

New Books

Human Well-Being and the Natural Environment

By Partha Dasgupta

Oxford and New York: Oxford University Press, 2001;

ISBN 0-19-924788-9

Environmental economics has its limits. The estimate of the economists who valued the world-wide flow of environmental services at 33 trillion dollars was, as Partha Dasgupta says, meaningless, since without these services there would be no life (human or nonhuman), and no recipients for this sum in exchange for the life-supporting services foregone (p. 138). Perhaps he should have said 'no life on Earth'. But even if terrestrial life-forms escaped to another planet, they would have little use for the dollars unless they found a destination which had so evolved that matching life-support systems were already in place (just about as likely an outcome as having your planet and eating it). Rather, it is with environmental changes that environmental valuation and evaluation begin to make sense, or so Dasgupta maintains. (But is not a recognition of pricelessness itself a case of environmental valuation?)

His main aims in this book are to relate issues concerning the natural environment to the reasoning of economics and to explain these matters to students and to professionals of other disciplines. His success in the latter regard is uneven. Considerable tracts of the book are accessible and illuminating, and numerous further passages become intelligible through (for example) the one-line footnote explaining the summation sign (p. 91); besides, certain 'technical' chapters are starred so that the discerning reader can skip them. Yet the 'proof' of the impossibility of Paretian liberalism, admittedly within a starred chapter, equips symbols with no less than three kinds of hats without explanation, as if the intended readership has been forgotten, and one of the main aims of the book therewith. More generally, conscientious readers should not spend too long decoding formulae and equations, many of which are thrown in playfully and later retracted, rather in the manner of Alvin Plantinga setting out misleadingly non-viable versions of the ontological argument as an exercise for graduate researchers.

Much more memorable is Dasgupta's remark made during a discussion of basic needs: 'To regard the medical and teaching professions as suppliers of marketable commodities to 'consumers' is to practise bad economics' (p. 49). Here, however, I focus on intergenerational issues, Third World matters, contingent valuation, sustainable development, and population ethics.

When discussing intergenerational well-being, Dasgupta eventually comes down in favour of discounting future costs and benefits. Initially (p.

New Books

95) he suggests that Koopmans has shown that positive discounting is indispensable. But through adding (p. 96) that Koopmans's approach could in some circumstances involve future consumption declining to zero he appears to cast doubt on this suggestion. However, he later argues (p. 98) that the implication of an eventual decline to zero is an inconclusive objection. For if the discount rate is small enough, the decline might not occur for a billion billion trillion years, by which time there will probably be no human life on Earth; hence discounting cannot be objectionable in principle. Dasgupta later argues (chapter 11) that discount rates should be variable, and should vary with relation to expected rates of output for different future periods.

But if the discount rate is so infinitesimal as to be negligible, discounting fails to make the practical difference that its advocates (who customarily peg rates of discounting to something like the current rate of inflation) are typically seeking. While Dasgupta's argument really does show that discounting is unobjectionable in principle, it does nothing to uphold Koopmans's generic defence of discounting, which is neutral between rates that are negligible and significant rates. The objection to discounting at significant rates remains that it makes the costs and benefits of a century hence count for virtually nothing, and hence represents as unobjectionable actions in the present that would release (say) toxic substances on the people of several centuries hence (as Richard and Val Routley once argued).¹ The counter-argument that the people of that period will also probably be enriched as a result of the fruits of the same present acts of investment seems not to offer sufficient or relevant compensation. Subsequently (p. 102) Dasgupta seems to claim that the remote possibility of human extinction, albeit at some unpredictable future date, supplies a reason for discounting future interests, although he may not wish to endorse this notion. But in any case this consideration supplies no ground for discounting at significant, non-negligible rates. The Routleys' arguments against actual (significant) discounting continue to stand, as do those of Parfit.²

Dasgupta later (pp. 105f) returns to the defence of discounting, remarking (though once again with reservations) that if the rate of discounting itself decreases towards zero prior to the distant future, there would be no serious jeopardy for generations of that part of the future. However, serious implications remain for the people of the period of between thirty years hence and whenever the distant future is taken to begin, as long as a significant discount rate (anywhere near the rate of inflation) is set for the *non*-distant future. Neither this defence nor the subsequent advocacy of variable rates of discounting suffices to rescue discounting as a practice.

¹ Richard Routley (later Sylvan) and Val Routley (later Plumwood), 'Nuclear Energy and Obligations to the Future', *Inquiry*, 21, 1978, pp. 133–79.

² Derek Parfit, 'Energy Policy and the Further Future: The Social Discount Rate', in Douglas MacLean and Peter G. Brown (eds.), *Energy and the Future*, (Totowa, NJ: Rowman & Littlefield, 1983), pp. 31–7.

Indeed Dasgupta himself decries as unjustifiable the current practice of setting discount rates at a constant level, and the variable-rate discounting which he advocates as justifiable (pp. 188f) is at best an innovatory procedure that could easily generate environmental hazards, and thus confront the precautionary principle (see below).

In a context of discussing uncertainty, Dasgupta (p. 103) puts forward Bewley's account, which justifies reluctance 'to undertake activities involving inestimable risks', and suggests that this is a version of the precautionary principle, adding however that we have no acceptable theory for coping with uncertainty. Standard versions of the precautionary principle, however, are less broad and less conservative, requiring intervention when there is reason to believe, even in the absence of scientific consensus, that serious or irreversible harm will otherwise take place. Unlike Bewley's theory, such versions would not frustrate most departures from the status quo, and would have a stronger claim to acceptance for the narrower but crucial sphere of cases of uncertainty that they address. The uncertainty of the near future has a more direct bearing on Dasgupta's text; the news of the resuscitation of the Kyoto Protocol must have come too late to prevent it being called a 'failure' (p. 186).

Dasgupta brings out well the limitations of markets with regard to environmental resources (p. 109), the widespread importance of community management of common resources (pp. 110–4), and also the often adverse effects of government policies such as the Brazilian subsidy for acquiring forest land and deforesting it (p. 114). Less reliable is his citation of Vitousek's claim that 40 per cent of the net primary production of the biosphere is currently being appropriated for human use (p. 118); Mark Sagoff has shown this estimate to be seriously exaggerated.³ Sagoff has also, as it happens, cast doubt on another of Dasgupta's claims (p. 120), the high economic value of the Catskill Mountains watershed ecosystem in New York State.⁴

Yet much of what Dasgupta says on the valuation of environmental resources is illuminating. Contingent valuation method (which measures value either by willingness-to-pay or willingness-to-accept or both) can be informative about use-value and existence-value, but has limits where the value of biodiversity is concerned, particularly among those (including growth economists) not yet sensitised to ecological processes, and tells us nothing about intrinsic value. However the very limits can prove useful. Thus a demonstration that preserving blue whales was the best policy with regard to use-values and commercial interests, independently of their intrinsic value, became even more persuasive when the argument from their intrinsic value was seen to supply corroborative support for this policy (pp. 135–7). On the other hand, recognition of accounting prices (as

³ Mark Sagoff, 'Carrying-Capacity and Ecological Economics', *BioScience*, 45.9, 1995, 610–20.

⁴ Mark Sagoff, 'On the Value of Natural Ecosystems: the Catskills Parable', *Philosophy & Public Policy Quarterly*, 22.1/2, Winter/Spring 2002, 10–16.

