DIMENSIONAL ANALYSIS:
ESSAYS ON THE METAPHYSICS AND EPISTEMOLOGY OF QUANTITIES

by

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Dieses Buch sei zur Ehre Gottes geschrieben.

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Abstract

This dissertation draws upon historical studies of scientific practice and contemporary issues in the metaphysics and epistemology of science to account for the nature of physical quantities. My dissertation applies this integrated HPS approach to dimensional analysis—a logic for quantitative physical equations which respects the distinct dimensions of quantities (e.g. mass, length, charge). Dimensional analysis and its historical development serve both as subjects of study and as a sources for solutions to contemporary problems. As this dissertation consists primarily of three related papers, I provide abstracts for each of them in the order in which they appear.

Metaphysics and Method, 1914-1917

This chapter recovers an important, century-old debate regarding the methodological and metaphysical foundations of dimensional analysis. Consideration of Richard Tolman's failed attempt to install the principle of similitude—the relativity of size—as the founding principle of dimensional analysis both clarifies the method of dimensional analysis and leads to the articulation of two metaphysical positions regarding quantity dimensions. Tolman's metaphysical position is quantity dimension fundamentalism. This is a commitment to a realism regarding a set of fundamental dimensions which provide real definitions for all further, derivative dimensions. The opposing, anti-metaphysical position, developed primarily by Bridgman, is quantity dimension conventionalism. Conventionalism is an anti-realism regarding dimensional structure, holding our that dimensional systems do not represent any objective structure and have their basic quantity dimensions fixed only by convention. This metaphysical dispute was left somewhat unsettled. It is shown here that both of these positions face serious problems: fundamentalists are committed to surplus dimensional structure; conventionalists cannot account for empirical constraints on our dimensional systems nor the empirical success of dimensional analysis. It is shown that an alternative position is available which saves what is right in both: a non-fundamentalist dimensional realism. On this position

the number, but not the identities, of the basic quantity dimensions is constrained by the world, suggesting objective dimensional structure, but this objective structure is not fundamentalist: basic and derivative quantity dimensions have symmetric dependency relations.

The Π-Theorem and Quantity Symmetries

In this chapter a symmetry argument against quantity absolutism is amended. Rather than arguing against the fundamentality of intrinsic quantities on the basis of transformations of basic quantities alone, a class of symmetries defined by the Π-theorem is used. These symmetry transformations define transformations of derivative quantities induced by transformations of basic quantities. The Π-theorem is a fundamental result of dimensional analysis and shows that all unit-invariant equations which adequately represent physical systems can be put into the form of a function of dimensionless quantities. Quantity transformations that leave those dimensionless quantities invariant are empirical and dynamical symmetries, avoiding counterexamples to the proposed symmetries of the original argument which show them to fail to be both dynamical and empirical symmetries. The discussion raises a pertinent issue: what is the modal status of the constants of nature which figure in the laws? Two positions, constant necessitism and constant contingentism, are introduced and their relationships to absolutism and comparativism undergo preliminary investigation. It is argued that the absolutist can only reject the amended symmetry argument by accepting constant necessitism. I argue that the truth of an epistemically open empirical hypothesis, constant determinism, would make the acceptance of constant necessitism costly: together they entail that all of the physical facts are nomically necessary.

Measurement and Systematic Error

I argue in this chapter that dimensional analysis provides an answer to a skeptical challenge facing a recent coherentist, holist epistemology of measurement: the theory of model mediated measurement. The problem arises when considering the task of calibrating a novel measurement procedure to the results of an established measurement procedure, when the novel procedure has a greater range than the established procedure. The skeptical worry is that the agreement of the novel and established measurement procedures in their shared range may only be apparent, due to the emergence of systematic error in the exclusive range of the novel measurement procedure. Alternatively: what if the two measurement procedures are not in fact measuring the same quantity? The theory of model mediated measurement can only say that the skeptical scenario is ineliminable; we simply assume that the two procedures measure a common quantity as a fundamental posit. Under this theory, there is no way to justify the assumption that is not circular. I argue that we may hope for more. I show that the satisfaction of dimensional homogeneity across the metrological extension is independent evidence for the absence of systematic risk. This is illustrated by Percy Bridgman's use of dimensional analysis in his high pressure experiments. This results in an extension of the theory of model mediated measurement, in which the posit of a commmon quantity between an established measurement procedure and its novel metrological extension is no longer an assumption, but an evidentially supportable hypothesis.

Chapter 1

Introduction

Let me begin with what this dissertation is not. It is not an introductory textbook to dimensional analysis with demonstrations of its descriptive power and problems of escalating difficulty.[1] Though it does introduce the basics of the method and has some demonstrations. It is also not a philosophical treatise that provides a systematic and thoroughgoing account of quantities, their logic, semantics, epistemology, and metaphysics. There was not enough time. It instead is something less than, yet something in between, those two models.

Though the dissertation has three main chapters, it really has two major parts with contextualizing material flanking them.

The first part is a partial account of the foundations of a method in physics, dimensional analysis. This first part comprises the first major chapter, which starts with the *logical* foundations of dimensional analysis. This involves, most importantly, a distinction between dimensional and unit systems whose confusion has marred most discussion of dimensional analysis since it became an explicit topic of discussion in the Victorian era.[2] This chapter, "Metaphysics and Method, 1914-1917", moves on to investigate the *historical* foundations of dimensional analysis in order to explicate its *metaphysical* foundations. Given the fact

[1] Such textbooks are legion. My recommendation for novice and expert alike is Gibbings (2011), which is more alive to the history and philosophy of the subject than most.

[2] Mitchell (2017) shows how the dimensional calculus is in some ways an elegant and in other ways an unholy synthesis of methods and motivations that span engineering, theoretical physics, and high metaphysics.

that “dimensional analysis” was only coined a century ago, the history of the method is surprising rich, complex, and deep, so only a very small cross-section of its history could be analyzed in this effort. That said, this historical period is of particular significance as it involves both a notable attempt to explicitly *re*found dimensional analysis on an unorthodox principle and, more importantly, a metaphysical question central to the heart of dimensional analysis: are quantity dimensions objective metaphysical kinds or are they mere representational tools for translating between unit systems? I present for the first time a metaphysical position which preserves what is right in both accounts of quantity dimensions which appear in the historical literature: a nonfundamentalist dimensional realism.[3]

The second part of the dissertation comprises two *applications* of dimensional analysis to contemporary issues in the metaphysics and epistemology of science. “The Π-Theorem and Quantity Symmetries”[4] uses a foundational result of dimensional analysis to provide a criterion for quantity symmetries—transformations of basic quantities, e.g. a doubling of all masses, that count as both empirical symmetries and symmetries of the laws. This not only requires an (implicit) acceptance of the modal significance of quantity dimensions defended in the prior chapter, but also requires an acceptance of a particular view, contingentism, regarding the modal status of the dimensional constants. I give reason to reject the alternative, constant necessitism, namely that it makes all too plausible a necessitarian view of the universe, but I do not offer a knockdown objection. “Measurement and Systematic Error” extends the founding principle of dimensional analysis, the principle of dimensional homogeneity, from guiding the manipulation and formulation of quantity equations to providing a necessary condition on one of the founding “assumptions” of a contemporary holist epistemology of measurement: the theory of model mediated measurement. Using a case study from the history of science, Percy Bridgman’s experimental work in high pressure physics, I show that the principle of dimensional homogeneity has been used to identify and eliminate systematic

[3] Simultaneously, a similar "structuralist" account has been developed by Jacobs (Unpublished Manuscript).

[4] This chapter has been accepted for publication prior to completion of the dissertation (Jalloh Forthcoming). No significant difference exists between the forthcoming version and the version in the dissertation.

errors and so provides independent evidence for the common quantity hypothesis so central to calibration in the theory of model mediated measurement.

With this work I hope to have shown philosophers what dimensional analysis is and what it can do for them. A great deal of material, conceived and unconceived, could not make it into the dissertation and so we must understand that this is only a beginning and not that the final word on the nature of physical quantities.

Chapter 2

Metaphysics and Method, 1914-1917

2.1 Introduction

This chapter studies a dispute about the methodological foundations of dimensional analysis in order to clarify its *metaphysical* foundations. In particular, consideration of the debate started by the failed attempt of Richard Tolman to install the principle of similitude—the relativity of size—as the founding principle of dimensional analysis both clarifies the method of dimensional analysis and articulates two metaphysical positions regarding quantity dimensions. One view, which I call fundamentalism, holds that there is objective dimensional structure and there is a set of objectively basic (i.e. fundamental) quantity dimensions. Another view, conventionalism, holds that dimensional systems do not represent any objective dimensional structure and that basic quantity dimensions are determined by convention. Objections to both positions presented in the historical debate are found to have (limited) validity and a third, alternative position, non-fundamentalist realism, is introduced. For the non-fundamentalist realist, the objective aspect of dimensional structure is constituted by the symmetric dependency relations between quantity dimensions. This allows for a synthesis of two methodological conceptions of dimensional analysis that *prima facie* are in tension: that dimensional analysis is a *logical* method and that dimensional analysis provides *explanations*.

The historical discussion will be restricted to the debate prior to Bridgman's landmark *Dimensional Analysis* and will focus primarily on an exchange between Bridgman and Tolman.[1] Other significant contributors to the debate, Edgar Buckingham and Tatiana Ehrenfest-Afanassjewa, cannot be given their full due here.

In what remains of this introduction, I will introduce dimensional analysis as a method for problem solving in physics, clarify its role as a logical method, and clarify an all important and not often made distinction between unit systems and dimensional systems. This introduction provides all the necessary technical background for the rest of the paper to follow.

2.1.1 Dimensional Analysis in Action

Dimensional analysis is well known to even beginning students in physics, though explicit instruction in the method is far from universal. Dimensional analysis finds use in (often heuristic) arguments in fundamental physics and in technical engineering applications alike. Let's consider an example of (standard) dimensional analysis in action.

Say we are tasked with deriving the equation for the period of oscillation, t, of an arbitrary pendulum. We assume that the system can be adequately described in terms of the following quantities: the mass of

[1]In this way it differs from the brief, but more comprehensive, account of the debates regarding dimensional analysis in Walter (1990). Her account is more comprehensive in that it covers the debates before and after *Dimensional Analysis*, but it is more narrowly focused on Bridgman—Rightly so, as Walter's book is a biography of Bridgman.

the pendulum, m, the length of the pendulum, l, and the constant acceleration of gravity, g.[2] Next we assume that the dimensions of these quantities are all reducible to mechanical dimensions such that:

$$[t] = \mathrm{T}$$

$$[m] = \mathrm{M}$$

$$[l] = \mathrm{L}$$

$$[g] = \mathrm{LT}^{-2}.$$

The square brackets are a function from quantities to their dimensions, here given in terms of the basic mechanical dimensions, mass, length, and time (capital un-italicized letters denote dimensions).[3]

We can restate the problem as that of finding the form of the function f such that $t = f(m, l, g)$, and so $[t] = f([m], [l], [g])$. This is the principle of dimensional homogeneity:

> (The Principle of Dimensional Homogeneity) Every representationally adequate physical equation is dimensionally homogeneous, and an equation is dimensional homogeneous iff the quantity terms[4] on each side have the same dimension.[5]

[2]This condition of "adequate description" is often called "completeness" (e.g. Buckingham 1914). That phrasing gives the wrong idea. Dimension analysis requires only that all of the *relevant* quantities are considered, many quantities that are also descriptive of the system (indeed there is an infinity of them) are excluded due to irrelevance or redundancy, etc. In this way dimensional analysis is a modeling practice (see Pexton 2014).

[3]There is a slightly different convention, following Maxwell (2002), in which $[L]$ represents the length dimension rather than L, etc.

[4]Each of these terms are monomials of quantity variables (or constants) and a dimenisionless scale factor, addition and subtraction distinguish terms. This captures the intuition that it makes no sense to add a length to a mass or to subtract a force from a velocity, etc.

[5]This principle is first made explicit by Fourier in his *Théorie Analytique de la Chaleur*: "It must now be remarked that every undetermined magnitude or constant has one dimension proper to itself, and that the terms of one and the same equation could not be compared, if they had not the same exponent of dimension." (Fourier 1878, p. 128) For more on the geometrical roots of dimensional analysis see De Clark (2017) and Roche (1998).

We assume that this function f takes the form of a monomial $km^\alpha l^\beta g^\gamma$, with numerical scale factor k.[6] From this assumption and the principle of dimensional homogeneity follows a set of linear equations that are to be solved for the *exponents* that t has each of the indicated basic quantity dimensions:

$$\mathrm{M}: 1\alpha + 0\beta + 0\gamma = 0$$

$$\mathrm{L}: 0\alpha + 1\beta + 1\gamma = 0$$

$$\mathrm{T}: 0\alpha + 0\beta - 2\gamma = 1,$$

where the Greek variables stand for the exponents of the variables in the monomial and their coefficients are the exponent of the indicated basic quantity dimension had by the corresponding quantities m, l, and g. By inspection $\alpha = 0$. Now with two equations and two variables (β and γ) we find the solution to be $\beta = 1/2$ and $\gamma = -1/2$, so

$$t = k\sqrt{\frac{l}{g}},$$

where k is some undetermined dimensionless constant. The equation for the period of oscillation of an arbitrary pendulum has been derived on the basis of dimensional analysis.[7]

2.1.2 Dimensional Analysis as Logic

Dimensional analysis was commonly thought of as a *logical* method by those who developed its foundations (see also Gibbings 1982). I've attempted, in the demonstration above, to make the logical character of dimensional analysis evident by distinguishing assumptions which draw upon our prior physical knowledge and the workings of dimensional analysis itself. In discussing his foundational paper on dimensional analysis (Buckingham 1914), Buckingham wrote:

[6] This is due to Bridgman's (1931) lemma, see Berberan-Santos and Pogliani (1999) and Jalloh (Forthcoming) for discussion.

[7] Such derivations can be done more systematically by way of the Π-theorem, a fundamental result of dimensional analysis, see discussion and references in §2.2.2.

> Some three or four years ago, having occasion to occupy myself with practical hydro- and aerodynamics, I at once found that I needed to know more about the method in order to use it with confidence for my own purposes...
>
> I had therefore, as it were, to write an elementary textbook on the subject for my own education. My object has been to reduce the method to a mere algebraic routine of general applicability, making it clear that Physics came in only at the start in deciding what variables should be considered, and that the rest was a necessary consequence of the physical knowledge used at the beginning; thus distinguishing sharply between what was *assumed*, either hypothetically or from observation, and what was mere logic and therefore certain. (Buckingham to Rayleigh, November 15 1915)[8]

It is clear from this that Buckingham understood dimensional analysis as a logical method insofar as it was certain and so did not depend on any further empirical claims, i.e. *a priori*. Modeling dimensional analysis on deductive logic, we can say that it provides a form of valid argument (more abstractly, transformation rules): *if* such-and-such quantities have such and such dimensions, relative to a dimensional system (see §2.1.3), *then* they are related by so-and-so functions.[9] In our extended post-logical-empiricism hangover, such a distinction between logic and experience may seem hopeless, and worse, old-fashioned—we cannot accept Buckingham's conception of dimensional analysis.[10]

Here I'd like to rehabilitate an idea of dimensional analysis as logic, by abandoning Buckingham's *epistemic* conception of logic, while accepting that it stands apart from ordinary physics in an important

[8] Courtesy of the American Institute of Physics, Niels Bohr Library and Archives, MP 2017-2296; 33.

[9] That the generation of Π-terms and so functional relations can be computed completely and without arbitrariness is shown in Gibbings (2011). That does not mean, of course, that in ordinary practice there is not an art in determining *which* Π-terms and so functional relations are of interest for the relevant system.

[10] In a later letter to Rayleigh on January 7 1916, Buckingham already expresses his feeling that his methodological strictures chafed against the zeitgeist: "It is evidently desirable that this subject should receive a clear exposition. Tolman does not, I imagine, care much for the distinctions between known facts, assumptions made for the sake of building up theories, and purely logical operations on these facts or assumptions. And it seems that many of the very clever rising generation of physicists have much the same feeling. I, on the other hand, regard these distinctions as very essential to clear thinking and sound progress." (p 6) Courtesy of the American Institute of Physics, Niels Bohr Library and Archives, MP 2017-2296.

way. The relations between dimensional analysis and experiment are too complex to segregate dimensional analysis from empirical assumptions, but there is still a sense in which dimensional analysis stands above (or below) the ordinary practice of physics in a way similar to the relative standing of logic and ordinary reasoning. For this rehabilitation, I will draw on Gil Sagi's (2021) recent defense of an exceptionalist conception of logic *as a methodological discipline*—this contrasts with the usual exceptionalist conceptions of logic on an epistemic basis, e.g. because it is *a priori*, that is now so unfashionable after Quine (1951). In adding dimensional analysis to the roster of methodological disciplines, I am accepting the invitation left open by Sagi that "[p]erhaps there are other methodological disciplines targeting scientific practice" (2021, p. 9741). I offer the claim that dimensional analysis is the methodological science peculiar to quantitative science, here narrowly considered as peculiar to quantitative *physical* science, and so can synonymously be understood as the *logic of quantities*.

What is a methodological discipline? We may do well to start with the characterization given by Sagi:

> As a start, by a methodological discipline, I mean a discipline that produces tools, methods or a methodology for some practice. I take a method to be a systematic procedure or system of rules for carrying out a practice. There may be methods for very specific practices (measuring the distance between the earth and the moon, solving differential equations) or general methods advising a whole discipline (how to conduct a scientific experiment, how to prove a mathematical theorem)[...] A methodology, in general, is aimed at a higher level of scientific practice, as it concerns the production and selection of scientific theories. A methodology, I assume, may give rise to a method (for, e.g., theory choice) or consist of a compendium of methods (for reasoning in science). (Sagi 2021, p. 9736)

A methodological discipline is defined *relationally* to what we may call a *client* discipline. The methodological discipline aids practitioners in aligning their scientific practice to the aims of their first-order client discipline. Put differently, the aims of a methodological discipline are to ensure that the products of some client discipline (e.g. theories or models) meet the internal aims of that client discipline (e.g. prediction, explanation). Here I am proposing that dimensional analysis has physics (broadly construed) as a client discipline—dimensional analysis provides principles and derivational techniques that allow physicists to check the validity of their quantitative equations and to efficiently derive new ones.[11]

What is the relation between a methodological discipline and a client discipline? One intriguing characterization of the relation between the two that Sagi gives involves an extension of the use-mention distinction: client disciplines *use* tools, methods, and concepts that are *mentioned* (e.g. criticized, constructed) by the corresponding methodological discipline. While physics uses concepts of quantity, principles of homogeneity, and dimensional systems, it is left for dimensional analysis to discuss the nature of quantities, justify and determine the consequences of dimensional homogeneity (e.g. the Π-theorem), and elaborate and distinguish dimensional systems.[12] It is important that this exceptionalist, relational conception of methodological disciplines does not lapse into a sort of epistemic foundationalism as attacked by Quine. We can capture both the special position of a methodological discipline and its revisability by distinguishing two phases of research:

> (Business as Usual) The methodological discipline constructs, describes, and regiments the techniques and concepts used by the client discipline. The rules set by the methodological discipline exert normative force on the practitioners of the client discipline, when there is a discrepancy, the principles set by the methodological principle take precedence.
>
> (Negotiation) First order problems or developments in the client discipline lead to a reconsideration of the principles of the methodological discipline and the relationship between the two—neither discipline takes normative priority to the other.

[11] A similar distinction between "framed" and "framing" inquiry has been articulated and defended by Henne (2023).

[12] A closely related and analogous methodological discipline is metrology, which provides the (experimental) physicist with units of measurement, values for constants, rules for error propagation, etc. Metrology is an important case to consider as the divide between methodological discipline and client discipline(s) has there become sociologically and institutionally regimented in a clarifying way.

In the Business as Usual phase the client-provider relation is as expected, the methodological discipline provides tools and method which hold normative force over the practices of the client discipline (they are *relatively a priori* in the sense of Friedman 2001)—an equation of physics found to violate dimensional homogeneity is an equation to be corrected (or at least used with great care in special circumstances). In the Negotiation phase, usual business is disrupted, internal pressures from the client discipline (e.g. empirical results, paradoxes) lead to adjustments in the methodological principle and even shifts in what aspects of the relevant scientific practice belong to which discipline. The historical episode to be considered here is usefully described in these terms: In the early twentieth century, pragmatic matters (above all the development of airplanes, see Sterrett 2005) led to a formalized business deal between the nascent methodological discipline of dimensional analysis and the physical sciences. While this deal quickly came to be "business as usual", Tolman attempted in 1914 to renegotiate the deal. Inspired by radical developments in the client discipline, physics, Tolman attempted to augment the foundations of the methodological discipline with a new relativity principle and thereby provide new constraints on the client discipline. While Tolman's negotiation failed, it made explicit many implicit aspects of the initial deal between dimensional analysis and physics, some which have still yet to be fully clarified. In the next two subsections I clarify an important aspect of the usual deal and raise one issue left to be negotiated: To what extent do features of our dimensional systems represent objective structure?

2.1.3 Dimensional Systems and Unit Systems

Dimensional analysis depends on some assumptions regarding physical quantities. They must form a complete dimensional system, meaning that the complete set of quantities are reducible to products of powers of fundamental units multiplied by a numerical scale factor:[13]

$$Q_i = k_i u_a^{\alpha} u_b^{\beta} u_c^{\gamma} \dots$$

[13]See Bridgman (1931) and Berberan-Santos and Pogliani (1999) for proofs.

Q_i is some arbitrary quantity. k_i is some numerical factor. u_x is some fundamental unit. The Greek exponents are known as dimensions, following Fourier (1878).[14] Each basic unit is assigned a basic dimension. For example, in a mechanical dimensional system,

$$m = u_{\mathrm{M}}$$

$$l = u_{\mathrm{L}}$$

$$t = u_{\mathrm{T}}$$

where m, l, and t are arbitrary mass, length, and time quantities set to be units by convention, e.g. a kilogram, a meter, and second. Each of these units have a basic dimension,

$$[m] = \mathrm{M}$$

$$[l] = \mathrm{L}$$

$$[t] = \mathrm{T}$$

which, in abstraction from the actual units, we can use to derive the dimensions of all other mechanical quantities.[15] Hence dimensional systems, which are determined by the basic dimensions, are more coarse-grained than unit systems. For each dimensional system there is an arbitrarily large set of logically

[14]This sometimes leads to expressions like "has exponent d in dimension X" which are equivalent to expressions like "has dimension X^d".

[15]Italicized capital letters are variables for quantities, I will, for the remainder of this section, retain lowercase variables for units (excluding dimensionless constants k_i). Unitalicized capital letters represent dimensions.

possible coherent unit systems which are all inter-convertible and hence form what I will call a "dimensional group".[16] For example, the dimensions of force, F, and the dimensions of velocity, V, are given so:

$$[F] = \text{MLT}^{-2}$$
$$[V] = \text{LT}^{-1}$$

These dimensional formulae correspond to definitions of mechanical units:

$$f = k_f m l t^{-2}$$
$$v = k_v l t^{-1}.$$

For a *coherent* system of mechanical units $k_f = k_v = 1$.[17] We can distinguish basic quantities, which have dimensional exponent 1 in only one of the basic dimensions, and derived quantities, which have arbitrary dimension in any of the basic dimensions. Basic quantities are measured by fundamental units and derived quantities are measured by defined units. The dimensions of the derived quantities encode formal relations between them and the basic quantities: these relations identify the transformation rules for derived quantities upon changes in the fundamental units.

[16] There is some complexity in the nature of units that I am suppressing here. The important thing is that dimensional groups consist of units defined by "similar scales" (Ellis 1964). The group structure of similar unit systems is not to be confused with the group structure of dimensional systems (which unit systems inherit), see de Boer (1995).

[17] The usage of the terminology "complete" and "coherent" varies widely. I am also here making a distinction between dimensional and unit systems that is not usually made, though see Abraham (1933). I reserve "complete" for dimensional systems with a reduction base as I go on to describe. I reserve "coherent" for any unit system of a complete dimensional system such that the derivative quantities are defined with dimensionless scale factors $k_i = 1$. Complete equations, which are interpreted according to a complete dimensional system, are unit-invariant (in algebraic form) for any coherent unit system of *that* dimensional system. This captures the lessons of Grozier (2020), though he does not make the distinctions I make, as the mistakes he diagnoses could be avoided by the recognition of the distinction between dimensional systems and the more fine-grained unit systems.

For any derived mechanical quantity, Q, its defined unit, q, will be a monomial function of the fundamental units, just as described above:

$$q = m^{\alpha} l^{\beta} t^{\gamma}$$

The Greek dimensional exponents determine how the defined unit changes with arbitrary scalar transformations of the fundamental units:

$$\frac{q'}{q} = \left(\frac{m'}{m}\right)^{\alpha} \cdot \left(\frac{l'}{l}\right)^{\beta} \cdot \left(\frac{t'}{t}\right)^{\gamma}$$

where the primed units are the new units. If we halve the fundamental time unit, $2t' = t$, and leave the mass and length units unchanged, for example, the unit of force, f, will quadruple because $\gamma_f = -2$ and the velocity unit, v, will double because $\gamma_v = -1$:

$$\frac{f'}{f} = \left(\frac{m'}{m}\right)^{1} \cdot \left(\frac{l'}{l}\right)^{1} \cdot \left(\frac{t'}{t}\right)^{-2} = \left(\frac{t}{2t}\right)^{-2} = 4$$
$$\frac{v'}{v} = \left(\frac{m'}{m}\right)^{0} \cdot \left(\frac{l'}{l}\right)^{1} \cdot \left(\frac{t'}{t}\right)^{-1} = \left(\frac{t}{2t}\right)^{-1} = 2$$

The use and operation of these unit transformation rules and their duality with dimensional formulae are uncontroversial. However, there remains controversy regarding the *meaning* of the subject matter of dimensional analysis: quantity dimensions and dimensional formulae.

One interpretation of dimensional analysis harks back to Buckingham's conception of dimensional analysis as a *formal* logic concerned with conventionally decided transformation rules on defined or stipulated "objects". On this view, dimensional formulae are understood to be formal rules for the use of units and numerical representations of quantities, which are purely conventional. On this reading, representations of dimensions like "M" are purely syntactic shorthand for change ratios like "m'/m". The basis of a dimensional system and the corresponding formulae for derived dimensions are therefore reducible to rules of translation between ultimately conventional unit systems that regiment our practice of assigning numbers to objects, systems, or measurements.

There is a competing interpretation of dimensional analysis that holds quantity dimensions to be entities in their own right, irreducible to mere convention and formal rules. On this view, dimensional formulae do not only represent unit transformation rules but also reveal the metaphysical character of quantities. Not only is a unit of force defined, but a *quantity* of force is *constructed* or *constituted* by the dimensions of mass, length, and time. It is as if the basic dimensions are the fundamental substances from which the more complex derivative quantity dimensions are composed. On this interpretation, as defended by Tolman, there is a uniquely correct dimensional system which represents the objective dimensional structure of quantities: its basic dimensions are *fundamental* dimensions, and its dimensional formulae represent *real* definitions—that is, *assymetric* and *metaphysically significant* definitions—of derivative dimensions.[18]

In order to further explicate and critically examine these two interpretations of dimensional analytic methods and objects, I will set them against questions regarding the objectivity of the two main features of dimensional systems discussed here: basic quantity dimensions and dimensional formulae.

2.1.4 Metaphysical Questions and Answers

A dimensional system is to be understood as a formal system that consists simply in a set of basic, that is independent,[19] quantity dimensions (a basis) and a set of dimensional formulae for the canonical derivative dimensions, like volume and pressure (this set of quantities will be theory dependent). We can alternatively

[18]This controversy dates back to the development of the dimensional calculus by Maxwell and others (see Mitchell 2017), which continues to present day, with Skow (2017) arguing against the interpretation of dimensional formulae as denoting constitution relations (but defending them as definitional relations).

[19]Independence can be understood thus: two quantity dimensions are independent if neither depends on the other, i.e. no product of powers of the one appears in the dimensional formula for the other and vice versa. One might say, well I can define the dimensions of mass to be $L^{-1}ML$ so mass is not independent of length. The response is that no exponents of like dimension in dimensional formulae are allowed to go unsummed (in this case the two powers of length cancel out). A set of basic dimensions spans a dimensional system in just the same way that a set of basis vectors span a vector space, see Corrsin (1951) and de Boer (1995).

represent a system *just* by dimensional formulae, that some quantity dimensions have a single dimension of power 1 indicates that they are basic.

Metaphysical questions concern the relations between dimensional systems and dimensional structure, if there is any. Dimensional structure would be the ontic analog of a dimensional system—if there is objective dimensional structure then there is a dimensional system that correctly represents this aspect of a system (or perhaps the world, see §2.3.2). This brings us to the first ontological question, the general question of realism:

> (Dimensional Realism) Is there objective dimensional structure that corresponds to a dimensional system?

Alternatively this can be put: Is there an objectively correct dimensional system for the world?[20] There is a subsidiary question which further specifies some particular aspect of dimensional systems which may be objectively determined:

> (Fundamental Basis) Is there a fundamental dimensional structure that corresponds to a dimensional basis?

Is it the case that the dimensions M, L, and T form a unique basis for mechanical dimensions (with $[F] = \mathrm{MLT}^{-2}$)? Or is there another set—e.g. F, L, and T (with $[M] = \mathrm{FL}^{-1}\mathrm{T}^{2}$)—which would serve just as well?[21] The general ontological question can be understood as raising the question of whether or not our dimensional systems represent anything at all. The fundamental basis question further speciates forms of realism. If a dimensional realist believes there is a set of objective basic quantity dimensions they are a fundamentalist. If not, they are a non-fundamentalist realist, a position I develop in §2.3. A conventionalist rejects objective dimensional structure *tout court* and so automatically rejects objective *fundamental*

[20]This dimensional structure is supposed to be "joint-carving" in the sense of Sider (2011).
[21]An explication of "just as well" will come in §2.3.1.

dimensional structure corresponding to the basis of a dimensional system.[22] The relationships between these metaphysical positions and the answers they provide to the questions above are summarized in the following flowchart.

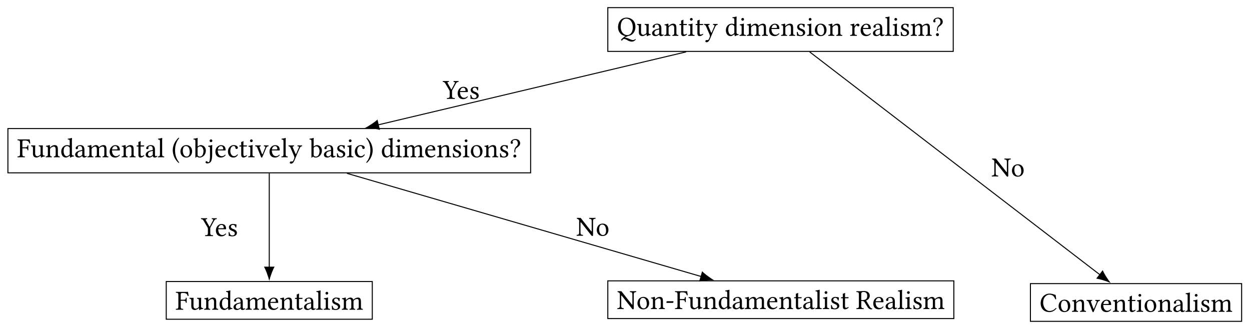

Figure 2.1: Flow chart through the logical space of quantity dimension metaphysics.

