as well as in popular science reports) that there is information encoded in the genes, more precisely not just any information but information about the phenotypic traits observed at the level of organisms. But how appropriate is this metaphor? Sahotra Sarkar argues that, overlooking technical details, it is a powerful metaphor as genes code for proteins, which themselves are codes for phenotypic traits, so by transitivity genes code for the phenotypic traits as well. Peter Godfrey-Smith agrees that metaphorically genes provide information for (or code for) proteins, but the metaphor breaks down at higher level of organisation, so is effectively only useful for the simplest organisms. Nonetheless, both debaters agree that the metaphor should not be used excessively to result in some sort of genetic determinism ('it was in his genes to kill'). But, Sarkar thinks, fear of such misuse should not prevent us from relying on a metaphor that can further our understanding of the biological processes involved in reproduction and phenotype development.

Sarkar develops an extensive account of how to exactly specify the concept of information to be used in the metaphor; so that it is related to but not reducible to the mathematical concept of Shannon information (Shannon, 1948), but also not specifically referring to biology or genetics. He specifies conditions (differential specificity, medium independence, template assignment freedom) that make a relation between sets of data (be it nucleic acid chains, proteins, sound waves, electrical charge etc.) a *coding relation*. Following such technical exposition one is left wondering whether this the sense of information we started with and whether it is worth arguing for this notion of information at all, but to do that would be to forgo a method of conceptual clarification much valued in analytical philosophy in general (cf. Dummett, 1979 again).

Nonetheless, there are two stumbling blocks to Sarkar's enthusiasm, that he deals with by relying on more detailed understanding of biological

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<sup>19</sup> Sahotra Sarkar, "Genes Encode Information for Phenotypic Traits" (pp. 259–274); Peter Godfrey-Smith, "Genes do not Encode Information for Phenotypic Traits" (275–289).

processes to be achieved in the future. The first is his own admission that his account is straightforwardly valid for simple pro-karyotic organisms (mostly bacteria) only, whilst in the case of eukaryotes (complex organisms such as animals and plants) there is a host of other factors at play (such as the entire history of the organism's interaction with its environment). But the additional factors, though very important, do not satisfy Sarkar's criteria for coding, so if anything carries information about resultant phenotypic traits it is genes alone (everything else can just be seen as noise in the communication channel). A further problem lies in the fact that proteins are not organismic traits (i.e. shape, size, colour etc. are not determined by single protein each), but Sarkar hopes that future research will show that, at the level of individual organisms, all traits will be shown to be based solely on some (no doubt complex) protein structure. This way genes will (though, not straightforwardly) encode information about phenotypic traits (though not, as Sarkar warns, about someone's predetermination to kill someone else).

Godfrey-Smith delivers a self-confessed philosopher's attack on a scientific dogma, and as such doesn't share Sarkar's optimism. He doesn't think Sarkar's technical account sufficiently captures all the senses of the information concept, as it deals satisfactorily with Shannon sense of information (but that is just about mathematical correlations, and therefore not widely interesting), but not with representational (or signal) sense. The latter is part of a particular system of meaning conveyance through suitably coded messages. And whilst genes can be said to achieve this for most immediately produced proteins, the path from genes to phenotypic traits in complex organisms is much more convoluted and does not conform to simple message-code-message metaphor (there are various protein production loops and statistical effects of environmental influence at play). Therefore, in the representational (and according to Godfrey-Smith closer to common sense) sense genes carry information for immediately produced proteins and nothing else, and those will not provide sufficient basis for the reduction of observed phenotypic traits.

### **Is the Mind a System of Modules Shaped by Natural Selection? Carruthers vs. Woodward and Cowie<sup>20</sup>**

The final discipline-based debate comes from psychology and like the one from biology, the debaters agree on part of the claim from the title and disagree on the extent of the remaining part. They agree that the mind is

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<sup>20</sup> Peter Carruthers, "The Mind is a System of Modules Shaped by Natural Selection" (pp. 293–311); James Woodward and Fiona Cowie, "The Mind is not (just) a System of Modules Shaped (just) by Natural Selection" (pp. 312–334).

undoubtedly shaped by natural selection, but whereas for Peter Carruthers the structure that it eventually evolved to is modular Fiona Cowie and Jim Woodward think that the modular picture is far too murky and fails to explain how the mind conceptualised through introspection (the check for yourself moment!) works as a unified whole.

