Calibrating The Theory of Model Mediated Measurement

Metrological Extension, Dimensional Analysis, and High Pressure Physics

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Abstract

I argue that dimensional analysis provides an answer to a skeptical challenge to the theory of model mediated measurement. The problem arises when considering the task of calibrating a novel measurement procedure, with greater range, to the results of a prior measurement procedure. The skeptical worry is that the agreement of the novel and prior 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 we *assume* that there is a common quantity. In contrast, I show that the satisfaction of dimensional homogeneity across the metrological extension is independent evidence for the so-called assumption. This is illustrated by the use of dimensional analysis in high pressure experiments. This results in an extension of the theory of model mediated measurement, in which a common quantity in metrological extension is no longer assumed, but hypothesized.

Keywords: dimensions, measurement, coherentism, calibration, epistemic grounding, systematic error

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1 Introduction

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 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, internal body temperature?[1]

I take it that *a* 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

[1]This is more general and philosophically significant than the special case in which we have two measurement devices of the same kind (e.g. two oral thermometers) and we wish to establish that they have the same calibration function.

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. On the other hand, 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.

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.[2] 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.[3] 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 to a

[2] As noted by Tal, there is a persistent type-token ambiguity in discussion of this issue—evident 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.

[3] 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; Franklin 1997; Soler 2015; Feest 2016; Zhao 2023).

phenomenon) 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 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, has been justified by reference to problems like 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 of theory, models, calibration, and observation, the entire program may have gotten detached from the truth.

I argue here that the principle of dimensional homogeneity, that the quantitative equality of two quantities requires that they are of the same dimension, an external check of the coherentist circles of justification generated by specific models of measurement processes. In a toy model I show dimensional homogeneity to be able to lend direct support to the common quantity assumption. In a more subtle and complicated case, I show dimensional homogeneity to provide independent evidence of a source of systematic error. Consideration of this case will show an actual use of dimensional analysis 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. 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 coherentist circle endorsed by defenders of the TMMM. My modification to the TMMM ameliorates a skeptical problem for a coherentist epistemology of measurement that arises from the possibility of systematic error in metrological extension.

2 The Theory of Model Mediated Measurement

Recently, philosophers of science have increasingly focused on the role of *models* in scientific inquiry.[4] 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 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

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

> approximately and why they depart from the laws in the way they do. (Cohen 1985, 169)[5]

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 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 confirmed by data. 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 distinguishes 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".[6] 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

[5] 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).

[6] 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, 12)

measurement indications:[7] 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 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 Woodward 1988, 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 Woodward 1988, 309)

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*, which I am here specifying as *model mediation* in the way explicated above. 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. Let me begin by listing some theses that characterize a TMMM:[8]

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

[8]I am *not* attempting to give necessary and sufficient conditions for a TMMM. This is merely a loose framework.

(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).[9] *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 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

[9]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.

(measurement indications) and maps to objective quantities (measurement outcomes) which are unit-independent—this is fundamental to dimensional analysis. 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".[10] If a model fails to be projectible, the agreement of the model mediated measurement outcomes with a class of standards is due to coincidence.[11]

The TMMM faces a skeptical problem. This problem arises from the possibility of systematic errors of unknown form and magnitude.[12] 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; making this assumption is a necessary condition on the possibility of calibration.[13] Like other transcendental posits, it is vulnerable

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

[11] 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.

[12] 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.

[13] 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

to skepticism: generally speaking transcendental arguments only determine how things must seem to us, not how they must *be* (see Stroud 1968).

While the common quantity assumption *is* essential to calibration, we may hope for more. I argue that in advance of measurement—in some cases—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. 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, 862) I argue that there can be an external necessary condition for the validity of the common quantity hypothesis: dimensional homogeneity. Dimensional analysis teaches us that physical quantities have properties of great significance, their dimensions. Dimensional 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

keep quantity concepts unified and background theories simple." (2019, 875)

[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).

which such identifications are established or rejected are independent of the specific causal models that correspond to each measurement process is argued in §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.