New Books

non-market values are here called) for use-values is surely an impediment when use-value and intrinsic value conflict, as with the value of members of whale populations that can be harvested on the basis of optimum sustainable yield. (All too often intrinsic values are prone to be disregarded if what they conflict with is more obviously quantifiable.)

However, accounting prices are the weights used to measure an economy's wealth (or capital assets, including stocks of bearers of intrinsic value) in the context of sustainable development. Sustainable development is itself defined as involving the expectation of perpetual increases in average social well-being (pp. 140–1), something which Alan Carter has argued to be inessential to this concept. (As Carter points out, sustainable development can as plausibly be regarded as the attainment by everyone in a society of a level of well-being that is regarded as a precondition for sustainable practices and lifestyles. Unlike Dasgupta's account, this one requires sustainable practices, and avoids both expectations of perpetual improvement and reliance on average levels of well-being.)⁵ Dasgupta is on stronger ground in rejecting Gross National Product as a measure of quality of life or social well-being (pp. 149 and 153). But his preference for wealth as a measure of social well-being (p. 171) is questionable, granted that bearers of intrinsic value (whether whales, apes or redwoods) contribute to wealth only insofar as accounting prices contrive to reflect that value (p. 191). Yet it is plausible enough, granted that it includes natural resources as well as other capital assets, to support the claim that over the past three decades the Indian sub-continent, as well as sub-Saharan Africa, has actually become poorer (p. 161).

Part IV has valuable treatments of economic change from the perspective of poor and vulnerable people in the Third World. Dasgupta here argues that redistribution and growth need not be incompatible; that Structural Adjustment Programmes (or, presumably, their renamed successors) can impoverish the vulnerable through their environmental impacts; and that the growth of markets in the absence of safety-nets can undermine salutary non-market institutions (pp. 192–204). In his drawing attention to poverty-traps and to the conditions of Kakotopia (or dysfunctional society), often ignored by economic theorists, lies a major strength of the book.

Part V, occupying the last thirty pages of the main text, is the Part of greatest potential interest to philosophers, and concerns the value of potential lives. Here for the first time the suggestion of the dust-jacket that this book is a contribution to moral philosophy (as well as political economy) begins to be delivered. As usual, the formalized passages can be skipped; as seldom previously, the text begins to soar, and to contain riveting passages about the value of life and of having children (pp. 222–30). The Repugnant Conclusion objection to classical utilitarianism is (rightly)

⁵ Alan Carter, 'Distributive Justice and Environmental Sustainability', *The Heythrop Journal*, XLI, 2000, pp. 449–60, at pp. 450–51. See also Robin Attfield, *Environmental Ethics: An Overview for the Twenty-First Century*, (Cambridge: Polity Press, forthcoming 2003), ch. 5.

claimed not to be repugnant, but the claim that the life of someone whose life is worth living but only barely so has negative value is too readily assumed (p. 219). The real objection to Classical Utilitarianism is argued to lie in its treating actual and potential lives as having equal weight; Dasgupta instead advocates Generation-Relative Utilitarianism, for which (independently of discounting) potential people count for less than actual people, even though good lives, both actual and potential, have intrinsic value.

In support of this stance, a thought-experiment is presented that is held to cast doubt on equal weight being given to both actual and potential lives. Parents faced with a choice between deploying resources to raise the quality of life of a seriously handicapped newborn daughter to a desirable level, and alternatively creating a second child and diverting the same resources so that this child attains such a desirable level instead would, most people would say, do better to do the former, whereas Classical Utilitarianism supposedly makes the two courses of action equally desirable (p. 224). But does it? It might instead support having the second child and spending some of the resources on both children, rather than diverting all to the second, so that both have a life that is worth living; indeed, if this option is available, it might treat both options suggested by Dasgupta as *undesirable*. There again, even if this option were somehow unavailable, modified versions of consequentialism that prioritise the satisfaction of basic needs but still give equal weight to actual and potential people could comprise the justification of preferring the first option to the second, since among the two options only the first avoids there being someone whose basic needs are avoidably unsatisfied.

This chapter culminates in a discussion of reasons for having children, which we are held to do to make our values durable and our lives meaningful. We are trustees both of values and of capital stocks, both cultural and natural, and are inescapably involved in intergenerational Burkean contracts concerned with their transmission (pp. 229–30). Yet contracts that are involuntary as well as inexplicit are scarcely contracts; children reject parents' values so standardly that parents can hardly rely on children for the persistence of their values; values are often transmitted in many other ways, such as through communities of work, professions and voluntary bodies; and (as I have argued elsewhere),⁶ life would not cease to be meaningful even if we knew we would have no successors. However, Dasgupta's speculations here, besides complying with his earlier recognition of intrinsic value both in nature and in culture, reach an unprecedented profundity that makes this chapter (his fifteenth) the one from which the philosophical reader is likely to derive greater nourishment than any other.

Robin Attfield

⁶ Robin Attfield, *The Ethics of the Global Environment*, (Edinburgh: Edinburgh University Press and West Lafayette, IN: Purdue University Press, 1999), ch. 4.

New Books

Causing Actions

By Paul M. Pietroski

Oxford University Press, 2000; £30.00 (HB) ISBN: 0198250428

With the overwhelming trend of modern philosophy of mind firmly materialist, the staking out of a dualist position is, outside the unabashed Cartesians (such as Swinburne and Foster), something rarely done, and when it is done in a conciliatory manner. *Causing Actions* by Paul Pietroski is best characterized as one of the conciliatory dualist works: suggestive, often programmatic, aiming less to convince the hardened mind-brain identity theorist (or 'neuralist', as Pietroski designates him) that he is wrong than to get a dualist position into view as plausible and able to deal with familiar objections. The question Pietroski poses is 'how there can be mental causation, given various facts about persons' (16).

The sort of position the author sets out, *event dualism*, has many aspects and subtleties about it, and is often expounded with admirable technical expertise and painstaking detail. To expound it fully in a short space cannot do it justice, so I will confine myself to a sketch of the terrain, and raise some concerns. First, Pietroski argues (following Hornsby) that most actions are tryings, and tryings are inner mental events taking place under the skin: a trying is the agent's contribution to the world (qua agent), the rest being up to nature. These inner mental events are the effects of external stimuli (via other mental event such as beliefs and desires), and themselves issue in bodily motions. Second, this causal chain is *distinct* from the chain beginning with external stimuli and moving via a string of neural (inner biochemical) events to final bodily motions. Third, causation is to be understood as the extensionalization of explanation: explanation is an objective relation between facts which are themselves intentional entities individuated by means of (Fregean) thoughts; causation is a distinct objective relation between events in virtue of which explanations are true. Fourth, mental causation, as an objective relation between mental events and physical events (bodily motions), has as its explanatory correlate a covering-law model using *ceteris paribus* laws relating an agent's reasons (understood as mental causes) and the bodily motions to which they give rise. Fifth, a Strawsonian conception of persons reveals them to be irreducible, primitive corporeal entities whose mental properties themselves are irreducible to neural or other physical properties. The 'space of reasons' as given by the Sellarsian 'manifest image', and the 'space of impersonal causes' as given by the 'scientific image', are essentially distinct. Sixth, the mental globally supervenes on the physical (broadly conceived): a difference in a mental respect implies a difference in some physical respect. Hence although persons and their mental properties are primitive, they are not ontologically basic since the configuration of the mental in a world is fixed by the configuration of the physical, not vice versa.