As I will show, both fundamentalism and conventionalism about quantity dimensions are articulated and defended in the years 1914-1917. A third view, non-fundamentalist realism is presented here as a synthesis of the two, responsive to problems to both historical positions.[23]

Tolman (1917) provides the first full articulation of quantity dimension fundamentalism. Quantity dimension fundamentalism combines a dimensional realism with a commitment to a fundamentality principle: there is a set of fundamental quantity dimensions that provide real definitions of—or metaphysically ground—the derivative quantity dimensions.

> (Fundamentalism) There is only one correct dimensional system and it represents objective dimensional structure. Dimensional formulae describe the natures of quantity dimensions.

[22]One might wonder if it may speciate forms of antirealism as well. One might think if an antirealist holds that there is such objective basis set, they are an operationalist. The operationalist of course cannot hold that this set is objectively basic in the metaphysical sense we are concerned with here: it must be an epistemic fundamentality (the operationalist distinction is often between primary and secondary quantities, see Ellis 1968). For this reason operationalism is not considered here, though this is closer to the view of Bridgman (1931) than conventionalism is. See also Gibbings (2011).

[23]Dialectically, this division of the logical space is similar to that in Skow (2017). The analogy would be that Skow's positivist stands in for my conventionalist, his contructivist for my fundamentalist, and his definitional connectionist for my non-fundamentalist realist. There are some differences: Skow's definitional connectionist is also a fundamentalist as they are committed to non-relativity, the position that there is an objectively determined basis for our dimensional system. That said, Skow's definitional connectionist comes closer to my non-fundamentalist realist due to an emphasis on the necessary connections between distinct quantity dimensions (Skow 2017, p. 194). An appreciation of the full force of conventionalist symmetries would lead Skow's definitional connectionist to drop the idea of unique real definitions of derivative dimensions, and so fundamental dimensional structure, yielding a non-fundamentalist realist account.

Tolman's fundamentalism comes out of a debate concerning his proposed principle of similitude, which was to replace the principle of dimensional homogeneity as the foundation of dimensional analysis:

> (The Principle of Similitude) *The fundamental entities out of which the physical universe is constructed are of such a nature that from them a miniature universe could be constructed exactly similar in every respect to the present universe.* (Tolman 1914a, 244, his emphasis)[24]

Tolman conceptualized his principle of similitude as a relativity principle, the relativity of size (i.e. length scale). In the first instance this principle is to be understood and was understood as a *particular instance* of quantity dimension fundamentalism. In this instance Tolman held that there was only one fundamental mechanical dimension, length. With the adoption of certain laws as providing dimensional formulae that grounded mass, time, and other mechanical quantities in length, Tolman was able to recover the intuition behind his relativity of size principle: a universal scale transformation of lengths ought to be an empirical symmetry, e.g. a doubling of all the lengths overnight would not be empirically detectable. First Tolman defends his principle by giving up the metaphysical, fundamentalist reading of it. He ultimately recants and gives up the principle and defends a more tenable fundamentalist picture (by adopting a richer fundamentalist basis).

As it turns out, Tolman's principle of similitude is false, owing to its conflict with the Newtonian Gravity and the relevant confirming evidence thereof—This was pointed out almost immediately by Buckingham (1914) and amplified by Ehrenfest-Afanassjewa (1916b) and Bridgman (1916). Tolman himself thought a new theory of gravity was imminent.[25] The empirical disconfirmation of Tolman's principle does not

[24] A major warning is to be heeded here. In this paper "the principle of similitude" or "the method of similitude" refers to uses of *Tolman's* principle. More generally "similarity methods" are just another term for using traditional dimensional analysis based on the principle of dimensional homogeneity and proportionality principles (see Sterrett 2017). At the risk of misunderstanding, I am sticking with the terminology used by those in the debate—though it is relatively clear that Buckingham (1914) intended to reclaim the terminology of similitude from Tolman. In the end Buckingham won out. I thank Susan Sterrett for pushing me on this point.

[25] The relationship between Tolman's principle and the emergence of novel theories of gravity, let alone questions about the nomological nature of the constants (see §2.2.3), is much too large a topic to be dealt with here. I will only note that Nordström (1915) developed a version of his scalar gravitational theory (an early competitor to Einstein's general theory of relativity) that is consistent with Tolman's principle. The development and significance of such a theory is left for future work.

undermine the interest of the methodological and metaphysical issues which were raised by the debate concerning his principle. The positions outlined in the debate and the arguments given for them have implications for the general study of dimensional systems.

2.2 The Debate About Tolman's Principle of Similitude and the Case Against Fundamentalism

In this section I discuss the debate surrounding Tolman's principle of similitude in three parts, roughly in historical order. Each subsection deals with a dialogue between Tolman and an interlocutor: Edgar Buckingham, Tatiana Ehrenfest-Afanassjewa, and Percy Bridgman. Each dialogue brings forward the metaphysical issues latent in the methodological debate, but special attention is paid to the dialogue with Bridgman, which leads to explicit metaphysical accounts of quantity dimensions.

First a brief note on the scientific context for this debate is necessary. The concern with the foundations of dimensional analysis is connected to other radical changes in the foundations of physics in the early 20$^{\text{th}}$ century.

2.2.1 Contextualizing Dimensional Analysis in the Wake of Relativity

This debate regarding the foundations of dimensional analysis was not about relativity, nor quantum mechanics.[26] That said, it is important for understanding this debate to understand some of the fundamental questions that were raised by relativity, which caused Tolman in particular to reconsider the very nature of physical quantities. Maila Walter situates the development of dimensional analysis as part of a broader reckoning with the radical consequences of relativity theory:

[26]See Semay and Willemyns (2021) for an initial look at the application of dimensional analysis to quantum mechanics. While Nordström (1915) moves the debate into one concerning a relativistic theory of gravity, this is not the primary concern of the dimensional analysts. See Porta Mana (2021) for a contemporary and systematic application of dimensional analysis to general relativity theory.

> [T]he dimensional analysis controversy revealed a generous amount of confusion about the meaning of relativity and measurement[...] Einstein's abrogation of the traditional meaning of measurement has demonstrated that the relationship between mathematics and physical reality had to be reconsidered. The dispute over dimensions was just one manifestation of a general concern that would be stated with more precision and politicized by the logical positivists. (Walter 1990, p. 84)

The following description of this broader context is based on Walter's more thorough accounting of the relevant foundational debates in the wake of relativity.[27]

The special theory of relativity was met with great suspicion and disbelief when it was brought to the attention of American physicists—the promulgation and acceptance of the theory in America is due in no small part to the efforts of Gilbert N. Lewis and Richard C. Tolman in 1908.[28] Lewis and Tolman (1909), in American pragmatist fashion, describe the principle of relativity as grounded in the generalization of experimental facts, most importantly the Michelson-Morley experiment, and as a principle about what is *measurable*:

> [Einstein] states as a law of nature that absolute uniform translatory motion can be neither measured nor detected. (G. N. Lewis and Tolman 1909, p. 712)

This is to say that only relative motion has "physical significance" or objectivity. This principle, combined with the postulate of the frame invariance of the speed of light, leads to the shocking consequences of relativity theory: time dilation and length contraction. Lewis and Tolman's grounding of relativity and its consequences in measurement results leads them to an antirealist interpretation of such consequences:

[27] One of the broader trends—interrelated with the dimensions debate—is the search for a natural or ultimate and rationally determinable set of fundamental units. This I cannot discuss here, interested readers should consult Walter (1990).

[28] They presented a paper "Non-Newtonian Mechanics and the Principle of Relativity" at the Christmas meeting of the American Physical Society in 1908, as stated by Kevles (1995, p. 90). However, I can find no trace of an article in *Physical Review* as he claims. The article (draft completed in May 1909) was published both in *Philosophical Magazine* and *The Proceedings of the American Academy of Arts and Sciences* the following year with an inverted title: "The Principle of Relativity, and Non–Newtonian Mechanics". Here I cite the latter, American publication, a citation for the former can be found in Walter (1990). See also Goldberg (1984); Goldberg (1987) on the American response to relativity and Lewis' and Tolman's roles in it.

> Let us emphasize once more, that these changes in the units of time and length, as well as the changes in the units of mass, force, and energy which we are about to discuss, possess in a certain sense a purely factitious significance; although, as we shall show, this is equally true of other universally accepted physical conceptions. We are only justified of speaking of a body in motion when we have in mind some definite though arbitrarily chosen point as a point of rest. The distortion of a moving body is not a physical change in the body itself, but is a scientific fiction. (G. N. Lewis and Tolman 1909, p. 717)[29]

The contrast drawn is between what they take to be the Einsteinian point of view on these distortion effects and the real contraction of Lorentz. They describe these phenomena as changes in *units* and "in a certain sense psychological". Lewis and Tolman claim that the acceptance of these distortions is the cost of retaining our fundamental conceptions of physics. The psychological unreality of these distortions owes to the fact that their occurrence appears to depend on whether or not some observer considers herself at rest, a judgment lacking in objectivity due to the relativity principle.

The more proper evaluation of the situation is given in Lewis and Tolman's claim that absolute motion has no significance—dilation and contraction are artifacts of an arbitrarily chosen rest point, thereby retaining something of our "fundamental conceptions". This is a common feature of symmetry arguments, which occurs in Tolman's argument for the principle of similitude as well as recent debates on quantity symmetries:[30] In arguing for the existence of a symmetry transformation and thereby the unreality of the supposed features of reality that vary under that symmetry, the basis for the symmetry argument seems to be undermined as there is no such feature to be transformed. In Einstein's case this is absolute velocities; In Tolman's case, with his supposed relativity principle, the principle of similitude, it is absolute lengths. This is of course only a matter of charitable interpretation and convenience in discourse:

[29] The special theory of relativity was seen as upending our fundamental concepts of physical *quantities*—when Lewis and Tolman refer to "units" they are conflating the functions of units as reference quantities (i.e. standards) and as numerical fixed points. The terminology of units vs quantities vs magnitudes was not to be standardized for decades.

[30] See Wolff (2020) and citations therein on the absolutism-comparativism debate in the metaphysics of quantity. The supposed mass doubling symmetry at the center of the debate is a direct analogue of Tolman's miniature universe transformation.

in either case any appearance of self-undermining can be removed by restating these relativity principles as statements about what objective structure there is. The theory of special relativity rejects any objective, frame-independent, velocity structure. Tolman's principle of similitude rejects any objective, absolute length *magnitudes*, which become dependent on a choice of comparative standard, analogous to how length quantity *values* (i.e. numbers) are relative to a choice of unit standard (i.e. a length defined to be represented by 1).

2.2.2 Tolman v. Buckingham

The inciting event for the debate is Tolman's publication of "The Principle of Similitude" (1914a) which puts forward a relativity principle—the relativity of size—as the founding principle of dimensional analysis.

> (Relativity of Size) A global transformation of the length scale is both a dynamical and empirical symmetry—there is no objectively determined length scale.

This is, I hope, a useful updating of Tolman's principle in conformity with how we generally understand the principle of relativity today. However, this gloss ultimately only gets us at Tolman's thought insofar as it has the same consequences as Tolman's own statement of the principle of similitude (repeated below):

> (The Principle of Similitude) *The fundamental entities out of which the physical universe is constructed are of such a nature that from them a miniature universe could be constructed exactly similar in every respect to the present universe.* (Tolman 1914a, 244, his emphasis)

Tolman exhibits the consequences of this principle by way of a thought experiment:

- Consider an observer O with a meter stick that measures the length of some extension, s, to be $l_s = 1\,m$.

- Now consider a counterpart world, a "miniature universe" such that there is a counterpart of the original observer, O', and both his "meterstick" *and* the extension s have been shrunk in length by a factor of x.
- Since both the length of the unit standard and the measured extension have changed by the same factor, the assigned numerical value of the length will be invariant: $l'_s = 1\,m'$.
- The length quantity of the counterpart extension in the miniature universe of O', expressed in the units of O, will be $l'_s = x \cdot 1\,m$ or, more generally, $l' = xl$.

Given that this transformation equation, $l' = xl$, is expressed in a single system of units (it is true in the language of either O or O') it is to be understood as an equation of quantities—this accounts for Tolman's interpretation of the transformation to the miniature universe as a metaphysical transformation but not an empirical one (O' does not have access to unit standards from the original O world). From this and the acceptance of the speed of light postulate, their temporal measurements must also stand in the same relation: $t' = xt$. From assuming the invariance of other laws (e.g. Coulomb's law), Tolman derives a whole set of symmetry transformations:[31]

[31]Table selectively adapted from Tolman (1914a), 226. Note the invariant quantities and the corresponding theoretical commitments of Tolman's principle: the constancy of the speed of light, electromagnetic theory, and the laws of thermodynamics.

Quantity Kind	Symmetry Transformation
Length	$l' = xl$
Time Duration	$t' = xt$
Velocity	$v' = v$
Acceleration	$a' = x^{-1}a$
Mass	$m' = x^{-1}m$
Force	$f' = x^{-2}f$
Energy	$U' = x^{-1}U$
Energy Density	$u' = x^{-4}u$
Electrical Charge	$e' = e$
Entropy	$S' = S$
Temperature	$T' = x^{-1}T$

Table 2.1: Induced transformations of quantity magnitudes under similitude transformations.

From these results Tolman determined the functional form of several physical equations describing important physical phenomena: ideal gases, blackbody radiation, the electromagnetic field, and so on.[32] For example, he finds the functional relation between an electron's mass and radius to be: $m_e = kr_e^{-1}$, where k is some constant.

In the same year Buckingham's landmark paper "On Physically Similar Systems" presents the most influential proof of the Π-theorem, and Buckingham argues that Tolman's principle is only a "particular case" of his result—this is true insofar as Tolman's principle is merely dimensional homogeneity plus an assumption that length is the only basic mechanical dimension (see §2.2.3). I will not here go through

[32]Further, in another paper, Tolman (1914b) derived the equation for the specific heat of solids.

the derivation of the theorem.[33] Buckingham states the essential content of the theorem in terms of absolute units, which corresponds to what I've defined as a coherent unit system above, and, using such a system, there is a duality between active and passive interpretations of changes of the fundamental units, corresponding to the distinction between transformations of formal and ontic dimensions (see §2.2.3).[34] Buckingham's statement of the content of the theorem:

> When absolute units are used, the validity of a complete physical equation is unaffected by changes in the fundamental units. Hence in changing from a system S to a similar system S' it is immaterial to the validity of the equation in question whether we do or do not retain our original fundamental units. If we alter the sizes of the fundamental units $[Q_1] \dots [Q_k]$ in the same ratios as the kinds of quantity $Q_1 \dots Q_k$ which they measure, the numerical value of any quantity of one of these kinds will be the same in both systems. And if we do not change the relations of the derived and fundamental units of our absolute system, every derived unit $[P]$ will change in the same ratio as every quantity P of that kind, so that the numerical value of every quantity in the system S will be equal to the numerical value of the corresponding quantity in the similar system S'. (Buckingham 1914, p. 354)[35]

While Buckingham here follows the Maxwellian fashion of discussing dimensional analysis in terms of invariance of "complete" equations under transformations of the fundamental units, we can understand his claim here as a generalization of Tolman's similitude principle—insofar as the principle of dimensional

[33] See Gibbings (1982, 2011), Sterrett (2009, 2017, 2021), and Pobedrya and Georgievskii (2006).

[34] This active-passive transformation duality can be made intuitive by considering the double interpretation of a *fundamental unit* in the case in which it is defined with respect to a material standard. A passive transformation corresponds to switch from a material meter-long standard for a length unit to a distinct, material foot-long standard for a length unit. An active transformation corresponds to the (metaphysical) compression of a meter-long length standard to a length of one foot. The dual active-passive interpretation of the Π-theorem is dealt with in more detail in Jalloh (Forthcoming).

[35] Walter's discussion contains a claim which requires correction. Walter distinguishes similitude, "a simple way to investigate the manner in which a change of scale affects the properties of physical systems", from dimensional homogeneity, which requires that "the operation of addition and the relationship of equality are valid only for objects [i.e. quantities] of the same kind [i.e. dimension]" (Walter 1990, pp. 86–7). The claim to be criticized is that "Buckingham, like everyone else" conflated these two bits of dimensional reasoning. This claim is false: Buckingham (1914) clearly distinguishes similitude and dimensional homogeneity as he uses the principle of dimensional homogeneity to provide a proof of the Π-theorem, which in turn defines a criterion for physical similarity. One follows from the other, but there is no indication that these are to be equated.

homogeneity is agnostic with respect to particular dimensional systems.[36] It is important to emphasize that the Π-theorem follows (almost) directly from the principle of dimensional homogeneity, and so, for all involved, the results of the Π-theorem, given the assumption of the orthodox dimensional system, wherein mass, length, and time are the basic mechanical quantity dimensions, are results of the *approach* that I am glossing as the principle of dimensional homogeneity. There is a logical distinction between the principle and the principle plus a dimensional system, but the principle has no function independent of the adoption of a dimensional system (hence the "almost").

Given a coherent (or "absolute") unit system, the relations between basic and derived quantities are defined such that arbitrary changes in the magnitudes of the basic quantities, including the fundamental units, *induce* changes in the derivative quantities, and the derived units, such that representationally adequate equations and dimensionally homogeneous (or "complete") equations, interpreted quantitatively or numerically remain true. This is done without stipulating a *particular* invariance with respect to transformations of the *length* quantities. In brief the Π-theorem states thus: All physical equations are dimensionally homogeneous and so can be put in the form:

$$A_1 + A_2 + \cdots + A_N = 0,$$

where each A-term is a product of powers of the fundamental Q-terms (the basic quantities of the dimensional system, e.g. masses, lengths, and times) and each term has the same dimension: $[A_i] = [A_j]$. Therefore, subtracting A_N and then dividing through by $-A_N$ yields an equation with dimensionless Π-terms—the dimensionless Π-terms and the theorem are named from the fact that the dimensionless terms of the equation have the form of product-functions: $\Pi = \prod_i^N Q_i^{x_i}$:

$$\Pi_1 + \Pi_2 + \cdots + \Pi_{N-1} = 1.$$

[36] For more on the Maxwellian prehistory of this debate see Mitchell (2017).

These dimensionless Π-terms will be invariant under any change of *numerical value* (passive transformation) or *magnitude* (active transformation) of the basic dimensions.[37] An example: Let an arbitrary Π-term be the ratio between two masses $m_a = 2000g$ and $m_b = 1000g$, so $\Pi_{\frac{a}{b}} = 2$. If we actively transform the dimension by doubling the masses then $m_a = 4000g$, $mb = 2000g$, and $\Pi_{\frac{a}{b}} = 2$. If we passively transform the dimension by a different choice of scale, a change to kilogram units, then $m_a = 2kg$, $m_b = 1kg$, and $\Pi_{\frac{a}{b}} = 2$.

Buckingham notes that Tolman's principle requires an assumption of speed, charge, and entropy as the invariants of its symmetries—see the table above. For Buckingham this is merely a specific realization of the general Π-theorem, itself a consequence of dimensional homogeneity. This specification is merely an unorthodox choice of dimensional system. To this Buckingham raises three objections to adopting this dimensional system and therefore Tolman's principle. For one, it moves what are thought of as empirical laws from the client discipline of physics to the relatively *a priori* methodological discipline of dimensional analysis:[38]

> "The unnecessary introduction of new postulates into physics is of doubtful advantage, and it seems to me decidedly better, from the physicist's standpoint, not to drag in either electrons or relativity when we can get on just as well without them." (Buckingham 1914, p. 356)

Secondly, it makes this move unnecessarily: Buckingham goes on to show that the principle of dimensional homogeneity with the ordinary dimensional system can derive equations that Tolman credits the principle of similitude with, e.g. the functional relation of the mass and radius of an electron. Thirdly, Buckingham derives the essential inconsistency of Tolman's system and the gravitational law.

Tolman (1915) responds to Buckingham and argues that the principle of similitude is superior to the principle of dimensional homogeneity on grounds of the latter's inability to constrain the functional form

[37] Where a basic dimension is understood as the set of all the quantities of that kind with an ordering that allows for the mapping by a choice of scale to a set of numbers, see Ellis (1964).
[38] Ehrenfest-Afanassjewa (1916b) makes this same complaint.

of equations with dimensional constants of unknown dimensions. These are cases in which dimensional homogeneity necessitates the introduction of *dimensional* constants: Consider Stefan's law, $u = aT^4$. By the lights of the dimensional analyst, *in advance of the establishment of the dimensions of* a, the equation could have a different algebraic form, e.g. $u = aT^3$.

In this case, the dimensional analyst is tasked with determining a function that relates the energy density of a blackbody, u, and its absolute temperature, T. Their respective dimensions, $\mathrm{ML}^{-1}\mathrm{T}^{-2}$ and Θ, are incommensurable, so the principle of dimensional homogeneity is of no help. Without either the dimensions of the mediating constant or the form of the function relating the two inhomogeneous quantities, the dimensional analyst armed only with the principle of dimensional homogeneity can make no derivations.

In contrast, the principle of similitude tells us that u must be numerically equivalent to its scale counterpart, u':

$$u = F(T) = u' = F(T') = x^4 F(x^{-1}T).$$

Referring to the table above we see that u scales with x^4 and T with x^{-1}, so the solution for this equation requires taking temperature to the fourth power, and the equation is only fixed up to a scalar factor, a,[39] yielding Stefan's law:

$$u = aT^4.$$

Now considerations of dimensional homogeneity non-arbitrarily yield the dimensions of the constant. As the dimensional analyst starts with neither the form of the equation nor the dimension of the constant, *the principle of dimensional homogeneity is not determinative.* If the dimensional analyst had the form of the law, the constraint of dimensional homogeneity would immediately yield the dimensions of the constant. If the dimensional analyst has the dimensions of the constant, the constraint of dimensional homogeneity

[39] One way to think about the nature of the functional results yielded by either form of dimensional analysis is that the results give the family of curves that corresponds to the function, but doesn't give you the value of the coefficients. Those are found by experiment (see Gibbings 1974, 2011, on the relation of dimensional analysis to experiment).

would (up to a scale factor) determine the functional, algebraic form of the equation, as is usually done in dimensional derivations.

Tolman puts the relation of the two principles thus:

> Where dimensional constants enter, the principle of dimensional homogeneity is of no avail in predicting the form of a relation, since we cannot tell beforehand what the dimensions of the constant are going to be. For such problems we must have recourse to the principle of similitude. On the other hand, when dimensional constants do not enter into the relation, although we may apply either principle, the principle of similitude is usually the less powerful since it merely prescribes invariance when the different measurements are multiplied by powers of a single arbitrary multiplier x, while the principle of dimensional homogeneity prescribes the more drastic requirement of invariance when the multiplications are carried out with a different arbitrary multiplier for each fundamental property. (Tolman 1915, p. 232)

Understanding Tolman's claim relies on distinguishing two ways in which a principle may be "stronger". The first way is that a principle may be *logically* stronger than another: in this case, the principle is the stronger principle as it provides more determinate derivations than the principle of dimensional homogeneity does, particularly in the cases in which there is a dimensional constant of unknown dimension.[40] The second is that is that one principle may be more *robust* than another: in this case the principle of dimensional homogeneity is more robust principle as it is commutes with more supposed symmetry transformations, particular arbitrary transformations of the mass, length, and time (and the induced transformations on the derivative dimensions) that do not conform to the similitude transformations given in the table. With some irony (but not too much), the standard of logical strength we associate with Euclid is the guide to fundamentality that all physicists adopt—the standard of logical strength. On the other hand,

[40] As the principle of dimensional homogeneity provides no constraint on the functional structure of such equations, one might like to say that these are cases in which the principle of dimensional homogeneity is *inapplicable* and that the principle of similitude enjoys a wider range of applicability.

in mathematical contexts, robustness seems to be the guide to fundamentality with respect to principles (the fewer assumptions the better).[41] The question with respect to the operative logical standard is one of efficiency: how much am I getting for what cost? On this standard the principle of similitude would win out—if it wasn't false.

2.2.3 Tolman v. Ehrenfest-Afanassjewa

There is an interpretative issue that will bring us back to the metaphysical considerations at hand. Tatiana Ehrenfest-Afanassjewa[42] most clearly states an objection to Tolman's principle shared by the other respondents: The principle of similitude is merely an application of the principle of dimensional homogeneity to a special dimensional system, and if the assumption of this dimensional system is unfounded, the principle is specious. From the first paragraph:

> An accurate analysis shows that Tolman's considerations possess at least a close connection with the reduction to a definite hypothesis of the conviction of the homogeneity [unit invariance][43] of all the equations of physics, a conviction which is commonly used without any foundation. This is not the intention of the author, as appears from his third paper on the same subject, yet he really does nothing else but construct a system of dimensions of his own

[41] The reason for the discrepancy between the standards of mathematics and physics comes from different standards of modality. From the physicist's perspective many mathematical models are just that and the robustness criterion is of no relevance when a class of possible worlds is fixed. So another way to put the debate between Tolman and his critics is that they disagree about the class of physically possible models (given the empirical data).

[42] Walter's (1990) account of this historical debate is overly dismissive of Ehrenfest-Afanassjewa's contributions, especially her later, post-*Dimensional Analysis*, mathematical intervention (Ehrenfest-Afanassjewa 1926), which is only described as "extensive and confusing" (Walter 1990, p. 101). This dismissal is unfortunately mirrored in responses by Bridgman (1926a) and Campbell (1926)—though Bridgman includes Ehrenfest-Afanassjewa (1926) in the list of important references which have appeared in-between editions of *Dimensional Analysis*. (The list can be found in the preface to the revised edition.) A major reconsideration of her work in dimensional analysis is under development, but see also San Juan (1947), Palacios (1964), and Johnson (2018) for developments of her approach to dimensional analysis. See Uffink et al. (2021) for a more general reevaluation of her work in mathematics and physics.

[43] Homogeneity, i.e. unit invariance, is sometimes treated as the fundamental principle of dimensional analysis in lieu of dimensional homogeneity. Authors vary on which is to be taken as axiomatic and which is to be derived, but the cases in which unit invariance and dimensional homogeneity come apart are so few and spurious as to be dismissed for our purposes (cf. Bridgman 1931). I treat both approaches as the "dimensional homogeneity" approach. For more on the mathematical definition of homogeneity, see Ehrenfest-Afanassjewa (1926), San Juan (1947), and Palacios (1964).

> (indeed one that in some respects deviates from the C.G.S. system), and he examines all equations with a view to homogeneity *as regards this system of dimensions*. (Ehrenfest-Afanassjewa 1916b, 1, her emphasis)

While Tolman (1916) rejects the presentation of his principle as determining a system of dimensions, he accepts the presentation of the relationship between the two principles: The principle of similitude involves a further empirical *ansatz* which is to be settled by the investigations into the nature of gravity, and the principle is to be given methodological priority due to its usefulness. His disagreement with Ehrenfest-Afanassjewa can be clarified by way of a distinction made in §2.1.3. When Ehrenfest-Afanassjewa states that Tolman is establishing a principle of homogeneity restricted to a special set of dimensions she is referring to *formal* dimensions—dimensions considered only as change-ratios for a group of unit systems. When Tolman claims that this is not the case, he is considering *ontic* dimensions—dimensions considered as descriptions of the nature of quantities via their dimensional formulae.

> (Formal Dimensions) Dimensions encode the transformations of numerical representations of quantities due to changes in unit systems.

> (Ontic Dimensions) Dimensions are properties of quantities in physical systems; they encode similarity relations that are invariant between scaled systems.[44]

We could just as well distinguish these as unit-dimensions and quantity-dimensions.

Ehrenfest-Afanassjewa argues that Tolman's similitude transformations should only be understood as *formal* or *representational*, transformations, i.e. unit changes.[45] She places conditions on Tolman's *ontic* interpretation of these transformations as indicating actual changes in size, e.g. a miniature universe:

[44]This distinction is given by Johnson (2018), 105-112. A similar distinction between dimension-first and unit-first attempts to provide a mathematical model for the quantity calculus is noted by Raposo (2018)—this distinction first became clear in the 1930s, see Abraham (1933). See also Sterrett (2009) for the connection between similarity relations and ontic quantity dimensions.

[45]"The transition from the numbers x_i to x'_i may also be thought of in another way: instead of imagining measurements to be made with the same units in two different worlds, we may conceive the measurements to be carried out applying two different sets of units to the same objects ('in the same world')." (Ehrenfest-Afanassjewa 1916b, p. 3)

(1) that a model universe in the sense defined above is possible,

(2) that we possess all equations which are wanted for a full description of the whole universe,

(3) that the latter condition is especially fulfilled by those equations which in the C.G.S. system serve to fix the dimensions of the different quantities. (Ehrenfest-Afanassjewa 1916b, p. 4)

To these conditions she raises three objections. First, the unit transformation coefficients (or scale factors) for time, length, and mass (and so on) are fixed independently of any investigation into the possibility of such model universes. Second, the full description condition necessitates that the transformation coefficients (she also says, in quotes, the "dimensions") of the other quantities are fixed by the transformation so that definitions of novel, i.e. non-mechanical, quantities are invariant under such transformations—this unnecessarily reduces the total number of dimensions ("the number of degrees of freedom of the transformation", i.e. the number of independent, basic dimensions). Third, there is no reason to think that the current fundamental dimensions are sufficient to capture all of nature ("which should give a *necessary* reduction of the degrees of freedom" in the dimensional system), and Tolman's reduced mechanical basis (consisting of just length) is insufficient to capture Newtonian gravity.[46]

Tolman objects to Ehrenfest-Afanassjewa's characterization of his principle as determining another "system of dimensions" distinct from that corresponding to the then standard centimeter-gram-second unit system[47]—at least insofar as dimensions are understood in the ontic sense, as dimensions of quantities

[46]Ehrenfest-Afanassjewa suggests a strategy for saving the ontic interpretation of the dimensional symmetries: the scaling of dimensional constants so as to guarantee quantity symmetries. See Roberts (2016); Jacobs (2022); Jalloh (Forthcoming) for contemporary arguments for this strategy; see Martens (In Press) for criticism of this strategy. The introduced constant can be understood two ways: either as some real quantity, like a postulated constant of matter, or else "denote it as a product of special values of the active variables occurring in the equation" (Ehrenfest-Afanassjewa 1916b, p. 5). She develops this more thoroughly as the introduction of "formal variables" in Ehrenfest-Afanassjewa (1916a). The upshot: such an extension of the " 'physical' meaning of the constants" trivializes the possibility of active scale transformations and the invariance of equations under such transformations, and so "ceases to afford a criterion for distinguishing between equations which are 'physically allowable' and arbitrary equations"(Ehrenfest-Afanassjewa 1916b, p. 6).

