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Is There an Independent Principle of Causality in Physics?

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ABSTRACT

Mathias Frisch has argued that the requirement that electromagnetic dispersion processes are causal adds empirical content not found in electrodynamic theory. I urge that this attempt to reconstitute a local principle of causality in physics fails. An independent principle is not needed to recover the results of dispersion theory. The use of 'causality conditions' proves to be the mere adding of causal labels to an already presumed fact. If instead one seeks a broader, independently formulated grounding for the conditions, that grounding either fails or dissolves into vagueness and ambiguity, as has traditionally been the fate of candidate principles of causality.

1 Introduction

In his ([2009a]), Mathias Frisch responds to a skeptical tradition to which I have contributed (Norton [2003], [2007]). That tradition doubts that the sciences are founded upon an independent principle of causality. His response leads him to argue that the requirement that certain physical processes are causal does add further physical content. His example is dispersion in classical electrodynamics. There causal considerations are invoked at a decisive moment in the derivation of the dispersion relations, purportedly to provide physical content not recoverable through the usual manipulations of electrodynamic theory.

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In this sense, Frisch is proposing a principle of causality. His principle asserts that effects cannot precede their causes. It is independent in the sense that it provides factual content not supplied by the relevant physical laws. Since he is sure only that it applies to a few physical processes, Frisch allows that the principle may not hold universally. So it is the proposal for an independent principle of causality of restricted scope.

While Frisch's example is both important and intriguing, my purpose in this note is to argue that Frisch is mistaken in his analysis of it. After a brief review of dispersion theory in Section 2, I will urge in Section 3 that the causal constraints at issue are merely shorthand for physical constraints already recoverable in classical electrodynamics, though possibly not easily recoverable. While I believe that we need to summon no causal metaphysics to complete dispersion theory, in Section 4 I will explore the consequences of persisting in efforts to do just this. Those efforts lead us in two directions. In one, we merely end up assigning an additional adjective 'causal' to a condition we believe on other grounds. In the other, we seek a precise, independent expression, usable in physical theorizing, for the general requirement that effects cannot precede their causes in processes like dispersion. Efforts to formulate this principle independently lead to failure or vagueness and ambiguity.

This note was drafted as a response to parts of an earlier paper by Frisch ([unpublished], Section 4). Frisch's present paper ([2009a]) is a rendering of the relevant material from that earlier paper and he has now written a rejoinder to this note (Frisch [2009b]). I do not believe that Frisch's rejoinder succeeds. However, I will refrain from responding point by point to his responses, lest that trigger a combinatorial explosion. The text in the remaining sections is unchanged other than in its references and in the elimination of a footnote that has become superfluous.[1] Readers of both Frisch and my notes are invited to make the final decision.

With regard to that decision, one point does bear discussion. My analysis follows the standard presumption that phenomenological results in classical electrodynamics are supervenient on a microdynamics governed by time reversible electrodynamics. These time reversible foundations cannot support a time irreversible principle of causality. Time asymmetry can only be introduced by stipulation through time asymmetric boundary conditions. Frisch has made clear in his rejoinder the importance to him of his doubts about this standard view. He believes ([2009a], Section 3) that 'there are reasons to doubt that dispersion theory with its causality condition can be rigorously derived from an underlying micro-theory'.

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[1] An earlier version of that text was posted on philsci-archive as <philsci-archive.pitt.edu/archive/00003832/> and the present revision was later posted to my website, <www.pitt.edu/~jdnorton>.

This issue is too great in scope to be settled here. Fortunately, my principal point stands, however it may be decided. Physicists developing dispersion theory arrive at a general result through a complicated mix of intuitions about how electrodynamical systems behave, the experimental evidence, and more precise computations on artificial examples. They have sought to legitimize that result by appeals to 'causality', apparently believing that this calls up a greater body of theory capable of grounding their inference. Yet there is no such greater body of theory of sufficient precision to be adequate to the task. Their use of causal terminology is an exercise in labeling that merely brings an illusion of more principled foundations.