These are the principal theses of *Causing Actions*. To expound, let alone discuss them fully in a brief review is impossible, so I shall confine myself to several issues that should give the flavour of Pietroski's thinking and

point to where some weaknesses lie. First is the obvious question of overdetermination: if distinct mental and neural events cause intentional (rationalized) behaviour, don't we have an embarrassment of riches? Are we to say that if neural event N had not occurred, Jones's finger would have moved anyway in virtue of its mental cause—in which case brain behaviour is causally superfluous, and appears not genuinely to subserve mental behaviour? Or (perhaps worse), should we say that if mental event M had not occurred, Jones's finger would still have moved in virtue of its neural cause—in which case what is the point of the mental in the account of agency, and why bother to defend its distinctive reality against materialists?

Pietroski is sensitive to the problem, but I am not sure that he is sensitive enough. His ground shifts; at first he argues that there is no reason to think overdetermination should occur, if we think of the mental and the neural as supervening on a base of the physical P (construed as a completed physics). Consider Jones's finger motion F: is it true that if N had not occurred, F would not have occurred? Overdetermination would, he says, be a problem if there were a relevantly similar possible world in which N did not occur, hence nor did F, but in which mental event M occurred that *should* have caused F (because, say, it paradigmatically causes F-like events in the actual world). But if N failed to occur, and P-type supervenience is true, there would be, according to Pietroski, a difference with respect to P such that 'it is not implausible' (185) to say that in such a world M failed to occur also, or M occurred but in conditions that prevented it from having its typical effects. Nowhere, however, does the author give us a *reason* for thinking that this would be the case, other than to suggest that the P-difference would be one with respect to what happened in the agent's head; but why should he suppose that? Because the neural supervenes on the mental, so that the absence of N generates ipso facto an absence of M? I doubt Pietroski would want to say that (nor would anyone else). Indeed, the author's countenancing of 'various supervenience theses' (184) is part of the problem, since apart from his belief in the 'global supervenience' of the non-physical on the physical it is hard to pin him down to just what kinds of supervenience he thinks obtain. He gives us no argument, but simply assumes P-type supervenience as a way (inadequate, as just indicated) of getting around overdetermination; and construed as a kind of global supervenience it is not strong enough to imply a solution to this problem. Indeed Pietroski does not pretend to have a solution, but rather a 'Scotch Verdict' (184), or a position which does not give a 'reason to suppose' (185) that overdetermination would not occur on the scenario he paints.

Indeed later in the book he happily *embraces* overdetermination in agency, asserting that it is not problematic since we are dealing with 'different explanatory frameworks' (244)—'interlevel' overdetermination can issue in a single effect such as the motion of a finger, but 'intralevel' overdetermination would issue in 'more specific effects' (243), such as when poison in the stomach and a bullet in the chest cause death and more besides. But why does Pietroski think more specific effects are the main

New Books

concern? Whether or not we are dealing with different explanatory frameworks (whatever that may mean), the thought that an agent's intentional bodily movements might be overdetermined militates against the very idea of the unity of the person. On this view, two utterly distinct realms of causation are always at work inside me whenever I lift a finger. Appealing to 'various supervenience theses' will not help, certainly without a much fuller spelling out than Pietroski provides. In fact Pietroski is at pains not to ontologise supervenience, since he sees it merely as a way of individuating possible worlds: if two worlds are physically alike then they *are* the same world, and nothing more needs to be said about dependence. Whatever the general unsatisfactoriness of such a view, it seems that on his picture one cannot help seeing the person as a weird complex—not weird because of the co-existence of mental and physical events, for on that I am in general sympathy with him—but weird because these two realms of the mental and physical are yoked together by thin theory with little illumination of just which realm is in the driving seat.

I said that Pietroski does not ontologise supervenience: this is part of a wider problem, namely that he does not ontologise enough. Of course this is deliberate, since he wants to eschew the wilder metaphysical shores of Cartesian dualism, where materialist traps are legion. Fair enough, but the opposite error is to shy away from an ontology of the mental altogether. Just like the property dualists (such as Nagel) before him, we get the impression that it is a case of dualism by name, physicalism by nature. This comes out strongly in chapter 5, where the Strawsonian picture of personhood is sketched. This picture is, as is well known, very short on ontology and very long on dualisms of perspective. Pietroski adopts the approach pretty much without major modification, but he also has something to say about why mental events are not neural events, namely that they have different 'vagueness graphs'. The idea is that there is far more spatio-temporal specificity true of any neural event or collection of neural events than is true of, say, a trying. Now the author is alive to the fact that the vagueness of either or both might be merely epistemic, but again he is fairly lame in his response: neuralists had better not beg the question against dualists by assuming that the vagueness properties are the same because the event-types are identical, and anyway 'I see no independent reason for supposing that tryings have much sharper boundaries than initial appearances suggest' (175). (Not that it is clear what 'initial appearances' he is referring to.)

I do not want to press the criticism too strongly, because I think his conclusion is correct. The problem is that you cannot reach it without doing some hard metaphysics. Pietroski is right to ask, for instance, '*Which* neural event is Bloggs's trying to open the door?', but the bite of the question has little to do with vagueness and much to do with the very absurdity of thinking that a trying, with all its phenomenal characteristics, its subjectivity, its integration with other mental events, states and properties, its propositional-cum-logical features, its strange combination of transience and durability, could possibly be located *anywhere* in a world of synapses,

ganglions and electrical conduction. We are dealing with a wholly different ontology: true, one that is subserved in obvious and important ways by the physical and the biochemical, but one that is distinct nonetheless. And if this thought is correct, one is obliged to look for the underlying ontological basis of mental phenomena; merely postulating mental events floating in a world of brain porridge will not do. For all that Pietroski says, the mental could simply be the macrophysical supervening on the microphysical. Perhaps neuralism is wrong for reasons he suggests: but then why shouldn't the materialist take the point and adopt a broader mind-body identity thesis, whereby a trying just is a certain state of the whole body? After all, tryings are 'under the skin', argues the author, so vagueness problems might well be eradicable by claiming that whatever the spatio-temporal boundaries of a trying turn out to be, they won't go further than the agent's whole body, and there might be sound physiological reasons for thinking that the whole body is indeed involved in every mental event in such a way that, unless one adopted a stronger form of dualism, one would be obliged to regard mental events as just *being* states of the body. No amount of appeal to explanatory frameworks and personal/impersonal perspectives will undermine what might turn out to be the hard but unpalatable ontology of the matter.

