1. Introduction. In the course of his journey from the statistical relevance model of explanation through the causal account to the conserved quantities theory, Wes Salmon was responsible for a number of significant changes in our way of thinking about both explanation and causation. Perhaps the most profound contribution that he made to the literature on these subjects was his insistence that ontic accounts of both causation and explanation can be made plausible. This kind of scientific realism, Salmon insisted, had to be compatible with a recognizable form of empiricism. And in so doing, he seemed to make the task of constructing and evaluating a satisfactory theory of explanation much more difficult than it had hitherto been. For unlike most previous accounts of explanation and causation, Salmon’s ontic accounts seem to be based on empirical facts rather than on conceptual analysis and they are, if true, contingently true. One of Salmon’s great insights was to see that we must understand the world on its own terms rather than to insist that a single a priori explanatory framework is adequate. This is exactly what a scientific realist should require. If our world had been different and devoid of causal processes then we should have to explain things differently. Salmon’s realism is, nevertheless, slanted in a particular direction. I argued in Humphreys [2000] that because they are

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based on conserved quantities, Salmon’s final theories of causation and explanation require a commitment to a quite radical form of physicalism, making the theory difficult to apply to social, economic, and psychological phenomena. This commitment would seem to narrow the scope of the theories considerably unless a comprehensive reduction of the social and psychological sciences to physics can be achieved.

I now think that the empirical content inherent in Salmon’s theories, although still present, is less problematical than I once thought and this makes the philosophical problems involved in evaluating them less severe than they might appear. Furthermore, the degree of commitment to physicalism that is required in Salmon’s most mature theories is quite minimal. Taken together, these suggest that the combination of realism and empiricism in Salmon’s theories requires less of a change in philosophical methods than might be expected.

2. The modal status of theories of causation and explanation. There are five kinds of features that theories of causation and, separately, theories of scientific explanation, may possess. The most familiar is that they are necessarily true\(^1\), where the modality is metaphysical necessity, i.e., what the theory says about causes and explanations is true in all possible worlds. This is the approach standardly taken by those wishing to define causation, as for example, David Lewis

\[^1\]I shall assume that all theories I discuss are true in order to avoid the continual use of ‘if true, then necessarily true’ and related hypotheticals.
did. So, if you subscribe to a counterfactual theory of causation, it is not an accident that causes happen in our world to be such that if the cause event had not occurred, then the effect event would not have occurred either. It is in the very nature of a causal relation that this must be true. Whatever the world might be like, wherever there is a causal relation, it must have the properties stipulated in the definition. This is a familiar property of any account arrived at by conceptual analysis. As a second, albeit less interesting, possibility, a theory of causation or explanation might involve conceptual necessity, i.e. it is impossible for cognitive agents similar to ourselves to consistently conceive of a counter-example to the theory. What is the strength of the impossibility in this claim? That depends upon your theory of mind. My own view, because I hold that psychological properties emerge from biochemical and physical properties, is that the appropriate impossibility for this position is a nomological impossibility, but others, especially those who adhere to a particular kind of supervenience position, may be able to make a case that it involves a metaphysical impossibility. Conceptual necessity is weaker than metaphysical necessity, for it allows that there might be worlds, the causal structure of which is conceptually unintelligible to us, within which causes operate but in ways we can never grasp.

As a third possibility, the theories of causation and explanation might appeal to nomological necessity, i.e. the theory of causation or explanation is true in all worlds having the same scientific laws as ours but there might be other kinds of causation in worlds with different laws. The fact that nomological possibilities can be involved in both the second and the third kinds of possibility does not make the second kind a special case of the third. To hold that it is

\[ \text{\footnotesize 2 Lewis [1973], [1986]. Because the definitive version of Lewis’s later paper ‘Causation as Influence’ has not been published at the time of writing, I have not classified that theory.} \]
nomologically possible or impossible for a cognitive agent to consistently conceive of a counterexample to a theory is quite different from asserting that such a nomological possibility does or does not exist. It is a striking feature of minds that they are capable, despite being physically implemented, of transcending, within the conceptual realm, what is nomologically possible.

These three kinds of necessity can be contrasted with a fourth feature that might be possessed by a theory of causation or explanation, that of being universally true, independently of subject matter. Although there need be no modal content to this kind of theory, it still makes a striking claim – that there is a uniform set of features possessed by all causes. It asserts, amongst other things, that there is no kind of causation special to physics or special to sociology.

