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# CAUSAL PLURALISM AND SCIENTIFIC KNOWLEDGE: AN UNDEREXPOSED PROBLEM[1]

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ABSTRACT

Causal pluralism is currently a hot topic in philosophy. However, the consequences of this view on causation for scientific knowledge and scientific methodology are heavily underexposed in the present debate. My aim in this paper is to argue that an epistemological-methodological point of view should be valued as a line of approach on its own and to demonstrate how epistemological- methodological causal pluralism differs in its scope from conceptual and metaphysical causal pluralism. Further, I defend epistemological-methodological causal pluralism and try to illustrate that scientific practice needs diverse causal concepts in diverse domains, and even diverse causal concepts within singular domains.

## 1. Introduction

Causal pluralism is currently a hot topic in philosophy. However, the consequences of this view on causation for scientific methodology and scientific knowledge are heavily underexposed in the present debate. The current literature in defence of causal pluralism seems to focus particularly on conceptual causal pluralism. Conceptual causal pluralists are convinced that our everyday notion of "causation" cannot be described univocally. No single current theory of causation seems

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sufficient to cover our notion of "cause" in all its diversity, which convinces conceptual causal pluralists that we will have to combine several causal theories in one way or another to get a grip on all these ways the notion of cause is used in our everyday causal talk. Another reason to break with conceptual causal monism is the finding that contextual factors influence the way we interpret the notion of "cause". Our interests, human limitations, and specific goals in asking for a cause, can affect the causal concept used and the causal selections made. Only a few authors overtly argue in one way or another that causal pluralism is not only a conceptual matter, but can also be defended on the basis of metaphysical considerations. The conviction is then that causation is also "plural" in reality, apart from how we perceive it and reason about it. One can, for example, argue that several empirical relations can be discerned which can all be labelled "causal", or that each level of organization has its specific causal characteristics which can only be captured by means of different approaches to causation in the world. However, one does not need to be metaphysical causal pluralist to be conceptual causal pluralist. One can for example argue that there is in fact only one kind of causal relation in the world - possibly at the fundamental organizational level - but that we need several causal concepts in everyday causal reasoning for pragmatic reasons. I am convinced one should discern epistemological- methodological causal pluralism as a still different approach, stating that we need different concepts of causation to gain scientific knowledge. Surprisingly, the latter kind of causal pluralism is currently hardly debated. If scientific examples are referred to in the debate on causal pluralism, they are used to defend a certain conceptual or metaphysical approach to causation, but hardly ever is it recognized that taking a conceptual and/or metaphysical pluralistic stance bears consequences for our scientific methodology and scientific causal knowledge, and that this in turn implies specific philosophical questions with respect to our scientific notion of causation.

My aim in this paper is to argue that an epistemological-methodological point of view (to which I will further refer by the contraction epimethodology) should be valued as a line of approach on its own, and to demonstrate how epimethodological causal pluralism differs in its scope from conceptual and metaphysical causal pluralism. In section 2, I discuss how an epimethodological approach is different from, but meanwhile related to, one's conceptual and metaphysical causal point of view. In section 3, I comment on Phil Dowe's argumentation in *Physical Causation* (Dowe, 2000) to make clear that different scientific domains lean on different approaches to causation in their attempt to gain appropriate causal knowledge, and to illustrate further that a metaphysical approach has to be distinguished from an epi-methodological approach to causation. Additionally, I strengthen my point by indicating the applicability of the totally different approach of J. L. Mackie (1974) to the engineering sciences. Section 4 demonstrates that different approaches to causation can be even useful within a single domain of science by referring to the biomedical sciences. Section 3 and 4 show the need of a pluralistic epimethodological approach. I further substantiate this claim in section 5, by arguing that we should subscribe to epimethodological causal pluralism if we aim at accurate and adequate causal conceptions to base our scientific methodology and knowledge on. Section 6 contains my final conclusions.

## 2. Epimethodological causal pluralism vis-à-vis conceptual and metaphysical causal pluralism

### 2.1. Differences

Philosophers working on the topic of causation often use scientific examples as similar to everyday examples, and hence to underpin the conceptual point of view defended. Although our notion of causation in science is not at all totally different from our everyday notion of causation, this treatment does not recognize that science has specific aims and interests which can affect the way we reason about causation within scientific practice. A scientist will, for example, often be looking for causal relations on which general *policy* decisions can be based, while in everyday causal reasoning, one will rather be looking for singular factors *responsible* for certain specific effects. This example shows why it is not justifiable to interpret an epimethodological approach to causation as completely similar to a conceptual one.

