New Books

Predicting the Future: An Introduction to the Theory of Forecasting
By Nicholas Rescher
(State University of New York Press, Albany, 1998), pp. xi + 315.

We live in a world in which predictions are not only possible but also necessary for the continued existence of human life. The business of predicting future events is usually placed in the hands of scientific experts, but making predictions is a very basic experience of human existence. The question of how to make predictions and become proficient in the art of forecasting is not the subject of this book. Nicholas Rescher is interested in an overall survey of the 'predictive land', in the course of which he lays down some of the fundamental principles underlying the predictive enterprise. The promise of the subtitle, to deliver an introduction to the theory of forecasting and, in fact, prediction, is therefore fully met.

Predicting and forecasting are not the same and not every statement referring to the future constitutes a prediction. A science fiction scenario of what the world may look like in 1000 years is not a prediction, nor is the statement that cats will be animals tomorrow. Predictions express an attempt on the part of a predictive agent to resolve some meaningful question about the contingent future in a rationally cogent manner. Rational answers about future questions must include a reliable evidential base (p. 39). Predictive statements come in different forms (categorical or conditional, probabilistic, general and open-ended). Rescher distinguishes them from forecasts, which foretell concrete events (like the weather), for a particular moment in time (p. 42). In later parts of the book, as we shall see, the distinction slips out of the author's mind and it becomes unclear whether predictions or forecasts are meant and whether this would make a difference to the argument.

The book has fourteen clearly written chapters. Rescher first gives a very brief historical synopsis of predictive activities from antiquity to the present century. As in the seventeenth and eighteenth century scientific predictions became the model, at least a brief mention of Condorcet's *Esquisse d'un Tableau Historique des Progrès de l'Esprit Humain* (1794) would have been beneficial. In the following chapters the book deals with basic epistemological and ontological issues, predictive limits and practices. At first the discussion is quite general and the ideas developed presumably apply as much to predicting and forecasting in everyday affairs as in the natural and the social sciences. The discussion is supported by numerous examples of a general kind. Towards the second part of the book some ideas begin to repeat themselves to the point that two sentences from p. 181 are repeated verbatim on p. 184. But then the book picks up again and offers more insights into the predictive enterprise, especially in its chapter on the social sciences. The general depiction of the shape of the predictive landscape is enhanced by philosophical reflections of an acces-

sible level of technicality. The book demonstrates in a practical way that both predictive principles and philosophical concerns are inseparably interwoven.

Rescher's approach to the predictive enterprise evolves against a general philosophical background of realism. This emerges in his rejection of the DN model's symmetry thesis of explanation and prediction, in the illuminating discussions of model-based predictions and of the looseness of fit between predictive performance and the truth of scientific theories. Consequently, he defends a predictive realism (p. 17), according to which prediction is an epistemic enterprise which is both enabled by the existence of structural patterns or regularities in the natural world and hampered by the three prediction spoilers chance, chaos and choice (p. 77). There are also cognitive limitations to prediction which are due to errors of commission (mistakes and misinformation) or omission (ignorance and vacuity) (p. 67). Nevertheless, reliable predictions are clearly possible and play a major part in many domains. As predictions must be based on sensible and reliable evidence, Rescher constantly reminds the reader of the importance of the availability of scientific laws. There is only a very brief discussion of what we are to understand by the laws of science and how they may differ from the laws of nature (p. 176). But the emphasis on the importance of structural patterns seems to suggest that the laws of science express in an idealized and abstract way fundamental structures of the natural world. Their availability for the purpose of prediction can therefore not be underestimated. Appeals to other epistemological features of science like models and theories also figure prominently in the book.

In the first part Rescher discusses prediction quite generally. No attempt is made at this stage to distinguish between, say, forecasts in everyday life and predictions in the natural and the social sciences. This leads to misleading assertions. Rescher quite rightly affirms that 'rational prediction as such does not require a mastery of scientific details (p. 57).' For instance most people can confidently forecast that there will be a full moon within the next 30 days or predict that magnets will attract iron filings. This is a safe and reliable prediction. It does not follow from this, as Rescher asserts, that scientific predictions are on the same footing *qua* predictions as everyday predictions.

For the safety of everyday predictions is acquired at the price of uninformativeness, but this does not necessarily hold for scientific predictions. Predictions should be both highly informative and preferably numerically accurate as well as firmly rooted in evidence. Unfortunately, the 'crux of the matter' seems to be that there must be a trade-off between security and informativeness in predictions: '*the more informative a forecast is, the less secure it is, and conversely, the less informative, the more secure it is*' (p. 62; italics in original. Rescher vacillates in his use of the terms prediction and forecast). This may be true of predictions/forecasts in everyday affairs and the social sciences but it is a questionable principle in scientific predictions and technological exploits. NASA space missions, for instance, violate this principle and so do many astronomical forecasts. It was a highly informative *and* secure forecast that Hale-Bopp's orbit was closest to the earth on

22 March, 1997. Voyager 2 was launched on 20 August, 1977 and after having crossed the orbits of Jupiter, Saturn and Uranus at a distance of 100,000 km finally flew past Neptune on 25 August, 1989 at a distance of less then 5,000 km. It is difficult to uphold that 'predictions must conform to this vexatious general principle,' without offering any further qualifications.

