Real Metaphysics: Replies

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Introduction

To be offered a Festschrift is a great honour; to have such editors and contributors makes the honour greater still; and to be given the last word makes the offer irresistible. The only drawback is that, as saying how much I agree with everyone would take too long, and be less useful to readers, than saying where and why we disagree, my replies may seem ungraciously combative. Still, since we all know how debate can advance philosophy, I hope no one will infer any disrespect from my disagreements. On the contrary, it is to the work and friendship of these colleagues, mentors and students, that I owe much of the understanding and pleasure that doing philosophy has brought me. For that I am very grateful to them all.

Truthmaking, truth and success

1   *David Armstrong*

In my theories of causation (1995) and of time (1998), I invoke the concept of truthmaking to resolve an ambiguity in 'giving a proposition's truth conditions'. This phrase may mean saying what makes a proposition true. But it may also just mean using a metalanguage to say when an object language sentence expressing the proposition *is* true; and that may tell us nothing about what *makes* it true.

   Take time for example. Advocates of tenseless time habitually use a tenseless metalanguage to say when tensed sentences are true, while their opponents use a tensed one to say when tenseless sentences are true. Each side then attacks the other's metalanguage, one giving tenseless (e.g. indexical) accounts of 'past', 'present' and 'future', the other saying that 'earlier' really means 'less future or more past'. And on this semantic issue both might be right: each might be able to say in its own terms when any temporal sentence is true. But they cannot both be right about what *makes* such sentences true, i.e. about whether time itself is tensed. And both are certainly wrong if they think the semantics of time – or of any other contingent subject matter – fixes its ontology. That is why we must distinguish the two and why, because the expression 'truth conditions' blurs the distinction, I avoid it in my (1998) in arguing for a tenseless ontology.

David Armstrong has of course never reduced metaphysics to semantics, as his (1993) theory of the mind shows. He may start with a behavioural account of mental *concepts*, but this is not what makes him identify mental *states* with physical states of the central nervous system. And even if some of us dispute that identity (Crane and Mellor 1990), few today still think that the meanings of mental terms suffice to tell us what makes psychological propositions true.

This is why I share David's belief in truthmaking, understood as his 'cross-categorial' link between a non-propositional entity S and a proposition 'P' that S makes true, a link whose paradigm is, as he says, that between S and 'S exists'. This does not of course reduce truthmaking to the entailment of 'P' by 'S exists', since that is not a link between S and 'P', but it does mean that no one who grasps the concept of existence can credibly claim not to know what truthmaking is or whether there is any.

David and I do however differ on details, and in particular on two of his initial claims: that 'every truth has a truthmaker', and that 'the determination of a truth by a truthmaker is a necessitation'. I disagree: I think that many truths do *not* have truthmakers, and also that some truthmakers do *not* necessitate what they make true. Let me take these points in turn.

First, because the identity of a necessary proposition entails its truth, I cannot see why any other entity must exist to make it true. So in particular, since any contingent proposition 'P' is *necessarily* contingent, I, unlike David, see no need of a truthmaker for the necessary truth that P is contingent and hence that, in this sense, ~P is possible. However, some modal propositions do need truthmakers, because they are contingent: their identity does not entail their truth. These include truths about chances, such as the chance ch(H) of a coin toss landing heads, which I take to measure how possible some fact, say about how the coin is tossed, leaves that outcome; and I discuss these further in §11.

Second, even some contingent truths need no truthmakers: notably true truth functions, whose truth follows from the truth values of their constituents. We may *say* of course that 'P&Q' and 'PvQ' are 'made true' by the truth of 'P' and 'Q'; but this is just the entailment of one proposition by others, not the 'cross-categorial' link between propositions and other entities that concerns us here. That is what true truth functions do not need and therefore, I claim, do not have.

The fact is that only *atomic* propositions, and such non-truth-functional compounds of them as '*a* believes that P', 'If P were the case Q would be' and 'ch(H)=p', need truthmakers. In particular, negative propositions do not need them, since if 'P' is made true by S, all it

takes to make 'P' false and hence '~P' true is that S *not* exist. (I do not of course claim that we can always tell which if either of two *sentences* 'P' and '~P' expresses an atomic proposition: if either does, it will be the one that *does* have a truthmaker.) To postulate a distinct 'falsemaker' for 'P', say ~S, to be a truthmaker for '~P', only raises the gratuitous question of why S and ~S, like an ontological Cox and Box, cannot coexist. It also, as David admits, makes it hard to explain how there could be *nothing*: for what entity could make it true that there *are* no entities? Once we see that negative truths need no truthmakers, that problem disappears.

And so does the otherwise intractable problem of saying what makes generalisations true. Imagine a world with just two particulars, *a* and *b*, to both of which a contingent predicate '*F*' applies. If, as David assumes, truthmakers must *necessitate* what they make true, it will take more than the truthmakers of '*Fa*' and '*Fb*' to make 'everything is *F*' true, since '*Fa&Fb*' does not entail this, because it does not entail that there are no other particulars. But as 'there is no particular that is neither *a* nor *b*' is a negative truth, it needs no truthmaker. All it needs is that no truthmaker for its negation exists, i.e. that no particular other than *a* or *b* exists. So if *a* and *b* are indeed the only particulars, whatever makes '*Fa*' and '*Fb*' true will also make true 'everything is *F*', even though it will not necessitate it.

Similarly for properties. Suppose there are N properties, $F_1…F_N$, for some finite or infinite N. David says in his §3 that we need 'a truthmaker … for the truth that [this] class of properties is the class of all the properties'. But not if all the negative truth 'there are no properties other than $F_1…F_N$' needs is the nonexistence of a truthmaker for its negation, i.e. the nonexistence of any property other than $F_1…F_N$. Here again we have a generalisation made true by entities, $F_1…F_N$, which fail to necessitate it.

In short, David's necessitation principle fails for generalisations, which are not entailed by the conjunction of all their instances, since that conjunction does not entail that there *are* no other instances. But this should not make us reject his principle altogether, only when a truth requires certain entities not to exist.

And once we allow this harmless (because principled) exception to necessitation, we may as well allow another: namely, that where it takes several entities to necessitate a proposition, we may as well call any of them, given the others, a truthmaker for it. Take truths about what is visible in a mirror. To necessitate these we need both the mirror and the objects reflected in it, not to mention the reflected light and the laws of reflection. Yet *given* the mirror, the light

and the laws, we may as well say that propositions about what is visible in it are made true by the objects it reflects.

Similarly with truths about David's beliefs, for example, that he is an Australian. For even physicalists will admit that it takes more than David's brain states to necessitate propositions about what he believes. It also takes laws linking his brain states to how he behaves, and perhaps his living in Australia and not in some 'twin Australia' elsewhere in the universe. Yet given all that, it is an innocuous abbreviation of physicalism to say that propositions about David's beliefs are made true by states of his brain.

2 *David Lewis*

'Any proposition has a subject matter, on which its truth value supervenes', says David Lewis: a proposition 'P' can only be true in one possible world and false in another if those worlds differ in its subject matter. Thus if 'P' is 'there is (actual) styrofoam', then in any world with styrofoam 'P' is true, and in any world without it 'P' is false and '~P' true.

It follows that, as we have just seen, the negative existential proposition '~P' needs no truthmaker in any world, merely the absence from that world of the truthmaker for 'P', namely styrofoam. Yet in their postscript to his paper Gideon Rosen and David argue that propositions like '~P' do in fact have a truthmaker of the kind he offers, namely the world '*qua* just as it is'.

I disagree, for the following reason. David's world is the mereological sum of all its parts, $S_1...S_N$, for some finite or infinite N. But $S_1+...+S_N$ will only be the sum of *all* the world's parts if the world has no *other* parts and, in particular, none that, by being styrofoam, would make 'P' true. Calling $S_1+...+S_N$ 'the world' only begs that question: it does not enable it to make '~P' true.

David's own paper offers truthmakers not for negative existentials but for predications of intrinsic properties. These truthmakers assume his theory of possible worlds, containing only counterparts of particulars in other such worlds (Lewis 1968), a theory which actualists like me and David Armstrong reject. What can we offer instead?

