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Epistemic authority: a pragmatic approach.

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Abstract

In this paper, I investigate how we can legitimate that certain regularities get *epistemic authority* in specific contexts of scientific practice. With "epistemic authority" I refer to the fact that regularities are trusted to achieve epistemic goals like prediction, explanation and manipulation. For my analysis, I use the Neuber rule, a regularity used to model creep in notches, as an exemplar. I distinguish two traditional ways of legitimating epistemic authority: a necessitarian approach and an epistemic mark approach. Throughout the paper, I argue that neither is, in its current form, sufficient to account for the epistemic authority of regularities like the Neuber rule. Regarding necessity, I expand arguments from Matthias Frisch's work in philosophy of physics to show that (1) the Neuber rule is currently not successfully derived from (more) fundamental laws, (2) the idea that there are truly fundamental laws that can be used to represent any phenomenon is not unproblematic given the functioning of scientific practice, and (3) even if there are such fundamental laws, there is no guarantee that their necessity is undamaged by the modelling practices of science. I then present an alternative that resembles the basic idea behind the epistemic mark approach, but is significantly more informative. For this part, I build on insights from Sandra Mitchell's work in philosophy of biology. This results in a pragmatic approach to epistemic authority. At the same time, this paper functions as a defence and expansion of both Frisch and Mitchell's work. I also emphasize the benefits of combining insights from various philosophical disciplines.

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Epistemic authority: a pragmatic approach.

1. Introduction

Consider the following question:

> Why does this metal rod lengthen when heated?

According to physics, this is due to the laws of thermal expansion. When a question can be answered by invoking "a law of nature", in many contexts, there can be no discussion. Laws of nature are seen as irrefutable, as the way things are. Laws are also considered special: not all regularities are laws. Bas Van Fraassen presented a much-cited example referring to the size of spheres:

> All gold spheres are less than a mile in diameter.
> All uranium spheres are less than a mile in diameter. (1989, p.27)

The first universal sentence is not considered to be a law, the latter one is. This distinction is thought to be important, because laws are seen as capable of performing functions that accidental generalisation are not. This is motivated by the observation that, throughout the sciences, laws are given what I will call *epistemic authority*: they are trusted as guides for epistemic activities such as prediction, explanation and manipulation. Consider the example of the metal rod: according to physics, it lengthens when heated because of the laws of thermal expansion. Another intuitive example is given by Van Fraassen:

> The moon orbits the earth. Why does it do so? What reason can there be for expecting it to continue to do so? (1989, p.183)

The reason for both is the law of gravitation (ibid, p.32). Many philosophers (though not Van Fraassen himself), think that it is precisely *laws* that allows us to explain e.g. why the moon orbits the earth and makes us successful in our prediction that it will do the same tomorrow.

Especially explanation has often been connected to laws. Cartwright discusses this via the distinction between phenomenological and theoretical laws (1983, p.1). Phenomenological laws describe what happens. But theoretical laws explain: they are not merely about appearances, but about the reality behind them (1983, p.1).[1] The idea that there is a special connection between laws and explanation has also influenced philosophical analysis of scientific explanation. Think for instance of the covering-law approaches, such as Hempel's deductive-nomological model (or DN-model). According to the DN-model, an explanation "consists of an explanandum E (a description of the phenomenon to be explained) and an explanans (the statements that do the explaining)" (Weber et al. 2013, p.2). In this model, a potential explanans is characterized as follows:

> (DN) The ordered couple (L, C) constitutes a potential explanans for the singular sentence E if and only if
> (1) L is a purely universal sentence and C is a singular sentence,
> (2) E is deductively derivable from the conjunction L&C, and
> (3) E is not deductively derivable from C alone.
>
> (ibid.)

In the formulation above, 'L' refers to a law. By deductively subsuming the phenomenon to be explained under a law, an explanation is given. As Cartwright argues, this is not limited to

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[1] Cartwright herself does not agree and argues that it is the causes that explain, not the laws.

Hempel's account. Patrick Suppes's probabilistic model of causation, Wesley Salmon's statistical relevance model, and even Bengt Hanson's contextualistic model, all rely on the laws of nature (Cartwright 1983, pp.44-45). So for many philosophers, if we can show that a phenomenon results from a law, it is explained. Laws and explanation are intimately connected.

A similar story can be told for other epistemic goals, such as manipulation and prediction. In the quote above for example, Van Fraassen asked what reason we have to expect that the moon will orbit the earth again tomorrow. The answer was "the law of gravitation". This reason behind the uniformity in nature ensures that we can make predictions regarding the behaviour of the moon:

> That there is a law of gravity is the reason why the moon continues to circle the earth. [...]and if we deny there is such a reason, then we can also have no reason for making that prediction. We shall have no reason to expect the phenomenon to continue, and so be in no position to predict. (Van Fraassen 1989, p.32)

So according to many philosophical views, we can a.o. explain and predict phenomena in a successful and warranted way by referring to laws. Laws ensure that our epistemic undertakings are trustworthy. This is what I mean when I say that laws receive epistemic authority in scientific practice and philosophical analysis. This is not a novel point. Nelson Goodman already said it wasn't a novel point back in the 80's (1983, pp.20-21). Laws are used predictively. Yet it is not clear why exactly laws are granted (and should be granted) this epistemic authority, contra to accidental generalisations. Philosophers have attempted to legitimate this in several ways, often by defining what makes a law a law. I will discuss two strategies: necessity and epistemic authority. I start with necessity.

2. Legitimating epistemic authority

2.1. Necessity

A first traditional way of distinguishing between laws and accidental generalisations has to do with modal power: something in nature is thought to necessitate the truth of laws, while this is not the case with other regularities (Carroll 2016, p.1). This is often expressed in terms of necessity (Van Fraassen 1989, p.28) and connected to the ability of laws (contra to accidental generalisations) to support counterfactuals (Psillos 2002, pp.145-146). The main idea behind this way of legitimating the epistemic authority of laws, is that what follows from the laws is, in some sense, necessary.

There has been a lot of debate regarding how to understand this necessity[2], yet the specific strength does not really matter here[3]. So I will adopt the weakest form of necessity as described by Van Fraassen: the intensionality of the laws. Intensionality of laws expresses that if "it is a law that A" is true, then "A" is necessarily also true[4] (Van Fraassen 1989, p.29). So even philosophers who admit that laws can be contingent generalisations, often still see them as necessitating the truth of their consequences. Note that this is not an epistemological criterion, but a metaphysical one: it is not what we know of the laws, their functioning etc, that warrants a belief in A, but the

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[2] Armstrong, Dretske and Tooley (hereafter ADT) are notable defenders of laws as necessity relations between universals (Psillos 2002, p.163). See also Amstrong (1983), Dretske (1977), Tooley (1977).