The existence of structural patterns and structural regularities, expressed in the laws of science, clearly destroys the presumed trade-off between security and informativeness. The many references to natural laws and structural patterns do not render the chapter on predictions *in* the natural sciences more illuminating. Although Rescher uses the familiar example of the prediction of the existence of the planet Neptune from slight deviations in the orbit of Uranus and gives a lucid discussion of quantum indeterminacy elsewhere in the book, his discussion under the heading *Law-based prediction* produces no more than the banal example:

> It is thus a safe prediction that if a town has 1,200 pupils in the first grade this year it will have roughly 1,200 second-grade students next year.

It is difficult to call this a good example of a forecast, let alone a *scientific* prediction.

It is not just the prediction of *new phenomena,* like the bending of light near massive gravitational bodies or Dirac's prediction of the positron, which is of interest in this connection. It is also the *accuracy* with which such predictions are made which should have been particularly stressed in this section. For in contrast to predictions in the social sciences and everyday affairs, scientific predictions clearly stand out with respect to the numerical accuracy with which they are made. Edmund Halley, the Astronomer Royal and friend of Newton's, predicted the return of what is now known as Halley's comet for the year 1758 which was then sighted on Christmas night of that year. In later years the predictive accuracy improved to astonishing levels: Einstein famously predicted the perihelion advance of Mercury to within 4% of the observed value; and quantum electrodynamics predicts that the magnetic moment of the electron has a value of 1,001 159 652 201 ± 0,000 000 000 030 (J/tesla). This compares very well with a recently observed value of 1,001 159 652 188 ± 0,000 000 000 004. In each of these cases of scientific predictions the apparent trade-off between security and informativeness is violated. Where established lawful regularities are available the predictions can both be highly informative *and* secure.

After the disappointing section on predictions *in* the natural sciences, a somewhat repetitive and redundant chapter follows on predictions *about* natural science. We cannot predict what specific scientific discoveries will be made in the future but we can predict the rate and volume of scientific growth. It is surprising that Rescher does not refer to de Solla Price's classic study *Little Science, Big Science* (1963), which said much about the exponential growth of science over the last number of decades. In his deliberation of the future *volumetric* development of science Rescher also neglects aspects of unification and the finalization thesis. It may be true

that we can 'safely assert that the adequate exposition of a future science (...) will take up many more pages than ours' (p. 189); but if the past development of science is a reliable judge of its future progression, the subsumption of many domains of scientific inquiries, considered to be independent today, under some unifying principles can also be considered to be likely to occur. In this connection the finalization thesis which was not only propounded by sociologists of the Sternberg Institute under the leadership of Jürgen Habermas, but also discussed by such eminent scientists as Werner Heisenberg and more recently Stephen Hawking is worth considering. According to this thesis the most fundamental equations which govern matter at the most basic level will soon be discovered, making all other material processes, *in principle,* deducible.

These somewhat disappointing sections are quickly forgotten when Rescher turns to the question of predictions in the social sciences. The major part of the discussion is devoted to the problems of predictions in economics, Friedman's instrumentalism and Popper's ideas about the unpredictability of historical events. Rescher mentions the major obstacles—the problem of non-closure of social systems, the human agency factor and the dependence of social trends on initial conditions—which make the business of prediction in the social sciences much less accurate and reliable than predictions in the natural sciences. Because of the lack of deterministic or even statistical laws, the aforementioned trade-off between security and informativeness comes to the fore again. The point is well illustrated in a difference in the use of models for predictive purposes between the two branches of science.

> In the case of modelling in physics, we understand the background phenomenology and its laws pretty well, and since they are stable we can *apply* this well-established theoretical information in making our predictive models. (...) But in economic modelling, it is the model itself that is supposed to provide for our understanding of the phenomenology—we have no prior independent, well-confirmed laws on which to base its operations (p. 197, italics in original).

This is not the first time that Rescher employs the notion of model. As in his use of the notion of laws of science, little is said of the different types of models used in the natural and social sciences respectively. There is some indication of what a model is but not how it is to be distinguished from a theory.

These critical remarks should not be read as a negative assessment of the achievement of this book. If this is indeed the first book on the theory of prediction-in-general, it clearly depicts the vagaries of the predictive enterprise. As Rescher stresses again at the end, it highlights the cognitive situation of *homo sapiens*. Because of the cognitive and ontological obstacles, humans must inhabit a predictive halfway house, which rests on pillars strong enough to support realism, not only of the models employed, especially in the natural sciences, but also of theories and predictions.

The book should be useful to both general readers with an interest in philosophy and undergraduate philosophy students who will detect the

philosophical implications of Rescher's predictive realism. For those with a more particular interest in the philosophy of science, Rescher offers an interesting discussion of the DN model of explanation, developed by Hempel and Oppenheim (1948), and in particular the symmetry claim between prediction and explanation. From his realist standpoint, Rescher cannot accept the symmetry thesis and replaces it by the harmony thesis. Although he regards 'predictive efficacy as the best token for the explanatory adequacy of scientific theories' (p. 164), he warns against the inference from predictive success to the truth of scientific theories. His warning is illustrated by a long (and familiar) list of theories, which resulted in good predictions, but were made on the basis of false explanatory mechanisms.

Rescher is a man of great erudition. He delivers a clear, coherent and constructive argument, with well-chosen and illustrative examples. The text has numerous footnotes, which contain explanatory notes or very useful references to the large body of literature. The bibliography is extensive and offers the interested reader a chance to delve deeper into the art or philosophy of the predictive enterprise. There is much else in the book, which would be worth mentioning: for instance a survey of predictive methods, elements of Bayesianism, prediction and probability and many finer distinctions about predictors. Hopefully enough has been said to describe the flavour of this book. It is in the best spirit of Rescher's theses if this reviewer desists from predicting how successful the book will be. But as a general theory of predicting and forecasting the book no doubt renders a dual service: it spells out clearly the scope of a predictive realism and in doing so provides an introduction to some basic issues in the philosophy of science from the predictive angle.