There are stronger versions of dualism, of course, from which the real dualists do not shrink (I have in mind Swinburne, Foster, Popper and Eccles, *et al.*, not to mention Aristotle, the medievals, and nearly all philosophers outside the second half of the twentieth century). Some are Cartesian in flavour, most are not. All see the mental as a distinct metaphysical realm, not merely as a bundle of events somehow yoked to physical space. Pietroski's stimulating discussion of causation in chapter 7 reflects some of the ideas that stronger dualists than he adopt, in particular a rejection (implicit in his case) of what the author calls MOVE: the Metaphysical Oomph View of Event-Causation, where 'oomph' is identified with energy transfer and energy is understood on the physical model (to the extent that it is even understood there). Unfortunately he ties it in again with overdetermination (239), arguing that a single effect such as a bodily motion can be twice caused without there being an extra effect as there would be in purely physical overdetermination; the reason being that mental causation does not require a transfer of energy. But there is little more than the usual Strawsonian thoughts offered to wean us off MOVE: we should 'see physics as a theoretical extension of our pre-theoretic views about what causes what' (242), and our pre-theoretic views are heavily influenced by our perception of our own agency and that of others, where 'oomph' has little to do with it and freedom, control, influence, manipulation, and so on, do (recall Anscombe's justly famous paper 'Causality and Determination'). As Pietroski rightly argues, if we succumb to MOVE and concede that 'physics describes the *real* movers and shakers in the world, and that *real* causation is to be found at the level described by physicists' (242), then we are saddled with real problems about just what causation *is* and how it is to be interpreted, especially given the weirdness of causation

New Books

at the quantum level. We might end up wondering, along with the Humeans, whether there is any *real* causation at all.

Pietroski's concerns are well motivated, and he addresses them with thoughts that certainly head in the right direction. But again, without biting the metaphysical bullet it is hard to know what to make of the situation. Descartes, unfortunately, saddled generations of sympathisers with the pessimistic view that biting the metaphysical bullet makes it only *harder* to know what is going on. Which is why the Frenchman deserves to be only a bit player in the sort of debate in which Pietroski and others are engaged. Nevertheless, in order to grasp mental causation we need to know how mental events are situated in the ontological economy of the world. We need to know what is in the driving seat—mind or brain? We need to do more than throw around the word 'supervenience', which covers a multitude of metaphysical sins. I doubt Pietroski would dissent from this latter thought, but the correct reaction is not to *weaken* the associated ideas—e.g. by appealing to individuation of possible worlds—but to *strengthen* them—by taking dependence seriously, defending the psychophysical nexus, and, quite simply, by countenancing the possibility of an immaterial element to the human mind. Here, I strongly suspect, it would be 'a bridge too far' for Pietroski, who probably remains more of a physicalist than he would like to admit.

For all the above objections, *Causing Actions* is a very good book, if rather wearing to read. I have not discussed the illuminating analysis of Frege and 'that' clauses, the defence of Homsbian neo-volitionalism, or the provocative and fascinating justification of *ceteris paribus* laws, which latter will no doubt serve as a reference point for future debate. When it comes to mental causation Paul Pietroski's heart is, I think, in the right place. Whether *Causing Actions* ends up converting a single neuralist to any form of dualism is another matter.

David S. Oderberg

Social Empiricism

By Miriam Solomon

Bradford Books/MIT Press, 2001. pp. 175 + xi £21.95.

It is widely agreed that a tradition in philosophy of science deriving in large part from logical positivism and reaching its height in the logical empiricism of the 1950s and 60s has been very widely abandoned. It is fairly widely agreed what went wrong with it. What is much less clear is what should take its place. Drawing together a number of the most influential strands in recent philosophy of science, Miriam Solomon's book offers a concise and plausible answer to that question. Central to this book is the rejection of several central themes central to the earlier tradition. One such theme, and one that remains widely influential to the present, is the individualist view of knowledge the rejection of which is implied by Solomon's title. Scientific knowledge is to be seen as the product of a com-

munity of enquirers rather than something that could be the achievement of a heroic isolated investigator. Another is the uniqueness and unity of knowledge, the assumption of one true story about the world. The rejection of this vision, in turn, makes possible the rejection of the widely held assumption that science must aim at consensus.

A positive theme of the book is naturalism, the thesis that we cannot decide on how we ought to investigate the world without seeing first how we have in fact successfully investigated it. Empiricism, for naturalists such as Solomon, applies to epistemology as much as to the scientific enquiries that are the subjects of epistemological reflection. This kind of naturalism marks a methodological divergence from the classical tradition. Whereas positivists, and many subsequent philosophers, saw epistemology as most importantly (perhaps exclusively) involving conceptual analysis, Solomon sees it as always dependent on the details of empirical knowledge. In accordance with this commitment, the book is well supplied with a good range of examples from the history of science, generally presented with rather more detail than the standard philosophy of science caricature. One interesting consequence of Solomon's naturalism is that, because of the diversity of the factors that need to be assessed in evaluating a programme of scientific research, Solomon sees epistemology as necessarily multidisciplinary. This conclusion fits well with the growing realization that the complexity of much contemporary science presents problems of interpretation that can be addressed only with the collaborative application of a variety of expertise. Neither the positive nor the negative themes are entirely unfamiliar. Indeed they have been at the centre of a good deal of recent debate in philosophy of science and science studies more broadly. However Solomon does assemble them into a generally satisfying picture as well as advancing them in some significant respects.

One respect in which Solomon's views diverge significantly from most recent precursors is in the variety of factors she sees as relevant to the determination of scientific belief. These include both factors that philosophers have traditionally thought of as good reasons for scientific belief, and factors that have generally been viewed as inappropriate—biases of the investigator, or personal or political objectives. In important respects this indicates an attempt to reduce the notoriously fraught theoretical space separating philosophers from sociologists of science. Solomon does, however, in keeping with the philosophical tradition, maintain a normative division between kinds of reasons, which she distinguishes as empirical and non-empirical—normative, because in the end it is only the empirical ones that lend rationality to scientific belief. But whereas the philosophical tradition has generally seen it as a matter for philosophical analysis what are legitimate kinds of reasons for believing a theory, for Solomon, in accordance with a more thoroughgoing naturalism than is common, this distinction is itself conceived of as empirical. Empirical factors are just those that are found, empirically, to increase the empirical success of the theories. There is no guarantee that this method will provide a distinction that coincides with traditional assumptions as to what kinds of factors are

New Books

rational grounds for belief, and Solomon claims that it sometimes fails to do so.

On the other hand, as this appeal to empirical success makes clear, Solomon does insist that science has overarching general aims, most notably empirical success (which she illuminatingly shows to be a quite diverse goal) and, to a lesser degree, truth. And she is also committed to the possibility of measuring and comparing the empirical successes of different theories. In this respect Solomon's position is clearly on the traditional philosophical side of the debate with those radical social constructionists who deny that there is any objective comparison of theories beyond the decision—on whatever grounds—of scientific practitioners. This is one of the points where this book is more conservative than is sometimes obvious. Without wishing to argue that there are no objective grounds for distinguishing between theories, Solomon's emphasis on the empirical is narrow. Many philosophers now doubt whether epistemic values can be sharply distinguished from social or ethical values. This is most widely true of feminist philosophers of science, with whom Solomon explicitly aligns herself. However she sees no difficulty in simply separating her own epistemological goals from the explicitly political goals of other feminist scholars, claiming merely to be pursuing a different agenda (p. 147). I suspect that the scholars she cites (Donna Haraway, Evelyn Fox Keller, Helen Longino) might be unconvinced.