[47]The dimensional system for which C.G.S. is a coherent unit system (see §2.1.3). In this respect there is no difference between the C.G.S. system and a M.K.S. system.

and not as dimensions of units. Tolman gives an initial statement of the fundamentalist conception of a ontic system of dimensions:

> The dimensions of a quantity may be best regarded, I believe, as a shorthand statement of the definition of that kind of quantity in terms of certain fundamental kinds of quantity, and hence also as an expression of the essential physical nature of the quantity in question. If, for example, we define force as mass times acceleration, the dimensions of force will be $[mlt^{-2}]$ and this may be regarded as a shorthand recapitulation of the definition of force in terms of mass, length and time, and also as an expression of the essential physical nature of force. (Tolman 1916, p. 9)

Tolman argues that the second principle invoked, that the dimensions of a quantity expresses the essential nature of that quantity *grounds* the principle of dimensional homogeneity. That an equation must have terms of equal exponent in each basic dimension on either side follows if equations are taken not only to describe numerical equalities, but also *quantity identities*. Here Tolman assimilates the definition of derived quantity dimensions and their metaphysical constitution. That the nature of physical quantities does not unproblematically follow from their dimensional formulae is discussed in the literature (e.g. Johnson 2018; Skow 2017)—Tolman's conflation of definition and constitution is a target of Bridgman's conventionalist critique.

The ontic interpretation of dimensional systems makes clear Tolman's reason for denying that the principle of similitude provides one. According to the principle of dimensional homogeneity force is defined and constituted by mass, length, and time, according to the formula: $[f] = \mathrm{MLT}^{-2}$. Under the system of dimensions that would be given by the principle of similitude, force is a function only of length, $[f] = \mathrm{L}^{-2}$. If Tolman were committed to a system of dimensions given by the principle of similitude, he would say the principle attributes force the *nature* of an inverse area. For this reason he later does not say that his principle provides a system of (ontic) dimensions, but rather is an *ansatz* which is useful in some circumstances and whose principal commitment, the possibility of miniature universes, is available for empirical (dis)confirmation, by way of the implied theory of gravity. This is a retreat from Tolman's original metaphysical interpretation of his similitude transformations.

2.2.4 Tolman v. Bridgman

Tolman's principle *qua* empirical *ansatz* is the target of Bridgman's critique:

> If the exact form of the equations and their mode of application should turn out to be exactly identifiable with the corresponding manipulations of the theory of dimensions, then the principle of similitude must be judged not to be new, in spite of the form of statement above. I shall try to show in this note that such an identification is possible; that in so far as the principle of similitude is correct it gives no results not attainable by dimensional reasoning, and that in its universal form as stated above it cannot be correct. (Bridgman 1916, p. 424)[48]

Bridgman's aim, then, is to show that Tolman's principle of similitude is more determinative than the principle of dimensional homogeneity at the cost of reliability.

Bridgman diagnoses Tolman's apparent examples of the greater determinativity of the principle of similitude (Stefan's law, the gas equation, etc.) by drawing attention to a special feature of the dimensional constants involved:

> The principle of similitude may be applied with correct results to all those cases in which the dimensional constants have such a special form that they are not changed in numerical magnitude by the restricted change of units allowed by the principle. (Bridgman 1916, p. 425)

[48] Where "the universal form" is the statement that the materials which constitute the universe could be used to create an empirically indistinguishable universe which differed only in size.

The dimensions of Stefan's constant, a, are $\mathrm{ML^{-1}T^{-2}\Theta^{-4}}$, so we can express a as $N_a m l^{-1} t^{-2} \theta^4$, where N_a is some dimensionless number and m, l, t, and θ are units of mass, length, time, and temperature, respectively. Now apply the principle of similitude:

$$a = a' = N_a x m' x l'^{-1} x^2 t'^{-2} x^{-4} \theta'^{-4} = N_a m' l'^{-1} t'^{-2} \theta'^{-4}.$$

The x factors cancel and the numerical value of Stefan's constant is invariably N_a. That only some such constants are invariant under dimensional scale transformations is evident in Tolman's failure to capture Newtonian Gravitation: $G = N_G \mathrm{M^{-1}L^3T^{-2}}$ scales with factor x^{-2}. The conclusion of Bridgman's argument is that the method of similitude requires an assumption regarding the dimensionality of the relevant constant(s) just as the method of dimensional homogeneity does: a user of the principle of similitude must assume that the dimensional constants which figure in the fundamental equations are such that their dimensional transformation coefficients cancel out. This assumption bears out surprisingly often: In addition to a, Bridgman cites the gas constant, the velocity of light, and the constant of quantum action. Is there some metaphysical significance to this seeming conspiracy of the dimensional constants?

Bridgman answers in the negative; the apparent conspiracy can be explained by the dimensional structure of our conventionally defined unit systems. By limiting valid unit transformations to those that leave some choice of constants invariant, e.g. c and e in Tolman's system, a number of consistent systems of dimensions can be defined. Bridgman amplifies Buckingham's observation that the number of independent basic dimensions or units can be determined by the number of unit-invariant quantity relations, i.e. laws, we chose to accept as axiomatic (or relatively *a priori* as indicated in §2.1.2). Apparently, then, the number of basic quantity dimensions (and so dimensional constants) is conventional. If force, for example, was to be set as an additional fundamental quantity, this could be accommodated by the introduction of a new dimensional constant to Newton's second law. Instead we take the law, with this would-be constant set to unity, as a unit-invariant axiom. Bridgman argues that we accept dimensional *definitions* not owing to some metaphysical identity but due to the frequency of the corresponding experimental fact.

Bridgman provides a helpful demonstration of the conventionality involved. I will modify his convention of using the square brackets $[x]$ to using curly brackets $\{x\}$ denote the unitless numerical value of x (in line with contemporary standards, see JCGM 2012). Bridgman provides a description of each of the constants of nature in terms of the fundamental units (5 constants and 5 basic units):[49]

$$G = \{G\}m^{-1}l^3t^{-2} = \{G'\}m'^{-1}l'^3t'^{-2}$$

$$c = \{c\}lt^{-1} = \{c'\}l't'^{-1}$$

$$k = \{k\}ml^2t^{-2}\theta^{-1} = \{k'\}m'l'^2t^{'}-2\theta^{'}-1$$

$$h = \{h\}ml^{-2}t^{-1} = \{h'\}m'l'^{-2}t'^{-1}$$

$$E = \{E\}e^{-2}ml^{-3}t^{-2} = \{E'\}e'^{-2}m'l'^{-3}t^{-2}$$

These equations can be used to determine the value of the constants under changes of fundamental units. Or instead they can be reformulated in order to determine the unit transformations that keep the values of the constants fixed:

$$l'^2 = \frac{\{h\}}{\{h'\}}\left(\frac{\{c\}}{\{c'\}}\right)^{-3}\frac{\{G\}}{\{G'\}}l^2$$
$$t'^2 = \frac{\{h\}}{\{h'\}}\left(\frac{\{c\}}{\{c'\}}\right)^{-5}\frac{\{G\}}{\{G'\}}t^2$$
$$m'^2 = \frac{\{h\}}{\{h'\}}\frac{\{c\}}{\{c'\}}\left(\frac{\{G\}}{\{G'\}}\right)^{-1}m^2$$
$$\theta'^2 = \frac{\{h\}}{\{h'\}}\left(\frac{\{c\}}{\{c'\}}\right)^{5}\left(\frac{\{k\}}{\{k'\}}\right)^{-2}\frac{\{G\}}{\{G'\}}\theta^2$$
$$e'^2 = \frac{\{h\}}{\{h'\}}\frac{\{c\}}{\{c'\}}\left(\frac{\{E\}}{\{E'\}}\right)^{-1}e^2$$

[49] G is the gravitational constant; c is the light constant; k is the (Boltzmann) thermodynamic constant; h is the quantum constant; E is the (Coulomb) electric force constant. The following two sets of equations are adapted from Bridgman (1916), 429.

Tolman's transformation equations can be derived by holding all constants fixed except for G. However different transformation equations can be defined by varying other constants and holding G fixed. In each of these systems *some* constant or other is the odd man out, i.e. is variant under similitude transformations. Generally speaking, if we wish to freely vary some number of the fundamental units (like Tolman does for length), we will have to vary the same number of universal constants. The indeterminancy of *which* constants are varied due to the conventional choice of *which* fundamental unit (i.e. basic dimension) to ground our dimensional system in (i.e. a choice of alternative similitude principles) was taken by Bridgman to undermine Tolman's characterization of his principle as an empirical *ansatz* to guide the development of a novel theory of gravity. There is no more reason to hope for a new theory of *gravity* guided by a similitude principle than a new theory of *electricity*. The constant, and so the physical theory, that "the" principle of similitude is in tension with is a matter of arbitrary choice. This arbitrariness—reducing time to length rather than reducing length to time—is unavoidable for Tolman in the absence of an ontic conception of his dimensional system. In other words, the choice of dimensional system associated with the principle of dimensional homogeneity is arbitrary and a generalized principle of similitude does not yield *unique* empirical predictions—which is to be expected given Tolman's retreat to presenting the principle as only defining a formal system of dimensions (see §2.2.3).

Tolman presents a full-fledged metaphysical account of "measurable quantities" in his final response regarding the principle of similitude. This account is in no way reactionary—it does not constitute an argument in favor of the principle of similitude—but rather is to serve a foundational purpose:

> The time is already ripe for a much more comprehensive and systematic treatment of the field of mathematical physics than has hitherto been attempted, and the completion of this task would make it possible to derive all the equations of mathematical physics from a few consistent and independent postulates, and to define all the quantities occurring in these equations in terms of a small number of indefinables. The purpose of this article is to discuss from a

> somewhat general point of view the nature of the quantities which occur in the equations of mathematical physics and to consider a set of indefinables for their definition. We shall thus hope to help in the preparation for that more complete systematization of mathematical physics which is undoubtedly coming. (Tolman 1917, p. 237)

Tolman aims to prepare the way for a generally axiomatic treatment of physics as a whole.[50]

Tolman reintroduces his metaphysical posit by way of discussing the relation that holds between fundamental and derived quantities, which is represented by dimensional formulae:

> *The dimensional formula of a quantity may be regarded as a shorthand statement of the definition of that kind of quantity in terms of the kinds of quantity chosen as fundamental, and hence also as a partial statement of the "physical nature" of the quantity in question.* (Tolman 1917, 242, his emphasis)[51]

Tolman holds that the apparent necessity of five fundamental quantity dimensions (three mechanical ones, one for electromagnetism, another for thermodynamics) is due to there being "five fundamentally different kinds of 'thing' ": space, time, matter, electricity, and entropy.

Beyond being sufficient to account for all known physical quantities, Tolman puts forth two further conditions on a set of fundamental quantity dimensions. The fundamental quantities must be extensive—this allows for extensive methods of measurement for all derived quantities even those that are themselves intensive (consider the role of a thermometer in measuring the temperature).[52] The set of fundamental quantity dimensions must also be such that they provide an optimal level of simplicity to the system of quantities.

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[50] Appropriate to the generality of his aims, Tolman takes Russell's (1903) distinction of magnitude and quantity as his starting point. Tolman's system, including his fundamental distinction of intensive and extensive quantities cannot be dealt with here in full.

[51] That dimension can at most only be a partial description of the nature of a quantity is here set aside, see Lodge (1888) and Mari (2009).

[52] "In case the derived quantity has intensive rather than extensive magnitude some more or less artificial correlation of the magnitude in question with quantities having extensive magnitude will then have to be used, as has been done in the case of our ordinary temperature scale." (Tolman 1917, p. 248)

With all this on the table, Tolman argues that Bridgman's conventionalism is due to a confusion of quantity-dimension and unit-dimension:

> The fact that it has become usual to pick out the units for derived quantities in the way indicated has sometimes led to an unfortunate confusion as to the real significance of dimensional formulae. Thus there has grown up the practice of speaking of the dimensions of a unit when what is really intended is the dimensions of the quantity involved. It certainly seems best, however, to use the dimensional formula of a quantity as a shorthand restatement of its definition in terms of the fundamental kinds of quantity. The dimensional formula is thus a symbol for the physical nature of the derived quantity and a recapitulation of the *necessary* relation between different kinds of quantity rather than the statement of a relation between units which we find convenient. (Tolman 1917, p. 249)

The dimensional relations between quantities are *necessary*, not conventional. This distinguishes quantity-dimensions from unit-dimensions, or dimensional systems from unit systems (see §2.1.3). Generally speaking a dimensional system or a unit system can be used to fix the other, by defining a coherent system of units. Non-standard dimensional systems are often defined in this way by setting a constant equal to one and eliminating one kind of unit for another, e.g. the spatialization of time *units* in relativity theory upon the adoption of the light postulate, if one takes this to be a true *elimination* of the constant c then one adopts a *dimensional* system in which time and length *quantities* are equivalent.[53] Tolman rejects any such conventionalism regarding the basic quantity dimensions. For him the reduction of the time dimension to space dimensions would be the same as reducing pressure to volume on account using them to form a two dimensional graph—a well founded correlation is insufficient for a dimensional reduction, let alone the

[53]Physicists often talk in this manner, but it is apparent that they usually take this to only be a change in unit systems and not in dimensional systems. The "suppressed" constants return when it is time for physical interpretation (compare Rücker 1888).

reduction of a *fundamental* quantity dimension.[54] By distinguishing the necessary dimensional relations of quantities from the conventional "dimensional" relations of units, Tolman takes himself to be reiterating what I am calling the ontic-formal dimension distinction he made in Tolman (1916). This confusion between the "dimensions of quantity" and "dimensions of unit" he claims may be "a contributory cause for a number of criticisms which have been made on the principle of similitude." (Tolman 1917, p. 251) That said, Tolman stops short of an explicit defense of his principle and, as far as I've seen, never defends or makes use of it again. As I will argue in the next section, the points he makes against Bridgman's libertine conventionalism does point the way to a metaphysics of quantity dimensions, but one weaker than the quantity dimension fundamentalism that he develops over the course the debate concerning his principle of similitude.

2.2.5 Verdicts

As mentioned above, the failure of Tolman's principle of similitude was overdetermined. There is, however, much to learn about the foundations of dimensional analysis from the debate concerning its relation to the principle of dimensional homogeneity. Here are the results we may take from each of the criticisms discussed above.

Buckingham correctly shows that the principle of dimensional homogeneity, and the Π-theorem which follows from it, can generate a broad class of symmetry transformations of which Tolman's "relativity of size" is only a special case corresponding to the adoption of an unorthodox dimensional system. Tolman is right to claim that the principle of similitude is the more determinative principle, it can be used to derive functional equations for systems in which the dimension of the relevant constant is unknown—a situation in which the principle of dimensional homogeneity alone is useless.

[54]Though Tolman is a metaphysical realist about dimension, he thinks what we take to be the number of dimensions is a manner of empirical inquiry. The special sciences, following the example of thermodynamics, may introduce new kinds of measurable quantities (e.g. economics). The reduction of the number of dimensions seemed to him impossible, but not logically so.

Ehrenfest-Afanassjewa sharpens the criticism that Tolman's principle is merely setting up different dimensional system from the standard one realized in the C.G.S. unit system. She argues that while a mechanical dimension system with only a basic length dimension may be set up, Tolman has not met the conditions needed to give what is in the first instance a *unit* transformation an *ontic* interpretation—one such condition will be a change in the magnitude of the gravitational constant across the similitude transformation, a transformation she takes to be nomologically impossible. Tolman capitulates that his principle only works as setting up a formal system of units—though he thinks this may still constrain the form of future theories of gravity—and puts forward a fundamentalist metaphysics of dimensions, independent of the form of fundamentalism (length fundamentalism) apparently adopted in his initial 1914 paper.

Bridgman shows the apparent extra domain of determinativity to *not* be an argument in favor of the methodological priority of Tolman's principle of similitude, contrary to Tolman's rejoinder to Buckingham. For one, the epistemic benefit of the principle is limited as it depends on an assumption about the dimensions of the relevant constant, though not its exact dimensional formula: its dimensions must be such that it is invariant under the similitude transformation. While this turns out to generally be the case (with the notable exception of G), Bridgman shows that given the number of constants and the number of basic dimensions ("fundamental units") any principle of similitude based on the scaling of a single such basic dimension would lead to *some* constant or another being left out, depending on which laws are held to be invariant under the transformation. The similitude transformations follow from this conventional choice and dimensional homogeneity, and Tolman's chosen unit system fails to be empirically adequate in the case of gravity. Tolman, systematizing his response to Ehrenfest-Afanassjewa, does not fully defend the principle of similitude but aims to clarify a confusion that he takes to be behind criticisms of the principle levied by Bridgman and others. Tolman distinguishes between ontic quantity dimensions and formal unit dimensions and claims that Bridgman's conventionalist argument depends on a confusion between the two. While unit systems are indeed conventional, dimensional systems, constituted by dimensional formulae, are supposed to be representative of the intrinsic metaphysical nature of the quantities they describe: we cannot conventionally chose the basic quantity dimensions. This marks a complete rejection of the ontic interpretation of the principle of similitude, but it also marks the beginning of a debate regarding the metaphysics of quantity dimensions that has largely been neglected.

2.3 Recovering Dimensional Realism: Arguments Against Conventionalism

In this section I summarize the two metaphysical accounts of quantity dimensions which emerge from the early methodological debate and propose a synthesis which overcomes difficulties with both positions. As described in §2.1.4, fundamentalism, the metaphysics of dimensions espoused by Tolman, and conventionalism, the anti-metaphysics espoused by Bridgman, can be understood as opposite positions regarding two theses:

> (Dimensional Realism) There is objective dimensional structure that corresponds to a dimensional system.
>
> (Fundamental Basis) There is a fundamental dimensional structure that corresponds to a dimensional basis.

The fundamentalist accepts both theses, and the conventionalist rejects both theses. The conventionalist case against Fundamental Basis relies on the symmetry in defining equations: we can just as well take $f = ma$ to define the force dimension in terms of the dimensions of mass and acceleration as we can take it to define the mass dimension in terms of the dimensions of force and acceleration—much like how the logical operators ($\neg$, $\vee$, $\wedge$, $\supset$) or the quantifiers ($\exists$, $\forall$) are interdefinable. The conventionalist case against Dimensional Realism therefore follows: If there exist a number of acceptable sets of basic dimensions, then there is no unique dimensional system that represents objective dimensional structure. The conventionalist takes the existence of such symmetry transformations and the following lack of a unique dimensional system to provide evidence for the further antirealist claim that there is no objective dimensional structure.

I will here make the case that there is a dimensional realism that can be recovered in light of the conventionalist symmetry argument. The conventionalist would be too rash if they were to take their symmetry argument to show that there is no dimensional structure whatsoever: Earlier I distinguished dimensional systems by their basis dimensions and their dimensional formulae (see §2.1.4); however, I will now show that the objective dimensional structure that is represented by such dimensional systems is more coarse-grained. Here, I will not attempt to give a new model of dimensional systems that is "reduced" so that there is nothing in a dimensional system that does not correspond to objective dimensional structure. I will rather present a "sophisticated" account of dimensional systems such that equivalent dimensional systems related by an isomorphism (a change of basis) are taken to represent the same objective dimensional structure.[55] In order to recover some form of dimensional realism some distinctions regarding the relations between dimensional systems and dimensional structure must be made. To do this I delineate different strengths of the realist theses, yielding two strong fundamentalist commitments and two weaker realist commitments:

> (Dimensional Representation) Dimensional systems represent objective dimensional structure.

> (Dimensional Uniqueness) There is a uniquely correct dimensional system that represents the objective dimensional structure.

[55]On the difference between reduced and sophisticated theories see Dewar (2019) and Martens and Read (2020). I will not make a stand here on whether a reduced theory is preferable to a sophisticated one or if a reduced one is in this case possible; It is just the case that a sophisticated theory of dimensional systems is readily available to me while a reduced one is not.

(Fundamental Basis Size) The size of the set of basic quantity dimensions is objectively determined.

(Fundamental Basis Identity) The individual identities of basic quantity dimensions are objectively determined.

Dimensional Representation and Dimensional Uniqueness are weaker and stronger versions of Dimensional Realism, respectively. This analysis is to be understood similarly to van Fraassen's (1989) analysis of scientific realism. Dimensional Representation is a statement that dimensional systems are to be taken *literally*: they purport to represent something objective, and so can be judged to do so more or less adequately; Dimensional Uniqueness and says that only one such dimensional system is ultimately correct. Similarly Fundamental Basis Size and Fundamental Basis Identity are weaker and stronger forms of Fundamental Basis, respectively. There are two possibly objective aspects of the fundamental dimensional structure. I will argue that we can be realist about one aspect of the basis of dimensional systems (size) without being realist about the other (identity).

The conventionalist argument against Fundamental Basis is only partially successful: conventionalist transformations of the *identities* but not the *number of* basic quantity dimensions are consistent with the empirical success of dimensional analysis. A dimensional system for mechanics which treats force as a basic quantity (and mass as derived) *is as empirically adequate as* a dimensional system which treats mass as a basic quantity instead. However, while there appears to be no natural constraint on *which* quantity dimensions appear as basic, there is a natural lower limit on the number of quantity dimensions that can adequately represent a physical system. In fact, in Tolman's rebuttal to Bridgman's conventionalism, he puts forward the essential argument in favor of the objectivity of the number of basic quantity dimensions: the problem of insufficient bases. The problem is that the reduction of the number of basic quantity dimensions reduces the determinative power of the principle of dimensional homogeneity—therefore it seems that the reduced dimensional system misrepresents some dimensional structure necessary to have a determinative dimensional analysis of physical systems. For example, Tolman (1917, p. 250) shows that the dimensional analytic derivation of the equation for the centripetal force,

$$f = k\frac{mv^2}{r},$$

becomes much less determinate when the dimensions of length and time are equated (reducing the basic mechanical dimensions to two by making velocity dimensionless):[56]

$$f = k\frac{mv^n}{r}.$$

This is evidence that a dimensional system which collapses the length and time dimensions lacks the representational capacity to adequately describe the centripetal force—Palacios (1964) calls such violations of this natural constraint the problem of insufficient or incomplete bases.[57] However, Tolman went too far in holding that this shows that the the *identities* of the basic quantity dimensions are objectively determined by nature; it is in fact the *number* of basic dimensions that are so determined.

If the dimensional realist must accept Dimensional Uniqueness and so Fundamental Basis Identity, then the conventionalist attack on Fundamental Basis Identity is enough to justify an antirealism about quantity dimensions. However, the dimensional realist can accept just the weaker theses, Dimensional Representation and Objective Basis Size: the form and ramifications of this non-fundamentalist dimensional realism will be sketched, but first the case against a thoroughgoing conventionalism needs to be given. In what follows I give two arguments against an antirealist conventionalism; the first is the problem of insufficient bases, which is revealed by the Rayleigh-Riabouchinsky paradox; the second is the inability of the conventionalist to account for the explanatory nature of dimensional analysis altogether.

[56] This is even worse when you consider that v^n could be folded into k, hiding any dependence on velocity.

[57] While Palacios sometimes discusses this problem in terms of “incomplete” systems, I prefer to use “insufficient”, following some of his usage and some of the terminology in the subsequent literature (e.g. Johnson 2018; cf. Gibbings 2011).

2.3.1 The Generalized Rayleigh-Riabouchinsky Paradox and the Problem of Insufficient Bases

In an early exposition of dimensional analysis, Rayleigh (1915) uses dimensional analysis to derive equations for a number of systems, including a case of heat transfer between a rigid rod and a stream of fluid (Boussinesq's problem). Riabouchinsky (1915) showed that by reducing the number of dimensions involved in describing the system from four to three—by eliminating the independent dimension of temperature through an adoption of the mechanical theory of heat—dimensional analysis results in a less determinate result. It would seem then that we have a paradox: *more* knowledge about the system—that temperature has equivalent dimension to energy—yields a *less* informative result! This surprising result shows that not all laws can be taken to give reductive dimensional formulae in any context, and so the size of the basis of a dimensional system is not fully conventional but rather is restricted on one side by nature—the elimination of an independent, basic temperature dimension leads to an inadequate representation of the heat transfer system.

We can better understand this so-called paradox and the problem it raises by consideration of a simpler case—the Rayleigh-Riabouchinsky paradox can be generalized to an observation regarding the determinancy of dimensional systems in general. The case of dimensional reduction I wish to consider here in fact appears in Buckingham's (1914, pp. 372–375) response to Tolman: the reduction of the mechanical dimensional system's basis from three to two basic dimensions by using Newton's force laws to define a dimensional formula for mass in terms of length and time.[58] First I will show how such a dimensional reduction is done and then show how it leads to lower specificity in the derivation of the period of a pendulum when compared to the treatment in §2.1.1.

[58]The choice of which dimension is reduced to the other two is arbitrary, though this kinematic reduction recalls a "Laplacian" reduction of mass (see Martens 2018).

First we set Newton's two force laws equal to each other,

$$G\frac{mm'}{r^2} = m'a,$$

where m and m' are the masses of the pendulum and the Earth (it doesn't matter which is which as the forces on each mass should be equivalent). Then by simplifying the expression we get:

$$G\frac{m}{r^2} = a.$$

Now we make G into a dimensionless number and, for convenience, assume we're working in a coherent set of units such that $G = 1$. The resulting equation,

$$m = ar^2,$$

will define the unit mass, with dimensions,

$$[m] = [a][r^2] = \mathrm{LT}^{-2}\mathrm{L}^2 = \mathrm{L}^3\mathrm{T}^{-2}.$$

We make the same assumption regarding the quantities which may be involved in modeling a simple pendulum, in our reduced kinematic dimensional system:

$$[t] = \mathrm{T}$$

$$[m] = \mathrm{L}^3\mathrm{T}^{-2}$$

$$[l] = \mathrm{L}$$

$$[g] = \mathrm{LT}^{-2}.$$

Now it is not clear from inspection that mass is irrelevant to the pendulum period. We can be more systematic and apply the algorithmic method supplied from the Π-theorem; this will provide us with the concepts needed to understand in full generality the problem of insufficient bases.

One of the most important aspects of the Π-theorem is that it informs us that for any system the number of quantities that describe the system, N, and number of basic dimensions from which the dimensions of those quantities are derived, B, determine the number of dimensionless Π-terms which are sufficient to describe the system: $N - B$. In this case $N = 4$ and $B = 2$, so we should expect there to be two Π-terms sufficient to describe the pendulum. To determine the forms of the Π-terms, we must solve two sets of equations for the dimensional exponents of the component terms. We choose the simplest case, with an eye towards an expression for t, for each Π-term: $\Pi_1 \propto t^1 l^0$ and $\Pi_2 \propto t^0 l^1$. So the Π-terms will each have the form:

$$\Pi_1 = t m^{\alpha_1} g^{\beta_1}$$
$$\Pi_2 = l m^{\alpha_2} g^{\beta_2}.$$

If we solve the resultant set of linear equations we get the solutions:

$$\Pi_1 = t m^{-\frac{1}{2}} g^{\frac{1}{2}}$$
$$\Pi_2 = l m^{-\frac{1}{4}} g^{\frac{3}{4}}.$$

Solving for t we get the equation $t = k m^{\frac{1}{2}} g^{-\frac{1}{2}} \Psi(l m^{-\frac{1}{4}} g^{\frac{3}{4}})$, where Ψ is some power function. Alternatively, we may represent this power function by introducing an arbitrary exponent n, resulting in the somewhat simpler expression $t = k l^n m^{\frac{1}{2} - \frac{1}{4}n} g^{-\frac{1}{2} + \frac{3}{4}n}$. This compares unfavorably with the more determinate equation derived in the full mechanical dimensional system, $t = k\sqrt{\frac{l}{g}}$. The move to a reduced basis generates spurious Π-terms, which leads to less determinate equations.

On the other hand, at some point an increase in the number of basic dimensions will not reduce the number of Π-terms that describe a system. Palacios uses these conditions to provide criteria for insufficient and superabundant systems:

> *[I]f it happens that in augmenting in some way a given basis, the number of independent π monomials decreases, then the original basis was incomplete [insufficient], whilst if the same system of such monomials is always obtained then, the original basis is complete and the augmented one is superabundant.* (Palacios 1964, 67, his emphasis)

I dub a dimensional system that is neither insufficient (or incomplete) nor superabundant with respect to a physical system to be a *well-tuned* dimensional system.

That a dimensional system can be more or less well-tuned, that there is an objective standard (maximally efficient dimensional analysis) for how well a dimensional system describes physical systems belies the conventionalist position. The conventionalist cannot account for the differences between insufficient, well-tuned, and superabundant dimensional systems, while the dimensional realist has an easy answer: a well-tuned system accurately represents the dimensional structure of the physical system in question, an insufficient dimensional system lacks certain representational capacities, and a superabundant system has unnecessary resources. Nature constrains the number of basic dimensions from below; a general Occamist norm constrains the number of basic dimensions from above.

2.3.2 Accounting for Dimensional Explanations

Recently philosophers of science have provided accounts of how dimensional analysis provides explanations, and in doing so have attempted to eliminate any sense of "paradox" from the Rayleigh-Riabouchinsky phenomenon discussed above. Lange (2009) has argued that dimensional analysis provides explanations of derived laws which screen off the fundamental laws. Dimensional analysis explains certain similarity features of systems that are independent of various aspects of their constitution (and so the sometimes distinct sets of fundamental laws that govern the relevant class of phenomena in question).[59]

I want to emphasize something about how Lange's account of dimensional explanations applies to the generalized Rayleigh-Riabouchinsky paradox. Dimensional explanations using dimensional systems with more basic quantity dimensions (particularly ones considered derivative in conventional systems) apply to a larger set of counterfactual cases; they apply to systems independently of the values and the dimensions of the constants that link the additional basic dimensions into the laws. Insofar as the values and/or the ("absolute")[60] dimensions of the constants are necessary components of the laws, the dimensional structure is more coarse-grained than nomological structure, i.e. dimensional possibility constrains nomological possibility, but perhaps not vice-versa.[61] Bridgman considers a case[62] where volume is treated as an additional basic mechanical dimension independent of length, which allows for the derived equation to apply even in non-Euclidean geometries where $v = l^3$ may not hold, where v and l are, respectively, units of volume and length.[63] This would introduce a dimensional volume constant ω, where $v = \omega l^3$ and $[\omega] = \mathrm{VL}^{-3}$, which could have a non-trivial value (i.e. not 1). In the thermodynamic case, it could be that the value of Boltzmann's constant or the gas constant was different, such that a unit of temperature would not be equivalent, in neither value nor in dimension, to a unit of energy, invalidating any mechanical reduction of thermodynamics. In both cases, the derivation that allows for the possibility of the variations in constants, i.e. does not treat the relevant laws as *a priori*, is the more explanatorily powerful in the sense

[59]Lange considers the dimensional similarities of waves in a fluid and standing waves in a string (Lange 2009, §4.)

[60]I mean here absolute in the sense of the fundamentalist. According to the non-fundamentalist no constants have invariant or absolute dimensions, i.e. none are fundamentally, objectively basic.

[61]Lange (2009) holds that such changes in the values or dimensions of the constants are counterlegal—physicists also often talk this way. It is to be noted that this depends on the somewhat controversial though underappreciated thesis that the values of the constants are part of the laws, e.g. nomologically necessary. See Jacobs (2022) and Jalloh (Forthcoming) for reasons why this may not be the case. Nothing here rests one way or another on this thesis.