2 Scattering in Classical Electrodynamics

In classical electrodynamics, a dielectric scatters an incoming field. In the roughest outline, the basic supposition is that the scattered field at a point **x** in space in the dielectric at time t, written here as '*scattered*(**x**, t)', depends linearly on the incident field '*incident*(**x**, t')' at the same point **x** and other times t'. Hence *scattered*(**x**, t) can be reconstructed if we know just which scattered fields G(**x**, t) arise from a delta function incident field, at the same point **x** but massed at time 0. [2] Linearity allows us to recover the scattered field from an arbitrary incoming field by the integration

$$scattered(\mathbf{x}, t) = \int_{-\infty}^{\infty} G(\mathbf{x}, t') incident(\mathbf{x}, t - t') dt'. \tag{1}$$

In this integral, the scattered field at **x** at time t is computed as a weighted sum of the incident field values at that same point **x** but at different times. Informally, we are just summing the effects of the many pulses that comprise the incident wave.

The quantity $G(\mathbf{x}, t')$ determines the times for which the incident field at **x** contributes. The causality condition at issue requires that no incident field at a time later than t can contribute to the scattered field at t. It is enforced by requiring that[3]

$$G(\mathbf{x}, t') = 0, \text{ for all } t' < 0 \tag{2}$$

for, with this condition, *incident*(**x**, $t - t'$) can make no contribution to the integral of (1) whenever $t - t' > t$, that is, whenever $t' < 0$. Condition (2) tells us that the scattered wave from an incident pulse is never earlier than the pulse.

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[2] That amounts to saying that G is a Green's function. Its characteristics may be easier to see if we rewrite (1) in terms of the variable $\tau = t - t'$. For then (1) becomes $scattered(\mathbf{x}, t) = \int_{-\infty}^{\infty} G(\mathbf{x}, t - \tau) incident(\mathbf{x}, \tau) d\tau$.

[3] Or that the integral of (1) be computed only between the limits of $t' = 0$ and $t' = \infty$.

3 Sufficiency of the Physics

The question at issue is the physical foundation of this condition (2). Frisch believes that it is founded upon a principle of causality that is independent of electrodynamic theory in the sense that it places additional factual constraints on the theory. I maintain that the condition can and should be founded upon existing electrodynamic theory alone.

To see why I hold this latter view, recall the physical process at issue. A dielectric in classical theory consists of electric charges bound by restraining forces in some sort of lattice, such as a crystal. Left to themselves, the charges do not move. In a scattering process, an incident electromagnetic wave impinges on them and accelerates them. That sets off an oscillatory motion that in turn leads the oscillating charges to emit electromagnetic radiation. The emitted radiation is the scattered field. The physical picture is analogous to a buoy sitting in calm water. When water waves impinge on it, the buoy wobbles and thereby sends out its own secondary ripples.

Classical electrodynamics is a time reversible theory. If it allows some process, the theory also allows its time reverse. That means that the time reverse of scattering is also allowed. In that case, we have a process in which electromagnetic waves collapse down onto electric charges that are already oscillating. They move in a way that is perfectly coordinated to collect the radiation coming in from all directions and re-emit it in just one direction, and then come to rest. It is possible to have even more complicated behaviors that combine the essential elements of the two types of behavior sketched.

In setting up the standard analysis of scattering theory, we *choose* to consider a small subset of all these possible processes. That is, we choose to consider the special case that starts with the charges of the dielectric at rest with a (typically) unidirectional, incident wave approaching. Our choice is expressed in initial or boundary conditions that merely describe mathematically the situation just sketched in words. We expect that, in these particular cases, the charges of the dielectric will remain at rest until the incoming wave arrives. Only then will they accelerate and emit the scattered field. Condition (2) merely translates that expectation into the particular case of a charge initially at rest interacting with a pulse incident wave; the charge does not accelerate and emit a secondary wave until the pulse has arrived.

The time asymmetry of the resulting processes arises purely from our decision to consider a subset of all possible processes that happen to have a strong asymmetry in time. No additional causal metaphysics is needed to impose the time asymmetry. We stipulate it.

Condition (2) on the quantity $G(\mathbf{x}, t')$ expresses formally the physical idea that the scattered field always comes after the incident field that excites it. In principle, that (2) obtains is deducible in a full analysis of how the charges of the dielectric are accelerated by the incident wave in which we compute a definite formula for $G(\mathbf{x}, t')$. In practice, the sorts of forces that hold the charges in the dielectric can have many forms and giving the completely general computation is prohibitively complicated. As we shall see, the computation can be done in some simple cases to get a full expression for $G(\mathbf{x}, t')$ that does conform with (2). There is little doubt that the most general computation, if it could be done, would still return (2), for it is really only saying that, in the cases we are considering, the dielectric charges respond to incident radiation; they do not anticipate it.