[3] One topic of discussion is for example if the laws themselves are necessary. In the weakest interpretation I am taking here, the laws themselves need not be necessary. They could have been different, but given that they are what they are, they necessitate their consequences.

[4] Many philosophers argue that this notion is far too weak to capture our intuitions regarding what it is to be a law. I am interested in scientific practice and not in intuitions, therefore I will not discuss this.

fact that the law is *there.* This switch from epistemological activities to legitimation in terms of metaphysical modality is subtle, but it is there and it seems to simply happen.

There is of course a rationale behind this. If the laws express what is "precluded or allowed by nature" (Beatty 1995, p.239), they are sensible guides in the sense that they discourage us from trying something that is precluded by nature. The laws tell us the rules of nature's game. If we know these rules (or 'reasons', as Van Fraassen says), we can exploit them to arrive at specific goals. In this way, there seems to be a strong connection between the regularities expressing necessities and being trustworthy guides in epistemic activities. Yet this idea has been undermined in some of the special sciences, e.g. biology. Thinkers like Beatty (1995), Gould (1990) and Carrier (1995) have argued that evolution could have given rise to other regularities in biology than the current ones. Because of this, they claim, many regularities in biology "do not express any natural necessity" (Beatty 1995, p.239) and should not be called laws. Not everyone agrees with a definition of lawhood in terms of necessity. In the next section, I discuss an alternative which focuses on epistemic authority.

2.2 Epistemic mark

Another attitude towards this epistemic authority of laws has been to *equate* lawhood with epistemic authority. This strategy, which Psillos called "the epistemic mark" (2002, p.141), can be captured as follows:

> It is a law that all Fs are Gs if and only if (i) all Fs are Gs, and (ii) that all Fs are Gs has a privileged epistemic status in our cognitive inquiry. (Psillos 2002, p.142)

This view was defended by among others, Goodman (1983), Braithwaite (1953) and Ayer (1963). They do not make reference to metaphysical properties of laws, but it also does not give us any understanding of why specific regularities are used in cognitive inquiry. Moreover, this strategy been criticised for being too subjective and anthropomorphic (Psillos 2002, p.142). Mill (1911), Ramsey (1928) and Lewis (1973) (hereafter MRL) are often put in the same boat. They defend an approach dubbed "web-of-laws approach" (Psillos 2002, p.148), where laws are the axioms in the systematic organisation of our knowledge. According to Lewis, a regularity is a law

> if and only if it appears as a theorem (or axiom) in each of the deductive systems that achieves a best combination of simplicity and strength" (73, 1973 in Psillos 2002, p.149)

Though this approach made lawhood more objective than other epistemic mark accounts, many philosophers still hold that it is too subjective (Psillos 2002, pp.155-157).

This is not meant as an in-depth overview of the debates surrounding laws, and I recommend one of the many excellent works on this topic that are readily available[5]. For my current point, this brief summary is enough to frame the problem of epistemic authority and the ways it has been handled in the past. The debate regarding laws has still not reached a consensus. In the philosophy of biology, the topic is even abandoned. Craver and Kaiser even see it as outlived and suggest we should instead focus on how generalisations in biology can play the role they do:

> Nobody anymore denies that there are stable regularities that afford prediction, explanation, and control of biological phenomena. Whether such stable regularities count as laws depends on what one requires of laws, but it is undeniable that generalizations of this sort do many kinds of work in biology. What remains is the admittedly difficult work of showing how this is possible. (Craver & Kaiser 2013, p.127)

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[5] See for instance Van Fraassen (1989), Carroll (1994), Lange (2000), Carroll (2004), Psillos (2002).

I agree with the suggested shift of focus, but there is more to be said. For one, which specific regularities provide a secure basis for explanation, prediction,… is not agreed upon by philosophers of science. And second, one of the reasons philosophers have sought a definition of laws, is to explain *why* certain regularities (viz. laws) can be used for prediction and explanation, and certain other regularities (viz. accidental regularities) cannot. So shifting the focus of the debate is not as easily done as Craver and Kaiser suggest. Yet in this paper, I will make an attempt.

I will investigate *how* regularities can play the roles they do in scientific practice, without posing the question in terms of what defines laws. This involves arguing that neither of the approaches mentioned (viz. necessity and epistemic mark) can successfully account for why certain regularities get epistemic authority in science. For one, focusing on metaphysical criteria for lawhood does not increase our understanding of scientific practice. Consequently, if scientific practice is what we are interested in, as I am, focusing on metaphysical markers for lawhood will not be useful. Second, merely referring to our epistemic attitude towards certain regularities is not very insightful. I will get back to this in section 4. I instead propose a shift in focus away from attempting to provide a definition of laws. I focus on what we successfully do with regularities and what this can tell us about those regularities. So contrary to many philosophers, I start from the observation that certain regularities have epistemic authority and attempt to understand what this tells us about why regularities are used in science. In that sense, I'm following the approach of Goodman, Ayer and Braithwaite, but I'm developing it. To show what this other perspective can teach us, I will take a closer look at engineering sciences. I first present a case study from this domain. This will help to get a better understanding of what the engineering sciences are and how they successfully use regularities. These cases will be the guiding sources of information for understanding the epistemic authority that regularities get in scientific practice.

3. Epistemic activities in the engineering sciences

3.1 What are the engineering sciences

Boon and Knuuttila (2009) defined the engineering sciences as striving "through modelling to explain, predict or optimize the behaviour of devices, processes, or the properties of diverse materials, whether actual or possible." Boon argued that the engineering sciences have a very distinctive modelling practice, which is not reducible to physics.

> […]applying scientific laws for describing concrete phenomena usually requires idealizations, approximations, simplifications and ad-hoc extensions. (e.g. Cartwright 1983, p.111). As a result, in technological applications predictions based on scientific theories are not at all straightforward since boundary conditions not accounted for in the theory may be involved. Scientific theories do not give rules on how to idealize, approximate, simplify and extend a scientific law in order to make it fit for concrete phenomena. Consequently, scientific approaches to understanding or predicting phenomena relevant to technological applications involves scientific modeling different from the way textbooks present the application of fundamental theories in the construction of mathematical models for concrete systems (e.g., by using Newtonian mechanics, thermodynamics, electricity and magnetism, or quantum mechanics). (Boon 2011, p.64)

Nor can the modelling practices of engineering sciences be reduced to technology or design.