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. An important 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 (Collins 1985, 2016; Boyd 2021), hard problems of coordination (Ohnesorge 2022), 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 aspects of the same circularity problem which appears to be an impediment to a central epistemic task of measurement: calibration. Calibration is a core task of the experimentalist: the experimentalist is to determine 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

[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.

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]

(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—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

[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, 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, but 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.

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]

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

Bridgman's operationalism, a 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

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

[20] Similar discussions of the failure of operationalism to properly account for the epistemology of measurement can be found in Chang (2004). See Mari 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, 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, 265).

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 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, where 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.

This problem generalizes: Insofar as foundationalist epistemologies bottom out in self-justifying propositions, those propositions will be incapable of error and inapt for accuracy claims. An externalist or reliabilist account of such foundationalist epistemologies of measure-

[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(2021b, 2021a); Jalloh (2022).

[23]See Bridgman (1927), 16.

ment are baldly question-begging—we are taking the (existence and form of the) calibration function to be a something in need of justification. More generally, the degree to which measurement outcomes inherit the infallibility of their ultimate grounds is the degree to which that foundationalist or quasi-foundationalist epistemology fails to meet the descriptive criterion—operationalism is the special case in which some measurement outcomes *just are* the ultimate grounds, so it maximally fails this criterion. Of course I do not aim to give an total refutation of foundationalist epistemologies of measurement here, at most I can characterize a scheme for the sorts of arguments that have led to the current consensus.

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, 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.
>
> (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).[24] In the absence of a foundationalist

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

epistemology of measurement, I argue, on the basis of an epistemic regress, that the existence of diachronic cycles of justification require us to accept the existence of synchronic circles of justification—This means that the coherentist alternative results in true circular reasoning and not merely "spiral" reasoning, free of a true *petitio principii*, as Chang (1995) argues.

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.

Even "progressive" iterative cycles of epistemic iteration are liable to regress arguments, so the TMMM must ultimately hold that a synchronic coherentist circle must lie at the bottom of every program of measurement. Generally speaking, coherentism is the way out of regress in the absence of foundations. In this model convergence between old and new measurement processes plays 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 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:[25]

> 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, 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 only sources of evidence for systematic errors, discrepancies between measurement indicators, are unavailable.[26] However, it just is the

[25] Throughout "systematic errors" should be understood as effective systematic errors, where an effective systematic error is a systematic error that is above the magnitude of the precision of our instruments. I thank a referee for making it clear to me that elmination of all systematic errors is too big an ask for calibration and it is only the elimination of effective systematic errors that is a reasonable goal for calibration.

[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, 859)

case that there are scale dependent systematic errors (see §4.1). Given that the 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.

I here amend the TMMM's coherentist model of calibration. In order to specify the alternative model, *grounded-coherentism*, I will model coherentism in terms of epistemic ground. This will explain the motivation—shared by the foundationalist and the coherentist—of avoiding epistemic regress and make clear how this view is a middle position between foundationalism and full coherentism.[27]

In modeling my epistemic position in terms of ground I am following a trend in recent epistemology of borrowing from technical developments in metaphysics. The grounding relation appears to have several uses in metaphysics including: articulating physicalism, defending monism, and unifying dependence relations.[28] I will *not* be using the metaphysical notion here, but I will appeal to an analogous *epistemic* notion of grounding. That epistemic regress is a *problem* of epistemological models of measurement is due to a requirement that epistemic justification meets the criteria of a grounding relation.[29] The formal conditions for an epistemic grounding relation that we will be concerned with are the following:

(Transitivity) If Q grounds R and P grounds Q, then P grounds R.

(Asymmetry) If P grounds Q, Q cannot ground P.

[27] I am not alone in seeing the appeal of a middle ground position (e.g. Haack 1993).

[28] See Bliss and Trogdon (2021) for a survey.

[29] See Siscoe (2022) for an argument for this conceptualization of epistemic regress problems and an argument as to why past candidate epistemic relations, particularly "basing", are not at issue.

