‘The most Sacred Tenet’?
Causal Reasoning in Physics

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Abstract

According to a view widely held among philosophers of science, the notion of cause has no legitimate role to play in mature theories of physics. In this paper I investigate the role of what physicists themselves identify as causal principles in the derivation of dispersion relations. I argue that this case study constitutes a counterexample to the popular view and that causal principles can function as genuine factual constraints.

1. Introduction

It appears to be both natural and intuitive to think of the world as causally evolving. We conceive of events in the present as being caused by events in the past and, in turn, as acting as causes for what happens in the future. Yet while causal notions are an integral part of our common sense conceptions of the world and arguably also play an important role in the special sciences, it is less clear whether there is a legitimate place for causal reasoning in mature physics. Is it the case, as Bertrand Russell famously argued ([1918]), that causal reasoning plays no role in modern physics and that causal concepts in physics have been replaced by other, non-causal notions? Or is, to the contrary, a principle of causality ‘the most sacred tenet in all of physics,’ as the physicist David Griffiths suggests ([1989], p. 399)? On the one hand, it appears to be a widespread view among philosophers of physics that causal notions are not part of how mature physics represents the world. According to this view, the notion of cause survives—if at all—as part of a ‘folk’ scientific conception of the world but has no place in our mature theories of physics (see, for example, van Fraassen [1993]; Norton [2003] and [2007]; Hitchcock [2007]). On the other hand, *pace* Russell, causal language remains pervasive in physics. What role then do putatively causal principles play in theorizing in physics? Do such principles provide a genuine factual physical constraint? Or do causal notions function merely as labeling devices—as empty honorifics, as John Norton has argued—that do not constrain the content of a theory? In this paper I hope to make some progress toward answering these questions.

Philosophers of science have proposed a range of general anti-causal arguments—many of which are descendents of Russell's attack on the notion of cause—intending to show that causal notions cannot play a scientifically legitimate role in physics. I criticize some of these arguments in (Frisch [unpublished]) and argue that none of these are sound. However, the fact that prominent anti-causal arguments fail does not, of course, show that the causal skeptic's conclusion is false. Thus, in the present paper I want to examine one concrete example of putatively causal reasoning in physics—the derivation of dispersion relations from a condition that in the literature is generally identified as a causality condition—with the aim of trying to understand what role the fact that the constraint is identified as 'causal' plays in the theory.

What do physicists mean when they invoke causal notions? Three dimensions of our notion of cause appear to play particularly important roles in theorizing in physics: that causes determine their effects, that causes act locally, and that the notion of causation is asymmetric. The last of these aspects, which frequently is taken to coincide with the temporal asymmetry, may even be thought to constitute *the* core of our notion of cause. My case study concerns a time-asymmetric causal condition.

I will use Norton's recent discussions of the role of causal notions in physics as my main foil and, thus, it might be helpful to distinguish at the outset the claims on which we agree (or which I do not want to challenge here) from those points of disagreement that will be the focus of my discussion. Norton rejects any a priori metaphysical commitment to a principle of causality and insists that any such principle ought to be justified empirically. With this I fully agree. The claim that causal principles play a legitimate role in theorizing in physics, if defensible at all, has to be supported by appeals to the scientific fruitfulness and the empirical success of such principles and cannot be supported by an a priori metaphysics. If Norton and I disagree, then it is only over the question whether causal reasoning does indeed play a scientifically legitimate role in physics and on whether and how causal principles can be scientifically justified.