The basic ideas behind Solomon's book seem to me, nevertheless, interesting and promising. I have some reservations about the detailed implementation. Where I have spoken above about factors determining belief, Solomon introduces the technical term 'decision vector'. There are, she claims, between 50 and 100 kinds of decision vectors, and several of these will be salient in any scientific controversy. Identifying them makes possible judgments of the appropriateness of the division of labour between competing scientific research projects. What is the criterion for a proper division of scientific labour? According to Solomon, 'If empirical decision vectors are distributed equitably, i.e., in proportion to the empirical success of the various theories under consideration, and non-empirical decision vectors are distributed equally, then, overall, the distribution of decision vectors will be equitable' (p. 77). This is not altogether easy to understand. Why should we care how decision vectors are distributed? Presumably only to the extent that they produce decisions, so that an equitable distribution of decision vectors must be one that produces an optimal distribution of effort. Then the point will be that empirical decision vectors produce allocation of effort in proportion to empirical success, and the non-empirical decision vectors, being equally distributed, cancel out and have no effect.

This surely isn't quite right. Non-empirical decision vectors will not cancel out. Since scientific decision-makers don't treat them as irrelevant (or they wouldn't be decision vectors at all) equally distributed non-empirical decision vectors will distort research allocation. To illustrate by example, suppose projects A and B have, in their favour 10 non-empirical deci-

sion vectors each, and 10 and 5 empirical decision vectors respectively, the latter reflecting accurately the empirical success of the projects. Then the allocation of effort would be in the ratio of 4:3 (20:15) whereas it should, on the basis of empirical success, be 2:1. So it seems that the 'equitable' distribution of non-empirical decision vectors need to be in proportion to the empirical success of the projects they favour. Indeed, as far as I can see, non-empirical decision vectors are simply biasing factors, with all the traditional negative connotations of this term, and the best we can hope is that they will (by good luck) make no difference.¹ Solomon's account is, in the end, considerably more conservative than she appears to admit.

One possible reply to all this would be to refuse to be drawn into these quantitative details. Solomon explicitly embraces quantitativeness, by proposing to evaluate particular episodes in the history of science with 'improper linear models'. This is actually a fancy term for counting: Add the positive factors and subtract the negative factors. In defence of this admittedly rather crude methodology. Solomon refers to studies that claim to show the superiority of this technique over informal ('intuitive') assessment. Perhaps so. Nonetheless it is hard to avoid the thought that some factors are a great deal more motivating than others, or that the classification of factors into empirical and non-empirical is a rough and ready one. More worryingly, the spurious precision suggested by undermotivated quantification is disturbingly redolent of the style of philosophy of science that has seemed wholly irrelevant from the perspective of most other approaches to science studies. I fear that this strategy will decrease the breadth of impact of the work.

Perhaps the most interesting thesis in this book concerns the analysis of consensus and dissensus. One idea that survives widely from classical philosophy of science is that part of the aim of science is to produce agreement. Solomon proposes on the other hand that there is nothing especially desirable about consensus. In some situations the empirical decision vectors are so overwhelmingly in favour of one set of beliefs that consensus is inescapable, but the more general case is one in which a number of different theories or approaches will be developed simultaneously. Solomon presents detailed historical examples in which she claims that consensus was premature and inappropriate. Pluralism is an increasingly popular view in the philosophy of science, but pluralists on the whole try to avoid allowing direct conflict between the various theories they allow to coexist. Coexisting theories are held to be about more or less subtly different things, for instance. In this respect Solomon proposes an unusually and admirably full-blooded pluralism. There is, however, a good deal of work to be done in reconciling this kind of pluralism with as much of a com-

¹ Solomon (personal communication) tells me she does not advocate adding up decision vectors without regard to whether or not they are empirical. In this case, though, I'm not sure why there is any 'equitable' role for them at all. They appear simply to be biasing factors that should be eliminated.

New Books

mitment to the pursuit of truth as she endorses, and to deal with the obvious difficulty that truths cannot contradict one another. I don't doubt that this can be done, but I'm not convinced it has been done here (though there are some interesting suggestions in the chapter in which Solomon develops her particular version of realism—provocatively labelled 'Whig realism'). And of course this is vital to the whole project. What makes social empiricism social is that the unit of epistemological evaluation is the community of investigators. And what may be most conducive of empirical success for the entire scientific community is the simultaneous pursuit of various paths of enquiry. However, the still orthodox view that it is rational to form consensus when the balance of reasons strongly favours one theory is surely motivated primarily by the thought that science aims at the truth. It is not clear that truth is even an intelligible goal for a radically social epistemology, and if it is, it is not clear that it is appropriate.

There are then, some substantive reservations I have about the details of this book, but these are outweighed by considerable merits. It builds on some of the most interesting ideas in contemporary philosophy of science and is thus very much on the cutting edge of the discipline. Certain ideas at the forefront of contemporary discussion in the field, most notably naturalism, are applied more consistently than has generally been attempted. The argument is always grounded in the empirical reality of scientific history. And, most admirably, it is deeply committed to providing the sort of philosophy of science that can offer substantive advice as to how science should most effectively be practised. Overall, this book makes significant contributions to many of the most interesting debates currently engaging the philosophy of science.²

John Dupré

Facing Facts

By Stephen Neale

Oxford University Press, 2001, pp. xv + 254. £25

As Stephen Neale explains (p. 9), a slingshot argument is a collapsing argument designed to demonstrate that there are fewer entities of a given kind than might be supposed previously. Neale's rigorous, scholarly and technically impressive monograph, based largely on two earlier articles,¹ is a sustained examination of the merits of those slingshots whose target is an ontology of facts. Such arguments, which have been infamously propounded by Gödel, Church, Quine and Davidson, purport to show that theories of facts are untenable because there could be at most only one such item.

¹ See 'The Philosophical Significance of Gödel's Slingshot', *Mind* 104 (1995), pp. 761–825, and (with Josh Dever) 'Slingshots and Boomerangs', *Mind* 106 (1977), pp. 143–68

² I am grateful to Miriam Solomon for comments on a draft of this review.

The main thesis of Neale's book can be put like this. Although those slingshots offered up to now can be countered by fact-theorists relatively easily, a more exacting slingshot, derived from that of Gödel, establishes that any theory of facts must meet the substantive 'descriptive constraint' (pp. 185–7), if it is to avoid ontological collapse. Neale also contends that his favoured slingshot both demands that theorists of facts 'say something very precise (if only disjunctively) about the semantics of definite descriptions' (p. 13), and provides 'indirect support' for Russell's theory thereof (p. 13). I shall return to these claims in due course. Let me start, however, by setting the scene.

A fact-theorist holds that connectives such 'the fact that ... = the fact that ...' (herewith, *FIC*), 'the sentence that ... corresponds to the fact that ...' (herewith, ©) and (if she takes facts to be causes) 'the fact that ... caused it to be the case that ...' are not truth-functional. If they were, it would follow, respectively, that there is only one fact, that all true propositions correspond to the same fact, and that all facts are causally related. Slingshot arguments aim to show that this appearance of non-truth-functionality is illusory. A successful slingshot will reveal the connectives in question to permit the substitution of material equivalents *salva veritate* within their scope. Having said this, it would not be wholly unfair to say that most commentators with a sensitive nose have detected a whiff of sophistry about the slingshot arguments proffered by Church, Quine and Davidson. In Chapters 2–5 and 8, Neale does an excellent job of laying bare the way in which the three authors' various slingshots are undermined by implausible premises and/or arguably mistaken applications of inference principles. In particular, Chapter 2 sees Neale elegantly explain how Davidson's slingshot relies upon both an assumption that a fact-theorist is likely to deny, and a dubious application of a substitution principle regarding singular terms.

