a prominence in my account of Descartes' younger years. I stressed Descartes' quest for truth in these quarters because this has often been overlooked in the light of a narrowly rationalistic interpretation of his philosophy. I do not claim that Descartes was deeply influenced by Raymond Lull but I was struck by the fact that he was sufficiently interested in the art of memory to contrast his own approach with that of Lull's *Ars Brevis* in a letter to Isaac Beeckman on 26 March 1619, and to ask the same correspondent on 29 April whether he found 'anything ingenious in his art'. On 6 May, Beeckman replied that Lull's aim is 'to provide a brief summary of everything, namely, he divides everything in such a way that there is nothing that cannot be reduced to some part of a division'. I cannot be the first to have been reminded on reading this of Descartes' four famous rules of method in his *Discourse on Method*. The second reads, 'to divide each of the difficulties I examine into as many parts as possible', and the fourth, 'to make enumerations so complete, and reviews so comprehensive, that I could be sure of leaving nothing out'. I do not wish to imply that dialectics was not developed in other traditions but I have not come across the names of Peter of Spain, Lambert of Auxerre, or Agricola in Descartes' writings, but I have found references, beyond Lull, to protagonists of natural magic such as Cornelius Agrippa, Lambert Schenkel, Giambattista della Porta, Francois de Soucy, sieur de Gersan, and Tommaso Campanella. I believe Gaukroger raises the question of influence at a level of generality that can only be answered by appealing to the *Zeitgeist*. This explains too little or too much. I prefer starting from the writers that Descartes mentions but I do not wish to preclude looking for tacit influences that may have been at work. In this sense, I believe that many of the suggestions made by Gaukroger in his stimulating paper, 'Descartes' Early Doctrine of Clear and Distinct Ideas' (mentioned in endnote 7 to his review) are important and invite further research. I am even willing to entertain the hypothesis that Descartes was influenced by the rhetorical and psychological theories of Quintillian, although to date I have found no evidence that Descartes studied his writing at La Flèche or elsewhere.

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Seeing the Laws of Nature

By John D. Norton

James R. Brown, *The Laboratory of the Mind: Thought Experiments in the Natural Sciences*. London: Routledge, 1991. Pp. xi + 175. A$120 HB.

What lies behind this book is that brief moment of transcendent wonder that one can experience through a really good thought experiment. Only those who have never experienced that moment of wonder will have trouble empathizing with Brown's ambitious project. For him, that moment came when he first heard about a thought experiment of Galileo – 'I almost fell out of my chair', Brown recalled (p. viii). In *Two New Sciences*, Galileo's Salviati delivers an irresistible assault on the Aristotelian view that heavier bodies fall faster than lighter ones. He imagined a rapidly falling, heavy stone and a slowly falling, light stone. If they are joined together, Salviati asked the stooge Simplicio, will the slower retard the faster so that the composite body falls at some intermediate speed? Or will the still heavier composite body fall faster than either of its parts falling alone? With Simplicio now thoroughly bamboozled, it was quite easy for Salviati to proceed to Galileo's own theory that both heavy and light bodies fall alike. In a brief but intense conflagration, the Aristotelian theory is consumed and Galileo's theory rises from the ashes.

The task of Brown's engaging book is to understand the epistemology of this remarkable thought experiment and others like it. An old theory dies and a new theory is born, but no novel experiences have been called upon to underwrite the transition. From where can this new knowledge of the physical world come? Brown will not accept a simple-minded empiricist account that insists that thought experiments can tell us no more about the physical world than we build into them in the form of tacit or explicit assumptions. In this empiricist account, whatever information a thought experiment can give us about the world must come from experience

through these assumptions. Thus thought experiments can be no more than picturesque arguments squeezing unanticipated juice from superficially dry assumptions. In fairness, I must disclose that Brown correctly names me as a leading proponent of this simple-minded view which he plans to supplant. (See my 'Thought Experiments in Einstein's Work' in *Thought Experiments in Science and Philosophy*, T. Horowitz and G. Massey, eds, Savage, Maryland: Rowan and Littlefield, 1991.)