Since, as David Lewis admits, truths can depend on 'whether something is, and ... how something is', the truth of '*Fa*' may depend on *a*'s properties as well as its existence. Even so, as he shows, such propositions can still be made true by particulars, if properties are sets of particulars. But this, we may all agree, is credible only if merely possible as well as actual particulars exist. For truthmakers we may therefore have a choice of package deals: David's many worlds of particulars *versus* an actual world of what in my (1995: ch. 13.4) I call 'facta'

and David Armstrong (1997) calls 'states of affairs', entities which contain properties that are not just sets of particulars.

To our actualist package deals David objects that he does not understand the 'unmereological composition' of our facta – 'unmereological' because a particular a and a property F can exist without a being F: hence the notorious regress of instantiation relations: I linking a and F; I' linking a, F and I; and so on. But I do not face this regress, since I, like Wittgenstein (1922), take ours to be a world of facta, not of particulars. Only my facta are not simples, i.e. tropes: they are structured, because they instantiate laws. Thus if it is a law that everything is G, I say that its instances are not the G-particulars a, b, … but the G-facta Ga, Gb, …, where G is what these facta share and a, b, … are their differentiae. In short, particulars for me exist only in facta, which they therefore need not combine with universals to constitute. All that follows from the possibility of a and F existing without a being F is that laws including F may have no instances that coincide or overlap in spacetime with Ga, which to contain a they would have to do.

In both my theories of change (1981: ch. 7; 1998: ch. 8) I divide temporally extended particulars into *events* (e.g. speeches), which have temporal parts, and *things* (including people), which do not. This I say means that whereas things can change, events cannot, since temporal variation in an event (e.g. a speech getting louder) is just a difference between distinct entities, namely distinct temporal parts of it. But if a is a thing, F is a changeable property and t is a time, I say now (in my 1998: ch. 8.6) that 'a is F at t' is made true not by a temporal part of a – a-at-t – being F but by an Fa-factum located at t. What I failed to see is that, as David rightly assumes, this factum must be *essentially* located at t, to enable it to necessitate the truth of 'a is F at t'. This however is no objection, since it does not imply that a itself must be F at t, merely that, if it is not, that individual Fa-factum will not exist.

David's other upgrade to my theory of change, giving it proxies for his temporal parts, I find less congenial. The idea is that a's history, a_H, is an event with a temporal part a_H-at-t which, by having a property F^* related to F, can provide a truthmaker for 'a is F at t'. But for me, as for Davidson (1970), events are particulars, whose parts, if any, are also particulars; whereas a_H's parts are not particulars but facta containing a. These of course I accept; but not their mereological sum, which is what a_H must be. For just as David does not understand unmereological composition, so I, for reasons I cannot go into here, reject the unrestricted mereological composition which he does understand and accept. That is to say, I deny that any two or more entities automatically compose another of which they are parts and,

specifically, that facta containing a compose any such entity as a_H. In short, *pace* David, I do wholeheartedly reject the temporal parts he offers me, by denying the existence of the whole he thinks they are parts of.

3 *Peter Smith*

I have long endorsed what Peter Smith follows Jamie Whyte (1990) in calling 'success semantics'. This is the thesis that the truth of our beliefs is what makes the actions they combine with our desires to cause succeed in achieving the objects of those desires. Unfortunately I also mistook this thesis to require truths to correspond to facta, or to facts in some other non-trivial sense of 'fact'. Peter and David Lewis (2001) have now persuaded me that this is wrong. Neither success semantics, nor the fact that some truths need truthmakers, either is or needs any such correspondence theory of truth. All they need is the equivalence principle, that any proposition 'P' is true if and only if P, a principle which (with Peter's qualifications) I now think tells us all we need to know about truth.

I also agree with Peter that only contingent propositions need truthmakers, since their identity does not entail their truth, as we noted in §1 that the identity of necessary propositions does. Contingent truths that are not truth functions of other propositions must therefore be *made* true by what our world contains. Peter and I agree moreover that most truthmaking entities are facta, containing contingent particulars (as opposed e.g. to numbers) and contingent 'natural' properties (including relations) whose sharing entails objective resemblance and a similarity of causal powers (see e.g. Shoemaker 1980) – these from now on what being what I shall mean by 'properties'. Properties so understood may be universals (as I think), sets of exactly resembling particulars or tropes, or something else again (see Mellor and Oliver 1997 *passim*); but that is another issue, which we need not settle here.

For what matters here is not what properties *are*, but what properties and hence what facta our world contains, and this I say depends on laws, as follows. First, I extend the idea of a law statement's so-called 'Ramsey sentence' by taking it to replace *all* that statement's predicates, not just its theoretical ones, with existentially bound variables. Then, calling the Ramsey sentence of the conjunction of all actual laws 'Σ', I say that our world's properties are those over which Σ's second order quantifiers must range in order to make Σ true. This is what, in my (1995: ch. 15.4–7), I call 'Ramsey's test' for what properties there are.

But how on this view, Peter asks in his §4, can my facta make propositions like his 'the ice-cream is in the freezer' true, given that neither being ice-cream nor being in a freezer are properties so understood? My answer is, as he says, that they can do so because for me the

world contains more properties than those that figure in 'the ultimate laws of fundamental physics'. Thus, for reasons given in §9 of my (2000b), I say for example that temperatures are properties, distinct from the micro-properties, like the mean kinetic energies of gas molecules, to which laws of nature link them (see §6 below). The freezer, *a*, and the ice-cream, *b*, can then be identified by their thermal, chemical, and spatiotemporal properties and relations (and those of their parts); since these, together with the relevant laws, suffice to make it true that *a* is a freezer, that *b* is ice-cream and that *a* contains *b*.

4   *Chris Daly*

Concepts

I am grateful to Chris Daly for telling me how I do philosophy, and I own up to much of what he says. But not to all. For a start I take our concepts to be less well-defined than he does. Before relativity, for example, we all took simultaneity to be a two-term transitive relation. Nowadays most of us think it is either a three-term relation (the third term being a reference frame) or not transitive. I would put this by saying that we have discarded one of simultaneity's connotations, which Chris thinks means we have changed our concept, since he takes that to be a connotation of 'connotation'. I disagree, because all I mean by calling an inference we feel entitled to draw from applying a predicate '*F*' a 'connotation' of '*F*' is that it is one of several such inferences which matter to us and which we think preserve truth.

I therefore deny that, when we find that one such inference sometimes fails when the others always succeed – as when indeterministic causes fail to 'necessitate' their effects – we must always infer, not that we have discovered something about causation, or simultaneity, but that our concept of it has changed. So I do not think, as Chris implies, that all 'folk' utterances about what is happening now must be false, just because most folk do not know that what is happening 'now' at a spatial distance is relative to a frame of reference.

Of course a concept *may* change, if we find that too much of what we have habitually inferred from applying a term fails to be true; and it may be hard to say how much is too much. But it may still be clear enough in a given case that *not* too much fails; and in showing this it helps to be able to explain the appeal of the connotation we are discarding. With causation I do this, as Chris notes, by exploiting the fact that most of its connotations come by degrees, measured I say by how much a cause C *raises* the chance of an effect E (not just, as Chris assumes, by E's chance with C). This, by providing a measure of what I call C's *efficacy*, explains why deterministic causation, where C raises E's chance from 0 to 1, is the ideal: because it meets causation's other connotations as fully as they can be met. This in turn

shows why, before the rise of indeterministic theories, determinism was itself an important connotation of causation. It also shows how and why causation can fall short of that deterministic ideal and still be causation, and reduces the question 'How far short?' to one of how to map a qualitative concept (causation) onto a quantitative one (efficacy) – a question with as uninterestingly context-dependent answers as 'How hot is hot?'.

Truth and belief

Chris's four objections to the success semantics I endorse in §3 may be met as follows. First, since, as Chris says, causes always precede their effects, beliefs that combine with desires to cause actions will always include beliefs about the future: in his example, Toad's belief that there will still be honey in his pot when he opens it.

