THE CONSERVED QUANTITY THEORY DEFENDED

Phil DOWE*

* School of Philosophy, University of Tasmania, Box 252-41, Hobart 7001, Australia.
E-mail: Phil.Dowe@utas.edu.au

BIBLID [0495-4548 (2000) 15: 37; p. 11-31]

ABSTRACT: I defend the conserved quantity theory of causation against two objections: firstly, that to tie the notion of "cause" to conservation laws is impossible, circular or metaphysically counterintuitive; and secondly, that the conserved quantity theory entails an undesired notion of identity through time. My defence makes use of an important meta-philosophical distinction between empirical analysis and conceptual analysis. My claim is that the conserved quantity theory of causation must be understood primarily as an empirical, not a conceptual, analysis of causation.

Keywords: Causality, Conserved Quantities, Causal Processes.

CONTENTS

1. The Conserved Quantity Theory

The conserved quantity (CQ) theory of causation (Dowe 1992c; Dowe 1995a) can be expressed in two propositions:

CQ1. A *causal process* is a world line of an object that possesses a conserved quantity.
CQ2. A *causal interaction* is an intersection of world lines that involves exchange of a conserved quantity.

What is this an account of, and what kind of account is it? As just defined the CQ theory aims to say what are causal processes and interactions. It aims to distinguish causal from pseudo processes, and it does this by distinguishing objects which possess conserved quantities from those which don't. It does not address related issues such as what the connection is be-

tween the facts or events we normally think of as causes and effects (Dowe 1999), and what distinguishes a cause from its effect (Dowe 1992b; Dowe 1996a; Dowe 1997). For example, as an answer to the former issue the CQ theory provides only a necessary condition (Dowe 1999).

Secondly, what kind of account is this? I call it an 'empirical analysis' of causation. The idea is that an empirical analysis can be articulated without looking closely at the everyday concept 'causation', and that the starting point should instead be hints taken from science -for example the idea of a causal process and the distinction between causal and non-causal processes found in special relativity (Dowe 1992c), the emergence of probabilistic causality in biomedical science (Dowe 1993; Dowe 1996b), and the backwards in time causation postulated in the transactional account of quantum interactions (Dowe 1997).

In this paper I want to explain and defend the idea of an empirical analysis, and then use the idea to answer two objections to the CQ theory, the first concerning the idea of conservation laws and the second concerning the presumption of genidentity of an object.

2. Empirical Analysis

We begin with the metaphilosophical question, *what is the task of philosophy in setting out a theory of causation?* As is the case with many philosophical questions, our question, 'What is causation?' is ambiguous, and consequently the philosophy of causation legitimately involves at least two distinct tasks. In approaching the topic of causation we need to be clear about which task we are undertaking.

We begin by considering two approaches to the task of philosophy[1]. The first is *conceptual*; to elucidate our normal concept of causation. The second is *empirical*; to discover what causation is in the objective world[2]. Clearly, it is important to be clear which task one is undertaking. In fact, we shall see that insufficient attention has been paid to this metaphilosophical question, as we examine recent cases where criticism against this or that theory of causation is misdirected simply because no attention has been paid to the relevant theorist's purpose in articulating the theory.

'Conceptual analysis' can mean a variety of things. I use it in the sense defined by Mackie in his question, "What is our present established concept of causation, of what cause and effects are, and of the nature of the relation between them?" (1985, p. 178). In this sense, conceptual analysis is a meaning analysis which begins with our everyday, commonsense understanding

of the relevant concept. That is, the way in which we commonly speak and think provides the primary data for the analysis. We look for the various intuitions that we, as mature users of the concept, can bring to bear. This is marked in philosophical discussion by judgments such as "this is counterintuitive", or "intuitively we say that..." Conceptual analysis is not just dictionary writing. It is concerned to spell out the logical consequences and to propose a plausible and illuminating explication of the concept. Here, logical coherence and philosophical plausibility will also count. The analysis is *a priori*, and if true, will be necessarily true.

Many philosophers in the twentieth century have taken the task of philosophy to be just conceptual analysis. Ducasse conceived of the philosophy of causation in this way:

> The problem of giving a 'correct' definition of the causal relation is that of making analytically explicit the meaning which the term 'cause' has in actual concrete phrases that our language intuition acknowledges as proper and typical cases of its use. (1926, p. 57)

Ducasse draws an analogy with a scientific hypothesis: as a scientific hypothesis aims to fit the facts ("perceptual objects and their relations"), conceptual analysis aims to fit the facts ("the intuited meanings of actual phrases in which the word to be defined occurs") (1926, p. 57). There is a simple test to see whether the definition 'fits the facts':

> To say that a definition of ['cause'] is correct means that that definition can be substituted for the word 'cause' in any [relevant] assertion (...) in which the word occurs, *without in the least changing the meaning which the assertion is felt to have.* (1926, p. 57)

Hart and Honore take a similar view:

> The ordinary man has a quite adequate mastery of various concepts within the field of their day-to-day use, but, along with this practical mastery goes a need for the explicit statement and clarification of the principles involved in the use of these concepts. (1985, p. 26)

Conceptual analysis as just described is not *revisionist.* In these cases there is no intention to improve or replace the established concept of cause, but merely to explicate the concept as given. Conceptual analysis, in the sense discussed so far, is an *explication* of everyday concepts. Sometimes, however, conceptual analysis may be revisionist. Such an approach will involve proposing changes to the way we currently think, perhaps on the grounds of logical consistency or economy[3].