Friedel Weinert

A New Stoicism
By Lawrence C. Becker
(Princeton University Press: Princeton, New Jersey 1998), 272pp;
ISBN 0-691-01660-7 £22.50

Lawrence Becker offers an effective antidote to the caricatures of Stoic theory which still infect the literary and philosophical world. On the one hand, Stoics are supposed to advocate indifference to all the things that ordinarily make a life worth living, and to make demands that no-one could ever expect to satisfy. On the other, they are supposed to have committed that gravest of meta-ethical crimes, 'the naturalistic fallacy', inferring moral norms from natural law. 'Doing what comes naturally' is right, but all the things that 'naturally' we desire or do are ethically indifferent. Some of these criticisms were made of Stoic theory from the beginning; others are directed to what 'stoicism' has become. Being 'philosophical' about one's life, one's troubles and attachments, is to seek to see them in the light of larger worlds, and so exchange ordinary unhappiness or eupho-

ria for a more widely diffused depression, immune to any particular evil. Good 'stoics' think that pleasure is a trap, and distrust personal attachments. Good Stoics, so Becker plausibly contends, are far less fearful, though they would agree that a 'good life' is not only one that's pleasurable, nor one devoid of suffering.

What Becker offers—though he makes good use of standard exegesis of the classical Stoics—is not intended as an accurate account of what the Founders said. His 'New Stoicism' is rather what Stoics would have come to say (he thinks) if their tradition had had a continuous history, and individual Stoics had had to confront the challenges of Enlightenment and Evolutionary thought. Ancient Stoics supposed that Nature and the Universe itself was a single teleological system, and that right-living lay in consciously going along with Nature', doing what Nature demanded because we *saw* it did (rather than, as other things must do, doing what Nature demanded just because it did). Becker considers this an aspect of the theory that is not now believable, and offers a Neo-Stoicism that dispenses with such cosmic themes as that the Universe is what God's will and reason makes it, and that it is to be endorsed by any who would be God's friends. 'To follow Nature', in Becker's novel interpretation, is just to take account of all the relevant facts when deciding what to do: to acknowledge natural limits and take account of natural consequences. No doubt this is good advice. Living the good life consists in flourishing as a member of one's kind, and the kind in question is, for us, the sort of rational agent that has many lesser goals and contexts. To live well, or as well as we can, we must seek to do what—all things considered—is the best we can, and learn to control our own mental states to enable us to reach that best. Of course no Stoic is entirely immune to disaster: some lives collapse in ruins, no matter what the agent's character and acts. But Becker, in the Stoic tradition, argues plausibly that suffering and public failure need not compromise a virtuous agent's happiness, and (still more plausibly) that being vulnerable to complete disaster is not itself a good that any reasonable person (*pace* Nussbaum) should desire.

Although he has abandoned what would once have been a vital element of Stoic theory, he seeks to sustain many of the inferences that the Stoics drew. Although his Neo-Stoic Universe is no longer thought of as a divinely-ordained whole, it is still deterministic. Like the ancient Stoics, Becker sees no problem in rejecting 'metaphysical liberty' while ill praising both negative and positive freedom. To live the lives we wish to live (all things considered) we need not to be impeded, and we also need such aids as friends, courage, physical strength, to enable us to act and to endure. We may often need to be 'liberated', but this does not demand that we be always *able* to do both one thing and its opposite. Although his Neo-Stoic Universe is not arranged for the benefit of human beings, or even human sages, it seems that human agency need take no account of any non-human interests. It is not clear why, learning that things are as they are, and that most people will be disinclined to give more weight to the interests of distant strangers or non-human animals than to their family and friends, we should be *glad* of this. Why should 'the effective exercise of agency always

bring joy, even in the midst of misery'? Certainly it doesn't always for 'us imperfect ones', and it is not clear that a Neo-Stoic sage (unlike a Stoic one) can find much reason to rejoice in the cosmic drama of which, willy-nilly, she's a part.

Becker's expository chapters, in which he examines issues of deontic logic, as well as the anatomy of virtues and the emotions, are accompanied by brief commentaries and bibliographies, laying out the original scriptures of the Stoic tradition. Occasionally, he imagines a larger distinction between the ancient schools than in fact is plausible: Aristotle's eudaimonism is not that different from the Stoic version (good fortune is no *part* of happiness, even if, for most of us, the exercise of virtue will need some luck); Epicurean nostrums may not be all that different, in practice, from the Stoic ones. It is also unfortunate that Becker takes so little account of the actual developments of Stoic thought in all the centuries when, he says, it was undeveloped: why not draw upon the resources of Boethius, or Spinoza, or Russell? Conversely, why not seek to adapt the Stoic, anti-animal, tradition in the light of recent evolutionary thought, as Rachels or Midgley have done? Finally, it is not altogether obvious that there is a real distinction, for anyone aspiring to be Stoical, between what we are *required* and what we *ought* to do (even if the latter is equated simply with what it would be good for us to do). If (all things considered) it would be good for us to perform some act (and not good for us not to) are we not required to do it?

But these criticisms, as well as the thought that some of Becker's chapters are a little tedious, identify small faults. *A New Stoicism* is a fine contribution to the theory and practice of philosophical therapy.