Second, success semantics does *not* say that all combinations of beliefs and desires cause actions whose success the beliefs' truth will ensure, precisely because the causation required is indeed contingent, e.g. on the agent's not being paralysed. All it says is that any actions these combinations *do* cause will succeed if all the beliefs involved are true. However I do now think that, as human beliefs and desires generally cause actions indirectly, by causing intentions to act, we should call their effects decisions, i.e. the forming of intentions. This moves much of the contingency that bothers Chris into the link between intention and action. (Though not of course all, since the causation of decisions by beliefs and desires is still contingent on – e.g. – the absence of stronger conflicting desires.)

Third, the fact that some causation is indeterministic does not rule out deterministic theories like (a) Newtonian mechanics and (b) decision theories which say that decisions are caused by combinations of belief and desire. And all the contingency of these theories shows is that (a) accelerations *might* not be caused by forces acting on masses and (b) decisions *might* not be caused by beliefs and desires. But just as accelerations that *are* caused by forces in the way Newton says *will* therefore be proportional to the net forces that cause them, so decisions caused by full beliefs and desires in the way decision theories say will, if carried out, succeed if all those beliefs are true.

Chris himself gives the answer to his fourth objection, that true beliefs can cause actions which fail, as when the pot Toad opens to get the honey he truly believes it contains is booby-trapped. The answer, taken from Jamie Whyte (1997), is that this is not the only belief Toad needs to make him decide to open the pot. He must also believe that if he opens the pot he will get what it contains, and this is the belief whose falsity makes his action fail. Chris says that invoking beliefs like this makes success semantics trivial; but it is not, any more than it is

trivial that objects have masses that satisfy Newton's laws of motion. Nor therefore is it trivial that we have states of mind (beliefs) with contents that both make them combine with other such states (desires) to make us decide to act in specific ways and that can also, by being true, ensure that the objects of those desires are achieved by acting in those ways.

Communication

Chris discusses two claims which he says I make in my (1990), namely that to tell you that P I must (1) make you believe that I *believe* I believe P, and (2) consciously believe P; and he also conjectures that I believe (2) because I believe (1). I am afraid he is wrong on all counts. What I do say is that to tell you that P I must (1*) make you believe I believe P, and – ignoring degrees of belief – (2*) have a conscious belief either in P or in not-P, depending on whether I want to tell you the truth or to lie. Nor is (1*) my reason for believing (2*): (2*) seems to me an observable fact, explained by my thesis (1980) that to believe any P consciously is to believe one believes it.

I do however agree with Chris that we can make statements, just as we can act in other ways, without the beliefs that cause us to do so being conscious, and I give an example of this in my (1990). What I deny is that these are cases of *communication*, i.e. of *telling* someone that P, as opposed to *showing* that P, or unconsciously revealing one's belief that P, revelations which may of course cause others to believe P too. But this need not be why, in Chris's example, a husband's absent-minded reply 'P' to his wife fails to tell her that P in my sense of 'tell'. For what the husband lacks in this case need not be a conscious belief that P, but any intention or expectation of making his wife believe P itself, which is after all the main point of telling someone that P. What makes his reply to his wife absent-minded, I suggest, is that he is not even *trying* to tell her that P.

The property of truth

Finally, Chris says that my Ramsey test for what properties there are makes truth itself a property as well as a concept. Not so, and Chris is right to conjecture that my 'talk about the property *truth* is intended as a *façon de parler*'. Maybe the Ramsey test would make truth a property if success semantics told us what truth *is*. But as I say in §3, I now see that all success semantics tells us about is *belief*, with *truth* being sufficiently defined by the principle that, for all 'P', 'P' is true if and only if P. This being so, the relevant parts of statements of the laws that success semantics requires need only read 'if *x* believes P, and P, then …', in which the predicate 'is true' does not occur. So since it follows that, whatever laws there are,

the Ramsey sentence ∑ of their conjunction *need* not quantify over truth in order to be true, my Ramsey test does not make truth a property.

Mind and Causation

5   *Tim Crane*

When Frank Jackson's (1986) Mary leaves the black-and-white room she was brought up in, she sees a red tomato and thereby learns what red looks like. Tim Crane endorses Frank's view that this is learning a fact, namely that (as Tim puts it) 'red looks like this', which only those who have seen something red can know. In my (1993b) I followed David Lewis (1988) and Laurence Nemirow (1990) in denying this, arguing that all Mary acquires is an ability to imagine and recognise red things; arguments which, despite Tim's rebuttals, I still accept.

I however, unlike many 'ability theorists', am not a physicalist. So, as Tim admits, physicalism is not my motive for denying the undeniably nonphysical fact that he and Frank think Mary comes to know. I believe in many nonphysical facts, not only in the weak sense of 'truths' but also as truthmaking facta, such as those containing the visual sensations Mary's tomato – call it Rudy – gives her. I also believe that seeing Rudy may well teach Mary four relevant truths, namely that both Rudy and its colour both are and look red. What I deny is that, besides these four unproblematic truths, there are any truths about *what red looks like*, i.e. about *what it is like to see red*.

I deny this largely because I still cannot see why, if there are such truths, we cannot state them. For, as I say in my (1993b: p. 8), while we

> have words for properties of experiences, like a 'loud' noise, a 'sweet' taste, a 'warm'
> feeling … we can say nothing about what they are like. What does [a loud noise]
> sound like, sugar taste like, warmth feel like? We cannot say. All we can say is that
> these experiences are more or less like, i.e. similar to, certain other experiences. But
> that does not tell us what, in the relevant non-relational sense of 'like', any one of a
> set of similar experiences is *like*.

Tim disagrees: he says that Mary's 'red looks like this' says what red looks like, at least to her. I deny this: I think that what Mary says means either 'this looks red', where 'this' fixes the reference of 'red' by referring to Rudy (or to its colour), or 'red looks like this looks', which uses the irrelevant relational sense of 'looks like' to say that red looks like Rudy or its colour, whatever that looks like.

Tim does say that no book could 'express the proposition … Mary expresses when she says "red looks like this"', but that seems to me both false and irrelevant. For Mary might as well have said 'red looks like (the colour of) Rudy', a proposition any book could state, illustrated perhaps by a colour picture of Rudy, to whose colour it too could then use 'this' to refer. But that is irrelevant if, as I claim, 'red looks like this' does not state what Mary learns when she learns what red looks like.

This however only reinforces Tim's main point, that to learn what red looks like Mary must see something red: black words on white paper will not do. But that too is irrelevant if what she learns is not a truth but an ability. And anyway, it is not a necessary truth that Mary cannot learn what red looks like without seeing something red: not everyone need be as unimaginative as she was in her black-and-white room. Sculptors, for example, who can plan work in their heads, can tell what a sculpture will look like before anyone sees it; while the score of (say) Berlioz's *Symphonie Fantastique* can make some musicians hear in their heads orchestral textures they have never heard in reality, thereby coming to know what these sound like before they hear them. This is not of course to assert what Tim would deny, namely that these musicians have knowledge of a proposition expressed by the score. They do not; but whereas Tim thinks this is because they know a fact that cannot be so expressed, I think it is because there is no such fact: what the score teaches them is not a truth but how to imagine and recognise Berlioz's orchestral sounds.

Mary's knowledge differs therefore from Tim's other example of knowledge that books cannot express, namely the knowledge his Vladimir expresses by pointing to (say) Thetford Forest on a map and saying 'I am here!'. For what makes Vladimir's knowledge inexpressible by any movable map with an 'I am here!' sign fixed to it is not his (in effect) affixing that sign by pointing, but the indexicality of 'here', which requires his map to be where he is for its 'I am here!' sign to express what he knows. But there is nothing indexical about what Frank (1986) says Mary learns: namely, *what red looks like*, i.e. looks like *to everyone* (with normal eyesight), not just to her. And if the truth that Tim and Frank say Mary knows is not indexical, why can it not be expressed – unless, as I claim, there is no such proposition?