> The models developed in the engineering sciences should be distinguished from the models produced in engineering. Whereas the latter usually represent the design of a device or its mechanical workings, models in the engineering sciences aim for scientific understanding of the behaviour of different devices or the properties of diverse materials. (Boon & Knuutilla 2009, p.1)

Technology and engineering are related however, since "much research in the engineering sciences is aimed at creating and understanding physical phenomena that may be put to technological use" (Boon 2011, p.66). Yet the relationship between these domains (fundamental physics, the engineering sciences and technology) is underinvestigated and does not benefit from the traditional debate regarding laws. If laws are considered to receive their epistemic status from the metaphysical necessity they express, only research regarding regularities that express necessity seems legitimate. So I believe the alternative, practice-engaged understanding of why certain regularities are trustworthy guides for predictions, explanations, etc., will benefit philosophical reflections on the engineering sciences.

To get a better grip on what it is that engineering scientists do, let's look at a case study from failure analysis. Failure analysis is a part of engineering sciences, which focuses on analysing and explaining failures of artefacts and formulating appropriate design recommendations. The case I will discuss here, deals with the collapse of a spray drier, analysed by Carter (2001). I will argue that a specific rule Carter uses for his analysis, namely the Neuber rule, gets epistemic authority, though it is not straightforwardly a law.

3.2 A creepy case

A spray drier is an artefact which "dries a finely divided droplet by direct contact with the drying medium (usually air)" in a short retention time (Considine & Kulik, p.5130). The failed spray drier in the case I will examine, suddenly collapsed after 20 years of service while operating normally[6] (Carter 2001, p.73). According to Carter, this was a result of creep, which refers to non-elastic (viz. irreversible) deformation due to stress at high temperatures. Based on his investigation, he recommends removing the "lagging and cladding in the region of the annular gas duct and the column-shell joints", in order to "avoid a similar fate on other more recent (and stronger) spray driers" (ibid, p.77). To understand Carter's analysis, I need to present some background information regarding creep. Under normal temperatures

> most metals and ceramics deform in a way that depends on stress but which, for practical purposes, is independent of time:
>
> $\varepsilon = f(\sigma)$ elastic/plastic solid[7]
>
> (Ashby & Jones 2012, p.311)

When temperatures rise, deformation becomes dependent of time. This process is referred to as "creep" or *viscoplasticity*

> As the temperature is raised, loads that give no permanent deformation at room temperature cause materials to creep. Creep is slow, continuous deformation with time: the strain, instead of depending only on the stress, now depends on temperature and time as well:
>
> $\varepsilon = f(\sigma, t, T)$ creeping solid
>
> (Ashby & Jones 2012, p.311)

Creep gives rise to creep strain; deformation of the material:

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[6] It is not clear what the author means with "operating normally". Arguably, this is a judgment based on his background engineering knowledge.

[7] *Stress* (σ) is applied force per unit area (Feynman et al. 2011, II 38-2). *Strain* (ε) is deformation that happens as a result of stress (Ashby & Jones 2012, p.34). More specifically, it is "stretch per unit of length" (Feynman et al. 2011, II 38-2). Two types of strain are distinguished, depending on whether deformation is reversible: elastic (temporary) strain and plastic (permanent) strain (Ashby & Jones 2012, p.117).

> A typical creep experiment involves measuring the extent of deformation, called the *creep strain,* ε, over extended periods of time, on the order of thousands of hours, under constant tensile loads and temperature. (Mitchell 2004, p.432)[8]

Finally, the creep exponent:

> By plotting the log of the steady creep rate, ε_{ss} against log s at constant T, [...] we can establish that
>
> $$\varepsilon_{ss} = B\sigma^n$$
>
> where n, the creep exponent, usually lies between 3 and 8. This sort of creep is called "power-law" creep. (Ashby & Jones 2012, p.315).

Carter specifically makes use of a Neuber calculation to determine the creep stress concentration factor in notches, which states that

> the product of shear stress and shear strain concentration factors of a notched body of nonlinear material was independent of the external load level. (Härkegård & Sørbø 1998, p.224)

The Neuber calculation is often mentioned in this form:

$$K_t^2 = K_\sigma K_\varepsilon$$

with

K_t theoretical stress concentration factor
$K_\varepsilon = {}^{\varepsilon_e}/_{\varepsilon_{ne}}$ actual strain concentration factor
$K_\sigma = {}^{\sigma_e}/_{\sigma_{ne}}$ actual stress concentration factor

Another way of understanding the Neuber calculation is the following:

> In connection with low-cycle fatigue analysis, where inelastic strains are generally confined to the notch root area, Neuber's (generalized) rule implies that the product of equivalent stress and strain, $\sigma_e \varepsilon_e$ is equal to the same product under linear elastic conditions, (ibid.)

Carter uses the Neuber rule (together with design information of the spray drier) to determine the actual stress in the column-shell from the value of stress and strain under elastic (viz. reversible) conditions.

Let's focus on this Neuber calculation. Since Neuber presented his rule in 1961, it received a lot of attention. For one, people found that in certain conditions, the rule overstates the stress. It has also been used in different ways, attempting to model different circumstances and materials. This practice, as well as the article by Carter, shows that the Neuber rule has epistemic authority: it is used to explain phenomena (e.g. the failure of the investigated spray drier) and predict phenomena (e.g. the behaviour some more recent and stronger ones). Carter's article was published in a specialized failure analysis journal, and reprinted as part of a "reference set of real failure investigations" (Jones 2001, v). The goal is to communicate findings to other engineers and engineering students (ibid.). So the engineering sciences community accepts this kind of explanation and prediction, based on regularities like the Neuber rule. Moreover, the articles (like Carter's) often mention that their recommendations (based on predictions) are successful.

How do we legitimate the fact that engineers trust the Neuber rule to make explanations and predictions? Coming back to the debate I sketched in the introduction, one option is to show that

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[8] Note that this is not a quote by the philosopher Sandra Mitchell, but Brian S. Mitchell, professor in the department of chemical and biomedical engineering at Tulane.

it is a law – since laws are thought to correctly receive epistemic authority. In the introduction, I discussed two criteria that have been proposed for lawhood: expressing necessity and having an epistemic mark. Let's first consider necessity: does the Neuber rule express any? One straightforward difficulty is that the Neuber rule is not without exceptions, since it overstates stress in some situations. This is not compatible with the intensional view on laws sketched in the introduction. If we can find an exception (say an observation of ¬A), then "it is a law that A" cannot be true, since we can derive "A" from this, which would yield a contradiction with our observed ¬A. This suggests that the Neuber rule is not a law in this sense and correspondingly, that we have no reason to believe that we can warrantedly extrapolate them to new contexts. Like Van Fraassen said, if there is no reason for the regularity, we cannot trust it to make predictions. Yet it *is* being trusted and used to make predictions. What does this entail for the validity and authority of the rule? It does not look good regarding necessity. Yet there are laws that are not universally valid, such as Ohm's law, which is not valid for e.g. diodes. Maybe one could still save the Neuber rule by showing that it does express some necessity, yet has a limited domain. She might want to show that it is derivable from laws that we think certainly express necessity, specifically the laws of physics. This is the topic of section 4.