Norton presents his causal skepticism as criticism of a view he calls 'causal fundamentalism' and which maintains that 'nature is governed by cause and effect and the burden of individual sciences is to find the particular expression of the general notion in the realm of their specialized subject matter' (Norton [2003]). Two important aspects of causal fundamentalism, as construed by Norton, are, first, that it is committed to a universal principle of causality, and second that it is a thesis about the metaphysics of causation. I want to resist drawing any metaphysical conclusions in this paper. I am interested in whether asymmetric causal constraints play a role in scientific theorizing on a par with other physical constraints, such as the principle of energy conservation or other nomic constraints. How the postulates of a theory ought to be interpreted metaphysically is a separate question. The debate between van Fraassen's constructive empiricism, instrumentalism, and scientific realism can serve as a useful comparison here. Both van Fraassen's empiricist and the realist agree that there are scientific theories that properly understood posit unobservable entities, such as electrons or quarks, yet they disagree on whether acceptance of these theories entails a commitment to the reality of the entities in question. By contrast, an instrumentalist denies that scientific theories, properly understood, posit unobservable entities. Analogously to the traditional instrumentalist the causal skeptic denies that asymmetric causal notions play a role in scientific theorizing proper (even though the arguments might be quite different from that of a traditional instrumentalist). I will argue against the skeptic for a claim that corresponds to that shared by the constructive empiricist and the scientific realist—the claim that asymmetric causal notions play a role in theorizing in physics.[1] But I do not here want to take sides in the debate as to what metaphysical conclusions we should draw from the fact that asymmetric causal relations play a substantive role in physical theorizing. Theorizing in physics involves appeals to causal constraints, just as it involves positing quarks or electrons, but what metaphysical commitments follow from this is a question I do not want to address here.

Is there a universal causal constraint? Norton presents the causal fundamentalist with the following dilemma. Either causal assumptions place a universal factual constraint on the content of a science, but any candidate for such a constraint turns out to be false. Or causal assumptions place no restrictions on the content of a science, but then causal notions are nothing but an empty honorific. Norton's own solution to the dilemma is to suggest that while there is no universal causal principle, restricted domains can display a causal character, and that the causal principles at issue, even though they are imprecise and vague and have the character of a 'folk science', are derivable through reduction relations from more fundamental laws. Norton's view is one among a range of possible views. At one extreme

[1] Van Fraassen himself is of course not a constructive empiricist, but more of a traditional empiricist with respect to causal relations.

lies Russell's view, who denied that causal notion can play any legitimate role in physics; at the other extreme is the view that causal principles impose universal constraints. Somewhere in between is a view like Norton's, which allows for causal principles to play some role in physics but only in restricted domains, only as ultimately dispensible, crude labels, and only if the principles can be generated by reduction relations from underlying non-causal laws. Yet there is a whole range of alternative middle positions. One of them is the view that causal principles play a legitimate and important role in at least some fields of physics and that the scientific legitimacy of these principles is derived independently of the existence of reduction relations. Reduction relations for causal notions may or may not exist, according to this view, but their employment can be licensed independently of their existence.

2. Causality and Dispersion Relations

Dispersion relations are used to characterize scattering phenomena in many different branches of physics, ranging from the dispersion of light in classical electrodynamics to quantum field theory and *S*-matrix program in high energy physics. Analogous equations find an application in electric-circuit theory. The basic approach to deriving dispersion relations for the interaction between a field and a scattering medium is to consider the state of the field only long before and long after the interaction, abstracting from the details of the interaction and appealing only to certain very general assumptions characterizing the interaction. Instead of specifying a Lagrangian for the interaction between field and scatterer and deriving approximate predictions with the help of a perturbation expansions, one aims to derive exact predictions from what are assumed to be general physical principles. Interesting for our concerns here is the fact that certain of these principles are invariably characterized as causality conditions in the literature.

I want to focus here on the classical electromagnetic dispersion of light in a dielectric medium characterized by a complex dielectric constant ε. A dispersive medium is characterized by a frequency dependent dielectric constant ε. Dispersion relations relate the imaginary part of ε, which characterizes the absorptive properties of the medium, to the real part of ε, which characterizes its dispersive (i.e. frequency-shifting) properties.

The derivation of the dispersion relations makes use of the machinery of Fourier analysis. In our case, Fourier transforms relate temporal representations of the incoming and outgoing fields—the electric field $\mathbf{E}(t)$ and the electric displacement $\mathbf{D}(t)$, respectively—to their frequency representations $\mathbf{E}(\omega)$ and $\mathbf{D}(\omega)$. The Fourier transform of $\mathbf{E}(t)$ is

$$\mathbf{E}(\mathbf{x},t) = \frac{1}{\sqrt{2\pi}} \int_{-\infty}^{+\infty} \mathbf{E}(\mathbf{x},\omega) e^{-i\omega t} d\omega \tag{1}$$

with a corresponding equation for **D**. The Fourier transform represents a temporally finite wave train as a superposition of monochromatic waves $\mathbf{E}_\omega$ that extend from $t \to -\infty$ to $t \to +\infty$. Thus, transient real-life scattering phenomena are represented as stationary processes—as interactions between a field and a scatterer that exists from $t \to -\infty$ to $t \to +\infty$.