My know-how view does, as Tim says, make Mary's knowledge 'irreducible to propositional knowledge', but not therefore as 'completely different' from it as he thinks. For the success semantics I espouse makes knowing that P, for many contingent P, special cases of knowing-how, namely of knowing how to act to get what we want. Take the example in

§4, of Toad opening a pot to get the honey it contains. His action succeeds because the belief (P_H) that combines with his desire for honey to cause this action, namely that the pot contains honey which he will get if he opens it, is true. So if, as I assume, for any P, knowing P entails believing P, and P, then by knowing P_H, Toad knows how to get the honey he wants. (Even if he cannot in fact get the honey, because he cannot open the pot, he still knows *how* to get it.) Similarly in all other cases. Mary's knowing how to imagine and recognise red is not so different from Toad's knowing how to get his honey by knowing that his pot contains it. All Mary's case shows is that propositional knowledge is not the only form of know-how.

But if Mary's knowledge is not propositional, it is not knowledge of a fact even in the trivial sense given by the principle that a proposition 'P' is true if and only if it is a fact that P. So in particular it is not knowledge of a subjective fact. But because indexical knowledge, like Vladimir's knowledge of where he is, *is* propositional, it is of a fact in this trivial sense. But it does not follow that this fact is what makes Vladimir's belief in it true, and I say it is not: Vladimir's indexical belief is made true by the non-indexical (and so for Tim objective) fact that Vladimir is in Thetford Forest. It is in this truthmaking sense of 'fact', for which I coined the term 'factum', that I say there are no indexical facts.

Here, however, my denial that truth-functional truths have corresponding facta poses a problem, as Tim notes. For Vladimir, by believing he is in Thetford Forest, also believes among other things that he is not in Russia, i.e. that (for him) Russia is not here. But this belief of his is not made true by the negative factum that he is not in Russia, since there are no such facta, any more than there are indexical facta. In what sense then can Russia's not containing Vladimir be a fact when its not being here for him is not? The answer is that our world's facta, by fixing which atomic (and other non-truth-functional) propositions are true, thereby fix which truth functions of those propositions are true. We may therefore extend the concept of facts as facta in an innocuous but non-trivialising way by saying that these truths too state facts. It is in this sense that I say it is a fact that Vladimir is not in Russia. But if, as I claim, there are no indexical *facta*, it is not a fact even in this extended sense that for Vladimir Russia is not here. This is the sense in which I deny Tim's subjective facts.

6 *Frank Jackson*

Frank Jackson argues from physicalism to 'the *a priori* passage principle' that 'for each true statement concerning our world, there is a statement in physical terms that *a priori* entails [it].' The validity of his argument I accept, but not its physicalist premise, for reasons Tim Crane and I gave in our (1990) and I think Frank has not refuted. Specifically, I still think that

physicalism faces a fatal dilemma: either all sciences (including psychology) count as physical and it is trivially true, or it is false that, as Frank puts it in his (1998: ch. 1), 'the kinds of properties and relations needed to account for the exemplars of the non-sentient are enough to account for everything … contingent'.

Why does Frank think they are enough? After all, his own examples, the microphysics of water and of heat, do not account for anything sentient. Still, they do use microscopic facts to account for macroscopic ones and, as he says (1998 p. 7), 'the mind is a macroscopic phenomenon'. That however is, as he might admit, a pretty weak induction, even if his examples work; and the fact is that they do not work. For despite what he and many others, misled by Kripke (1972) and Putnam (1975), have said, water is *not* H_2O, and heat is *not* molecular kinetic energy: in neither case does microphysics account in Frank's sense for the macroscopic phenomena.

In §9 of my (2000b) I gave several reasons for denying that heat is molecular kinetic energy, one of which may be summarised as follows. First, temperatures pass my Ramsey test for being real properties, being quantified over in many laws: the laws of thermodynamics itself; the laws linking them to the masses, pressures and volumes of samples of given gases; to the mean kinetic energies of gas particles; to the rates of chemical reactions; to the frequency distributions of emitted radiation; and so on. Second, suppose we take the laws of thermodynamics, and those linking temperatures to such other properties of macroscopic objects as their pressures and volumes, to specify what Frank and others call the 'heat role'. Then, *pace* Frank, this role has at least two 'occupants': not only the mean kinetic energy E of gas particles, but also the energy flux X of black body radiation. But neither of these can *be* the temperature T to which different laws of nature link them: for as I show in my (2000b), the way in which gas and radiation initially at different temperatures in the same vessel must interact to reach thermal equilibrium requires X, E and T to be distinct properties.

But what if the laws linking X, E and T are necessary, as I shall reluctantly admit in §10 they might be? Certainly, if X and T are correlated necessarily, any energy flux X of black body radiation will entail that its temperature is the corresponding T. But also *vice versa*: the supervenience is symmetrical, as it would be between states of mind and brain correlated by deterministic and necessary laws. There is no sign here of the asymmetrical supervenience that physicalism needs. And there is certainly no sign of it with the law linking E and T, which advocates of $T=E$ must pretend is deterministic even though they know very well it is not. For since the real law only links any T to a chance of the corresponding E which,

although high, is always less than 1, it will, even if it is necessary, positively prevent T supervening on E.

In short, the non-thermal 'kinds of properties and relations needed to account for' gas particles are *not* enough to account for the thermal behaviour of gases, which they do not even entail, never mind *a priori*. Similarly, although for different reasons, with water and H_2O. For first, suppose we again take the laws that link water's macroscopic properties – its solvent powers, density, freezing and boiling points, latent heats, and so on – to define the 'water role'. Then to be water cannot possibly be to be H_2O since, even if we count ice and steam as water, these allegedly identical properties have quite different extensions. In particular, no single H_2O molecule can be water, since it instantiates hardly any of water's laws, having no solvent powers, density, freezing or boiling points, or latent heats. Water's relation to H_2O is at best that of a heaps of sand to its grains; but to say therefore that it *is* H_2O is as absurd as saying that people are not human bodies but human cells.

Moreover, unlike a temperature, water is not for me a property at all, since the Ramsey sentence of all laws need not quantify over it. What 'water' names is not a single property but a natural kind, a congeries of macroscopic properties, such as those listed above. And the microphysics of the H_2O (and other) molecules that water contains are not, as Frank supposes, enough to account for this congeries: if only because, as we have just seen, it cannot account for the temperature of water (nor, for example, for its pressure), on which most of its other properties depend. But if even a mature microphysics cannot account in Frank's sense for the most important macroscopic properties of water, I see no reason to share his faith that the sciences of the non-sentient will one day account in his sense for all mental phenomena. On the contrary, to me it seems obvious that peculiarly psychological 'kinds of properties and relations' will always be needed to do that, just as peculiarly thermal and other macroscopic kinds of properties are needed to account for the phenomena of heat and of water.

7   *Paul Noordhof*

Epiphenomenalists owe us a theory of causation to explain why non-physical mental entities can have causes but not effects, a debt that I agree with Paul Noordhof they cannot discharge. All serious theories of causation link causes to effects (or their chances) in one or both of Hume's (1748: sect. VII) two ways: by counterfactuals, or as instances of generalisations. And nothing about either way stops mental entities figuring as easily in their antecedents as in their consequents. If you would (probably) not have thought it was cold out had you not seen

the snow, why might you not have (probably) gone out had you not thought it was cold? If everyone in brain state B (and …) feels embarrassed, why may not everyone who feels embarrassed (and …) blush?

Paul discusses the stock answer to such questions, the 'causal closure' principle that all effects have *only* physical causes, and accepts my and Tim Crane's (1990) objection that their all *having* physical causes does not entail this. However, Paul thinks our argument requires non-physical causes to overdetermine their effects, and notes that an unwillingness to admit 'systematic overdetermination … is the major reason why most philosophers of mind have become physicalists'. But, as I note in §8, an effect's physical and non-physical causes will *not* overdetermine it when they are linked, as they usually are, by laws that make both present or absent together. What Paul calls the '*a priori* implausibility of systematic over-determination' is as bad an argument for epiphenomenalism as it is for physicalism.