After discussing necessity, I will get back to the epistemic mark in section 5. As I have shown, engineers have the epistemic attitude associated with laws towards the Neuber rule. So according to e.g. Goodman's account, the Neuber rule counts a law[9], and laws can be trusted to make predictions and explanations. This can be seen as a step in the right direction, but what does this actually tell us? It tells us very little. We trust the regularity, so we can trust it. Though this is a proper beginning for an epistemic account of why we trust regularities, it is not informative enough. I will expand this argument in section 5.

4. The laws of physics: the real deal

I now turn to the option to ground the Neuber rule by showing that it is deducible from more fundamental regularities that are thought to be necessary. Someone advocating this strategy hopes that, since the Neuber rule is a deductive consequence of laws that do necessitate their consequences, the necessity will carry over to the Neuber rule. The most obvious candidates for such fundamental laws are the laws of physics. In this section, I investigate the assumptions on which this line of reasoning rests and raise problems for two of them. First, I use arguments from Matthias Frisch to scrutinise the idea of fundamental laws. Second, I expand his arguments regarding modelling to argue that, even if there are fundamental laws, there is no guarantee that their necessity will make any difference for the modal power of the Neuber rule.

4.1 Grounding the Neuber rule

An important observation regarding the possibility of reducing the Neuber rule to more fundamental (and necessary) laws of physics, is that up until now, no attempt has been successful. From the engineering literature on creep, we can conclude that there is no model of creep in terms of fundamental laws – let alone of creep in notches which is crucial to this case. There are attempts though, like Nabarro (2004). This is an article by Frank Nabarro, one of the pioneers of dislocations in solids (Brown 2009, p.275), who spent a significant part of his life studying various modes of creep and having one specific type of creep in crystals named after him (viz. Nabarro-Herring creep), only two years before he died. In this article Nabarro discusses an overview of the different models of power law creep. Power law creep, or steady-state creep, refers to the third stage of creep (after initial rapid extension and primary creep)

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[9] Note that MRL might disagree.

which occurs before rupture (Nabarro 2004, p.659). This is a particularly interesting stage for engineers (ibid.), and is also the type of creep present in the spray drier case study. Throughout the decades, many models have been presented and three are often cited: two models by Weertman and one by Spingarn and Nix. These models attempt to explain why, for a wide range of stresses, "the creep rate is [...] closely proportional to a power of the stress, with an exponent of about 4.5-5.0" (Nabarro 2004, p.661). Each model starts from a different idea regarding the mechanism of creep (viz. by glide of dislocations of a certain density until they are blocked by other dislocations, by glide two dislocation configurations, by dislocation glide on a single slip system in each grain (Nabarro 2004, p.660)), resulting in different models and different predictions of the creep rate. Nabarro argues that every model

> either lacks physical probability or fails to predict a rate of creep of the order which is observed experimentally". (ibid.)

So a definitive or accepted theoretical model of creep has not been developed yet.

But science is a gradual endeavour and we might at some point in the future succeed in deriving the Neuber rule from thermodynamical laws. Should we keep on using the Neuber rule counting on the fact that it will at some point be grounded? To answer this question, let's look more closely at what's happening here. The idea is that the laws of physics apply to all possible phenomena, and we just need to figure out how. This is related to the prestige physics, especially fundamental physics, has: it is seen as the most mature science, a role model for other sciences (e.g. Norton 2003). Correspondingly, the laws of physics are often considered as exemplars of laws of nature: universally valid, necessary, fundamental[10]. According to this view, the Neuber rule is, like all other regularities about the physical world, simply shorthand for a model in terms of more fundamental laws. As a result, all the rule's properties (including any necessity it expresses) are due to its relation to the fundamental laws.

This way of reasoning actually rests on many assumptions regarding the laws of physics and modelling practices. Specifically, the way of reasoning rests on the ideas that (1) the laws of physics express necessity and (2) are fundamental, which in some sense implies that they can capture all phenomena. And regarding modelling practices, there is the assumption that (3) necessity carries over through modelling practices. Though this final assumption is less easy to recognise, I will argue in section 4.3. that it is in fact there. I will not deal with the first assumption, but will show that (2) and (3) are not as unproblematic as often assumed. In the following section I first scrutinise the purported fundamental nature of the laws of physics. For this part, I build on Matthias Frisch's arguments against foundationalism developed in the context of causal reasoning in physics.

4.2 Frisch on the laws of physics

According to Frisch, many of the issues in the debate surrounding causation in physics are based on a view of physics that does not line up with scientific practice. He argues that the debate silently assumes what Cartwright has called the "vending machine view of science" (1999), which expresses that

> a physical theory's representational content – what the theory says about the world – is given simply by a statement of the theory's basic equations, which are taken to define the theory's representational structures. (Frisch 2014, p.28)

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[10] This is obvious from the debates surrounding whether biology has laws, mentioned in the introduction. In analysing the nature of regularities in biology and debating their law-likeness, laws of physics were often used as a contrast class or point of reference (see Beatty 1995, Brandon 1997, Carrier 1995).

Frisch uses cases from physical scientific practice to argue that this view is mistaken. Instead, we need to take into account the user and the context to fully understand the representational content of a theory, even for physical theories. His alternative account of scientific practice in physics starts from a "pragmatic and structural account of representation" (Frisch 2014, p.37). The structural part is the claim that

> Representation is purely structural, since the models or representations employed, at least in the physical sciences, are mathematical structures and the only relevant resemblance between mathematical structures and physical systems is structural resemblance. (Frisch 2014, pp.27-28)

The similarity between representation and physical system is structural. The pragmatic part builds on Van Fraassen and is the claim that

> there is no representation except in the sense that some things are used, made, or taken, to represent things as thus and so (Van Fraassen 2008, p.23)

Frisch argues that this pragmatic part solves certain problems associated with a purely structuralist account, such as symmetry. In a pragmatic account of representation, there can be

> no "natural representations" – no naturally produced objects or phenomena that represent other phenomena without being used by someone to represent. (Frisch 2014, p.37)

In other words, we cannot simply let mathematical equations 'talk for themselves' in representing phenomena: the user and the context matter and cannot be ignored. Especially this latter part is fairly controversial, even though it explicitly builds on work in general philosophy of science by a.o. Cartwright (1983, 1999) and Van Fraassen (2008). Though Cartwright's book was published over fifteen years ago, many debates in philosophy of physics mainly study the fundamental equations, which are seen as covering the entire content of a theory. Frisch, building on Cartwright, shows that this does not correspond to scientific practice: we cannot simply reach all the epistemic goals we want via just laws or equations. We need to build *models*. The

> […] physical processes that interact with the production and annihilation of elementary particles are not, and in fact cannot be, modeled quantum field theoretically. Instead physicists use resources from theories such as classical electrodynamics, fluid dynamics, and solid-state physics to model the causal structure within which the quantum-field theoretic interaction is embedded. (Frisch 2014, pp.80-81)

It is the modelling practices that really matter. This lines up well with my current purpose: understanding how regularities get epistemic authority. Part of epistemic authority of the regularities is that they are being used to model phenomena.