In the context of the precise computations of scattering theory, this last claim is an awkward one to make. Other formulas are computed precisely; this one is just opined. It is quite understandable that physics authors might succumb to the temptation of proclaiming the real foundation of (2) to lie in 'causality', for that gives it an apparently more principled foundation. Perhaps they imagine that somewhere, somehow, some philosopher has spelled out all the details needed to make the inference precise. Yet, as I will argue in Section 4 below, any such confidence is misplaced. The apparent security of this foundation can persist only as long as one does not try to spell out precisely what that foundation is.

The extent to which writers of physics texts do succumb to this temptation is not entirely clear to me. Frisch quotes Jackson's ([1999]) text as his primary source. Yet Jackson (in my edition) does not explicitly deduce (2) from an independent principle of causality or give a precise formulation of such a principle. What he does do, as Frisch notes, is to deduce the condition (2) from standard electrodynamics for a special case (Section 7.10.B) without drawing on causality conditions. He then observes (Section 7.10.C) that this outcome is 'in accord with our fundamental ideas of causality in physical phenomena' and finally announces that (1) and (2) combined comprise 'the most general spatially local, linear, and causal relation…'

The development does not make clear how the step from the special to the general case is taken. It might be, as Frisch suggests, that Jackson is calling upon some more fundamental principle of causality that lies outside classical electrodynamics. Or it might just be that Jackson is following a more benign approach that needs no additional physical principles. That is, he is suggesting that, in all of the more complicated cases, a more detailed and possibly very difficult analysis fully within electrodynamics would return the same result (2). Since he is not giving the analysis but, nonetheless, is confident of its outcome, he tries to make the result plausible by noting that it fits with causal expectations, although a precise causal principle is not actually formulated and used to deduce the result.[4]

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[4] There is a similar ambiguity in another source Frisch cites. Toll's ([1956]) principal goal is to prove the logical equivalence of dispersion relations in a fairly general context and 'strict causality'. The latter asserts that 'no signal can travel faster than [c]' and is also glossed by a condition that

4 Failure of the Principle of Causality Proposed

While I believe a principle of causality is not needed to complete dispersion theory, we can ask what are our prospects of formulating a non-trivial principle that could serve this purpose. The question is worth exploring since Frisch at least interprets the physics literature as asserting that the derivation of results in dispersion theory are completed by invoking an independent principle of causality.

Such attempts require a great deal more than a vague gesture at 'our fundamental ideas of causality'. They require the presentation of a viable principle of causality in a sufficiently precise form for its applicability and proper functioning in this case to be apparent. My contention in this section is that neither Jackson nor Frisch formulates such a principle. Frisch's candidate for the applicable principle is that (Frisch [2009a], Section 2): 'an effect cannot temporally precede its causes'.

I will seek to show the following. When we try to formulate a general statement of the principle in terms sufficiently precise for physical theorizing, the principle either fails or becomes too vague to use. If, however, we retract and merely declare the one case of dispersion to be an implementation of a principle for which we offer no more general statement, then all we have achieved is a relabeling of the facts of one case in suggestive but physically empty causal language.

4.1 A sometimes principle

Frisch does not want to commit to the universal applicability of this principle. 'It might in fact be true', he allows, '. . .But I think we can allow for the possibility that a certain causal condition is not true in general and nevertheless take it to be physically well founded'. ([2009a], Section 3)

If Frisch is serious about this possibility, then his sometimes principle may be no independent principle at all. It looks like a principle that holds, except when it doesn't. For, speaking figuratively, how are we to know whether it applies to some system? We must call upon the properties of that system and affirm that the principle holds for them. That is, the obtaining of the principle of causality threatens merely to be a restating of the properties of the system already known. The danger is that the conformity of the system to the principle

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is not obviously equivalent to it, 'no output can occur before the input' (p. 1760). However, Toll does not make clear whether strict causality is a universal principle to be required independently of all physical theories or merely a result to be discovered and demonstrated within each physical theory under consideration. Remarks in the concluding section suggest the latter. For Toll finds it (p. 1770) 'an open question whether or not strict causality is a valid physical hypothesis' and then considers merely as a possibility that 'strict causality. . . prove[s] to be invalid or unenforceable in future theories'.

places no additional factual constraint upon it. In that sense, it would be merely honorific.