If we are to follow Brown, what sort of epistemology must we endorse? It is here that so many who play with thought experiments may falter. What Brown has seen is that no half measure will suffice. A coherent epistemology of thought experiments that seeks to replace a simple empiricist account must be a very radical one indeed. For thought experiments must now be portrayed as yielding information about the world that comes neither from direct sense experience nor from inferences from these experiences.

Brown's approach is radical in its extreme conservatism, casting us back into 4th century BC Athens. He adopts a Platonic account of natural laws. These laws really exist in some Platonic realm, just as chairs and tables really exist in our mundane world. And, just as we see tables and chairs with our eyes, so we observe the laws directly through certain special types of thought experiments. 'With the mind's eye, we can see the laws of nature' he exults (p.155). This provides Brown's explanation for why these thought experiments are so compelling and their conclusions irresistible. They give us *a priori* knowledge of the laws of nature themselves. Weary scientists need not be condemned to chase these laws eternally in the fleeting shadows of their laboratories. Through thought experiments, they can turn their faces to the light.

Even Plato was unable to convince his star student Aristotle of the reality of the forms. So we might well expect that Brown will have a hard time convincing us of his Platonic view of thought experiments. Fortunately Brown does not shy from the task. He approaches the problem from several perspectives. He argues (pp. 46-48) that my simple empiricism fails to do justice to some thought experiments, so that something else must be tried. (I do not agree. Simple empiricist analysis seems entirely able to handle every one of his examples.) Next Brown turns to his natural allies. Platonism is taken most seriously in mathematics and endorsed by no lesser figure than Kurt Goedel. There the doctrine urges that mathematical objects are as real as physical objects. Chapter 3 gives us a crash course in the subject and rehearses the standard objections and replies. Chapter 4 gives us a similar rapid tour of Brown's other ally, the new account of laws advanced variously by Armstrong, Dretske and Tooley within the last

fifteen years. In this account, laws are not just regularities of experience, as the empirical tradition has demanded. They are relations among universals (although it should be said that this modern view insists that laws are relations between *instantiated* universals). And it is a small step for Brown to reify these relations and allow them to be observed through the window of thought experiment.

The next aspect of Brown's case is really best described as a general strategy – or a sleight of hand, depending on your point of view. For millennia, critics have attacked the idea that there is a Platonic realm and that we have knowledge of its inhabitants. To deflect these attacks, following other authors, Brown talks of our access to this world as observation or perception, and draws an analogy to ordinary observation or perception, with its known problems. It is now a simple matter to translate an attack on Platonism into an attack on Platonic observation; then into one on ordinary observation; and then to dismiss it. For example, we know about sets, a knowledge the Platonist represents as perception. Thus to reject their reality is likened to a sceptic denying the reality of what we perceive through ordinary observation (p. 63). Critics complain that the actual process through which we are supposed to grasp or apprehend the Platonic world is completely mysterious. Brown responds that good parts of the ordinary perception of everyday objects remain mysterious to us (p. 65). Again, when it is complained that this Platonic perception involves no causal connection between the perceiver and the Platonic world, Brown replies by pointing to a much discussed, pathological example of quantum theory. An observer on one wing of the quantum mechanical Einstein-Podolsky-Rosen thought experiment can know the outcome on the other, although special relativity rules out the possibility of causal signalling and the statistics rule out a common cause (p. 73).

This strategy is used most successfully to answer what I believe is a very serious problem for the Platonist view. If thought experiments can give *a priori* knowledge by perception of laws, why is it that so many give incorrect results? On Brown's view, thought experiments give us an odd form of knowledge, the fallible *a priori*. It is a vexing problem, Brown retorts, but our ordinary senses can be deceived as well (p. 55, pp. 92-3). Elaborate inferences from ordinary experience can also deliver impressive cases for falsehoods such as the phlogiston, caloric and aether theories, yet we do not doubt the reality of the objects of ordinary perception. This recovery from an apparently fatal difficulty is brilliant.