Stephen R. L. Clark

Living High and Letting Die: our illusion of innocence
By Peter Unger
(Oxford University Press: New York & Oxford 1996), 199pp.
ISBN 0195075897 £35.00; 0195108590 £13.50

Most of us would agree that anyone who saw a child drowning and could easily save her, but did not, at least has some explaining to do. Most of us would agree that, if the explanation was only that they were busy, or tired, or cross, or wanted not to damage their new suit, they must be moral monsters. But very few of us do all that we quite easily can to save the lives of children far away—perhaps by donating minor sums of money to allow the purchase of oral rehydration salts for children who will otherwise die of diarrhoea. Even fewer of us think that we are moral monsters for not doing so. Unger's typically blunt conclusion, after Peter Singer, is that we are wrong.

The chief point of Unger's book is doubtless to encourage his fellow-Americans to considerably more 'charitable giving' and international aid. But there is a different, meta-ethical issue of almost as much philosophi-

cal importance: granted that most of us do treat the cases (of the Drowning Child and Oral Rehydration Pack) entirely differently, what is the proper source of ethical commitment? Must we seek to preserve our intuitions in the face of theory, or allow some of our intuitions to be trumped, or changed, by argument (or by superior intuition)? Unger's goal is not to answer that question, but rather, by successive stories, to amend our attitudes. How this is done, he acknowledges, may be as much a matter for psychologists (or rhetoricians) as for philosophers.

Sometimes Unger reports 'our' intuitions incorrectly, or at least contentiously: it is not obvious that Washington and Jefferson, for example, are excused for owning slaves merely because they lived before the moment when slave-owning ceased to be respectable. Maybe Americans excuse them only because it is important, to Americans, that those great men were good. Britons, by contrast, seem to want their heroes to be rogues. But the conclusion may still be accurate: that we may now complacently do things that our descendants will think terrible, without ourselves being utterly terrible people. We may, for example, complacently let distant innocents die needlessly. In the future (maybe) people will take it for granted that they must act on the Pauline request to meet the needs of others from their surplus (*2 Corinthians 8.1–15*): now, that is seen as supererogatory.

Sometimes Unger adopts a metaphysical agnosticism that is perhaps an error: it does not matter, so he says, whether or not it's *rational* to help the innocent nor whether or not it's really *true* that we ought to do so. Perhaps it doesn't even matter whether they are 'really' in any need, or that we know they are. It's enough that 'we', being decent people, wouldn't let an innocent child suffer or die for the sake of some immediate advantage. If that is so, it remains a puzzle why we condemn ourselves and others for not giving aid to drowning children, or bleeding strangers, when they are close at hand, and yet think nothing of ignoring pleas for help from charitable agencies concerned to feed the hungry, heal the sick or rescue the oppressed. Even if it's not *true* that we ought to help, nor *rational* to help, why are our feelings different? Standard responses (that we can't avert all distant tragedies or that there are others who could help as well but don't or that there isn't a *particular* child we help by donating money) don't mark real differences between the cases. To save the life of someone close at hand we are prepared even to excuse such minor crimes as theft, but still reckon it wrong to encourage the expropriation of company or aristocratic assets to save distant lives. These feelings, so Unger contends, can be gradually brought in line, until we are as ready to help distant as present victims and by whatever means.

Like Singer, Unger advocates a form of utilitarian calculation that denies the existence of any prior restraints on what may be done to multiply utility. It is right to cheat, steal, maim or even kill if this is how to save sufficient lives. It must accordingly be right to accept such minor harms ourselves, and so respond to 'charitable appeals' even if it is 'at the cost of hardship' (as for most of us, it isn't).

The situation Unger is addressing is unfortunately very real. Lives that

could be saved, and lived in beauty, if we were all (or enough of us) prepared to care, will actually be lost or lived in misery. There are enough real cases without tedious use of the stock of philosophical puzzle cases (runaway trolleys, famous violinists, the medicine extracted from a boiled human foot, and all the rest). The chapters where Unger makes most use of these are at the least unhelpful. They may be worse: once it is agreed that we may assess harms, caused and prevented, in this way, it is no longer quite so clear that we should seek to save the distant innocent. What Unger calls 'disastrous further futures' (perhaps to be avoided by not helping distant victims now) are not necessarily an 'hysterical fantasy' (p. 37). Institutional aid, to be sure, has had a worse record than has 'private charity' in distorting markets, damaging environments, and raising foolish expectations, but we need clearer arguments than Unger actually provides to answer the Social Darwinist challenge—that it may be positively *wrong* to aid the weak—or the environmentalist—that only a minority can ever be allowed to enjoy as expensive a life-style as Contemporary American. And if it *may* be wrong, we cannot wholly condemn ourselves or others for not thinking that it must be wholly right.

Unger's humane intuitions are more robust than any utilitarian calculation of the greater good (which *might* require us to surrender all our assets to assist the starving, but might instead demand the death of millions to preserve a global future). What is odd is his failure to notice the existence not just of Social Darwinists or radical environmentalists but of traditional moralists who may doubt that 'improving life' (in the sense of providing more consumables) or even 'saving life' is ever a strict duty. Pity has not always been a virtue. Helping others, whether present or distant, may be to impose on them. Helping is all too often equivalent to harming: many of those for whom appeal is made may actively resent being categorized as 'victims', whom others 'ought' to aid. Traditionally, those who save a life are thereafter held responsible for it: filling the charity envelope may be to evade responsibility. It may be that we are required to care for family and friends and neighbours (Including chance-met strangers) rather than for 'everyone in need', in part because those near at hand are in a position to protest, and to return the compliment: 'at the moment your surplus meets their need, but one day your need may be met from their surplus' (as Paul said). Once again, *not* filling the charity envelope may be the better course—and if it *may* be so, we cannot condemn ourselves or others for not always thinking that it isn't.