Here is a brief sketch of how the dispersion relations are derived.[2] We begin by assuming that the total output field $\mathbf{D}(\mathbf{x}, t)$ is a linear functional of the input field $\mathbf{E}(\mathbf{x}, t)$ and that the system is time translation invariant. That is:

$$\mathbf{D}(\mathbf{x},t) = \mathbf{E}(\mathbf{x},t) + \int_{-\infty}^{+\infty} G(\tau)\mathbf{E}(\mathbf{x},t-\tau)d\tau \tag{2}$$

The displacement $\mathbf{D}(\mathbf{x}, t)$ is equal to the incoming field **E** at $(\mathbf{x}, t)$ plus any output that results from the interaction between incoming field and the dielectric medium. A crucial feature of (2) is that it is non-local in time: the displacement **D** at t depends on the electric field **E** at all other times, both before and *after t*. Applying the convolution theorem for Fourier integrals, we can transform (2) into an expression relating the frequency-dependent Fourier transforms of $\mathbf{D}(\mathbf{x}, t)$, $\mathbf{E}(\mathbf{x}, t)$, and $G(t)$:

$$\mathbf{D}(\mathbf{x},\omega) = \varepsilon(\omega)\mathbf{E}(\mathbf{x},\omega). \tag{3}$$

Alternatively, one can begin by positing (3) as constitutive relation for $\mathbf{D}(\mathbf{x}, \omega)$ and then introduce $G(t)$ as the Fourier transform of the dielectric constant.

The next step in the derivation is to impose an additional constraint that is generally identified as a causality condition. The condition is, as John Toll ([1956]) puts it, 'no output can occur before the input.' More precisely, we demand that the output field at the time t is fully determined by the input field at all times prior to t:

$$\mathbf{D}(\mathbf{x},t) = \mathbf{E}(\mathbf{x},t) + \int_{0}^{+\infty} G(\tau)\mathbf{E}(\mathbf{x},t-\tau)d\tau \tag{4}$$

Comparing (3) and (4) and demanding that (4) hold for arbitrary input fields, we see that (4) requires that

$$G(\tau)=0 \qquad \text{for } \tau<0, \tag{5}$$

[2] My discussion follows mainly the derivations given in (Jackson [1975]). See also (Toll [1956]), and (Nussenzveig [1972]).

which ensures that there is no contribution to **D**(**x**, *t*) for $t<T$ from electric field values in the future of *T*. In particular, if an (otherwise arbitrary) input field **E** vanishes for times $t<T$, the same has to be true for the output field **D**. The dispersion relations relating the real and imaginary parts of the dielectric constant ε to each other follow as a mathematical theorem from the fact that the Fourier transform of ε, $G(\tau)$, vanishes for negative τ.

In fact, the following three statements imply one another:

i) Condition (5).

ii) The condition that the Fourier transform of *G*, i.e. ε, is an analytic function that is holomorphic in the upper half of the complex ω-plane (that is, for complex frequencies $\omega=u+iv$ with positive imaginary part *v*).

iii) The dispersion relations, which specify a relation between the real and imaginary parts of ε on the real axis.

This mathematical result is an equivalence statement, but the main use to which physicists put the result is to argue that there is a general physical condition, expressed by (i), which implies a constraint on the dispersive behavior of any medium. Moreover, physicists are univocal in maintaining that (i) is implied by, or expresses a causality condition. Thus, J. D. Jackson, anticipating his discussion of the dispersion relations later in his book, says that 'a priori, any connection between [the wave number] *k* and ω is allowed, although causality imposes some restrictions' by imposing restrictions on $\varepsilon=c^2k^2/\omega^2$ ([1975], p. 223). He stresses that (5) is 'in accord with our fundamental ideas of causality in physical phenomena' and says that (4) is 'the most general spatially local, linear, and causal relation that can be written between **D** and **E** in a uniform isotropic medium. Its validity transcends any specific model of ε(ω).' ([1975], p. 309) The dispersion relations 'are of very general validity, following from little more than the assumption of the causal connection [(4)] between the polarization and the electric field' (p. 311) They are 'extremely useful in all areas of physics. Their widespread application stems from the very small numbers of physically well-founded assumptions necessary for their derivation.' (p. 312)) Similarly, H. M. Nussenzveig says that the 'fundamental assumption' at the core of a treatment of dispersion relation is 'the *causality condition*' ([1972], p. 4, italics in original). He refers to causality as a 'general physical property' ([1972], p. 7) and says that he is interested in clarifying 'the relation between causality and the analytic properties' of the functions or operators characterizing the scattering interaction. (*Ibid.*)