On the other hand, empirical analysis seeks to establish what causation in fact *is* in the actual world. Empirical analysis aims to map the objective world, not our concepts. Such an analysis can only proceed *a posteriori.*

This program has variously been called "empirical metaphysics," (David Armstrong) "ontological metaphysics" (Aronson 1982), "speculative cosmology" (Jackson 1994), "physicalist analysis" (Fair 1979, p. 233) and "factual analysis" (Mackie 1985, p. 178).

According to Mackie this is the way David Hume's famous and influential regularity theory is to be understood:

> In his definitions, which aim at reform rather than analysis of our ordinary concepts, [Hume] equates causation as it really exists in the objects with regular succession. (1974, p. 59)

Mackie himself wishes to understand the ultimate task of philosophy this way:

> The causation I want to know more about is a very general feature (...) of the way the world works: it is not merely, as Hume says, *to us,* but also *in fact* the cement of the universe. (1974, p. 2)
>
> [T]his is an ontological question, a question about how the world goes. In Hume's phrase, the central problem is that of causation 'in the objects.' (1974, p. 1)

Other philosophers who have adopted this general approach to causation include David Fair: "the hypothesised relationship between causation and energy-momentum flows is expected to have the logical status of an empirically discovered identity" (1979, p. 231); Jerrold Aronson: "this 'ontological' approach (...) allows us to perceive causation in objective terms, retaining its rightful place in the physical sciences" (1982, p. 291); John Bigelow and Robert Pargetter: "causation is a robust ingredient within the world itself" (1990b, p. 294); and perhaps Dorothy Emmet: "[we need] to get beyond the discussion of logic and epistemology of causal statements and get into the ontology underlying them." (1985, p. 5). All these philosophers agree that to understand causation we need to go beyond words, and look at the world. This is what is intended by the label 'empirical analysis'.

But 'empirical analysis' as discussed so far covers several distinct concepts of analysis. For example, a given analysis may either have the status of a contingent truth, or of a necessary truth in the style of Kripke (1980). Wesley Salmon (1984) seeks merely to articulate what causation is as a contingent fact while others such as Bigelow and Pargetter (1990b) try to

establish what causation is as an *a posteriori* necessity. We will use the term 'empirical analysis' to cover the first of these options.

Empirical analysis also may or may not be revisionist. A theory of causation which is empirically true may or may not be a good account of our commonsense understanding of causation. Whether it is or not is itself an empirical matter. Hume, for example, thought not: one plausible reading of Hume attributes to him two theories of causation -the conceptual analysis of the everyday notion in terms of necessary connections or hidden power, and the empirical analysis in terms of constant conjunction[4], together with the advice that we should free ourselves of the false connotations of the commonsense notion. If this is correct then Hume was a revisionist. On the other hand, a non-revisionist empirical analyst would claim either that empirical analysis and conceptual analysis coincide, so commonsense delivers the empirically correct theory, or that for some reason commonsense need not match the empirically correct theory where there is a discrepancy.

Insufficient attention to this distinction has been the source of numerous errors in the literature. These mistakes typically arise when philosophers criticise a theory of causation without paying attention to what type of theory it is intended to be. To criticise a theory which aims to explicate the meaning of everyday usage on the grounds of what quantum physics tells us about reality, for example, or to criticise a theory which claims only to provide an empirical analysis on the grounds of its shortcomings as a meaning analysis is to seriously misdirect the criticism. Yet it seems that very commonly this is what is done.

One very common form of this error is to criticise empirical theories on the grounds that they do not adequately account for this or that feature of the way we talk about causation[5]. For example, Beauchamp and Rosenberg claim that much criticism of Hume and J. S. Mill commits this error, by wrongly assuming that their theories are trying to account for everyday usage. Collingwood, Flew and Anscombe are all implicated (1981, pp. 285-286). Thus, Mill and Hume "ought not to be faulted for neglecting to provide the analyses they never intended to provide and had no philosophical reason to undertake." (1981, p. 294).

Although not mentioned by Beauchamp and Rosenberg, Ducasse is most explicit in promoting this kind of criticism of Hume:

> I believe [Hume's] account of the nature of causation -simply as *de facto* succession- represents an incorrect analysis of the ordinary notion of cause (...) To make evident the incorrectness of that analysis it will be sufficient to show, on the one hand,

> that there are cases which conform to Hume's definition but where we judge the events concerned not to be related as cause to effect; and on the other hand, that there are cases which do not conform to Hume's definition but which we nevertheless judge to be cases of causation. (1976, p. 69)

Ducasse then proceeds to provide cases of both types: the first type is a case such as Reid's example of day following night (regular succession but not causation) and the second type is illustrated by Ducasse's brown paper parcel wrapped in string, which, on being pressed at one end glows at the other (causation without regular succession)[6].

A more recent example is the criticism by the Dutch philosopher Dieks of the transference theory of Fair (1981). Dieks, while recognising that Fair is attempting to discover "the true nature of the causal bond" as an ontological category, nevertheless concludes his critique: "So we see that the new analysis of causation has its own share of problems, that is, divergences from the everyday language use of the concept 'cause'." (1981, p. 105).

Perhaps these critics desire in a theory of causation some kind of conceptual analysis of the everyday concept, and they may have good reasons for such a preference. But this debate ought to be conducted explicitly at the metaphilosophical level to avoid the possibility of the errors of the type which I have just described.