Stephen R. L. Clark

A World of States of Affairs
By D. M. Armstrong
Cambridge University Press, 1997, xiii + 285 pp., £14.95 & £40.00.
ISBN 0521589487 (pbk); 0521580641 (hbk).

David Armstrong's metaphysics, first spelt out in his 1978 *Universals and Scientific Realism,* has had a huge influence over the last twenty years, and

still sets the agenda for much contemporary discussion. In many ways it represents a development of Bertrand Russell's metaphysics as described in his lecture series, 'The Philosophy of Logical Atomism' (1918). Like Russell, and Locke before him, Armstrong is trying to characterize the world as it *really* is; that is, not as it merely comes across from one viewpoint or another. The world as it really is stands apart from our characterizations of it, but related to them by being their *truthmaker*. Armstrong also claims that there is nothing transcendental about the world as it really is; we have access to it by means of science. So he is a scientific realist.

Since 1978 Armstrong has found several opportunities to reiterate and develop his views. But they have remained pretty consistent up to and including his latest book, *A World of States of Affairs*. There are no great surprises in this book, but there is useful engagement with the arguments that have sprung up since 1978 as well as some adaptations of positions. The Armstrong approach to metaphysics is presented clearly and comprehensively. The presentation concentrates on being concise at the expense of going more deeply into the arguments, but gives the reader enough reference points to follow up the detailed arguments elsewhere.

Armstrong's central claim is that all that there is is a world of states of affairs. States of affairs are simply ordered instantiations of n-adic universals with n particulars. An example of the sort of thing that counts as a state of affairs is a particular, *a*, standing in a certain relation, *R*, to a particular, *b*. In the first couple of chapters we get some useful stage-setting for Armstrong's approach to metaphysics and in particular we get some clue to what he means by the phrase 'all that there is'. Armstrong is looking for some basic grounding for the existence of things. We might observe telephone poles and people and conversations, but describing the world in terms of these things does not describe what is 'at the bottom of the world'. What Armstrong is looking for is something that exists and *entails* the existence of everything else—something which is the basic 'truthmaker' for all truths.

The significance of this search for what there is at the bottom of the world depends on two assumptions. The first assumption is a kind of reductive physicalism: there is some way of describing the world which does not mention things like telephone poles; but the world described that way *entails* the existence of all things. This way of describing the world is assumed to be based on what Armstrong calls 'completed physics'. What this physicalist assumption amounts to is the *claim* that there is nothing whose existence is not entailed by the existence of things which are governed by the laws of physics—i.e., according to Armstrong, the space-time system.

For Armstrong, one thing *entails* the existence of another if it is impossible for the first thing to exist and the second not to. What Armstrong means by this is spelt out paradigmatically in terms of identity and partial identity. One thing's existence is entailed by another when the first thing is *contained* in the other. This containment may be merely mereological, but need not be. For example, it is a central thesis of the book that a state of affairs contains particulars and universals and so entails their existence,

but is not just the mereological sum of the particulars and universals, and so is not itself entailed by these constituents.

The second assumption is described by Armstrong as the principle of the ontological free lunch. This, I think, is the key structuring principle of Armstrong's metaphysics, and many of the stranger results that emerge in this book can be traced directly back to it. It is the principle that if the existence of one thing is entailed by others then that thing is 'nothing over and above' the others. 'What supervenes is no addition of being' (12). For example, the existence of particulars and universals is entailed by the existence of any state of affairs that contains them, but not *vice-versa*. So, according to the principle of the ontological free lunch, particulars and universals are nothing over and above states of affairs.

This result is quite problematic. It means, for example, that the sun has no existence over and above the state of affairs of the sun's being hot. But, if the sun really has no existence over and above *that* state of affairs, how can it make sense to say that the very same object is a constituent of entirely distinct states of affairs like that of the sun being such-and-such a distance from the earth? Armstrong seems forced to accept both that particulars and universals have no existence over and above any state of affairs that instantiates them and that particulars and universals exist *independently* of any particular state of affairs, since they can be repeated in different states of affairs. There seems to he some unexamined tension here between the search for the basic *constituents* of the world and the search for the basic *truthmakers* in the world.

In the next few chapters Armstrong outlines his position on universals. Armstrong claims to be a realist about universals, even though, given the existence of states of affairs, universals are just ontological free lunch. What the realism seems to amount to is a refusal to admit that universals might be mere abstractions from true predications. But his realism, here as well as more generally, is constrained by an aversion to transcendentalism—an aversion that is formalized in his physicalist claim that nothing exists that is not entailed by the laws of physics. So universals do not exist except as being instantiated by particulars in states of affairs. (Uninstantiated universals apparently have no role in physics.) But, for Armstrong, universals are not tied to particular instantiations. One and the same universal exists as part of many different states of affairs. This distinguishes universals from tropes or 'property instances' which are essentially connected to the particulars that instantiate them and merely *resemble* instances of the same property in other particulars.

Armstrong's position on particulars parallels his position on universals. His realism is such that he does not even consider the influential Kantian view, developed by Strawson in *Individuals* (1959), that the nature of particulars is constrained by the pragmatics of identification. Rejecting the identity of indiscernibles, he has to admit some degree of haecceity ('thisness') to particulars. A world consisting just of the two states of affairs, *Fa* and *Gb,* is taken to be distinct from a world just consisting of *Fb* and *Ga*. Identity and difference of particulars (as of universals) is taken to be basic—independent of the instantiations the particulars figure in.