These two principles also hold for partial grounding, upon which we will model justification. Partial grounding is the relation that holds between P and Q iff P and some other proposition (or set of propositions) γ ground Q. This maps well onto the sense of justification that applies to, e.g., partial confirmation, as opposed to the full justification that figures in Gettier-style analyses of knowledge.

I will take it that the epistemic justification of relevance to the epistemology of measurement is such a relation of partial ground—the *prima facie* objection to the coherentism of the TMMM is that it violates asymmetry by the example of chains of reasoning of the following form:

> (Coherentist Circle) For any proposition P in a coherent set Γ there is at least one $Q \in \Gamma$ and some subsets $\gamma, \gamma', \ldots$, such that $P\&\gamma$ ground Q and $Q\&\gamma'$ ground P. By transitivity of partial ground P, partially grounds P.[30]

Violations of asymmetry mean that skepticism about one node in the calibration model, particularly the common quantity assumption, can generate a general skepticism regarding the validity of calibration altogether. We ought to distinguish two possible objections here: (1) violations of asymmetry make it impossible for the coherentist to have an account of justification that can be modeled as partial ground; (2) symmetric justification means symmetric doubt, making doubts to any part of the coherentist's belief set doubts regarding

[30] See Berker (2015) for graph models of coherentism which can be adapted to this ground model of justification. Berker argues that there are two structural forms a coherentism can take. A linear coherentist is committed to there being a flow of justification between beliefs which ultimately form a circle. A holistic coherentist rejects the existence of justifications for individual beliefs and holds that rather only "relations of support" exist between beliefs—justification is made wholesale. Berker and others hold that a holist coherentism is more defensible than a linear one. While defenders of a coherentist TMMM have called it a version of "holism", it is not clear to me that they have been sensitive to this distinction between coherentisms. When justification is understood in terms of partial ground as opposed to full ground ("links of support" vs "links of justification" in Berker's language), it is not clear to me that the TMMM admits of a meaningful distinction between these sorts of holism. It can be shown that circles of *partial* ground can still be generated by a holist coherentism that eliminates circles of *full* ground—so if Berker means to claim that the holist coherentist can escape circles of justification as the term is used in the sciences, he is wrong. If he means by "justification" the only "success term" used by epistemologists, he is right on this point.

the total belief set. Obviously (1) is question-begging against the coherentist: asymmetry is a modeling assumption thus far only justified by a foundationalist intuition. (2) is more serious: It seems that the joint confirmation of the coherentist's belief set implies the joint *disconfirmation* of that set. That is to say: a skeptical and significant doubt regarding the absence of systematic errors (as is the case in metrological extension) threatens to undermine a calibration under the TMMM to an extent it might not have been able to if it could somehow be compartmentalized. What is needed is a position that does not concern itself with the question-begging constraint given by objection (1) but is responsive to the real worry expressed in objection (2).

What I offer is a grounded-coherentism which allows for circles of partial justification, only if there is some justification coming from without the circle. Dimensional analysis can provide a release valve for this pressure by moderating the coherentism of the TMMM, while preserving the coherentist commitment to symmetric justification, by weakening the conditions for justification being a species of grounding relation.[31] The principle of dimensional homogeneity can provide a partial ground for the common quantity assumption that lies outside the coherentist circle. This means adopting a grounded-coherentist view that conforms justification to the following principle instead of asymmetry (making a concession to the foundationalist):

> (Docked Asymmetry) If P partially grounds Q, Q can partially ground P only if for the entire coherent set $P, Q, \cdots \in \Gamma$, there is some distinct (non-overlapping) coherent set Ω (or proposition ω) which asymmetrically partially grounds some subset of Γ, γ, such that $\gamma \& Q$ partially ground P.[32]

[31] This is a claim that epistemic ground has a different structure than standard metaphysical ground; however, there are similar metaphysical "foundherentisms" on offer (e.g. Dixon 2023).

[32] Again, by transitivity we recover reflexivity. The coherentist's grounding criteria must abandon the irreflexivity of partial ground regardless. However, the coherentist can preserve the irreflexivity of complete ground given the docked asymmetry condition.