Another common error is to require that an empirical analysis hold good for all logically possible worlds. As we have seen, not all theorists are attempting to provide such an analysis. This mistake is made by John Earman, who uses a possible worlds argument against Aronson, when Aronson's theory of causation as energy/momentum transfer is intended as an empirical analysis which holds in the actual world[7]. Earman's objection that we would call a collision in a possible world where energy is not conserved 'causal' (1976, p. 24) ignores the fact that Aronson is not seeking to provide a necessary identity. The same mistake is made by Michael Tooley (1987, ch. 7), who uses a range of possible world arguments to disprove Salmon's theory of causation[8], when Salmon's explicit purpose is to discuss causation as it is found in the actual world. Again, the mistake arises because insufficient attention is paid to the metaphilosophical intentions.

Other examples could be given but this suffices to illustrate the point: there is more than one distinct task a theory of causation might be asked to do, and it is essential to understand what the intentions of the author are with respect to that task before criticising the theory. This is not to say that a critic is obliged to agree with an author about the task of philosophy,

but disagreements about the task of philosophy need to be distinguished from disagreements about whether a theory fits the task for which it was designed.

It is uncontroversial that conceptual analysis has a legitimate role to play in philosophy. On the other hand, the legitimacy of an empirical analysis of causation *has*, at times, been questioned. We shall briefly consider two lines of criticism.

The first argument is one which has sometimes been used in the defence of understanding philosophy purely as linguistic analysis. According to this argument it is not possible to know anything about any language-independent entity called 'causation', because we have no procedures for investigating such an entity (for example, Alston 1967, p. 388). However, the empirical analyst can reply that there are procedures for investigating such an entity, namely the methods of science, which is in the business of investigating language independent objects. Empirical philosophy can draw on the results of science, and so can investigate such things, in this case causation 'in the objects'.

For example, science has shed light on the nature of energy. 'Energy' has today a technical scientific meaning. When we ask the meaning of the term we simply give the scientific definition. Adequate explication of that definition took several centuries, but prior to that achievement, the term simply had a vague range of meaning in everyday language, somewhat as the word 'cause' does today. We can say that application of the scientific method of theorising and experimentation produced an 'empirical analysis' of energy. In the same way, science may reasonably be expected to throw light on the language-independent entity called 'causation'.

The second argument is related to the first. This argument asserts that it is not the role of philosophy to deal in synthetic *a posteriori* matters -that is the exclusive task of science. Ducasse, for example, held this view: "No discovery in any of the sciences has or ever can have any logical bearing upon the problems of philosophy." (1969, p. 120).

The most direct way to answer this is to show how science does inform philosophy about causation, which would indicate that fruitful interaction is possible. Data of this sort is not difficult to find: quantum mechanics has (arguably) shown us that the law of causation (interpreted substantially[9]) is false; entropy and the time reversibility of the basic laws of nature inform us about causal asymmetry; tachyons, Bell's theorem, and Kaon decay have alerted us to the logical possibility of backwards-in-time causation; biomedical science and econometrics have shown us how to directly test causal

claims via path analysis, attribution theory in cognitive psychology tests for us other aspects of causal theorising. And so on. This indicates that science is able to inform philosophy and that empirical analysis in philosophy can draw on scientific results, on empirical, synthetic facts, and use them in analysis. Of course, this might not convince someone like Ducasse whose pronouncement was most likely prescriptive with respect to the nature of philosophy. But if we have clear cases of such an enterprise then the burden of proof lies with those who deny the possibility (see Dowe 1997, sec. 5).

The reply to this second objection throws light on the nature of empirical analysis. There are many ways that science does inform philosophy, and where philosophy takes these results into account, *that* is empirical analysis. So we may reasonably claim to be warranted in assuming that the task of empirical analysis is legitimate[10].

This, of course, is not to deny that conceptual analysis is legitimate. In undertaking an empirical analysis no implication is intended that this is the only way to do philosophy.

It may be objected here that although it is legitimate in its own right, empirical analysis cannot be undertaken without conceptual analysis. Even revisionist approaches require some degree of conceptual analysis of the common usage, or else it would not be clear in what way or degree the theory *is* revisionist, or even whether it deserves the name 'cause'. To do this one must have some account of common usage. As David Lewis says,

> Arbiters of fashion proclaim that analysis is out of date. Yet without it, I see no possible way to establish that any feature of the world does or does not deserve a name drawn from our traditional (...) vocabulary. (1994, p. 415)

So, it will be objected, even in undertaking empirical analysis, some attention to conceptual analysis is required. We want to know the true nature of the thing *we call* causation rather than the true nature of something altogether different. Thus conceptual analysis is needed at the very least to serve as a 'rough guide' or 'an introduction' to an empirical analysis (Compare Mackie 1974, p. 2.)[11].

Bigelow and Pargetter address (or perhaps, avoid) this issue (1990a; 1990b) by taking the concept of cause as a primitive notion so far as meaning analysis goes. They comment

> It is important to recognise that there is a bridgeable but problematic swamp lying between the metaphysics and the semantics of causation. And in offering a metaphysics of causation, we are not pretending to solve all the semantic problems

> (...) As far as semantics is concerned, this causal relation is primitive (...) Our task is metaphysical, not semantic. (1990a, p. 102; 1990b, pp. 278-279)

Unfortunately this does nothing to answer the objection that we need to know whether the thing we find in science deserves the name 'cause'.

However, I think this objection can be met, as follows. In drawing explicitly on scientific judgements rather than intuitions about how we use the word we nevertheless automatically connect to our everyday concept to some extent, because the word 'cause' as scientists use it in those scientific situations must make some 'historical' or 'genealogical' connection to everyday language. This is especially likely given on the one hand 'cause' is not a technically defined term in any scientific theory, and on the other hand the word is not being used playfully or ironically (as is the word 'quark' or the phrase 'eight-fold way'). So to deny that there is an adequate connection is to deny that scientists are competent users of natural logic.