Toll ([1956]) gives the following intuitive argument for why imposing the causality condition implies that there can be no pure selective absorber—that is, there can be no medium whose only effect on an incoming wave is to selectively absorb waves of a certain frequency—and that there has to be a constraint relating the frequency- or shape-shifting properties of a medium to its absorptive properties. Consider an incoming wave pulse **E** of a finite duration that is zero at all times before the time $t=0$. This wave can be decomposed into its Fourier components—a large number of sine and cosine waves—each of which extends from $t \to -\infty$ to $t \to +\infty$, but which destructively interfere for all times $t<0$. If a medium were selectively to absorb a component of a certain frequency $\mathbf{E}_\omega$ of the incoming wave *without* shifting the frequencies of the remaining components, then the output wave would simply be the complement of the absorbed wave, $\mathbf{E}-\mathbf{E}_\omega$, which is not zero for all times $t<0$. That is, the output wave would be non-zero even *before* the arrival of the incoming wave. The causality condition denies that this is possible. Hence the condition implies that selective absorption has to be accompanied by a shifting of the frequencies of the remaining components. Without imposing this condition no constraint on the relation between the real and imaginary parts of the dielectric constant can be derived.

Standard accounts of dispersion relations in the physics literature appear to be unanimous in stressing the following three points: first, the dispersion relations are derived from a general, time-asymmetric constraint; second, this constraint is physically well-founded and quite general; and third, the constraint is motivated by, or expresses a causal condition. While physicists usually do not spell out carefully what work considerations of causality do in supporting equations (4) or (5) above, I think one can reconstruct the causal reasoning behind the adoption of (5) somewhat more explicitly and more precisely as follows.

We begin with either equation (2) or (3) by positing a general mathematical relation between incoming and outgoing fields, assuming only linearity, time-translation invariance, and that $G(\tau)$ is square-integrable. This equation defines a class of models, which can be divided into subclasses corresponding to different choices for $G(\tau)$, the response function, which determines how much the incoming field at different times contributes to the outgoing field at t. We then interpret the relation between incoming and outgoing fields causally: the outgoing field $\mathbf{D}(\mathbf{x}, t)$ on the left-hand side of (3) is taken to be caused by the incoming fields on the right-hand side. That is, we take the causal relation to be a relation between values of variables (see, e.g., Woodward [2003]) and posit that the values of $\mathbf{D}(\mathbf{x},t)$ are caused by the values of the $\mathbf{E}(\mathbf{x},t-\tau)$ on the right-hand side. Formally, this corresponds to introducing an asymmetric relation *C*, which defines on the set of field variables a partial ordering that is transitive and non-circular. The result is a class of what we might call *potential causal models*. Potential causal models are generated by adopting a particular causal interpretation for the models of a set of dynamical laws. Up to this point, interpreting the relation between **D** and **E** causally may appear to be mere labeling: the physical content of the theory is captured by the dynamical laws and its models, it might seem, and calling certain variables ‘causes’ and others ‘effects’ does not add to the theory’s factual content.

But then we postulate as additional constraint on all *causally possible* models that an effect cannot temporally precede its causes. Given the causal interpretation of (3), this constraint can be implemented unambiguously and in a well-defined manner: for each ordered pair $\langle \mathbf{D}(\mathbf{x},t), \mathbf{E}(\mathbf{x},t') \rangle$ such that $\mathbf{E}(\mathbf{x},t')$ is a cause of $\mathbf{D}(\mathbf{x},t)$, it has to be the case that $t' > t$. The constraint is violated by all those potential causal models satisfying (3) for which $\mathbf{D}(\mathbf{x}, t)$ depends on $\mathbf{E}(\mathbf{x}, t')$ for some time $t' > t$. This additional constraint restricts the class of potential causal models to a subset comprised of those models that are causally possible and is satisfied exactly by those models of (3) that also satisfy (5). Finally, from the constraint that all causally possible models have to satisfy (5) we can derive the dispersion relations.