Frisch uses this pragmatic account of representation to formulate a convincing argument against what he calls scientific foundationalism. This is meant to capture the view that

> physics aims to discover fundamental micro theories that have a universal domain of application and in principle possess models of all phenomena. (Frisch 2014, p.37)

Frisch shows that scientific foundationalism is inherently incompatible with a pragmatic account of representation, like the one he defends. His argument draws from physical modelling practice and is pretty straightforward:

> [C]ontrary to what the foundationalists assumes, we do not have fundamental models representing macroscopic phenomena. To actually construct a quantum-mechanical model of a

> macroscopic body of water, we would have to solve the Schrödinger equation for on the order of 1025 variables – something that is simply impossible to do in practice. (Frisch 2014, p.38)

Because Frisch defends a pragmatic account of representation, a hypothetical solution to the Schrödinger equation for a body of water, does not qualify as a model of the body of water. In order for something to count as a representation of a system, it needs to be used to represent that system or some other system sufficiently close to the one we want to represent. So if we successfully model a glass of lemonade with the Schrödinger equation, this is a pretty good indicator that the equation can also be used to represent a glass of water. Yet a hypothetical solution, or a model for a very different system cannot provide this indication. His point holds for all the so-called fundamental laws. Note that it is not clear which laws should be seen as fundamental. Sandra Mitchell gives us a pragmatic definition:

> It is not clear that anything that has been discovered in science meets the strictest requirements for being a law. However, if true, presumably Newton's Laws of Motion, or The Laws of Thermodynamics, or the Law of the Conservation of Mass/Energy, would count. (Mitchell 2002, p. 330)

Frisch's argument goes through regardless of any specific set of 'fundamental' laws: looking at current physical modelling practices, no laws are effectively used to represent all phenomena, so no set of laws is really fundamental. Instead, macro phenomena need to be represented by macro theories. Our

> […] putatively fundamental micro theories do not represent higher-level macro phenomena […] (Frisch 2014, pp.24-25)

I have shown that Frisch lays out what is, in my opinion, a strong argument against the view that the fundamental equations of (theoretical) physics can (1) represent all the phenomena we are interested in and (2) are all we need to understand scientific practice, and I have supplemented his arguments with regards to epistemic authority. In scientific practice, modelling is what matters, and not all phenomena (especially not macro phenomena) can be modelled with the fundamental equations. Simply because a steam engine is thought to behave according to the laws of thermodynamics, does not mean it can be represented by referring to the ideal gas law and the conservation of energy principle.

Frisch's conclusions do hinge on the acceptance of a pragmatic account of representation, and people may disagree with this – and they have. But Frisch does not merely posit this view, he defends it, by showing how important the role of modelling is in physical practice. Moreover, since I am explicitly interested in scientific practice, I agree that focusing on modelling and actual representations is a legitimate choice. Only representations and models that are actually being used (or were used at some point) in science to represent certain phenomena, can be part of scientific *practice*. So a pragmatic view of representation is actually quite suited for understanding scientific practice. And if we take scientific practice seriously and accept such a pragmatic account of representation, there can never be a set of laws that 'in principle' represents all possible phenomena, not even laws that express necessity. Laws only capture phenomena via models, and only those phenomena for which models have actually been built and are used in epistemic practices (or phenomena close enough to those). Frisch specifically wants to draw attention to this neglected part of physics (viz. the modelling practices) and argues that they are an important part of the scientific practice. Because of this, a pragmatic account of representation fits best.

So if Frisch's arguments go through, attempting to show that the Neuber rule can be deduced from fundamental laws will not help us in warranting its epistemic authority, since there are no real fundamental laws that can actually be used to model everything.

Because I am specifically focused on the engineering sciences, it is noteworthy that Frisch's findings go against a tendency in philosophy of physics – a tendency that has negative influences on the understanding of the engineering sciences:

> [T]he picture of science that arises is that, in the end, a complete knowledge of the fundamental laws and/or building-blocks presents us with knowledge from which everything else can be deduced, and therefore makes any other epistemic practice intellectually empty. (Boon 2011, p.64)

Boon has criticized this tendency in her defence of the engineering sciences. Though it might be useful in some contexts to try to reduce models of complex phenomena (like artefact behaviour) to more fundamental laws, Boon (2006) shows that this view ignores a big part of actual modelling. Her arguments cohere with those made by other philosophers in different contexts, such as Cartwright (1983).

4.3 The problem of modelling

As I said above, Frisch's arguments are quite convincing, but controversial. Yet even if his arguments don't go through and there are indeed some fundamental laws, there is another complication for trying to warrant the Neuber rule's epistemic authority in the candidate fundamental laws. Here I will show that even if we hold that a set of purported fundamental laws is able to cover all phenomena, there still is no guarantee that the models that we build with them express the same necessity.

The candidate fundamental laws, regardless of what they are, will be used to build models, and those models will guide us in epistemic activities like manipulation and prediction. This at least complicates the connection between necessity and epistemic authority of laws, since there is a 'layer' of modelling between the phenomenon and the law. If it is to be necessity which grants regularities their epistemic authority, the necessity needs to be something that is not damaged by the modelling practices.. Yet there is nothing necessary about the way physicists model phenomena. The models do not follow from the laws in any necessary way. On the contrary, as Frisch says, depending on the interests of the user and depending on the context, different choices will be made, resulting in different models.

> To the extent that resemblance plays a role in representation, it does so as a function of the representation's use. For example, in certain contexts we identify a representation's target with the help of selective resemblances between representation and target. Yet which aspects are important in assessing the likeness between representation and target is given by the context in which the representation is used. (Frisch 2014, p.28)

Frisch makes this point as part of his pragmatic framework, but does not focus on it since he is mainly interested in causation.