On the other hand, science is often interpreted as a unique point of reference for finding out what causation is in the world within the literature on causation. In other words, science is used as a means to develop a metaphysical approach to causation. Phil Dowe describes his approach in *Physical Causation* (Dowe, 2000) precisely as one that leans on the results of science to find out what the language-independent entity called "causation" is:

> [...] there are procedures for investigating such an entity, namely, the methods of science, which is in the business of investigating language-independent objects. Empirical philosophy can draw on the results of science, and so can investigate such concepts, in this case causation 'in the objects'. (Dowe, 2000:7)

He further argues that this "empirical analysis" is not a conceptual analysis of the way this term is used by scientists, but rather a conceptual analysis of the concept inherent in scientific theories. This way, Dowe hopes to get rid off false intuitions on what causation is. However, his approach is still problematic. First, science itself is not entirely free of (causal) intuitions. The weight of these intuitions is reduced to minimal portions in the acquisition of scientific knowledge, but science can nevertheless not do without a minimum of intuitions as Timothy Williamson (2004) argues. Further, there is no way to ensure ourselves that these "scientific" intuitions, or these intuitions on which science is based, are better than our everyday intuitions. All the more because the former are in line with the latter.

Secondly, the assumption that there is an objective, univocal concept of causation implicit in our scientific theories can be questioned, as is done by Daniel Steel[2]. Steel argues that any empirical analysis of causation will inevitably be a substantive thesis over and above what is given by the theories from physics or any other scientific discipline, since the term "cause" is never explicitly defined within these theories. Further, since the aggregate of scientific theories enables us to interpret "causation" in widely divergent ways, the choice for a single approach as the most adequate will always appeal to intuitions on the proper usage of the term "cause".

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[2] This is argued for by Daniel Steel in a draft version of a book chapter entitled "Causal Structures and Causal Mechanisms" which I received through personal communication and which is to appear in (Steel, forthcoming).

Additionally, it can be argued that causal knowledge in general is not perpective-free, as is extensively defended by Huw Price in (Price, 2001) and (Price, 2007):

> I don't want to eliminate causation altogether from science, but merely to put it in its proper place, as a category that we bring to the world — a projection of the deliberative standpoint. Causal reasoning needn't be bad science, on my view. On the contrary, it's often an indispensable construct for coping with the situation we find ourselves in, as enquirers and especially as agents. It is bad science to fail to appreciate these facts, but not bad science to continue to use causal notions, where appropriate, having done so. Some perspectives simply cannot be transcended. (Price, 2007: 290)

Huw Price does not want to claim that causality is ontologically subjective, in that the existence of causal relations would depend on the presence of an observator. In his pragmatist approach, Price disregards this problem of ontological realism, namely whether there are real causal relations in the world when there are no agents observing them. Price's pragmatic causal view states that causality is practice-subjective. Practice-subjectivity is no ontological matter but neither a psychological matter in that talk of causation would be talk purely about agents or agency, and not about the world. Price's practice-subjective pragmatism about causation is tied to the conviction that we cannot understand the notion of "causation" if we do not understand its origins in the lives and practice of the agents using this notion. According to this view, the concept of "cause" is essentially tied to the experience of agents. This implies that our notion of cause is developed from a specific perspective, which cannot be transcended. Hence, an adequate philosophical description of causality should refer to the role of the concept in the lives and practice of these agents. This does nonetheless not justify the claim that there would have been no causation in the world if there had not been any agents observing them, nor that our notion of "cause" cannot tell us anything about the world. To the contrary, it tells us a lot about the world, but it does this from a specific perspective which we cannot escape from.

In conclusion, the inevitable influence of scientific intuitions, the absence of an objective univocal concept inherent in our scientific theories, and the perspectivalism of (scientific) causal knowledge demonstrate that it is not justifiable to treat an epimethodological approach as similar to a metaphysical one.

**2.2. Relations**

Notwithstanding that an epimethodological approach to causation should be discerned from a conceptual and metaphysical one, such an approach should be compatible with the conceptual and metaphysical position taken toward causation. One should situate an epimethodological approach in between a conceptual and metaphysical approach. As has already been said, our scientific notion of causation lies in line with our everyday notion, but will on the other hand be affected by the specific goals, interests, etc. of the scientific domain which is involved. On the other hand, although scientists aim at a precise description of causal relations in the world, their views will not be perspective-free, will be determined by human restrictions, and built on their specific scientific interests and convictions. These features of scientist's causal view on their domain have not to be interpreted as a problem, but might just be necessary to be able to gain scientific knowledge at all, and hence to reach a description which is as precise as possible but meanwhile also useful in practice. Just like Huw Price argues with respect to perspectivalism:

> [...] unmasking the perspectival character of a concept does not lead to simple-minded antirealism — we may continue to use the concept, and even to affirm, in a variety of ways, the objectivity of the subject-matter concerned, despite our new understanding of what is involved (of where we 'stand') in doing so. Nevertheless, there is a tendency to think that perspectivity is incompatible with good science, in the sense that science always aims for the perspective-free standpoint, the view from nowhere. (Price, 2007:253)

I will focus here on the relations between an epimethodological and metaphysical approach to causation. Taking a certain metaphysical position will entail specific questions concerning scientific causal knowledge. These kinds of questions become even more significant if one takes a pluralistic metaphysical position toward causation. It are these kinds of questions which characterize this separate line of approach to causal pluralism.