The connection may be to some extent tenuous, in that the resulting analysis is highly revisionist. We certainly do want to avoid assuming *a priori* that for any feature X of our everyday concept of causation, causation actually has feature X. No matter, the historical connection is sufficient to warrant our use of the term.

To return to our previous example, before the development of classical physics 'energy' was a word from everyday vocabulary which, over centuries of scientific endeavour, came to have a very precise scientific meaning. But at no stage of this development was it felt necessary to spell out how the emerging scientific concept differed from the everyday concept, or to what extent it differed. To some extent what one then said was a matter of the conventional assignment of labels: one can imagine comments such as "No, what you're talking about is really 'force' in the scientific sense, not energy." Yet also in some ways the whole development showed that in certain ways common sense was mistaken, for example in the way it had incorporated Aristotelian physics. However I note again that it is not my concern to establish the extent to which common sense is wrong. But the use of the term 'energy' bore an historical connection to the scientific definition, sufficient to warrant that use of the term, even though popular usage of the word continued to be somewhat vague and loose. So the connection between our everyday concept 'cause' and the result of a satisfactory empirical analysis is guaranteed just in virtue of the fact that scientific discussions employ the English word 'cause'.

A further objection may be raised here, as follows. The distinction between conceptual analysis and empirical analysis is not as cut and dried as

has been presented. Any empirical analysis will still be a kind of conceptual analysis, for example, of scientists' usage of the word. This is not the same as analysing the concept in everyday thought and language, but it is the same type of activity.

However, in replying to this objection, we need to note immediately that the task of empirical analysis as undertaken here is not a conceptual analysis of scientists' usage of a term. It is an attempt to understand causation in the world. Certainly we need to look to science to provide us our best information about the world, but for a word such as 'cause' -which is not a technical term in science- scientists' usage may reflect aspects of the everyday concept which are not part of the concept as it emerges from science itself, or which even contradict that concept. In his book *Time's Arrow and Archimedes' Point* (1996), Huw Price accuses scientists of doing this with respect to the direction of time. According to Price science itself is time-symmetric, and the direction of causation is something that we impose on the world. But scientists are not always immune from slipping back into thinking and speaking as if there is an objective direction, and Price catalogues numerous significant examples of what he calls 'that old double standard'. While I don't entirely agree with Price's views (see Dowe 1996a, ch. 8), this example does illustrate the possibility that scientists' linguistic habits may not be the best guide to the structure of a concept emerging from science. Empirical analysis looks to science itself rather than to the linguistic practices of scientists.

Does this undermine my claim that scientists' use of the word 'cause' is sufficient to establish that an empirical analysis of 'cause' has a right to the word? Not at all. Again we must urge that no assumption can be made about the extent to which the common use of the term will match the empirical analysis. The existence of discrepancies is not in itself reason to deny the right. That there are not too many discrepancies for the analysis to have the right is guaranteed by the scientists' use of the term, together with the assumption that scientists are competent users of the language.

The objection may be modified as follows. The distinction between conceptual analysis and empirical analysis is not so cut and dried, any empirical analysis will still be a kind of conceptual analysis, for example, of the concept implicit in scientific theories. I am happy to grant this, and accept that in this sense the distinction is not as cut and dried as I have presented it. As a scientific realist (see Dowe 1996c), I take it that scientific theories are the best guide to the structure of reality, and therefore an empirical analysis, which seeks an analysis of an aspect of the structure of

reality, must look to scientific theories. So I am happy to think of the task of empirical analysis as a conceptual analysis of a concept inherent in scientific theories.

3. Conserved Quantities

A conserved quantity is any quantity which is governed by a conservation law, where current scientific theory is our best guide as to what these are: quantities such as mass-energy, linear momentum, and charge. The idea that the quantities associated with causation are conserved quantities is a suggestion which I present as a plausible conjecture. I have argued elsewhere against Aronson's idea that velocity and certain other physical quantities are the right quantity (Dowe 1995b), and against Salmon's idea that an ability to transmit a mark is the right property (Dowe 1992a; Dowe 1992c), but I have no real quarrel with Fair's position that it is energy/momentum (Fair 1979). I simply offer the conjecture (following Skyrms) that other conserved quantities, such as charge, may also serve the function.

Conservation laws play the role of identifying which quantities are significant for causation. The claim is not that certain quantities are locally conserved in an interaction or by the process in the absence of interactions, although that will follow. Rather, the account focuses on those quantities which are globally, or universally conserved, and connects causality simply to the possessing of those quantities.

It is important that conserved quantities are understood in a way that does not appeal to causation, or else circularity threatens. It is common to define conservation in terms of constancy within a closed system. Now if a closed system is simply one with no external causal interactions, i.e. a system causally isolated from all others, then we face an immediate circularity. The idea is fine as a rule of thumb, that is, it is true; but it cannot work as an analysis. Instead, we need to explicate the notion of a closed system in terms only of the quantities concerned. For example, energy is conserved in chemical reactions, on the assumption that there is no net flow of energy in or out of the system.

It is also important to note that the reference to current theories does not relativise causation to human knowledge -the point is simply that current theories are our best guide to what the conservation laws are. The reason that we cannot simply define a conserved quantity as one that is universally conserved is that some quantity may be accidentally conserved, and

such a quantity should not enter into the analysis of causation. Further, regularities are not by any means the only form of evidence about conservation laws -theoretical considerations are also important.