I use this point to develop another difficulty for grounding the epistemic authority of the Neuber rule in candidate fundamental laws. Once we acknowledge that in our manipulations and predictions, we only use fundamental laws via models, and that those models do not follow necessarily from the laws, warranting regularities like the Neuber rule is not as unproblematic as it seems. Because, in order to warrant the rule, scientists need to model the phenomenon described by the rule (viz. creep in notches) via more fundamental laws (a.o. laws of thermodynamics) and derive the Neuber rule from this model. Yet modelling a phenomenon is dependent on the user and the circumstances. In order for necessity to warrant the epistemic authority of a regularity, the necessity has to be something that is undamaged by the modelling practices. But there is no necessity to the practices, so it is hard to see how this would work or why this would be the case. So even if there are fundamental laws, the chance that necessity will actually carry over through the modelling is really slim. Simply assuming that it will carry over is actually not taking modelling practices seriously and again would not be in spirit with the science in practice perspective.

To sum up, whether we consider a law as a tool for new discoveries or guideline for manipulation, depends on whether we consider the law to represent the phenomenon we are interested in. This is not straightforwardly captured in the formulation of the theory, but depends on how the law is used in practice. Epistemic authority is never innately present in the laws. Even if we accept that the laws of thermodynamics express necessity, they only receive epistemic status in the practices where they are actually used to represent phenomena. It is worth mentioning that the importance of (contextual and pragmatic choices involved in) modelling practices is not commonly accepted among physicists:

> Thermodynamics is the much abused slave of many masters • physicists who love the totally impractical Carnot process, • mechanical engineers who design power stations and refrigerators, […] It is therefore natural that thermodynamics is prone to mutilation; different group-specific meta- thermodynamics' have emerged which serve the interest of the groups under most circumstances and leave out aspects that are not often needed in their fields. To stay with the metaphor of the abused slave we might say that in some fields his legs and an arm are cut off, because only one arm is needed; in other circumstances the brain of the slave has atrophied, because only his arms and legs are needed. Students love this reduction, because it enables them to avoid "nonessential" aspects of thermodynamics. But the practice is dangerous; it may backfire when a brain is needed. (Müller & Müller 2009, preface)

From the analysis presented here, I conclude that the necessity-approach to epistemic authority falls apart completely. Necessity, it seems, is not the way to understand why the Neuber rule is used and trusted in engineering practice. But then what is? In the following section, I will build on arguments by Sandra Mitchell to argue that engineers warrantedly use regularities like the Neuber rule, depending on the context. This will also enhance the alternative strategy to epistemic authority I mentioned in the introduction, namely the epistemic mark.

5. Contextual and pragmatic authority

Recall that traditionally the debate on laws consisted of two strategies to explain the epistemic authority laws get in science: laws express necessity, or laws have some epistemic mark. I spent the previous sections arguing that the first does not explain why regularities like the Neuber rule are used in engineering sciences. In the current section, I will present an alternative that I believe does explain why the Neuber rule is used. This alternative expands the second strategy, namely the epistemic mark, and uses arguments by Sandra Mitchell, to make it more informative. I will first present Mitchell's contributions to the debate on laws in the life sciences and then adapt it to understand the Neuber rule and the engineering sciences more generally.

5.1 Mitchell's pragmatic account of laws

Sandra Mitchell developed her pragmatic account of laws in the context of biology. I already mentioned this matter in the introduction. Let me briefly recapitulate. From about the 1970s to well in the 2000s, philosophers debated the nature of biological regularities, and more specifically, whether they should be considered laws. Beatty, for example argues against calling them laws, based on their contingency:

> […] all distinctively biological generalizations describe evolutionarily contingent states of nature— moreover, ‘‘highly’’ contingent states of nature in a sense that I will explain. This means that there are no laws of biology. For, whatever ‘‘laws’’ are, they are supposed to be more than just contingently true. (1995, p.46)

In 1997, Mitchell distinguished 3 strategies of characterising laws: a normative, a paradigmatic and a pragmatic. The *normative* strategy encompasses approaches that start with a "definition of lawfulness" and then compare all candidate laws to this definition. If the specified conditions are met, the candidate qualifies as a law (Mitchell 1997, S469). Most of the accounts mentioned in section 2 are normative. Beatty's account is also a normative one. His definition includes natural necessity: laws are only "those generalisations that could never […] failed to be true" (ibid). This corresponds to the traditional debate focussing on necessity which I sketched above and which Cartwright and Frisch criticize. The second strategy, the *paradigmatic*, "begins with a set of exemplars of laws (characteristically in physics) and compares these to generalisations in biology" (ibid.). I will not spend a lot of attention on this strategy.

The final strategy, *pragmatism*, is the one Mitchell puts forward in her article (and will go on to develop throughout later work). This is the one I will use to expand the epistemic mark account of laws. In Mitchell's pragmatic view, reference to definitions and exemplars is replaced with "an account of use of scientific laws" (1997, S475). According to Mitchell, we should entirely abandon the "received view" of what is required to be a law, viz.

1. logical contingency (having empirical content)
2. universality (covering all space and time)
3. truth (being exceptionless)
4. natural necessity (not being accidental) (Mitchell 2002, p.330)

Instead, we should focus on how the generalisations in science are used. The specific contexts in and purposes for which generalisations are used can differ, naturally. Mitchell presents different parameters in virtue of which generalisations can be "evaluated for their usefulness":

- Degree of accuracy attuned to specified goals of intervention
- Level of ontology (populations vs individuals)
- Simplicity: we use generalizations ranging from rules of thumb like Ptolemaic astronomical "laws" to navigate, to ideal gas laws that yield approximations within engineering tolerances.
- Cognitive manageability: prior to the development of high-speed computation, mathematical equations were restricted to solvable linear formulations.

(Mitchell 1997, S477)

The main point I want to take away from this for the current purposes is that, depending on the phenomenon we wish to study and the specific epistemic activity we are undertaking, different generalisations can prove more useful. [11]

Mitchell's project fits well with the focus of this paper. Though Mitchell's account was developed for biology, I can use her framework to expand the analysis from the previous section. By building on her insights, we can get a better understanding of why the Neuber rule is successfully and warrantedly used in engineering sciences. This is the topic of the next section.

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[11] I am not engaging with metaphysical questions, as this falls outside the scope of my paper. But for those interested in a methaphysical companion story to my analysis, I recommend the work of Barry Ward (e.g. Ward 2002).

5.2 Pragmatic laws and epistemic authority

I believe that a pragmatic way of valuing regularities helps us understand the diversity of regularities that are used in the engineering sciences better than the necessity approach I discussed in section 4. If we take a pragmatic approach to laws, then their epistemic authority does not result from any metaphysical necessity they express, but depends on the way they are used to model phenomena. Accordingly, different laws can gain more or less authority, depending on how successful they are with respect to the specific demands of the context.