This is to say, asymmetry can be violated only if there is some proposition (or set of propositions) outside the coherent set that partially grounds some member of the coherent set. The external proposition(s) serve as a dock to which the coherent set, oft compared to Neurath's boat, may be tethered, giving a truly independent check on the coherent set. In other words, the grounded-coherentist's circles must have spokes that connect to independent (sets of) propositions. In this case of the calibration circle, one such dock is dimensional homogeneity. Docked Asymmetry does *not* say, however, that there is not some larger coherentist model in which dimensional homogeneity figures—one can be coherentist all the way out, as it were. One can be foundationalist on the outskirts for that matter. However, in this local context, we can partially approximate a foundationalist picture in a coherentist framework, thereby remaining consistent with scientific practice and our normative intuitions.

3.3 Extending the TMMM: Nomic Coherence as Dimensional Homogeneity

My grounded-coherentism and its 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 will argue that it should instead be understood as sometimes having a source of independent partial justification (conforming to the docked-asymmetry model above), dimensional homogeneity. However, sciences which lack a dimensional system, or areas of physical science which are not amenable to dimensional modeling, will need to appeal to other sources for a move beyond the TMMM account of calibration.

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, 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, 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.

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 *fully justify* the common quantity assumption, its violation does so justify a rejection of the common quantity assumption.[33] I should also note that this is not a reversion to a foundationalism but is a (locally) grounded-coherentism, as dimensional models are similar to calibration models in their dependence on theory etc. This move is intended to give the TMMM more robustness against a particular sort of skepticism, that the magnitudes we ascribe to quantities are inaccurate due to systematic errors. The *general* skeptical problem for coherentism is beyond our scope.

[33]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.

4 Calibration 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."[34] Bridgman's extension of the domain of measurable pressures is precisely the sort of metrological extension[35] which requires adoption of the common quantity hypothesis. I will argue that Bridgman used the principle of dimensional homogeneity to provide *independent epistemic support* to the common quantity *hypothesis* in his establishment of an electrical resistance gauge for high pressure.

We can 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

[34] From "The Nobel Prize in Physics 1946" (n.d.).

[35] Chang (2004) describes such extensions as part of a broader class of "semantic extensions" which modify our scientific concepts. Tal (2017a, 2017b) 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.

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 confirmed to be such by a dimensional model, which is independent of the causal model of the electrical resistance gauge.

4.1 Pressure 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. My sketch of how manometry works will be grossly simplified—my intention is just to describe enough of the detail to make my point with respect to the epistemic role of dimensional analysis in this area of experimental physics.[36]

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, 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

[36]See Sterrett (2023) for an account of the epistemic role of dimensional analysis in fluid mechanics.

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.[37] Bridgman recognizes that there is no primary gauge of pressure "in the strict sense". For my purposes, the distinction between primary and secondary gauges is taken to be *relational*—we are dealing with *degrees* of mediation. Secondary gauges are calibrated to the measurement outcomes of primary gauges. Primary gauges are treated as epistemically privileged.[38]

Of the primary pressure gauges, Bridgman singles out an open column of mercury as the most basic and earliest in use. 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 is an indicator which, via this modeling equation, yields a measurement of pressure.

The role of dimensional homogeneity in the coordination of a theoretical definition and an experimental operationalization of pressure will serve as a model for the role of dimensional homogeneity in calibrating different *experimental* operationalizations. We take pressure to be defined as a measure of force on some surface:

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

[37]In Bridgman (1931), direct measureability is a requirement for "primary" or "fundamental" quantities.

[38]In this context Bridgman will sometimes refer to the primary gauge as the "absolute gauge", see e.g. Bridgman (1909b), 232.