[62]See the discussion beginning at Bridgman (1931), 59.

[63]This fails to hold in a very mundane case: A liter of volume was defined (by the CGPM in 1901, until 1964) as the volume occupied by a kilogram of pure water in standard conditions, rather than as a cubic decimeter, as it is currently. While the two definitions aim to define the same quantity, the correspondence is not exact, meaning the former definition requires a constant to relate the volume and length unit, and the conceptual independence of volume from length in this this defintiion requires that this constant be *dimensional*. See Petley (1983), 137.

that it is more general.[64] Pexton (2014) gives a different, though consistent account of how dimensional analysis explains: dimensional analysis provides *models* of systems that make apparent patterns of *modal* dependence (i.e. counterfactual dependence) on the quantities. On Pexton's modal-model theory of dimensional explanations, Rayleigh-Riabouchinsky phenomena can be accounted for by the fact that for some systems such dimensional reductions, like that of temperature to energy, are simply *irrelevant*. It is no surprise that irrelevant factors can introduce noise (in this case in the form of extra degrees of freedom) that interfere with the power of an explanation given by the model. As seen with Lange's account, there is a tradeoff between abstraction and explanatory power.

The conventionalist makes both the general explanatory power of dimensional systems—and also the gradations of explanatory power between them—mysterious. Surely if some choice of convention is better than another, not as a matter of what is convenient to deal with, but in its *explanatory capacities*, we ought question whether dimensional systems are indeed a matter of convention after all. The dimensional realist has a nicer story to tell about the explanatory power of dimensional analysis: dimensions exist and some dimensional systems better describe (some aspects of) their nature than others.[65] However, the conventionalist critique still has some bite. Generally, the Rayleigh-Riabouchinsky paradox only shows that the *number* of basic quantity dimensions, the degrees of freedom in the dimensional system, is constrained by nature. Practice gives reason to believe that the basis of a dimensional system is not unique. This conventionalist constraint on our metaphysics of quantity dimensions can be seen by considering the symmetric nature of defining equations: the relation between volume and length is equally well expressed by the formulae $\mathrm{V} = \mathrm{L}^3$ and $\mathrm{L} = \mathrm{V}^{\frac{1}{3}}$. What is needed is a metaphysics of dimensions that captures the objective *structure* of dimensional systems while leaving open for convention a choice of basis. Further, this

[64] The explanation is powerful because it applies to more possible (or impossible) worlds; the derivation has greater modal robustness.

[65] An extended argument for dimensional realism from dimensional explanations has been provided by Jacobs (Unpublished Manuscript).

structure needs to be such that it provides a foundation for the representational and explanatory success of dimensional analysis.

Both Fundamentalism and Conventionalism have been found to be inadequate metaphysics to account for the precise mix of empirical success and conventionalism fround in dimensional analysis. The option that is left is a Non-Fundamentalist Dimensional Realism:

> (Non-Fundamentalist Dimensional Realism) There is an objectively correct set of dimensional systems—each system describing dimensional structure equally well. While there is no unique basis for these dimensional systems, the number of quantity dimensions that are basic is objectively determined.

One may extrapolate from this a form of structural or "sophisticated" realism, wherein quantity dimensions are without fundamental intrinsic natures or quiddities, but rather have their natures as a matter of their relative positions in the (quotient) dimensional system.[66] I prefer a *functionalist* reading of such a structuralism, wherein quantity dimensions are determined by and up to their nomic roles.[67] However, the view as stated and argued for here need not include such further metaphysical baggage. The view can be put precisely by consideration of a real definition operator, :=, such that the fundamentalist believes that $\mathrm{V} := \mathrm{L}^3$ but not vice versa, $\mathrm{L} :\neq \mathrm{V}^{\frac{1}{3}}$. The conventionalist notes that $\mathrm{L} = \mathrm{V}^{\frac{1}{3}}$ is perfectly fine and so rejects the real definition operator as a constraint on dimensional systems, holding that any definition holds by convention. The non-fundamentalist holds that there is a constrained set of definitions which

[66] For simplicity's sake I will collapse the set of objectively correct dimensional systems into a single dimensional system. In group theory this operation is called "quotienting" and the resultant quotient dimensional system can be understood as that invariant under all of the transformations between different dimensional bases. A similar *structuralist* defense of dimensional realism can be found in Jacobs (Unpublished Manuscript), though he does not take on the historical orientation I have here.

[67] This is functionalism by analogy to the sense of "functionalism" in the spacetime literature, wherein "spacetime is as spacetime does" (Knox 2013, 2019; Lam and Wüthrich 2018). The relative positions in dimensional structure are the invariant objects *described by* different dimensional formulae given a choice of basis—no quantity dimensions is *reducible* to any particular dimensional formula but only to an ensemble of them related by the (conventionalist) symmetry transformations of the dimensional system. Ultimately, these positions are to be understood as *nomic roles*. See Wang (2016) for a survey of nomic essentialism/structuralism. It is worth noting that dimensional structure seems to be an additional "high-order mathematical feature" which tells in favor of a *nomological* rather than a *causal* structuralist account of physical properties, see Berenstain (2016) on the distinction.

are metaphysically acceptable and so accepts the operator, but it is perfectly symmetric: if $V := L^3$ then $L := V^{\frac{1}{3}}$.[68]

One final issue needs to be clarified: what is the scope of the dimensional structure that the non-fundamentalist realist is committed to? Given the metaphysical and general nature of the view, one might think that dimensional structure refers to the dimensional structure *of the world*. However, the arguments for realism given from the problem of insufficient bases and dimensional explanations are weaker than that—the viability of a dimensional system is only determinable relative to a system or class of systems. There are cases in which reduced bases serve the dimensional analyst (this motivated the conventionalist). All that can be said on the basis of the arguments here is that there is some objective dimensional structure given a physical system (or class of physical systems). It may be the case that there are global arguments that can be given; however, here we must be satisfied with the more narrow claim. I note that some work in the aftermath of this debate consider more seriously the dependency of our dimensional systems on the laws under consideration (see Campbell 1926; Palacios 1964). There are some provisional facts about the relation between nomic and dimensional structure that can presently be said. Dimensional structure is an order of modal structure that is more coarse-grained than that of the nomological modal structure of the laws as traditionally conceived. If the laws are considered as strict equalities between quantities, then dimensional structure captures the more coarse-grained proportionality relations between quantities that hold with natural necessity. These dependence relations are central to the nature of the laws, though they undetermine their "strengths"—their relative strengths being captured by the relative values of their characteristic constants.[69] Further, they may be said to constrain the forms of the laws insofar as the

[68]On the viability of symmetric dependency relations see Barnes (2018).
[69]This last point was made clear to me by Bryan Roberts. See also Dahan (2020) on the idea of constants characterizing the laws.

dimensional system appropriate to a system indicates the existence, or not, of a dimensional constant relating quantities in the laws.[70]

2.4 Conclusion

This paper has exposited an unduly neglected debate regarding the methodological and metaphysical foundations of dimensional analysis and has evaluated the merits of the two major positions, conventionalism and fundamentalism. Both positions are found lacking: conventionalism regarding quantity dimensions fails to account for the explanatory success of dimensional analysis and representational constraints on dimensional systems; fundamentalism fails to fit with the conventionality found in scientific practice and fails to give reason to privilege any particular basis over others for a dimensional system. I've set forth the basic outline of a non-fundamentalist but realist account of quantity dimensions, wherein the empirical constraints on the number of basic quantity dimensions and the conventionality regarding *which* quantity dimensions are treated as basic are both respected. The metaphysical residue that the non-fundamentalist is realist about are the symmetric, nomologically necessary dependency relations between quantity dimensions, which correspond to the dimensional forms of the laws and so encode *metaphysically robust* proportionality relations. Much work is to be done in specifying various possible metaphysical developments of this non-fundamentalist account, including those along structuralist or functionalist lines.

[70]In this way dimensional structure is more basic than nomic structure, just as metaphysical structure is more basic than nomic structure. Corresponding to this relative fundamentality of modality is a possible world inclusion relation: the set of metaphysically possible worlds contain the set of dimensionally possible worlds, which in turn contain the set of nomic posibilities. Call this the modal "Russian doll" model of fundamentality.

Chapter 3

The Π-Theorem and Quantity Symmetries

3.1 Introduction

There is an old question which has recently gained renewed and generalized attention; most famous is Poincaré's statement of this question regarding space:

> Suppose that in one night all the dimensions of the universe became a thousand times larger. The world will remain *similar* to itself, if we give the word *similitude* the meaning it has in the third book of Euclid. Only, what was formerly a metre long will now measure a kilometre, and what was a millimetre long will become a metre. The bed in which I went to sleep and my body itself will have grown in the same proportion. When I wake in the morning what will be my feeling in face of such an astonishing transformation? Well, I shall not notice anything at all. The most exact measures will be incapable of revealing anything of this tremendous change, since the yard measures I shall use will have varied in exactly the same proportions as the objects I shall attempt to measure. In reality the change only exists for those who argue as if space were absolute. (Poincaré 1914, p. 94)

It is apparent that such considerations generalize to other quantity dimensions beyond the spatial ones. There is an apparent paradox: Everywhere in the laws of physics it appears that solutions depend on the absolute values of quantities. Yet, there is also an intuition behind thought experiments like Poincaré's: if all quantities of a kind were scaled by the same factor, *including the relevant measurement standards*, that world would be in every way empirically indistinguishable from the actual world. This chapter provides a reconciliation of the absolutist form of the laws and comparativist intuitions about measurement.

A case which has recently captured the attention of some philosophers: would it make a difference if all the masses doubled overnight? The answer turns on a metaphysical debate regarding quantity absolutism and quantity comparativism:

> (Absolutism) Intrinsic quantities are fundamental, qualitative properties, quantity relations supervene on them.[1]
>
> (Comparativism) Quantity relations are at least as fundamental as intrinsic quantities and do not supervene on them.[2]

Intrinsic quantities are determinate properties of particular physical objects and not relations. We think of them as having an *essentially* monadic logical form.[3] An object's property of being two kilograms in mass is intrinsic. Alternatively, the comparativist grounds the object's being two kilograms in mass relationally: the object stands in a relation of being twice as massive as, say, some standard kilogram in Paris. The comparativist holds that these relations are not grounded in intrinsic quantities, but are (relatively) fundamental.

[1]Maybe this is better put in terms of "dependence", "determination", or "grounding" (see discussion in McKenzie 2014, 2020; Sider 2020). I am not here concerned with whatever the proper relation between fundamentals and non-fundamentals is, just that there is some distinction to be drawn which at least implies a supervenience relation describable with possible world semantics.

[2]Here I focus on arguments against the fundamentality of intrinsic quantities. This corresponds to the weak absolutism and weak comparativism described in Martens (2021). Some, including Dasgupta (2016), have presented the argument against absolutism in eliminativist terms. The argument is taken to show that intrinsic quantities comprise surplus structure which ought to be eliminated from our ontology. See Ismael and van Fraassen (2003) and Dasgupta (2016) for accounts of such symmetry arguments. See Martens (2018) for an argument against mass eliminativism. See Sider (2020) and Wolff (2020, chap. 8) for accounts of the absolutist-comparativist dispute in terms of fundamentality.

[3]For an example of a relatively standard metaphysical account of intrinsicality, see Langton and D. Lewis (1998). See Sider (1996) for a distinction between metaphysical and syntactic criteria of intrinsicality and a discussion of the latter type.

Central to the debate is a symmetry argument against absolutism.[4] Such arguments have a general form: some supposed fundamental feature of reality, F, varies under some symmetry transformation, so F is not a fundamental feature of reality. In this case the supposed fundamentals are intrinsic or absolute quantities. The comparativist argues that there is a class of symmetries that leave quantity ratios invariant while varying intrinsic quantities. If this is the case, a supervenience principle follows:

> (Comparativist Supervenience) No change in intrinsic quantity Q of object O without some change in relation R between Q and Q' of some O'.[5]

The relevant symmetries are *physical* symmetries which map physical systems to physically indistinguishable systems. Well known examples of such symmetry arguments include the argument against absolute velocities due to velocity boost symmetries and arguments against absolute space due to translation and rotation symmetries.

Dasgupta (2013) has influentially levied such a symmetry argument against the fundamentality of intrinsic quantities. The argument depends on a notion of mass doubling as a transformation that doubles the mass of every massive object in some physical system and *leaves everything else unchanged.* This *ceteris paribus* condition requires mass doubling be a "full symmetry", meaning a *dynamical* and *empirical* symmetry. Dynamical symmetries map nomically possible systems to nomically possible systems. Empirical symmetries map systems to observationally indistinguishable systems. The argument runs so: Mass doubling is a full symmetry. Intrinsic mass quantities vary under this full symmetry. Properties that vary under full symmetries are not fundamental. Therefore, intrinsic mass quantities are not (relatively) fundamental.

[4]Martens (2021, p. 2523) usefully distinguishes three approaches to the debate regarding the empirical adequacy of comparativism which are present in the literature: (1) the symmetry approach, (2) the detectability approach, and (3) the possibility-checking (i.e. possible worlds) approach. He only discusses (1) in passing and shows that insofar as (2) is useful it is equivalent to (3). Further, I take (1) to be equivalent to (3). I understand possible world semantics to provide a model theory for discussing the symmetries of physical equations. However, as will be made evident, I believe the symmetry approach in is some ways more illuminating and useful for some of the unsettled modality questions (see section 5).

[5]The comparativist likely is committed to more than this, but this minimalist principle excludes absolutist possibilities, e.g. mass doubling. Dasgupta puts this principle somewhat differently. For him the principle justified by these global quantity symmetries is a global supervenience principle. The important thing is whatever set of facts are more fundamental are not explained by the other set of facts (Dasgupta 2013, pp. 108–9). Dasgupta actually makes the case that his "pluralistic" grounding is less demanding than the individualistic condition I am stipulating.

Baker (2020) and Martens (2018, 2021) have shown that this argument is unsound because its first premise is false. Counterexamples, such as a two body system in which a projectile escapes a planet, show that Dasgupta's *ceteris paribus* clause is untenable. Either the mass doubling transformation changes the empirical situation because the projectile fails to escape, or the projectile's trajectory breaks the laws. Mass doubling is not a full symmetry. Further, any other global transformation which acts on a single basic quantity dimension is not a full symmetry.[6] By contrast, I refer to any quantity transformation that leaves both the laws and the observable situation unchanged a *full* quantity symmetry. That all basic quantity transformations fail to be full quantity symmetries will be made clear by consideration of the Π-theorem.

The argument against absolutism can be rehabilitated by showing that there is a class of full quantity symmetries that shows that intrinsic quantities are not fundamental. This class of symmetries is characterized by Edgar Buckingham's (1914) Π-theorem, a foundational result of dimensional analysis. This theorem establishes a general form of physical equations which is invariant under both representational unit transformations and ontic quantity symmetries. The general structure of these quantity symmetries has implications for the nomological role of the dimensional constants. I will argue that whether these quantity symmetries are accepted as *dynamical symmetries* depends on whether or not the values of physical constants (e.g. the gravitational constant) are fixed by the laws or are instead contingent. The absolutist's escape route from the amended symmetry argument requires that the values of physical constants are nomically necessary. I argue that this is a costly move, conditional on the truth of an open and plausible physical hypothesis.

3.2 The Quantity Calculus and the Argument Against Absolutism

The argument against absolutism requires the existence of symmetries that are ontic counterparts to a class of broadly accepted representational quantity symmetries, unit transformations. It is necessary that these

[6] I assume throughout that mass, length, and time are the basic quantity dimensions of mechanics. This is a standard convention but is inessential to the argument—any adequate basis will do.

ontic quantity symmetries are both dynamical symmetries and empirical symmetries. The relationship between these two classes of symmetries will be made clear by an explanation of the quantity calculus.[7]

As terminology varies I will establish my vocabulary with a tripartite distinction:

> (Quantity) A property of a physical object that is representable by class of a number-unit pairs, usually by a number multiplied by a unit (e.g. my quantity of height is approximately 1.854 meters or $1.854 \times \mathrm{m}$);
>
> (Quantity Dimension) A collection of quantities which are all representable by the same set of units, i.e. commensurable (e.g. my quantity of height, yours, the length of route 66, an Angstrom);
>
> (Unit) A standard magnitude of quantity in some dimension whose assignment to the numerical representation 1 induces numerical values to all quantities in that dimension (e.g. the standard lengths defined by the meter stick, the foot of Julius Caesar, the distance a beam of light travels in a vacuum in one second).

I have defined these terms circularly—My aim here is not an analysis but a specification of their relations so as to avoid confusion.

The representation of any quantity as a product of a number (synonymously: value, magnitude, measure)[8] and a unit informs us that these quantities of concern exist on a ratio scale. Further we keep track of the units of some derivative quantity by not only performing algebraic operations on the numerical representations of quantities but also on their units, e.g. $\frac{5 \times meters}{2 \times seconds} = 2.5 \times \frac{meters}{seconds}$. We will

[7] See de Boer (1995) for a history of the development of the quantity calculus. See JCGM (2012) for the contemporary metrological standard, which I am broadly in line with.

[8] This may seem odd to some. B. Russell (1903) roughly used magnitude for what I here call quantity, but his usage is still consistent with that of the literature since—at least the physics literature with which I will largely be concerned—the phrases "the magnitude of a quantity", "the value of a quantity", "the measure of a quantity" are all felicitous and equivalent. They all refer to its numerical extent relative to some defined unit. See Berberan-Santos and Pogliani (1999) for a useful discussion and formalism. One major caveat is that "magnitude" is sometimes contrasted with "value" or "measure" as referring to the objective, unit-independent extent or size of a quantity, as is done in the definition of "unit" above (for a metaphysics agnostic definition of unit-independent magnitudes see Tal 2021). Context will make it clear when the "magnitude" or "value" of a quantity is meant in a unit-relative or unit-free way.

see that the algebra of units obeys a necessary condition on the wellformedness of physical equations—dimensional homogeneity. This necessary condition has to do with the dimensions which each unit instantiates, e.g. the dimensions of force: $[N] = [dyn] = \mathrm{MLT}^{-2}$. Complex, derivative dimensions are constructed from products of powers of basic dimensions, usually M, mass, L, length, and T, duration.[9] Any quantity has a dimensionality or dimension, $[Q] = \mathrm{D}$, which can be multiplied and divided arbitrarily, e.g. $[Q_1 \times Q_2] = [Q_1] \times [Q_2] = \mathrm{D}_1 \times \mathrm{D}_2$.[10] Consider $[F] = [m] \times [a] = \mathrm{MLT}^{-2}$. However, only quantities of like dimensions can be summed or subtracted. In other words, if $k_1 Q_1 + k_2 Q_2 = Q_3$ is coherent, then $[Q_1] = [Q_2] = [Q_3]$. That the terms of a physical equation must have equal dimension is *dimensional homogeneity*.[11] Intuitively, it makes no sense to add a length to a force, etc. The dimensionality of a dimensionless quantity (i.e. a number) is $[1]$, which is the identity—for a quantity Q of arbitrary dimension $[Q] \times [1] = [Q]$.[12] The product of a quantity of some dimension and another of inverse dimension is dimensionless: $[Q] \times [Q]^{-1} = [1]$.

Equations are mere representations of relations between quantities, which are themselves "worldly".[13] Relations between quantities are physical systems, and equations are their representative counterparts, mathematical models. Quantities are either represented by variables associated with dimensions or numbers associated with units. Whether or not the units or dimensions of the quantities in some equation are literally represented, the structure of a dimensional system (or, derivatively, that of a unit system) determines the possible forms of any quantitative equation. As quantitative equations represent physical systems, the structure of a dimensional system determines what relations among the quantities themselves are possible: the algebra of dimensions mirrors the algebra of quantities. If such a dimensional system is

[9]Outside of mechanics, additional dimensions for electrical charge, C and for temperature, Θ, are introduced. I will only deal with mechanics here for simplicity.

[10]The square brackets denote the dimensionality extraction function. Basic dimensions will be denoted by un-italicized letters, like L for length. Products of powers of these basic dimensions are the values of the $[X]$ function. Tolman (1917) and Dewar (Forthcoming) give more sophisticated accounts of the operations of the quantity calculus.

[11]We owe this formulation of the principle to Fourier (1878), see De Clark (2017).

[12]Quantity dimensions form an Abelian group. The formal properties of dimensions deserves a much more thorough discussion. See Raposo (2018); Raposo (2019) for some details and a fiber bundle model.

[13]My usage here is at odds with Martens (2021), for whom "quantity" refers to the representation and not the physical property.

at hand, any definable system of units on those quantity dimensions is *coherent*. That we are dealing with such dimensional or unit systems is a foundational assumption of dimensional analysis and the source of its utility. As Sterrett has it:

> Thus, if it is known that the system of units is coherent, it follows that the numerical relation has the same form as the fundamental [dimensional] relation. *The form of the numerical equation can be known independently* of actually using units and numerical expressions to express the quantities and then deriving the numerical equation from the quantity equation—so long as the requirement that the system of units is coherent is met. (Sterrett 2009, p. 806)

This is what generates the representational-ontic symmetry duality described below, which is essential to my argument (see 4.1).

The numerical representations of quantities are determined by the system of units used. We understand a unit system as a collection of maps from physical quantities to numerical representations. Each particular unit system partitions the physical quantities into equivalence classes independent of the particular homomorphism it adopts, e.g. mass-in-grams vs mass-in-kilograms. These unit systems are related by two kinds of isomorphisms—those that act on the quantities themselves and those that act on the unit system mappings. Distinguishing the quantities from their representatives, we can define two classes of symmetry transformations:

> (Representational Symmetries) Transformations on the assignment of numerical representatives to quantities that leaves the quantities and their ratios unchanged, e.g. unit system transformations.
>
> (Ontic Symmetries) Transformations on the quantities themselves which change the numerical representatives of quantities for any given unit system, leaving their ratios unchanged, e.g. universal velocity boosts.[14]

[14]The ontic-representational distinction corresponds to the active-passive distinction that some may be familiar with.

Representational symmetries are transformations of mere representation. Ontic symmetries are transformations of physical systems.

The comparativist holds that quantity ratios are more fundamental than intrinsic mass quantities, owing to their invariance under *ontic* scale transformations. The (naive) comparativist argues that scale transformations of basic quantity dimensions, like mass doubling, are full symmetries:

> (Comparativist Commitment) Basic quantity (ontic) scale transformations are full symmetries.[15]

The absolutist rejoinder shows that basic mass doubling cannot meet both criteria required of a full symmetry, so it is important to distinguish the two conditions:

> (Empirical Symmetry) An empirical symmetry is a map from one physical system to another that leaves unchanged all observable phenomena, i.e. it takes a system and generates an observationally indistinguishable system.
>
> (Dynamical Symmetry) A dynamical symmetry is a map from one lawful physical system to another lawful physical system, i.e. a transformation that leaves the laws invariant.[16]

Again, a full symmetry, one that justifies the variance-to-unreality inference used by the comparativist, *must* be both dynamical and empirical.

An explicit version of the argument against absolutism can be stated:

1. If quantity Q is variant under some full symmetry then Q is not fundamental.

[15]The range of anti-absolutist views includes more than just comparativism. To accept that these scale transformations are full symmetries only requires the denial of intrinsic mass quantity *quiddities*. The denial of quiddities can be accommodated by multiple views. Most weakly it implies a sophisticated substantivalism (Wolff 2020). More strongly there would be no quiddities if there were no intrinsic quantities at all—or at least no objective facts about them, as in a relationalist view (Dasgupta 2020). There are also a variety of comparativisms on offer, as developed by (Martens 2017, 2018, 2020).

[16]In this context, a criterion of a dynamical symmetry is that that the application of transformation to a system *commutes* with the lawful time evolution of the system. See Ismael and van Fraassen (2003), Roberts (2008), and Wigner (1979) for discussions of the relation between these two classes of symmetries.

2. Mass doubling is an empirical symmetry: If all of the mass quantities were doubled there would be no observable difference.[17]

3. Mass doubling is a dynamical symmetry.

4. Mass quantities are variant under a full symmetry. (2, 3)

5. Mass quantities are not fundamental. (1, 4)

Premise (1) of this argument is a form of the more general variance-to-nonfundamentality (or unreality, or non-objectivity) principle commonly accepted by physicists and philosophers alike. Premise (3) is a posit that there is a comparativist paraphrase of the laws that is genuinely Newtonian but is indifferent to mass doublings. The problem raised against this argument is the inconsistency of (2) and (3). Generally counterexamples to this argument are taken to target the empirical symmetry premise, (2), but my presentation below will focus on how the counterexamples can be used to bring more focus to (3). By showing that the problem with the argument against absolutism is the misclassification of a basic quantity transformation as a full symmetry, I provide an argument against absolutism that is immune to counterexample.

3.3 Baker's Counter Example

Baker (2020) presents a counter example to comparativism, showing that mass doubling is not a full symmetry.[18] Consider a two body system: a projectile traveling with velocity v_{pro} away from a planet's surface. From Newton's laws we can derive an equation for the critical escape velocity such that if $v_{pro} > v_{escape}$, the projectile will escape the orbit of the planet:

[17] This argument is meant to directly parallel arguments against the existence of absolute velocity, see Dasgupta (2013); Dasgupta (2016). Crucially this argument depends on absolute mass, and some class of physical quantities more generally, not being observable. Roberts (2008) and Dasgupta (2016) cash this out in terms of the impossibility of constructing absolute quantity detectors. Both parties to the debate tend to accept that absolute quantities like mass are not directly observable. For criticism of the detectability interpretation see Martens (2021, pp. 2540–44). In light of this we might drop the observable adjective and say that empirical symmetries leave all the qualitative facts unchanged (see J. S. Russell 2014).

[18] Martens (2018); Martens (2021) has shown this to be only one instance of a broader class of counterexamples in which two or more particles either collide or escape each other, depending on their absolute masses. These are all equivalent for my purposes.

(Escape Velocity) $v_{escape} = \sqrt{\frac{2GM}{R}}$,

where G is the gravitational constant, M is the mass of the planet, and R is its radius. Note that the equation for the escape velocity depends only on the mass of the planet and not the projectile mass. On Earth, the M and v_{pro} are such that the projectile escapes. In the mass doubled counterpart system, the planet Pandora's mass is such that the projectile does not escape. The sticking point is that the comparativist sees initial states of the Earth and Pandora systems as *empirically equivalent*. So the comparativist cannot hold that the initial state of the two body system has a unique future as determined by the laws.

Baker presents this counterexample to comparativism as showing that comparativism introduces indeterminism into deterministic systems. A different presentation will better serve our purposes: the counterexample generates an inconsistent triad. The three inconsistent propositions are:

(a) The initial states of the Earth and Pandora systems are indistinguishable;

(b) The final states of the Earth and Pandora systems are indistinguishable;

(c) The dynamics are left invariant by the transformation that maps the Earth system to the Pandora system.

The first two propositions follow from mass doubling being an empirical symmetry, the third from mass doubling being a dynamical symmetry. If mass doubling is an empirical symmetry, then it cannnot be a dynamical symmetry: the trajectory required is inconsistent with the escape velocity equation. If mass doubling is a dynamical symmetry it cannot be an empirical one: either the initial state or final state of the system must be changed for the position of the projectile to match the Earth case at the opposite temporal state (final or initial) while having a trajectory consistent with the laws. Generally the issue has been characterized as one of empirical adequacy or indistinguishability, this presentation highlights the sometimes implicit assumption that the relevant empirical symmetries are a subset of the set of dynamical symmetries.[19] Mass doubling cannot be both an empirical and a dynamical symmetry, so it is not a full symmetry.

3.4 The Argument Against Absolutism Amended: Lessons from Dimensional Analysis

Dimensional analysis depends on a number of basic (though not totally uncontroversial) assumptions. An account of these assumptions provides a route to a proof of the Π-theorem. The first assumption is that all of the basic quantities that figure in equations which are representationally adequate have a ratio scale structure. This means that all quantities of a basic dimension can be related by a scalar multiplication operation of the form $f : x \mapsto \mathbb{R}x$. Mass, length, and time all share this structure and so are suited to form the dimensional basis for mechanics. This can be attributed to the fundamental idea that these are all extensive quantities, i.e. the magnitude of a whole is an additive function of the magnitudes of its parts.[20] By treating these quantities as *basic* we are treating them as building blocks from which all other quantities are defined.

In a "complete" system of units (or dimensions) the derived quantities inherit some properties from the basic quantities which define them, i.e. they too exist on a ratio scale. The ratio scale structure of the mechanical quantities defines a group of unit transformations:

> (Unit Transformation) For any quantity $Q = V \times U$, there is a class of maps $Q \mapsto Q'$, $U \mapsto U'$, $V \mapsto V'$, such that $U' = xU$, $V' = x^{-1}V$, $Q' = Q$, where $x \in \mathbb{R}^+$.

[19] Martens makes this explicit. The dynamical condition is pronounced even in the guise of the possibility checking approach: "Comparativism should provide at least one metaphysically distinct (and dynamically allowed) possible world for each empirically distinct possible world allowed by absolutism. If the metaphysically distinct worlds that comparativism acknowledges fail to differentiate between those distinct empirical possibilities, then comparativism is wrong. If, on the other hand, the set of all the metaphysically distinct possible worlds acknowledged and *dynamically allowed by comparativism* contains all the empirically distinct possible worlds (that are *dynamically allowed by absolutism*), then we may opt for comparativism over absolutism based on an Occamist norm." (Martens 2021, 2524, my emphasis)

[20] I here ignore any distinction between additivity and "proper" extensivity, cf. Perry (2015).

An example: the representational Q-transformation $10 \times$ kilogram $\iff 10,000 \times$ gram. It involves a U-transformation, $1 \times$ kilogram $\iff 1000 \times$ gram, and a V-transformation, $10 \iff \frac{1}{1000} \times 10,000$. While there are an indefinite number of representations of a quantity in some dimension owing to the indefinite number of reference units, all of the representations are equivalent under the group action.

Insofar as we take quantities and equations of quantities to be describing physical phenomena and not our measurement standards we require all "objective" quantity equations to be invariant under unit transformations. Given that these unit transformations are multiplicative in nature it is intuitive that quantities defined by products and divisions (and iterations thereof, i.e. powers) of the basic quantities will also be so invariant. As for equations, it is also relatively intuitive that equations only involving terms of like dimension will be invariant under unit transformations. The equation $Z = X + Y$ remains true under transformations of the form $Z \mapsto cZ$, $X \mapsto aX$, $Y \mapsto bY$, in cases in which $a = b = c$, i.e. $Z = X + Y \iff aZ = aX + aY$. These are cases in which all three quantities share the same dimension and so the same unit transformation factor. If, say, Y was of different dimension such that for some unit transformation $a = c \neq b$, then $aZ = aX + bY$ would not remain true in any case in which $aY' \neq bY$, where $Y' = Z - X$ as defined in the original units. This violation of dimensional homogeneity does not guarantee variance under *all* unit transformations, there may be some unit transformations of some equations in which it just happens that $a = b$ even though $[X] \neq [Y]$.