Let me illustrate the specific character of an epimethodological approach by way of a biological example of James Woodward (2003):

> As an illustration, consider the lac operon model for E. coli due to Jacob and Monod, which was widely regarded as a seminal discovery in molecular genetics. When lactose is present in its environment, E. coli produces enzymes that metabolize it, but when lactose is absent, these enzymes are not produced. What determines whether these enzymes are produced? According to the model proposed by Jacob and Monod, there are three structural genes that code for the enzymes as well as an operator region that controls the access of RNA polymerase to the structural genes. In the absence of lactose, a regulatory gene is active which produces a repressor protein which binds to the operator for the structural genes, thus preventing transcription. In the presence of lactose, allolactose, an isomer formed from lactose, binds to the repressor, inactivating it and thereby preventing it from repressing the operator, so that transcription proceeds. Biologists describe this as a case of "negative control". Unlike "positive control," in which "an inducer interacts directly with the genome to switch transcription on" (Griffiths, Miller, Suzuki, Lewontin, and Gelbart 1996, p.550), the inducer in this case, allolactose, initiates transcription by interfering with the operation of an agent that prevents transcription. [...] A causal relationship is clearly present between the presence of allolactose and the production of the enzymes, and the former figures in the explanation of the latter, but there is no transfer of energy from, or spatiotemporally continuous process linking, the two. (Woodward, 2003:225-226)

Woodward in fact used this example to underpin his view that one should resist a proliferation of concepts of causation in favour of a monistic conceptual approach. Specifically, he was arguing against the distinction made by Ned Hall (2004) between causation as dependence (which can be grasped in terms of counterfactuals) and causation as production (which needs another than a counterfactual approach). According to Hall, some causes can be causes in, for example, the dependence sense, but not in the production sense. Allolactose in the example above would form an instance of such a cause. In contrast, Woodward defends a manipulationist account following which allolactose should straightforwardly be taken as a cause, without making any distinctions with regard to those inducers involved in what biologists refer to as "positive control". Biologists also seem to refer to both of these causes as just straightforward causes, and this convinces Woodward that one does not need Hall's distinction. I think Woodward is too fast in making this conclusion. In fact, Woodward's example conflicts with his own conclusion, since biologists do make a distinction. Although they refer to both kinds of influence of inducers as "causes", they introduce the labels "positive control" and "negative control" to discriminate between both. These labels clearly refer to two discernable ways of causing the transcription.

The answers to metaphysical questions will bear consequences for our notion of causation in the sciences and will lead to specific epimethodological questions. Suppose we accept that causality in the world consists of a single kind of empirical relation, but that we nonetheless find that a distinction between production causes and dependence causes is frequently used in biology. In that case we should be able to justify this distinction in biology and to relate it to a univocal metaphysical approach. Typical epimethodological questions that will follow are: Why do we need this distinction in scientific practice? How are they related to the univocal metaphysical account? Does one of both refer to real causation, and the other to a kind of quasi-causation[3]? Alternatively, are they both subconcepts of the univocal metaphysical concept? In that case, can all relevant causal relations within the biological domain be captured by way of these two subconcepts, or do we need to make further conceptual distinctions? Etc. Suppose, on the other hand, that we accept that the distinction between production and dependence refers to a real distinction between two kinds of empirical relations in the world. This conviction would, for example, lead to the following question: Are both kinds of causal relations present at all levels of organization and are they by consequence of equal importance for our causal knowledge of all domains of science?

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[3] The term quasi-causation is introduced by Phil Dowe to make a distinction between real causes according to his theory in terms of conserved quantities, and, on the other hand, omitters and preventers. (Dowe, 2000; Dowe, 2004)

All these questions form typical epimethodological questions, related to, but not equal to, metaphysical questions regarding causation. Given the importance of causality in science, it is important to be aware of the specific character of the epimethodological approach. Epimethodological questions become even of crucial importance if one takes a pluralist position toward causation. However, specific epimethodological questions are hardly tackled in the current debate on causal pluralism. I am nevertheless convinced that we should appreciate the epimethodological approach as important on its own within the debate on causal pluralism. Generally spoken, such an approach should investigate whether scientists reason in a pluralistic way in their search for knowledge; which convictions lie at the basis of pluralistic scientific causal reasoning; what the consequences are of causal pluralism for scientific methodology; whether one can find differences between different domains in the answers to these questions; and if so, what these differences are.

To concretize the concerns of the epimethodological approach to causal pluralism, I tackle in the following sections two general epimethodological questions of central importance, namely whether we need diverse concepts of causation in diverse scientific domains (section 3), and whether we need diverse concepts of causation within single scientific domains (section 4). The examples will make clear that a positive answer should be given to both of these questions.

## 3. Diverse concepts in diverse scientific domains

### 3.1. Conserved quantities in the physical sciences

In section 1, I indicated that Phil Dowe (2000) describes his approach to causation as one that leans on the results of science to find out what the language-independent entity called “causation” is. In this section, I will argue against the line of argumentation of Dowe (2000) to make clear that different scientific domains lean on different approaches to causation to gain appropriate causal knowledge, and to demonstrate that a metaphysical approach has to be distinguished from an epimethodological approach to causation.