Davidson's contested assumption is that logical equivalents may be substituted *salva veritate* within the scope of the relevant connectives; or, in other words, that the connectives in question permit the use of the following inference principle on any sentence within their scope:

$$\text{PSLE:} \quad \frac{P \mid \ddot{A}B}{\frac{\% (P)}{\% (B)}}.$$

Davidson's questionable application of an inference principle, meanwhile, is his use of the following principle of the substitutivity for singular terms for sentences containing definite descriptions:

$$\text{PSST:} \quad \frac{K=2}{\frac{\% (K)}{\% (2)}}.$$

New Books

With these two assumptions in place, we construct a characteristically Davidsonian slingshot (in which ‘ $\hat{\cdot}$ ’ takes the place of one of the fact-theorist’s connectives, and ‘ $\hat{\cdot}$ +PSLE’ means that the connective permits the use of PSLE on any sentence within its scope):

1	[1]	P	Prem
2	[2]	B	Prem
3	[3]	$\hat{\cdot}$ P	Prem
3	[4]	$\hat{\cdot}(\hat{\cdot}1x(x=d)=\hat{\cdot}1x(x=d\bullet P))$	3, $\hat{\cdot}$ +PSLE
1,2,3	[5]	$\hat{\cdot}(\hat{\cdot}1x(x=d)=\hat{\cdot}1x(x=d\bullet B))$	4, $\hat{\cdot}$ +PSST
1,2,3	[6]	$\hat{\cdot}$ B.	5, $\hat{\cdot}$ +PSLE

Needless to say, if $\hat{\cdot}$ is +PSLE, and Davidson is entitled to his use of PSST, Davidson’s slingshot hits its target. But as Neale lucidly explains, precisely these points are highly controversial. Neale notes (p. 221) that Barwise and Perry, Bennett and Searle have all disputed that the connectiveness in question are +PSLE, and with good reason: ‘ $\hat{\cdot}1x(x=d)=\hat{\cdot}1x(x=d\bullet P)$ ’ contains a singular term that does not occur in ‘P’, and it is tempting to conclude from this that the two truths differ with respect to their truth-relevant entities, and hence express different facts. The appeal to PSST to licence the move from [4] to [5], meanwhile, presumes that the definite descriptions ‘ $\hat{\cdot}1x(x=d\bullet P)$ ’ and ‘ $\hat{\cdot}1x(x=d\bullet B)$ ’ are genuine singular terms, and this presumption causes trouble. First, it is plainly contradicted by Russell’s theory of descriptions. Second, even if definite descriptions *are* taken to be singular terms, Neale points out that it is by no means obvious that there is a plausible semantics available which generates the argument’s claimed logical equivalences (p. 56). It would be interesting to hear Davidson’s response to Neale’s elegant unpicking of what remains, somewhat surprisingly, Davidson’s leading argument against the correspondence theory of truth.

Granted that Davidson’s slingshot is unlikely to hit its target, how does Neale improve upon it? By taking his cue from Gödel. Neale replaces Davidson’s assumption concerning the substitutivity of logical equivalents with something less permissive, and uses an inference principle concerning definite descriptions, whether they are taken to be singular terms or not. Specifically, Neale replaces PSLE with the altogether less permissive 1-CONV (pp. 177–80), and uses 1-SUBS (pp. 157–65) in place of PSST:

1-CONV:	$\frac{\% (x/K)}{K=\hat{\cdot}1x(x=K\bullet \% (x))}$	$\frac{K=\hat{\cdot}1x(x=K\bullet \% (x))}{\% (x/K)}$
1-SUBS:	$1xP=1xB \quad \hat{\cdot}7xP=K$	$1xP=K$
	$\frac{\% (1xP) \quad \% (1xB)}{\% (1xB) \quad \% (K)}$	$\frac{\% (K)}{\% (1xP)}$

It should be noted that \neg -CONV does not license Davidson's moves from [3] to [4], and from [5] to [6], whilst a Russellian about definite descriptions holds that substitutions involving definite descriptions can only be licensed by \neg -SUBS, and not by PSST.

With these differences in mind, we can follow Neale in formulating his Gödelian slingshot as follows (pp. 183–7):

1	[1]	Fa	Prem
2	[2]	$a \neq b$	Prem
3	[3]	Gb	Prem
1	[4]	$a = \neg x(x = a \bullet Fx)$	1, \neg -CONV
2	[5]	$a = \neg x(x = a \bullet x \neq b)$	2, \neg -CONV
2	[6]	$b = \neg x(x = b \bullet x \neq a)$	2, \neg -CONV
3	[7]	$b = \neg x(x = b \bullet Gx)$	3, \neg -CONV
1,2	[8]	$\neg x(x = a \bullet Fx) = \neg x(x = a \bullet x \neq b)$	4, 5, 7-SUBS
2,3	[9]	$\neg x(x = b \bullet Gx) = \neg x(x = b \bullet x \neq a)$	6, 7, 7-SUBS
10	[10]	$\ulcorner (Fa)$	Prem
10	[11]	$\ulcorner (a = \neg x(x = a \bullet Fx))$	10, \ulcorner + \neg -CONV
1,2,10	[12]	$\ulcorner (a = \neg x(x = a \bullet x \neq b))$	11, 8, \ulcorner + \neg -SUBS
1,2,10	[13]	$\ulcorner (a \neq b)$	12, \ulcorner + \neg -CONV
1,2,10	[14]	$\ulcorner (b = \neg x(x = b \bullet x \neq a))$	13, \ulcorner + \neg -CONV
1,2,3,10	[15]	$\ulcorner (b = \neg x(x = b \bullet Gx))$	14, 9, \ulcorner + \neg -SUBS
1,2,3,10	[16]	$\ulcorner (Gb)$	15, \ulcorner + \neg -CONV

One thing for sure: this argument is valid and so conclusively demonstrates the *descriptive constraint*: namely, that if ontological collapse is to be avoided, '[t]he friend of facts needs a theory according to which these connectives are either— \neg -SUBS or— \neg -CONV' (p. 187). Neale's main thesis has been proved and, what is more, his slingshot proceeds with strikingly weaker premises than that of Church, Quine and Davidson. As we shall see, however, what is less clear is the extent of this result's philosophical importance.

Largely as a response to objections made by Graham Oppy to his original article,² Neale sets out to make the case for the proof's philosophical significance by doing two things. He carefully explains the proof's *point*, namely, that it 'imposes a *structural* constraint on theories of facts' (p. 210) which enables us to 'filter out theories of facts that are inconsistent' (p. 207); and he couples this with an enlightening account of how and why certain facts-theorists (notably Neil Wilson, J. L. Austin and, arguably, the early Wittgenstein) have come to assume the relevant connectives to be both \ulcorner + \neg -SUBS and \ulcorner + \neg -CONV, thus precipitating the aforementioned collapse (pp. 205–10). This, Neale claims, is sufficient to demonstrate the proofs clear philosophical bite (p. 210).

True enough, Neale has shown that his Gödelian slingshot has some

² 'The Philosophical Insignificance of Gödel's Slingshot', *Mind* 106 (1997), pp. 121–41.