The last kind of causal or explanatory theory is one that is domain specific or subject matter specific, one that is true for some subject matters but inapplicable to others. For a theory to be subject matter specific is for its conditions of adequacy either to make essential reference to some specific subject matter or subject matters or to preclude the application of those conditions to certain kinds of subject matter.

These divisions are important because they affect how we assess the truth of these theories. Traditional a priori analyses can effectively address the truth status of metaphysically or conceptually necessary theories of causation and explanation, but each of the last three kinds of theory requires additional, empirical, knowledge. The more domain specific the theory, the more scientific and the less philosophical the theory appears to be. This is not an insurmountable objection to domain specific theories, but it does require us to say something explicit about how they are to be judged.
3. **The status of Salmon’s theory.** The two principal divisions used in the taxonomy above, the more traditional one of necessary versus contingent theories, and the perhaps less familiar one of universal versus domain specific theories, are such that all four combinations are logically possible. There can be necessarily true domain specific theories and contingently true universal theories, as well as contingently true domain specific theories and necessarily true universal theories. Most traditional analyses of causation are intended to be both universal and necessarily true. In contrast, if the structure of our world is such that an ontological reduction of the subject matter of all other sciences to that of physics is possible, but this reduction is a contingent fact about our world, then a theory of causation that grounds causation in physical processes will give a contingently true but universal theory. As a third possibility, because David Fair’s energy transmission theory is nomologically necessary and, if the kind of reduction just discussed is not possible because the laws of certain non-physical sciences are sui generis, then Fair’s will be a nomologically necessary but domain specific theory.³

My main focus in this paper is the status of Salmon’s theories and in order to assess his final accounts of causation and explanation, we need to trace the intellectual history that led to their development. Salmon’s early statistical-relevance approach to explanation (Salmon [1970]) undercut the nomic expectability basis of Hempel’s deductive-nomological and inductive statistical models of explanation by showing that events with low probabilities could be explained. This was done by citing factors that assigned events to objectively homogenous

³I am not aware of any metaphysically necessary domain specific theories of causation or explanation but such things are surely possible. In mathematics, for example, arithmetic and geometry are necessarily true but domain specific.
reference classes. To explain an event was to assign it to the broadest such homogenous reference class, that class being arrived at by successive specifications of statistically relevant factors. Because such a theory needs to distinguish between statistical associations that are the result of direct causal connections and those that are not, Salmon began to construct a theory of probabilistic causation. Yet the task of completing Hans Reichenbach’s purely probabilistic theory of causation proved elusive for Salmon. Faced with the problem of distinguishing between mere correlations and genuine causal relations, Salmon decided to view statistical relevance relations as simply the evidential starting point of explanations, a basis that itself had to be explained. This was done in terms of an ontology of spatially continuous processes and of interactions between those processes. The criterion of a genuine causal process, as opposed to a pseudo-process, was its ability to have its structure altered after an interaction. This criterion seemed to have a counterfactual claim embedded in it – what would happen if a mark were to be introduced – and conscientious empiricists such as Salmon wanted no part of irreducible counterfactual claims. I should add that it was not just empiricist qualms that led Salmon to reject counterfactuals but a dissatisfaction with their context-dependence: ‘A major part of the motivation for [the change from a marked processes view to a conserved quantities view] was an aversion to counterfactuals. I was seeking completely objective causal concepts; counterfactuals are notoriously context dependent.’ (Salmon [1997], p.470). So following the lead of Phil Dowe (Dowe [1992a]), causation was instead taken to consist in the transfer of conserved quantities: ‘...causal processes transmit conserved quantities; and by virtue of this fact, they are causal...’ (Salmon [1994], p.303)
Salmon’s account of explanation based on the conserved quantity theory of causation is a straightforward example of a domain specific contingent theory. Salmon was quite explicit about this when he wrote: `What constitutes an adequate explanation depends crucially, I think, on the kind of world in which we live; moreover what constitutes an adequate explanation may differ from one domain to another in the actual world. ... The ontic conception mandates attention to the mechanisms that actually operate in the domain in which explanation is sought.’ (Salmon [1985] p.299) Salmon also maintained that ontic explanations are usually grounded in causes: `According to the ontic conception – as I see it at least – an explanation of an event involves exhibiting that event as it is embedded in its causal network and/or displaying its internal causal structure.’ (Salmon [1985], p.298), but he also allows that causal explanations are a subclass of the more general class of ontic explanations: `...causal explanations of the sort just discussed are adequate and appropriate in many domains of science but that other mechanisms – possibly of a radically non-causal sort – operate in the quantum domain.’ (Salmon [1985], p. 298)