That threat is realized if we retreat to the safety of merely considering dispersion processes in isolation. For then we can comfortably declare the incident field a 'cause', the scattered field an 'effect', and the relation (2) of dispersion theory as expressing the principle of causality in that it assures us that these particular effects never precede their causes. If we just consider dispersion theory in isolation, the exercise is purely one of labeling. Nothing is added by the causal talk beyond restating the specific result already at hand in new causal language.

That exercise in labeling is clearly not what Frisch intends. But if the exercise is to be anything more, there must be some relation to systems and processes outside dispersion theory. Of course there is such a connection in an informal sense. The incident wave is analogous to my shout, as is the scattered wave to the startled cats, fleeing from the overturned cream jug. But that informal connection merely makes the application of the causal language comfortable. It does not locate dispersion processes within broader factual regularities; and it does not supply a theoretical instrument of sufficient precision to enable completion of physical computations in dispersion theory. What we need is some more general property of the system that would mark it antecedently as causal. Then we would know antecedently that the restricted principle of causality must apply to it and that it falls into a greater causal order in nature, even if not a universal order.

In the following I will investigate what is needed to provide a more general principle that is also precise enough for application in dispersion theory and other physical theories.

4.2 The conditions of applicability are obscure

Let us presume that Frisch's principle of causality is formulated precisely enough for us to apply it in physical theories. If the principle is to serve as indicated in scattering theory, it must be clear that the principle applies. To see that its applicability is questionable, we need to recall that classical electrodynamics is a time reversible theory. If the theory allows a process, then it also allows its time reverse. The theory allows a dielectric to scatter an incident wave. Therefore, it also allows a time reversed, scattered wave to collapse back onto the dielectric and return the time reverse of the incident wave. Of course this reverse process is highly unlikely in ordinary circumstances, just as it is possible, but highly unlikely, for ripples in a pond to converge and eject a stone.

Now imagine a universe completely empty excepting two processes that we will call 'A' and 'B'. Process A has an incident wave, a dielectric, and a scattered wave. Process B is the time reverse of A. The two processes are completely isomorphic in all properties. Any property of one will have its isomorphic correlate in the other. Any fact about one will have a correlate fact obtaining for the other. One might be tempted to imagine that one of the two processes is 'really' the ordinary one, progressing normally in time; while the other is a theoretician's fantasy, a possibility in principle, but in practice unrealizable. The essential point of the example is that no property of the A and B systems distinguish which is which. Every property of one has a perfect correlate in the other.

Let us assume that Frisch's principle of causality applies to one of these processes, the A process, for example. That will be expressed as a condition that the present state of the process depends only on its past states. Exactly what 'depends' may amount to is to be decided by the principle. All that matters for our purposes is that an exactly isomorphic condition of dependence will be obtained in the B process, except that it will be time reversed. Indeed, using the time order natural to process A, we would have to say that the principle of causality requires the present states of process B to depend upon its future states.

In short, if the principle applies to process A, it fails for process B; and conversely. This is a *reductio ad absurdum* of the applicability of Frisch's principle of causality to scattering in classical electrodynamics. Or, to put the outcome another way, if the principle holds for one process but not the other, then the decision as to which process is properly causal cannot be based on any physical difference between the two processes. For every physical property of one process has an exact correlate in the other. The declaration that the principle holds for one process but not the other has become an arbitrary stipulation without a physical basis.

It seems to me that there is only one escape. It is to propose that there is, as a factual matter outside electrodynamics, a natural time direction. When we require that electrodynamics must respect that direction, we are able to preclude one of the two processes. Possibilities of this sort have been repeatedly offered, weighed, and found wanting. Nearly a century ago, for example, Ritz (in Einstein and Ritz [1909]) urged that electrodynamics should be formulated in terms of retarded potentials only, thereby denying the time reversibility of the theory. The mainstream agreed with Einstein's response. He insisted that the time-reversed processes were possible, just statistically very unlikely.[5]

To get a sense of why the mainstream has flowed in this direction, recall that the time-reversed process can be broken up into many small parts. Locally, each small section of the collapsing incident wave is merely a wavefront propagating in quite ordinary ways; momentarily, the force exerted by the incident wave on the unscattering charges just follows the Lorentz force law; and so on

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[5] Ritz's proposal is of no help to Frisch's principle of causality. For with it, no principle of causality is needed to complete the scattering computation. The restriction to retarded potentials is a precise electrodynamic expression of the restriction that the present electrodynamic state depends only on its past state.

throughout. It is only when the many small pieces are assembled that we find an unfamiliar process. It seems mistaken to invoke additional laws to prohibit a total system, perfectly admissible in all its parts. Indeed, we have little doubt that were we somehow to contrive a perfect time reversal of a scattered wave, its future course would be the time reverse of ordinary scattering.