Carruthers admits that he cannot define the modular structure precisely at the outset, but relies on the (supposed) tradition in science of setting off with a vaguely defined notion that is sharpened through further research. His main argument rests on the finding that some things are learnt too early in childhood to be a result of a very general learning mechanism. Modules are a good model for this kind of channelled and facilitated learning, and these modules are part of the pre-wired structure of the mind rather than canalisation achieved through excessive learning. The latter is intended to explain the similarities in modularisation amongst individuals from different cultures and of different learning capacities. However, as Carruthers aims to stress early on, modules shaped by natural selection are not inconsistent with 'free' learning per se, and he offers succinct examples from both the animal world as well as human psychology to show such instances even in cases where flexibility in behaviour (usually demanding 'free' learning) isn't expected. As the final touch, Carruthers posits the language module in humans as the overarching faculty that brings the fruit of different specialised modules together into a unified whole: the mind capable of unconstrained thought. But he admits that showing exactly how this works will require a lot more research.

Cowie and Woodward disagree from the outset with the method (reverse engineering) employed by evolutionary psychology to identify individual modules and place them into an overall structure. The method follows the principle that 'form follows function', but in doing that it projects into an unknown territory of past adaptive pressures (guesstimating what the past environments demanded of the individual) and relies on a simple 'one function – one module' mechanism despite functions being nested in other functions as well as overlapping in their domain of applicability. Furthermore, if combinatorial explosion (so many possible 'flights of thought') is used as an argument for modularity (thus allowing expansion of possible combinations through combinations of specialised modules), it can equally well, according to Cowie and Woodward, be used against the 'central frame' (in Carruthers' case the language module) that coordinates the processing of different separate modules (cf. the 'Frame Problem' in the philosophy of mind). We are merely setting the stage for infinite regress of explanation of conscious processing.

They also see as a problem the separation of mind from brain that modular structure demands on the one hand, whilst on the other expecting

the modules to be a product of natural selection through genetics. Thus, despite seeing the place for modularity in explanation of some of the mind functions, they think it insufficient to explain the whole range of complexities of the human mind and the empirical evidence from disciplines other than evolutionary psychology. So they say: “The mind is not (just) a system of modules shaped (just) by natural selection.”

Despite starting off from a point where psychology meets philosophy (more precisely, epistemology) both debaters verge off swiftly into territory of psychological result reporting (and presentation of examples) and away from philosophical argumentation. The ‘arguments’ outlined above are interspersed amongst dense technical jargon and calling upon a range of scientific findings that can be used to support some claim or other, without ever aiming to synthesise them into a unified whole and pair them against one another. One is also left with a feeling that of all the debates on offer this one is least confrontational, in that both sides share a lot of common ground and differ only in the detail. The issue at hand carries with it an impression of being so cutting edge, so fresh, that even very little further reading (outside the bibliographies for sources cited) is provided.

## **Philosophy, Science, and a Book**

### EPISTEMOLOGY

The most prominent epistemological issue in philosophy of science is the one of empiricism vs. rationalism debate, namely what the source and justification of scientific knowledge is. In Hitchcock’s volume the issue is taken up in a more specific context of thought experiments, widely used in developing foundational physical theories but not directly subsumed under the standard view of the scientific knowledge production. Furthermore, despite importance of observational evidence in science one is still confronted with the issues of underdetermination of theory by data, as well as the historical vulnerability of scientific theories to being superseded by ‘better’ theories in the future. The role of probabilities (as opposed to certainties) in the confirmation of scientific theories (hypotheses?) by evidence is taken up in another. This further ties in with the issue of justification of theories (or more importantly for the given debate: universal laws) of social sciences by the empirical data collected. The problem here, as well as in a great deal of contemporary theories of natural sciences (especially the theories of physics which are often seen as somehow fundamental to all the rest), is in the role played by the postulated unobservable entities (cf. contributions to Hitchcock’s volume by Leplin, and Kukla and Walmsley, pp. 115–148), the ontological commitment of a theory.