3.4.1 From the Representational to the Ontic

Here I describe the main metaphysical move made by my usage of the Π-theorem. For those interested in proofs of the theorem, see appendices A and B and the references therein. Those uninterested in any technical detail (or those who take the active-passive duality as a matter of course) can skip to 4.2 and 4.3 for the solution of the escape velocity case and a summary of the amended argument against absolutism. In order to provide proofs of the Π-theorem, we must first prove *Bridgman's Lemma*: that all derivative quantities take the form of product of powers of the basic quantities. I do not provide a proof here,[21] but will briefly describe the reasoning and importance of the lemma.

The task of the lemma is to show that the derived quantities in a coherent systems of units have an essential form. It is established that dimensionally homogeneous equations are unit independent (see above). We take as a constraint on the form of a derived quantity that it is a function of basic quantities. Further, the *defining* equation is itself unit invariant, e.g. $F = kma$. That these defining equations only take the form of products of powers of the basic quantities (with a numerical scale factor, *k*) is Bridgman's Lemma. Bridgman's proof of the lemma is presented as an analytic elaboration of "our" requirement that relative magnitudes have absolute significance—independent of numerical representation, i.e. units. For Bridgman (1931, p. 21) this naturally follows from an operationalist point of view: the measurement of relative magnitudes is first and foremost a comparison of bodies which could not be affected by a change in our operational standards. Setting aside operationalism, we take it as an assumption that there is some unit transformation invariant relation underlying the comparative measurement of basic quantities.[22] From this it can be proved that all derived quantities must be defined as powers of products of the basic quantity units: $Q_{mechanics} = km^x l^y t^z$. This defines a complete dimensional system: $[Q_{mechanics}] = [m^x][l^y][t^z]$. So much for Bridgman's Lemma.

The Π-theorem, in a nutshell, states that any adequate physical equation that describes a system can be put into Ur-Equation form:[23]

(Ur-Equation) $\psi(\Pi_1, \Pi_2, \ldots, \Pi_n) = 0$,

[21]Readers can consult Bridgman (1931) and Berberan-Santos and Pogliani (1999) for proofs of the lemma.

[22]Perhaps we do so on the basis of the necessity of objective communication, see Roberts (2008). Even the absolutist will accept the invariance of quantity ratios of like dimension under unit transformations.

[23]This is my terminology. Sterrett (2017) calls this "The Reduced Relation Equation of 1914". Others sometimes refer to this equation as the Π-theorem itself, but I think it is more proper to consider the theorem the claim that any complete physical equation can be put in this form.

where the Π-terms are dimensionless derived quantities—products of powers of basic quantities—adequate to describe the system and ψ is some arbitrary function. The Π-theorem gets its name from the form of the functions that define the Π-terms: $\Pi = k\prod_i^n Q_i^{a_i}$, as established by Bridgman's Lemma.

I will distinguish two version of the theorem not often distinguished. Usually authors have one interpretation or another of the result,[24] but all agree that there is an important sense in which the result is about mathematical structure. The question is the proper location of that structure: is this a result of the algebra of quantities or of the numbers which measure them? For the representational proof, the invariance of the numbers which measure quantities is essential to the resulting Π-theorem. For the ontic proof, it is rather the quantities themselves and their dimensional properties which are essential to the result. My argument here is that these two proofs can indeed be seen as mere differences in "interpretation" such that both readings of the transformations described—unit transformations and ontic scale transformations—are available.[25] Further, I argue that a commitment to the representational theorem and some minimal assumptions regarding measurement entail a commitment to the ontic theorem.[26]

We first reconsider the nature of the unit transformation discussed above. Let us first specify a neutral conception of an equation between the numerical and the quantitative. We take an equation to represent relations between quantities either directly or indirectly, in either case we take the representatives which figure in an equation to have the canonical form of a numerical value multiplied by a unit quantity: $Q =$

[24]For example, Bridgman (1931) takes the formalist approach indicated by the representational interpretation of the theorem, while Buckingham (1914) takes on (somewhat reluctantly) the metaphysical significance of the ontic interpretation, as does Tolman (1915), more enthusiastically. For discussions of the history of the theorem, including priority disputes see Pobedrya and Georgievskii (2006) and Sterrett (2005); Sterrett (2017) and their references. Gibbings (1982); Gibbings (2011) gives a typology of proofs and his own metaphysical account. See also Walter (1990) and Mitchell (2019) on the historical metaphysical dispute regarding dimensions—see Skow (2017) for a contemporary discussion.

[25]Sterrett (2009) has brought it to my attention that Maxwell (2002) also noted this ambiguity in the interpretation of physical equations. My understanding of these equations as *quantity equations* is in line with Sterrett's view and the account of Lodge (1888). Accepting quantity equations means accepting the application of mathematical operations to quantities. This avoids the awkward work around of Maxwell who avoids the supposed inapplicability of algebra to physical quantities by converting between numbers which can be so manipulated and proper quantities via the introduction and elimination of units—Bridgman (1931) takes this implicit constraint to be the total significance of dimensions. For more on the "double interpretation of physical equations" see de Courtenay (2015) and Mitchell (2019).

[26]That, therefore, the denial of the ontic symmetry transformations would force the absolutist to reject the representational symmetry transformations (unit transformations) is argued by Wolff (2020, pp. 149–150). This is a difficult and involved argument so I will not press the point here. Further, I discuss a different loophole that the absolutist may take in section 5.

$V \times U$. If we take the representation to be direct, then we take the dimension associated with the unit to be constitutive of Q such that the principle of dimensional homogeneity is a metaphysical instantiation of Leibniz' Law.[27] Alternatively, we take the dimension of the unit to be inessential to the representation[28] and merely a bookkeeping device which reminds us of the conventionally decided *rules* which correspond to the principle of dimensional homogeneity. Under either interpretation of units, they are taken to represent a member of a group of homomorphic maps from quantities to numbers which represent the magnitude of the quantity, here represented by $V \in \mathbb{R}$.[29] That the units of some dimension form a group is simply another way of saying that unit transformations are symmetries of the form $U_{trans} : V \mapsto V'$. For the representationalist or conventionalist, this is a direct numerical transformation and the new units associated with V' merely indicate a different standard for measuring Q.

For the metaphysician there is another set of symmetries that share the form $V \mapsto V'$ with U_{trans}. These symmetries are transformations of the quantity itself $Q \mapsto Q'$, defined as automorphisms of the quantity dimension: $Q, Q' \in D$. These ontic transformations can change the appropriate numerical representation of a quantity while leaving the units they are described with invariant. The unit map is preserved under the transformation $U_{map} \circ Q = V \to U_{map} \circ Q' = V'$.[30] This provides another set of transformations under which physical equations may be invariant: quantity dimension transformations.

The Π-theorem provides a bridge from the invariance of physical equations under unit transformations to the invariance of physical systems under quantity transformations. That any physical system can be represented by an equation of dimensionless quantities, is the crux of the amended argument against absolutism. All symmetries of an Ur-Equation representation of a system are dual. On the one hand

[27] Here only the dimension associated with the unit, but not the unit itself is essential to the quantity. Here a unit is just another quantity.

[28] The *unit* is of course essential to the *representation*—a change of units constitutes a change of representation.

[29] Or, if we commit to abstract or maybe only hypothetical quantity magnitudes (which then mediate the application of value-unit representations to concrete quantities), unit systems are *isomorphic* maps (see Tal 2021; Wolff 2020). This makes no difference to my arguments.

[30] Note that the value of the unit quantity $U_{map} \circ Q = 1$ will change $V \neq V'$. This subtlety is (as far as I can tell) largely irrelevant for what follows, but see Wolff (2020, pp. 151–153) for a discussion of quantity dimension translations vs quantity dimension dilations.

we have the representational symmetries accepted by all parties—unit transformations. These change the numerical values associated with constituent dimensional quantities but they leave the dimensionless Π-terms unchanged. This requires us to understand the Π-terms as providing a semantic link between an equation and a system itself: the Π-terms represent the quantity relations of the system that have absolute significance, their values have unit-independent meaning. As shown above, there is also a class of unit-independent, ontic symmetries which act on the constituent dimensional quantities of Π-terms—these symmetries act on the system's quantities themselves. The class of ontic physical symmetries which leave the Π-terms invariant are the class of *non-trivial empirical* symmetries. For this reason the Ur-Equation provides a well-tuned representation of physical systems—its formalism is coordinated to the physical structure of systems without excess representation. A change in the value of a Π-term necessarily represents a change in the physical system, while a change in the value of a constituent dimensional quantity may be an artifact of a purely representational change, like a change of units systems.[31]

3.4.2 Symmetries Defined by the Π-theorem: The Escape Velocity Case

Recall that one historical aim of this theorem was ultimately to provide a standard for scale models in aeronautics.[32] This theorem provides a condition that must be met for one physical system to serve as a model of another, i.e. the theorem defines empirical symmetries for physical systems.[33] We can describe two systems S and S':

$$S : \psi(\Pi_1, \Pi_2, \ldots, \Pi_i) = 0$$

[31]Sterett's analogy between Buckingham's theorem and Wittgenstein's *Tractatus* has greatly clarified my thinking on this point. We may consider the dimensional quantities as atomic objects and the dimensionless Π-terms as propositions about the relations of these objects (atomic facts). As it were, the world consists of facts and not things; the Π -terms are accordingly isomorphic to the physical facts while the dimensional quantities fail to represent in isolation. ψ represents higher-order propositions which are decomposable into relations, here dynamical rather than logical, between the basic propositions, Π-terms. *The equation itself serves as a model of the system.* See especially the diagrams on pages 225 and 227 of Sterrett (2005).

[32]See Sterrett (2005) for more on the historical development of the formal results of dimensional analysis.

[33]It should be noted that the notion of "physically similar systems" that the Π-theorem allows us to formalize is more fine-grained and sophisticated than the standard of empirical symmetry I am considering here. Besides dynamical similarity, there is e.g. geometrical similarity and kinematic similarity. Philosophers concerned with symmetries would do well to consider physical similarity, see Sterrett (2009); Sterrett (2017).

S': $\psi'(\Pi'_1, \Pi'_2, \ldots, \Pi'_i) = 0$

S and S' are empirically indistinguishable if and only if the values of the dimensionless Π-terms are invariant, i.e. $\Pi_i = \Pi'_i$, under some transformation of the basic dimensional quantities which compose the Π-terms.

In our escaping projectile case $\Pi = \sqrt{\dfrac{2GM}{rv_{pro}^2}}$ and ψ is a function that yields the Ur-Equation form $\Pi - 1 + \epsilon = 0$.[34] The ratio between the projectile's escape velocity and its actual velocity is conserved across symmetry transformations that leave Π invariant at approximately 1: $\dfrac{v_{ecscape}}{v_{pro}} + \epsilon = 1$. If two systems are to be dynamically similar and share the same ψ, it must be the case that the numerical values of the Π-terms are equivalent between the two systems. ψ is a high level description of the dynamics of the system: it constrains solutions to the equations of motion to those in which the projectile *just* has the velocity necessary to escape the planet's orbit.

Buckingham's argument is that changes in the basic quantity *dimensions* will leave the Π-terms unchanged in the transformation $f : S \mapsto S'$, because they are *dimensionless*. If all of the operands and the values of ψ and ψ' are identical, then the functions must be the same.[35] This identity signifies a symmetry. In the cases we are concerned with ψ and ψ' stand in for the *dynamical laws*. This makes good on an assumption made by comparativism, that the relevant empirical symmetries of a system are a subset of its dynamical symmetries and are hence full symmetries. A formerly problematic principle is justified: measurable quantities must be invariant under dynamical symmetries.[36] This licenses the inference from the existence of a class of empirical quantity symmetries to the existence of a class of *full* quantity symmetries.

[34] ϵ is simply a small constant added so we can deal with equalities rather than inequalities since strictly speaking the escape situation requires $\Pi < 1$.

[35] As stated this is an invalid inference. Consider the operations of addition and multiplication which have the same value, 4, with the same operands 2 and 2. The argument relies on the operands being more fine-grained than the values of Π. If we distinguish different instances of the Π-terms by the values of their constituent basic quantities, then we can claim that ψ is identical to ψ' iff for every instance of a set of Π-terms related by basic quantity symmetries (i.e. of the same value) they yield the same value. The dynamical laws are shielded from non-empirical differences in quantity values.

[36] This measurability-invariance-principle is the puzzle that is taken up by Roberts (2008). Roberts denies that the principle is analytic and I agree. The synthetic principles at work here are dimensional homogeneity and Bridgman's Lemma. I think it is plausible that these are equivalent or closely related to the publicity principle Roberts proposes. Note that—with Roberts—I take this to also provide an explanation of Earman's (1989) prescription that geometrical symmetries should not exceed dynamical ones in a "well-tuned" theory.

We can generate a full quantity symmetry by: (i) arbitrarily transforming any of the basic quantity dimensions; (ii) adjusting the derivative quantities according to their dimensional composition, in line with Bridgman's Lemma, which will keep the values of the Π-terms are invariant.

Now we can return to the escape velocity case and show that a *full* mass doubling symmetry, which involves more than doubling the masses of *objects*, does not generate indeterminism or violate the laws. Consider again a situation in which the projectile escapes, $v_{pro} = \sqrt{\frac{2GM}{r}}$.[37] In step (i) of the transformation, as mass is a basic quantity dimension, we apply an arbitrary ratio transformation: $m_i \rightarrow 2m_i$ for i massive objects. We can describe the transformed situation thus: $v_{escape} = \sqrt{\frac{4GM}{r}}$, so v_{pro} is now insufficient to escape, *but we do not stop here.*

In step (ii) we change one of the derived quantities in order to preserve the relevant Π-term. The Π-term is $\Pi = \sqrt{\frac{2GM}{rv_{pro}^2}}$, or $\Pi = \frac{v_{escape}}{v_{pro}}$, and the derived quantity to be transformed is the gravitational constant G, whose dimensions $\mathrm{L}^3\mathrm{M}^{-1}\mathrm{T}^{-2}$ define the compensating transformation the value as a halving, according to dimensional homogeneity.

Here's an explicit derivation modeled on Bridgman (1916): Let's define G as the product of a dimensionless number γ and its dimensions $\mathrm{L}^3\mathrm{M}^{-1}\mathrm{T}^{-2}$ (in abstraction from any particular units). If we define mass doubling as operating directly on the dimension, then $\mathrm{M}' = 2\mathrm{M}$. So then the new gravitational constant G' equals $\gamma\mathrm{L}^3\mathrm{M}'^{-1}\mathrm{T}^{-2}$, and by substitution $G' = \frac{1}{2}\gamma\mathrm{L}^3\mathrm{M}^{-1}\mathrm{T}^{-2}$. Therefore $G' = \frac{1}{2}G$. Another way to understand this induced transformation of G is that G *has* (inverse) mass; its dimensionality has negative exponents in the mass dimension, so if all quantities with mass dimensions double, those with inverse mass dimension will halve. Under the completed symmetry transformation,

$$v_{escape} = \sqrt{\frac{4G'M}{r}},$$

[37] From here on I drop the ϵ.

where $G' = \frac{1}{2}G$. The original empirical situation in which the projectile escapes is preserved:

$$v_{escape} = v_{pro} = \sqrt{\frac{2GM}{r}},$$

and

$$\Pi = \frac{v_{escape}}{v_{pro}} \approx 1.$$

That the Π-terms are invariant under some transformation of quantity dimensions is what I dub *Buckingham's Criterion* for a full quantity symmetry:

> (Buckingham's Criterion) Only those quantity transformations which preserve the values of Π-terms that represent a physical system are full symmetries of that system.

If the absolutist is committed to the principle that physical equations are unit invariant (representational symmetries) and some fundamental principles of dimensional analysis, they are committed to the Π-theorem. This in turn commits them to ontic quantity symmetries, which, if they accept the validity of variance to unreality (or non-fundamentality) inferences, provides a decisive argument against their absolutism.[38]

Put somewhat more simply: a quantity transformation is a full symmetry if and only if it leaves the ratios of all quantities sharing some dimension invariant according to their exponent in that dimension.

3.4.3 Executive Summary of the Amended Argument Against Absolutism

I here present an amended symmetry argument against quantity absolutism. First consider the original, failed argument:

[38]Wolff (2020) comes to a similar conclusion, though by way of measurement theory rather than dimensional analysis. Roberts (2016) also responds to the counterexample to comparativism much the same as I do, but works on the basis of a less general principle than the Π-theorem. See also Dewar (Forthcoming)—it is not clear to me whether or not his group-theoretic sophisticated absolutism is equivalent to group theoretical presentations of the Π-theorem and the results of dimensional analysis, compare Corrsin (1951); Boyling (1979); Curtis, Logan, and Parker (1982); Raposo (2018); Raposo (2019).

(1) For any supposed fundamental property F, if F varies under a full symmetry, then F is not fundamental. (variance-to-unreality inference)

(2) Mass doubling is a full symmetry. (naive comparativist commitment)

(3) Intrinsic mass quantities vary under mass doubling. (definition of mass doubling) Therefore, intrinsic mass quantities are not fundamental. (1, 2, 3)

Premise (2) was falsified. A full symmetry is a transformation that is both a dynamical and an empirical symmetry. It was established that mass doubling cannot be both. I amend the argument by substituting (2) and (3) with (2*) and (3*):

(2*) There is a class of *full* quantity symmetries defined by the Π-theorem, one of which, full mass doubling, doubles the masses and halves the gravitational constant. (Buckingham's Criterion)

(3*) Intrinsic mass quantities vary under full mass doubling. (definition of full mass doubling)

Note the generality of the result: any quantity which is not dimensionless, is not fundamental.[39]

The argument for the pivotal amended premise (2*) is the establishment of Buckingham's Criterion for a general quantity symmetry. The first part of the establishment of Buckingham's Criterion is to provide a general form for physical equations, the Ur-Equation. Its generality is justified by the assumption of dimensional homogeneity and the completeness, or unit invariance, of the physical equations in question. These are undeniable, at least for the equations we call physical laws.[40] Bridgman's Lemma tells us the form of the quantities, Π-functions, that figure in the Ur-Equation. These are measurable quantities, dimensionless products of powers of basic quantities. With this all in place, the Π-theorem can be be proved.

[39]This avoids the "pushing-the-bump-under-the-carpet" objection that can be made against other comparativisms, see Martens (2020, p. 15).

[40]But see Grozier (2020) for some of the issues regarding "unit-invariance".

As the Ur-Equations which represent systems embed dynamical equations, i.e. equations of motion, quantity transformations which leave their ψ-functions invariant are by definition dynamical symmetries.[41] As only Π-terms figure in these equations, such a dynamical symmetry must leave their values invariant as well. All absolutely significant quantities are Π-terms, therefore any transformation that leaves the Π-terms invariant is an empirical symmetry. So we have an intersection of the dynamical and empirical quantity symmetries of a system. These are symmetries in which individual quantities may be transformed according to their ratio structure, and constraints defined by the preservation of the systems dynamics according to general law will induce transformations on other quantities such that the empirical situation, specified by Π-terms, remains invariant.

It is important to clarify the results of this argument against absolutism. This symmetry argument, impervious to the sorts of counterexamples raised to Dasgupta's symmetry argument against absolutism, has a more modest aim than its predecessor. It shows that absolute quantities are not *more fundamental* than quantity relations. However, if one adopts a strong, Occamist variance-to-unreality principle, one may use the Buckingham class of symmetries to argue for the *non-existence* of absolute quantities and the absolute fundamentality of quantity relations. For Dasgupta, these relations are between actual bodies, but one may instead adopt a quasi-Platonist structuralist view in which the fundamental quantity relations are between unworldly quantity magnitudes or second order properties which physical quantity relations somehow participate in (a *gross* gloss of Wolff 2020). As I see it, there is no reason not to extend the argument in any of these directions: weak comparativism (Martens), relationalist comparativism (Dasgupta), and structuralism (Wolff) are all consistent with my argument as it stands.[42] As it stands my argument is

[41]For example, similitude methods exploiting the results of the Π-theorem are widely used in fluid mechanics, where analytical methods are intractable. Examples of such derivations of dynamical equations can be found in textbooks like Gibbings (2011).

[42]One may worry that my appeal to a constant (G) is in tension with Dasgupta-style relationalist comparativism in which the fundamental quantity relations are between *bodies*. This is not the case. The constants can be understood as highly complex relations between bodies or regions of empty space—they are distinctive in that they are, in some sense, the same relation everywhere.

not an argument *for* a comparativist account of quantity, but rather propaedeutic to one. Further commitments are needed to have a complete metaphysics of quantity.

This account of the quantity symmetries takes into account *inter*-quantity relations. Quantity symmetries generally require the transformation of multiple quantities, though they may transform only a single *basic* quantity *dimension*. By ignoring inter-quantity relations, the contemporary debate has been built on a fallacious assumption—a primary target of one of Galileo's two new sciences:

> Only by a miracle could nature form a horse the size of twenty horses, or a giant ten times the height of a man—unless she greatly altered the proportions of the members, especially those of the skeleton, thickening the bones far beyond their ordinary symmetry.
>
> Similarly, to believe that in artificial machines the large and small are equally practicable and durable is a manifest error. Thus, for example, small spire, little columns, and other solid shapes can be safely extended or heightened without risk of breaking them, whereas very large ones will go to pieces at any adverse accident, or for no more cause than that of their own weight. (Galilei 1638–1989, p. 14)

3.5 The Nomological Role of Constants

There is one lingering issue. I cannot hope to settle it here, but I'd like to open this vista for surveying. The account of dimensional analysis above gives no special role to the constants of nature, particularly the gravitational constant. The constants are merely parameters of equations and are available to be transformed by quantity symmetries. Indeed, they are only special in the sense that they are most apt to be manipulated in quantity symmetries as they describe the coupling of various logically independent basic quantity dimensions.

Let me clarify what I mean by "constants of nature". Johnson (2018) distinguishes three kinds of quantities called "constants": scale factors, system-dependent parameters, and system-independent parameters. The system-independent parameters are *universal* constants and are my concern here. Scale factors are mere numerical artifacts that can be inserted or removed from equations at will by unit changes.[43] System-dependent parameters on the other hand are quantities that correspond to aspects of particular physical systems. For example, the density of a fluid ρ may be defined as the ratio of its mass and volume $\rho = m/V$. For the treatment of some particular fluid, like an idealized incompressible fluid, this quantity may indeed remain constant, but its value differs for different fluids.

Among system *independent* parameters there are two philosophically relevant subkinds of constants of *nature*.[44] We distinguish: properties of the fundamental particles (e.g. m_p, m_e, e) and properties of the fundamental fields (e.g. c, h, G). My concern here is solely the third class of constants, the interaction constants which describe various sorts of fields.[45] Given that the debate between the comparativist and the absolutist has concerned the possibility of changing the basic quantities, i.e. constants describing fundamental properties of the fundamental particles we can understand the question raised here as: Do transformations of the particle constants induce transformations in the interaction constants?

However, interaction constants seem to play a more significant role in all physical laws: their values seem *constitutive* of the laws. It seems that if the gravitational constant or any other constant of nature

[43]E.g. the 4π factors that appear and disappear in electromagnetism (Maxwell's equations) depending on whether one is working in rationalized or unrationalized unit systems (see Silsbee 1962).

[44]I assimilate Johnson's third category, numerical artifacts, to scale factors. A core example is Avogadro's constant N_A. N.B.: This is contrary to the usual conception of Avogadro's constant as a constant of nature and a mole as a unit of "amount of substance" that can be found in the SI unit system (BIPM 2019). See Johansson (2011) for some common sense dissent.

[45]There is a somewhat similar delineation of the constants given by Lévy-Leblond (1977); Lévy-Leblond (2019). Lévy-Leblond distinguishes class B constants which describe general classes of phenomena (e.g. electromagnetic) and truly universal class C constants. Johnson (2018) finds the distinction unfounded. As Lévy-Leblond holds that the classification of the constants is context dependent, it makes no difference to me here whether or not e.g. c is considered as a mere electromagnetic constant or a universal constant (describing the causal structure of spacetime). All of the constants I am concerned with here may be considered class C constants. I should also note that in a quantum field theory context this distinction breaks down, by my lights, in favor of a constant contingentism.

is changed, then *the laws* have changed.[46] This would mean that there is some discrepancy, though unobservable, between the two escape velocity cases, vindicating the absolutist—full mass doubling would fail to be a *dynamical* symmetry. The question is then whether the values of the constants determine the nomically possible worlds.[47] Broadly, there are two views one can have towards the gravitational constant in particular and interaction constants which appear in the laws in general:

> (Constant Contingentism) The magnitudes of the dimensional constants are independent of the laws and depend on non-nomic quantity regularities—they vary across nomically possible worlds.[48]
>
> (Constant Necessitism) The magnitudes of the dimensional constants are fundamental and necessary across nomically possible worlds. These values constrain non-nomic regularities.[49]

Contingentism naturally pairs with comparativism. For the contingentist comparativist, it is only the general, dimensional relation between the constants and dimensional quantities that constitutes the laws, the actual magnitudes of dimensional constants are irrelevant. Some pioneers of dimensional analysis thought of the constants as properties of the environment, the gravitational constant and the permittivity constant were thought to be properties of empty space.[50] Though one may not need to accept this sort of *ontological* grounding for physical constants: Bridgman (1916) held that the constants are *conventional* conversion factors. One of the most intuitive cases for constant contingentism is the fact that we take constants to be *measured* quantities.[51]

[46] I will not here consider "singularity" limits where the constants are taken to be infinity or 0. We take such cases, e.g. $G \to 0$, to represent the absence of the relevant physics which tells in favor of their necessity to the law. Another case, $c \to \infty$, represents the classical limit of relativity. I believe that these cases are different in nature from a mere doubling, etc. of the magnitudes of the constants. See Lévy-Leblond (2019) for an introduction to some of the relevant issues.

[47] I set aside issues regarding spatiotemporal variations of the constants in a single universe. See Barrow (2004) and Barrow and Webb (2005) for accessible introductions.

[48] This view has been floated in Ehrenfest-Afanassjewa (1916b); Ehrenfest-Afanassjewa (1926) and Nordström (1915) and has recently come under criticism by Martens (2020).

[49] This is to be distinguished from Dahan's (2020) view of the constants as (defeasible) *identifiers* of universal laws—Dahan makes this point herself. Though necessitism is consistent with the idea that constants "baptize" universal laws, it is independent of it.

[50] For example, Mercadier and Vaschy, see De Clark (2017, pp. 312–19).

[51] Recent changes in SI units aside. That the values of constants are there treated as *defined* is to be understood as a conventional fiction. The high-precision measurement of the constants are to be cordoned off to the special science of metrology, while those

Similarly, absolutism naturally pairs with necessitism.[52] It is nomically impossible that the gravitational force be stronger than it is. We might understand the role of the constants in this way: Rather than have the gravitational constant as a parameter in functions that represent gravitational systems, $f(G, x, y, z \ldots)$, the gravitational constant is an essential constituent of that function which represents the dynamics of gravitational systems, $f_G(x, y, z \ldots)$.[53] One natural understanding of necessitist absolutism is that it leads to a total determination of the facts of the world by the laws—a theory of everything would have no terms left to be determined by experiment:

> One plausible view of the Universe, is that there is one and only one way for the constants and laws of Nature to be…The values of the constants of Nature are thus a jigsaw puzzle with only one solution and this solution is completely specified by the one true theory of Nature. If this were true then it would make no more sense to talk about other hypothetical universes in which the constants of Nature take different values than it would make sense to talk of square circles. There simply could not be other worlds. (Barrow 2004, p. 178)[54]

One ought not be misled by Barrow's comparison to square circles; what we are and have been concerned with throughout this work is nomological or natural necessity and not any broader sense of metaphysical or logical necessity. The view expressed here, then, is that constant necessitism leads to necessitarianism or strong determinism: that everything that is true is (nomically) necessarily so. A sketch of the argument for this: Assuming the world is lawful, the combination of the necessity of the laws, including the values of the constants, and the absolute significance of intrinsic quantities, including the constants, would entail

measured values are taken as analytic truths by the rest of the sciences (see Petley 1983). It is merely cognitive division of labor—we do not, in the same practice, design the tools that we use.

[52] Both of these "natural" pairings are superior to their mixed counterparts: necessitist comparativism and contingentist absolutism. These mixed views entail a mismatch between the metaphysics of quantities and the metaphysics of the laws in a way that generates unsynchronized changes—both violate some of our modal scruples. I will not provide arguments here. For similar reasons I will resist the deflationist move of holding contingentism and necessitism to merely generate different gradations of nomological necessity. The evaluation of counterfactuals (and their contrast with counterlegals) is central to scientific practice and we should hope for a univocal standard.

[53] Something like this picture is worked out more thoroughly and formally in Jacobs (2022) and Martens (In Press).

[54] Note: Barrow himself makes the case for the opposing, contingentist conception of the constants—though it seems that this goes along with a (metaphysical) contingentism regarding the laws as well.

a strict, two-way supervenience relation between the laws and the quantities they govern. As changes in the laws are, by assumption, impossible, so too are changes in the quantities.[55]

That sketch is not likely to be convincing. Let me quickly elaborate on what is meant by strong determinism and the case for its entailment by the conjunction of quantity absolutism and constant necessitism. Chen (2022) has recently given a definition of "strong determinism" and has laid out some of its consequences. Strong determinism is logically stronger than determinism and logically weaker than superdeterminism, which comes up in the foundations of quantum mechanics literature. I adopt this definition from Chen:

> (Strong Determinism) A world is strongly deterministic if its fundamental laws are compatible with only one possible world.

This view is compatible with the leading accounts of physical laws. As Chen has it, there are a couple of toy models in which strong determinism can be shown to hold, but only one realistic physical theory which is stongly deterministic: the Everettian Wentaculus (see also Chen 2023).[56] If I can make the case that strong determinism is entailed by quantity absolutism and constant necessitism then I will have shown that strong determinism is easier to get than Chen makes it seem—it will be theory *independent* and follow from *metaphysical* theses.

I know of no way to make this case. In order to argue that constant necessitism implies strong determinism, I must substitute a metaphysical thesis for an empirical conjecture. Rather than involve absolutism in the argument directly, I only hold that a strategy to avoid the amended argument against absolutism given above is to adopt constant necessitism, so a commitment to absolutism weakly implies a commitment

[55]This presentation of the these two positions is intended as a clarification of what has been at stake in debates concerning the comparativist reformulation of the laws. The laws seem intuitively to refer to absolute quantities—the escape velocity equation does not (explicitly) refer to any mass other than that of the planet. Starting with Dasgupta (2013) and continuing with Baker (2020, pp. 83–92) and Sider (2020, pp. 145–50), many different formulations of the comparativist Newtonian laws have been proposed and criticized. To debate whether or not there is a coherent comparativist statement of the laws just is to debate the merits of constant contingentism.

[56]I am not dealing with quantum mechanics here, so I will say nothing about this case.

to constant necessitism (until another solution is found). The empirical conjecture is one that naturally comes out of a tradition of taking the magnitudes of the constants of nature to determine many important facts about our world—upper and lower bounds on planet size, the existence of habitable planets, the existence of life, etc.—of which Barrow was an heir (e.g. Press et al. 1983; Weisskopf 1975). The conjecture is this:

> (Constant Determinism) As our understanding of physical theory increases we will increasingly find that the magnitudes of the constants determine more and more of the facts of our universe: in the limit the constants will be sufficient to determine the initial conditions of the universe.