The identity of 'causal process' with 'the worldline of an object that possesses a conserved quantity' is contingent, and not metaphysically necessary. As we have seen, the hypothesis is that in our world, and in close enough worlds, such as most of those which obey our laws, a causal process is the worldline of an object which possesses a conserved quantity. We leave aside the question of how far we can stray from actuality before this hypothesis stops making sense. In calling this an empirical analysis we emphasise the priority of the claim that the identity holds in actuality. In calling the analysis a contingent identity, we mean that it is contingent on the laws of nature and perhaps even on boundary conditions.

In particular the theory does not purport to tell us what happens to the identity in distant merely possible worlds. Suppose the set $\{q_a, q_b, q_c, q_d\}$ is the complete set of conserved quantities in the actual world W_a, and consider a world W_e where none of this set is in fact conserved, and where a conservation law holds instead for q_e. Is the worldline of an object in W_e which possesses q_e but none of the set of quantities conserved in W_a a causal process? Or again in W_e, is the worldline of an object which possesses q_a say, but none of the quantities conserved at W_e, a causal process? The answer in both cases is that the theory does not say.

The theory may tell us about closer worlds, for example, those with the same conservation laws as ours. In a world where q_a is conserved, but there is only one object which possesses q_a, the worldline of that object is a causal process. Thus the account is not a (Humean) actual-regularity account.

This raises the question of whether the theory is a singularist account (ontologically, not conceptually). I say the account is singularist in the following sense: a particular causal process is not analysed in terms of laws about that type of processes; rather, that a type of process is causal is a matter of generalisation over the particular instantiations of that process-type. The particular is basic.

Thus whether something is a causal process depends only on local facts about the process, namely the object's possession of a certain kind of physical quantity. It does not depend on what happens elsewhere in the universe, so in that sense being causal is an intrinsic property of a process.

Is this a supervenience (ie non-singularist) account in the sense (eg Tooley) that whether the worldline of a is a causal process supervenes on

whether a possesses a quantity q such that there is a law governing q? No, no such claim has been made. The theory simply says at this world, just if an object possesses one of the quantities which is actually conserved, then the worldline of that object is a causal process. This is a local, particular matter.

Alexander Rueger (1998) has argued that in some general relativistic spacetimes, on the conserved quantity theory, it is not a local matter whether a process is causal. Rueger points out that in general relativity global conservation laws may not hold. In the nonrelativistic case a differential conservation law such as the electrodynamic continuity equation:

$$\text{div } \mathbf{j} = -\partial/\partial t\, \rho$$

(where j, is the current density vector (the amount of electric charge moving through a unit volume in a unit time), such that $\mathbf{j} = \rho v$ where v is the charge velocity and *?* is the charge density) entails, via Gauss' theorem, the integral conservation law:

$$\partial/\partial t \int \rho dV = - \int \mathbf{j}\, \mathbf{n}\, dS$$

for a surface S of a volume of integration V. The differential is the local, the integral the global form of the conservation law.

In the general relativistic context, however, a differential conservation law holds for energy-momentum,

$$\nabla^a T_{ab} = 0$$

for the covariant derivative ∇^a, given Einstein's field equations. But unless spacetime possesses special symmetries there will be no integral formulation. Reuger concludes that whether conservation laws hold is contingent on the global properties of spacetime, and that the choice is therefore either to insist that causation is intrinsic, and that there are no genuine causal processes, or to abandon the intuition that causation is intrinsic to a process or event.

However, there is a third option, which follows from what I have already said. The conserved quantity theory is a contingent hypothesis, contingent on the laws of nature, for example. This means if the laws turned out to be a certain way, the theory would be refuted. This may be the case if it turns out that there actually are no conservation laws.

But that there are general relativistic spacetimes in which global conservation laws do not hold does not entail that global conservation laws fail in our world. Whether they do or not depends on the *actual* structure of spacetime, and in particular whether certain symmetries hold. As I understand it, our spacetime does exhibit the right symmetry, and that global conservation laws do hold in our universe as far as we know. I take it, then, that the conserved quantity theory is not refuted.

I suggested that the account holds in all physically possible worlds, that is, in all worlds which have the same laws of nature as ours. Has Rueger shown that this is not so? Not at all. To say, for example, that non-symmetric spacetimes are possible can be misleading. It means simply that it is a solution to the equations of the General Theory of Relativity. But this doesn't mean that such a world is a physically possible world in the sense given above. If such a world violates other laws that hold in the actual world, then that world is not physically possible. This is exactly what we have in these non-symmetric spacetimes. Symmetries and conservation laws that hold in the actual world break down, so it is not a physically possible world in my sense.

Therefore we need not give up on the Conserved Quantity theory, understood as a contingent hypothesis.

4. Identity Through Time

The notion of a process, as explicated in the CQ theory, involves the idea of identity through time. A process is the world line of an object, so fundamentally, to constitute a process, an object must persist over time. This analysis presupposes a notion of identity through time; since a worldline is the line traced out by an object through time and so it is necessary that the object is the same object at different times (Dowe 1995a; Dowe 1999).

In rejecting the identity assumption implicit in the conserved quantity theory Wesley Salmon writes

> I have offered a concept of causal transmission analysed in terms of the "at-at" theory for which Dowe has traded an unanalysed concept of genidentity. This is not, I think, an advantageous exchange. (1997, p. 468)

Salmon comments on his own revised version, that "it yields a criterion that is impeccably empirical, and thus it provides an acceptable answer to the fundamental problem Hume raised about causality." (1997, p. 468).