What reasons do physicists have for adopting a causal interpretation of the relation between **D** and **E**? It seems that an important guide to causal order is provided by how we can manipulate or intervene on the values of the physical quantities at issue. For example, results like the dispersion relations can be derived for electric circuits, such as an *RL*-circuit consisting of a resistance *R* and an inductance *L* in series. The two quantities of interest are the voltage across the series and the voltage taken across the resistor, but which is the input and which the output? Since there is no explicit spatial variation in the problem, we cannot define input and output in the same way as in the case of the electromagnetic field, where the input is the wave propagating toward the scatterer and the output is propagating away from the scatterer. But we may be able to draw a distinction by appealing to the notion of control: we can control the variation of the voltage across the series $V_{in}(t)$ and this results in a variation in the voltage across the resistor $V_{out}(t)$, but we cannot directly control $V_{out}(t)$ and thereby affect $V_{in}(t)$. In our case study, the same intervention asymmetry exists, even though the distinction between input and output fields can be drawn by other means as well. The asymmetry of control supports a causal interpretation of the relation between the two variables, as interventionist accounts of causation argue (see, e.g., Woodward [2003]). Roughly, and ignoring important qualifications, according to interventionist accounts, the variable *C* is a cause of the variable *E*, if and only if interventions on the value of *C* lead to changes in the value of *E*. That is, it is at the very core of an interventionist account that we intervene on or control a variable by manipulating variables causally upstream from the variable at issue.

3. Norton's scepticism

Norton has argued that when causal notions are used in physics, they are 'a crude and poorly grounded imitation of more developed sciences' ([2003], p. 2). They function as mere labels and are ultimately 'dispensible' ([2003], p. 8). How does the appeal to causal notions in the derivation of dispersion relations fare with respect to Norton's challenge? I want to work toward an answer to this question in stages. Let us begin with the standard derivation of the dispersion relations that I sketched above. At the core of the derivation is a purely mathematical result, which I have not presented explicitly: an analyticity condition—(ii) above—implies a constraint on the real and imaginary parts of the dielectric constant ε. While physicists emphasize the generality of this result, they also stress the importance of providing a physical underpinning for this mathematical relationship, which the causality condition (5) is meant to do. Thus, Cushing ([1990]) contrasts the classical dispersion relations, for which the analyticity condition can be given a secure physical foundation through the causality condition, from the quantum theoretical S-matrix program, in which many physicists, as he characterizes it, pragmatically adopted the relevant analyticity conditions, without however being able to provide a similarly convincing physical rationale for their adoption.

In the case of classical dispersion relations, the causal interpretation of the theory's dynamical models plays a crucial role in motivating the adoption of (5). There is no purely formal or mathematical reason for the constraint on $G(\tau)$ expressed in (5). But if we interpret the relation between incoming and outgoing fields causally, then (5) is the formal expression of the 'natural requirement that an electromagnetic fields, vanishing at the place of the atom for all time $t<0$ and beginning to act only thereafter, cannot cause the emission of scattered waves before the time $t=0$.' (Kronig [1946], quoted in Cushing [1990], p. 57). That is, it is precisely its causal interpretation which makes (5) 'physically well-founded,' as Jackson says.

Thus, purely within the context of dispersion theory the causal interpretation of (5) functions as more than an empty label. Moreover, as I have argued above, the causal interpretation of the relation between input and output fields is neither crude nor vague and can be formally implemented by defining a transitive, non-circular partial ordering over the class of field variables.

Next let us consider the dispersion relations within the context of the more general theory governing the behavior of light in dielectric media—macroscopic classical electrodynamics. One might think that the macroscopic Maxwell equations fully determine the evolution of any macroscopic electromagnetic system and that therefore the causality condition could not provide an additional constraint. However, the macroscopic Maxwell equations contain two electric fields, the electric field strength **E** as well as the electric displacement **D**, and in order to solve the equations we also need to be given (3), which is the constitutive relation for **D** and contains the dielectric constant ε.