This increase of theoretical understanding may include, for example, the discovery of further fundamental constants related to novel forces or the unification of multiple constants in a more fundamental theory. The argument then is that the conjunction of constant necessitism and constant determinism implies strong determinism (in the sense of Chen 2022). While this seems to be alternative route to strong determinism than Chen countenances, it is clearly not theory-independent and so is a matter ultimately for empirical investigation rather than a metaphysical thesis.

While constant determinism is endorsed by some impressive figures, its plausibility is questionable and its empirical confirmation a long ways away. That said, *if* it is accepted, then my argument against absolutism can ultimately be understood as a *reductio*, with the consequences of constant necessitism (with the assumed fact of constant determinism) being the *absurdum*.[57] It is worth noting, however, that this result is not regarded as absurd by some: Einstein, who held a version of (nomological) Spinozism,[58] seems to have to taken constant necessitism and constant determinism as a package deal.

[57]For some of the problems with strong determinism see Chen (2022).
[58]See Paty (1986).

> Or one could put it like this: In a reasonable theory there are no (dimensionless) numbers whose values are only empirically determinable.
>
> Of course, I cannot prove this. But I cannot imagine a unified and reasonable theory which explicitly contains a number which the whim of the Creator might just as well have chosen differently, whereby a qualitatively different lawfulness of the world would have resulted.
>
> Or one could put it like this: A theory which in its fundamental equations explicitly contains a non-basic [nicht rationelle] constant would have to be somehow constructed from bits and pieces which are logically independent of each other; but I am confident that this world is not such that so ugly a construction is needed for its theoretical comprehension. (Einstein to Rosenthal-Schneider 1945, in Rosenthal-Schneider 1980, pp. 37–8)

The differences between Einstein's typology of the constants and the typology used here is of little philosophical importance. Further, Einstein's typology of constants is too complicated to give a full interpretation here; however, some notes must be made to forestall confusion. Rosenthal-Schneider translates "rationell" as "basic" (see Rosenthal-Schneider 1980, p. 36). In the same letter as that quoted above Einstein gives π and Euler's number, e, as examples of "rationelle Zahlen" (which meet further special simplicity conditions). Other rationelle Zahlen include the fundamental dimensionless constants that would appear in a final, complete theory of physics (they would be constructable from some subset of the dimensional, "universal" constants). In such a theory a dimensional constant like G would be merely apparent while a dimensionless one, like α would be real—if α could be given a rational (theoretical) derivation. Why would the arbitrariness of value of the constructed dimensionless constant lead to a fractured theoretical structure (logically independent bits)? I propose that the free variation of a dimensionless constant constructed from dimensional constants implies that the relative magnitudes of the dimensional constants are only empirically determinable—this would mean, e.g. that the relative strength of the gravitational and electromagnetic forces would only be empirically determinable and so there would be two logically independent sorts of forces.[59]

The apparent arbitrariness in values of the constants to which the contingentist is committed, that of the *real*, fundamental constants and not merely *apparent*, eliminable ones, troubled Einstein. Whatever one thinks of the metaphysical possibility of strong determinism, Einstein's expression of it puts the contingentist comparativist on notice: The comparativist ought to endeavor to show that the arbitrariness of the connections between logically distinct quantity dimensions is not so ugly a construction after all.

Let me say a bit in defense of a contingentist comparativism that points the way to the work still to be done to fully flesh out what the comparativist's commitments are. The contingentist comparativist does not think that "anything goes" with respect to the real, fundamental, physical constants; there is a feature of them that is nomically necessary. What remains invariant under the comparativist symmetries is the algebraic relation between the constants and the other parameters in the laws. Their relation is encoded by their *relative* dimensionality. With $[G] = \mathrm{L}^3\mathrm{M}^{-1}\mathrm{T}^{-2}$, as required by the dimensional homogeneity of Newton's law of gravitation, $F = \dfrac{GMm}{r^2}$, it is nomically necessary that G scales inversely with M and cubicly with L. This is independent on any changes of convention regarding units or the basic quantity dimensions—the relation holds if force is treated as basic and mass as derived.[60] It is a contingent matter of fact what magnitudes quantities have, including constants. It is matter of convention which units we use to measure them and (maybe) which dimensions we stipulate as basic. The dimensional (i.e. scaling)

[59] Elsewhere, Rosenthal-Schneider (1949) discusses Einstein's views of the constants (and quantities more generally) in the broader context of his epistemology of science—of special significance is the connection she draws to the reality principle in Einstein, Podolsky, and Rosen (1935). It is worth noting that Einstein endorses Rosenthal-Schneider's interpretation of his remarks on the elimination of arbitrarily valued constants in a letter to Rosenthal-Schneider on the 24th of March, 1950 (Rosenthal-Schneider 1980, p. 41). Many thanks to Caspar Jacobs and Dennis Lehmkuhl for raising some of these interpretational difficulties to me—remaining murkiness or errors are my responsibility.

[60] On the scope and limitations of conventionality in dimensional systems see (Johnson 2018; Palacios 1964).

relation between different quantities is *nomically necessary*.[61] A full account of comparativism must find some way of accounting for these inter-quantity relations and their boundedness by physical law.

Something more can be said to sharpen this problem and to present an initial answer to it. Some, like Sider (2020) and Baker (2013), seem to hold that the value of some constant as *essential* to that constant to the extent that they introduce units into the laws by their interpretation. Therefore, comparativist and absolutist alike have a problem retaining the representational symmetry of unit system changes. Sider attempts to get around this problem by relativizing the laws to a choice of representation functions (unit systems), but this only forces the (mixed) absolutist into a constant contingency or into the naive absolutism—where unit transformations fail to be symmetries, an absurd conclusion.[62] Baker argues that the comparativist may posit fundamental mixed relations among quantities of different dimensions in order to avoid the positing of numerical constants, but in doing so the comparativist loses any parsimony advantage over the absolutist, sacrificing the original motivation for the comparativist.

This would seem to be an ugly construction indeed. Let me suggest that the notion of a constant used here causes no such problem. First let me simply reiterate that on the contingentist conception of the constants the nature of the constants is entirely *unit-free*, my understanding here is of the (field) constants as dimensional *quantities*. I have not endeavored to give a full account of the constants and their ontology, however positing them as something like fundamental mixed relations between quantity *dimensions* seems right.[63] Though there has not been much philosophical work on the nature of the constants, recently Jacobs (2021, chapter 6) has, by way of giving a very similar solution to the escape velocity case as that above, offered an account of G as a fundamental mixed relation that is part of the structure of Newtonian Gravity.

[61] I cannot discuss this material fully here, but it may be worth comparing the results here to the discussion in Duff (2014). Compare also recent discussions in Grozier (2020) and Riordan (2015) on the question of the fundamental constants and dimensionality.

[62] I've avoided discussion of this issue, see Eddon (2013a) and Wolff (2020) for discussions of this view.

[63] Here fundamental is only relative to the ontology of quantity dimensions—quantity dimensions themselves may depend on something more fundamental. That said, quantity dimensions seem like natural candidates for the fundamental ontology of physics. There are different sorts of realist views we can have about quantity dimensions, as discussed in Skow (2017). We could alternatively be irrealists and conventionalist regarding quantity dimensions and therefore regard the constants, *qua* fundamental mixed relations, as mere conceptual guides to thinking about the relations between measured quantities (Bridgman 1931). I believe the argument against absolutism can run regardless of the metaphysics of dimension—this is the point of the two different proofs of the Π-theorem—but I defend a moderate quantity dimension realism elsewhere.

Still we can ask where G ought to be placed in the structure of Newtonian Gravity.[64] The suggestion here is that G, *qua* dimensional relation, plays a *nomological* role, hence no violation of ontological parsimony. The *dimensions* of G fix the functional form of the gravitational force law. This generalizes to a principle that every ineliminable constant of mixed dimension determines the functional form of a fundamental law. It is precisely these constants that tell us how otherwise logically independent quantity dimensions come together to form a physical universe. Rather than "number[s] which the whim of the Creator might just as well have chosen differently" that "would have to be somehow constructed from bits and pieces which are logically independent of each other" (as Einstein would have it), we have a picture of the constants as construction principles which glue together the fundamentals of creation into a coherent and lawful world.

The comparativist may take inspiration from the example provided by Plato's *Timaeus*:

> So if the body of the universe were to have come to be as a two dimensional plane, a single middle term would have sufficed to bind together its conjoining terms with itself. As it was, however, the universe was to be a solid, and solids are never joined together by just one middle term but always by two. Hence the god set water and air between fire and earth, and made them as proportionate to one another as was possible, so that what fire is to air, air is to water, and what air is to water, water is to earth. He then bound them together and thus he constructed the visible and tangible universe. This is the reason why these four particular constituents were used to beget the body of the world, making it a symphony of proportion. They bestowed friendship upon it, so that, having come together into a unity with itself, it

[64] More recently Jacobs (2022) has made the case that G is part of the kinematic structure of Newtonian gravitation and so my full mass doubling (or what Jacobs calls an "inclusive active mass scaling") is not quite a dynamical symmetry, but rather a similarity. I will not adjudicate this issue here. I have already alluded to the fact that similarities seem to be more general than the sorts of symmetries philosophers have so far been concerned with and that the Π-theorem is a guide to similarities, some of which correspond to dynamical and empirical symmetries (see Buckingham 1914; Sterrett 2009). I locate G in the *nomology* of Newtonian gravitation and hence in its dynamics.

> could not be undone by anyone but the one who had bound it together. (Plato 1997, pp. 1237–1238)

3.6 Conclusion

This paper presents an amendment to the symmetry argument against quantity absolutism. Rather than requiring that any universal scale transformations of the quantities of some basic dimension are empirical and dynamical symmetries, the argument against absolutism depends only on those symmetries defined by the Π-theorem. These symmetries may involve scale transformations of basic quantities, but they also involve transformations of derived quantities, most notably the physical constants. The symmetries defined by the Π-theorem are transformations that leave the dimensionless quantity ratios which describe some system invariant—these transformations are both empirical and dynamical symmetries.

The transformation of the constants in some symmetries defined by the Π-theorem raises the question of their modal status. On the one hand is constant contingentism, which states that the laws and the relations of the basic quantities determine the values of the constants—their values can vary in nomically possible worlds, supervening on variations of the relations of the basic quantities. On the other hand is constant necessitism, which states that the values of the constants are fixed across nomically possible worlds and are fundamental—their values and dimensions fix the laws and the relations between the basic quantities. My purpose has been to introduce the debate and set some of its terms. Though I give reason to prefer constant contingentism to constant necessitism, not least of all its superior fit with quantity comparativism, the discussion here is not conclusive.

3.7 Appendix A: Proof of the Representational Π-theorem

This proof proceeds on an understanding of equations as relations between numbers or representations of numbers (i.e. variables) and unit transformations as transformations of numbers. The structure of the proof is the same in both versions. The fundamental assumption being that we are only dealing with unit invariant or dimensionally homogeneous equations. The ratio scaling symmetries of basic quantities will propagate to derived quantities according to Bridgman's lemma. This allows any physical equation equation to be recast enitrely in terms of dimensionless derived quantities, by way of the reduction of superfluous variables in the expression of some equation.

A review of the requisite assumptions:

0. *Zeroth assumption*: Any equation describing a physical system can be represented by some function of numbers which represent quantities set equal to zero:

   $f(Q_1, Q_2, \ldots Q_N) = 0.$

1. *First assumption*: We are only concerned with "complete" equations whose algebraic form is unit-invariant. For such equations there is a class of representations:

   $f'(Q'_1, Q'_2, \ldots Q'_N) = 0,$

   where $x_i Q_i = Q'_i$ and the unit transformation factors $x_i \in \mathbb{R}^+$.

2. *Second assumption*: If the equation describing the system is unit invariant, then the numbers representing derivative quantities are unit-transformed by transformation factors that can be defined as products of powers of the unit-transformation factors of the numerical representations of the constituent basic quantities.

These are the fundamental assumptions of dimensional analysis; to give them up would be to forgo many important patterns of physical reasoning and would threaten the marriage of measurement and number.

The proof proceeds:[65] We define the number of derivative quantities, r, as the difference in the total set of quantities describing the phenomena, N, and the subset of basic quantities, n: $r = N - n$. We can understand the n basic dimensions to serve as a *reduction base* for the original description of the system by N quantities. If r is non-zero, then the reduction exists, and with Bridgman's lemma, we can define the relations between the derivative and basic transformation factors with a set of r equations:

$$\left\{ \begin{array}{l} x_{n+1} = x_1^{a_{n+1,1}} x_2^{a_{n+1,2}} \dots x_n^{a_{n+1,n}} \\ x_{n+2} = x_2^{a_{n+2,1}} x_2^{a_{n+2,2}} \dots x_n^{a_{n+2,n}} \\ \vdots \\ x_{n+r} = x_1^{a_{n+r,1}} x_2^{a_{n+r,2}} \dots x_n^{a_{n+r,n}} \end{array} \right\},$$

where the exponents $a_{i,j}$ are defined by the relation $Q_i \propto Q_j^{a_{i,j}}$, with $i = n+1, n+2, \dots, n+r$ and $j = 1, 2, \dots, n$.[66] These $x_{n+1}, \dots, x_{n+r}$ factors are the numerical scale factors for unit transformations of the numerical representations of the derived quantities. The values of these factors depend on the unit transformations on the basic quantities and the defined relationships between the derived and basic quantity representatives.

From this equation set, we define r unitless Π-terms, eliminating all of the transformation factors and involving all of the relevant representations of quantities:

[65] This presentation of the proof is based on Ehrenfest-Afanassjewa (1916b).
[66] The exponent will be zero for all irrelevant quantities Q_j.

$$\left\{\begin{array}{l} \Pi_{n+1} = \frac{Q'_{n+1}}{Q'^{a_{1,1}}_1 Q'^{a_{1,2}}_2 \dots Q'^{a_{1,n}}_n} = \frac{Q_{n+1}}{Q^{a_{1,1}}_1 Q^{a_{1,2}}_2 \dots Q^{a_{1,n}}_n} \\ \Pi_{n+2} = \frac{Q'_{n+2}}{Q'^{a_{2,1}}_1 Q'^{a_{2,2}}_2 \dots Q'^{a_{2,n}}_n} = \frac{Q_{n+2}}{Q^{a_{2,1}}_1 Q^{a_{2,2}}_2 \dots Q^{a_{2,n}}_n} \\ \vdots \\ \Pi_{n+r} = \frac{Q'_{n+r}}{Q'^{a_{r,1}}_1 Q'^{a_{r,2}}_2 \dots Q'^{a_{r,n}}_n} = \frac{Q_{n+r}}{Q^{a_{r,1}}_1 Q^{a_{r,2}}_2 \dots Q^{a_{r,n}}_n} \end{array}\right\}.$$

Since the Π-terms are unit transformation invariant, the Π-terms are equivalently defined out of the Qs and the Q's—and encode the essential relations between derived and basic quantity measurements. This means that any equation describing the measurement results of a physical system can be recast in Ur-Equation form:

$$f(\Pi_1, \Pi_2, \dots, \Pi_r) = 0.$$

This is the (representational) Π-theorem. It was derived from three assumptions: (0) that numerical representations of physical systems exist which can be described as a function of numbers set equal to zero; (1) There is a subset of such numerical equations that are unit invariant; (2) Bridgman's lemma, i.e. the numerical measures of derived quantities are products of powers of the numerical measures of basic quantities.

3.8 Appendix B: Proof of the Ontic Π-theorem

This proof understands equations to directly represent quantity relations themselves and proceeds by considerations of ontic transformations of quantity dimensions rather than unit transformations. The relation between the two was more fully analyzed in 4.1, but the outline bares repeating. What is meant by calling this proof "ontic"? Of course, in neither the ontic or in the representational case are the *syntactic* objects which make up equation tokens taken to be quantities or numbers (rather they are variables and numerals). The ontic-representational distinction is this: either equations represent relations between quantities which are properties of physical systems or they represent relations between numbers which *measure* quantities according to some externally defined convention. Given the assumption of faithful measurement conventions, conclusions drawn under interpretations of the latter kind entail counterpart conclusions under interpretations of the former kind. This is to say: the two interpretations are in an important sense interchangeable, if numbers can measure quantities at all.

We begin again with a generalized functional form of a complete (i.e. unit-invariant) equation describing a physical system:[67]

$$f(Q_1, Q_2, \dots Q_N) = 0 = f({Q'}_1, Q_2, \dots, {Q'}_N),$$

where each Q_i is a quantity composed of a dimensionless number and a unit quantity, $Q_i = V_i U_i$, and the primed quantities are related by dimensionless transformation factors x_i. Here we abstract from the (conventional) determinancy of "value" and the "unit" of some quantity to its magnitude, M, and dimension, D, where each unit transformed quantity counterpart is identical in these respects: $Q_i, {Q'}_i, {Q''}_i, \dots = M_i$ and $[Q_i], [{Q'}_i], [{Q''}_i] = D_i$. This abstraction serves us with a *unit-free* representation of the quantities, much like tensor calculus allows us *coordinate-free* representations of spacetime—this makes it clear that we are dealing with the ontic quantities and not their mere representations.[68]

Given Bridgman's lemma, we can define each quantity as products of powers of the basic quantities, $Q_1, Q_2, \dots, Q_n$:

$$\left\{ \begin{array}{c} Q_1 = Q_1^{a_{1,1}} Q_2^{a_{1,2}} \dots Q_n^{a_{1,n}} \\ Q_2 = Q_1^{a_{2,1}} Q_2^{a_{2,2}} \dots Q_n^{a_{2,n}} \\ \vdots \\ Q_N = Q_1^{a_{N,1}} Q_2^{a_{N,2}} \dots Q_n^{a_{N,n}} \end{array} \right\}.$$

[67]This presentation of the proof is based on Gibbings (1982); Gibbings (2011).

[68]Note that this is merely a presentational move, the "representational" proof given above proceeds in a unit fixed representation, but defines transformations and relations which are invariant under any unit standard. There is an important sense in which these two approaches are equivalent, compare Wallace (2019) and Wolff (2020, chap. 9).

Since we are dealing with a coherent dimensional system, the same construction applies to quantity dimensions themselves; they can be defined in terms of basic quantity dimensions, $D_1, D_2, \ldots, D_n$:

$$\left\{\begin{array}{c} [Q_1] = D_1^{a_{1,1}} D_2^{a_{1,2}} \ldots D_n^{a_{1,n}} \\ [Q_2] = D_1^{a_{2,1}} D_2^{a_{2,2}} \ldots D_n^{a_{2,n}} \\ \vdots \\ [Q_N] = D_1^{a_{N,1}} D_2^{a_{N,2}} \ldots D_n^{a_{N,n}} \end{array}\right\}.$$

Now we take a quantity Q_i from a subset $Q_i \subset Q_N$ of quantities such that some of Q_i's basic quantity exponents, $a_{i,j}$, are zero, meaning that their dimension does not require all D_n and divide through each row of the quantitative matrix by Q_i so as to cancel its dimension $[Q_i]$ in all the other quantities. As this elimination process iterates, we will be left with dimensionless quantities. For the first dimension D_1 and each Q_j, $j \neq i$:

$$\frac{Q_j^{a_{i,1}}}{Q_i^{a_{j,1}}} \rightarrow \frac{D_1^{a_{j,1}a_{i,1}}}{D_1^{a_{i,1}a_{j,1}}} = 1.$$

The division procedure described above guarantees that the power of the dimension in the numerator and the denominator is equal, hence the dimension is eliminated in the quotient quantity. This creates the functional, complete equation:

$$f\left(\frac{Q_1^{a_{i,1}}}{Q_i^{a_{1,1}}}, \ldots, \frac{Q_N^{a_{i,1}}}{Q_i^{a_{N,1}}}\right) = 0.$$

Successive cancellations up to D_n for all Q_i lead to all dimensions being eliminated and so all quantities in the function are dimensionless Π-terms of the same form as those defined in the last subsection:

$$\begin{Bmatrix} \Pi_{n+1} = \frac{Q'_{n+1}}{{Q'}_1^{a_{1,1}}{Q'}_2^{a_{1,2}}...{Q'}_n^{a_{1,n}}} = \frac{Q_{n+1}}{Q_1^{a_{1,1}}Q_2^{a_{1,2}}...Q_n^{a_{1,n}}} \\ \Pi_{n+2} = \frac{Q'_{n+2}}{{Q'}_1^{a_{2,1}}{Q'}_2^{a_{2,2}}...{Q'}_n^{a_{2,n}}} = \frac{Q_{n+2}}{Q_1^{a_{2,1}}Q_2^{a_{2,2}}...Q_n^{a_{2,n}}} \\ \vdots \\ \Pi_{n+r} = \frac{Q'_{n+r}}{{Q'}_1^{a_{r,1}}{Q'}_2^{a_{r,2}}...{Q'}_n^{a_{r,n}}} = \frac{Q_{n+r}}{Q_1^{a_{r,1}}Q_2^{a_{r,2}}...Q_n^{a_{r,n}}} \end{Bmatrix},$$

yielding a proof of the (ontic) Π-theorem:

$$f(\Pi_1, \Pi_2, \ldots, \Pi_r) = 0.$$

The divisional procedure of eliminating dimensions highlights an important aspect of the Π-theorem. It provides a definitive answer to the number of variables and the number of dimensionless groups required to describe some system. As indicated above there are $r = N - n$ dimensionless groups of variables, Π-terms, necessary to describe a system of N quantities formed by n basic dimensions. The removal of each dimension is associated with the addition of a variable to each Π-term, yielding a number of $n+1$ variables per Π-term.[69]

[69]For details and exceptions see Gibbings (2011, pp. 59–61).

Chapter 4

Measurement and Systematic Error

4.1 Introduction: Dimensional Analysis and Quantitative Measurement

Issues of calibration and measurement are common to everyday life. We are all familiar with oral thermometers, often used at the doctor's office: the metal appendage of the device is placed under the patient's tongue and after a short period of time the reading on the device—hopefully close to 98.6 degrees Fahrenheit—indicates the internal body temperature of the patient. Either due to efficiency needs or to avoid possible infections, most of us have recently become familiar with another kind of thermometer, the infrared thermometer. Fewer of us know how this device exactly works, but the external aspects of the measurement process are clear enough: a light gun is pointed at the patient's forehead, presumably some reflection of the infrared (hence invisible to us) light is reabsorbed, and a reading on the device indicates the internal temperature of the patient. Now this is the essential problem of calibration: How can we establish that these two measurement processes, using two devices with distinct causal pathways, measure the *same* quantity, namely the internal body temperature of a human being? The reader may be more familiar with a special case of the calibration problem that arises *within* a measurement process paradigm: how can we establish that these two measurement processes, using the same kind of device, are measuring the same quantity? Issues of accuracy arise for both the specific and the general problem, but more philosophically fundamental issues arise in the general problem which we will take as our focus.

I take it that the central task of the epistemology of measurement is to adequately account for the calibration of measurement processes. There are two conditions of adequacy for an account of calibration which pull in two different directions. First, an account of calibration must be faithful to scientific practice; it must serve as a descriptive model for actual calibration procedures, at least when they are successful. Second, an account of calibration must explain how it is that calibration is *successful*; it must serve as a normative standard for the evaluation of calibration procedures, distinguishing the promising from the regrettable. The first condition can be called the descriptive condition and the second condition can be called the normative condition. I take it that these conditions are common constraints in naturalistic philosophical approaches.

Recently, an epistemology of measurement has been developed which has taken seriously the need to meet both conditions of adequacy: the theory of model mediated measurement (TMMM). The TMMM takes seriously the centrality of the task of calibration to an epistemology of measurement and holds that all measurements are mediated by models of the measurement process. Models are *representations* of phenomena that involve both theoretical and empirical aspects. While models are best understood functionally (i.e. if it acts like a model then it is a model) they can be understood as non-fundamental theories with limited scope and built in empirical assumptions, that often are inconsistent with their mother theories, but increase their usefulness in several respects. Further, models need not have been developed *from* more fundamental theory, but may have lives of their own (see Morrison 1999). We might consider there being at least two paths to a model: top-down, adding assumptions and restrictions to a theory, and bottom-up, generalizing and abstracting from data. This chapter is an extension of the TMMM framework.

In order to meet the descriptive condition of an epistemology of measurement, the appearance of epistemic circles and the centrality of the common quantity assumption to the calibration process must be accounted for in the TMMM (following Tal 2019). In calibrating one measurement process to another, the experimenter must assume, in order to detect and correct for systematic errors, that the two measurement processes are measuring the same target quantity.[1] This means that the experimenter must assume what she sought to test by comparison of the measurement results of the two measurement processes: that the novel measurement process is indeed a valid measurement process—that is to say, that the novel measurement process measures the intended (kind of) quantity.[2] The task of calibration is complete when a reliable function from the measurement indicator (i.e. the reading of a device) to the measurement outcome (i.e. the attribution of a quantity magnitude) is established for the measurement process in question.

The normative condition requires that successful calibration *justifies* (belief in) measurement outcomes yielded by the application of the same measurement process on phenomena beyond those calibrated against. Phenomena outside of the calibration set (whose magnitudes are established independently by *a priori* stipulation or by a distinct, established measurement process) have unknown magnitudes. The normative condition on calibration would be most perfectly achieved in the case in which we can compare the measurement result, the quantity magnitude attributed to some phenomenon, to its "true value". The essential problem is that this ideal is impracticable if not impossible—independent, unvarnished access to true values would make measurement unnecessary. The satisfaction of the descriptive condition reveals circles, or iterative cycles, of justification in the calibration process that chafes with some epistemic principles taken to be normative in the scientific domain. The acceptance of coherentist justifications of calibration, and so measurement generally, is generated by what is called, among other names, the experimenter's regress or the problem of nomic measurement. Coherentist epistemologies, in this case the TMMM, face a skeptical challenge: for all the coherence and predictive success generated by the network

[1] As noted by Tal, there is a persistent type-token ambiguity in discussion of this issue—corresponding to the general and specific calibration problems illustrated in the first paragraph of this introduction. I follow him in carrying both senses unless otherwise indicated—though the type reading, relevant to the *general* task of calibration, is the primary one.

[2] This is especially problematic in the case of novel measurement targets, where there is no prior indication of the magnitude of the target quantity, or whether there even is one (see Collins 1985; Feest 2016; Franklin 1997; Soler 2015; Zhao 2023).

of theory, models, calibration, and observation, the entire program may have gotten detached from the truth.

My focus is on the common quantity assumption node in the epistemic web of the TMMM. I argue here that the principle of dimensional homogeneity, that the quantitative equality of two quantities requires that they are of the same dimension, provides a necessary condition for the common quantity assumption, independent of the coherentist circles of justification generated by specific models of measurement processes. My modification to the TMMM ameliorates the skeptical problem for a coherentist epistemology of measurement that arises from the possibility of systematic error in metrological extension (which threatens the validity of the common quantity assumption). Consideration of a case study will show an actual use of the principle of dimensional homogeneity in the identification and elimination of systematic error. The case under consideration is a canonical example of metrological extension done by a pioneer of both metrology and dimensional analysis, Percy Bridgman. Consideration of his use of dimensional analysis in calibrating secondary gauges for high pressure experiments, pressures at which no extant gauge was competent to measure, shows the role of dimensional models in calibration. Dimensional analysis yields *dimensional* models of commensurable measurement processes that are independent of the details of *causal* models of particular measurement processes; hence, the common quantity hypothesis can enjoy epistemic support independently of the existence of causal sources of systematic error.

4.2 The Theory of Model Mediated Measurement

In recent decades, there has been an increased focus in the philosophy of science on the role of *models* in scientific inquiry.[3] Models are representations of important features of a system which are describable in the language of some theory. For example, heliocentric orbits in a manner (approximately) described by Kepler's Laws are models of planetary motions in the solar system. Models are generally understood to be

[3] A landmark text is Morgan and Morrison (1999).

distinct from theories and data, while having the function of *mediating* between them. Models make theories applicable to data (e.g. by making detailed predictions possible) and make data applicable to theories (e.g. by showing how they are relevant to the evaluation of a theory). For example, apparent retrograde motion in itself cannot be taken as evidence for or against Newton's theory of gravity. This is because three body problems (necessary for apparent retrograde motion) are not exactly solvable in the theory. The simplifying (and false) assumptions that constitute the Keplerian orbital model make possible the observation of the apparent retrograde motion of a planet *as* a confirming prediction of Newtonian gravity. In the absence of such a model, retrograde motion may be taken instead to be evidence of a planetary epicycle, i.e. a real reversal of motion. Generally speaking, models involve additional assumptions that often make them *inconsistent* with the same theories they are models with respect to: e.g. assumptions regarding the non-interaction of planets allow for Keplerian orbits to be approximated by Newton's inverse-square gravitational law.

> The law of universal gravitation explains why the planets follow Kepler's laws approximately and why they depart from the laws in the way they do. (Cohen 1985, p. 169)[4]

Theories explain why models work in some respects and why they do not in others. This is the hallmark relation between theories and models (or fundamental laws and special laws).

We can understand the role of models as analogous to or just the same as *phenomena* in the Bogen and J. Woodward (1988) sense. Bogen and Woodward distinguish *data*, observations or reports of observations, from *phenomena*, which are putatively objective (and not necessarily observable) features of reality. Phenomena are models (generally) or realizations of models (in the factive case). On this account of scientific inquiry theories are not directly confirmed by data, nor do theories explain data. Rather, there are two distinct epistemic interfaces: theories explain and are confirmed by phenomena; phenomena explain and are

[4]I am following Cohen in considering Kepler's laws in the context of Newtonian theory, so I am being anachronistic with respect to the role that they had in Kepler's own thinking. For more on the changing role of Kepler's laws before and after the "Newtonian Revolution" see Baigrie (1987).

confirmed by data. J. F. Woodward (2011) makes clear that this does not mean there is no epistemic relation between theory and data—theory often plays a role in interpreting data, relating them to phenomena—but the epistemic relations of *explanation* and *confirmation* are only made indirectly, through phenomena. Likewise the TMMM distinquishes two particular kinds of phenomena and data: measurement outcomes and measurement indications. Measurement indications are data, like (reports of) the number of Geiger counter clicks or (reports of) number-unit pairs like "1 meter".[5] Measurement outcomes are statements that project the data onto some object or system and are partially constituted by models of the measurement process that led to the corresponding measurement indications:[6] that some sample is radioactive or that the length of some object is 1 meter. Bogen and Woodward make the case that measurement indications are data and measurement outcomes are phenomena themselves in consideration of the case of the melting point of lead:

> [O]ne does *not* determine the melting point of lead by observing the result of a single thermometer reading. To determine the melting point one must make a series of measurements[. . .] These constitute data. (Bogen and J. Woodward 1988, p. 308)

Thermometer readings constitute data, and it is only with various assumptions, particularly regarding the (statistical) phenomena-model that a claim regarding the actual melting point itself is inferred.