So Salmon's objection to taking identity over time as primitive in a theory of causal processes appears to be that it violates the empiricist's stricture that one should not invoke empirically inaccessible elements as unanalysed or primitive in a philosophical theory. In this section we consider the main accounts of identity through time, and we will find that there are two options that meet Salmon's objection, provided that we restrict our goal to an empirical analysis.

According to the currently popular causal theory of identity[12], the relation of identity through time involves the relation of causation: for an object to display identity over time it is required as a necessary condition that its temporal parts are related as cause and effect.

However, such an account is not available for present purposes. If the CQ theory of causal processes is correct then the relevant notion of causation is itself dependent on the notion of identity through time, so the causal account of identity is excluded, or else the whole account would be circular. So I reject the causal account of identity through time. I do agree that there is a strong connection between causal connection and genidentity, but I claim that identity is the more basic notion: causal processes cannot be understood except in terms of identity over time.

If the causal theory is not available, what account can we give of identity through time? There are two main alternatives, strict identity and similarity-continuity.

One way to analyse identity is as literally strict identity. An object is identical with its other temporal parts in the same way that it is identical with itself, and in the same way that different things may have the same property (see Armstrong 1980). According to this view *an object must be wholly present at a time* in order to exist at that time. That is, when you have an object at a time, it's not that strictly speaking you just have a part of that object- a temporal part. If you have an object then you have the whole object at that time.

A major difficulty with strict identity is the problem of 'temporary intrinsics', in Lewis' phrase (1986, pp. 202-420). If an individual can have contradictory intrinsic properties at different times, how can it exhibit strict identity? One reply is to say that such properties are time indexed, and that there is no contradiction in having one intrinsic property at one time and another at a different time. One way to explicate this, mentioned (but not endorsed) by David Lewis is to treat these properties as disguised relations -relations to times.

Salmon's objection would be circumvented simply by adopting the strict identity position. Contrary to a widely held opinion, it doesn't require any unanalysed concepts -for a start nothing is clearer than self identity, and in any case, the requirement of identity over time can be stated without using the concept of identity by using a property of totality (I thank David Lewis for this suggestion): there is an object and the whole of it is located at t_2. (See Lewis 1986, pp. 192-193). So one option is simply to take identity as strict identity (see Dowe 1999).

The alternative to taking objects as wholly present at a time is to take objects as essentially four dimensional, existing in time in exactly the same way as they exist in space. On this view timeslices of the worldline are parts of the object -temporal parts. To adopt the 4-D conception would require some further way of identifying causal processes as genuine, some way of ruling out time-wise gerrymanders. One example is the attempt by Fair to achieve this by appeal to the identity over time of the quantity itself; unfortunately that notion can be shown to be incoherent. (Dowe 1995b) Another is Salmon's appeal to the notion of transmission. To do so via the concept of identity, without appealing to strict identity, one must say what is the relation between the temporal parts. Common ways to do this are the causal theory and the similarity-continuity theory of identity and we now turn to the latter.

The 'similarity-continuity' approach to identity through time is sometimes called a 'Humean' conception, even though strictly speaking Hume's own theory of identity was a causal theory, and it makes particular sense on a Humean metaphysics: *viz.* that the world is a world of 'bits', particular local matters of fact, with no logical connections between spatiotemporally separated bits. Since there are no logical connections between the bits, there can be no relations of strict identity across time. However, there can be surrogate relations between the bits which can link bits as being temporal parts of the same object. The similarity-continuity account of identity says that bits at different times are connected as temporal parts of the same object, in a looser sense of 'same', just if appropriate spatiotemporal and resemblance relations hold between the bits. For example, a round black hard object at x at t_1 is the same object as one at y at t_2 if, roughly, the second object is also black, hard and round, and at every spatiotemporal point between x and y there is a hard round black object. Obviously more complicated relations are expected to obtain for more general cases involving more radical changes.

A common line of argument against such Humean accounts is well represented by an argument due to David Armstrong. Armstrong gives an example of two gods who are creating and destroying things independently. By coincidence, one destroys a certain item just at the same instant that the other creates an exact replica of that item at the same location[13]. It may seem to everyone that the same object still exists, but they would all be wrong. It is really a different thing. Armstrong concludes that similarity-continuity theories cannot account for identity through time.

However, this kind of objection does not hold in this context, since we are seeking an empirical analysis. We want to know what causation is in this world, and perhaps in worlds with the same laws of nature as ours, and so what would happen in far distant worlds is of no relevance. In this context there is no need to satisfy criteria for conceptual analysis.

Salmon's objection is therefore met on the similarity-continuity account. There is nothing in this account that can offend the empiricist demand that one should not invoke empirically inaccessible elements as unanalysed or primitive in a philosophical theory.

So, in summary, we have seen that of the main current approaches to identity over time the most popular, the causal theory, is unsatisfactory; but that either the strict theory or the continuity-similarity theory would do the job, both meeting Salmon's objection. In particular, since Salmon's empiricist requirements are easily met by the continuity-similarity account, that approach seems best fitted to meet the objection.

5. Summary

I have argued that the conserved quantity theory is to be taken as an empirical analysis. As such it seeks to establish what causation in fact *is* in the actual world, to map the objective world, not our concepts. Such an analysis proceeds *a posteriori*. Conceived this way, the conserved quantity theory avoids two recent objections.

First, Alexander Rueger (1998) has argued that in some general relativistic spacetimes, on the conserved quantity theory, it is not a local matter whether a process is causal, since in general relativity global conservation laws may not hold. So whether conservation laws hold is contingent on the global properties of spacetime.