Of course, once we have specified a particular model for the dielectric constant ε, the causality condition provides no additional constraint on that particular model. Rather the condition provides a general constraint on any physically legitimate model of ε and as such has content going beyond what is contained in any finite list of such models. We might begin with a list of particular physically plausible models for ε and derive for each individual case that the model satisfies the dispersion relations and condition (5). And we might then simply treat (5) as a 'shorthand' for some rather complicated physics embodied in each model for ε. Alternatively, we might be struck by the fact, first, that (5) allows us to unify different models of scattering interactions and, second, that a derivation of the dispersion relations that begins with (5) allows us to ignore the details of the medium in question and its detailed interaction with the field. We might then ask whether there is a common physical explanation for these facts and the causality condition offers just such an explanation: It is precisely because the causality condition constitutes a general constraint on all physically plausible models for ε that each such model satisfies the dispersion relations. This is appears to be how Nussenzveig sees the role of the causality condition. The general derivation of the dispersion relations has the advantage, according to him, that 'the nature of the scatterer need not be specified beyond assuming that some general physical properties, including causality, are satisfied.' (Nussenzveig [1972], p. 7) And he suggests that it is because the causal assumption is common to all models of scattering interactions that it explains or provides the 'physical reason' for the dispersion relations—an explanation, which tends to be obscured in derivations of the relations from any particular model for ε.

Recently there has been quite a bit of interest in the philosophy of science literature in the distinction between so-called 'principle theories' and 'constructive theories'—a distinction that is usually attributed to Albert Einstein, even though a closely analogous distinction was earlier drawn by Hendrik A. Lorentz, who distinguishes theories positing mechanisms from those beginning with general principles which, as he says, 'express generalized experiences.'[3] The advantage of appealing to such principles, according to Lorentz, is that they are versatile and apply to a wide variety of phenomena, since they abstract from and are independent of 'the inner constitution of bodies.' Two examples of general principles, which Lorentz cites, are the principle of energy conservation and the second law of thermodynamics. It seems to me that the causal principle in dispersion theory is another example of a Lorentzian general principle: arguably the condition that effects do not precede their causes is a condition generalized from experience and the condition is one of a small number of general assumptions from the conjunction of which dispersion relations for dielectric media can be derived without making any assumptions about the medium's 'inner constitution.'

For Lorentz, theories based on general principles and theories positing mechanisms both have their own distinct advantages and disadvantages. Both approaches are scientifically legitimate: the principle approach does not play only a secondary or derivative role, and its legitimacy does neither depend on, nor is it undermined by our being in possession of an underlying mechanism-theory. Some philosophers have argued that constructive or mechanism theories, when available, are always explanatorily superior to principle theories (see, e.g., Brown [2005]). I think this is a mistake, and it is mistake that is at least partly due to an overly narrow conception of the roles general principles can play in physics. The claim that principle theories are in general explanatorily inferior gains much of its plausibility from focusing exclusively on purely phenomenological principles, such as the second law of thermodynamics, which is often cited as paradigm example of a general principle. But there is an important second class of principles, as Lorentz's example of the principle of energy conservation shows, which function as general meta-constraints that more detailed theories or models of underlying mechanisms have to satisfy. And the way

[3] For a discussion of Lorentz's view, see (Frisch [2005b]).

physicists treat the causality condition in dispersion theory suggests that they view it as a general constraint of the latter kind.

Even if a principle is shown to be derivable from an underlying mechanism theory for a certain domain, it does not immediately follow that the principle is dispensable, since a principle-approach has its own explanatory advantages. For instance, showing that a certain phenomenon follows from general principles, independently of the details of a particular model or mechanism-theory, may make the phenomenon seem less arbitrary, than an account that invokes the details of a particular model may do. Also, general principles may permit exact derivations in circumstances in which derivations from corresponding mechanism theories may have to rely on approximation techniques, such as perturbation expansions. Indeed, as we have seen above, Nussenzveig maintains that it is precisely because of its generality that the causality condition constitutes the physical reason for the dispersion relations. And, as Cushing ([1990]) shows in his detailed examination of quantum mechanical dispersion theory and the S-matrix program, it was the promise of deriving exact predictions for quantum-mechanical scattering interactions from general physical principles without the need to make detailed assumptions about the nature of the scatterer that attracted physicists, many of which treated the approaches as complementary to a field-theoretic program.