However, in the final analysis, the degree of success of this strategy depends on how closely we are prepared to accept the grasping of the Platonic laws as analogous to ordinary perception. My view is that the

analogy is not close enough. We understand ordinary perception well enough to understand why it fails when it does and to know when it is likely to succeed. The mechanism of Brown's perception of Platonic laws is so completely unknown to us that we have no understanding of why it fails when it does and we have no idea of when it is likely to succeed. Were ordinary perception so unreliable, I doubt we would have much confidence in it.

In addition to arguments for the Platonic view, Brown's book contains other material. He gives a fairly simple taxonomy of thought experiments in Chapter 2. Brown characterizes only a very special subset of thought experiments as Platonic. These are the ones that destroy one theory and simultaneously allow direct construction of its replacement, such as Galileo's falling stones thought experiment. I found this whole discussion to be one of the least satisfactory in the book. The taxonomy seems quite arbitrary and I could see no reason for selecting just the destructive/direct-constructive thought experiments as Platonic.

The final chapters 5 and 6 are described as investigating consequences of Brown's theory of thought experiments (p.155). However, they are more independent studies that happen to treat thought experiments. Chapter 5 deals with Einstein's use of verificationism. Its most interesting thesis calls into question the traditional view of Einstein making a slow pilgrimage from Machian empiricism in his early life to a realism in his later life. In its place, Brown offers an intriguing and ingenious view of an Einstein with a more static philosophical outlook. He was a verificationist when dealing with what Einstein called principle theories, such as special and general relativity. But he was a realist when dealing with constructive theories, such as quantum theory. Brown claims that the focus of Einstein's philosophical pronouncements shifted from relativity to quantum theory in the course of his life. Thus his philosophical outlook merely appeared to shift as the locus of his pronouncements changed.

Brown is surely correct in criticizing the developmental picture of Einstein proceeding from a simple positivism in his early years to a simple realism in his maturity. However, Einstein's philosophical views were often elusive and even opportunistic. Just as the simple developmental picture oversimplifies them, so does Brown's static picture. There clearly were some very major shifts in Einstein's philosophical outlook, even as applied to the principle theories of relativity. In his 1933 Herbert Spencer lecture he gave us the decidedly non-verificationist proclamation 'In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed'. His first example was the generation of the source free field equations of general relativity as the mathematically simplest law.

This is a very significant change in attitude from 1912 and 1913. Then his refusal to pursue this canon of mathematical simplicity set him off on a three year wild goose chase. I would have expected this episode to be of special interest to a Platonist. The experience was decisive for Einstein and led him to turn away from physical intuition and towards mathematical simplicity as the dominant heuristic in theory construction.

The final chapter is devoted to the current problems of quantum theory. It drives towards an audacious proposal in its closing pages that could only be the work of the most optimistic of believers in the Platonist dogma. A generation of philosophers and physicists have grappled with the problem of understanding the correlations that arise in Einstein-Podolsky-Rosen experiments – and can reach no consensus. Standard views of causation fail provably. There is no common cause for these correlations. Brown has a cause, however. In his words, 'Distant correlations are caused by the laws of nature' (p.152). The reasoning behind this claim is apparently quite simple. The Platonist holds that laws are as real as ordinary physical objects and therefore as real as ordinary physical causes. So why should we not allow them to act causally in just the same way?

Setting aside my complete scepticism about the Platonic world, my feeling is that we cannot allow it if we wish to retain even the vestiges of the usual understanding of the term 'causation'. In this refined scientific context, I am only able to speak of A causing B when I know that A and B are governed by some law and only after that law imposes a particular type of connection on A and B. Thus the correlations of the EPR experiment are law governed. But that is not enough. After we examine the regularities these laws impose, we discover that they are not causal. Let us apply these same requirements to Brown's proposal. What sort of law governs the connection between the Platonic laws and phenomena in Brown's account? Under what criterion is this connection causal? Brown gives us no account of these meta-level laws. So we cannot even begin and we are very far from being able to test if the connections they sanction are causal in anything like the sense sought. In particular I should want to see why we should accept that the laws of quantum mechanics cause the EPR correlations, but presumably we should not accept that the relevant classical laws could cause the corresponding classical correlations. For these latter correlations, we already have fully acceptable physical causes, so that further causes would be superfluous.