Of course, Frisch is well aware of the problems of combining a time-asymmetric principle of causality with a time-symmetric physical theory and discusses them at some length in his ([2009a], Section 3). None of the arguments given there escapes the difficulty just described for his principle of causality. His most interesting proposal is that we might recover the time asymmetry of causation empirically. We intervene in a process and discover as a matter of experimental fact that our intervention perturbs the future and not the past.

It is precisely to avoid such considerations that my example of the A and B processes assumes an otherwise empty universe in which there are no agents to intervene. In any case, it is unclear what the intervention experiments reveal. The system at issue is now exceedingly complicated and poorly understood: it is the scattering system plus human beings who poke their fingers into the beams and have sensors—eyes—presumed not to emit radiation but only to absorb it. How are we to know whether asymmetries arising in a system that complicated are due to an intrinsically asymmetric causal relation? Or are they due to some asymmetry introduced into an otherwise fully time symmetric theory through the conditions that describe the vastly complicated system of humans with intervening fingers and watching eyes? And do we doubt that, if those fingers could bring about the time reversal of a scattered wave as an initial condition, then the time reverse of scattering would ensue?

4.3 Effects can come before their causes

What of the principal content of the principle, that effects cannot precede causes? Frisch ([2009a], Section 3) supports his principle of causality with the remark that neither of my earlier writings (Norton [2003], [2007]) includes counterexamples to it. That lacuna is easily remedied here, for there are many cases in which the effect preceding the cause is accepted as a possibility.

There is a flourishing literature on the physical possibility of time travel (Arntzenius and Maudlin [2005]). It is no longer believed that problems of causality—the 'grandfather paradox'—preclude its physical possibility. Every instance of time travel involves effects preceding their causes.

Another example arises in the case of tachyons in special relativity. If one observer judges a tachyon to be propagating forward in time, then we can always find a second observer who is moving inertially with respect to the first and who judges the time order of the states to be reversed. If the first observer judges that the direction of propagation coincides with the direction of causal action, then the second observer would describe that judgment as affirming that the later states causally affect the earlier states.[6]

Finally, there is a third example in the A and B processes above. Let us say that there is some sense in which the incident wave of A is the cause of the scattered wave of A, the effect. Then, under the isomorphism indicated above, we will conclude that the later state of the time-reversed incident wave of B is the cause of the earlier time-reversed scattered wave of B.

The idea that effects might precede their causes has sufficient currency that it has become the subject of an article, "Backwards Causation" in the *Stanford Encyclopedia of Philosophy* (Faye [2005]).

4.4 Vagueness of the relata and of the notion of causal process

If Frisch's principle of causality is to be applicable to physical systems, we must know *precisely* what is a cause, what is an effect, and which processes are causal. Otherwise we will not know how to apply the principle in concrete cases of physical theorizing.

Consider the first two questions. What precisely is a cause? What precisely is an effect? Can anything be a cause and an effect? Surely there must be some restrictions and they need to be made explicit. Presumably, for example, causes and effects must be the states of physical systems restricted in time, for the principle makes assertions on their time order. Are they any state extended over some short time interval? Or are they any state at an instant? If we are in special relativity, which instant do we choose? The relativity of simultaneity allows many competing instants, according to the inertial frame of reference chosen. If we seek to avoid the problem by requiring the states to be localized at a point in space, are we thereby precluding by fiat that non-local quantum states can enter into causal relations?

Now the third question: What sorts of processes are properly labeled as causal? In the case of scattering, causal dependence was translated into the mathematical dependence of a field at one time on another field at other times. Are all such dependencies causal processes? If so, what are we to make of Lagrange principles? A particle's trajectory is picked out as the one that extremizes the integrated action for its entire history between a start and an end point. As a result, the computation of the acceleration of the particle now involves its motion at earlier and later times. Is this dependence causal? If so, we have a violation of Frisch's principle of causality, for a future state is affecting the present state. If it is not causal, how are we to pick out the computations that correspond to real causal processes?

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[6] Should we judge tachyons to be physically impossible since this sort of backward causation might lead to paradoxes? While that concern has been debated at some length in the literature, I agree with Arntzenius' ([1990]) conclusion that paradox-free tachyonic theories are possible.