But this seems to be just the kind of transcendental thesis that Armstrong's physicalism should rule out. What seems strange is a radical difference between Armstrong's treatment of existence and his treatment of identity. The *existence* of particulars just as with universals) is dependent on their being instantiated. This is demanded by physicalism. Since uninstantiated particulars and universals would have no causal role, they will not need to he postulated by completed physics, and are accordingly ruled out of Armstrong's ontology. But, according to Armstrong, the *identity and difference* of particulars (or universals) is not dependent on how they are instantiated. So he regards the identity and difference of particulars and universals as transcendental with respect to physics. The distinction between the two worlds—the *Fa* and *Gb* world and the *Fb* and *Ga* world—is one that transcends anything that completed physics might pick up, yet, for Armstrong, it is no less real for that.

Chapter 8, 'States of Affairs', brings particulars and universals together. States of affairs are made up out of universals and particulars. But they are not just the mereological sums of universals and particulars. This is because there are plenty of combinations of particulars and universals which are not instantiated in states of affairs. Such combinations can he made promiscuously and correspond for Armstrong to *possible* states of affairs. To describe the actual world it is not enough to specify the particulars and universals that exist; one must specify the states of affairs as elements over and above their constituents.

For Armstrong, possibility is based on actuality, but not to the extent that the only possibility is actuality. The possibility of a state of affairs is made true by the actual existence of its constituents in some perhaps quite different combinations. Recombinations within actual states of affairs give possible states of affairs. But it is quite difficult to see how the mere existence of particulars and universals could make true the possibility of some state of affairs involving a new combination. According to Armstrong, a possible state of affairs is not just a possibly new combination of particulars and universal. A state of affairs is something over and above the combination—it is an instantiation. So possible but non-actual states of affairs are constructed out of actual particulars and universals, but also involve non-actual *instantiations*—entities which are over and above these constituents. In Armstrong's combinatorial account of modality, there are no actual features of the world which make possible the non-actual instantiations of particulars and universals. There is nothing one could point to in the world which makes combinatorialism about possibility, as opposed to actualism, true.

In fact, the contingency of different combinations of particulars and universals is at least partly grounded in Armstrong's principle of the ontological free lunch. If one state is necessary given others, then, according to this principle, it does not exist over and above these others. So there can be no necessary connections between properly distinct states of affairs. If Armstrong had allowed negative states of affairs, then this argument could have been extended to show that no negative state was necessary, and therefore that all positive combinations are possible. But Armstrong does

not allow negative states of affairs (nor does he allow disjunctive or inferential states of affairs). Nevertheless, it is clear that there is an intimate connection between Armstrong's claims about contingency and his principle of the ontological free lunch.

The denial of necessary connections between states of affairs might be assumed to lead to a regularity theory of causation and of laws. But Armstrong tries to allow for a strong conception of laws while maintaining no necessary connections between distinct states of affairs. Laws are *second order* states of affairs which relate is of first-order states of affairs. It is true, according to Armstrong, that given the existence of a law, and the existence of a certain token state that satisfies the antecedent of the law and given the existence of the totality of non-interference conditions, the existence of the effect is entailed. But, given that the states of affairs figuring in this entailment are a mixture of higher- and lower-order states, we cannot assume that there is no overlap between them. Necessary connection between overlapping states of affairs is not a problem for Armstrong.

In the end, Armstrong's book is a useful and interesting development of Russell's metaphysical project. If one can swallow the assumption of physicalism and the principle of the ontological free lunch, one can use this book as a guide for exploring the extraordinary world of ontology that opens up as a result. But it is frustrating that Armstrong does not regard it as necessary to defend his basic assumptions in the light of alternative approaches to metaphysics. Someone who worries how the search for the fundamental constituents of the world could make any sense at all may feel stranded from the very beginning.

Rowland Stout

Harmless Naturalism
By Robert Almeder
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Philosophy is unique among human pursuits in the amount of attention it pays to itself. Ever since people first began calling themselves philosophers some 2,500 years ago, much of their time has been spent in trying to understand what is it exactly that they are supposed to be doing. But no clear answer has emerged, as is often the case with perennial philosophical questions. In modern times the debate has intensified. Some philosophers, citing the appalling lack of progress in their discipline, demanded that traditional ways of doing philosophy be either abandoned altogether, in favour of some new methodology, or at least very drastically modified. These arguments were promptly met with counter arguments which contended, no less zealously, that traditional philosophy ought to be carried on and that it is the objections against it that are unsound. Robert Almeder's *Harmless Naturalism* falls squarely into this latter category.

Almeder's arguments are forceful and lucid, but they suffer from one, not altogether inconsiderable flaw. His arguments are constructed on the

basis of the time-worn methods and assumptions of traditional philosophy. These are the very methods and assumptions condemned by its opponents. So if the book is to be judged on the basis of its ability to convince the critics of traditional philosophy—rather than only those who accept its basic principles with little or no scruples—then the outlook is not very promising. As will be seen in a moment, these critics will find in *Harmless Naturalism* much of what they believe to be wrong with the philosophical orthodoxy.

Ostensibly the book is a critique of naturalized epistemology. But Almeder's real purpose is far more expansive. His defense of traditional epistemology is only a prelude to his main thesis. This thesis is a conception of philosophy which is intended to vindicate traditionalism—or at least one version thereof—and silence its critics at the same time. But to evaluate this thesis, it is necessary to take a look at Almeder's attack on naturalized epistemology first.