In sum, Brown has given us a bold work. His readers will enjoy his light and engaging style of writing, which makes the book inviting and entertaining. However, this ease comes at a price. All too often Brown glosses rapidly over details that I felt needed careful attention. In particular,

the gloss of his core thought experiments are often historically quite inaccurate. However, when so much work in philosophy of science advances carefully hedged theses of utter scepticism in which all knowledge is delusion and organized science conspiracy or self-deception, I find it especially welcome to be told that scientists do actually know something and that science is not just an elaborate work of fiction. The assurance is comforting, but we do need to work a bit more on the details.

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Author's Response

By James Robert Brown

In his review, John Norton does an excellent job of describing the content and even the spirit of my book on thought experiments. His own well developed empiricist account of the topic[1] is about as far removed from my platonic rationalism as anything could be, so he is naturally quite critical of my main claims. While writing *Laboratory of the Mind*, I had him more than anyone else in mind as someone to worry about and so I spent a few pages either directly attacking his empiricism or trying to head off his inevitable objections. Alas, he remains unpersuaded. I suspect that in time the rapidly growing literature on thought experiments may come to regard our respective views as the polar positions, with new contributions trying to carve out intermediate views. This was the case, for example, at the recent Philosophy of Science Association meeting in Chicago (October 1992) where participants David Gooding, Nancy Nersessian, and Ian Hacking would have no truck or trade with either my platonism or with Norton's argument account.[2] But here I'm in sympathy with Norton when he says it may be an 'all or nothing' matter; it's the polar views which are the more plausible ones.

Norton's own view of thought experiments is that they are really arguments (possibly disguised). They start from empirical premisses and finish with a conclusion which is inferred using the laws of deductive or inductive logic. Everything else in the thought experiment is just fluff which might play a useful heuristic role. I have tried to meet this claim by

giving examples where the knowledge acquired in a thought experiment is more like an observation and only very implausibly construed as an argument. Space prevents my going through these in detail; readers will have to see for themselves. (Those interested might also wish to see my more recent essay on the topic and Norton's defence of the argument view.[3])

It is often claimed that 'seeing' platonic objects is mysterious, perhaps even impossible. Much of my book is spent trying to undermine this class of objections, and Norton is kind in his appraisal of this project. But in the final analysis, he will not accept the analogy between sense-perception and the perception of abstract entities: 'We understand ordinary perception well enough to understand why it fails when it does and to know when it is likely to succeed. The mechanism of Brown's perception of Platonic laws is so completely unknown to us that we have no understanding of why it fails when it does and we have no idea of when it is likely to succeed. Were ordinary perception so unreliable, I doubt we would have much confidence in it.' I suspect that many readers will share this opinion. But let me ask, 'when did we come to understand ordinary perception well enough?' Was it in this decade? In the 17th Century? And were people justifiably sceptical of their sense-perception *before* that historic discovery? Like any other sensible person I'm very happy to accept current theories of sense-perception, which tell us that (in the visual case) photons are transmitted from the physical object, they interact with rods and cones in the eye, a signal is sent down the optic nerve into the visual part of the brain and so on. But how this translates into *belief* is still a very deep mystery – in fact, it's just the mind-body problem. We are not *more* ignorant of our perception of abstract entities than that. After all, we weren't expecting anything like photons anyway; it's just the question of how we get the *belief* that matters, and here the mystery – though exceedingly deep – is no deeper than in the case of ordinary perception.