There being no other argument for epiphenomenalism that I know of, the inefficacy of the mental can only be an axiom. But as Paul says in his §1, it is an axiom that rules out both obvious examples of mental causation and good causal theories of how we know and refer to our own states of mind. I find these objections to it stronger than Paul does, since I deny that they need more defence just because epiphenomenalists can explain them away. Compare Kripke's (1971) proof that laws of nature which we cannot know *a priori* could still be metaphysically necessary: this is no reason to think that such laws *are* necessary, given other arguments for their contingency; and similarly here. It is epiphenomenalists, not their opponents, who should be on the defensive: since it is they who need independent arguments for the inefficacy of the mental, to set against all the apparent examples of mental causation, and the independent arguments for causal theories of knowledge and reference.

This is of course no objection to Paul's new argument against epiphenomenalism: it does no harm to make a strong case stronger. I do however jib at the changes he thinks he needs to make to my theory of causation, for the following reasons.

First, I do indeed think it is metaphysically necessary that, as Paul puts it, 't precedes t' if there is some fact C at t which causes some fact E at t'''. The necessity of this is clearly consistent with there being possible worlds where spacetime is not dense, or where special relativity is false, or where – as in our world – all parts of a solid object can move together at the same uniform velocity; and I cannot see why Paul says in his §3 that it is not.

Second, my argument against simultaneous causation does not stop two facts coinciding when – as in Paul's example of Jim's being both the fittest and the shortest man – neither

causes the other. Moreover I show in my (1995: ch. 17.2) how to accommodate facts which coincide and *do* seem to interact, as when a gas sample's pressure at *t* is apparently caused by its volume at *t* and *vice versa*.

Third, Paul misreads my argument against the possibility of simultaneous causation at a distance. If backward causation is impossible, then simultaneous causation between non-coincident facts must also be impossible if there are *any* possible worlds where it would yield backward causation. But there are, since it does so in all worlds, like ours, where special relativity is true. So what my argument needs is not, as Paul thinks, that special relativity be necessary, merely that it be possible.

Fourth, Paul disputes my *reductio* proof that no two facts C and E can cause each other, and hence that causal loops, and thus backward causation, are impossible. This proof assumes that, *if* C and E can interact, any individually possible values of E's chances with and without C, and of C's chances with and without E, can coexist. Yet elsewhere, as Paul notes, I rule out combinations of laws that would impose incompatible time orders on spacetime points which instantiate them. Why then, he asks, instead of ruling out causal loops, should we not rule out the combinations of E's and C's chances that generate contradictions?

To this good question I have a four-part answer. First, in the time-order case we have no choice: there is no other way of ruling out incompatible time orders. But the contradictions that backward causation seems to make possible may be ruled out in two ways. Either backward causation is impossible, or all and only the members of an infinite, unspecifiably complex and totally *ad hoc* set of combinations of individually possible chances are impossible. The former is a vastly simpler theory, which is my second reason for believing it.

My third reason is that, as the chances that C and ~C give E are located in different possible worlds, I do not see how they can constrain each other; and similarly for the chances that E and ~E give C. And my fourth is that, on my theory of chance, the facta that are the chances which C gives E and E gives C not only follow from different laws, they have different locations: this chance of E being where C is, and this chance of C being where E is. This means that Paul's theory must postulate necessary links both between otherwise independent laws, and between regions of spacetime that may be widely separated. Such links contradict attractive Humean assumptions about the independence of laws and of spacetime regions; and while my theory also violates the latter, since I say that any $ch(E)=1$ entails E, mine is a single and independently argued exception (2000a), not a farrago of *ad hoc* expedients.

That is the case for my theory that backward causation is impossible. It is not a logical knockout – the rival theory contains no contradiction – but then it does not need to be. In philosophy, as in boxing and science, one can still win decisively on points. It is a mere dogma of analysis, which I reject, that metaphysical theories, like theories in logic and mathematics, can only be established by showing that all their rivals entail contradictions.

Finally, to Paul's own case against epiphenomenalism I have only one objection: I do not see why we cannot credit mental facts that lack effects with temporal locations. For since these facts do have causes, the principle of no unmediated action at a distance, which we can all accept, will locate them at the earliest time that is later than all their causes. They must still, as Paul says, coincide with facts that have effects, in order to make them earlier than those effects. But why should epiphenomenalists not buy this consequence of our view of spacetime? For even on Paul's theory that view need not, as he thinks, make any law 'take the form it [does] because of the presence of mental facts': since on that view it matters neither which facts with effects coincide with which facts without them, nor what those effects are, just so long as there are some.

8 *Peter Menzies*

My views on causation owe as much to Peter Menzies as his do to me. And we agree on more than he supposes, if not on whether causation is a relation. But before showing that, I must tackle his objections to my claim that causes must raise the chances of their effects.

Peter showed in his (1989) how denying unmediated action at a distance deals with all cases of what, following David Lewis (1986b: §E), he calls 'early' pre-emption, where the 'process that …brings about the effect cuts short all alternative processes before the effect'. But this, as he says, does not dispose of 'late' pre-emption cases, like his Case l, where 'the effect itself [the falling of the victim, Tony (say)] cuts short all alternative processes'. But the problem here is not, as he implies, that no *immediate* cause can raise the effect's chance: for effects need never have immediate causes if spacetime is dense and (as I argue in my 1998: ch. 10) causes must precede their effects, since causation must then be as dense as time is.

Peter is however right to say that coping with his Case 1 means making the effect – Tony's falling – a different entity if caused by assassin A than if caused by assassin B. Now I have an initially tempting way of doing this, based on an identity criterion for facts with causes and effect (about which I have been less reticent than Peter claims): namely that, for any such facts D and D′, D=D′ iff D and D′ have all the same actual causes and effects (1995: ch. 9.3). This, however, being only a criterion of actual, not of counterfactual identity, does

not show that a given fact *could* not have had different causes or effects, nor therefore that B's firing would not have caused (a counterpart of) the very effect that is actually caused by A's firing.

How then in Case 1, if causes must raise their effects' chances, can Tony fall because A fires? Well, as no one thinks A's firing causes Tony to fall on any other occasion, the effect here must really be (as Peter notes that I say in another case) that Tony falls roughly when he does, say at time *t*. And A's firing *must* raise the chance of that, simply because, to make the pre-emption late, B must only fire if and therefore *after* he sees Tony fail to fall at *t*. That is how, on a chance-raising theory, 'Tony falls (when he does) because A fires' can be true. And if it is, then it follows on my theory that *A's shot causes* (or at least *affects*) *Tony's fall*, by causing it to occur earlier than it otherwise would (Mellor 1995: chs 11.3–12.2) – where *A's shot* and *Tony's fall* are particular events with the same identity criterion as my facts, namely that for 'any events *d* and *d′*, *d=d′* iff *d* and *d′* have all the same causes and effects' (Mellor 1995: ch. 9.3; Davidson 1969). And similarly for all other cases of late pre-emption, with which, as Peter conjectures, I therefore believe I can deal, as I do in another case in my (2001) reply to Laurie Paul (2001).

I can also cope with Peter's Case 2, where A and B fire together, provided that each only fires if the other does: since that makes each assassin raise Tony's chance of falling from its value if neither fires to its value if both do. This is how, I argued in my (1995: ch. 8.7), mental and physical facts linked by psychophysical laws can determine the same effects without overdetermining them (thus refuting another bad argument for physicalism: see §7 above). But this will not work if it is only a coincidence that A and B fire together, and such coincidences, although rare, do indeed pose a problem for most counterfactual theories of causation. But not for those who, like me, take causes and effects to be facts in the extended sense of §5, because those facts, unlike particular events, can be disjunctive. For even if neither A's nor B's firing, given the other, raises the chance that Tony falls, their disjunction does: his chance of falling would have been less had neither fired.