Although Almeder discusses several proposals to reform epistemology, the brunt of his criticism falls on the so-called 'replacement thesis.' This doctrine, usually identified with the name of Quine, seeks to transfer all questions about the formation, retention, and rejection of beliefs from the domain of philosophy to the domain of science. It denies that traditional epistemological questions about the nature of knowledge and justification are capable of advancing our understanding of these matters in any meaningful sense. By focusing on the 'replacement thesis,' Almeder hopes to refute, once and for all, the general notion he calls 'scientism,' according to which no correct answers or explanations about the world are attainable by any means other than those of the natural sciences. If the refutation of 'scientism' is successful, there must be some features of the world discoverable by other methods, and these, Almeder hopes to show, are the methods of traditional philosophy.

After an exhaustive survey of the various versions of the 'replacement thesis,' with their respective objections and counter-objections, Almeder finally delivers what he believes to be the decisive argument—the charge of inconsistency. '[T]he replacement thesis,' he writes, 'as both an assertion and a proposed answer about the limits of human knowledge, is inconsistent with the very statement of the thesis' (68). In other words, if science has a monopoly on correct answers, as the 'replacement thesis' states, then the thesis itself, not being a scientific answer, must be incorrect. This seeming inconsistency leads Almeder to conclude that 'not only is there no sound argument presently available supporting the replacement thesis, there never will be a sound argument for it' (74).

As Wittgenstein's followers will undoubtedly be quick to point out, this triumphant conclusion is somewhat premature. The alleged inconsistency, it will be argued, depends entirely on the interpretation of the 'replacement thesis' as an answer to some meaningful question 'about the world,' i.e. about 'the limits of human knowledge.' But this interpretation is not the only one available, and it presupposes precisely what Wittgenstein and others have denied—that it is the business of philosophy to give answers or explanations 'about the world.' Philosophy, according to this well-

known Wittgensteinian dictum, is nothing more than the 'battle against the bewitchment of our intelligence by means of language.' Its role is to unmask the illusions created by grammar, and among them is the illusion that there are valid epistemological questions capable of teaching us something 'about the world.' The 'replacement thesis,' then, does not answer or explain anything; it simply points to the inherent errors of one particular way of looking for answers. So if one were to adopt the Wittgensteinian dictum, one could adhere to the replacement thesis' without the slightest inconsistency: since the thesis itself does not purport to be a philosophical answer 'about the world,' it is unaffected by the contention that all such philosophical would-be answers are illusory.

Whether this is a sound argument may well be worth considering, but it is clearly a mistake to ignore it altogether, as Almeder does. The difficulties multiply when from this putative refutation of the 'replacement thesis' he infers that there must be valid philosophical answers or explanations which are 'sources of factual information about the world' (163). Convinced by the strength of his own argument, Almeder accepts it as a necessary truth that there must be such philosophical explanations, and he feels compelled to produce some evidence of their existence. This quest forms the substance of Almeder's main thesis—his 'harmless naturalism.' But the edifice erected atop such a hastily constructed foundation proves to be a very shaky affair indeed.

The central feature of 'harmless naturalism' is the line of demarcation separating scientific from philosophical arguments, presumably designed to stop the harmful usurpation of the latter by the former. Scientific explanations, Almeder writes, are distinguished by the fact that they are explicitly empirically testable. At the time when they are formulated, it should be possible to specify the detailed conditions under which they can be tested and either confirmed or disconfirmed. By contrast, philosophical explanations are implicitly empirically testable. This means that they must also be capable of being either confirmed or disconfirmed by empirical data, but it is not necessary that at the time of their formulation the exact test conditions be known. The only thing that is required, according to Almeder, 'is that there be some real probability that there is some specific observational data implied by the hypothesis and the occurrence of which data, under suitable provisos, would allow us to accept or reject the hypothesis' (168).

Although Almeder insists that *all* philosophical arguments are empirically testable and falsifiable, he relies mainly on two examples of what he calls 'noncontroversially successful [empirical] refutations of philosophical explanations' (181). Since Almeder stakes the fortunes of 'harmless naturalism' on these two examples, it may be instructive to take a closer look at them to see how well they really support his thesis.

The first example is the argument attributed to Aristotle, which states that humans are superior to animals because humans do and animals do not use tools. This argument, Almeder writes, has been peremptorily refuted by Jane Goodall's research on non-human primates which showed that they do in fact use tools. This much is undoubtedly true. The question is whether 'Aristotle's argument' satisfies Almeder's definition of a

philosophical argument. If it does, then there is at least one piece of solid evidence in support of the 'harmless' thesis. But does it? Almeder's definition demands that if 'Aristotle's argument' is to be counted as philosophical, it is necessary that at the time of its inception neither Aristotle nor anyone else be able to say specifically what observational data would be required to have the argument either confirmed or disconfirmed. Otherwise the argument would be scientific rather than philosophical, which would make it useless for Almeder's purposes. Now, does this mean that the father of classical logic had no idea that the discovery of tool-using non-human animals would refute his argument (if it was indeed his argument)? No, says Almeder, but he [Aristotle] 'had no specific idea of what to do under what circumstances to produce disconfirming observational data' (169–170). In other words, if Almeder is correct, Aristotle would have been at a loss had someone asked him to devise an empirical test for the hypothesis that animals do not use tools. Whether or not this is true may be a fascinating subject for Aristotelian scholars to debate, but those who are more interested in Almeder's defense of 'harmless naturalism' will probably want something more tangible than these nebulous speculations as to what Aristotle knew or did not know.