I have argued that a Lorentzian explanatory pluralism, seems to accord well with how physicists have treated causality conditions and other general principles both in classical and in quantum mechanical dispersion theory. Independently, however, of what one's ultimate view is on the relation between general principles and underlying mechanism theories, the question as to what the relations between the general assumptions of dispersion theory and a 'constructive' micro-theory are is a question on the relation between two different properly scientific frameworks. Even if one unequivocally were to take the micro-theory to be explanatorily more fundamental, it does not follow that the general principles of dispersion theory are only part of a crude 'folk science' or of a poorly grounded imitation of a more developed science.

To what extent can the causality condition and the dispersion relations in classical electrodynamics relation be generated from the microscopic Maxwell-Lorentz theory through formal reduction relations? Unfortunately, it is not clear (at least to me) whether this question permits of a simple answer. The problem is that there seems to be no fully satisfactory and consistent treatment of the interaction between a discrete microscopic charge and the electromagnetic field that includes the so-called 'self-interaction'—the interaction of a charge with its own field.[4] Standard models of the dielectric constant include a damping term that accounts for the absorption of a the incoming field in the medium. From a fundamental perspective this damping term should be due to damped motions of the individual electrons in the medium and should be entirely accounted for by the fact that accelerated charges radiate energy. However, the most widely discussed equation of motion for a classical electron including radiation effects, the Lorentz-Dirac (L-D) equation, predicts that an electron starts to accelerate *before* the onset of an external force.[5] Thus, if we interpret the L-D equation causally, then the equation allows for backward causality. Correspondingly the equation results in a dispersion formula that *violates* the causality condition (see Nussenzveig [1972], sec. 1.9). Thus, there are reasons to doubt that dispersion theory with its causality condition can be rigorously derived from an underlying micro-theory. A further question is whether the reducing theory, microscopic electrodynamics, is itself a causal theory. I have argued elsewhere (Frisch [2005a]; [2006]) that it is, but there is no room to repeat the argument here.

Norton has argued that a defender of a role for causal notions in science faces the following dilemma. Either causal assumptions place a factual constraint on the content of a science. But, Norton maintains, any candidate for such a factual constraint turns out to be false. Or causal assumptions place no restrictions on the content of a science. But then causal notions are nothing but empty honorifics. I have argued that the causality condition does provide a factual constraint needed for the derivation of dispersion relations. But does our taking the condition to provide a physically well-founded constraint commit us to its truth? Norton argues that any non-empty principle of causality is false by presenting counterexamples from different domains of physics to each putative candidate for such a principle. Thus, he must assume that a non-empty causal principle can play a legitimate role in any scientific theory only if we have good reasons to believe in its universal applicability. I think that this assumption is much too strong. It might in fact be true that effects never precede their causes. But I think that 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. Causal assumptions—just as other scientific assumptions—can play a substantive and legitimate role in a certain domain—and their use can be physically well-founded in that domain—even if

[4] For discussions of the conceptual problems resulting from self-interaction effects see (Frisch[2004]; [2005a]; [2008]).

[5] The Lorentz-Dirac equation is discussed in detail in (Rohrlich [1990]). I discuss the conceptual problems of the equation and its backward causal behavior in (Frisch [2004]; [2005a]).

we do not take the assumptions to have universal scope and even if we in fact believe that the assumptions fail outside of their domain of application. It would not, then, undermine Jackson's claim that the assumption of forward causal dependence is physically well-founded in the context of scattering phenomena, if we were to discover backward causation in another domain.

Moreover, a defender of causal notions in science need not be committed to the view that if causal conditions place factual constraints on scientific theorizing, then they must function as prior metaphysical constraints on all theorizing. Like other scientific assumptions, causality conditions need not be posited a priori, nor need they be taken to be universal. Rather, different conditions function as scientific constraint of varying universality or generality. The time-asymmetric assumption that effects do not precede their causes may well have universal scope—tellingly, Norton's list of counterexamples does not include a counterexample to this principle—but other putatively causal constraints may have a more restricted scope. Thus, causal *locality* conditions may be physically well-founded and play a legitimate role in some theories, such as classical electrodynamics, even if we believe that this assumption is violated in Newtonian gravitational theory. We could not be committed to the *truth* of a universal locality principle, if we also believed—as we in fact do not—that Newton's law of gravitation is true. Yet we can accept that a causal locality principle plays a legitimate role in some mature sciences, even if we believe that no such principle is true in general. Thus, if it were indeed the case that there is no 'principle of causality' that does not fail in some domain or other, as Norton argues, then this might show that causal *fundamentalism* is false, but it does not follow that no causal principle can play a substantive role in a mature science.