If disjunctive causes sound odd to those who think of causes as particulars, so much the worse for that view; and anyway hard cases make bad law. It is no mere intuition that makes me require causes to raise the chances of their effects, but the fact that, as I show in my (1995: ch. 8), key connotations of causation – that causes explain, are evidence for and means to their effects – require them to. Still, given that requirement on the basic concept, we can soften some hard cases by extending it (as I extended 'facta' to 'fact' in §5). Hence for

example David Lewis's extension in his (1986b: §B) of what I call 'causation' and he calls 'causal dependence' to its ancestral, in order to make it transitive. If, as I think, his extension loses too many connotations, at least the extension is clear and easily reversed. Similarly with Peter's extension of causation to processes, like those linking A's and B's firing to Tony's falling, each of which would, without the other, have raised the chance of that effect. Calling disjuncts of disjunctive causes 'causes' too is no big deal.

However, if hard cases make bad law, unclear ones, like Peter's cricket ball examples, are worse. Still, they do support my chance-raising requirement, as is obvious from the way our response to them depends on whether we take their 'backup' walls or hands to be included in the causal setups. This certainly supports Peter's view that causation is embodied in intrinsic properties of law-based systems, and that what we think causes what depends on what we hold fixed in assessing the relevant counterfactuals. Both of these claims of Peter's I accept.

All I deny is that Peter's process view of causation requires it to be a *relation*. Of course the evolution and consequent effects of law-based systems depend on their intrinsic properties and relations, but those relations need not include causation itself. Thus of course, when hooking a cricket ball causes it to go for six in one piece, that system's – the ball's – holding together when hit depends on the law-governed properties and relations of it and its parts. Nevertheless, this causation only requires such facta to make true certain conditionals, about the ball's chances of having various trajectories if it is or is not hit in various ways. That, for the reasons given in my (1995: ch. 13), does not make causation a relation, and Peter fails to show that it does. The instances of possibly negative properties F and G in his (9), for example, need only be facts in the ontologically vacuous sense of §5 above. They need not be the real facta – like a cricket ball's mass – whose existence is what makes G-instances depend causally on F-instances.

Peter therefore could and should accept the arguments he cites against a relation of causation. In particular, he need not reject one of mine because it rests on 'the dubious principle that if some fact P is entailed by, but does not entail, some other fact Q, then P cannot be a genuine factum'. That I now agree must be wrong, since any atomic factum P is entailed by but does not entail its conjunction with any independent fact, and is none the worse for that. But in my example Sue's pulling her drive, P, is *not* a conjunction of her driving, D, with an independent fact, but a disjunct of the disjunction D of various ways of driving a golf ball. So the reason why only P could be a factum is not the 'dubious principle' above, given I confess in my (1995: p. 165), but the fact that there are no disjunctive facta. If

Peter will accept that – together with my thanks for correcting me so politely! – I hope he will also then accept that causation is not, after all, a relation.

Dispositions and Laws

9   *Isaac Levi*

I am less immune than Isaac Levi thinks to his views on dispositions and conditionals. While we do disagree on many points, on much that matters to us we agree in substance, if not in our interests nor hence in our idioms. Take the conditional which I said above that Chris Daly's Toad needed to believe: 'If I open the pot I'll get what's in it'. I say, and Isaac denies, that this conditional has a truth value. But I do agree that it differs from any unconditional proposition, since I say that Toad's belief in it is his disposition to believe its consequent if he believes its antecedent (Mellor 1993a). And what matters to both of us is that this disposition be truth-preserving, not whether its content has a truth value.

Still, I do think Toad's conditional has a truth value, partly because it can occur within other conditionals, like 'If I'll get what's in the pot if I open it, I'll buy it' and 'If it's safe, I'll get what's in it if I open it'. I also think, despite Isaac's objections, that Toad could easily 'suspend judgement concerning the truth or falsity' of such conditionals (and of modal variants like 'If I open the pot I *may* get what's in it'), 'judge them probable to varying degrees … desire that they be true in varying degrees and the like'.

This is why I can say that conditionals are *entailed* by beliefs about dispositions, like the one I believe Toad has (to believe that he'll get what's in his pot if he believes he'll open it), whereas Isaac can only say that this belief of mine *supports* that conditional (that if Toad believes he'll open his pot he'll believe he will get what's in it).

But these are just different idioms: what matters is *which* conditionals are supported or entailed by such beliefs, and here too we can agree in substance. Take the two hard cases Isaac offers me in his §5: (1) an object a can have a sure-fire disposition (D) to R if S'd and yet fail to R if S'd, because being S'd causes a not to have D; and (2) a coin c can land heads if tossed (T), but not if tossed by Sydney Morgenbesser, by whom it is in fact tossed. How can I cope with these?

(1) I say that a's disposition D is 'sure-fire' if and only if a's chance $ch(Ra)$ of being R is 1 when a is S'd *if it remains D*. So what 'Da' entails is not 'If a were S, $ch(Ra)$ would be 1' but 'If a were S and D, $ch(Ra)$ would be 1'. So if a is both S and D, Ra may have more than one actual chance: the $ch(Ra)=1$ that is a fact about $Sa\&Sd$, and a smaller chance that is a fact

about *Sa*. But that is no problem for me, since the theory of chances in my (1995: ch. 2.1) lets a proposition have many actual chances of being true, each one a fact about a different earlier fact.

This means that, in Isaac's case (2), it can be a fact about how a coin *c* is tossed (by someone other than Sydney) that, as it is tossed, its chance *ch*(H) of landing heads is 0.5; and it can also be a fact about *c*'s facing heads up as it lands that *ch*(H) is then 0.99. Naturally not *every* prior fact about *c* gives *ch*(H) a value, any more than *Sa* has to give *ch*(*Ra*) one. In particular, while the way Sydney tosses the coin *c* does give *ch*(H) a value, the mere fact that *c* is tossed (*Tc*) does not. But then Isaac's

(a)   'On the supposition that *c* is tossed, it might land heads.'
is ambiguous in the way I discuss below in §11. For if his 'might' means merely that *Tc* does *not* make *ch*(H)=0 – because it gives *ch*(H) *no* value, high or low – then (a) is true; whereas if (a) means that, as tossed, *ch*(H) has a value, greater than 0, then (a) may or may not be true, depending on how *c* is tossed.

This is not of course what Isaac says about these cases, but it is consistent with what he says. It is also immune to his objections to David Lewis's (1973) 'closest worlds' theory of the relevant conditionals, which in my (1995) I too reject as an account both of their semantics (ch. 1.7) and of what makes them true (ch. 14.1).

As with chances, so with dispositions. Isaac and I agree that propositions like '*Da*', which ascribe dispositions to things, have truth values. But when I add that not all dispositional predicates correspond to properties, Isaac claims to have 'neither understanding of nor interest in an "ontological" distinction between predicates characterising properties and predicates that do not'. Well, that is his prerogative, as it is mine not to understand or be interested in American football. But that does not mean there is no such game, or no answer to the question of what properties there are. And in fact a distinction which Isaac does draw, between 'problem-raising' and 'problem-solving' predicates, fits my answer to that question quite well. For what makes predicates 'problem-solving' for him is their 'integration into adequate theories'; while my Ramsey test makes properties correspond to simple predicates in statements of laws of nature. So when our theories really *are* adequate, i.e. are true, his problem-solving predicates will be those that I say correspond to properties.

10 *Alexander Bird*

As Alexander Bird knows, I think 'dispositional' applies primarily to predicates, namely those, like 'is fragile', whose extension is given by a conditional, something like 'would

break if dropped'. And as I have observed in § 3 and §9, most predicates do not correspond to properties in my sense. In particular, the extension of 'is fragile', like that of 'is red', will certainly differ from that of any property ranged over by the Ramsey sentence ∑ of all laws (see my 1997).

On the other hand, every property *F* does correspond to an actual or possible predicate 'is *F*'. So we can transfer the epithet 'dispositional' from predicates to properties by applying it to *F* if and only if all *F*-things satisfy one or more conditionals, i.e. (as Alexander puts it) have 'certain conditional powers'. In this sense I, like Popper (1990), think all properties are dispositional, since my Ramsey test makes them all occur in laws, which say that all *F*s are *G*s (or *vice versa*), so that anything would be *G* if it were *F* (or *G* if it were *F*) or – if the law is indeterministic – would have a certain chance of being *G* if it were *F* (or *vice versa*).

