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An Empiricist View on Laws, Quantities and Physical Necessity

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Abstract: In this article I argue for an empiricist view on laws. Some laws are fundamental in the sense that they are the result of inductive generalisations of observed regularities and at the same time in their formulation contain a new theoretical predicate. The inductive generalisations simultaneously function as implicit definitions of these new predicates. Other laws are either explicit definitions or consequences of other previously established laws. I discuss the laws of classical mechanics, relativity theory and electromagnetism in detail. Laws are necessary, whereas accidental generalisations are not. But necessity here is not a modal concept, but rather interpreted as short for the semantic predicate "... is necessarily true". Thus no modal logic is needed. The necessity attributed to law sentences is in turn interpreted as "necessary condition for the rest of the theory", which is true since fundamental laws are implicit definitions of theoretical predicates use in the theory.

Keywords: laws, natural necessity, implicit definition, classical mechanics, electromagnetism, induction

1. Introduction

The concept of a law of nature has been debated by philosophers for a long time and many views have been proposed. I have discerned at least eleven different positions in the debate: (i) laws are contingent relations between universals (Dretske, 1977; Tooley, 1977; Armstrong, 1983), (ii) laws are axioms and theorems in a complete theory about the world (Lewis, 1983, 1986), (iii) laws are those universally generalised conditionals true in all possible worlds (McCall, 1984; Pargetter, 1984; Vallentyne, 1988), (iv) laws are relations between essential properties (Bigelow et al., 1992; Bird, 2007), (v) there are no laws (van Fraassen, 1989; Mumford, 2004), (vi) laws are grounded in causal powers (Ellis, 1999), (vii) laws are grounded in invariances based on dispositional properties (Woodward, 1992), (viii) laws belong to non-maximal sets of counterfactually stable propositions (Lange, 2009), (ix) laws are relatively *a priori* principles for empirical knowledge (Friedman, 2001), (x) laws are primitives (Carroll, 1994; Maudlin, 2007) and (xi) laws are metatheoretic propositions (Roberts, 2008). The list is not complete.

The debate has been characterized by Earman (2002, p. 1) in the following way:

> It is hard to imagine how there could be more disagreement about the fundamentals of the concept of law of nature – or any other concept so basic to the philosophy of science – than currently exists. A cursory survey of the recent literature reveals the following oppositions (among others): there are no laws of nature vs. there are/must be laws; laws express relations between universals vs. laws do not express such relations; laws are not/cannot be Humean supervenient vs. laws are/-must be Humean supervenient; laws do not/cannot contain ceteris paribus clauses vs. laws do/must contain ceteris paribus clauses.
>
> One might shrug off this situation with the remark that in philosophy disagreement is par for the course. But the correct characterisation of this situation seems to me to be "disarray" rather than "disagreement". Moreover, much of the philosophical discussion of laws seems disconnected from the practice and substance of science; scientists overhearing typical philosophical debates about laws would take away the impression of scholasticism – and they would be right!

Earman's remark that the discussion about laws is disconnected from the substance and practice of science is indeed true and in my view one reason why it has been so inconclusive. In this article I will try to avoid this mistake.

A fruitful approach is, I believe, to begin the discussion about laws with some concrete examples from physics, examples that everyone interested in the debate would accept as prime examples of scientific laws. My aim is then to discern the reasons why everyone agrees that these examples are laws and what information scientists themselves convey by thus calling them "laws". For it is an astonishing fact that there are many uncontroversial examples of laws, thus the extension of the predicate "natural law" is not much in dispute. By contrast, the metaphysics of laws is highly controversial among philosophers, hence also the meaning of "natural law".

I assume that the meaning of a general term determines its extension, but not the other way round. Metaphysical disputes about laws are disputes about the meaning of the term "natural law". One aspect of this dispute is whether terms occurring in laws, such as "mass", "charge", "force", "current", etc., refer to quantitative properties and relations, or whether we should conceive of them as general terms with extensions but lacking reference.

Many positions in the debate seem to be motivated by metaphysical convictions about the existence of universals, such as properties, essences, relations or irreducible dispositions. Led by these convictions many philosophers try to define the concept of law in terms of the preferred metaphysical notion. This is not my cup of tea. I share empiricists' general scepticism concerning the explanatory force of postulating such things as properties, essences, relations or dispositions, and, moreover, I don't think that that is the kind of reason scientists have for calling certain sentences of theories they hold true for "laws".

All empiricists concur, I believe, with Hume's criticism of the idea of hidden powers being responsible for the lawful regularities in nature. But I cannot rest content with Hume's psychological explanation of why we tend to think there are lawful necessities in nature. His observation that we are conditioned to expect the continuation of an observed regularity is certainly correct, but that cannot be the full explanation of our beliefs in laws (and our use of the associated notion of physical necessity), because it is easy to conceive situations where we have this expectation without referring to a law or principle being operative. My goal in this article, then, is to discern the reasons why *some* true sentences in science are called "laws" without postulating any metaphysics.

Van Fraassen (1989) took a harsher route by dismissing the concept of natural law as unnecessary. Being a leading empiricist, he criticized the first three options listed above (these were the main alternatives when he published his 1989 work) as failing the goal of analysing the concept of law; and several newer ideas would fall prey to more or less similar criticism. But, he claims, this failure is no reason for concern, for we have no need for the concept of natural law. One can give a fully satisfactory account of science without assuming that there is a specific category of propositions, *laws.*

In the strong metaphysical sense of "law" according to which laws are necessary *de re* propositions I agree with van Fraassen; we have no need for such things. But the expressions "natural law", "physical law", "scientific law", etc., are commonly used, so one is prone to ask: "What is the point of making a distinction between some sentences, called 'laws', and other true, general sentences?" And what is this distinction based upon, if not a difference in modality?

Van Fraassen has, of course, not convinced opponents of a more metaphysical bent. Several philosophers – for example, Bird (2007) and Bigelow et al. (1992) – hold that laws are grounded in relations between essential properties of things and are therefore necessary. The empiricist's natural reaction is to ask: how do we know that? Observable phenomena cannot be used to distinguish the support for "It is a law that P" from the support for "P". By the same token, the empirical support for a sentence of the type "a is F" and for "a is necessarily F" is the same, so we have no *empirical* reason to make modal distinctions.

Metaphysicians accept that, of course, arguing that we need assumptions about modal properties for explaining lawhood, not for making correct predictions. Well, I will here explain our calling some sentences "laws" without using modalities, so it is not needed. But of course, it all depends on what we require of an explanation. Van Fraassen's conclusion that the concept of law is not needed for ascertaining empirical adequacy of a theory is correct. But scientists use the concept, so they use it for some other purpose.

Maudlin (2007) argues that we should view laws as primitives not analysable in terms of necessitation, counterfactuals, dispositions, etc.; it is rather the other way round. I agree that we should not try to analyse the concept of law in terms of these metaphysical notions; no scientist has, to my knowledge, ever claimed that a universally generalised conditional in a theory is a law because it fulfils the criteria of any of these popular concepts in philosophical discourse. However, one is immediately led to ask how we obtain knowledge about laws, *qua laws*, if they are primitive? Maudlin (2007, p. 17) admits this difficulty: "To the epistemological questions I must, with Armstrong, admit a degree of skepticism. There is no guarantee that the observable phenomena will lead us correctly to infer the laws of nature".

One should observe that Maudlin talks about inferences from *observable* phenomena, not from observed phenomena. Everyone knows that inductive generalisations from observed phenomena to general statements about observable phenomena is uncertain, no matter if we call the conclusion a "law" or not. So I take Maudlin to be a bit sceptical about the inference from a generalisation of observations to its being a law.

In this article I will suggest a solution to this problem, namely, that in *some* cases of inductive generalisations we introduce a new predicate in order to formulate the regularity; thus the conclusion of the induction also functions as an implicit definition of the new predicate. These generalisations are in an epistemological sense *fundamental* laws, which is one subcategory of laws that I will discern in this article. The two other subcategories are derived laws and laws being explicit definitions of new quantities. But before arguing these points in some detail, some preliminary reflections are necessary. In the next section I will discuss the extension of the predicate "law of nature", in Section 3 I will show how to bring equations to the standard logical form of laws and in Section 4 I will consider some semantical issues. In Section 5 I will discuss in more detail induction and concept formation, in Sections 6 to 9 I will analyse some laws in, respectively, classical mechanics, relativity theory, electromagnetism and quantum mechanics. Finally, in Section 10 I will give my explanation of why we say that laws are necessary. Postponing the discussion of physical necessity to the end of the article is motivated by two considerations: (i) I treat physical necessity as a semantic predicate, not a modal operator in the object language, and (ii) I explain physical necessity in terms of laws, not the other way round.