This lack of universality is in stark contrast to the situation with Hempel’s deductive-nomological and inductive-statistical models of explanation because the deductive-nomological and inductive-statistical models of scientific explanation are both metaphysically necessary. (The logical empiricists, of course, would have been horrified by that terminology.) They are necessary and also universal because their central concept of nomic expectability based upon logical inference is applicable independently of subject matter. For those subjects or worlds

\[\text{\textsuperscript{4}Salmon goes on to say in the next paragraph that there could be non-causal ontic explanations and that there may be quantum mechanical explanations for which continuous causal processes are not involved.}\]
lacking laws – a famous and controversial candidate was history – there simply are no Hempelian explanations: `The decisive requirement for every sound explanation remains that it subsume the explanandum under general laws.'\textsuperscript{5} But what of other worlds in which there exists subject matter not of our world – suppose, to take an elementary case, that a genetically engineered species had been invented that fitted into none of the biological categories existing in our world? Although Hempel did not as far as I know address this kind of issue, it is reasonable to infer from the way his theories were presented that nomic expectability was essential to any adequate scientific explanation and that such other-worldly cases must conform to his criteria of adequacy for explanations.\textsuperscript{6}

Of the other rivals to Salmon’s theory in the area of explanation, the pragmatic account’s context-dependency is not inherently subject matter dependent. The relevance relation changes from context to context because of a questioner’s interests but while these interests may differ with areas of investigation, they can equally well be determined by factors that are independent of the subject matter. As for the unification approaches, it has often been pointed out that whether or not they are universally true in our world is a contingent matter, but that objection underestimates the degree to which Kantian motivations are driving the theory. Indeed, the whole thrust of the unification approach is to erase the boundaries between areas that are currently considered to be separate.

\textsuperscript{5}The quotation is from p. 258 of the slightly revised 1965 reprint of Hempel [1948].

\textsuperscript{6}This is clear in the case of D-N explanations from Hempel’s four conditions of adequacy for that model (Hempel [1948], pp. 137-138).
In contrast to his later conserved quantity account, Salmon’s original statistical relevance account was universal because the statistical relevance relations upon which the theory is based are subject matter independent. Whether a given factor A is statistically relevant to the frequency of another factor B within reference class R is, of course, a contingent matter, discoverable only by examining sequences of empirically generated data, but the statistical relevance relations themselves, at least on the relative frequency approach, rest on arithmetical relations and hence can be applied whatever the origin of the data.

I mentioned earlier that Salmon’s shift to a conserved quantity account seems to commit him to physicalism in a fairly dramatic way. There are few, if any, conservation laws in the social sciences and the emphasis on conserved quantities thus seems to either severely limit the scope of the theory or to commit him to a reductionist or eliminativist program of a quite extreme kind. It seems to entail that all anthropological and sociological causation, for example, must be accounted for in terms of the transfer of mass-energy, linear and angular momentum, and other conserved quantities. And so all explanations in those sciences must ultimately be given in terms of physical causation. Perhaps this is what physicalists believe, and it presents in a stark form the kind of reductionist ontology that produces the various problems of mental causation. There is, in consequence, a kind of ineliminable subject-dependence in the conserved quantity account of causation, a dependence upon the availability of a certain kind of physics underlying all phenomena we consider to be causal, and there is no reason to think that this dependence must hold across all subject matters in all possible worlds, whether those worlds are nomologically or metaphysically possible. In contrast, analyses in terms of sufficient or necessary conditions, or those in terms of statistical relevance relations, have no difficulty in dealing with social or economic causation because they are universal accounts and in
consequence can remain neutral on the reductionism issue. Even Salmon’s middle period theory of markable processes can, in principle, be applied to any material subject matter. Marked bills, distorted states of consciousness, changes in social structure, are all plausible examples within, respectively, economics, psychology, and sociology.