In fulfilling his goal, Dowe rejects one by one all of the "*major theories of physical causation*" (Dowe, 2000:12), such as Lewis' counterfactual theory, Hume's regularity theory, Suppes' probabilistic account and Salmon's process theory. For example, in the introduction to the second chapter, Dowe announces:

> First, the Humean deterministic accounts are rejected on the grounds that science yields examples of indeterministic causation, and second the probabilistic accounts of causation, including Lewis's counterfactual probabilistic theory, are shown to fall to a well-directed example of chance-lowering causality. (Dowe, 2000:14)

In both cases, the examples used are mainly taken from the domain of physics. For Dowe even a single physical counterexample forms a reason to reject a whole theory:

> In particular, probabilistic theories, taken as aiming to provide an empirical account of singular causation, *fall to an important counterexample from subatomic physics*. (Dowe, 2000:40) (my italics)

Subsequently, transference accounts are rejected, mainly because they cannot cope with immanent causation, a kind of causation which Dowe wants to be able to deal with because it is necessary to understand certain physical processes such as a spaceship's inertia as a cause of its continuing motion:

> there are a number of difficulties with the transference account. These concern problems of the identity over time of the transferred quantities, and the direction of causation. Further, I argue that there is a kind of causation, immanent causation, or causation as persistence, which is neglected by the transference accounts. (Dowe, 2000:41)

Subsequently, Dowe proclaims Salmon's theory as "superior" to the others discussed, though still not adequate. The destructive criticism on the alternative approaches paves the way for the defence of Dowe's own process theory of causation. In view of the foregoing chapters, it is clear that Dowe will conceive his own theory as the single appropriate theory of "physical causation". In a nutshell, this theory comes down to the following:

> 1. A *causal interaction* is an intersection of world lines which involves exchange of a conserved quantity.
>
> 2. A *causal process* is a world line of an object which possesses a conserved quantity.
>
> A "conserved quantity" is any quantity which is universally conserved, and current scientific theory is our best guide as to what these are. Thus, we have good reason to believe that mass-energy, linear momentum, and charge are conserved quantities. (Dowe, 1995:323)

Now, what is wrong with this approach of Dowe? My criticism here does not concern the content of Dowe's theory and the "internal" relation with alternative ones. What I think of are criticisms about the way he presents his own theory and opposes the others. First of all: Why basing his approach almost exclusively on examples from physics? What is precisely meant by "physical causation"? The term is simply thrown into the arena without further clarification. Nowhere in his book, nor in his articles, Dowe makes explicitly clear which domain(s) of application he is talking about. His reference to all other theories of causation as theories of "physical causation" gives the impression that the word "physical" has to be interpreted very broadly since these alternative theories are meant to be broadly applicable. However, when taking a closer look at Dowe's argumentation, his description of a conserved quantity and the examples used in his book, it appears to concern a theory of physical causation in a rather narrow sense. It is in fact quite clear that his theory is not at all apt to get a grip on, for example, the concept of cause in the social and behavioral sciences. The interests of researchers in those domains of science are not in line with a physical approach to causation focussing on the conservation of momentum, energy, etc. This is not the kind of knowledge which social scientists search for and use in their explanations. For example, how applying Dowe's theory to the way scientists explain the fluctuation of quotations on the stock exchange as an effect of political or social incidents, or the mental condition of people as an effect of life events? Dowe's theory is clearly not able to give an adequate description of the way scientists come to these kinds of causal explanations, while other approaches to causation are better apt to get a grip on the kind of causal reasoning involved when investigating such kinds of events.

From the book (Dowe, 2000) and his 1992 article (Dowe, 1992) it becomes eventually clear that Dowe is reasoning from a strong reductionistic point of view combined with some kind of supervenience account of causes on conserved quantities:

> Another possible criticism concerns reduction. Fair's approach involves a commitment to reduction of all science to physics, which some may not like. Salmon avoids this with his vague notions of "structure" and "characteristic". These can apply to any area of science, whereas energy and momentum may not. The present suggestion does not share this advantage. One answer is that the generality of "conserved quantity" might allow this to be used as a testable conjecture in various fields of science. But it is unlikely that it would stand the test: conservation laws seem to be confined to the physical sciences. A more desirable option is to take a middle road and adopt a supervenience account such as that of Kim (1984) where causes supervene on conserved quantities (see Menzies 1988). (Dowe, 1992:214-215)

However, this view is not explicitly referred to as the basis for his line of argumentation, and neither is it thoroughly motivated. His whole theory is nonetheless strongly connected to this reductionistic presupposition and the presupposition itself is not at all self-evident. Not everyone will easily accept this reductionistic causal point of view on the world.[4] And even if Dowe's reductionistic point of view would be justified as a metaphysical approach, we are clearly not able, and probably even not interested, in studying causal processes at all organizational levels in terms of causal processes on the elementary organizational level, which would not make a difference for Dowe's approach given his metaphysical aims, but would be an important fact for an epimethodological approach.