It is not very hard to see why 'Aristotle's argument' does not do what Almeder wants it to do. The argument is derived from two premises. One of these premises is a purely normative statement which asserts that tool-users are superior to non-tool-users. The other premise is a purely empirical one. It states that non-human animals do not use tools. Aristotle's argument follows syllogistically from these two premises, and it is refutable by way of falsifying one of them. But the first premise, being normative, is clearly unfalsifiable. So the only way to falsify 'Aristotle's argument' is to falsify the hypothesis that animals do not use tools. This hypothesis, however, does not sound much different from any other empirical proposition, and this is why most people will find no reason to believe that its testability is any less explicit. To make his example work, Almeder needs to supply this missing reason, but all he can offer are some vague conjectures about Aristotle's state of knowledge. Not surprisingly, the result is less than convincing.

Almeder's second example of an empirically refuted philosophical argument is what he calls 'Hume's argument for other-mind solipsism.' 'Any reasonably accurate formulation or reconstruction of Hume's argument,' Almeder writes, 'will need to list as a premise in the argument Hume's claim that there is no idea for which there is not a corresponding impression of sense from which it is derived' (171). And this latter claim is 'straightforwardly refuted by the empirical fact, for example, that we do have an idea of a chiligon [sic] which could not plausibly be regarded as derived from a sensory impression of a thousand-sided figure' (172). Almeder goes on to say that any of Hume's theses which rely on the same premise about ideas and impressions can also be safely regarded as conclusively refuted; and these include 'his theses arguing for the non-existence of the self, the non-existence of God, the non-existence of necessary connections and causality, and the non-existence of the external world' (172).

Some readers will probably be surprised by this rather loose interpretation of Hume's theses. Hume too may have been amused to find himself labelled an 'other-mind solipsist.' But if it is at least debatable whether or not Hume ever argued for the 'non-existence of the external world,' one thing seems to be fairly certain—Hume never claimed that 'there is no idea for which there is not a corresponding impression of sense. 'A more careful reading of the pertinent texts would have revealed that only *simple* ideas, according to Hume, are derived from impressions. Had Almeder taken trouble to ensure that his reading of Hume is accurate, he may have realized that his chiliagon objection is put to rest by this minor correction. Since the idea of a chiliagon does not fall under Hume's conception of a simple idea, it does not threaten his hypothesis any more than the idea of New Jerusalem does. But even setting aside this unfortunate confusion, it would still be interesting to learn in what way exactly does our ability to form an idea of a chiliagon invalidate Hume's 'solipsism.' The burden of proof is on Almeder, and he probably asks a little too much of his readers when he expects them to take it on faith that a 'reasonably accurate formulation' of Hume's argument will reveal the connection.

But the most serious problem with Almeder's second example is its inability to illustrate the crucial distinction between philosophical and scientific arguments which lies at the heart of 'harmless naturalism.' To make use of Hume's argument, Almeder must show not only that it has been empirically refuted, but also that it meets his definition of a philosophical argument. As we have seen, the first part of this demonstration leaves much to be desired. The second part is even less promising. It will be recalled that on Almeder's theory an argument is philosophical if at the time of its assertion nobody can specify what observational facts are needed to refute it. But Hume not only knew exactly what it would take to refute his contention about simple ideas and impressions, he said so rather explicitly in his *Treatise* (p. 4 in Selby-Bigge). He went even further and challenged his readers to refute his contention by producing a simple idea without a corresponding impression. The fact that Almeder attempts to do just that—albeit unsuccessfully—makes nonsense of the claim that Hume had no idea what sort of evidence would disprove his hypothesis.

So it appears, after all, that Almeder's two examples are not very helpful in establishing the truth of the 'harmless' thesis. But why go to Aristotle and Hume for examples? Why not turn the spotlight of 'harmless naturalism' on itself and see how well it copes with the rigours of Almeder's definition. If it is a philosophical argument, as it seems to be, then it must bear the marks of implicit empirical testability. Uncovering these marks would arguably be of no less significance than anything that can be said of Hume's or Aristotle's arguments,

But as soon as we turn our attention to the testability of 'harmless naturalism,' problems begin to emerge. First of all, it is not clear whether the thesis is descriptive or normative. If Almeder's definition of a philosophical argument is intended to pick out one quality shared by all arguments commonly known as philosophical, then his thesis is doomed from the outset. Surely Almeder must be aware that many philosophical arguments—

such as various theories of logic, for example—are deliberately insulated from any impact of experiential data. And was it not Socrates, after all, who sought the knowledge of the unseen and the absolute, discoverable 'by the light of reason only, and without any assistance of sense' (Republic, 532)? Is it not absurd, then, to claim that all arguments known as philosophical contain 'factual information' and are empirically testable? The only way to avoid this absurdity is to interpret 'harmless naturalism' as a normative, rather than descriptive, thesis. In other words, 'harmless naturalism' makes sense only as a prescription as to which arguments *ought* to be called philosophical. To put the same thing differently, what Almeder proposes is a new rule for the use of the word 'philosophical.' But as such, it is obviously not empirically testable—either implicitly or explicitly. Whether the word 'philosophical' is used in one way or another is determined by a linguistic convention. There cannot, in principle, be any empirical facts apart from the fact of common usage—which would make one rule more 'true' than any other. So the probability is exactly zero that Almeder's thesis may meet with some refuting empirical evidence at any point in the future. The inescapable conclusion, then, is this: on Almeder's own definition, 'harmless naturalism' turns out to be neither science nor philosophy. What is it then? The reader will have to decide.

Arthur Rubinstein