2. The Extension of the Predicate "Law of Nature"

Quite often scientists do not use the word "law" when describing the core of scientific theories; instead they talk about "equations", "principles" or "postulates", as in "Schrödinger's equation", "Pauli's exclusion principle" or "Einstein's postulates". However, it is pretty obvious that these labels refer to things philosophers would call "laws of nature". And many scientists use the word "law" as a generic label for these things; Penrose, for example, has called his magnum opus *The Road to Reality. A Complete Guide to the Laws of the Universe* (Penrose, 2005).

Henceforth, I will assume that the extension of the concept of law in physics comprises a large number of equations, principles and postulates. Whether there are laws in chemistry, biology and other natural sciences depends on the analysis to be given for these fields and I leave that for another occasion. Hence, in this article it is implicit that "laws" means "physical laws".

Laws in physics do not contain any *ceteris paribus*-clauses, in contrast to so-called "laws" in many other disciplines. The reason is obvious, if one accepts my account of physical laws to be given in this article. Earman and Roberts (1999) have the same view, based on other arguments.

The set of laws seems to be a rather heterogeneous collection, even if we consider only physical laws, and I am unable to give a fully unified account of them. But I will discern some types which together at least cover all the well-known examples.

3. The Logical Form of Laws

A common but not undisputed view is that laws have the logical form $\forall x(Ax \rightarrow Bx)$, i.e., that they are universally generalised conditionals, UGCs, for short. (Adherents to the theory that laws are relations between universals hold that such relations provide the metaphysical grounds for calling a true UGC a "law".) Some simple laws are easily seen to fit this schema, such as "All pieces of metal expand when heated", or "All portions of gas expand in proportion to increase of temperature when heated under constant pressure". But these are of lesser interest; laws, properly so called by scientists, are more precise. For example, the rather imprecise sentence about the expansion of metals under heating has been replaced by a family of precise laws that for each metal states a coefficient for the increase in length per unit length and unit increase in temperature.

Laws that relate quantities to each other in the form of equations are not obviously of the UGC form; some interpretative work is needed to show that. Consider, for example, the law of gravitation:

$$f = \frac{Gm_a m_b}{r_{ab}^2} \tag{1}$$

which gives the gravitational force f between two masses m_a and m_b at a distance r_{ab} from each other. (G is the universal gravitational constant.) Bodies are attributed mass, and force and distance are attributed to pairs of bodies. These quantitative attributions can be expressed in the notation of predicate logic as:

The body a has mass m_a : $M(a, m_a)$

The body b has mass m_b : $M(b, m_b)$

The distance between a and b is r_{ab} : $D(a, b, r_{ab})$

The gravitational force between a and b is f: $F(a, b, f)$

Equation (1) is valid for all pairs of bodies, so the implicit generalisation is to all pairs of bodies. The letters symbolising mass, distance and force magnitudes, i.e., m_a, m_b, r_{ab} and f_{ab}, are functions of the variables a and b. We quantify over material objects.[1] Now the complete law of gravitation can be expressed as:

Law of Gravitation:

$$\forall a \forall b [M(a,m_a) \& M(b,m_b) \& D(a,b,r_{ab}) \& F(a,b,f_{ab}) \leftrightarrow f_{ab} = \frac{G m_a m_b}{r_{ab}^2}] \tag{2}$$

This sentence is not exactly of the canonical form $\forall x(Ax \rightarrow Bx)$: it is a biconditional instead of a conditional, and it is a double generalisation, instead of a single one. But these are minor points; to include this and similar cases, we could simply say that laws are universally generalised conditionals or biconditionals.

However, it is well known that many true sentences have this form without being laws. ("All prime ministers of Sweden are shorter than 2 m.", is a case in point, taking "are" non-temporarily.) So being a true, universally generalised conditional or biconditional is at most a necessary condition for being a law; our problem is to say what more is needed.

It is clear that we need a criterion for distinguishing between two classes of true sentences of this form, laws and the rest, usually called accidental generalisations. This was the central problem emerging in Goodman's (1946) seminal paper, where he discussed the problem of distinguishing between true and false counterfactuals. He found that true counterfactuals were associated with laws, whereas false ones were associated with accidental generalisations. But then, what is the distinction between true UGCs being laws and those being accidental generalisations? Since Goodman was a staunch empiricist and nominalist he tried, unsuccessfully, to solve the problem without drawing on modal notions.

1 So the *application* of the law of gravitation presupposes that we have identity criteria for bodies. The law itself does not presuppose the existence of bodies; it would be vacuously true if there were no bodies. But, of course, we would never be able to discover this law if there were no bodies.

His conclusion was that no such distinction could be drawn and the reader is tempted to conclude (though Goodman did not) that we need stronger resources than first order predicate logic for this task. So the question is: what further conditions than being a true UGC should a sentence fulfil to count as a law-sentence?

A very common idea is that laws are, in some sense, necessary. This form of necessity is often referred to as *natural*, *physical* or *nomological necessity.* In Section 10 I will discuss the relation between the predicates "... is a natural law" and ".... is necessary". Many philosophers argue that a sentence is a law because it is necessary, but in my view it is the other way round. That is to say, I will first give an account of why some sentences in physical theories are labelled "laws", and then explain the predicate "physically necessary" using the predicate "natural law".

4. Semantics and Ontology

Saying that we quantify over physical bodies, as in equation (2), when we express a law in first order predicate logic entails a commitment to bodies as referents for the variables. This is uncontroversial, but what about the existence of forces, masses, electromagnetic fields, etc., i.e., all the quantities in physics? Do they exist?

Clearly, we may consistently hold that, e.g., Newton's second law, $f = ma$, is true, while denying that there are any forces, masses or accelerations. Using the predicates $M(x, m_x)$ for "mass of x is m_x", $A(x, a_x)$ for "acceleration of x is a_x" and $F(x, f_x)$ for "force on x is f_x", Newton's second law is:

$$\textbf{Newton II}: \forall x[M(x,m_x) \& A(x,a_x) \& F(x,f_x) \leftrightarrow f_x = m_x a_x] \tag{3}$$

If this law is true, but not vacuously so, there exists at least one object being the referent of the variable x and this referent can be attributed the three quantities FORCE, MASS and ACCELERATION fulfilling the condition $f = ma$.

Prima facie, one might think that quantities are the referents of quantitative predicates. But there is no need to reify. There must be a referent for the singular term in a true sentence, but the predicate in a true sentence need not refer; it suffices that the object talked about belongs to the extension of the predicate. So I will adopt a nominalist stance about quantities and all general terms; they do not refer to anything.

Since I do not invoke universals as referents to quantitative predicates in my ontology, I can allow myself to use the word "quantity" as short for "quantitative predicate". In order to avoid any use-mention confusion I use SMALL CAPITALS when talking *about* quantities = quantitative predicates.[2,3]

The reader may observe that FORCE is a three-place predicate in Newton's law of gravitation (and in Coulomb's law) whereas in Newton's second law it is a two-place predicate. Furthermore, in Newton's second law it may take vectors[4] as arguments at the second argument place, whereas this is not so in the law of gravitation. This tension may be resolved by recognizing that expressions of the form "the force between x and y is z" may be viewed as short for "The magnitude of force on x is z ∧ the magnitude of force on y is z ∧ the forces are oppositely directed" (assuming as usual that no other bodies are sufficiently close to these two and that the bodies have no charge, as is the usual assumption when discussing the law of gravitation). The differences in syntax for "force between" and "force on" do not lead to any incoherence; as usual, the context is sufficient to determine what the label "f" stands for in a particular case.

I guess that some readers, those who call themselves realists, now are inclined to ask: "But do you really deny that there are masses, forces, electromagnetic fields, energy, etc., in the real physical world? Don't we have good reasons to say that these things exist and that our discovery of them is the best explanation for the success of physics?" This argument, which is of the form "inference to the best explanation" is often rehearsed by realists as their core argument for scientific realism.