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[4] It has, for example, been criticized by Nancy Cartwright (1999) and by Stephen Webster (2003).

Hence, although Dowe claims that his approach is founded on the causal concepts inherent in scientific theories, his choice for physics is clearly based on a further metaphysical presupposition in favor of physics, which is crucial for his point of view but nonetheless not defendable on the basis of "the results of science" on which Dowe nonetheless claims to found his whole approach. Whether this metaphysical presupposition is justified, and whether the resulting metaphysical approach to causation is justified, is not the concern of an epimethodological approach to causation. What will concern such an approach is that the choice for another scientific domain than physics would have resulted in a totally different approach to causation. The causal concepts inherent in other than physical scientific theories will clearly not all be of the kind on which Dowe's approach is based.

Seen from an epimethodological point of view, and provided that one takes a pluralistic stance, there is no problem at all in the inadequacy of Dowe's process theory of causation for the characterization of causal processes in other domains such as the social sciences. From such a point of view, one can appreciate Dowe's approach as one that characterizes the concept "cause" prominent in the physical sciences. As Dowe in fact admits himself, the same characterization will not be applicable in any area of science. Apart from causal reasoning in the social sciences, conserved quantities do neither seem to play a prominent role in causal reasoning in, for example, the domain of engineering or biomedicine, which will be focussed on in the following two sections.

### 3.2. Sufficient and necessary conditions in the engineering sciences

Let me start with trying to find out which kind of approach to causation is prominent in causal reasoning in the engineering sciences.[5] Engineers have to think in terms of function. The physical structure of the things they design, should be a means to produce a certain effect Y. Hence, building an X has the function to produce Y. In other words, whenever a physical structure enables X to take place, Y should occur. Put still differently, if X is a good tool to produce Y, then Y will occur under the usual ceteris paribus condition that no disturbances occur. However,

[5] My analysis here is based on (Kroes, 1998) and (Kroes, 2003).

different physical structures can form a useful tool to perform the same function, leading to the same effect. Common to all these structures will be that they are designed with knowledge of some physical phenomena and with the purpose to take certain actions that lead to the desired effect. Hence, what engineers need to think of are designs of physical structures which have, within the normal background conditions, the necessary and sufficient characteristics to function in the production of a specific effect. I take an example from Peter Kroes (in a detailed way presented in (Kroes, 1998)), namely the Newcomen engines, which are one of the earliest types of steam engines. Those engines were used for pumping water, which is their function Y. An explanation of how the fulfillment of this function was brought about by the machine, will have to refer to certain physical phenomena (transforming water into steam increases its volume manyfold, cooling of steam in a closed vessel creates a vacuum, etc.), the design of the engineer (the steam engine consists of certain parts such as a cylinder, piston, great beam, boiler, etc.; the piston may move up and down in the cylinder, etc.), and a series of actions (after opening the steam valve the cylinder fills with steam and the piston moves up, closing of the steam valve and injection of cold water creates a vacuum in the cylinder; etc.) (Kroes, 1998:8). All these elements together explain how the function is fulfilled by the machine in normal circumstances.

This example indicates that the primary concept of causation within the engineering sciences can probably best be captured by John L. Mackie's approach to causation (Mackie, 1974). This approach is based on the approach of John Stuart Mill (Mill, 1973). Mackie takes Mill's "plurality of causes" as the basis of his own approach:

> It is not true that one effect must be connected with only one cause, or assemblage of conditions; that each phenomenon can be produced only in one way. There are often several independent modes in which the same phenomenon could have originated. One fact may be the consequent in several invariable sequences; it may follow, with equal uniformity, any one of several antecedents, or collections of antecedents. (Mill, 1973: Book III, Chapter 10, Section 1; cited in Mackie, 1974:61)

This is formally expressed by Mackie as " 'All (ABC or DGH or JKL) are followed by P' and 'All P are preceded by (ABC or DGH or JKL)' "

(Mackie, 1974:62). He ascribes the following characteristics to the elements of this definition:

> the complex formula '(ABC or DGH or JKL)' represents a condition which is both necessary and sufficient for P: each conjunction, such as 'ABC', represents a condition which is sufficient but not necessary for P. Besides, ABC is a *minimal* sufficient condition: none of its conjuncts is redundant: no part of it, such as AB, is itself sufficient for P. But each single factor, such as A, is neither a necessary nor a sufficient condition for P. Yet it is clearly related to P in an important way: it is an *insufficient* but *non-redundant* part of an *unnecessary* but *sufficient* condition: it will be convenient to call this (using the first letters of the italicized words) an *inus* condition. (Mackie, 1974:62)

The single factors of the disjunction of conjunctions can further also be negative conditions. $\bar{A}$ is then the absence of a counteracting cause A. Mackie recognizes these causes as real causes, contrary to Mill. Further Mackie also introduces the importance of a causal field (F), which forms the background of the causal event, but does not make part of it. An explosion in a block of flats will, for example, be attributed to a gas leak, while other factors such as the presence of the building and its gas pipes, and of people living in that building and lighting cigarettes from time to time, will rather be interpreted as the "normal" background situation within which the causal event arose.