The dimensions of pressure are also given by this equation:

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

where $[F] = \frac{\mathrm{ML}}{\mathrm{T}^2}$ and $[A] = \mathrm{L}^2$.[39] 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: $[h] = \mathrm{L}$; $[\rho] = \frac{\mathrm{M}}{\mathrm{L}^3}$; $[g] = \frac{\mathrm{L}}{\mathrm{T}^2}$. So then, $[P] = \frac{\mathrm{M}}{\mathrm{LT}^2}$ and $[P] = [P_{Def}]$. The equivalence of dimension alone does not guarantee the identity of theoretical pressure and manometer pressure. Further physical reasoning is necessary to relate the variables in the equations to their physical counterparts.[40]

Onto secondary gauges: Bridgman describes the Bourdon spring as "the most common" and "one of the most convenient" secondary gauges (1949, 68). The problem with the Bourdon spring gauge—a general problem for elastic deformation gauges—is hysteresis error, which increases with the pressure. Hysteresis is a type 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 to a value (i.e. loading down).

> 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

[39] Normal italicized variables represent quantities. Unitalicized vairables represent "basic" quantity dimensions. Square brackets indicate "dimensions of", i.e. they are functions from quantities to their dimensions. Throughout I assume a simple mechanical dimensional system in which the dimensions of all quantities are products of powers of the basic dimensions mass, length, and time.

[40] For more on Bridgman's innovations and influence in high pressure physics see Hemley (2010).

> 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, 221)

Bridgman's development of the electrical resistance gauge—first invented by Lisell—was directly motivated by the prevalence of such errors at high pressures.

4.2 Compressibility Error and the Crystal Lattice Model

Bridgman's high pressure experimental work appeared in the early days of 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 from resistivity, an intrinsic property of materials. Resistance, R, is defined by Ohm's law in terms of voltage, V, and current, I:

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

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 samples of various resistances may be made:

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

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. 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.[41] The classical model is something like the following: an atom consist of negatively charged electrons that orbit a positively charged nucleus. In crystalline solids, these atoms form lattices that balance the attractive forces between positive nuclei and negative electrons and the repulsion of like charges. 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 probability of scattering. The scattering is due to imperfections in the lattice, most fundamentally thermal oscillations 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

[41] This discussion is greatly simplified and is primarily based on Mott and Jones (1936), particularly chapters 3 and 7. For more on the historical development of the classical and quantum theories of solids, see Hoddeson et al. (1992), chapters 1 and 2.

of the mean free path:

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

This already reveals interest in the empirical fact that resistance *decreases* with pressure—According to the classical model of conduction, the inverse relation ought to hold: that the effect of pressure is to *decrease* the mean free path of electrons as the lattice is compressed, thereby *increasing* the resistance.[42] The explanation of the observed decrease in the resistance of metals under high pressures, an anomaly for the classical model, depends on a quantum 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.

In the quantum model of conductivity the scattering mechanism for resistance must be understood probabilistically—I set aside questions of a detailed mechanism here.[43] Most important is the definition of the relaxation time as approximately the inverse of the probability per unit time of a collision:[44]

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

I will not go into the varying derivations of this probability through wave function models of the electron in a lattice. The new equation for conductivity can be expressed in several ways; however, the most relevant form for our ensuing discussion involves the effective scattering area, A, and the volume per atom, Ω_0:

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

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

[43] Bridgman developed a gap theory over the course of several papers (1917, 1921, 1922): 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 with a useful comparison to Bridgman's account.

[44] I give the relation as an approximation as there is an additional factor that depends on the angle of the scattering—this should suffice for our purposes.

Quantum mechanical aspects of this model are packed into the A factor. 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.[45]

In the absence of established new physics, Bridgman had to develop his resistance gauge on a phenomenological basis. There are exceptions,[46] but generally "the high melting, mechanically hard, strongly metallic elements" all share a monotonic (and nearly linear) decrease in resistance with pressure which is reproduced in Figure 1 below (original in Bridgman 1949, 261):

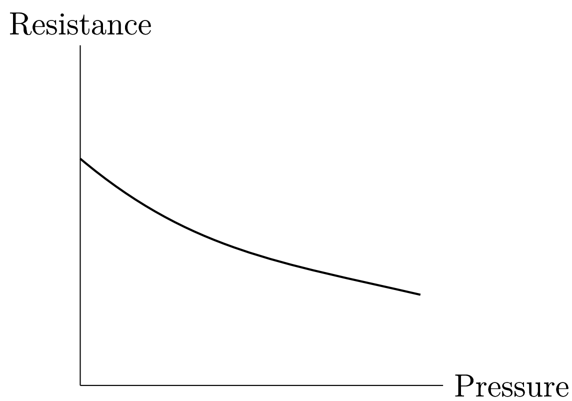

Figure 1: Recreation of Bridgman's qualitative graph of falling resistance curves.