My reply is: what one counts as the *best* explanation for the success of science, in this case physics, depends very much on one's metaphysical world view. What to count as a scientific explanation is a highly controversial issue, and the question about the *best* explanation is, if possible, even more controversial. Van Fraassen, to mention the most well-known anti-realist, holds that the best explanation for the success of science is that theories are empirically adequate; see van Fraassen (1985).

2 A quantitative predicate is a general term with well-defined rules for application (given in the SI-system); it is not merely a word or string of words. Therefore I need something else than quotation marks when indicating that I talk about such predicates.

3 If we simply define the property of having mass as belonging to the set of objects satisfying the predicate "mass of ... is ... kg", and similarly for other quantities, there is of course neither any problem, nor any gain, in accepting that quantitative predicates refer to properties and relations. The real ontological dispute is between those who hold that properties and relations are something else than mere sets of objects and those who deny that.

4 These vectors are mathematical objects, which I accept in my ontology; but there is no need to assume that a vector in the mathematical sense represents, or corresponds to, a physical universal. Moreover, numbers, and all mathematical objects constructed from numbers, are most naturally viewed as individuals, not universals.

In general, I see no added explanatory value in assuming that quantitative predicates refer to physical universals. Predictive power is the prime epistemic demand upon scientific theories, and explanatory force is to a great extent context sensitive. Two persons agreeing about a certain theory's testability and predictive power may nevertheless disagree vastly about its explanatory value, due to their background assumptions and world views. A particularly illustrating case is quantum mechanics, where all agree on its astonishingly accurate predictive power, whereas there are still, 90 years after its formulation, profound disagreements about its interpretation. The different interpretations are clearly based on different metaphysical presuppositions. The conclusion to be drawn is that explanatory power cannot be used as an argument for realism about physical properties and relations; it begs the question.

Perhaps the most severe problem for those who believe that quantitative predicates refer to properties is to provide identity criteria for such properties. The problem is that quantitative predicates can be transformed to each other via natural constants. For example, if they hold, as I guess they would do, that *length* and *time* are different properties, they have a problem with the common convention of putting the velocity of light equal to unity without dimension! Doing so enables us to measure distances as times, i.e., to hold the quantities LENGTH and TIME are coextensional predicates (and we are accustomed to talking about lengths in time units in astrophysics). One cannot at the same time accept that putting $c = 1$ without dimension is a mere convention and still distinguish TIME and LENGTH as referring to different properties.

Henry Kyburg (1997) discussed the ontological status of quantities and arrived at the position that quantities are functions whose ranges are magnitudes. One may think that Kyburg assumes that magnitudes are properties of physical objects, state of affairs or events. If so, I beg to disagree; Carnap's view, that values of quantities are real numbers, is all we need.

5. Induction, Concept Formation and Discovery of Fundamental Laws

Our belief in laws of nature is grounded on observations of results of systematic experiments. In most cases the connections between a particular law and observations are indirect, being transmitted by long chains of derivations, assumptions about measurement instruments, etc. For example, one cannot *directly* observe electric fields and electric charges and observe whether values of these quantities instantiate or conflict with Maxwell's first equation. No hypothesis, or law, can be tested in isolation; in testing we always assume a certain amount of background information, which could contain mistaken assumptions. This conclusion has often been called the Duhem–Quine thesis, albeit the exact formulation of this thesis is a matter of debate.

Sometimes we observe a regularity in a series of experiments and sometimes this regularity is still observed when the experiment series is prolonged, in which case we make an inductive inference to the general conclusion. The formulation of such a generality is *sometimes* accompanied by the introduction of a new quantity, a quantitative predicate so far not thought of. In such cases the inductive generalisation is a candidate for being a scientific law. These two steps are fundamental in the development of a new theory and, as we will see in the case of classical mechanics and electromagnetism, to be discussed in Sections 6 and 8, this is how *fundamental* laws are established.

There is an amount of circularity in the application of a physical theory to concrete situations. Consider, for example, electromagnetism; in order to determine whether a system is sufficiently isolated we need to know whether there are any measurable electromagnetic fields from external sources affecting the system in question, and that we cannot know unless we have determined a way of measuring these fields. In principle no system is ever completely isolated of course; the rest of the world is not an infinite distance away and hence the probability of interaction is not exactly zero. So the question of isolation is a practical question: is the system being observed sufficiently well isolated so that possible interactions with the rest of the world only affect the system's state within the margins of error? But this is precisely the reason why one cannot, other than analytically, separate discoveries of laws and the introduction of precise quantities in theory development. If we fail to isolate the system sufficiently, we will sooner or later hit upon a case where unknown factors interfere and disturb the predicted outcomes, thus producing a counter instance.

Neither in practice, nor in the conceptual analysis, can we proceed by first defining a set of new quantities and then performing experiments to see how they relate to each other. Observing, experimenting and developing quantitative concepts are inseparably intertwined, as will be clearly shown in Sections 6 to 9. This is why fundamental laws at the same time are implicit definitions of new predicates and have empirical content.

5.1 Laws, physical theories and observations: top-down or bottom-up?

My conception of physical theories might be described as "bottom-up": theory construction starts with descriptions of observed regularities. By contrast, the common view is that a physical theory is a mathematical structure built upon some abstract principles, whose laws are declared to be fundamental in the logical sense. By starting from "above", the concepts occurring in fundamental laws are not yet given any physical interpretation; they have merely mathematical relations to each other. The physical interpretations are only given when part of this structure, the empirical "edges", are compared with observations, or, as in the semantic view of theories, a part of the structure is thought of as a mapping of observed phenomena.

In this top-down view one faces the task of explaining how mathematical equations and functions relate to observations. According to classical empiricism, it is provided by "coordination principles" (Reichenbach, 1920). In the words of Friedman (2001, p. 76): "They serve as general rules for setting up a coordination or correspondence between the abstract mathematical representations ... and concrete empirical phenomena to which these representations are intended to apply". Somewhat similar views are expressed by many philosophers of physics, van Fraassen (1980) being one clear example.

The problem with this statement is the word "phenomena". In order to set up a correspondence between general statements in a theory ("abstract mathematical representations") and something else, you must describe that something else, "the phenomena", in some way. One cannot establish any correspondence between a mathematical structure and something which is not yet organized as the content of a perception. In other words, the correspondence is a correspondence between a mathematical structure and a part of the contents of our observations, i.e., descriptions of observations. The question is what predicates to use in such descriptions? If these are purely empirical predicates whose application rules are fully independent of any theory, we may truly ask how there could be a correspondence between "phenomena" thus described and theoretical statements constructed independently of any description of empirical "phenomena".

Think, for example, of electromagnetic theory: it describes the dynamics of charged particles in electric and magnetic fields; it relates electric and magnetic fields and the motion of charged particles to each other. But we cannot directly observe electric fields, magnetic fields, or charges. What we observe are physical bodies in space and time. (Observing the value of a meter of some kind is obviously an observation of a body at a certain place.) In order to establish a correlation between descriptions in terms of moving bodies and electromagnetic predictions we need to sort out those bodies that are sensitive to electric and/or magnetic fields and compare their motions with theory. But in doing so, we use the electromagnetic concepts. For example, we attribute charge to *some* bodies and different charges to different bodies with the same mechanical properties. So it is no longer any theory-independent individuation of things to which electromagnetic properties are attributed in the empirical realm. Descriptions of "phenomena", in the sense intended by Friedman and others, depends on the theory.

I cannot see how it is possible to sort out those motions of observable bodies that are related to electric and magnetic interactions without using electromagnetic concepts, or some others with the same extensions. So the correspondence does not have the character of a correspondence between items in two conceptually independent realms.

It is of course easy to set up a correspondence between two domains containing different types of entities, if the individuation of things in one domain is determined by the individuation of things in the other domain, as in the "correspondence" between facts (in German: "*Tatsache*") and sentences (in German: "*Sätze*") in Wittgenstein's *Tractatus.* No empirical investigations are needed, or indeed possible, to check this correspondence; it is in a profound sense trivial. (Wittgenstein claimed that it could be shown, albeit not talked about; but I doubt the intelligibility of this statement.) Certainly, the relation between theory and empirical evidence is not of this kind. So how should we understand the correspondence between mathematical structures and empirical phenomena from the viewpoint of Friedman, Reichenbach and others using this concept?