4. Hitchcock’s criticisms and the hybrid theory. So, Salmon’s theories of causation and explanation are neither necessarily true nor universal. Despite this, I think that Salmon’s theory is much more general than it might appear from the features discussed in the last section. A considerable amount of emphasis is placed by Salmon on what he calls causal processes and causal interactions. Yet there is in his use of the term ‘causal’ the possibility for a serious confusion, a possibility that is compounded by not infrequent references to the transmission of causal influences. We can see that the role played by the term ‘causal’ in these references to ‘causal processes’ is minimal by recalling that more than one conserved quantity can be transmitted by a causal process and that more than one conserved quantity can be exchanged during a causal interaction. If it were the transmission of ‘causal influence’ that was crucial for causation, we would have the analog of a causal overdetermination problem for the conserved quantity theory. As it is, either conserved quantity will do to make the processes and interactions causal and it is irrelevant which quantity is conserved or exchanged as long as at least one is present. And indeed, the explanatory factors in Salmon’s final theory are not ordinarily those that make the interactions and processes causal. Under the pressure of criticisms due to Chris Hitchcock ⁷, Salmon modified his theory in the mid-1990s so that it became a hybrid. The appeal to conserved quantities distinguishes between causal and pseudo-processes, but the explanatory sufficient

⁷ See Hitchcock [1995].
factors in any given case must include those factors that are statistically relevant. As Salmon put it: ‘In [Scientific Explanation and the Causal Structure of the World ] I characterized scientific explanation as a two tiered structure, consisting of statistical relevance relations on the one hand and causal processes and interactions on the other. As a result of Hitchcock’s analysis, I would now say (1) that statistical relevance relations, in the absence of connecting causal processes, lack explanatory import and (2) that connecting causal processes, in the absence of statistical relevance relations also lack explanatory import. In various discussions I have focused on (1) to the virtual neglect of (2)...this was a mistake. Both are indispensable’. (Salmon [1997], p.476.) The re-emphasis on the statistically relevant factors in the conserved quantity approach thus restores many of the examples that made the statistical-relevance model initially plausible. We explain delinquency in terms of broken homes, unemployed fathers, depressed economies, and low levels of education, not in terms of conserved quantities. Of course, mass-energy conservation holds in the social realm as well, but it is not the relevant variable.

This means that despite the frequent use of examples involving colliding billiard balls, baseballs breaking windows, spots of light moving along walls, and so forth, the role played by the so-called causal processes in explanations is minimal. All that is required is a commitment to a widely held form of physicalism, the view that every non-physical property is carried by one of these causal processes, or, as I prefer to call them, ‘carrier processes’. Thus, every economic mechanism, every sociological variable, and every psychological property must be conveyed by a physical carrier process. And whether you are a reductionist, a supervenience advocate, a property dualist, or almost anything other than a substance dualist or an idealist, this is a small commitment indeed. It does not get us any degree of necessity for the processes and interactions part of Salmon’s theory, although I believe that one could argue that the nomological necessity
of the ontic view ought to follow from this position if conservation laws were insisted upon rather than mere regularities. In addition, the fact that the principal explanatory role is played by statistically relevant factors means that a considerable part of, although certainly not all, the explanatory theory is indeed subject-matter independent.

5. A Residual Tension. Yet there remains something unsatisfactory about this hybrid account. There had long been a tension in Salmon’s theory between, on the one hand, the kind of example typified by the hexed salt example, where what was important was the relevancy or irrelevancy of cited characteristics and, on the other hand, the kind of considerations which were introduced by examples such as the transmission of heritable characteristics and the decay of radioactive atoms, for which it was not the increase or decrease of the probability value that was important but the transmission of a probability value or distribution. The rejection of the increase in probability approach to explanation was, it seems safe to say, motivated for Salmon by examples such as those involving the transmission of genetic characteristics that he frequently cited in support of the transmission of probabilities view. And in fact such appeals to statistical mechanisms underlay the entire ontic account of causal processes: “Scientific understanding according to [the ontic] conception involves laying bare the mechanisms – etiological or constitutive, causal or non-causal – that bring about the fact to be explained.” (Salmon [1985], p.301). On the ontic view, explanation must promote understanding of how the world works and that understanding is mechanical: In a previously unpublished article that first appeared in his Causation and Explanation collection (Salmon [1998]), Salmon wrote: “I shall examine two general forms of scientific understanding...The second involves understanding the basic mechanisms that operate in our world, that is, knowing how things work. This kind of
understanding is mechanical.’ (Salmon [1998a], p.81) Salmon goes on to say a few pages later ‘It is the kind of understanding we achieve when we take apart an old-fashioned watch, with springs and cogged wheels, and successfully put it together again, seeing how each part functions in relation to all the others’ (Salmon [1998a], p.87).