The function described by the above graph (Figure 1) 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

[45]One might question the applicability of the crystalline model to Bridgman's experiemental apparatus, given that he used liquid metals like mercury or alloys like manganin, with imperfect crystals. Such deviations from the paradigm do not effect the general results regarding resistivity, though there are some complications.

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

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, as the validity of these equations are dependent on the causal structure of the particular measurement apparatus.[47] In his early work, Bridgman establishes empirical, numerical equations between 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.[48] 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, 240)

Evidently dimensional analysis plays no role in Bridgman's establishment of a dimensionally

[47] 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[. . .] 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, 13)

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

unhomogenous *empirical correlation* between pressure and the resistance of mercury; I make no argument for that here. Dimensional analysis plays the role of identifying and eliminating a source of systematic error that constrained Bridgman's early work to such dimensionally unhomogeneous equations. The empirical-mathematical equations lack intrinsic theoretical significance (and so fall short of establishing a true pressure gauge) due to a failure of projectibility: the calibration function resulting from experiment and curve-fitting is dependent on the units and both the causal model and causal specifics of the apparatus used. Of these two sources of the failure of projectibility, the brute statistical model and the causal model of the particular apparatus, it is the later which is fundamental and which is to be contrasted with dimensional models in what follows.

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 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 [resistivity] 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, 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 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 corrects for this confounding effect by distinguishing the effect on observed resistance due to resistivity and the effect due to compressibility (as we did above):[49]

> 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, 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, *resistance-decreasing*, effect on the dynamics of conduction, i.e. resistivity.[50] This confounding effect can be accounted for by determining the variable compressibility of the relevant material.

There remains 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 to Bridgman's notion of "theoretical value". One reason is relatively direct: to test and develop microphysical models of conductivity, by a precise determination of the factors responsible for the effect of pressure on resistance. Another reason, more central to our concerns here: to secure 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

[49]Bridgman (1909b, 244) has an early and confusing distinction between specific volume resistance and specific mass resistance which approximates the distinction here. He drops this terminology afterwards, so I will ignore this historical complication.

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

function. Theoretical significance is in this case the physical interpretability of the empirical results: a dimensional model aids 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 evidence the validity of his calibrations.

4.3 Dimensional Models and Their Epistemic Independence

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, 23)

This is further corroborated by Bridgman's explanation of the inclusion of a paper on dimensional analysis in his collected experimental papers. In the introduction to the seven volume collection of his experimental papers Bridgman writes:

> 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[...] *[one of the] two exceptions noted above [is] 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*[...] (Bridgman 1964, xxv, my emphasis)

The work of this section will be to show *how* dimensional analysis figured into Bridgman's experimental work, and, further, that this usage provided independent support to the common quantity assumption in his metrological extension of pressure in a manner according to the epistemological analysis provided in §3.2 and §3.3.

Fitting into the pattern of the TMMM, compressibility error, proposed to explain the deviation from linearity of the pressure-resistance curve at high temperatures, became a subject of study for Bridgman—using the very sort of resistance pressure gauge that estimates of compressibility would then be able to correct.[51] Work on compressibilities prior to Bridgman's 1923 paper involved known inaccuracies. Bridgman's innovative experimental design which eliminated these inaccuracies was predicated on (to use terminology from Smith 2014; Smith and Seth 2020) taking Born's crystal lattice model as true of the metals in question:

> Recent theoretical work, in particular that of Born is now bringing within the reach of the possibility of computation the compressibility of substances in terms of their crystalline structure. Born's theory of the compressibility of substances of the type of sodium chloride is far enough advanced to give an expression for the variation of compressibility with pressure. It seems therefore that the time is ripe for a more careful experimental examination of the question of the compressibility of the metals, although we may not have as yet a satisfactory theory of the metallic state itself. (Bridgman 1923, 166)