Finally, when we have a causal process, how do we decide which of the relata is the cause and which is the effect? We cannot merely use their time order to decide; that is, we cannot just pick out the cause as the earlier relatum and the effect as the later. That reduces the principle that causes cannot precede their effects to a definition. If the principle is to have physical content, it must provide an independent characterization of what intrinsically distinguishes a cause from an effect. What is that characterization?[7]

5 Conclusion

In sum, it is not so hard to find vague and sometimes grand-sounding causal talk in the physics literature. It is easy to yield to the temptation of saying that this shows that there really is an independent principle of causality at work in physics. There is, however, a chasm between vague and grand causal talk and a precise principle of causality that can and should be used to augment the physical content of existing physical theories. Frisch's proposal has not closed that gap and, on the basis of the many obstacles just presented, it seems unlikely that this gap can be closed.

In (Norton [2003]), I suggested that causal fundamentalism and its associated principle of causality succumbed to a dilemma: either the view places factual constraints on the world (in which case they proved false); or they did not (in which case the causal labels were honorifics). Frisch's attempt to locate physical content in the idea that dispersion is causal meets an analogous fate. Insofar as it is precisely stated, his causality condition is not derived from a broader regularity; it merely attaches the adjective 'causal' to a particular result in scattering theory. The result is factual; but it is already so and not made any more or less factual by the adding of the adjective 'causal'. When we seek the broader regularity, we find that it most likely cannot be stated precisely enough for application in physical theorizing, even allowing that this broader regularity need not hold universally.

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[7] One might call upon Reichenbach's famous idea that real causal processes are distinguished as those that can carry a 'mark'. That idea is of no help to Frisch's principle of causality since, as Grünbaum ([1973]) has argued, the mark criterion fails to pick out a direction in time and reveals only causal relatedness.

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References

Arntzenius, F. [1990]: 'Causal Paradoxes in Special Relativity', *British Journal for the Philosophy of Science*, **41**, pp. 223–43.

Arntzenius, F. and Maudlin, T. [2005]: 'Time Travel and Modern Physics', in Edward N. Zalta (*ed.*), *The Stanford Encyclopedia of Philosophy* (Summer 2005 Edition), <plato.stanford.edu/archives/sum2005/entries/time-travel-phys/>.

Einstein, A. and Ritz, W. [1909]: 'Zum gegenwärtigen Stand des Strahlungsproblems' *Physikalische Zeitschrift*, **10**, pp. 323–4.

Faye, J. [2005]: 'Backward Causation', in Edward N. Zalta (*ed.*), *The Stanford Encyclopedia of Philosophy* (Fall 2005 Edition), <plato.stanford.edu/archives/fall2005/entries/causation-backwards/>.

Frisch, M. [unpublished]: 'Causal Reasoning in Physics', In 2008 Causation Workshop (Pittsburgh, PA; January 26, 2008), <philsci-archive.pitt.edu/archive/00003820>.

Frisch, M. [2009a]: '"The Most Sacred Tenet"? Causal Reasoning in Physics', *British Journal for the Philosophy of Science*, **60**, pp. 459–74.

Frisch, M. [2009b]: 'Causality and Dispersion: A Reply to John Norton', *British Journal for the Philosophy of Science*, **60**, pp. 487–95.

Grünbaum, A. [1973]: 'The Causal Theory of Time,' in R. S. Cohen and M. W. Wartofsky, (*eds*), *Philosophical Problems of Space and Time: Boston Studies in Philosophy of Science*, Volume XII, Dordrecht: Reidel, pp. 180–208

Jackson, J. D. [1999]: *Classical Electrodynamics*, 3rd edition, New York: Wiley.

Norton, J. D. [2003]: 'Causation as Folk Science', *Philosophers' Imprint*, **3**(4), <www.philosophersimprint.org/003004/>. Reprinted in H. Price and R. Corry (*eds*), *Causation and the Constitution of Reality: Russell's Republic Revisited*, Oxford: Clarendon Press, pp. 11–44.

Norton, J. D. [2007]: 'Do the Causal Principles of Modern Physics Contradict Causal Anti-Fundamentalism?', in P. K. Machamer and G. Wolters (*eds*), *Thinking About Causes: From Greek Philosophy to Modern Physics*, Pittsburgh, PA: University of Pittsburgh Press, pp. 222–34.

Toll, J. S. [1956]: 'Causality and the Dispersion Relation: Logical Foundations,' *Physical Review*, **104**, pp. 1760–70.