In order to lessen the tension between the statistical-relevance view and the transmission of probabilities view, I want to suggest an approach that lies within the broad outlines of the empirical realist framework Salmon has given us. It is, I am sure, one with which he would have disagreed. So I shall present the arguments for the alternative, but leave the conclusion in the form of a choice that must be made between two quite different ways of conceiving of causation and explanation. Each has its merits and although I have a preference for one of them, I can quite well see the appeal of the other.

6. Conserved quantities are not sufficient for causation. We saw above that in his 1997 article (Salmon [1997]), Salmon had suggested that a greater degree of balance needed to be achieved between the emphasis on causal processes and mechanisms and the emphasis on statistically relevant factors. To provide an ontic explanation is to specify the mechanisms and patterns that were involved in the production of the explanandum, and these mechanisms are to be analyzed in terms of the process account. There are two aspects to this process approach adopted by Salmon. One is the `at-at’ formulation of processes, which conforms to the idea that there are no mysterious non-humean connections between events within single processes. The other is his rejection of regularity accounts of causation. In a 1985 article he wrote: `It may be possible – thought I seriously doubt it – to construct a regularity analysis of causality that would be adequate within the context of Laplacean determinism... Although I do not have any knock-down argument to support my contention, my sense of the objections to [theories of probabilistic
causality based on statistical regularities] convinces me (at least tentatively) that no such regularity analysis of probabilistic causality will be adequate. We must instead look to mechanisms’ (Salmon [1985], pp.296-297) Let me begin with the at-at theory as it is employed in the conserved quantities account of causation.

Salmon’s formulation of the conserved quantities theory is encapsulated in the following three propositions:

1. A causal interaction is an intersection of worldlines which involves exchange of a conserved quantity. (Salmon [1994], p.303)

2. A causal process is a worldline of an object that transmits a nonzero amount of an invariant quantity at each moment of its history (each space-time point of its trajectory). (Salmon [1994], p.308)

3. A process transmits a conserved quantity between A and B (A\#B) if and only if it possesses [a fixed amount of] this quantity at A and at B and at every stage of the process between A and B without any interactions in the open interval (A,B) that involve an exchange of that particular conserved quantity. (Salmon [1997], p.462)

And he says of this definition that `...it yields a criterion that is impeccably empirical, and thus it provides an acceptable answer to the fundamental problem Hume raised about causality.’ (Salmon [1997], p.469)
This definition is perhaps too humean, for proposition 1 allows as causal interactions things that are clearly neither causal nor interactive. Consider two marching bands wearing identical uniforms. They are skilled at performing the kinds of intricate pass-through maneuvers characteristic of such bands but they lack a certain sort of discipline. Whenever the bands intersect, the individual members spontaneously decide whether to reel off left or right according to their own whim. They are sufficiently skilled that nobody ever collides with another bandsman, and the emerging bands, which will be composed of mixtures of members of the original bands and will ordinarily be of different sizes, always coalesce into two ordered bands ready for the next intersection. Now take \( L = \) total number of bandsmen in the left hand band and \( R = \) total number of bandsmen in the right hand band. \( L + R \) is a conserved quantity—the total number of bandsmen emerging from a band intersection is always the same as the number going in. This renders the above definition of a causal interaction applicable and the two marching bands constitute two causal processes involved in a causal interaction simply because of the conservation of number principle. This should strike you as odd.

In response to this objection, Salmon responded in conversation that number is not an conserved quantity—for example, placing two rabbits of the opposite sex in a hutch will quickly result in a serious violation of rabbit number conservation. Point taken, but there are three counters to this reply. First, if this kind of response were legitimate it would show that the laws of arithmetic were not necessarily true, and there are well-known strategies one can adopt in order to show that such examples are misplaced. A less trivial counter is to note that Salmon allows, in response to a point made by Phil Dowe (Dowe [1992b]), that he, Salmon, wants the theory of causal processes and interactions to be considered at the theoretical level, where a number of idealizations and abstractions from actual processes is permissible. (Salmon [1997], p.
464). This is a perfectly reasonable move to make and we can preserve the conservation of number principle by imposing closure conditions on rabbit systems. These will be straightforward constraints on rabbit (and bandsmen) interactions—no opposite sex interactions, no applicability to pregnant rabbits, confinement to a closed region, and application to living rabbits only. Indeed, with these restrictions on the scope of the generalization, conservation of rabbit number is as good a candidate for a true conserved quantity principle as are the genuine scientific laws of conservation of baryon number and conservation of lepton number. To remind you what these are: baryons are particles which exert strong nuclear forces and have fractional spins. Examples are neutrons and protons. In all known interactions, the number of baryons entering into an interaction is equal to the number of baryons exiting the interaction. A similar law holds for leptons, which are particles exerting weak interactive forces and having spin \( \frac{1}{2} \), such as electrons, positrons, muons, and neutrinos. Finally, it might be said that the conservation of number principle is not a scientific law. If that were so, then it is sufficient to point out that Salmon does not require generalizations about conserved quantities to be law like; he merely requires that they be true ([1994], p.310). He does this to prevent re-entering the modal circle via law likeness, a re-entry that he wants to avoid, given that the motivation for shifting from the mark transmission criterion for causal processes to the transmission of invariant quantities was to avoid recourse to counterfactual criteria for mark transmission. But if all that is required is for the invariance of number principle to be true, rather than to be a law, then with the idealizations mentioned earlier, that condition is satisfied.