Does this make my properties 'categorical' in Alexander's sense, i.e. such that they 'confer, of themselves alone, no …causal powers …but [do so] only because there is a law relating [them] to some other property'? I cannot tell, because for me this is a false contrast, since I say that for any property *F* to exist *is* for laws to relate it to other such properties. However, I do take properties to be categorical in two more usual senses. For first, I have just agreed with Isaac Levi that, for any dispositional predicate '*F*' (whether *F* is a property or not), '*a* is *F* (at *t*)' is a categorical statement, i.e. has a truth value, even if the conditionals that give '*F*' its extension do not. All ascriptions of dispositions are categorical in this semantic sense, just as all actual properties are categorical in the ontological sense – i.e. real – whether they are dispositions or not.

In short, I think the war between Alexander's 'categorical' and 'dispositional' 'monists' is a phoney war, since all properties, including triangularity, are both. I largely endorse Alexander's defence of the view that triangularity is as dispositional as it is real; but I do have three comments to make about what he says. First, even if it is trivially analytic that a figure's triangularity is what makes counting its corners correctly give the answer '3', its having this property can still be what makes my counting its corners *cause* me to get that answer. Second, since machines can count corners as well as people can, triangularity is indeed 'independent of any power to produce effects in human observers'. And third, since I think that occurring in laws is what makes triangularity a property, I agree in substance with Alexander's claim that its 'conditional characterisation [needs] appropriate generality' to show it 'to be genuinely dispositional', i.e. to be a real property.

However, the interesting question about a dispositional property *F* remains, as Alexander says, whether it is *essentially* dispositional, i.e. whether nothing could be *F* while lacking the 'conditional powers' that the laws *F* occurs in give it. This however is ambiguous, since properties occur in many laws, like all those containing temperatures listed in §6, and each law that *F* occurs in will give *F*-things a distinct conditional power. So something might have been *F* while lacking *some* of these powers, if not while lacking most or all of them. Thus just as Alexander might have been a Labour Member of Parliament but not perhaps a microbe, so our relativistic masses (which acceleration increases) might perhaps have been Newtonian (not increased by acceleration) but not temperatures.

Alexander thinks however that some individual laws, and hence powers, *are* essential to some properties, and he may be right. Indeed a truthmaking consideration tempts me to the even stronger claim in Stephen Mumford's (1998: ch. 10), that *all* properties necessitate *all* the laws they occur in. Take the example, in §1 above, of truths about what is visible in a mirror. To necessitate these we need not only the mirror, the objects it reflects and the light by which it does so, but also the laws of reflection. Yet, as I say in §7 of my (2000b),

> the ontology of laws is notoriously problematic, with candidates ranging from Humean regularities to relations between properties … It is tempting therefore to bypass the problem … by taking the existence of factual properties to entail the laws they occur in. For then we can dispense with laws as truthmakers, even for law statements, which can all be made true by the existence of the properties and relations they refer to.

However, while I feel this temptation, I have not yet succumbed to it. I cannot yet believe, for example, that masses *could* not be as unaffected by acceleration as Newton thought; and I do not despair of saying what in the world contingent laws of nature are. But if in the end no credible account of what laws are lets them be contingent, I may then have to follow Oscar Wilde's advice, that 'the only way to get rid of a temptation is to yield to it'.

11 *Arnold Koslow*

The range of cases covered by Arnold Koslow's logic of natural possibilities is a revelation. Its removal of the concept's common restriction to truths and worlds is especially welcome to my reply to Tim Crane in §5, by making knowing-how even more like knowing-that. For although Arnie does not give the example, his theory shows how abilities are as much natural possibilities for know-how as intelligible truths are for propositional knowledge.

As a logic of possibility and necessity, Arnie's theory has one obvious defect, of which he is well aware, namely that on it 'necessarily x' does not always imply 'possibly x'. Its always doing so when x is a single natural possibility (i.e. a singleton of the power set N* of the set N of such possibilities) seems to me not enough, since this does not cover every possibility we would naturally call 'natural', like that of getting an odd number (1, 3 or 5) on a throw of a die. If however this is (as Arnie conjectured in an e-mail) 'an artefact of the construction [he] gave for these possibilities', it should be remediable, and I hope it is.

But whether it is or not, one question that Arnie's list of kinds of possibilities prompts is what distinguishes them from each other. What, in particular, distinguishes the contingent and quantitative physical possibilities that I call 'chances' (2000a), like a chance ch(H) of a coin toss landing heads? I think the answer is that, being contingent, simple statements of chance like 'ch(H)=0.4' need truthmakers, which most of Arnie's other possibilities, being necessarily possible, do not. I said in §1 that because 'P is contingent' and hence '~P is possible' are necessary if true, they need no truthmakers. Similarly for the sense in which truth and falsity are the possible truth values of any 'P' and '~P'. Similarly again for the necessary possibility of possible worlds, and of possible cases invoked in mathematical proofs.

Still, not all of Arnie's other possibilities are necessarily possible. Take the possible states and transitions ascribed by theories to systems, such as the possible orbits ascribed to planets by Newton's theory of gravity. If the theory is contingent, so are these possible orbits. However, given whatever makes the theory true, nothing more is needed to make just these orbits *possible*. It is statements of planets' *actual* orbits that need something more to make them true. And so do statements of their *chances* of being actual, whether these be 1, on a deterministic theory, or something less, on an indeterministic theory: since for no contingent 'P' or value of p are propositions of the form 'ch(P)=p' complete truth functions of 'P'.

This is why propositions like 'ch(H)=0.4' need to be made true by chances. Or rather, since ch(P)=1-ch(~P) for all P, by chance *distributions*, in this case the distribution $\langle 0.4, 0.6 \rangle$ over $\langle$H,~H$\rangle$. But not all propositions about chances need truthmakers, because, for reasons already given, no truth function of 'ch(P)=p' needs one. In particular, therefore, '~(ch(H)=0)' needs no truthmaker. But then, as I said in §I of my (2000a), this can be true, i.e. (as I noted in §9) H can be made possible, 'not by there being a ch(H)>0 but by there being no ch(H) at all, zero or otherwise': a coin toss can land heads simply because nothing prevents it, whether or not it has any positive chance of doing so.

Arnie is therefore wrong to say that for me 'any B is a possibility if and only if it has a non-zero chance'. A non-zero chance is not necessary for this kind of possibility. Nor is it sufficient, since B may have more than one actual chance, as I also noted in §9, and one of its chances may be zero, which I say entails ~B. But provided we distinguish B's being *left* possible (i.e. not being ruled out) by a fact A from its being absolutely possible (i.e. ruled out by *no* fact), then as I say in §IV of my (2000a),

> there is no contradiction here either, even on the view that non-zero chances are real possibilities. For a toss's landing heads can easily be left possible to some extent by one fact about the toss, to a different extent by another, and made either necessary or impossible by a third.

None of this affects Arnie's case for his two main claims, that laws and explanations rule out possibilities. However, his argument for the latter claim does make one assumption I reject. This is that I require any B explained by any A to have chances with and without A, $ch_A(B)$ and $ch_{\sim A}(B)$, such that $ch_A(B) > ch_{\sim A}(B)$. Not so: some B (e.g. laws) have no chances, high or low, and some A (e.g. least action explanations of trajectories) do not work by raising chances. It is only *causes* that I require to raise the chances of their effects, and many explanations are not causal. Yet these too rule out possibilities, and for Arnie's reason. For all his argument really needs is what he calls explanation's 'facticity', which makes the mere existence of any explanation A of B entail B, even if A itself does not. And this, as Arnie says, on my account rules out $ch(B)=0$, thereby ruling out a possibility, namely a possible value of $ch(B)$.