New Books

philosophical significance. There is no doubt that any theorist of facts had better explain why the relevant connectives are \neg -SUBS or \neg -CONV. But it remains the case that it is all too easy for certain fact-theories to meet the descriptive constraint. Indeed, as Neale himself explains (p. 204), according to a Russellian theory, which takes facts to be structured entities with objects and properties as constituents, and which treats definite descriptions as quantified noun-phrases rather than singular terms, *FIC* comes out as *both* \neg -SUBS and \neg -CONV. This brings me on to my major reservation with the book, a reservation that first materialised as I realised that a work entitled *Facing Facts* was wholly concerned with slingshot arguments.

Let me lead up to my worry this way. Although a Russellian can neatly evade what is, undoubtedly, the best slingshot money can buy, he is clearly by no means home and dry. Indeed, he is certainly not entitled to claim the prize until he has done the following, at least: satisfactorily motivated his theory of facts (perhaps by means of saying why we should accept a truthmaker principle); explained why we should not simply follow Frege in identifying facts with true propositions; and justified the suggestion that facts are genuine causes. Indeed, until the Russellian has addressed these characteristically philosophical questions, his avoidance of the slingshot will cut no ice with a sceptic about facts. Neale, however, has very little to say about these less technical concerns, suggesting that a fact-theorist should simply proceed by examining whether a theory of facts can avoid his slingshot whilst doing justice to 'the semi-ordinary, semi-philosophical idea of what facts are' and permitting 'facts to do some philosophical work' (p. 223). But this approach surely constitutes an attempt to apply, in Gareth Evans's phrase, a metaphysical wet blanket to substantive issues. For there *is no* single semi-ordinary, semi-philosophical idea of the nature of facts, as a cursory examination of our ordinary language reveals.³ Likewise, there is a glorious lack of consensus on the 'philosophical work' that should be done by facts. Are facts needed to act as truthmakers, or could tropes do this job? Do truths need truthmakers at all? Can facts be causes, or is this job done solely by events, as Davidson believes? Could facts be causes and yet be true thoughts, as Bennett has suggested? If a fact-theorist cannot provide answers to these questions that satisfactorily motivate an ontology of facts in the first place, the issue of whether a given theory avoids the slingshot becomes little more than a technical side-show. As a consequence, I expected Neale to be a little more committal in these areas. Perhaps Chapter 11, in which he helpfully sets out some of the options, could have been extended and a little more decisive.

When it comes to Neale's claim that his favoured slingshot provides

³ One example should suffice. On the one hand, as Neale explains (p. 48), we customarily describe facts as being *about*, rather than as *containing*, objects, which suggests that we view facts as true thoughts, rather than as things to which true thoughts correspond. But on the other hand, we describe thoughts, but not facts, as true, which seemingly embodies a resistance to this identity-thesis.

indirect support for Russell's theory of descriptions (p. 13), I found myself less than wholly convinced. Neale's argument would seem to be this. The Russellian about definite descriptions can relatively easily avoid the slingshot's conclusion because he is free to deny that *FIC* is *either* +1-SUBS or +1-CONV. For the Russellian,

FIC is not +1-SUBS because two definite descriptions of the same object will not, in general, contribute the same descriptive properties to a fact. Secondly, the structured character of facts guarantees that [the connective] will not support 1-CONV since the quantificational nature of descriptions introduces properties not present in the pre-*iota*-conversion fact. (p. 204)

By contrast, Neale suggests, things are not so straightforward for the fact-theorist who is inclined to regard definite descriptions as singular terms. If such a fact-theorist is to deny that *FIC* is +1-CONV, he must provide a precise semantics for definite descriptions which has them as singular referring expressions, validates 1-CONV in truth-functional contexts, and yet has 1-CONV break down within *FIC*'s scope; and such a theory, Neale argues (Ch. 10), is not obviously to hand. Given that this is so, could the friend of facts who takes definite descriptions to be singular terms avoid ontological collapse by denying that *FIC* is +1-SUBS? No, claims Neale. For if definite descriptions are singular terms, the question of whether *FIC* is +1-SUBS becomes the question of whether it is +PSST; and this, Neale claims, is something which 'no fact theorist who intends to get some metaphysical work out of facts wants to deny' (p. 221).

In response to this, let us grant what Neale says about 1-CONV. My worry concerns his claim that any serious fact-theorist will accept that *FIC* is +PSST. If, as is claimed by the position under discussion, definite descriptions are construed as singular terms, and thus fall within the ambit of PSST, it seems plain that non-extensional contexts will come out -PSST; and this for the simple reason that such contexts are -1-SUBS. This, in fact, is precisely D. H. Mellor's view when it comes to ©. Following Mellor,⁴ let us suppose that several climbers fall but that

(17) Don falls first because Don's rope is the weakest rope.

If we use 1-SUBS to replace 'Don's rope' with 'the weakest rope', we get a clear falsehood, *viz.*,

(18) Don falls first because the weakest rope is the weakest rope.

Now, given the presumption that definite descriptions are singular terms, this means that '... because ...' is -PSST. But since a fact-theorist who takes definite descriptions to be singular terms can so readily treat '... because ...' as -PSST, is it so obvious that she will not want to adapt the counter-examples to attempt to demonstrate the same for *FIC*? Why should not Mellor, for example, deny that the fact that Don's rope is the

⁴ *The Facts of Causation* (London: Routledge, 1995), p. 117.

New Books

weakest rope is the same fact as the fact that the weakest rope is the weakest rope? After all, if definite descriptions are singular terms, the former fact is contingent, whilst the latter is necessary. Furthermore, it is unclear, to me at least, why someone who took this line would then be unable to have his facts do any metaphysical work. Mellor, remember, takes facts to be causes. Given that a fact-theorist who holds definite descriptions to be singular terms may, in this way, relatively easily deny that *FIC* is +1-SUBS, and thereby deny that it is +PSST, I have difficulty seeing why we should accept Neale's claim that his Gödelian slingshot provides indirect support for Russell's theory of descriptions (p. 13). We might well be pushed towards Russell's theory for all sorts of reasons, but Neale's slingshot provides no *new* such reason.

It is of the nature of the beast that a book review will spend a good deal of time outlining the points with which the reviewer takes issue. Putting such concerns to one side for a moment, what has to be stressed is that Neale's book is meticulous in its scholarship, compellingly written and rigorously argued. In the course of its careful thread of argument, it has extremely helpful and enlightening things to say about, for example, the truth-theoretic approach to meaning, the dualism of scheme and content, and the semantics of definite descriptions. More than this, it demands to be read by anyone interested in slingshot arguments. Nonetheless, and as I have said already, I was a little disappointed to see Neale fail to commit himself on many of the distinctively philosophical questions about the nature of facts. For if a theory of facts cannot be adequately motivated to begin with, whether or not it evades Neale's slingshot will begin to seem curiously beside the point. Perhaps one can be too much in love with philosophical logic.

Julian Dodd

Future Pasts: The Analytic tradition in twentieth-century philosophy

Edited by Juliet Floyd and Sanford Shieh

Oxford University Press, 2001, 465+xv pages.