Mill's description of "causation" is reflected in the notion of cause prominent in the engineering sciences, as described above. Note the following properties: its focus on deterministic causation, the incorporated idea of the importance of the plurality of single factors leading all together to a certain effect, and the plurality of different possible conjunctions of conditions all sufficient to lead to the effect, the inclusion of the absence of certain interfering elements as genuine causes and the importance of the causal field as the background within which the causal event arises. All these characteristics describe a notion of "cause" which lies much closer to the way one reasons about causation in the engineering sciences than, for example, in the physical sciences or maybe even clearer, in the biomedical sciences.

## 4. Diverse concepts within a single scientific domain: average effects in the biomedical and social sciences

While the interests of physical scientists lie in detailed descriptions of the causal processes and causal interactions leading up to some particular effect, social and biomedical scientists are interested in general overall causal patterns recurring in the population. These causal patterns are not necessarily exceptionless, as is presupposed in the case of the engineering sciences. The precise causal history leading up to one particular effect in singular cases is often very complicated and intractable and not interesting for the general purposes of the biomedical sciences. This led e.g. John Dupré (1993) and Ronald N. Giere (1997) to the following view with respect to probabilistic theories of causation:

> [...] causes should be assessed in terms of average effect not only across different causal routes, but also across varying causal contexts. (Dupré, 1993:199)

and hence:

> One interesting fact about these models is that it could turn out that **C** is causally irrelevant for **E** in the *population* U even though **C** is *not* causally irrelevant for **E** in all *individuals* in U. [...] Population models always average over individuals and, therefore, ignore what might be important differences among individuals. (Giere, 1997:204-205).

It is argued one should execute controlled experiments on fair samples to find statistically significant differences between experimental and control groups. These differences are claimed to be good standards for causal judgments about populations. Dupré opposes the rival view of e.g. Nancy Cartwright (1979), Paul Humphreys (1989) and Ellery Eells (1991). They hold on to the context unanimity condition. This condition maintains that:

> a genuine cause must raise the probability of a genuine effect of it **in *every* causal background context**. (Eells, 1991:94) (my bold)

More specifically, this would come down to the following:

> To use an example of Cartwright's (1979), ingesting an acid poison (X) is causally positive for death (Y) when no alkali poison has been ingested (~F), but when an alkali poison has been ingested (F), the ingestion of an acid poison is causally negative for death. I will argue that in a case like this it is best to deny that X is a positive causal factor for Y, even if, overall (for the population as a whole), the probability of death when an acid poison has been ingested is greater than the probability of death when no acid poison has been ingested (that is, even if $Pr(Y|X) > Pr(Y|{\sim}X)$. I will argue that it is best in this case to say that X is causally *mixed* for Y, and despite the *overall* or *average* probability increase, X is nevertheless not a positive causal factor for y in the population as a whole. (Eells, 1991:94)

As John Dupré (1993) argues, this kind of causal knowledge is not of interest in social and biomedical scientific practice. What he nonetheless does not recognize is that the context unanimity approach would form a more adequate approach than the average effect approach for finding out what the real causal structure of the world consists in. Dupré fails to discriminate here between a metaphysical and an epimethodological approach. He takes his own approach, which is clearly an epimethodological one, as a metaphysical approach. His own approach should then replace the unanimity approach, which he doesn't want to appreciate as a metaphysical one because of its impractibility:

> We should avoid metaphysical doctrines for which we neither have, nor possibly could have, empirical evidence of applicability. This is a methodological principle that the unanimity thesis fails dismally to satisfy. (Dupré, 1993:201)

Although the unanimity approach may not be practicable, it would form an adequate toolbox for metaphysical purposes. It would however not be adequate for the main interests of social and biomedical sciences, where the context unanimity condition is of little or no practical use. What the approach does, is demonstrating the limitations and presuppositions of our research methods. This seems precisely what a metaphysical approach should do. Being of practical use is to the contrary something one should expect from an epimethodological approach to causation.

Dupré (1993) convincingly argues that what one needs in the practical context of the biomedical and social sciences is an average effect approach rather than a context unanimity approach to causation. If we take a closer look at scientific practice within specifically the biomedical sciences, one will nevertheless rapidly be convinced of the importance of the search for causal mechanisms, next to the search for probabilistic evidence. Causal mechanisms are necessary for further confirming and explaining the causal relations derived on the basis of correlations.