Provisional adoption of Born's model allowed Bridgman to adopt a method that would test the model's ability to "close the loop" (another Smithism) by predicting compressibility magnitudes that would explain away the errors in the observed pressure resistance curve, providing further justification for the calibration of the electrical resistance gauge. The

[51] Generally, the metal in the resistance gauge and the metal whose compressibility was being measured would differ. Further, attempts were made to minimize the compression of the metal in the resistance measuring wire. Additionally, corrections for any compression in the measuring wire were made available by earlier experiments. These details should not matter for what follows.

assumption of a lattice structure means that Bridgman can measure linear compressibility as a proxy for volumetric compressibility as a whole.

Why bring dimensional analysis into this? It seems that one can take this as a straightforward case of epistemic iteration with the aim of coherence. Bridgman observed a discrepancy in his results (against theory), used theory to identify an error, and made further measurements to correct for this error.[52] One trivial reason to bring up dimensional analysis is because Bridgman does and made it clear that he thought that dimensional analysis was *important* to such experimental work. We must account for the role of dimensional analysis in Bridgman's compressibility measurements in order to satisfy our descriptive criterion. Another reason comes from the normative criterion: If Bridgman thought dimensional analysis helped his experimental work, how did it do so? By looking more closely at an explicit dimensional argument Bridgman makes in the 1923 paper, I will show that dimensional analysis provided independent corroboration that the lattice model fit with his results. This lends support to a version of the common quantity hypothesis: the measured quantity is of the same sort as that of the target, theoretical quantity.

Now onto Bridgman's dimensional argument.[53] 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):

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

Changes in the compressibility with the pressure can induce further changes to the observed resistance of a metal through the mechanisms described in §4.2:

[52] I thank a pair of reviewers for pressing this point.

[53] A similar but much more concise dimensional argument regarding compressibility (in a different context, viscosity measurements) can be found in Bridgman (1926), 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 $[(\mathrm{M}^{-1}\mathrm{LT}^{2})]$ is to be built up from the electronic charge e (dimensions of e^2 are $[\mathrm{ML}^3\mathrm{T}^{-2}]$) 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, 222–23, my emphasis)[54]

The mechanical dimensions of charge used, $[e^2] = \mathrm{ML}^3\mathrm{T}^{-2}$, 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 (e.g. Gaussian), wherein the Coulomb's constant

[54]A note on my corrections of the dimensions of compressibility and charge: Bridgman here states the dimensions of k to be $\mathrm{M}^{-1}\mathrm{LT}^{-2}$. 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.

is dimensionless (and dropped from the equation):

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

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 and electric charges are given quantities in the lattice model, Bridgman shows that the a quantity with the dimensions of compressibility may be calculated in terms of these given quantities:

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

So we find that changes in the compressibility of a metal correspond with changes in the volume (a positive power of δ), which distorts the independent relationship between pressure and resistance (resistivity), given a Born model of conductivity in metals.

In this dimensional derivation there are three threads to pull apart; the first two are part of the independent confirmatory power of dimensional models and the third is one often lumped into that independent confirmatory power but has a more complicated (and for our purposes, largely irrelevant) confirmatory power that *is* part of the coherentist TMMM circle. First is the fact that a quantity of the dimensions of compressibility is derivable, which is not guaranteed with only two given quantities, as Bridgman notes.[55] Second is that this derived quantity has the observed correlation with the experimental variable, the atomic distances (which correlates with the length of the total sample). Third is that the estimated magnitudes of this quantity match (roughly) the measured magnitudes—this aspect is external to the dimensional model itself and so will not be taken as part of its role as *independent*

[55] This can be shown to be a simple matter of linear algebra when the vector representation of dimensional systems is adopted. For more details and deep, insightful, and careful analysis see Jalloh (Forthcoming).