In an EPR thought experiment or in an actual experiment of an EPR-type we can stand at one wing of the apparatus and know what the remote measurement outcome will be. Relativity prevents any direct causal influence letting us know this outcome, and the quantum statistics prevent there being a common cause in the past that is somehow responsible for this piece of knowledge. *And yet we know*. I find this example stupendous. As far as I know, previous commentators have focused on the outcomes of EPR and Bell setups; none have worried about our *knowledge* of those outcomes. Yet our knowledge of the remote outcome is truly amazing, for (among other things) it clearly refutes any theory of knowledge that requires a physical causal connection between knower and known. I try to explain the knowledge by positing abstract laws of nature that we are somehow in

contact with. But while we're at it, why not let the laws do something more? Let's have them cause (and hence explain) the actual outcomes as well as our knowledge of those outcomes.

Norton finds this extremely far fetched and asks rather embarrassing questions about the causal connection between laws and physical events. I realize that my woolly views on this topic make me an easy target, but I certainly don't want to hold anything like the view Norton rightly ridicules. The view of causation he implicitly holds (and foists on me) is causation between events. This is fine for the physical world, but clearly abstract entities (being outside space and time) are not events, and so cannot be thought of as events that cause the EPR outcomes. (I might add that if we did try to think of them this way we'd need super laws to link laws to physical events, then super-super laws, etc.; a new version of Plato's third man regress.) I think of the causation of events by laws of nature as a kind of structural constraint on events, but I confess to not knowing what else to say about it now. Figuring it out remains a current project.

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1. J. Norton, 'Thought Experiments in Einstein's Work', T. Horowitz and J. Massey (eds), *Thought Experiments in Science and Philosophy*, Rowman and Littlefield, 1991. Though only recently published, Norton's paper has been in circulation since 1986. I should like to take this opportunity to thank SSRHC for its generous support of my work.

2. Their papers will be published in *PSA 1992*, D. Hull, M. Forbes, and K. Okruhlik (eds), forthcoming. Another 'intermediate' work is the recent book by Roy Sorensen, *Thought Experiments*, Oxford, 1992.

3 J. Brown, 'Why Empiricism Won't Work', *PSA 1992*, D. Hull, M. Forbes, and K. Okruhlik (eds), forthcoming, and J. Norton, 'Are Thought Experiments Just What You Thought?', forthcoming.

Creationists

Ronald L. Numbers, *The Creationists: The Evolution of Scientific Creationism*. New York: Alfred A. Knopf, 1992. Pp. xvii + 458. US$27.50.

By Christopher P. Toumey

On a hot and steamy Monday afternoon in July 1925, the Champion of Scientific Truth battled the Arch-Inquisitor of Obscurantism. When they met on a wooden platform on the courthouse lawn in Dayton, Tennessee, a modest Southern hamlet on the muddy banks of the Tennessee River, Clarence Darrow and William Jennings Bryan personally assumed all the historic and philosophical burdens of evolutionary and anti-evolutionary thought. The entwined destinies of these two belief systems were to be decided by their debate, which took the form of Darrow cross-examining Bryan. This microcosmic struggle between incompatible values was not pretty, but it was decisive. After humiliating Bryan in a few hours of bloody work, Darrow discredited creationism for all time.

Such is the usual fiction about the Scopes Trial and the history of American creationism: a simple tableau of Good and Evil. The credibility of that little just-so story is predicated on two assumptions. First, we must accept that science and religion are linked to each other in a zero-sum tension, so that the rise of science requires the demise of religion, or at least of conservative religious belief. This way, the story of the Scopes Trial fits into the same tradition as the Oxford Debate of 1860 between T. H. Huxley and Samuel Wilberforce, plus the persecution of Galileo and the suppression of the works of Copernicus.

Secondly, one must think that the great struggle between Darwinism and creationism was satisfactorily resolved in that long-ago summer. Supposedly Darrow dismantled creationism as surely as a vampire-hunter put a stake through the heart of Dracula, so that now we have the luxury of bemused retrospection, uncomplicated by resurrected creationists.

Both of those assumptions seemed entirely plausible four decades ago. The popular credibility of Darwinism appeared secure when the

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