In fact, I do not see how one could give a substantial content to the notion of correspondence in the sense intended by Reichenbach and his followers. Hence I do not think the notions of correspondence or coordinating principles are useful for understanding the relation between theory and empirical evidence; either the correspondence is completely trivial or else it is impossible.

A similar critique may be directed against van Fraassen's notion of isomorphism between "the empirical substructure" of a model of a theory and "appearances", the latter being characterized as follows: "[T]he structures that can be described in experiential and measurement reports we can call *appearances*" (van Fraassen, 1980, p. 64). The problem with this conception is that experimental and measurement reports are almost never void of theoretical predicates, such as "mass" or "electric field", and these are defined within a system of equations. Hence most appearances cannot be described without employing theoretical concepts. How can we describe structures of appearances without using theoretical concepts? So the isomorphism between an empirical substructure and appearances cannot be conceived as an independent empirical check on the model, or as a relation between theory and evidence.

Van Fraassen's conception of scientific theories is one version of the general idea that a theory is a set of models, and my critique of van Fraassen's conception of the relation between model and theory applies generally. Models must be described in order to be explicitly related in any way to a theory, and descriptions of models require theoretical predicates.

Reflections similar to these might have been the reason why Kuhn (1970) drew the conclusion that there are no theory-independent observations whatsoever. This general conclusion is false; there is a meagre basis for theory-independent observations, in physics exemplified by positions and motions of nearby visible bodies.

But Kuhn had a point; sometimes we introduce new theoretical concepts when generalising our observations, the result being what I have called "fundamental laws".[5]

Summarising this section, my view is that observation reports ordinarily so called in many cases utilise theoretical predicates. But it is possible to discern a subset which do not utilise any such theoretical terms. This subset is the ultimate empirical basis for theory construction. Some fundamental theoretical concepts are constructed during the process of inductive generalisations from such observation reports, and using these we can continue and explicitly define new useful quantities and thus construct an empirical theory.

I will now show that classical mechanics, relativity theory and classical electromagnetism fit my account of laws and how they are based on observations.

6. Laws and Fundamental Quantities in Classical Mechanics

6.1 The discovery of momentum conservation and the introduction of MASS and FORCE

Classical mechanics consists of kinematics and dynamics. Kinematics describe the motion of physical bodies, usually called "particles" in the theoretical exposition, since their inner structure is not considered, while dynamics is the theory about interactions between particles.

Classical mechanics is from an *epistemological* point of view the fundamental physical discipline; motions of bodies are clearly the most directly observed events. But it is also basic from a conceptual perspective because all physical quantities ultimately are defined in terms of TIME, DISTANCE and MASS. This fact is easily recognised when looking at the definitions of the SI units.

TIME and DISTANCE are the two fundamental quantities in kinematics; these two are used when describing particles' positions, velocities and accelerations. These quantities are operationally defined in terms of how to use meter sticks and clocks in measurements.[6]

5 In the postscript to the second edition of *The Structure of Scientific Revolutions*, Kuhn used the concept of *disciplinary matrix* instead of the concept of *paradigm.* The first component of the disciplinary matrix is the set of symbolic generalisations, and it seems pretty clear that by this term he refers to what we usually call scientific laws. But why did he not use the term "law"? One reason was, I think, that using the term "law" one is inclined to miss his point that the terms in a theory get their meaning implicitly (just as I argued above), by being used in the theory, not by any explicit definition.

6 This view has often been criticized with the argument that changes of operational definitions would change the meaning of quantitative predicates, which is taken to be unacceptable. My reply is: so much the worse for the concept of meaning. When the definition of the meter unit was changed from being based on the meter prototype to a certain distance travelled by light in vacuum, the extension of the predicate "one metre" underwent a slight change, since its precision increased. But since I have no need for referents of quantities, there is no conceptual problem here. Why bother about meaning?

In performing such measurements we take for granted that those physical objects utilised as measurement devices are invariant when being moved from one place or time to another. We take for granted that meter sticks do not change length and that clocks tick with the same speed when moved from one place to another. These are not purely empirical assumptions; if we have determined concrete procedures by which to compare time intervals and distances, i.e., instructions about time and distance measurements, we have stated how to apply the truth conditions, for example for the statement that two objects at different places or at different times have the same length.

But then, how is it possible to replace, e.g., a time unit with a better one? Why did we replace the definition of one second as 1/86,400 of the diurnal day with a number of oscillations in a certain kind of electromagnetic radiation? Well, one reason was that according to our theory of gravitation, the diurnal day varies slightly, whereas quantum theory tells us that nothing affects the frequency of electromagnetic radiation. This topic is discussed at considerable length in van Fraassen (2008, p. 130 ff).

Determining fundamental units, i.e., determining how to apply fundamental units (such as metre and second) in practical measurements, is decided by IUPAP (the International Union of Pure and Applied Physics), and these decisions may change; for example, in the 1960s it was decided to change the metre definition from an ostensive one ("One metre is the length of the meter prototype in Sevres") to one based on the distance travelled by light in a vacuum during a very short period of time. But this change did not affect the lengths attributed to objects (within a very small margin of uncertainty) and since this is what counts, and not the intension of the expression "length", we may conclude that theory-ladenness of this predicate is innocent.

It may be observed that "fundamental" here means "fundamental relative to the theory at hand". It is no claim about fundamentality in an absolute or metaphysical sense.

The reason why we need two fundamental quantities in kinematics is that, so long as we do not consider relativity theory, we need two kinds of measuring instruments (meter sticks and clocks) to measure and observe kinematic quantities.[7] One also needs some geometry and arithmetic in doing mechanics, but these disciplines belong to mathematics; no measuring instruments are needed.

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7 When we proceed to relativity theory, we can, since the velocity of light is a universal constant, reduce the number of fundamental quantities to only one, namely, TIME, since distances can be expressed in terms of times for light travel. So considering physics in its entirety we may say that only one quantity is fundamental. But we have arrived at this conclusion using classical theories as starting points (i.e., classical mechanics and electromagnetism) and these theories presuppose two fundamental quantities, TIME and LENGTH. One might say, following Wittgenstein, that once we have climbed the ladder we may throw it away!

How, then, do we proceed to dynamics? The actual history is illuminating. By using only kinematical quantities Descartes failed to construct an empirically adequate theory about interactions between bodies. But some years later John Wallis took the first step in advancing a successful dynamics, according to Rothman (1989, p. 85). In a report to the Royal Society in 1668 Wallis described his measurements of collisions of pendulums. Huygens and Wren performed similar experiments. All three found that there is a constant proportion between the velocity changes of two colliding bodies:

$$\frac{\Delta v_1}{\Delta v_2} = constant \tag{4}$$

which can be written:

$$k_1 \Delta v_1 = -k_2 \Delta v_2 \tag{5}$$

The minus sign is introduced so as to have both k_1 and k_2 positive.

By testing with different bodies, they found that the constants really are constants following the bodies, i.e., they are permanent attributes of the bodies. These constant attributes are their *masses*, and we may choose a mass prototype giving us the unit. So we have:

$$m_1 \Delta v_1 = -m_2 \Delta v_2 \tag{6}$$

This is the law of momentum conservation, a law that from an epistemological point of view must be said to be fundamental in physics.[8]

The very first line of Newton's *Principia* is the definition "The quantity of matter is the measure of the same, arising from its density and bulk conjointly" (Newton et al., 1687/1999).[9] This quantity he then calls "mass". But how can we measure density without using the quantity mass? In fact, Newton relied on the findings of Wallis, Huygens and Wren, as is clear from the *Scholium* following corollary VI in the first section of the first book of *Principia.* Wallis, Wren and

8 Konopinski's (1969) account of classical mechanics begins similarly by considering collisions; he states that "The Principle that *the total momentum of any isolated system is conserved* forms part of the basic framework on which all physical theory has been constructed" (p. 35).

9 Mach (1960, p. 241) criticized this definition and rightly observed that mass must be defined using observations of interactions between bodies: "Definition 1 is, as has already been set forth, a pseudo-definition. The concept of mass is not made clearer by describing mass as the product of the volume into density as density itself denotes simply the mass of unit volume. The true definition of mass can be deduced only from the dynamical relations of bodies".

Huygens had introduced the concept of quantity of matter without using the word “mass”.