This is clearly argued for by, among others, Paul Thagard in *How scientists explain disease* (Thagard, 1999). Thagard argues that knowledge of mechanisms is not strictly necessary, but nonetheless often searched for by medical researchers, in line with everyday causal reasoning:

> Whereas causal attribution based on correlation (covariation) alone would ignore mechanisms connecting cause and effects, ordinary people are like medical researchers in that they seek mechanisms that connect cause and effect. [...] Reasoning about mechanisms can contribute to causal inference, but it is not necessary for such inference. In domains in which causal knowledge is rich, there is a kind of feedback loop in which more knowledge about causes leads to more knowledge about mechanisms, which leads to more knowledge about causes. But in less well-understood domains, correlations and the consideration of alternative causes can get causal knowledge started in the absence of much comprehension of mechanisms. (Thagard, 1999:109)

Thagard (1999) and Ahn & Kalish (2000) indicate three ways in which causal mechanisms can contribute to our search for causal explanations. First, they can confirm the existence of possible causal relations supposed to be present on the basis of a correlation. Hence a mechanism can help as a confirmative tool. Secondly, when a causal connection between two variables is supposed on the basis of statistical information, but no plausible mechanism can be found that elucidates this connection, we may be confronted with a spurious or a coincidental relation[6]. In this case the mechanism works as a disconfirming tool with respect to statistical evidence. Thirdly, a hypothetical mechanism points to possible causes that can be (dis)confirmed by other indicators. In this case, mechanisms work as a heuristic tool for the generation of hypotheses which are then further testable on the basis of, for example, statistical information. As Thagard points out, these characteristics constitute three ways in which mechanisms can enhance the explanatory coherence of causal explanations.

According to Thagard's analyses, biomedical scientific practice is based on at least two different causal approaches: a probabilistic approach in terms of average effects, and an approach in terms of causal mechanisms[7]. Since the idea of a causal mechanism encloses precisely the conviction that there is a process at an *underlying* organizational level connecting cause and effect, causal mechanisms will clearly not be of any practical use when trying to get a grip on the causal relations at the elementary organizational level[8], with which Dowe is concerned. Consequently, this example does not only illustrate that one will (often) need diverse concepts of causation to successfully gain causal knowledge within a single scientific domain, but adds as well to my conviction that one will need diverse causal concepts in diverse scientific domains.

## 5. Methodology for the epimethodological causal pluralist

The examples discussed in the previous sections demonstrate that one needs a pluralistic epimethodological approach to causation. Scientific practice cannot do with a single, univocal approach to causation.

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[6] The usefulness of this method is nevertheless denied for the social sciences by Daniel Steel in (Steel 2004), by means of the argument that one can very easily imagine a plausible mechanism connecting nearly any two variables representing aspects of social phenomena.

[7] A defence of the need for causal pluralism within the social sciences can be found in Weber (2007).

[8] See, for example, Glennan (1996).

Although this is not yet a generally accepted idea, it seems not that strange. Let me refer to the words of G. L. Newsome to illustrate this:

> Theorists might integrate (these) approaches by assuming (1) that cognizers' conceptions of causality often vary as a function of their existing world knowledge, and (2) that the role played by different aspects of these conceptions may vary as a function of the situation in which the causal judgment is made. (Newsome, 2003:100)

This statement forms part of Newsome's plea to integrate a covariation approach with a mechanism approach to causation, but the basic idea can be generalized to all approaches to causation.

As is clear from the examples above, one will be confronted with (a) certain specific conception(s) of causality once one is focussing on a specific domain of science. This conception will depend on the specific kind of causal knowledge — related to the typical organizational level of reality — researchers in the involved domain are working with. Further, the type of research involved and/or the scientific (sub)domain itself form a typical situation or context in which the causal judgments are made — this includes a typical kind of interest one starts from in the investigation.

To make this clearer, I introduce the distinction between accuracy and adequacy, in line with Philip Kither's view on scientific knowledge (Kitcher, 2001). In his book, Philip Kitcher compares "making science" in general with the making of maps in that what is accurate information on a map is dependent on the needs and desires of the users, and hence does not need to be in full correspondence with reality:

> Like maps, scientific theories and hypotheses must be true or accurate (or, at least, approximately true or roughly accurate) to be good. But there is more to goodness in both instances. Beyond the necessary condition is a requirement of significance that cannot be understood in terms of some projected ideal - completed science, a Theory of Everything, or an ideal atlas. (Kitcher, 2001:61)

Translated to our framework, accuracy refers to the ability of a causal theory to describe the way scientists achieve justified knowledge of the causal relations in the world; adequacy refers to the ability of a causal theory to describe the way scientists achieve the kind of causal knowledge requested in the involved situation. The latter is related to the way they look at the (scientific) information they have, the point of view from which they analyze the situation, the aim in questioning for the cause, the domain of science they are working in, ... A theory of causation adequately describes causal reasoning in a certain context if it shows us how scientists achieve the kind of information they are interested in, that is, which is useful, relevant, and accessible within the context they are reasoning from. Further, as Kitcher demonstrates, what is believed to be an accurate description (of "causation") is in turn dependent on what is believed to be an adequate description (of "causation") in the involved context:

> What counts as an omission or an inaccurate spatial representation depends on the conventions associated with the kinds of maps, and, in their turn, those conventions are in place because of the needs of the potential users. (Kitcher, 2001: 56)

Let me illustrate this by means of Kitcher's example, namely the map of the London Underground. Since this map enables ten thousands of people a day to successfully find their way through the underground network, it clearly provides them with an accurate description of this network. The map is nonetheless not meticulously describing the real situation of the network in that it does, for example, present all underground lines as straight lines, and denies the real distance between the underground stations in its representation of the situation. As travellers we nevertheless perceive the map as accurate, since it is accurate in view of its goal of informing us on how to travel from one underground station to another. In view of this goal, it should provide certain information and neglect other information, which precisely makes it an adequate map for users of the London Underground. The same map would however be totally inadequate for a building contractor planning public works in the London Underground, since for use in that context, it would be far too inaccurate with respect to, amongst others, the precise length of the underground lines and the precise position of the underground stations. This demonstrates that what is interpreted as an accurate description depends on what is perceived as adequate in the context.

When returning to the probabilistic versus the mechanical approach in the biomedical sciences, there seems no objective reason to prefer one analysis above the other, nor to demand that each accepted causal relation should be confirmed by both approaches. In fact, it will be the situation within which we need to decide on the acceptance of a causal relation which will be decisive for the approach to causation to prefer. As Thagard suggested, in a phase of the survey in which knowledge of specific mechanisms is lacking, statistical information can be taken as a sufficient proof for a causal relation, while it is on the other hand possible that in other situations one will require an explaining mechanism before accepting a certain causal relation. Similarly, if we really want to understand the specific process leading to a certain effect, we may focus on causal mechanisms, while focussing on probabilistic evidence if we are mainly searching for general tendencies. As Hitchcock (2003) claimed:

> When we are asked what causes what, we may pay attention to one of these relations in one scenario, to another of these relations in a different scenario. One of these relations may be a component of one philosophically significant concept, while another is a component of another. All of these relations are causal, in a broad sense, and worthy objects of study within a theory of causation. (Hitchcock, 2003:8)

Hence whether a certain causal theory is adequate for our purposes, depends on the context within which we are reasoning and the particular interests connected with it. Striving for a causal theory adequate for use in all or a lot of divergent possible contexts in which causal reasoning occurs, will be at the expense of the required accuracy since no single theory seems able to capture our notion of "causality" in all its diversity in an accurate way. Hence, it is the situation in which the causal judgment is made that determines which approach we will choose as accurate and adequate for our purposes, and consequently, which factor will be labelled as the cause.

The question that may follow is whether this does not inevitably result in a very particularistic approach? Do we need to analyze the usefulness of a certain approach to causation for each causal reasoning process on its own? Of course this forms no solution. Consequently, the problem is to find a feasible approach that stands midway between a too monolithic and a too particularistic one.

Given that the requirement of accuracy and adequacy demands for a pluralistic approach to causation, my proposal is precisely to start thinking from domains of application when developing an epimethodological approach to causation. With this I come back to my conclusions from the plea of Newsome. To sum up, if we reason from a certain fixed (sub)domain of science, we are confronted with a fixed kind of knowledge and organizational level of the world researchers are reasoning from and with a fixed kind of research situation and some typical research interests. The former seem to ask for a proper form of accuracy, the latter seem to ask for adequacy, such that the demanded accuracy is attainable after all. Hence reasoning from specific (sub)domains offers a tool to fix the way in which one can reason accurately and adequately and hence justifies generalization over the concerned (sub)domain. Focussing on domains will give us the possibility to generalize over a broad area of application. The unity in the way of reasoning about causal relations within one such domain will justify these generalizations.

Remark that we have to do with a continuum ranging from generalizing over several contiguous domains of science (e.g. the social and behavioral sciences, the natural sciences) to specifying into particular subdomains (e.g. medical etiology). The more we specify, the better our characterization of the involved causal concept(s) will do and the easier it will be to increase the accuracy of our causal theories, but the more our characterizations can become fragmented. However, working this way will offer us anyhow some theories of causation that are much closer to the scientific reality than the ones who aim at an overall characterization of the concept of causation inherent in scientific theories.

## 6. Conclusion

I tried to convince the reader that one should discriminate between three approaches to causal pluralism: conceptual causal pluralism, metaphysical causal pluralism, and epimethodological causal pluralism. I have further tried to demonstrate what kinds of questions can be answered if one takes an epimethodological approach as an approach of importance on its own. This led me to the defence of epimethodological causal pluralism, by demonstrating that science needs diverse causal concepts in diverse scientific domains, and even diverse causal concepts within singular domains. A lot of work is to be done yet to substantiate epimethodological causal pluralism by further investigating its characteristic research questions in relation to specific scientific domains. The resulting pluralistic approach to causation will anyhow be closer to scientific reality, and will not force one to adopt certain metaphysical positions which cannot be (directly) derived from it.

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