But if we concede that a causal constraint might not be universally valid, how can we know that it applies to a certain kind of system? I take it that causal assumptions are justified in the very same manner in which any other assumptions we make in modeling the phenomena in a certain domain are justified. There might be various background beliefs and methodological assumptions that go into positing a certain principle, law, or equation, or go into adopting a certain interpretation of a formal framework, but ultimately we aim to test our assumptions by examining whether the empirical implications of the assumptions accord with experimental results. Thus, in our example, empirical evidence for the dispersion relations supports the assumptions that go into their derivation. The role played by causality conditions in theorizing in physics is no different in kind from that of other constraints: as Nussenzveig says, 'Dispersion relations were initially regarded as broad restrictions on physical theories (like the principle of conservation of energy). Comparing them with experiment would, hopefully, allow one to test the validity of the underlying basic assumptions, specially causality.' ([1972], p. 6)[6] Sometimes negative evidence suggests that an assumption ought to be abandoned, but sometimes such evidence merely serves to establish the validity limits or the domain of application of a theoretical assumption. For example, in our case the linearity assumption (3) is not universally true for all media. But this does not mean that it is always a mistake to make this assumption, or that this assumption is simply false. Many materials can be modeled by assuming that the response field generated in the medium is a linear function of the input and there appear to be scientifically and empirically legitimate reasons for adopting a linear model in these cases. Similarly, there are legitimate reasons for postulating a time-asymmetric causal constraint, even if this constraint, too, had only a limited domain of applicability.

I think it is an open question whether causal fundamentalism is true. In particular, it is an open question, whether there is a fundamental causal asymmetry that lines up with the temporal asymmetry, governing all phenomena. But we do not need to settle this question in order to accept that temporally asymmetric causal assumption play a legitimate and substantive role in modeling the phenomena in *at least* certain restricted domains of application.

4. Conclusion

It has frequently been claimed that causal assumptions play no role in theorizing in mature physics. In this paper I discussed what I take to be a counterexample to this view—the appeal to time-asymmetric causal principles in the derivation of dispersion relations, where, I argued, causal principles function as general factual constraints. More specifically I argued:

i) The time-asymmetric assumption from which the dispersion relations are derived depends crucially on a causal interpretation for its motivation.

ii) The causal interpretation of the relation between incoming and outgoing fields can be implemented formally precisely and is not inherently vague or crude.

iii) Physicists posit the causality condition as physically well-founded general constraint that has explanatory value.

Since many of my claims were developed through a critical discussion of views advocated by Norton, I want to end by once more stressing the important points of agreement

[6] It is not obvious to me what role the qualification 'initially' is meant to play in this quote. In subsequent sentences Nussenzveig describes what he characterizes as an even more ambitious program.

between Norton's views and my own. Both Norton and I believe that the question of the role of causal notions in physics is not one that can be settled by considering a priori metaphysics. Whether causal reasoning has a legitimate place in physical reasoning can only be decided by considering actual examples of putatively causal reasoning. Also, neither of us is a friend of causal fundamentalism—that is a commitment to the view that nature at its most fundamental level is governed by cause and effect. Norton believes that causal fundamentalism is false, while I think that a defense of the importance of causal notions in physics should not have to rely on this view and that there are instances of causal reasoning in physics that can be legitimated independently of the truth of causal fundamentalism. Finally, at least some of the things Norton says suggest that he and I even agree that causal reasoning can and does play a legitimate role in physics. Ultimately, then, our only disagreement may be over the question whether legitimate uses of causal notions necessarily need to be underwritten by reduction relations. Norton believes that the answer to this question is 'yes', while I have argued that causal principles can play a legitimate and fruitful role in physical theorizing independently of whether the principles are reducible to non-causal notions.

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