Change and Time

12 *Gonzalo Rodriguez-Pereyra*

Asking how things can be, with no reason to think that they cannot be, is a bad habit to which many otherwise sensible philosophers are oddly prone. Knowledge is a case in point; and so is change. We all know what change is: things having at different times different properties or relations (different shapes, temperatures, distances, etc.) that they could not have together at a single time. Why is this a problem? Why should the inability of things to be simultaneously hot and cold at once stop them heating up or cooling down? I see no reason why it should, nor therefore any reason to ask how change is possible. That question would only make sense if we had no consistent theory of change, but we have several: what we face is not a famine but a glut; not a paradox but a problem of choice.

The theory of change Gonzalo Rodriguez-Pereyra discusses is the relational theory, which makes changeable properties relations to times. He and I both reject many objections to this theory, notably the unargued denial of David Lewis (1986a: ch. 4.2) and others that properties like temperatures *are* relations. For this looks obvious only in present-tense statements like '*a* is hot', meaning '*a* is hot now', and on the B-theory that David, Gonzalo and I all accept, what makes this true at any B-time *t* is that *a* is hot at *t*. But then, given the relational form of '*a* is hot at *t*' and '*a* is cold at *t′*', a denial that temperatures are relations needs arguing, and I find the arguments Gonzalo cites as weak as he does. For example: the relational theory alters our ontology less than a theory of temporal parts, which makes what is hot at *t* (*a*-at-*t*) neither *a* itself nor what is cold at *t′* (*a*-at-*t′*). Nor does the theory make duplicates as hard to define as Mark Johnston (1987) thinks: at any time *t′* a duplicate of *a* at *t* is anything with all the same relations to *t′* that *a* has to *t*. And I agree with Gonzalo's answer to Katherine Hawley's (1998) objection, namely that *a*'s temperature relation to *t* need not be entailed by any other properties of *a*, *t*, or their fusion.

While Gonzalo and I reject these and other objections to a relational theory of change, we also reject the theory itself, only for different reasons. Mine is that, as things can be related at a distance, the theory fails to explain why things must be *at* any spacetime points where they have changeable properties. To this Gonzalo replies first that 'instantiation versions' of the theory do entail this. But the first version he gives does not. For suppose *a* and *b* are events, with *a* earlier than *b* (*a*<*b*). This, on an 'adverbial' instantiation theory, requires a three-place instantiation relation *I* to link *a*, < and *b*. But while that may make < share *a*'s and *b*'s locations, it cannot make *a* and *b* coincide, or *a* would not be earlier than *b*. But then, if what makes *a* hot (*H*) at *t* is that *I* links *a*, *H* and *t*, this too cannot entail, as it should, that *a* is at *t*.

Gonzalo's other instantiation theories may do better, by building *t* into *H* or into *I*: *I*'s linking *a* to *H*-at-*t*, or *I*-at-*t*'s linking *a* to *H*, may well make *a* be at *t*. But they face other objections: for example, that building *t* into *H*, by making *H*-at-*t* differ from *H*-at-*t′*, masks the difference between *a*'s changing and its staying the same, and makes no sense of (e.g.) *a*'s being hotter at *t* than *b* is at *t′*; while building *t* into *I* makes no sense even of *a*<*b* – as we can see by asking at what time *a* is earlier than *b*.

In any case, whatever the relative merits of these theories, I have other reasons, indicated in §2, for rejecting all instantiation relations and hence any theory that invokes them. And Gonzalo's other reply to my objection to relational theories of change seems to me to miss the point. Of course, as he says and I admit, some relations require their relata to coincide in

time and/or space. But most relations derived from changeable properties do not: a can be hotter than, or share the shape or colour of, objects anywhere in space and time. These properties imply nothing about coincidence. Why then, if they are relations to times, must their possessors be at those times, as we know they must? Building that necessity into these relations by definition is not an explanation, merely a restatement of the fact to be explained. Hence my preference for a theory (outlined in §2) which, by keeping properties like H monadic, automatically gives a the spacetime location of any atomic fact whose constituents are a and H.

Gonzalo's own objection to the relational theory is that, by replacing incompatible properties like H (hot) and C (cold) with compatible relations to times, it denies rather than explains change. I cannot see this. H and C are if anything *less* compatible as relations than as properties, since no ordered pair $\langle a,t \rangle$ can be both H and C, whereas any a can be both H and C, albeit at different times. I think the relational theory explicates perfectly the concept of incompatibility between H and C that makes a's being first H and then C both possible and a case of change, by showing how anything can be H and C at different times but never at the same time. But since Gonzalo disagrees, and thinks for this reason, if not for mine, that the relational theory fails to solve 'the problem of change', I am happy to offer him my theory instead.

13 *Nathan Oaklander*

Nathan Oaklander shows in detail why the presentism of William Lane Craig's (2000a, 2000b, 2001) is as subject to McTaggart's (1908) contradiction as any other A-theory of time. This matters because it is from the flow of time, which makes events change their A-series locations from future to present to past, that McTaggart derives his contradiction, by arguing that it requires all events to have all these mutually incompatible locations. Many of those who accept his argument believe therefore that presentists escape it, by holding that only what is present exists, and hence that nothing in reality ever has any other temporal location. This is why presentism is widely held to be the safest as well as the most radical A-theory of time.

I have nothing to add to Nathan's demolition of this delusion, and also of Craig's canard that I think B-relations, such as being earlier than, can be derived from the A-series. Instead I shall amplify their shared criticism of Arthur Prior's failure to add an ontology to his semantics of time, by showing how it makes his presentism both vacuous and question-begging.

In Prior's (1957: ch. II) system, temporally unqualified sentences like '*Ga*' are taken to be present tense, i.e. to say that *a* is *G now*. To these the iterable operators '**P**' ('it has been the case that') and '**F**' ('it will be the case that') may be prefixed to make statements about the past or the future. Thus '**P***Ga*' says that *a was G*, '**F***Ga*' that it *will be G*, '**FP***Ga*' that it *will have been G*, and so on. But all such sentences, however complex, are also about the present, since they all say that it is *now* the case that *a* was *G*, will be *G*, will have been *G*, etc. These sentences could therefore always be made explicitly present-tense by prefixing an operator '**N**' ('it is now the case that') without changing their meanings or truth values. This shows, as Prior says, that '**N**' is redundant, and that *all* tensed truths are truths about the present; from which it follows, on Prior's (1971) view of facts as true propositions, that all temporal facts are present facts: hence Prior's (1970) presentism.

I indicated in §1 how a semantics for time can fail to settle its ontology, as Prior's does, by not saying what makes it true, and hence for Prior a fact, that *a* was *G*, will be *G*, will have been *G*, etc. Prior's failure stems from his purely semantic conception of a fact, which stops his system entailing the ontological doctrine, that whatever *makes* it true that *a* was *G*, will be *G*, will have been *G*, etc., is in the present. That is what makes his presentism vacuous.

It is also question-begging. For if, as it needs to assume, all truths are present-tense, all B-truths must reduce to A-truths. In particular, the meanings of B-predicates like 'earlier' must follow from those of Prior's primitive operators '**P**' and '**F**' rather than *vice versa*, which Nathan, I and other B-theorists deny for many reasons, of which the following is one.

All facts or events, like Queen Anne's birth and death, must become more past at the same rate: otherwise, as time passes, Queen Anne's age at death could vary, which is absurd. Hence, for all A-propositions α and β, and for all real numbers M and N, the following past tense version of Prior's (1957: ch. II) axiom 5,

if $\mathbf{P}_M\mathbf{P}_N(\alpha\&\beta)$, then $\mathbf{P}_{M+N}(\alpha\&\beta)$,

where '$\mathbf{P}_M$' means 'it was the case M units ago that', must be necessarily true. But what makes it so: why can different facts and events not become more past at different rates? The answer is obvious: past time intervals between any two facts or events are mere logical consequences of the interval between their becoming present, i.e. – for a presentist – between their coming to exist or occur. But then, to stop *that* interval varying over time, any statement of it must be a temporally invariant B-statement, like 'Queen Anne's birth occurs 49 years *earlier than* her death'. And as in this example, so in others. The axioms of Prior's presentist system, which express the semantics of his operators '**P**' and '**F**', cannot derive their

necessity just from A-concepts: they must also invoke the irreducibly B-concept of some facts and events becoming present more or less *earlier* than others.

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