This book is one of a rash of recent quality edited collections (others include those edited by Stocker, by Reck and by Tait) on an increasingly recognized 'topic' in the philosophical history of philosophy: the 'history of the present' of philosophy, or (more prosaically) the *history of contemporary* philosophy. In other words, this volume attempts to think of figures such as Frege, Russell, Carnap and Quine, not just as our contemporaries, but as antecedents sometimes needing serious hermeneutical attention if we are to understand their work adequately (or, sometimes, *at all*). Just as (for instance) there have been major efforts recently to re-interpret Descartes, including efforts which have been built on the worry that Descartes's assimilation into contemporary debates has been very much at the cost of any real idea of what he was trying to accomplish or succeeded in accomplishing, so scholars and philosophers have started to argue in

some numbers that we have sometimes unwisely assimilated Frege et al, without carefully enough looking to see whether their concerns are actually our's, and what we might learn from the differences.

The collection of essays assembled by Floyd and Shieh makes a major contribution to this relatively new enterprise. In essay after essay, by such distinguished authors as Føllesdal, Quine, Friedman, Putnam, Cavell and Rawls (who contributes a charming memoir on Burt Dreben, to whose memory this volume is appositely dedicated), we see serious efforts being made to comprehend modes of thought which are and yet are not our own. This volume does indeed skilfully sketch for us a set of possible pasts for our discipline, and invites us to choose one, more on the basis of what the great thinkers of our recent past were actually about than on the basis of what concerns those authors filling the front half of MIND right now, or of what the latest trends heading our way from New York (or even from Paris) happen to be.

To my mind, an essay in this collection which stands out as exemplifying the importance of the new philosophical historiography of English-speaking philosophy is Joan Weiner's 'Theory and elucidation: The end of the age of innocence'. The appearance in print of this paper—which continues Weiner's meticulous long-running project of re-reading Frege, and of displacing Michael Dummett as one's primary guide to Frege's philosophy—has been long-awaited. In it, Weiner illustrates her non-theoretical reading of Frege—her understanding of him as seeking at the most crucial points to avail his reader of an understanding only conveyable by means of elucidations, and not by means of sentences expressible in a proper scientific language—by means of an intriguing (elucidatory?) reading of Edith Wharton's great novel, *The Age of Innocence*. Specifically, by suggesting that that novel's elucidatory aim, of understanding the orientation (to others and to his society) of its protagonist (Newland Archer), as utterly altered, by the novel's end, but as not relevantly different—everything having been left as it is—as regards matters of fact, is an aim not unlike Frege's aim at a number of key moments hitherto neglected by mainstream commentators (on Frege).

One reason why Weiner's paper has been eagerly awaited is that it also features her first extended grappling in print with the early Wittgenstein. And, if one figure bestrides this collection, seeming for many of its contributors to define what was best about twentieth-century philosophy, it is surely Wittgenstein.

The Wittgenstein who emerges from the volume is again perhaps not that familiar to many readers, particularly British readers. This Wittgenstein not only emerges from a rich culture of relatively little-known European thought (Hintikka for example discusses the influence of Mach) but is very difficult to assimilate to 'Oxford philosophy' or to other 'mainstream' trends in English-speaking philosophical thought. Juliet Floyd's own long essay on 'Number and ascriptions of number in Wittgenstein's *Tractatus*' is especially important: in this paper, Floyd presents the most extended argument yet in print for a reading of

New Books

Wittgenstein's early work even more radical than the new ('therapeutic', antimetaphysical, 'resolute') interpretation pioneered by Conant and Diamond, an interpretation called by some in Britain now the 'American' interpretation of Wittgenstein. Floyd suggests that Diamond's distinction between the 'frame' and the 'body' of the *Tractatus* breaks down in the end, and that one way of seeing this is to read closely more of the 'body' of the work (e.g. the sections on mathematics), and to understand that Wittgenstein is endeavouring in these sections to get one to give up even the very idea of clarity of expression embodied in (the very idea of) a concept-script. Diamond, along with others who have essays in this book, such as Goldfarb, Ricketts and Weiner, has pioneered a re-reading of Frege such that Frege anticipates much more of Wittgenstein (and much less of Analytic philosophy) than has previously been urged; but Floyd now argues that Wittgenstein himself, already in his early work, is far further down the track toward a successfully antimetaphysical and nontheoretical philosophical practice even than this new Frege.

Floyd's attempted radicalisation of the 'resolute' reading of Wittgenstein will surely be one of the talking points engendered by this volume.

Some readers may find the subtitle of the volume inaccurate, in that several essays in the volume treat (at least in part) of 'Continental' philosophy. An extreme case in point is Ed Minar's paper, 'Heidegger's response to skepticism in *Being and Time*'. Minar's paper gave me a significantly better handle on Heidegger's difficult writing on this topic—a topic of very general interest—than I had had previously; but isn't Heidegger paradigmatically a Continental thinker, and *not* an Analytic thinker?

A subtheme of the volume, and one which may be in significant part an outgrowth of its interest in Wittgenstein as a figure who has in the main (until recently) been catastrophically misunderstood, is the continued breaking-down of the split between the 'Analytic' and 'Continental' 'schools'. Not through the making of 'Rortian' grand declarations, but through detailed work on the Frege-Husserl debate; on how to read Wittgenstein in the context of the actual influences on him (and not as if he had never read anyone bar perhaps Russell); and on how to read Heidegger. Not through the utterly distorting lens of Carnap—but in a way that nevertheless renders him perhaps surprisingly close to some classically 'Anglo-American' concerns. Concerns, indeed, which are very current for some of our leading English-speaking philosophers.

For instance, Minar's handy rendition of Heidegger in terms comprehensible to the likes of me, yet without I think simply translating him into an alien idiom, brings out quite clearly how he can be read as anticipating central elements of Ian Hacking's pragmatic 'entity-realism': 'our "procedures" for ascertaining the existence and character of particular real entities are not [according to Heidegger] innocent of commitments to the real "world"'. (Minar, p. 202, *Future Pasts*) And Michael Williams's diagnostic attack on what he calls epistemological realism' too can be seen as anticipated by Heidegger's claim that '*world*' as the background on which

beings are discovered and understood has not been made a target of epistemological inquiry. But then it is not clear what the skeptic is asking in demanding a justification of our belief in the external world. At this point it is fair to say that the skeptic's innocence is compromised and that he must try to *make* sense out of the question he wants to raise by *excluding* the meaningfulness of the world as context for *Dasein's* dealings. The price is the credibility of his conviction that his question is self-evident and his way of pursuing it fully natural.' (Minar, p. 209; cf. also p. 202.)

It seems to me, then, that Floyd and Shieh's volume does not descend into incoherence or over-ambitiousness through the breadth of its focus. Rather, it genuinely helps to refocus its reader. And while it is in some respects a heterogenous collection (there will be few who will simply want to read it from start to finish), it is one to be highly commended to *anyone* interested in knowing what our great 'contemporaries' really thought, and to understanding how their thought fails to fit into some of the compartments which we have become accustomed to (such as, for starters, the 'split' between 'Anglo-American' philosophy and its 'Continental' other). The volume is almost entirely American—that is to say, U.S. authors make up 90% of it—but it will be a rash British 'Wittgensteinian', or 'Russellian', or 'Fregean', or 'Quinian', or (for that matter) 'Heideggerian', who ignores it.

Rupert Read