As it is formulated, merely stating that epistemic authority of laws depends on the context does not give a more informative account of epistemic authority than the epistemic mark mentioned in section 1. Yet this is where Mitchell's account comes in. She formulated several parameters by which we can understand and compare when regularities are best fit for the context and purposes. Reflecting on different epistemic activities and goals that can be part of the engineering sciences provides a way of understanding why regularities like the Neuber rule are used in some contexts, and not in others.

In her original article, Mitchell distinguishes accuracy, ontology, simplicity and cognitive manageability as possible factors that influence the choice of regularity. Yet this is not an exhaustive list. In light of failure analysis specifically, I want to stress *feasibility* and *intelligibility* as important factors. In Carter's case of the collapsed spray drier, he needs to specify recommendations to modify the newer and stronger driers before they collapse as well. Because of this context, he is confronted with time-limitations and restrictions regarding redesign options. Given the task he has, it is not useful to come up with a completely new design for a spray drier, since this will not influence the faith of the existing driers. So modelling the spray drier in terms of the materials with which it was constructed e.g., will not be ideal. Moreover, Carter needs to move fast and cannot spend months modelling the collapse of the drier in terms of more fundamental or micro laws and calculating all the variables. So the regularities he uses need to be intelligible.

Yet looking at the diversity of the engineering sciences, it's important to see that these demands differ when we consider different domains of the discipline. Note that this fits well with my focus on the user and context, inspired by Frisch. A scientist who wants to create a new material, more resistant to creep than others, may need to model creep in more fundamental or micro terms. To understand the gravity of choices regarding modelling, consider another example of a creep model. Mishin et al. (2013) developed such a "general and rigorous theory of creep deformation". In their view, such a theory should contain

> (i) a thermodynamic model of a mechanically stressed crystalline solid with nonconserved lattice sites, (ii) a model of microstructure evolution that includes redistribution of vacancy sinks and sources and the motion of interfaces separating different phases and/or grains, and (iii) a set of kinetic equations derived from the entropy production rate and identification of the appropriate set of fluxes (including the creep deformation rate) and the conjugate driving forces. (Mishin et al. 2013, p.1).

They arrive at a sort of master equation, which they combine with assumptions about the physical properties of materials (e.g. whether it is isotopic, whether thermodiffusion cross-effects can be neglected) to derive a set of "phenomenological relations between fluxes and forces" that are part of this equation (Mishin et al. 2013, p.12). They apply this to an example and arrive at three equations that, with appropriate initial and boundary conditions, describe the entire dynamics of their system in deformed configuration. The equations are:

$$\frac{\partial \varphi}{\partial t} + v_L \nabla_x \varphi = -\frac{B}{T}\left[w'(\varphi) - \epsilon \nabla_x^2 \varphi\right]$$

$$\frac{\partial c_v}{\partial t} + v_L \nabla_x c_v - D_v \nabla_x^2 c_v = \nabla_x \, v_L$$

$$\nabla_x v_L = -\,B_r w(\varphi)\left[\frac{kT}{\Omega_0}\ln\frac{c_v}{c_v^0} - \sigma_{11}^{\infty} + w(\varphi) - \frac{1}{2}\epsilon(\nabla_x\varphi)^2\right]$$

With c_v the vacancy site fraction and $D_v = {}^{k\,\Omega_0 L}/_{c_v}$ is the vacancy diffusion coefficient assumed

to be constant, B_r a constant, σ_{11}^{∞} the coordinate-independent normal stress inside the grains c_v^0 the equilibrium vacancy concentration in the absence of normal stress $\Omega_0$the stress-free value of the volume per site (Ω), k Boltzmann's factor, $w(\varphi)$ is a double-well function with an amplitude W creating a free-energy barried between two lattice orientations, ϵ is the gradient energy coefficient. These are the equations for a one-dimensional model. As they state,

> Due to the simplified geometry of this example, we will obviously not be able to model a real three-dimensional creep process taking place in polycrystalline materials. (Mishin et al. 2013, pp.15-16).

While this model has the potential to provide insight in creep in specific materials and can aid in explaining why certain materials behave the way they do, they will not likely be helpful for a failure analyst like Carter[12]. But they might be useful in other epistemic contexts e.g. developing new materials. A pragmatic view on laws thus gives us a positive reason for engineers to use the Neuber rule: in certain contexts, the Neuber rule best fits the demands of the engineer and discipline.

The great diversity in approaches of creep-research seems to reflect this need for distinct regularities depending on the context. In some cases, the need for diversity is even explicitly acknowledged by engineering scientists. For example, Härkegård and Sørbø (1998) investigate the applicability of the Neuber rule because, regardless of the existing FEM techniques[13] to calculate stresses,

> […] it is still important for design engineers to have a qualitative notion of the key factors effecting stress and strain at notches. […] Therefore, validated and well-documented simplified methods for the approximate analysis of notches may still prove valuable. (Härkegård & Sørbø 1998, p.224)

Correspondingly, the specific regularities that are used and trusted in various contexts, will differ depending on the goal of the context and users. So whether and why a regularity receives epistemic authority is a question that can only be answered from within a specific context. This alternative, pragmatic view on laws thus presents a way to understand why engineers keep on using regularities like the Neuber rule. And while it builds on Goodman and others in the sense that the epistemic status of a regularity in a community is central for epistemic authority, the different parameters of usefulness help us get a better understanding of why certain regularities are trusted for certain purposes. In a sense, it is as Goodman said: laws are laws because they receive epistemic authority. This has been criticised as an anthropocentric and subjective

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[12] Note that because this model cannot be used to describe three-dimensional phenomena, it also does not fulfill Nabarro's criteria mentioned in 4.1.

[13] FEM stands for Finite Element Methods, and refers to discretization technique in structural
Mechanics developed to solve mathematical equations by dividing them into non-overlapping components of simple geometry. (Lin 2010, p.1)

criterion. And compared to the necessitarian view, it is. We could *in principle* have given epistemic authority to other regularities. But by formulating the parameters to evaluate whether a regularity is best fit, there is a less subjective way of giving regularities authority. At the very least, it is a mind-independent criterion. Moreover, the regularities also have to be based on evidence (which I have not spent time on here, but see XXX) and they need to be successful, the need to work. These are all mind-independent. Moreover, which regularities receive epistemic authority in which contexts constantly changes. New regularities and applications are being developed (see above, the developments regarding creep). And they are being used and trusted. Understanding how and why this happens can best be done with a pragmatic story.