However the conserved quantity theory is a contingent hypothesis, contingent on the laws of nature, for example. This means if the laws turned out to be a certain way, the theory would be refuted. But that there are gen-

eral relativistic spacetimes in which global conservation laws do not hold does not entail that global conservation laws fail in our world. Whether they do or not depends on the *actual* structure of spacetime, and in particular whether certain symmetries hold. Our spacetime does exhibit the right symmetry, and that global conservation laws do hold in our universe as far as we know. So the conserved quantity theory is not refuted.

Second, Salmon (1997) has criticised my version of the conserved quantity theory on the grounds that its appeal to genidentity violates the empiricist requirement that all elements of the thory be epistemically accessible. The problem is that the account rules out the popular causal account of identity, and that the alternatives the strict identity and similarity-continuity accounts of identity are open to well-known fatal objections. However, given that the Conserved Quantity theory is simply an empirical analysis, not conceptual analysis, the main objections against the similarity-continuity account disappear, since that account is much more plausible taken as an empirical analysis as opposed to conceptual analysis.

Notes

[1] These are not the only ways that the philosophy of causation has been conceived. Compare, for example, Skyrms' pragmatic analysis (1980) and Mellor's 'middle way' (1995).

[2] What is intended by these terms will become clear in the subsequent elucidation. It's the distinction Bigelow and Pargetter denoted by the terms 'semantics' and 'metaphysics' respectively (1990b, pp. 278-279), and Jackson by the terms 'conceptual analysis' and 'metaphysics' respectively (1994). In fact, it's virtually impossible to find a pair of terms that is not objectionable on some grounds. Conceptual analysis is empirical in the sense that it is concerned with how a word is used in a language, an *a posteriori*, empirical matter. It's also ontological insofar as it articulates the logical ontology of the concept. It's also what many philosophers mean by 'metaphysics'. On the other hand, empirical analysis is also about concepts; the concepts we think map the world. And so on.

[3] Compare Strawson's distinction between revisionary and descriptive metaphysics (1959). By 'metaphysics' Strawson means what I mean by 'conceptual analysis'. See also the 'reformatory analysis' of Ackerman (1995).

[4] See also Beauchamp and Rosenberg (1981, especially ch. 8).

[5] Bigelow and Pargetter warn "It will be no objection to our proposals to cite one or another causal idiom which we have failed to explain" (1990b, p. 278). See also Millikan 1989 and Neander 1991.

6 A similar example of this kind of mistake is Hart and Honore (1985, pp. 22, 34), who criticise Mill on the grounds that his theory neglects several aspects of normal speech; and by Hugh Mellor, who argues that both Hume and Salmon fail in their respective theories because they do not capture all the connotations of causation (1988, p. 231), when neither Hume nor Salmon are attempting any sort of conceptual analysis of the everyday meaning of cause.

7 "The transference theory is intended to make sense of how causation takes place in *this* world, (...) not in some alien universe where the laws of physics do not in the least resemble ours" (Aronson 1982, p. 302).

8 (Salmon 1984). For a detailed analysis see Dowe (1989).

9 That is, 'every event has a sufficient cause'. This informs our thinking about the concept of causation if, for example, we are accustomed to thinking that causes are sufficient conditions for their effects, and yet are forced to accept that there are cases which we cannot but call 'causation', where the full cause is not a sufficient condition for its effect.

10 For a defence of an alternative view see Tooley (1987; 1990).

11 Jim Woodward pointed out to me that if objectivity is part of our concept of causation then an adequate conceptual analysis will need to respect empirical results. See also the approach of Mellor (1995).

12 For example, Armstrong (1980b). A causal theory of identity was defended by Hume, who thought identity through time could be analysed in terms of resemblance, contiguity and causation (1975, p. 246). For Hume, causation reduces to contiguity and resemblance; and resemblance is a relation of ideas.

13 (1980, p. 76). (In that version the 'items' were Richard Taylor and his twin).

BIBLIOGRAPHY

Ackerman, F.: 1995, 'Analysis', in J. Kim, E. Sosa, E. (ed.): *A Companion to Metaphysics*, Oxford, Blackwell, pp.9-11.

Alston, W.: 1967, 'Philosophy of Language', in P. Edwards (ed.): *Encyclopedia of Philosophy*, New York, Macmillan, pp. 386-390.

Armstrong, D.: 1997, *A World of States of Affairs*, Cambridge, Cambridge University Press.

Armstrong, D.M.: 1980, 'Identity Through Time', in P. van Inwagen (ed.): *Time and Cause*, Dordrecht, Reidel, pp. 67-78.

Aronson, J.: 1982, 'Untangling Ontology from Epistemology in Causation', *Erkenntnis* 18, 293-305.

Beauchamp, T., Rosenberg, A.: 1981, *Hume and the Problem of Causation*, New York, Oxford University Press.

Bigelow, J., Pargetter, R.: 1990a, 'Metaphysics of Causation', *Erkenntnis* 33, 89-119.

Bigelow, J. and Pargetter, R.: 1990b, *Science and Necessity*. Cambridge: Cambridge University Press.

Dieks, D.: 1981, 'A Note on Causation and the Flow of Energy', *Erkenntnis* 16, 103-108.

Dowe, P.: 1989, 'On Tooley On Salmon', *Australasian Journal of Philosophy* 64, 469-471.