> [T]he true melting point is certainly *inferred* or *estimated* from observed data, on the basis of a theory of statistical inference and various other assumptions[. . .] (Bogen and J. Woodward 1988, p. 309)

[5]I do not wish to engage in a "protocol sentence" debate here, choose the formulation which best fits your philosophical conscience. One intuitive reason for emphasizing that the data may be construed as reports is to make obvious the gap between them and measurement outcomes. A reason for wishing to do this is that measurement outcomes (often implicitly) have error ranges embedded in them: "When I measure a table and obtain the result 75.3 centimeters, that is the number I write in my notebook, and that is what I report if someone asks me for my result. I do not mean to claim, and 1 do not believe, that the ratio of the length of the table to the standard meter is 0.753 000 000." (Kyburg 1984, p. 12)

[6]As Tal has it, they therefore *predict* future indications and *explain* past indications.

Recasting the TMMM in light of this general and familiar theory-phenomena-data account of scientific inquiry goes some way towards clarifying the meaning of "mediation".

Eran Tal (2017a) has made the case that measurement is essentially *model based.* This view has important predecessors (e.g. Chang 2004) and there appears to be a growing consensus around some version of this view (see Mari, Wilson, and Maul 2021). I will not attempt a survey of the literature, but will focus on the account developed by Tal in several papers. I argue that there is an extension of this theory of model mediated measurement which makes it more resistant to a persistent skeptical challenge from the risk of systematic error. A principle of dimensional analysis supplies the resources needed to ameliorate this problem in the TMMM—dimensional models lessen the likelihood of systematic error and provide independent evidence for the *common quantity assumption.* In the TMMM, the common quantity assumption and the absence of systematic error form part of a coherentist circle of justification, unresponsive to a skeptical challenge that motivates foundationalist epistemologies of measurement. Skeptical challenges can never be eliminated in general, but in this case the skeptical challenge can be guarded against, leaving the TMMM account of calibration on more robust and descriptively accurate footing.

Let me begin by listing some theses that characterize a TMMM:[7]

> (Model Mediation) The interpretation of measurement indicators *requires* the use of a model of the measurement process (the interaction of the measurement device with the measurand).[8]
> *Example: The interpretation of the numeral markings on a ruler as lengths associated with objects that meet them requires modeling rulers as rigid bodies.*
>
> (Inductive Projectibility) Models of measurements are robust repeatables; the connection between measurands and indicators must be projectible beyond the class of independently known

[7] I am *not* attempting to give necessary and sufficient conditions for a TMMM. The idea is to sketch a framework from which particular TMMMs may depart in some way or another.

[8] A measurand, or target quantity, is the actual quantity that is intended to be measured by the measurement process being modeled. The "true value" of this quantity may differ from the predicted value by the measurement outcome—the difference between the two is the accuracy of the measurement process.

measurands (standards) used in calibration. *Example: As a ruler is a rigid body, the length associated with the marking that matches a standard inch is an invariant property of that partition of the ruler, hence the marking will indicate the length of other one inch objects.*

(Objectivity) Measurement outcomes are attributed to the measurand and not to the process of measurement. *Example: An object measured with two different rulers or a ruler and caliper will have an invariant length—a property therefore of the object and not any ruler or caliper measurement procedure in particular.*

The function of models in the TMMM is to link the indications of our measurement devices to properties of the measurand. We can understand these three theses as highlighting different aspects of the process of objectifying data. Model Mediation is a statement to the effect that data without a model are dumb: the data themselves make no claim unless something is supposed about the measurement process which generated the data (compare Boyd 2018). Inductive Projectibility is the condition that models must meet to objectify data—models must organize the data into claims about the world that are not fragile, i.e. that do not depend on the particularities of the measurement process. Importantly this invariance includes an invariance relative to a choice of units: inductive projection takes number-unit pairs (measurement indications) and maps to objective quantities (measurement outcomes) which are unit-independent. The degree to which models are projectible determines the degree to which the phenomena therefore described are objective. Objectivity makes it such that Model Mediation does not result in an *undesirable* idealism—measurement outcomes are intersubjectively invariant or "robust".[9] If a model fails to be projectible, the agreement of the model mediated measurement outcomes with a class of standards is due to coincidence.[10]

[9]See Tal (2017a); Tal (2017b) for more on this robustness condition.

[10]To be clear, by "standards" I mean either operational realizations of "defined" quantities or the measurement outcomes of other measurement procedures that are contextually taken as ideally accurate. Following Tal, I do not hold that standards need to be significant in any non-contextual way. For more on the roles of local and global standards in coherent calibration, see Tal (2017a) and Tal (2017b), §5.

The TMMM faces a skeptical problem. This problem arises from the possibility of systematic errors of unknown form and magnitude.[11] The possibility of such errors raises the spectre of an incongruence of a categorical kind: what if my measurement process is measuring something other than the intended measurand? This issue is not so much as solved by the TMMM as it is recognized as a fundamental limitation. What Tal (2019) has dubbed the common quantity assumption is core to the TMMM. We must, in advance of measurement, *assume* that the quantity our modeled measurement process is designed to measure is in fact what is measured—only then can systematic sources of error be identified and minimized. The iterative process of error detection, cross-calibration, and model adjustment is all a refinement of the fundamental assumption. In this way the common quantity assumption is a transcendental posit central to the TMMM project; it is a necessary condition on the possibility of measurement.[12] Like other transcendental posits, it is vulnerable to skepticism: generally speaking transcendental arguments only determine how things must seem to us, not how they must *be* (see Stroud 1968). This is particularly of concern in cases of metrological extension, the calibration of measurement processes that exceed the range of any other measurement processes, often in domains that are not well controlled by theory (as in §4.4).

While the common quantity assumption *is* essential to calibration, we may hope for more. I argue that in advance of measurement we have a better epistemic standing than the name "common quantity assumption" suggests. The common quantity assumption is in fact a hypothesis, with evidential vulnerability, independent of the cycles of justification which depend on it.[13] I argue that there is a necessary condition for the validity of the common quantity hypothesis—dimensional homogeneity. Dimensional analysis teaches us that quantities have properties of great significance, their dimensions. Dimensional

[11]Systematic errors differ from random errors in being occurrant in repetitions of measurement, they cannot be eliminated by increasing the number of measurement trials, etc—systematic errors have a non-zero expectation value. The existence of such errors can cause issues in the calibration of discordant measurement processes. See Ohnesorge (2021, 2022) for analysis and case studies of such "problems of hard coordination". See also Isaac (2022) for some suggestive remarks regarding the malleability of the distinction between systematic and random error.

[12]For example, in the context of scale extension of a quantity, Tal writes: "this sort of dogmatic supposition [of a common quantity] can be regarded as a manifestation of a regulative ideal, an ideal that strives to keep quantity concepts unified and background theories simple." (2019, p. 875)

[13]This is counter to Tal's claim that "the tasks of establishing what instruments measure, how accurate they are, and whether they agree, are epistemically entangled, and cannot be accomplished in isolation from one another." (2019, p. 862)

homogeneity is a principle that holds that a necessary condition on the identity of quantities is that they have the same dimension (following Fourier 1878):[14]

> (Dimensional Homogeneity) A representationally adequate physical equation must have terms of equal dimension.

This is often put as a principle restricting the manipulation of quantity equations: Masses can only equate to masses, forces to forces, etc. I here extend its scope to measurements. A necessary condition on the common quantity assumption is that the different measurements target a common quantity *dimension*. Dimensional homogeneity is therefore a necessary condition for any common quantity hypothesis. In order to suppose that the same quantity is measured by two distinct measurement processes, it must be established that the quantities which appear in the models of each measurement process are of like dimension—or at least it must not be established that they have distinct dimensions. That the dimensional models by which such identifications are established or rejected are independent of the specific causal models that correspond to each measurement process is argued in §4.4.3. This provides a defense against the skeptical argument from systematic error that is epistemically independent of the skeptical scenario itself, something which an unmodified TMMM cannot provide.

4.3 The Task of Calibration

In order to motivate my modification of the TMMM, the problem which generates the necessity of the common quantity assumption needs to be considered in more detail. The central problem in the epistemology of measurement has been discussed in the literature under a number of different guises: the problem of nomic measurement (Chang 1995, 2004), the problem of quantity individuation (Tal 2019), the experimenter's regress (Boyd 2021; Collins 1985, 2016), hard problems of coordination (Ohnesorge 2022),

[14] I here assume that physical equations represent quantity relations and not numerical relations. This view is not uncontroversial and has a long history, see: de Courtenay (2015); De Clark (2017); Mitchell (2017); Sterrett (2021).

and so on. I will not be pedantic regarding the exact logical relations between these possibly distinct problems.[15] For my purposes they are all versions of the same circularity problem which appears to be an impediment to the central epistemic task of measurement: calibration. The task of calibration is the core task of the experimentalist: The experimentalist is tasked with determining that her measurement process is reliable, that is, that there is a law like relation ("calibration function") between the indicator values of her device and the magnitudes of measurands. Once this task is done, the experimentalist's measurement process yields *measurement outcomes* which are ascriptions of magnitudes to measurands according to interpretations of the indicator values according to the calibration function. The determination of the calibration function—the establishment of the reliability of the measurement indications of a measurement process—is the task of calibration.

We might characterize the essential epistemological problem as a transcendental one: How is calibration possible?[16] The descriptive precondition is met by an observation of the success of empirical science. The epistemological problem is a normative one: what epistemically justifies the calibration that we in fact do? Something of a consensus has grown around the thesis that the epistemic structure of calibration requires a coherentist epistemology.

The commitment to coherentism follows from the impossibility of a foundationalist method of calibration, on pain of regress. In order for the readings of an indicator to mean anything, they must be projected, via a model, onto the measurand. This projection is done by the establishment of a calibration function.[17] A calibration function, f_C, is a function from a target quantity, T, to an indicator quantity, I:[18]

[15]Tal (2019, 859, fn 11), for one, claims that the necessity of the common quantity assumption (and so the problem of quantity individuation) is a consequence of the problem of nomic measurement.

[16]Compare Tal's characterization of the epistemic project as answering the question: "How, [given the inaccessibility of true quantity values], is the evaluation of measurement accuracy and error possible?" (2017a, p. 238) This question transcendental in the Kantian sense insofar as it is a question of *right*, i.e. *quid juris*.

[17]I make two simplifications: (1) I am only dealing with the simplest "black-box" cases here, this all should generalize to cases in which internal sources of error are accounted for as well; (2) I am collapsing what Tal (2017b) distinguishes as the forward and backward calibration functions into a single calibration function—in a successful case they are inverses of each other.

[18]The indicator I need not be a quantity, it can be more coarse-grained, but I assume so here and throughout for the sake of uniformity.

(Calibration Function) $I = f_C(T)$

In the simplest case this function is determined by the repeated measurement of the magnitudes of standard quantities, whose magnitudes are verifiable by a different measurement process (or a different instance of the same measurement process, or by fiat (theory), etc.)—a regress is generated by consideration of the justification of the magnitudes assigned to these standards by these external processes. If this function is established, its inverse is used to project (interpretations of) indicator readings onto the real quantity:

(Inverse Calibration Function) $f_C^{-1}(I) = T'$

If the calibration function is faithful, then $T' \approx T$, within error. In order to establish the faithfulness of f_C, we not only have to appeal to various theoretical assumptions and practical approximations, but we must depend on the estimates of the magnitudes of the standards by other calibration functions f'_C,f''_C, f'''_C, etc. The result is a large web, or network, of mutual reinforcing and correcting measurement processes—Chang (1995) describes this as "the mutual grounding of measurement methods". The essential issue for any coherentist epistemology is truth—these measurement outcomes may cohere, but they could, at the same time, be inaccurate due to undetected systematic errors. If we cannot privilege at least one such calibration function as foundational, then this skeptical worry requires a leap of faith to be overcome: the common quantity assumption.[19]

4.3.1 Against Foundationalism: Metrological Extension

In this section I will show that an indicative foundationalist epistemology, operationalism, fails to account for a central aspect of the task of calibration, metrological extension. This lends support to the idea that the task of calibration necessitates a coherentist epistemology such as the TMMM. In the next section,

[19]See Isaac (2022) for an argument that increased precision limits the strength of such worries.

I show how another aspect of calibration, the risk of systematic error, threatens the coherentist TMMM epistemology.[20]

Bridgman's operationalism, the highly influential and infamous account of scientific concepts, can be summarized so:

> (Bridgman's Semantic Thesis) The meaning of some quantity term is just the set of quantities measurable by some set of operations.[21]

Similar to verificationist theories of meaning (see Uebel 2019), the operationalist theory of meaning generates spurious analytic truths; that some operation measures a target quantity is decided by fiat. For example: that a barometer faithfully measures atmospheric pressure would no longer be an empirical fact but rather true by definition. Such an account of calibration only helps insofar as it trivializes calibration itself by eliminating the very possibility of error. If the possibility of error is eliminated, then this foundationalist epistemology of measurement fails to meet the descriptive desideratum.[22]

Recall that the task here is to answer the transcendental question: How is the evaluation of measurement accuracy and error possible? If the operationalist defines some quantity as that which has the magnitude ascribed to it by some operation, then not only is error impossible but so is evaluating measurement processes. Under the account of calibration that naive operationalism provides different measuring processes measure different quantities *by definition* and so cannot serve as checks on each other. This is a disaster in cases of metrological extension, where a new measurement process is calibrated so as to measure magnitudes outside of the range of extant measurement processes (e.g. temperatures below the

[20]Similar discussions of the failure of operationalism to properly account for the epistemology of measurement can be found in Chang (2004). See Mari, Carbone, et al. (2017) and Tal (2019) for a more general discussion of the failures of various forms of empiricist foundationalism.

[21]"In general we mean by any concept nothing more than the set of operations; *the concept is synonymous with the corresponding set of operations*." (Bridgman 1927, p. 5) I generally share the opinion of Sigmund Koch on this matter: "It is to be emphasized that the famous 'criterion' on p. 5 is perhaps the most uncouth and ill-considered sentence that Bridgman ever wrote" (1992, p. 265).

[22]This is in a way a cheap shot at operationalism. Bridgman (1938, 1950) repeatedly modified (or perhaps clarified) his view, concluding that operations are only *necessary* and not sufficient for meaning, and further, the restriction on meaning is purpose dependent. I cannot here defend a sophisticated operationalism, though there are a number of reevaluations and rehabilitations of operationalism worth consulting: Koch (1992); Chang (2017); Vessonen(2021a,b); Jalloh (2022).

freezing point of mercury). This situation is especially dire in (the usual) case in which the novel measurement process extends into ranges where we cannot expect our current theoretical models to be valid; theory cannot insulate calibration from the possibility of new physics. A slightly less naive operationalism, in which different measurement processes may be said to measure the same quantities *by convention* if they give the same measurement outcomes to the same quantities,[23] still cannot handle cases of metrological extension, where the novel measurement process cannot be calibrated to any extant measurement process in its new range of measurement. I leave here as an undefended premise that the failures of operationalism to account for calibration in the case of metrological extension generalize to other foundationalist views.[24] In the next section I show how the coherentist epistemology of the TMMM handles metrological extension and demonstrate its limits with respect to the risk of systematic error.

4.3.2 Against Coherentism: The Risk of Systemic Error

First I show how the TMMM accounts for metrological extension, then I show that this account comes at a cost: skepticism rooted in the possibility of systematic error. As Tal (2019, p. 862) has it, there are three "necessary and jointly sufficient conditions" on successful calibration between two measurement processes:

> (Common Quantity Assumption) Each measurement process is modeled as measuring the same (kind of) quantity.

[23]See Bridgman (1927), 16.

[24]I cannot deal with foundationalism is full generality here. However, it is noteworthy that a very recent defense of foundationalist empiricism and a critique of "theory-laden measurement" (i.e. the TMMM), Thalos (2023), ends up looking very similar to my extended version of the TMMM. Thalos aims to show how the *certification* of a scale for a quantity can be determined in a theory independent way, while I aim to show that the *validity* of a calibration function can be given support independent of a circle of justification identified by the TMMM. Thalos distinguishes certification from reliability, the latter regarding the magnitude of particular measurands, the former regarding the measurement of a type of quantity (determining the amount of mathematical structure appropriate for representing the quantity)—this parallels nicely my claim that dimensional models allow a validation that a measurement process targets the right dimension of quantity and leaves the fine-grained determination of the function to experimental work. Thalos denies that her account of the certification process requires theory; I find this view implausible and would argue that her argument regarding the conjoint measurement of temperature relies crucially on dimensional models, as outlined in §4.4.3.

(Detection and Elimination of Systematic Error) Measurement outcomes from each process are corrected for systematic errors accounted for in the models.

(Reliability) Measurement outcomes from the two processes converge within model determined accuracy limits.

Tal's claim is that no one of these conditions can be justified independently of the other two. There are two ways of making sense of the epistemic loop structure of calibration according the the TMMM (as distinguished by Isaac 2019): synchronic circles of justification and diachronic cycles of justification (epistemic iteration).[25] In the absence of a foundationalist epistemology of measurement, I argue that the existence of diachronic cycles of justification require us to accept the existence of synchronic circles of justification.

In the calibration of our measurement processes we require standards, quantities of known magnitude, in order to establish the reliability of the procedure. As we do not have direct access to the "true values" of quantities, *ex hypothesi*, we must rely on some other measurement process that is already calibrated to the kind of quantity in question. This generates a regress. Besides our general aversion to regress, this is not a satisfying epistemic model. As Chang has persuasively argued, this process of calibration leads to epistemic *progress*. New measurement processes are developed not only to pragmatically improve upon past processes, but to epistemically improve as well. New processes allow for the detection and elimination of systematic errors in past measurement processes and expand upon their range of measurement. This cannot be made sense of if agreement with past measurement processes remained an epistemic standard after the calibration of new measurement processes.

Avoidance of an epistemic regress without a foundationalist alternative and accounting for epistemic progress call for a synchronic coherentist model of calibration. In this model convergence between old and new measurement processes play a central role, but the measurement indicator of the old measurement process may take on a new (weaker) interpretation in light of systematic errors informed by the

[25] Chang (2007) describes epistemic iteration as a spiral rather than a circle, wih growth in the vertical dimension corresponding to progress.

model of the new measurement process. These systematic errors can only be relevant, however, if the common quantity assumption is made—the two measurement processes are measuring the same quantity, to differing degrees of accuracy. The common quantity assumption would never be made if there were not some correlation between the indications of both measurement processes. Each condition is used in the justification of the other two.

The necessity of the common quantity assumption follows from the possibility of systematic errors:

> To test whether the calibrated and calibrating instruments agree, one must first model both instruments under the assumption that they measure the same specific quantities associated with objects in the calibration sample. Recall that only under this assumption can one assign systematic error corrections and uncertainties to the relevant measurement outcomes, and prior to the evaluation of error and uncertainty there can be no test of agreement. In other words, any claims about agreement and disagreement are conditional on the common quantity assumption, and therefore cannot be viewed as independent evidence for or against it. Testing the common quantity assumption independently of the results of the calibration would require yet another calibration, leading to an infinite regress. (Tal 2019, p. 863)

However, the risk in adopting the common quantity assumption increases in domains where the probability of systematic error increases. The primary case of this sort is one of metrological extension, where an extra inductive step is introduced into the calibration process: if systematic error is not appreciable in the shared range of the mature and novel measurement processes it will not become appreciable in the exclusive range of the novel measurement process. This inductive step is necessary as the sole source of evidence of evidence for systematic errors, discrepancies between measurement indicators, are unavailable.[26] However, it just is the case that there are scale dependent systematic errors (see §4.4). Given that the

[26]"Comparing the quantified indications of different instruments[...] provides evidence for the likely *existence* of systematic errors, but leaves underdetermined the magnitude and distribution of such errors. The quantified indications merely imply that at most one of the thermometers can be deemed accurate without correction, but they do not determine whether any of the instruments are accurate, nor the magnitudes of the corrections required." (Tal 2019, p. 859)

common quantity assumption is in part justified by the assumption of the absence of unaccounted for systematic errors, it cannot be taken to reduce the risk of such systematic errors, even if we accept coherentist circles of reasoning. The common quantity assumption is dialectically inert with respect to the skeptical challenge. This makes it desirable that the TMMM be extended to include an epistemically independent source of evidence for the common quantity assumption or equivalently the absence of systematic errors in the extended range of the novel measurement process.

4.3.3 Extending the TMMM: Nomic Coherence as Dimensional Homogeneity

My modification of the TMMM's model of calibration by grounding the common quantity assumption in dimensional homogeneity has anticipations in Tal's version of the TMMM, which explicitly relies on the common quantity *assumption*. The common quantity assumption has so far been understood to initially be a bold conjecture, which iterative measurement, coordination of the measurement results, and identified sources of error vindicate over time. However, it is never evaluated in isolation—the common quantity assumption, the existence of systematic errors, and the reliability of some measurement process are always evaluated together and adjustments in the face of discrepancies are underdetermined. I have argued above that it should instead be understood as having a source of independent partial justification, dimensional homogeneity.

Here I argue that dimensional homogeneity is an explication of the nomic coherence condition already recognized by Tal:

> In order to individuate quantities across measuring procedures, one has to determine whether the procedures can be *coherently and consistently modeled in terms of the same type of quantity in the background theory*. If the answer is 'yes', then these procedures measure the same type of quantity *relative to those models and the background theory*. (Tal 2019, 872, his emphasis)

One clarification of this model-based account is of particular interest here:

> [T]he phrase 'same type of quantity in the background theory' requires clarification. A precondition for even *testing* whether two procedures provide consistent outcomes is that the outcomes of each instrument are represented in terms of the same theoretical parameter. By 'same theoretical parameter' I mean a parameter that enters into approximately the same nomic relations with other theoretical parameters. This definition is intentionally coherentist: the requirement to model outcomes in terms of the same type of quantity amounts to a weak requirement for nomic coherence among models specified in terms of that type of quantity, rather than to a strong requirement for identity of extension or intension among quantity terms. This nomic coherence among models is what I mean by 'coherently modeled'. (Tal 2019, p. 873)

Tal fails to notice that this nomic coherence condition could provide epistemic support to the common quantity assumption, from *outside* the coherentist circle. That Tal specifies his criterion for being the same theoretical parameter in terms of *nomic* inter-parameter relations gives use a guide to a further explication of this precondition. In the first instance, we can understand "parameters" as quantities, if the laws are to relate quantities (or kinds of quantities),[27] then this constraint amounts to requiring distinct causal measurement process models of the same quantity to obey the same proportionality relations embedded in the laws. For instance, Newton's second law states that a quantity of force (acting on a body) is proportional to a quantity of mass and a quantity of acceleration (of said body). If one model for a measurement procedure of force fails to be responsive to the quantity of mass involved in the force quantity to be measured, it cannot be said to measure force—it is nomically incoherent.

[27]See Mari (2009) on the ambiguity in metrology concerning kinds of quantity. I am primary concerned here with concrete quantities and so will ignore questions about whether kinds of quantities are of a higher order and the exact relationship between kinds like Length and Diameter, etc. However, for some investigations into the (higher-order) metaphysics of quantity kinds see Mundy (1987) and Eddon (2013a); Eddon (2013b).

As it turns out, this sort of nomic coherence has been worked out formally in dimensional analysis. The condition of nomic coherence *qua* necessary condition on quantity identity, is here explicated as a condition of *dimensional homogeneity*. Further, dimensional homogeneity is establishable independently of the specific causal models invoked in the calibration process because the relevant models for dimensional homogeneity are *dimensional models*, which are more coarse-grained: That pressure is dimensionally homogeneous with some product of powers of quantities is independent of the causal connection of those quantities in the design of a physical apparatus, i.e. dimensional models are multiply realizable. Hence there can be independent evidence for the common quantity assumption. While the satisfaction of dimensional homogeneity does not *justify* the common quantity assumption, its violation does justify a rejection of the common quantity assumption.[28]

4.4 Dimensional Analysis in High Pressure Physics

Percy W. Bridgman won the 1946 Nobel prize in physics "for the invention of an apparatus to produce extremely high pressures, and for the discoveries he made therewith in the field of high pressure physics."[29] This section studies some of the work for which he earned this Nobel Prize: the development of an electrical resistance gauge for high pressure physics. Bridgman's extension of the domain of measurable pressures is precisely the sort of metrological extension[30] which requires adoption of the common quantity hypothesis. I will argue that Bridgman used the principle of dimensional homogeneity to provide *epistemic support* to the common quantity *hypothesis*.

[28]Isaac (2019) has defended a form of measurement realism which is committed to the existence of such modally stable "fixed points" that nomic coherence requires. I take dimensional homogeneity to be at least one aspect of such a fixed point realism—though some of Isaac's fixed points, like the relative magnitudes at which phase transitions happen (e.g. boiling and freezing temperatures), involve commitment to structure *beyond* that fixed by dimensional analysis.

[29]From "The Nobel Prize in Physics 1946" (2021).

[30]Chang (2004) describes such extensions as part of a broader class of "semantic extensions" which modify our scientific concepts. Tal (2017a,b) also suggests semantic issues are relevant in such cases. I agree, but I will only focus on epistemic issues, insofar as they can be isolated from semantic issues. However, see Jalloh (2022) for a recent semantical approach to these issues.

We can clearly model Bridgman's successful calibration of a novel secondary gauge for exotic pressures by appeal to the TMMM. Bridgman's experimental work was groundbreaking for at least three reasons which correspond to the three major components of the calibration task:

> (Common Quantity Hypothesis) Bridgman invented an apparatus that would produce *pressures* higher than any that had previously been produced artificially (see Bridgman 1914).
>
> (Detection and Elimination of Systematic Error) Bridgman established the existence of and adopted a measurement process to avoid two sources of systematic error he detected: hysteresis error and compressibility error.
>
> (Reliability) Bridgman established reliable measurement standards for such high pressures.

Clearly all three of these achievements are deeply intertwined, each an aspect of the process of calibration described in the TMMM. In particular, the detection and elimination of systematic errors confirm the common quantity hypothesis and strengthen claims of reliability generated by the minimization of random error and the stability of repeated measurements. The reliability of the measurement standard then gives further reason to believe that the apparatus is *producing* the pressures it claims to measure. This, of course, is circular: we need to assume that high *pressures* are being produced and measured (however faithfully) *before* we can establish and measure errors! I will show below that dimensional analysis provides a method for Bridgman to detect and eliminate some sources of systematic error in the resistance gauge. A confounding quantity which also varies with the pressure, compressibility, is revealed as such by a dimensional model of the resistance gauge.

4.4.1 Primary Gauges, Secondary Gauges, and Hysteresis Error

Manometry is the art of measuring pressure. I will follow the typology of manometers or pressure gauges given in Bridgman's *High Pressure Physics* (1949), in order to reproduce the logical structure of Bridgman's achievements in high pressure physics. The vast degree of complexity, both in apparatus design and in inferential structure (the latter is discussed a bit in §4.4.3), means that my sketch of how manometry works and how pressure gauges are designed will be grossly simplified—my intention here is to simply describe enough of the detail to make my point with respect to the epistemic role of dimensional analysis in this area of experimental physics.[31]

In Bridgman's account of the measurement of pressures, he invokes a distinction between primary pressure gauges and secondary pressure gauges:

> Pressure gauges may be conveniently classified into primary gauges—that is, gauges so constructed that the absolute pressure can be at once approximately found from the construction of the instrument itself; and secondary gauges, the readings of which can be interpreted into absolute pressure only after a proper calibration. (Bridgman 1949, p. 60)

This is something like the distinction between direct and indirect (or fundamental and derived) measurement made in the philosophical literature (e.g. Ellis 1968; Kyburg 1984): a quantity is directly measured if the measurement process does not involve the measurement of different kinds of quantities. A canonical example of a quantity that admits of direct measurement is length—one directly measures the length of an object by in terms of the length of some other object.[32] Bridgman recognizes that there is no primary gauge of pressure "in the strict sense". I do not want to here attend to the possibility or even the necessity of direct measurement, the distinction between primary and secondary gauges is here *relational*—we are dealing with contextually determined *degrees* of mediation. Secondary gauges are calibrated to the measurement outcomes of primary gauges. The primary gauges are treated as epistemically privileged.[33]

[31]See Sterrett (2023) for an account of the epistemic role of dimensional analysis in fluid mechanics.
[32]For Bridgman (1931), direct measureability is a requirement for "primary" or "fundamental" quantities.
[33]In this context Bridgman will sometimes refer to the primary gauge as the "absolute gauge", see e.g. Bridgman (1909b), 232.

Of the primary pressure gauges, Bridgman singles out an open column of mercury as the most basic and earliest in use. Here I give a toy dimensional model of such a simple primary gauge. The height of mercury in an open column will correlate with a source pressure according to the manometer equation:

$$P = h\rho g,$$

where h is the height of the mercury in the tube, P is the pressure, ρ is the density of mercury (or whatever fluid is used), and g is the gravitational acceleration. The height of mercury (i.e. its maximal displacement from a zero-point) is an indicator which, via this modeling equation, yields a measurement of pressure.

With this model of the measurement process we can consider the role of dimensional homogeneity in the coordination of a theoretical and a experimental definition of pressure—this will serve as a model for thinking about the role of dimensional homogeneity in calibrating different experimental methods. We take pressure to be defined as a measure of force on some surface:

$$P_{Def} = \frac{F_{\perp}}{A}.$$

The dimensions of pressure are also given by this equation:

$$[P_{Def}] = \frac{[F]}{[A]} = \frac{\mathrm{M}}{\mathrm{LT}^2}.$$

If we are given a manometer for the first time, how is it that we know that the displacement of the mercury column is an indicator of the pressure acting on the mercury? Setting aside the derivation, we can check that P_{Def} and the P as calculated from the manometer equation are commensurate by inspection. The dimensional equation for the "pressure" as determined by a manometer is:

$$[P] = [h][\rho][g].$$

The dimensions of the constituent quantities are: $[h] = \text{L}$; $[\rho] = \frac{\text{M}}{\text{L}^3}$; $[g] = \frac{\text{L}}{\text{T}^2}$. So then, $[P] = \frac{\text{M}}{\text{LT}^2}$ and $[P] = [P_{Def}]$. The equivalence of dimension alone does not guarantee the equivalence of theoretical pressure and manometer pressure. Physical reasoning is necessary to relate the variables in the equation to their physical counterparts and so will be the same for extended uses of pressure in domains where normal manometers break down.

As the pressures of interest get higher—above 1000 kg/cm^2—a free piston gauge is required as a primary gauge. A free piston gauge simply is a piston exposed to the pressure to be measured on one end. The distance that the piston slides out of its cylinder (generally with weight attached for high pressures) correlates to the pressure. Various modified designs exist with aims to minimize leak between the piston and its cylinder, strengthen buckle points, increase range and accuracy, and so on—a leak proof design for pressure transfer points was crucial for Bridgman's achieving Nobel-worthy pressures.[34] With further increases in pressure, practical constraints, like size limitations, make it such that free pistons are no longer useful.

There are several secondary gauges that can be adopted for pressure; Here I contrast two notable classes of secondary gauges, elastic deformation gauges and electrical resistance gauges. I will take the Bourdon spring as illustrative of the class of elastic deformation gauges. Bridgman describes the Bourdon spring as "the most common" and "one of the most convenient" secondary gauges (1949, p. 68). The mechanism of the gauge is a spring, made from a tube of metal bent into a circular arc—as the fluid pressure inside the tube increases, the spring unwinds. Usually the end of the tube is connected to a dial-pointer from which one can read off the pressure. The accuracy and validity of the secondary gauge is to be determined by the reproducibility of its results measured against the primary gauge.