A pragmatic approach to epistemic authority also helps to bypass the theory-focus of traditional philosophy of science that a.o. Boon (2011) criticises. If the laws of physics (and equivalent laws) were the only ones that we can trust to make predictions and warranted explanations, then the engineers who rely on regularities like the Neuber rule would behave quite unsystematically and unmethodically. After all, their actions would not be guided by anything reliable, but the resulting diagnoses and predictions are trusted to make changes to existing artefacts, or to design new ones (see also De Bal, forthcoming). Yet because of all the successful applications and the general merits of engineering (sciences), this is highly unlikely and somewhat undervalues the methodology of a profession with great influence on our daily lives. A pragmatic understanding of laws, in combination with arguments against foundationalism and a focus on modelling, allows for a proper validation of the engineering sciences and their scientific practice.

The context-dependence of epistemic authority also helps to understand the distinction Boon and Knuutilla (2009) draw between engineering and the engineering sciences. They are different epistemic practices, with different goals and therefore different regularities. Looking back at the design recommendation Carter formulated for the spray drier (viz. to remove the lagging and cladding), it should be noted that this seems more straightforward than it might be. Actually designing a spray drier without the lagging and cladding might need some other adaptations in order for the resulting spray drier to function in a stable way. Implementing the changes suggested in the design recommendations from failure analysts is a different epistemic practice than discovering what caused the failure. The first, I would say, is part of engineering design – a discipline with its own challenges and goals (see e.g. Kroes 2009 and Radder 2009). The second is part of engineering sciences (since it aims at general knowledge). Correspondingly, the two practices might require different regularities to achieve their goals. Going from the recommendations to a new functioning artefact may require other regularities that the failure analysis, regularities of another level, of a different specificity, knowledge of specific materials and threshold values,… This difference can also be explained in the pragmatic approach to lawhood and epistemic authority I presented here.[14]

Finally, I want to reflect on the three strategies of defining laws that Mitchell distinguished. When we adopt a pragmatic view on regularities, this does not entail that is some contexts, laws cannot be used normatively or paradigmatically by scientists. On the contrary, depending on the context, scientists can use the concept of law in a normative or paradigmatic way. All of this is possible in a pragmatic account on laws and epistemic authority, while helping us understand why those regularities are used in that specific way. Because of this and all the other reasons above, I believe a pragmatic view on laws is remarkably well fit to reflect on scientific practice

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[14] For a detailed and informative discussion of the technology-engineering-science relation that corresponds with my analysis, see e.g. Radder 2009, Boon 2006 and Boon 2011.

and specifically epistemic authority. If nothing else, it is better than the necessity approach. But hopefully, my analysis has shown that the pragmatic view can do more: it draws our attention to underinvestigated problems in philosophy of science and helps us understand the scientific practice of less visible disciplines such as the engineering sciences.

6. Conclusion

In this paper, I investigated how we can legitimate why certain regularities receive *epistemic authority* in certain scientific practices. With "epistemic authority" I referred to the fact that regularities are trusted to achieve epistemic goals like prediction, explanation and manipulation. I tackled this question from the point of view of the engineering sciences, specifically failure analysis and used the Neuber rule from creep modelling as an exemplar.

I showed that in the philosophical literature, epistemic authority is often connected to the distinction between laws and mere regularities: laws can be trusted for epistemic goals, mere regularities cannot. Yet what makes a law a law is not agreed upon by philosophers. I discussed two common strategies for defining laws and for legitimating their epistemic authority: a necessitarian approach and an epistemic mark approach. Throughout the paper I argued that neither was, in its current form, sufficient to explain why the Neuber rule is trusted in engineering practice. Regarding necessity, I argued that the most obvious way to claim that the Neuber rule expresses necessity, was to derive it from more fundamental laws that are thought to express necessity. Building on Frisch's work in philosophy of physics, I then showed that (1) the Neuber rule is currently not successfully derived from (more) fundamental laws, (2) the idea that there are truly fundamental laws that can be used to represent any phenomenon is not unproblematic given the functioning of scientific practice, and (3) even if there are such fundamental laws, there is no guarantee that their necessity is undamaged by the modelling practices of science. I concluded that a necessitarian approach to epistemic authority does not help us to understand why the Neuber rule is trusted and used successfully in failure analysis.

As an alternative, I presented a pragmatic approach to epistemic authority, based on the work of Mitchell regarding laws of biology. I argued that whether a regularity receives epistemic authority depends on the specific demands and purposes of the scientific practice and undertaking. This entails that, even if we succeed in expressing the Neuber rule in more fundamental or micro terms, the resulting regularity might not receive epistemic authority in failure analysis, since it might be less apt to reach the specific goals of the discipline. I stressed that *feasibility* and *intelligibility* are important features for regularities in failure analysis. I argued that this pragmatic approach can explain the epistemic authority of the Neuber rule in failure analysis better than a necessitarian approach, while also accounting for the great diversity of regularities in different scientific disciplines. Moreover, I argued that this alternative account is more informative than the epistemic mark account of a.o. Goodman.

For my analysis, I also combined and expanded on arguments from philosophers of physics and philosophers of biology. Physics is often still seen as the exemplar, as the most mature science. The debate on laws in biology started from a comparison with laws of physics. Precisely Because of the prestige that is connected with different sciences, with laws, with fundamentality, it is important that we combine insights from different fields in the philosophy of science. Thanks to philosophers like Frisch, who provide us with a more nuanced and practice-engaged view of physics, we can redraw the comparison. And this has consequences for other scientific disciplines as well. By moving away from a theory-focus view of physics as point of comparison and example for other sciences, the field opens up for legitimate research into different domains, like the engineering sciences.

A whole bunch of philosophical debates are influenced by the definition and conception of law. As is clear from the way I conducted the analysis, the philosophical tools have long been in the making. They are here, but need to be combined. I believe it is time we take the image that arises from combining them seriously: scientific practice does not differ as much across the domains as often thought, and the way in which it differs is worth investigation. To give but one example: the relationship between philosophy of more traditional sciences (such as physics) and philosophy of engineering and technology. Similar points to Frisch's against foundationalism and for the importance of models have been made from the perspective of philosophy of technology. I already mentioned Boon's arguments. But Hans Radder (2009) made a similar point in discussing the relation between science and technology: for fundamental theories to become empirically applicable, they have to be "developed and specified with a view to particular domains of empirical phenomena" (p.72). He also defends the importance of modelling in science. Yet Frisch's points are still considered controversial, and philosophy of engineering and technology is still not booming in the way that philosophy of biology e.g. is. Integrating work from different debates can help strengthen the legitimacy of all these not so traditional disciplines. And that can, in my opinion, really benefit our understanding of science in all its forms and applications.

7. References

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