If we now divide both sides of equation (6) with the collision time, we get (neglecting the difference between differentials and derivatives since this is of no relevance for the present argument):

$$m_1 a_1 = -m_2 a_2 \tag{7}$$

Let us further introduce the term “force”, labelled “f”, as shorthand for the product of mass and acceleration. This gives us Newton’s second and third laws:

$$N2 : f = ma \tag{8}$$

$$N3 : f_1 = -f_2 \tag{9}$$

Thus we have got Newton’s second and third laws based on an observed regularity, namely, momentum conservation during collisions between bodies.

Forces are often thought to be causes of accelerations; when a body changes its velocity, we say it has been affected by a force. This force is the momentum change of another body, perhaps a remote one, in which case the momentum exchange is transmitted by a field. Thus the claim that force is *defined* as *dp*/*dt* is compatible with the common conception that forces are causes; it is the momentum change of another body that is the cause of an observed body’s momentum change. However, if we want to use causal idiom, we must say that cause and effect occur simultaneously. Furthermore, the notion that forces are causes is hardly compatible with my stance that quantitative predicates do not refer to anything, since causes are normally presupposed to be a kind of entity.

6.2 Types of laws in classical mechanics

I have so far discerned three different types of laws in classical mechanics:

- *Fundamental laws* are those UGCs which at the same time express generalisations about observations and function as *implicit definitions* of new quantities. (I will generalise and give a more precise definition of a fundamental law in Section 9.)
- *Explicit definitions* of new quantities, i.e., quantitative predicates.
- *Derived laws*, which logically follow from fundamental laws and explicit definitions of new quantities.

Let us now look at the law of gravitation to see whether it fits into one of these categories:

$$f = G\frac{m_1 m_2}{r^2} \tag{10}$$

The force can be replaced by its definiens, *ma*, so we have (identifying $m = m_1$)

$$a = G\frac{m_2}{r^2} \tag{11}$$

Thus we can derive the acceleration of a body,[10] given knowledge about the mass of the other gravitating body and its distance. Now we can check the law of gravitation by measuring the body's acceleration and we may find that prediction and observation always coincide. So the law of gravitation seems to be a purely empirical law. But isn't this remarkable? How could it be that the quantities MASS, ACCELERATION, FORCE and DISTANCE, defined independently of the law of gravitation, without exception also satisfy this extra condition? Collisions between bodies and gravitational interactions seem to be quite different kinds of events; it appears to be a cosmic coincidence, a brute fact that cannot be further explained. But surely there must be an explanation.

The first step, as is well known, is to realize that we are talking about two different mass concepts, INERTIAL MASS and GRAVITATIONAL MASS; INERTIAL MASS is defined using the regularity observed in collisions, GRAVITATIONAL MASS is defined using the regularity observed when bodies interact at distances. But this does not really remove our bewilderment, for now one asks instead: how could it be that the gravitational and inertial masses of all bodies are proportional? Newton saw it, but had no explanation.

It was Einstein who solved the problem within the general theory of relativity. The solution is simply that gravitational and inertial mass *are* the same quantity, since gravitation and inertia at bottom are not different kinds of phenomena. This is the basic idea in general relativity.

This step, by the way, strongly supports my view, presented in Section 4, that quantities should be understood as quantitative predicates, not as physical universals. It is not only superfluous to assume that quantitative predicates refer to universals; it also raises obstacles for our understanding of relativity theory. If we accept GTR and assume that quantitative predicates refer to universals, we must either say that the two predicates GRAVITATIONAL MASS and INERTIAL MASS refer to the *same* universal, or that they refer to two different universals with the same extension. Both alternatives give us more problems

10 I have here presumed that we talk about a body only involved in gravitational interaction.

than they solve. The very point of a quantitative predicate is that its identity is determined purely extensionally and that removes any need to postulate a referent for it.

Einstein's crucial step was to generalise the relativity principle used in the special theory of relativity. Special relativity is restricted to inertial, i.e., non-accelerated systems. In the general theory of relativity this restriction is removed; all coordinate systems, whether accelerated or not, are equally legitimate and the laws should have the same form in all of them. This has the consequence that the distinction between gravitation and inertia disappears. (Einstein's argument was that we cannot by local observations decide whether the force on a body is gravitational attraction from another body, or inertia due to the system being accelerated.)

Returning to classical mechanics considered *per se* and disregarding relativity theory, we may conclude that INERTIAL MASS is determined by the law of momentum conservation, that GRAVITATIONAL MASS is determined by the law of gravitation and FORCE is explicitly defined as *ma.* Thus the law of gravitation satisfies the definition of a fundamental law (an empirical regularity and simultaneously an implicit definition of a new concept).

In classical mechanics we now have two fundamental laws, momentum conservation and the law of gravitation and two fundamental quantities defined by these laws. Together with TIME, DISTANCE and functions of these, we have a complete set of fundamental quantities in classical mechanics. All other quantities, such as FORCE, ENERGY, WORK, POWER, ANGULAR MOMENTUM, etc., can be explicitly defined in terms of the kinematic concepts + INERTIAL MASS + GRAVITATIONAL MASS.

There are several different theory formulations of classical mechanics, but they all rely on the kinematical quantities + MASS, although one might not think so at first glance. The fundamental notions in for example Hamilton's and Lagrange's versions of classical mechanics are generalised coordinates and their corresponding momenta, which are treated as independent variables. But when applying the theory to observable phenomena, one identifies MOMENTUM as *mv*, where *v* is measured in the chosen generalised coordinate. And just as in my account, FORCE is introduced as a derivative notion, this time as the derivative of a potential function. So the empirical foundations in any version of classical mechanics are descriptions of observed regularities in which MASS is used. This is the reason why I concur with Gauss, who famously held that TIME, DISTANCE and MASS are the fundamental quantities in physics.

Being an empiricist, I believe it crucial to state the empirical basis consisting of theory-independent observations for any empirical theory we may consider. In physics (and I would claim also in the rest of natural science) this basis consists of observations of bodies at particular places at particular times.[11]

In biology and chemistry we may be interested in how things smell or taste, or what colour they have, but still, the things observed are bodies at particular places. So I do not see any alternative to taking the kinematical concepts of classical mechanics, i.e., *time, distance* and the time derivatives of distance, as pretheoretical and given in advance, whereas *mass* (inertial and gravitational) is introduced with our formulation of the laws of momentum conservation and gravitation respectively.

The number of fundamental quantities implicitly defined and the number of fundamental laws must be the same. All other useful quantities, such as FORCE, KINETIC ENERGY, POWER, etc., can then be introduced as explicit definitions in terms of previously defined quantities.

It is impossible to state the law of momentum conservation without using the concept of INERTIAL MASS and this is the crucial point. Equation (6), interpreted as short for an UGC ("For every pair of colliding bodies …"), is at the same time an inductive generalisation from a set of observations and a contextual, i.e, implicit, definition of the quantity INERTIAL MASS. By a similar reasoning we may conclude that equation (10) at the same time is an implicit definition of GRAVITATIONAL MASS and an inductive generalisation of observations.[12]

In the very construction of quantitative concepts in classical mechanics we use fundamental laws of nature as definitions, or better, discovering new laws and constructing new quantitative concepts go hand in hand; these are closely related processes. The traditional view that one first has to define one's concepts and then apply them in describing one's observations is incorrect. This is, by the way, one good reason to dismiss the analytic/synthetic distinction as a fundamental premiss in epistemology.

Newton's second law is an *explicit* definition of the quantitative predicate FORCE; it does not express any generalisation of observations and "force" can always be replaced by "mass times acceleration". No one has ever *directly* observed a force and compared it to the product of mass times acceleration.

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11 Bridgman (1960, p. 58) once expressed an almost similar opinion: "What we observe are material bodies with or without charges (including eventually in this category electrons), their positions, motions, and the forces to which they are subject". I disagree on two points; (i) we never observe forces, we only observe moving bodies, and (ii) electrons are not bodies.

12 My view that some laws are implicit definitions has some affinities with Herbert Simon's (1970) view on axioms of physical theories: "In the former case, new definable terms are likely to enter the system embedded in statements of physical law. These statements will partake of the nature both of definitions and of laws" (pp. 22–23).

The critical reader might point to calculations in statics where we attribute several forces to an element in, e.g., a building. Nothing moves, so there are no accelerations; still, we analyse the stability of the building by calculating forces at different points. Doesn't this indicate that in physics we assume forces? No. Consider an element in a construction. Since it is not moving, the vector sum of all forces upon this element is zero. We can replace each force f_i with its definiens $m_i a_i$ and the total acceleration is of course zero. Talking about forces is convenient but logically superfluous.