Dowe, P.: 1992a, 'An Empiricist Defence of the Causal Account of Explanation', *International Studies in the Philosophy of Science* 6, 123-128.
Dowe, P.: 1992b, 'Process Causality and Asymmetry', *Erkenntnis* 37, 179-196.
Dowe, P.: 1992c, 'Wesley Salmon's Process Theory of Causality and the Conserved Quantity Theory', *Philosophy of Science* 59, 195-216.
Dowe, P.: 1993, 'On the Reduction of Process Causality to Statistical Relations', *British Journal for the Philosophy of Science* 44, 325-327.
Dowe, P.: 1995a, 'Causality and Conserved Quantities: A Reply to Salmon', *Philosophy of Science* 62, 321-333.
Dowe, P.: 1995b, 'What's Right and What's Wrong with Transference Theories', *Erkenntnis* 42, 363-374.
Dowe, P.: 1996a, 'Backwards Causation and the Direction of Causal Processes', *Mind* 105, 227-248.
Dowe, P.: 1996b, 'Chance Lowering Causes: Old Problems for New Versions of the Probabilistic Theory of Causation', in P. Dowe, K. Korb, J. Oliver (ed.): *Information, Statistics and Induction in Science*, Singapore, World Scientific Publishing Company, pp. 226-236.
Dowe, P.: 1996c, 'JJC Smart and the Rise of Scientific Realism', in P. Dowe, M. Nicholls, L. Shotton (ed.): *Australian Philosophers*, Hobart, University of Tasmania Press, pp. 25-37.
Dowe, P.: 1997, 'A Defense of Backwards in Time Causation Models in Quantum Mechanics', *Synthese* 112, 233-246.
Dowe, P.: 1999, 'Good Connections: Causation and Causal Processes', in H. Sankey (ed.): *Causation and Laws of Nature*, Dordrecht, Kluwer, pp. 323-346.
Dowe, P.: 2000, *Physical Causation*, New York, Cambridge University Press.
Ducasse, C.: 1926, 'On the Nature and the Observability of the Causal Relation', *Journal of Philosophy* 23, 57-68.
Ducasse, C.: 1969, *Causation and the Types of Necessity*, New York, Dover.
Ducasse, C.: 1976, 'Causality: Critique of Hume's Analysis', in M. Brand (ed.): *The Nature of Causation*, Urbana, University of Illinois Press, pp. 67-76.
Earman, J.: 1976, 'Causation: A Matter of Life and Death', *The Journal of Philosophy* 73, 5-25.
Emmet, D.: 1985, *The Effectiveness of Causes*, Albany, State University of New York Press.
Fair, D.: 1979, 'Causation and the Flow of Energy', *Erkenntnis* 14, 219-250.
Hart, H., Honore, T.: 1985, *Causation in the Law*, Oxford, Clarendon.
Hume, D.: 1975, *An Enquiry Concerning Human Understanding*, 3rd ed., L.A. Selby-Bigge, P.H. Nidditch (ed.), Oxford, Clarendon.
Jackson, F.: 1994, 'Armchair Metaphysics', in M. Michael, J. O'Leary-Hawthorne (ed.): *Philosophy in Mind*, Dordrecht, Kluwer, pp. 23-42.
Kripke, S.: 1980, *Naming and Necessity*, Oxford, Blackwell.
Lewis, D.: 1986, *Philosophical Papers Volume II*, New York, Oxford University Press.
Lewis, D.: 1994, 'Reduction of Mind', in S. Guttenplan (ed.): *A Companion to Philosophy of Mind*, Cambridge, Mass., Blackwell, pp. 412-431.
Mackie, J.: 1974, *The Cement of the Universe*, Oxford, Clarendon.
Mackie, J.: 1985, *Logic and Knowledge - Selected Papers*, Oxford, Clarendon.
Mellor, D.: 1988, 'On Raising the Chances of Effects', in J. Fetzer (ed.): *Probability and Causality: Essays in Honour of Wesley C. Salmon*, Dordrecht, Reidel, pp. 229-239.
Mellor, D.: 1995, *The Facts of Causation*, London, Routledge.
Millikan, R.: 1989, 'In Defense of Proper Functions', *Philosophy of Science* 56, 288-302.
Neander, K.: 1991, 'Functions as Selected Effects: The Conceptual Analyst's Defense', ***Philosophy of Science*** 58, 168-184.

Price, H.: 1996, *Time's Arrow and Archimedes' Point*, Oxford, Oxford University Press.
Reuger, A.: 1998, 'Local Theories of Causation and the *A Posteriori* Identification of the Causal Relation', *Erkenntnis* 48, 25-38.
Salmon, W.: 1984, *Scientific Explanation and the Causal Structure of the World*, Princeton, Princeton University Press.
Salmon, W.: 1997, 'Causality and Explanation: A Reply to Two Critiques', *Philosophy of Science* 64, 461-477.
Salmon, W.: 1998, *Causality and Explanation*, New York, Oxford University Press.
Skyrms, B.: 1980, *Causal Necessity*, New Haven, Yale University Press.
Strawson, P.: 1959, *Individuals*, London, University Paperbacks.
Tooley, M.: 1987, *Causation: A Realist Approach*, Oxford, Clarendon.
Tooley, M.: 1990, 'The Nature of Causation: A Singularist Account', in D. Copp (ed.): *Canadian Philosophers Celebrating Twenty Years of the Canadian Journal of Philosophy*, Calgary, The University of Calgary Press, 1990, pp. 271-322.

Phil Dowe is Professor in the Department of Philosophy, University of Tasmania. He has published widely on process theories of causation, and related topics, and is finishing a book entitled *Physical Causation.*