The problem with the Bourdon spring gauge in particular, and with elastic deformation gauges in general, is that it suffers from hysteresis errors, which increase with the pressure. Hysteresis is a type

[34] For a bit more on Bridgman's unsupported area seal and general influence in high pressure physics see Hemley (2010).

of systematic error in which there is a discrepancy in the measurement of a quantity when the same magnitude is reached by varying a system up to some value (i.e. loading up) vs varying a system down (i.e. loading down) to a value.

> It seems to be a fact[...] that any elastic deformation gauge becomes unsuitable at high pressures, even when once calibrated, because of the entrance of hysteresis effects. It is true that the existence of elastic hysteresis effects has frequently been doubted, and it has even been stated that proof of their existence would give us knowledge of a new elastic property. It nevertheless seems to be a fact that hysteresis may be inappreciable at low values of the stress but become increasingly important at higher pressures. (Bridgman 1909b, p. 221)

This means that a curve describing the increase in the deflection of the Bourdon spring with the increase in pressure will differ from the curve describing their mutual decrease from some maximum pressure. The higher the maximum, the greater the discrepancy. Hysteresis effects show up in many different areas of science and have a variety of causes; in this case the cause is in the elastic properties of the Bourdon spring. Bridgman's development of the electrical resistance gauge, first invented by Lisell, was directly motivated by the systematic hysteresis errors that plague elastic deformation gauges at high pressures.

4.4.2 Bridgman's Resistance Gauges and Compressibility Error

Bridgman's high pressure experimental work appeared in the early days of molecular (and then quantum) models of matter. His development of the electrical resistance gauge for pressure both exploited and tested one such model: Born's model of conductivity.

Central to understanding the relation between pressure and resistance is to distinguish (observed) resistance and resistivity, an intrinsic property of materials. Resistance, R, is defined by Ohm's law in terms of voltage, V, and current, I:

$$R = \frac{V}{I}.$$

Given that $[V] = \mathrm{ML^2T^{-2}Q^{-1}}$ and $[I] = \mathrm{QT^{-1}}$, where Q is the basic dimension of charge,[35] $[R] = \mathrm{ML^2T^{-1}Q^{-2}}$. Resistance is a property of a particular sample and is dependent on the geometry of the sample (usually a wire), both the length of the sample, l, and the cross-sectional area through which the current flows, A, are involved in the relation between resistance and resistivity, or "specific resistance", ρ, an intrinsic property of the substance from which various samples may be made:

$$\rho = \frac{RA}{l}.$$

The resistivity is the reciprocal of the conductivity of the substance, σ, the former can be understood as the substance's tendency to impede current flow and the latter can be understood as the substance's tendency to allow current flow:

$$\sigma = \frac{1}{\rho}.$$

As will be seen, the primary effect of pressure on resistance is through its effect on resistivity—secondary effects on the geometric dimensions of the sample are systematic sources of error.

To understand the intrinsic effect of pressure on *resistivity* more needs to be said of the atomic model of crystalline solids and the corresponding theory of conductivity in metals.[36] The basic, classical model is something like the following: the fundamental parts of matter, atoms, consist of positively charged nuclei and orbiting, negatively charged electrons. In crystalline solids, these atoms form lattices that balance the attractive forces between positive nuclei and negative electrons in adjacent atoms and the repulsion of like charges in adjacent atoms. In metals, some higher-energy electrons are free to flow throughout the lattice and are, in a sense, shared by all of the atoms—in this way they are similar to a gas suffusing the crystal lattice. In the presence of an external electric field electrons flow, but, as with a gas, they have some

[35]The current convention in SI is to have the basic electrical dimension be that of amps, i.e. $[I] = \mathrm{A}$. I here adopt the older convention of having charge be the basic electrical dimension, as it is more illustrative.

[36]This discussion is greatly simplified and is primarily based on Mott and Jones (1936), particularly chapters 3 and 7, to which I refer the reader for further detail. For more on the historical development of the classical and quantum theories of solids, see Hoddeson et al. (1992), chapters 1 and 2.

probability of scattering. In this case the scattering is off of imperfections in the lattice, most fundamentally due to thermal oscillations (statistical fluctuations) of the atoms. This scattering is the basic mechanism of resistance—a perfect lattice has no resistance. The mean free time, the time between collisions, of an electron in a metal exposed to an external field is 2τ, where τ, is known as the time of relaxation. The mean velocity of electron drift is $\bar{v} = eF\tau/m$, where e is the electron charge, F is the intensity of the field, and m is the mass of the electron. This generates a new expression for the conductivity, which depends on the number of electrons, N:

$$\sigma = \frac{Ne^2\tau}{m}.$$

The conductivity is also often described in terms of the mean free path between collisions, $l = 2\tau\bar{v}$:

$$\sigma = \frac{Ne^2 l}{2m\bar{v}}.$$

We see from this that qualitatively the resistivity of a metal depends inversely on the length of the mean free path:

$$\frac{1}{\rho} \propto l.$$

This already reveals interest in the empirical fact that resistance *increases* with pressure—Intuitively the inverse relation ought to hold: one would intuit that the effect of pressure is to *decrease* the mean free path of electrons as the lattice is compressed.[37] The explanation of the observed decrease in the resistance of metals under high pressures, an anomaly for the classical model, depends on a quantum mechanical model of conductivity. Bridgman's high pressure experiments used exotic phenomena to guide new physics—the viability of a reliable resistance gauge was by no means guaranteed by theory.

The quantum model of conductivity is much the same as the classical gas model, but the scattering mechanism for resistance must be understood probabilistically—I set aside questions of a mechanism

[37]See e.g. Bridgman (1917), 640.

here.[38] Most important is the definition of the relaxation time as approximately the inverse of the probability per unit time of a collision:[39]

$$\tau \approx \bar{P}^{-1}.$$

I will not go into the varying derivations of this probability through wave function models of the electron in a lattice; I will note, however, that this probability (or rather its root) is usually referred to as the scattering amplitude.[40] The new equation for conductivity can be expressed in several ways and, under certain conditions, approximates the classical formula; however, the most relevant form for our ensuing discussion involves the effective scattering area, A, and the volume per atom, Ω_0:

$$\sigma = \frac{Ne^2\Omega_0}{mvA}.$$

Quantum mechanical aspects of this model are packed into the A factor—the effective area being determined by the probability of an electron being scattered with an allowed solid angle range.

With our theoretical understanding of the conduction and resistance of solids, we can now come to understand how pressure *decreases* resistance. In the quantum model of conduction in crystalline solids high pressure causes the atomic lattice to be bound tighter, with stronger forces of cohesion; this, in turn, reduces the amplitude of atomic vibrations, to which resistance is proportional. We should note that Bridgman's first resistance gauge used liquid mercury, but in the end he returned to using a manganin wire (an alloy of copper, manganese, and nickel), as first used by Lisell, neither of these being crystalline elements. However, the results for solid normal metals given by the crystal model described above extends

[38] Bridgman developed a gap theory over the course of several papers (1917, 1921, 1922b): electrons pass through atomic centers and scatter off of the interatomic gaps, the shrinking of the gaps leads to decreased resistance. Zwicky (1927) gives a model closer to the contemporary account (which is closer to the classical gas model, electrons scatter off of atomic centers) with a useful comparison to Bridgman's account.

[39] I give the relation as an approximation as there is a factor that depends on the angle of the scattering—the aim of this treatment here is mere a qualitative descriptive sufficient to make the sense of the dimensional reasoning in Bridgman's experimental work.

[40] Note that this, $\bar{P}$, is distinct from, but directly related to, the amplitude of atomic vibrations that Bridgman and others often speak of.

both to liquid metals and to alloys as used by Bridgman, though there are some complications. Further, a full theoretical account (even of crystals) was not available to Bridgman in the early phases of his work, though he worked to develop a theoretical model according to the constraints provided by his results (Bridgman 1917, 1921, 1922b).

In the absence of established new physics, Bridgman had to develop his resistance gauge on a phenomenological basis. There are exceptions,[41] but generally "the high melting, mechanically hard, strongly metallic elements" all share a monotonic decrease (nearly linear) in resistance with pressure (Bridgman 1949, p. 261):

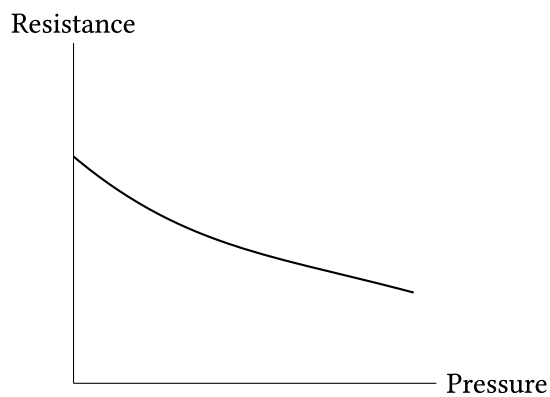

Figure 4.1: Recreation of Bridgman's qualitative graph of falling resistance curves.

The function described by the above graph is the result of some empirical, purely numerical equations the Bridgman uses to model his data. These equations are empirical in that (1) they are abstractions of the data (i.e. bottom-up models) and (2) they "have no theoretical value", about which I will say more in the next two sections. These equations are purely numerical in that the variables in them cannot be understood as *physical* quantities with dimensions—on pain of violating dimensional homogeneity. The interpretation of the equations is constrained to the context of the experimental apparatus and they ought not be used outside of that context.[42] In his early work, Bridgman establishes empirical, numerical equations between

[41] See Lawson (1956) for a nice distinction between "normal metals" the different kinds of exceptions.

[42] Such unhomogenous equations are common in empirical work and engineering: "[T]he reader should be warned that many empirical formulas in the engineering literature, arising primarily from correlations of data, are dimensionally inconsistent[. . .]

a *dimensionless* ratio $\hat{\rho} = \frac{\Delta R}{R_0}$, the ratio between a change in the resistance and the original resistance (and not to be confused with resistivity), and, some approximately linear function of pressure, p (the form of the function being a matter of guess work and curve-fitting to a supposed power law). Bridgman settles on two equations:

$$\hat{\rho} = ap10^{bp^c}$$

and

$$p = \alpha\rho 10^{\beta\hat{\rho}^\gamma},$$

where a, b, c, α, β, and γ are all determined by fitting the resultant curves to the experimental data.[43] Bridgman makes it clear that these equations are only to be understood as *numerical*, empirical equations:

> The above formulas are only empirical representations of the facts throughout a given pressure range, and their use by extrapolation over any considerably greater range is doubtful. No theoretical value is claimed for them, and it is evident that they cannot represent the actual form of the unknown function. (Bridgman 1909b, p. 240)

Evidently dimensional analysis plays no role in Bridgman's establishment of a dimensionally unhomogenous *empirical correlation* between pressure and the resistance of mercury; I make no argument for that here. Dimensional analysis plays the role of establishing and eliminating a source of systematic error that constrained Bridgman's early work to such dimensionally unhomogeneous equations.

Bridgman outlines what needs to be done to establish theoretical significance:

> The formulas given above connect the change of resistance of mercury in a capillary of specified glass with the pressure, and are all that is required for use with a secondary standard of

though [dimensionally inconsistent equations] occur in engineering practice, [they] are misleading and vague and even dangerous, in the sense that they are often misused outside their range of applicability." (White 2015, p. 13)

[43] The simplification of uniting these two equations into a single ratio between the two quantities produces massive errors, see Bridgman (1909b), 240.

> pressure. The observed change of resistance, however, is due to a combination of two unrelated effects; the change of dimensions [volume] of the glass, and the changed specific resistance of mercury. The results given above will not possess theoretical value, therefore, until the two effects are separated. In the following an experimental determination of these two effects is given. (Bridgman 1909b, p. 244)

When Bridgman says that the "results above" lack theoretical significance, he is referring to the fact that empirical equations which establish the correlation of indicator readings of a secondary gauge to that of a primary gauge is not enough to establish a valid, projectible calibration function prior to the elimination of a major source of systematic error—here the variable compression of the mercury container. It is necessary to distinguish the effect on the observed change of resistance due to the changed specific resistance of mercury and the confounding effect due to the compression of the container. While they are both correlated with the change in the pressure, only one is supposed to be a reliable indicator of high pressures—the second order volume effect is subject to significant affection by the temperature.

Bridgman's corrects for the confounding effect of the variable compression of the material with pressure from the direct effect of pressure on resistivity by distinguishing further resistance quantities:

> We may distinguish two specific resistances of mercury, both of which are altered by pressure. The first may be called the specific volume resistance, and is the resistance of a body of mercury of invariable form, but of mass variable with the pressure. The second may be called the specific mass resistance of mercury, and is the specific volume resistance multiplied by the ratio of the masses within the given surface at the variable and standard pressure, i.e., the density. The specific mass resistance seeks to correct for the increased conductivity to be expected at any pressure because of the increased number of conducting particles in a given volume. (Bridgman 1909b, p. 244)

Specific volume resistance, or volume resistivity, is the intrinsic resistance of the metal per unit volume, as defined above as the simple “resistivity”. Specific mass resistance or mass resistivity, is the volume resistivity weighted by the change in density accounted for by the compression of the sample of metal. Bridgman drops this terminology in future work and describes the situation in a way more easily understood in light of the discussion above:

> The change of resistance due to the changed electrical properties of the mercury may be further divided into two effects: that due to the change in the conducting power of the separate molecules, and that due to the change in the number of molecules occupying a given space. This latter effect is determined directly by the compressibility of the mercury. (Bridgman 1909a, p. 255)

So there are two causal pathways from increases in pressure to decreases in resistance that Bridgman distinguishes: a change of the intrinsic resistivity by changes in A, the dynamics of conduction, and a change in the number of particles in a given volume through compression, which decreases Ω_0, the volume per atom, but also modifies the dynamics, A. The net effect of compression is an *increase* in resistance, but this is outweighed by the main effect on the dynamics of conduction, as represented by A.[44] This confounding effect can be accounted for by determining the variable compressibility of the relevant material. The resistivity of a metal may serve as a gauge for the pressure on that sample of metal, however the inference to the pressure-resistivity calibration function is confounded by the compressibility, which varies with the pressure but also affects the observable resistance. By correcting for the variable compressibility of a sample, Bridgman was able to establish a *projectible* pressure-resistivity function from observations of the empirical pressure-resistance correlation.

Now there is the question of why Bridgman felt the need to distinguish these two effects: Wouldn’t a bare correlation of observed resistance and pressure serve to calibrate a gauge? Both reasons come back

[44] See e.g. Bridgman (1917), 644.

to Bridgman's notion of "theoretical value"—nearly all of his experimental papers close with a section on "theoretical considerations". One reason is relatively direct, as mentioned above, the relation between resistance and pressure in metals was anomalous for the classical gas theory of conduction, a precise determination of the factors responsible for the effect would be necessary for the development of a microphysical theory adequate to the phenomena. The other reason also falls under Bridgman's notion of "theoretical value": the projectibility of the pressure-resistance calibration function. Sources of systematic error, possible causes of deviation from the pressure-resistance function, must be identified and corrected for in order to assure the projectibility of this function for metals that are not in the initial calibration set. As described in §4.4.3, theoretical significance in the case of projectibility means the physical interpretability of the empirical results: a dimensional model is the first step in objectifying the function between pressure and resistance. It is when Bridgman attempts to bring his experimental data to bear on theory, and theory to bear on his data, that he appeals to a dimensional model to establish the validity of his calibrations.

Now onto the Bridgman's dimensional argument.[45] Let compressibility hereafter be designated by k, with dimensions $\mathrm{M}^{-1}\mathrm{L}\mathrm{T}^{2}$—the inverse of the dimensions of pressure, $\mathrm{M}\mathrm{L}^{-1}\mathrm{T}^{-2}$. Compressibility is defined as the ratio of change in a volume to total volume per pressure applied to the body (at a constant temperature); it is reciprocal to the "bulk modulus":

$$k = -\frac{1}{V_0}\left(\frac{\partial V}{\partial P}\right)_{\Theta}.$$

As $\partial V/V_0$ is dimensionless, the dimensions of compressibility are reciprocal to those of pressure. Changes in the compressibility with the pressure can induce further changes to the observed resistance of a metal through the mechanisms described above, i.e. by a generalization of Born's model of crystal dynamics:

[45] A similar but much more concise dimensional argument regarding compressibility (in a different context, viscosity measurements) can be found in Bridgman (1926b), 66-67. A dimensional argument regarding compressibility gets carried into even later work, though it is not discussed in detail, see Bridgman (1949), 166-8. It is noteworthy that "dimensional relations of compressibility" gets an entry in the index.

> By far the most successful theoretical attempt to account numerically for the compressibility of solid substances is that which Born has developed and applied to crystals of the type of NaCl and also to CaF_2 and ZnS[...] Naturally the first inquiry of an attempt to extend this theory to include metals is whether the fundamental thesis still holds, namely that the forces are essentially electrostatic in nature and are due to single elementary charges or small integral multiples of them situated at the centers of the atoms. *A dimensional argument as to the order of magnitude of the quantities involved suggests that the same fundamental thesis does indeed hold.* A quantity of the dimensions of compressibility [($M^{-1}LT^{2}$)] is to be built up from the electronic charge e (dimensions of e^2 are [ML^3T^{-2}]) and δ, the distance of separation of atomic centers (L). The required combination is at once found to be δ^4/e^2. *The very fact that it is possible to build up a combination of these two quantities of the right dimensions is presumptive evidence of the correctness of our general considerations, because in general it would require three (instead of two) quantities to give in combination the dimensions of any one arbitrarily given quantity.* This dimensional argument suggests, therefore, that compressibility should be of the order of magnitude of δ^4/e^2. (Bridgman 1923, 222–223, my emphasis)[46]

The mechanical dimensions of charge used, $[e^2] = ML^3T^{-2}$, is given by the joint requirements of dimensional homogeneity and the form of Coulomb's Law in a dimensional system corresponding to an "absolute" system of units like the Gaussian or Heavyside-Lorentz systems, wherein the Coulomb's constant is dimensionless (and so dropped from the equation):

$$F = \frac{q_1 q_2}{r^2}.$$

[46] A note on my corrections of the dimensions of compressibility and charge: Bridgman here states the dimensions of k to be $M^{-1}LT^{-2}$. However, elsewhere in the text he correctly identifies the dimensions of compressibility to be the inverse of those of pressure, so I assume this is a typo, though it persists in the collected volume version of the paper as well. Further, the stated dimensions for e^2, MLT^{-2}, do not make sense given Coulomb's law nor do they make sense for Bridgman's dimensional argument as given. The coherency of my interpretation of his dimensional argument ought to show these corrections to be well placed.

If $q_1 = q_2 = e$ then the dimensional equation for the charge becomes:

$$[e^2] = [F][r^2] = (\mathrm{MLT}^{-2})(\mathrm{L}^2).$$

Since atomic distances are a given in the systems under discussion, Bridgman deduces that the compressibility must be calculated in terms of the distances between atoms and the electric charges that determine the balance of attractive and repulsive forces:

$$\frac{[\delta^4]}{[e^2]} = \frac{\mathrm{L}^4}{\mathrm{ML}^3\mathrm{T}^{-2}} = \mathrm{M}^{-1}\mathrm{LT}^2 = [k].$$

So we find that changes in the compressibility of a metal correspond with changes in the volume, which distorts the independent relationship between pressure and resistance (resistivity), given a Born model of conductivity in metals. This finding is supported by the commensurability of compressibility with electrostatic quantities that describe characteristic properties of a crystal.

The role of dimensional homogeneity played in Bridgman's experimental work was as a method for identifying systematic sources of error—additional pressure effects—allowing for the calibration of a secondary resistance gauge even in the absence of a primary gauge with which to compare it and without well developed physical theory for the exotic domain. This case serves as an existence proof for my claim that dimensional homogeneity can provide evidence for the common quantity hypothesis, independently of the circle of justification which is so central to past TMMMs.[47]

[47] I should note further that this is not to say that the external validation of a calibration function by dimensional models is fool-proof. Dimensional homogeneity is only a *necessary* condition on quantity identity and may fail to aid in establishing a calibration function when certain scale dependent phenomena intervene on the quantities of interest, e.g. phase transitions.

4.4.3 Experimental Physics and Dimensional Models

Consideration of the role of dimensional analysis in Bridgman's experimental work allows the development of our understanding of how dimensional models mediate between theory and experiment. One idea this work may disabuse us of is the idea that dimensional analysis can obviate the need for experimental work. While this may seem a silly idea, the exuberance of early development of dimensional analysis and the realization of its powers led to unguarded expressions of this cavalier attitude, by no less a physicist than Lord Rayleigh:

> I have often been impressed by the scanty attention paid even by original workers in physics to the great principle of similitude [dimensional analysis].[48] It happens not infrequently that results in the form of 'laws' are put forward as novelties on the basis of elaborate experiments, which might have been predicted a priori after a few minutes' consideration. (Rayleigh 1915, p. 66)

A temperate skepticism of such apriorism motivated Bridgman's investigation of dimensional analysis:

> As it stands the book [*Dimensional Analysis* (1922a, 1931)] is mainly a record of a personal experience which I went through when starting my career in physics. Hersey, inspired by you, had given us some lectures on Dimensions, and was so enthusiastic about the possibilities and power of the method that he was quite willing to believe that all the results of my program of high pressure measurement might be anticipated by arm-chair meditation. If any such catastrophe were possible it evidently was important for me to know about it before tying up my life in so futile an enterprise. Of course I did not for a moment really believe that any such clairvoyance was possible, but I found that I could not argue about the thing convincingly[. . .]

[48]This is not "the principle of similitude" as espoused by Tolman (1914a). Rayleigh means it only as a synonym for dimensional analysis, as is common usage today.

> In thinking the situation through, I naturally came across many of the misconceptions of which the literature is full. (Bridgman to Buckingham, July 10, 1932)[49]

One limitation of dimensional analysis that Bridgman's high pressure experiments make clear is the generality of its models. Dimensional models at best give lawlike proportionality relations between quantities that describe a system; they do not determine magnitudes or causal pathways. This is not to say that dimensional models are therefore useless, on the contrary their generality is the source of their explanatory power (see Lange 2009; Pexton 2014). Experiment is needed to fill in the details of dimensional models, but these dimensional models can serve as guides for experiment. As shown above, dimensional models allow for the determination of sources of systematic error and suggest methods for the estimation of their magnitudes. We can therefore make a distinction between two sorts of models relevant to experimental work:

- Causal models of the measurement process, which allow for the generation of data and statistical error corrections (as evident in Bridgman's initial empirical model of the resistance-pressure correlation);
- Dimensional models of the measurement process, which determine proportionality relations between the physical quantities which may be involved in a number of distinct casual realization of the measurement process.

This makes clear the sense of Bridgman's notion of "theoretical value": causal models alone cannot tell us about lawful relations in the world, because they are too bound to a particular causal pathway. The more coarse-grained and abstract dimensional models are needed to project evident quantitative correlations beyond a particular causal realization of a measurement process.

[49] This can be found both in the Percy Bridgman Papers at Harvard (HUG4234.10, Box 1) and the Edgar Buckingham Papers in the Niels Bohr Library (AR238, Box 4, Folder 37).

I argue that while dimensional analysis did not play an *a priori* role of predicting the results of Bridgman's experiments, it played a role—often implicit—in the planning of experiments and identifying sources of error—and so determining a lawful, nomologically coherent relation between pressure and resistance. It is in the mediation of experiment and theory, modeling, that dimensional reasoning makes its appearance. In closing, I will briefly present some evidence that Bridgman, though skeptical of Rayleigh's exuberance, recognized the significance of dimensional analysis for his work and for physical experiment in general.

That dimensional analysis was significant for Bridgman's experimental work is evident in the testimony of some of Bridgman's former students:

> It did not take him long to discover in the technique of dimensional analysis an essential theoretical device needed in the planning of his experimental program. His success in eliminating what he regarded as metaphysical obscurities in that theory was the lure which eventually launched him on a career of philosophical analysis. (Kemble and Birch 1970, p. 23)

Ultimately, Bridgman's explanation of the inclusion of a paper on dimensional analysis in his collected experimental papers provides direct evidence that he came to see dimensional analysis as significant to his experimental work. In the introduction to the seven volume collection of his experimental papers Bridgman writes:

> Nearly all these papers deal with some aspect of high-pressure phenomena, but there are a few others. The decision was not always easy as to whether a paper should be included in this collection or not. The decision not to include was easy for a number of papers which would be described as relating to "philosophy of science," but there were a number of others in which the contact with experiment is much closer. With two exceptions, the criterion for inclusion was finally taken to be whether the paper involved any immediate experimental work on my part[...] *The two exceptions noted above are a paper of 1926 (No. 61) dealing with*

> *Dimensional Analysis, which is included because of the important applications of Dimensional Analysis in experimenting with models*, and a review paper of 1946 (No. 151), included because of the survey it gives of the whole high-pressure field. (Bridgman 1964, xxv, my emphasis)

Whatever the limitations of my analysis above, it is incontrovertible that dimensional analysis played some significant role in Bridgman's experimental practice—I have argued here that it served as a guide for Bridgman in the metrological extension of pressure.

4.5 Conclusion

This paper has provided a partial solution for a central problem of the epistemology of measurement: the task of calibration. As explained above, a major account, both descriptive and normative, of measurement, the theory of model mediated measurement, is open to skeptical challenges due to its coherentist epistemology. Without completely abandoning coherentism, I show that the TMMM can be extended to provide an independent partial ground for the common quantity assumption—that two models or a modeled measurement process and the target of its realization are designed to measure the same quantity. The acceptance of the common quantity assumption, or hypothesis, as it turns out, is necessary in order to posit and detect systematic errors responsible for discrepancies between measurement procedures. The principle of dimensional homogeneity provides a necessary condition for the identification of quantities and explicates a standard of nomic coherence left indeterminate in existant TMMMs. An important example of calibration in the extension of measurement scales, Bridgman's experiments in high pressure physics, serve as historical evidence that the principle of dimensional homogeneity can play the provide a necessary condition for the projectibility of a calibration function—serving as a partial justification for the validity of a metrological extension.

Chapter 5

Conclusion

While this dissertation has not been written in the style of a monograph with mutually dependent chapters but rather as a collection of relatively independent essays, there are significant and important themes that emerge from their consideration as a whole. In this conclusion I will sketch out some of those themes and indicate some directions for future work. As indicated by the title of the dissertation, these persistent themes can be split into two classes: metaphysical and epistemic.

Regarding the epistemic theme which may be abstracted from this work: Dimensional analysis is particular system of modeling physical systems, and, as a modeling practice, it has a mix of *a priori* and *a posteriori* elements that are hard to disentangle. While this is discussed most explicitly in chapter 4, debates regarding the conventionality of dimensional analysis in chapter 2 turn on the validity of considering dimensional analysis as a logic. Much may be done to fully integrate dimensional analysis with the contemporary philosophy of models. In particular, the relationship between dimensional models and other sorts of models is here underdeveloped. In chapter 4, I contrast dimensional models of types of measurement processes with causal models of particular measurement processes, i.e. particular ways of realizing a process that uses one quantity as an indicator (proxy) for the other. While this distinction is intuitive enough, it is not totally clear what the relation between dimensional models and causation is: dimensional models contain counterfactual—and possibily counternomic—information (see Pexton 2014), meaning that they may serve as the ground of causal inferences (at least on a relatively standard counterfactual account of causation). As said earlier, however, it is clear that this is a more coarse-grained degree of causal information than can be gathered from a causal model of e.g. a particular design for a pressure gauge, with representations of anvils, buckle points, etc. Further, it is also clear that these dimensional models cannot have any such substantive counterfactual content and be purely *a priori*. In distinguishing dimensional analysis as a logical method in a *methodological* sense from dimensional analysis as a logical method in an *epistemic sense* in §2.1.2 I've taken the first step towards an account of our dimensional systems as an instance of what Friedman (2001) calls the "relative *a priori*". On this view, scientific theories essentially involve constitutive principles which underlie the coordination of the terms in the theory with objects of experience. These constitutive principles enjoy a certain degree of immunity to empirical disconfirmation, as their assumption is required to empirically evaluate the claims of the theory in the first place. However, as described for dimensional systems in §2.1.2, this is only a *relative a prioricity*. Therefore, there are situations—somewhat exceptional, but not necessarily Kuhnian revolutions—under which these constitutive principles in general and our dimensional systems in particular, may change.[1] While I've given a preliminary exposition of the conditions under which such changes may by called for (§2.3.1), such situations are not well understood and have yet to be connected to related and important issues regarding intertheoretic reduction.

This brings me to the metaphysical theme which underlies the entire dissertation: modality. A full understanding of dimensional analysis requires that it be embedded in a metaphysical system incoporating laws, constants, and properties. As comes out in my discussions of the dimensional constants (§3.5) the level of the fineness of grain of nomological necessity is not yet clarified. Since last century, the "received view" of theories lead to a conception of natural laws as first-order universal conditionals, with many

[1]See Miller (2014) for a historical informed case study of such a sub-Kuhnian scientific revolution.

variations on this model. It is by no means clear that the mathematical form of physical laws can be accomodated by such a syntactic representation; however, the "semantic", model-theoretic representations of scientific theories, as n-tuples of essential set-theoretic objects (sets and functions) does not make apparent certain central features of the *quantitative* structure of physical theories either. In particular, the distinction between variables and constants has been under theorized, and this has lead to an ambiguity in what we take the laws to be.[2] It is often said that laws need to be unit invariant (see Grozier 2020), but this leaves open the possibility of another variation in the laws. If the *form* of the laws is what is so invariant, the magnitudes of the quantities involved may be related by quantity symmetries, as outlined in chapter 3. These symmetries preserve dimensional structure, the relative magnitudes of quantities across dimensions, but they do not preserve "absolute" magnitudes, particularly those of the constants. Constant necessitism, the nomological necessity of the magnitudes of the constants, is a perfectly comprehensible view, and though not often made explicit, seems to me to be the default view among philosophers of science and metaphysicians. The question of its truth is closely related to the question of whether or not dimensional structure is a modal structure identical to or more coarse-grained than that of the laws. If these modalities come apart, the nomological and the dimensional, then there remains an issue of relative fundamentality (which I broach in §2.3.2): Do the symmetric dependency relations between quantity dimensions constrain the forms of the laws or do the laws fix the possible dependency relations between quantity dimensions? One path to settling this issue will be to work out the non-fundamentalist dimensional realism I've developed here and connecting it to the relevant literature in the metaphysics of science.[3]

Surely there are many other general considerations raised by this dissertation and there are a lot of paths along which my suggestions can be developed. My hope is that some of this work should provide an impetus to further work, and that at the end of inquiry lies a comprehensive view of the world and our knowledge of it

[2]Some salutary efforts at this, from a model-theoretic perspective, have recently been taken by Martens (In Press).
[3]Particularly the literature on nomic essentialism, see Berenstain (2016), Sider (2020), and Wang (2016).

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