As argued earlier, we have no reason to assume that quantitative predicates refer to universals, in this case that the predicate FORCE refers to a force; we only need to assume the existence of those things talked about, i.e., that the singular terms refer. We may paraphrase a sentence attributing a force to an object as in the second paragraph of Section 4.

A similar conclusion can also be drawn about other predicates, such as colour words. We benefit greatly from our colour discrimination ability and our use of colour predicates, but that does not entail that we have reason to believe in the existence of referents of colour words in predicate position. Why not? Because postulating referents for predicates has no additional testable consequences beyond those of the sentences in which the colour words are used; no empirical evidence could be had for such assumptions.

In ordinary discourse we talk as if colours exist. But if so, how many colours are there in reality? It is a well-known fact that different cultures divide the spectrum differently, so identity criteria for colours are relative to culture, and there are no arguments for holding any one way of differentiating colours is the correct one; we make colour distinctions when we need them. The sensible conclusion is that there are no colours in the real world.

Newton's third law is a consequence of momentum conservation and the definition of force. There are lots of such relations between quantities derivable from the definitions, some of which are called "laws". So if we want to keep close to the established use of the word "physical law", we should say that some laws are consequences of other laws.

I will now extend the discussion to fundamental laws in two other theories: the special theory of relativity and classical electromagnetism.

7. Laws in the Special Theory of Relativity

The special theory of relativity is built on two fundamental postulates, the relativity principle and the constancy of the speed of light.

The relativity principle says that experiment results should be the same in all inertial systems, or, in Einstein's words:

Special principle of relativity: If a system of coordinates K is chosen so that, in relation to it, physical laws hold good in their simplest form, the same laws hold good in relation to any other system of coordinates K′ moving in uniform translation relatively to K (Lorentz, 1952, Part A, §1).

How do we know this is true? The basic reason is derived from an objectivity demand on physical descriptions, namely, that the physical content of the description of the state of a physical system should be independent of the observers' perspective. Thus if two observers move with a constant velocity relative to each other, they should give similar descriptions of a physical system they both observe. So we know the relativity principle is true because we hold it true; apparent violations are explained as mistaken observations of, e.g., uniformity of motion of the observer.

Galileo apparently was the first to formulate a relativity principle and Newton followed suit. However, Newton did not view it as a fundamental principle for objective descriptions; he claims to have derived it as a corollary (Corollary V) in *Principia.* But that derivation is a non sequitur, as shown by Harvey Brown (2005, ch. 3). I think it fair to say that Einstein was the first to conceive it as a fundamental epistemological principle, a requirement of observer independence.

The relativity principle does not fit into any of the three categories of laws so far identified, and that is perhaps a reason why it is not called a law. It is a condition for objective descriptions of nature.

The constancy of the speed of light is generally stated as a basic postulate of special relativity. However, it is in fact no fundamental law; it follows from the relativity principle, Newton's laws and Maxwell's equations, as shown by Feynman et al. (1964, ch. 18, p. 5) and Dunstan (2008). Dunstan (2008, p. 1865) concludes:

Special relativity derives directly from the principle of relativity and from Newton's laws of motion. The parameter values of a = 1 or k = 0 were compatible with all experimental information available in Newton's day. However, Maxwell's equations permit a more accurate determination, from Faraday's and Ampère's experimental work and Maxwell's own introduction of the displacement current. Discussions of the Michelson and Morley experiment and of theories of the ether are quite unnecessary. The behaviour and the mechanism of the propagation of light are not at the foundations of special relativity.

The parameter a is the transformation formula $1/\sqrt{1-\epsilon_0\mu_0 v^2}$ and $k = -\epsilon_0\mu_0$, i.e., $k = -c^{-2}$. The constancy of the speed of light is thus a derived law.

It is rather well known, too, that Einstein was not primarily motivated by the negative outcome of Michelson–Morley's attempt to measure the ether-wind when he stated that the velocity of light is constant and an upper limit for all velocities. His fundamental inspiration was the thought experiment of an observer travelling with the same speed as an electromagnetic wave front. He realised that the observer would see the front as a stationary electromagnetic field and that contradicts Maxwell's equations. Hence, the assumption of an observer travelling at the speed of light must be wrong. So Dunstan's proof is a mere spelling out of an older insight.

It is interesting to note that we begin with two kinematical quantities in classical mechanics, TIME and DISTANCE, which require two distinct kinds of measurement devices, and then, based on this theory + electromagnetism, we have constructed a more general theory, the special theory of relativity, which entails that we can reduce the number of fundamental kinematical quantities to one and we no longer need any meter sticks, only clocks![13]

8. Laws of Electromagnetism

The conceptual structure of electromagnetism is more convoluted than that of mechanics. The first thing we have to notice is that although the words "electricity" and "magnetism" were used long before we had any theory about these phenomena, the more precise quantitative concepts were not fully developed until the publication of Maxwell (1873) and his introduction of DISPLACEMENT CURRENT.

The second thing to notice is that one cannot find any single law in electromagnetism that individually introduces a new quantity; it is only jointly that a set of laws implicitly defines the electromagnetic quantities.

There is general agreement that the fundamental laws are Maxwell's equations and Lorentz's law (see, e.g., Feynman et al., 1964, ch. 18), so these laws together should function as joint implicit definitions of the fundamental quantities in electromagnetism. And indeed they do.

The effects of electromagnetic interactions are observable as changes of the motions of bodies.[14] So in the electromagnetic theory we need a law that connects electromagnetic quantities to mechanical quantities attributed to bodies, such as mass and velocity, which is done by Lorentz's law:

$$\mathbf{F} = q(\mathbf{E} + \mathbf{v} \times \mathbf{B}) \qquad (12)$$

where **F** stands for the force on a body, q for its charge, **v** for its velocity, **E** for the electric field and **B** the magnetic field. (Boldface letters stand for vector

13 The connected question about the number of fundamental dimensional constants is a topic of debate; see Duff et al. (2002).

14 There is no other option, as observed by, e.g., Born (1924, p. 189): "Electromagnetic forces are never observable except in connection with bodies".

quantities.) Then we need laws that implicitly define CHARGE, ELECTRIC FIELD and MAGNETIC FIELD. That is done by Maxwell's equations:

$$\nabla \cdot \mathbf{E} = \frac{\rho}{\epsilon} \tag{13}$$

$$\nabla \cdot \mathbf{B} = 0 \tag{14}$$

$$\nabla \times \mathbf{E} = \frac{\partial \mathbf{B}}{\partial \mathrm{t}} \tag{15}$$

$$\nabla \times \mathbf{B} = \frac{4\pi \mathrm{k}}{\mathrm{c}^2}\mathbf{J} + \frac{1}{\mathrm{c}^2}\frac{\partial \mathbf{E}}{\partial \mathrm{t}} \tag{16}$$

There are no independent definitions of the electromagnetic quantities, so these laws must also function as implicit definitions of these quantities. This means that the number of independent laws must equal the number of fundamental electromagnetic quantities. In order to see this clearly, we cannot count the number of equations in the form given above, since several quantities are vectorial quantities and each component of such a quantity is independent of the other. Furthermore, these equations are invariant under Lorentz transformations, so for the present purpose it is more convenient to express Maxwell's equations in Lorentz invariant form with the help of the tensor.

$$F^{\mu,\nu} = \begin{pmatrix} 0 & E_x & E_y & E_z \\ -E_x & 0 & B_z & -B_y \\ -E_y & -B_z & 0 & B_x \\ -E_z & B_y & -B_x & 0 \end{pmatrix}$$

its dual $F_{\mu,\nu}$ and the fourcurrent $\mathbf{J} = (\rho, J_x, J_y, J_z)$ as the two equations:

$$J^{\beta} = \frac{\partial F^{\beta,\alpha}}{\partial x^{\alpha}} \tag{17}$$

$$0 = \partial_{\alpha} F_{\beta,\gamma} + \partial_{\gamma} F_{\alpha,\beta} + \partial_{\beta} F_{\gamma,\alpha} \tag{18}$$

where the inhomogeneous equation expresses equations (13) and (16) and the homogeneous one expresses (14) and (15). Now, (17) and (18) are in fact each four independent equations, and since Lorentz's law consist of three independent equations, we have in total 11 equations. That equals the number of quantities we need to determine: three components of the electric field, three of the magnetic field, four components of the four-current and finally total charge. In other words, Maxwell's equations + Lorentz's law together completely determine the quantities ELECTRIC FIELD, MAGNETIC FIELD, TOTAL CHARGE and FOUR-CURRENT, given only the directly observable properties of a system. Hence, we may say that Maxwell's equations + Lorentz's law together constitute implicit definitions of the fundamental electromagnetic quantities; they are the fundamental laws of electromagnetism. Other electromagnetic quantities are explicitly defined in terms of the fundamental quantities and all other laws of electromagnetism are derivable from the fundamental ones + explicit definitions.