## METAPHYSICS

The very debate on unobservable entities ties in with another group of traditional philosophical material, the questions concerning metaphysics. But metaphysics is also taken up in the debate on the existence of social sciences, this time through the discussion of the essence of law (as opposed to a mere scientifically inconsequential regularity) and its role in natural and social processes. A closely related concept of causation gets its own debate, focused on the specific issue of a physical connection between causes and their effects (though excessively loaded with supposedly intuitively appealing examples from non-scientific practices such as jurisprudence). A more encompassing problem of the metaphysics required by scientific explanation is referred to in more than one debate, whilst, sadly, none is devoted exclusively to it. Thus it appears in the debates on unobservable entities (one of their main purposes is to furnish an explanation for the observable phenomena), on the scientific status of the social sciences (as part of the wider debate of the role of science as explainer of the reality), on connection of causes and effects and on the possibility of physical explanation of the origins of time (and the universe).

## THE SCIENCES

From the perspective of individual scientific disciplines the selection of topics for the debate is much more straightforward (but also archaic): the traditionally fundamental physics, biology, psychology and social sciences get a representative debate each. In this respect the ‘philosophy of science in action’ as represented by the debates lags behind the current scientific practice of interdisciplinary probing of reality. Ecology, to name but one such, produces serious scientific advances whilst lacking a fundamental theory and calls for thorough philosophical investigation of its foundations. Contemporaneity does not get much hotter than this (cf. McMahon, Miller and Drake, 2001; Nicholls, 2006; for more references see also <http://online.sfsu.edu/~webhead/>; combining novel mathematical tools of network analysis with multi-disciplinary world view of ecology).

As admitted in Hitchcock’s introduction, there is a wide range of philosophical issues engendered throughout practice of physics. Whilst one debate focuses on thermodynamics specifically, other debates will readily help themselves to (supposedly) well known examples from other physical theories in arguing their point (e.g. the example of general relativity’s successful prediction of deflection of starlight or the conceptual problems of the motion of the centre of mass of the entire universe in Newtonian mechanics). Biology, largely irreducible to the ‘fundamental sciences’ of mathematics and physics is riddled with a range of philosophical prob-



lems, from issues of individuality, intentionality, questions concerning information. The one debate dedicated to a specific problem from biology concerns the latter (though the debate dedicated to psychology may be seen as stepping into the biological territory as well).

Psychology, historically perhaps the most recent of philosophy's contributions to science contributes a debate from a narrow field of evolutionary psychology (EP). The final science to get an exclusive mention is actually a group of them called the social sciences. The related debate is at the same time the most self-contained (from the perspective of a given scientific discipline) and fundamental one. It concerns the very justification for the disciplines in the group to be considered as sciences at all. In doing that it provides a thoroughgoing overview of the philosophical problems that plague them, from prediction through explanation to issues of *a priori* knowledge, though once again deliberately ignoring the field of ethics.

#### CONTEMPORARY DEBATES IN ONE VOLUME

Overall, as a collection of philosophical papers, Hitchcock's *Contemporary Debates in Philosophy of Science* has a use for a buff and a novice alike, by providing a quick and accessible reference to some of the most fringe topics in the contemporary philosophy of science. But for the novice it cannot replace a good introduction to the subject nor an encyclopaedic reference material (though it provides a good reference list for those). For the buff this may provide a solid and accessible foundation to her pet research topics in philosophy of science, but can also prove frustratingly short of a more thorough focus on any single issue (a sort of an oxymoron of an 'advanced introduction') whilst ignoring some of the specific issues (especially those closely tied with specialist sub-disciplines of the sciences) that the buff may have a professional interest in. Supposing that these are the most contemporary and telling issues in the sciences at the time of going to press, one may wonder whether the really important philosophical issues admit of the dimension of contemporaneity. Likewise, novel scientific practices (as well as more technical fringe fundamental research in traditional disciplines) invite the well known philosophical issues of the underlying epistemology and metaphysics. Some might claim that overall an 'age-old debates in the philosophy of contemporary science' would make a more informative and interesting volume.

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