So, we have an example that seems to show Salmon's theory is too broad. I mentioned above that it is odd to call something an interaction where what we have is, in the case of the
marching bands, merely a spatial coincidence and concomitant change. But its oddness is merely a reflection of what is involved in these hume an accounts of causal interactions. In the marching bands case there is no interaction of any recognizable kind—the bandsmen reach their appointed spot on the field, then scatter in one of two directions by a purely chance-like process. In fact the scattering could be arranged by spontaneous fusion in a single band after a set period of time (thus creating a `y fork’) and the interactive content of this is even less obvious.

This detour through the nature of causal interactions has brought us back to the basic issue of the modal status of Salmon’s account. For one thing that these considerations suggest is that some conservation laws play the role of what Michael Friedman calls `constitutive a priori principles’. Roughly speaking, constitutive a priori principles are principles that are not metaphysically necessarily but must be adopted in order for a specific scientific theory to be applied. Well-known examples of constitutive a priori principles are the choice of geometry in classical gravitational theory and general relativity, and the adoption of Newton’s three laws in classical mechanics. Certain conservation principles such as the conservation of energy are good candidates for the role of constitutive a priori principles. If they are, then because such principles are a priori in nature – they are accepted or rejected on grounds other than coherence with or conflict with empirical data – that aspect of the ontic account has something like a philosophical rather than a scientific aspect.

8See, for example, Friedman [2001]. The idea, happily, goes back to Reichenbach, who called them `coordinative definitions’. For a different view of the role played by these principles, see the Appendix to Chapter One of Ryckman [forthcoming].
That said, I shall finish this section by noting a residual problem. Unless we can exclude the conservation of number principle somehow – and note that it is has empirical content in virtue of the subject-specific idealizations that are required to make it true for a given domain – the conserved quantities approach does not even mandate the kind of minimal commitment to physicalism that I described earlier. For even granting Quine’s concerns about the difficulties of counting certain non-physical entities such as possibilia and beliefs, there is no difficulty at all in counting objects that are specifically social or economic in form. There are currently thirty three fraternities at the University of Virginia. A fraternity is not a physical entity – it is partly a social unit, partly a cultural unit, partly a legal entity, and it exists quasi-independently of its current human members and physical assets. It thus seems possible to apply certain conservation principles to social entities without any commitment in those cases to physicalism. Deciding what to do with such examples leads us into interesting but complex metaphysical territory and so I shall not pursue the issue further here.

7. Conclusion. What does all this say about Salmon’s causal theory? I mentioned that Salmon’s most recent account of causation and explanation was a hybrid theory. It contains references to processes and interactions on the one hand and to statistically relevant factors on the other. How best to weight these components is, I think, a key question that Salmon has bequeathed to us. My own taste, because I hold that causation is primarily a relation between properties (see Humphreys [2004], section 2.9), leans towards emphasizing the relevance relations and a more radical version of the anti-regularity approach to causation than I think Salmon would have been comfortable with. In particular, singular causal relations are basic for me. Yet there is also an undeniable appeal to the kind of neo-humean position that Salmon
developed in his at-at account of processes and interactions. It requires us to bring into close
contact answers to Why-questions and answers to How-questions and in doing so to rest content
with a scientific description of the world. But even if one goes in that direction, I hope to have
convinced you that this does not require a wholesale abandonment, but only a modification, of
traditional philosophical methods for deciding which approach is the better of the two.9

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