9. Fundamental Laws that Do Not Introduce New Quantities

There are quite a number of basic principles which generally are said to be fundamental laws, but which do not establish relations between quantities. One example is, as we just saw, the relativity principle. Three other examples are:

Conservation of charge: $\forall x$, if x is a closed system and q(x) is the total charge in x, then $\partial q/\partial t = 0$.
Pauli's exclusion principle: $\forall x \forall y \forall z$, if x is a quantum system, y and z are fermions belonging to that system and $y \neq z$, and if S(y) is the ordered quadruple of quantum numbers of y and T(z) is the ordered quadruple of quantum numbers for z, then $S(y) \neq T(z)$.
Quantization of interaction: $\forall x$, if x is a quantum system, x emits or absorbs energy E only in discrete portions $E = h\nu$.

For each of these I will now explain why we are prone to say that they are laws.

Charge conservation is one of the conservation laws, which all have the same form, i.e., expressing the conservation of a quantity in a closed system. The interesting thing with these laws is that we have no independent criterion for what to count as a closed system. In other words, if an experiment indicates that e.g., charge or energy is not conserved in a system, one has two options: either to reject the assumption that the observed system is closed, or to accept that conservation of the quantity is violated. A well-known example of this is the first experiments (around 1932) in which weak interactions (as they were later called) were studied. Neutrinos are produced in such interactions and they carry energy. But neutrinos were not known or observed; they very rarely interact. So the experiments seemed to violate energy conservation. This was also suggested by Bohr, but Pauli disagreed and instead held that the system was not closed; he suggested that a so far unknown particle had been produced and carried away the missing energy. A theory was developed and 20 years later new experiments confirmed the existence of neutrinos.

The conservation laws jointly define what we mean by a closed system, provided that the quantities involved are independently defined. But they do not satisfy the definition of a fundamental law given above, because *closed system* is not a quantitative concept. However, conservation laws are in an important respect similar to fundamental laws, as defined above, in that they are generalisations of observations and implicitly and partly define the theoretical concept *closed system*; each conservation law contributes to the determination of the identity criteria for things satisfying the predicate “closed system”.

Conservation laws are viewed as so certain that it is inconceivable that any physicist would have doubts. The reason is that using Noether’s theorem they can all be derived from symmetry requirements. Noether’s theorem says roughly that if a system is symmetric under a continuous parameter transformation, the conjugate quantity to that parameter is conserved. A little more precisely: to every differentiable symmetry generated by local actions, there corresponds a conserved current. In the derivation of Noether’s theorem one uses the Lagrangian, so the theorem does not apply to systems that cannot be modelled by a Lagrange function. This corresponds to systems not being closed, i.e., dissipative systems; hence conservation applies only in closed systems.

Thus, time translation symmetry entails energy conservation, spatial translation symmetry entails momentum conservation, rotation symmetry entails angular momentum conservation and gauge invariance entails charge conservation. (Gauge invariance is invariance under phase transformations of the electromagnetic vector potential.)

The symmetry requirements are thus the basis for the conservation laws. These symmetry requirements may in turn be understood as being part of objectivity requirements on descriptions of physical systems. For example, the requirement that the Lagrangian for a system be invariant under the transformation $t \rightarrow t + \Delta t$ is an objectivity requirement: the objective features of the system described by the Lagrangian do not depend on the choice of when to start the clock, i.e., when to put t = 0, which means that transforming a description given by one observer to another who has started his clock earlier (or later) should leave the description invariant. So the requirement of invariance under time translations is an objectivity demand. Similar considerations apply for spatial translations, rotations and phase transitions in electromagnetism.

Symmetry under certain parameter transformations is a necessary condition for objective descriptions of the physical world. The relativity principle is, as we saw, another such condition for objective description.

Pauli’s exclusion principle and *quantization of interaction* are two of the basic principles of quantum mechanics. They both describe properties of quantum systems. What, then, are the identity criteria for a quantum system?

Just as with closed systems, we have no independent criteria for the identity of quantum systems, so these two principles contribute to establishing identity criteria for quantum systems and for their representation in the formalism. For example, if an observation report purports to indicate violation of Pauli's exclusion principle, the scientific community again has two options, either to give up Pauli's exclusion principle, or to dismiss the assumption that the system was closed. All scientists would say that the condition of being closed was not fulfilled. In other words, if two or more fermions are found to have the same quantum numbers, they must belong to different quantum systems, which means that one may not construct a tensor product of their wave functions. For if we were to construct such a tensor product, we would treat the two systems as now being one system which interacts with the environment as a unit, and such a unit cannot contain two fermions with the same set of quantum numbers.

We may remember that in the quantum world one cannot identify quantum systems by spatiotemporal criteria, because of their wavelike behaviour during propagation. Individuation and identity among quantum systems are given by our theory, by the way we manipulate wave functions.

Two quantum systems prepared to be in the initial states $|\Psi\rangle$ and $|\Phi\rangle$ respectively, which do not belong to the same ray,[15] are thus distinguished as different systems and Pauli's exclusion principle applies within each system separately. So a fermion being part of the system $|\Psi\rangle$ can have exactly the same set of quantum numbers as a fermion in $|\Phi\rangle$. But if the two systems interact and the total state is the tensor product $|\Psi\rangle|\Phi\rangle$, no two fermions can be in the same state in this joint system. So Pauli's exclusion principle contributes to determining criteria for identity and individuation of quantum systems. This means that the new combined system must be treated as a unit when it interacts with other systems by exchanging energy, momentum or other conserved quantities.

That a closed system is attributed definite quantum numbers is a consequence of the fundamental quantum principle, discovered by Planck, that exchange of energy only occurs in discrete portions; in other words, interactions between quantum systems are quantized.

Thus Pauli's exclusion principle and quantization of interaction are in a general sense fundamental laws, albeit they do not fit my definition of fundamental quantitative laws because they do not express relations between quantities. However, there are profound similarities.

Fundamental quantitative laws have two features; they are generalisations of observations and they implicitly define a theoretical quantity. The first feature is also present in the conservation laws, Pauli's exclusion principle and quantization of interaction, which are all supported by observations (though in a more indirect

15 Rays are sets of wave functions and two wave functions $|\Psi\rangle$ and $|\Phi\rangle$ belong to the same ray if $|\Psi\rangle = c|\Phi\rangle$ for any complex number c.

way). The other feature is not exactly the same but it has a close analogue; a definition of a quantity must contain information about how to determine values of that quantity, whereas definitions of the concepts *closed system* and *quantum system*, respectively, require information about identity and individuation among systems satisfying these descriptions, since we quantify over them.[16]

One might ask: why not require the same of quantities? The answer is that we need no quantitative properties in the ontology. As already argued, there are no good reasons to say that quantitative predicates refer to properties; it suffices that they have extensions.

We may now generalise and make more precise the informal characterization of a fundamental law given earlier by generalising from quantities to theoretical predicates in general:

Definition of Fundamental law: A physical law is a fundamental law if and only if (i) it belongs to the set of implicit definitions of theoretical predicates used in a physical theory, (ii) it is supported by observations, and (iii) it is part of a theory which enables us to make testable predictions.

Another use of the expression "fundamental law" is to be found among adherents to the syntactic view of theories, such as Carnap and Gardner (1995) and Hempel (1970). In this tradition the intended meaning of "fundamental law" is "logically fundamental". It is well known that one and the same theory can be given different formulations with different laws being the fundamental ones in this logical sense; the best example is perhaps classical mechanics which can be given a Newtonian, Lagrangian, Hamiltonian or a d'Alembertian formulation, each with different axioms. So "fundamental" in this logical sense must be relativized to theory formulation.

By contrast, my conception of fundamental law is not relative to theory formulation. Those true universally generalised conditionals which satisfy the conditions in the definition given above are fundamental in an *epistemic and semantic sense.*

10. Lawhood and Necessity

Consider the well-rehearsed contrast between:

1 All spheres of gold are less than 1 km in diameter.

and:

2 All spheres of U^{235} are less than 1 km in diameter.

16 This is an application of Quine's "no entity without identity". Using the expressions "for all x" and "there is an x" in a meaningful way requires an identity criterion for entities in the domain.

We believe that #1 and #2 are both true. (If someone were to discover a counter instance to #1, a huge heap of gold somewhere in universe, one could simply use a bigger diameter.) Knowing that U^{235} is a radioactive isotope for which the critical mass is 52 kg (a sphere with a diameter of 17 cm), we are prone to say that #2 is a law, whereas #1 is not. It is also natural to say that #1 is contingently true, whereas #2 *must* be true, i.e., it is necessary. In fact, we are prone to say about all laws that they are necessary. Why?

The specific kind of necessity attributed to laws is often called "physical necessity" (or "nomological necessity"). Should we now say that a certain sentence *p* is (or expresses) a law because it is necessary, or should we say that since *p* is a law it is necessary? Those positions in the debate about laws that postulate universals or relations between universals as the metaphysical basis for lawhood naturally would say that *p* is a law because it is necessary. I am not tempted to go in that direction. The previous discussion is, I think, a plausible explanation of why at least some important laws are classified as such without talking about necessity. So I prefer to explain physical necessity in terms of lawhood.

We have three cases to consider: fundamental laws, derived laws and explicit definitions of new theoretical predicates.

10.1 Fundamental laws

Why do we say that fundamental laws are necessary? What is the intended meaning of "necessary" in this context?

If we use a quantitative predicate such as ELECTRIC FIELD in a theory, we need a definition of that predicate and, as shown above, Maxwell's equations function jointly as implicit definitions of this and other electromagnetic quantities. Thus, these equations are *necessary conditions* for the coherent use of ELECTRIC FIELD in our calculations. Often we abbreviate; instead of saying that Maxwell's equations are necessary conditions for theoretical descriptions of electromagnetic phenomena, we simply say that they are *necessary.* Since the sentence "p is a necessary condition for q" has the form of a material conditional, we do not intend any modal distinction at the level of object language when we express this conditional with the short version "p is necessary".

Then, since "necessary" here is not intended as marking a modal distinction, the logical form of "p is necessary" is not that of $\Box p$, but rather "$\ulcorner p \urcorner$ is necessary", i.e., the statement that a law L is necessary may be understood as that it is a necessarily true part of the theory. Since the law sentence said to be necessary, i.e., necessarily true, is *talked about, not used*, we must put the law sentence in quotation marks. Thus we do not enter quantified modal logic at all.

Saying that laws are necessary in the sense given above does not entail that they are absolutely certain, or that violations are inconceivable. Electromagnetism might one day be replaced by a better theory, but such a replacement means changing the electromagnetic laws, hence changing the extension of the quantitative predicates CHARGE, CURRENT, MAGNETIC FIELD, etc., if these words would still be used in the new theory.

10.2 Derived laws

If a sentence q is derivable from another sentence p it follows that we have established the material conditional $p \rightarrow q$. Hence, $\ulcorner q \urcorner$ is a necessary condition for $\ulcorner p \urcorner$, which we in ordinary parlance may ascertain by the expression "q is necessary", thus as before suppressing p as unnecessary to mention in the context at hand (and, as is usual in ordinary parlance, disregarding the use-mention distinction).

Not mentioning a condition is common in natural language in cases where the speaker and listener assume mutual awareness about it. For example, if I say to my visitor, "Now you *must* hurry", we both understand the use of "must" as indicating a tacit condition, such as "if you want to catch the train", which we both want to be fulfilled. This is also similar to our use of "must" in mathematics and logic; we may say, for example, "if 3x + 32= 83, then x *must* be 17". The truth of the sentence "x = 17" is a necessary condition for the truth of "3x + 32 = 83". Hence, since a derived law is a necessary condition for the truth of the fundamental laws from which it is derived, we say *about* derived laws that they are necessary.

10.3 Explicit definitions of new theoretical predicates

Explicit definitions are usually not called "necessary", but it is entirely correct to say that a definition of a technical term is a *necessary condition* for the meaningful use of that term in discourse. Hence we may reasonably say that having a definition of a quantity (or any other theoretical concept) is a necessary condition for the use of it in that theory. For example, we may say that Newton's second law is a necessary condition for the use of the quantity FORCE in calculations and predictions in mechanics. Accepting classical mechanics means accepting $f = ma$ as giving the extension of FORCE. Again, the sense of "necessary" intended here is simply "necessary condition", i.e., the consequent in a material conditional with tacit antecedent. And as before, the word "necessary" is here a semantic predicate, not a sentence operator.

A true accidental generalisation, such as #1, is not necessary in this sense; in #1 we use predicates which are defined independently of that sentence, and neither is it a consequence of such definitions. So my explanation of our saying that laws are necessary suffices for distinguishing between #1 and #2.

In short, all the laws that constitute a particular theory, fundamental laws, explicit definitions and all their logical consequences, are necessary conditions for our acceptance and use of the concepts in that theory.[17] No modal distinctions in the object language are assumed by using the word "necessary" in this sense.

My view is thus that we have means to discern some true UGCs as laws and it is their status as laws that motivates our calling them necessary. Not all the logical consequences of a set of laws are UGCs; we may also derive singular conditional statements from a set of laws and it is entirely reasonable to say that such statements are also physically necessary. So we arrive at the following definition:

Physical Necessity: ⌜*p*⌝ is physically necessary if ⌜*p*⌝ is a law, or a logical consequence of a set of laws.

Both the expressions "is a law" and "is physically necessary" are thus used as predicates taking sentences as arguments, not as sentence operators. This is not common; usually "necessary" is taken as a sentence operator. But if we do that and apply it to quantified sentences, we enter quantified modal logic. In this realm we arrive, via the Converse Barcan Formula and Distribution of necessity, at what Quine (1976) called "Aristotelian essentialism", i.e., a distinction between essential and contingent properties. Being an empiricist, this is too much metaphysics for my taste. Any such metaphysical commitments are avoided if we conceive "necessary" as a semantic predicate, a modifier of "true".

Van Fraassen (1977) argued that physical necessity, which he conceived as a sentence operator, is a species of verbal necessity. This is a possible stance so long as one does not apply "necessity" to sentences containing quantifiers, and van Fraassen did not discuss that. But this is somewhat astonishing since law sentences are UGCs. It seems to me that he succeeds in arriving at his conclusion only by avoiding quantified modal logic.

Henry Kyburg (1990), like myself, argues that necessity should be construed as a semantic predicate; but I disagree with him about the status of laws and quantities, as already mentioned in Section 4.

11. Summary

In this article I have not been able to cover all physical laws, but I do think that I have given good reasons for the thesis that most physical laws fit into one of the three types of laws here described: (i) fundamental laws, which are

17 This was arguably a core idea in Kuhn's (1970) talk about paradigms. But he would have won much clarity had he talked about extensions of predicates instead of paradigms, metaphysical assumptions, etc. But if so, the book might have been less famous.

generalisations of observations and at the same time implicit definitions of either quantitative predicates used in reporting generalised observations, or identity criteria for systems being quantified over; (ii) laws that are explicit definitions of quantities; and (iii) laws that are derivable from other laws.

The expressions "... is a law" and "... is physically necessary" are best viewed as predicates in metalanguage, not operators in the object language. Saying that laws are necessary may be interpreted as talk about law sentences; we thereby distinguish a subclass of true sentences. One may consistently say about laws that they are necessary, i.e., necessarily true, in this sense, since they establish rules for use of general terms in the theory, without granting the existence of any metaphysical categories such as essences, dispositions or relations between universals.

Theoretical predicates in physics ultimately get their meaning, i.e., their rules of application, from observations, and theory construction in physics must ultimately be built upon directly observable things, i.e., bodies, attributed measurable quantities.

This account of laws is Humean in spirit. But in contrast to Hume, who held that necessity just is a projection of our expectations, I hold that attributing physical necessity and lawhood to some sentences in scientific theories is motivated by conceptual and epistemological arguments.

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