confirmation.[56] The agreement in dimension and in functional relation to the atomic distances of this derived compressibility quantity is clearly confirming evidence that the Born model explains the compressibility error and, ultimately, that the electrical resistance gauge can be calibrated by correcting for this source of systematic error. My more controversial claim is that this evidence is *independent of details of the lattice or gauge models*. This is because the dimensional model is coarse-grained; it only relies on a quantity of dimension L and a quantity of dimension $M^{-1}LT^2$ being sufficient to characterize any causal model of the phenomena. The details of the particular quantities involved or their causal roles are irrelevant to the dimensional model. Dimensional models therefore are models that are independent of the sorts of models that tend to figure in model-mediated-measurements, causal models of measurement processes. If this is right, the Bridgman calibration case serves as an existence proof for my claim that dimensional homogeneity—the fundamental principle of dimensional derivations—can provide evidence for the common quantity hypothesis, independently of the circle of justification which is so central to past TMMMs.[57]

Let me say more about this distinction between dimensional models and causal 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 a limit of dimensional analysis but also a source of its strength. This means that the results of dimensional analysis are independent of the details of causal interactions which constitute the models of measurement processes.[58] I call causal models all physical models which *do* depend on the causal details that are abstracted away in dimensional models. This includes

[56]A wide discrepancy between the magnitude of a quantity estimated by a dimensional model (augmented by known magnitudes of other quanitites) and a calculated or observed model indicates a problem with the causal model of the measurement process.

[57]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.

[58]For general arguements that the generality of dimensional models is the source of their explanatory power see Lange (2009); Pexton (2014).

most of what we think of as physical models and even mathematical statistical models (see §4.2), which do rely on causal assumptions. 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.

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 (that of the measurement apparatus). The more coarse-grained and abstract dimensional models are needed to project evident quantitative correlations beyond a particular realization of a measurement process. Further, as claimed above, these models are independent of the coherentist circle of justification in the standard TMMM, therefore Bridgman's calibration of the electric resistance gauge satisfies the modified TMMM characterised by the docked asymmetry condition in §3.3.[59] The three elements of the usual coherentist circle are (1) the common quantity assumption, (2) the elimination of (effective) systematic errors, (3) agreement within precision and domain constraints with a prior accepted measurement procedure. The truth conditions of all three of these elements rely on the identification of a particular quantity—(1) and (2) in its causal role and (3) in its magnitude—all of these aspects are ignored in a dimensional model, so the dimensional model must be independent of propositions whose truth value it does not determine and is not determined by.

[59]For further evidence that this is relevant to the calibration of the electrical gauge, consider: "On electron theories of metals, it will pay to emphasize two points. The first is that the coefficients tabulated are the actual observed coefficients, measured by the ordinary methods with electrodes permanently fixed to determinate parts of the surface. But in theoretical discussion we are more inclined to be interested in the variation of specific resistance. To get this, the observed results must be corrected by a factor equal to the change of linear dimensions. It is easy to see that for normal metals the temperature coefficient of observed resistance is numerically smaller than the temperature coefficient of specific resistance by the linear thermal dilatation, and the pressure coefficient of observed resistance is numerically less than the pressure coefficient of specific resistance by the linear compressibility." (Bridgman 1917, 637) Bridgman goes on to discuss the effect of the "correction" for compressibility.

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 elimination of systematic errors in turn lends support for the common quantity hypothesis. Both the common quantity hypothesis and the absence of systematic errors in a properly calibrated measurement process receive independent support from the principle of dimensional homogeneity. 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 experimental work in high pressure physics, serves as historical evidence that the principle of dimensional homogeneity can provide a necessary condition for the projectibility of a calibration function—serving as a partial, independent justification for the validity of a metrological extension.

References

Baigrie, Brian S. 1987. "Kepler's Laws of Planetary Motion, Before and After Newton's Principia: An Essay on the Transformation of Scientific Problems." *Studies in History and Philosophy of Science Part A* 18 (2): 177–208. https://doi.org/10.1016/0039-3681(87)90017-3.

Berker, Selim. 2015. "Coherentism via Graphs." *Philosophical Issues* 25 (1): 322–52. https://doi.org/10.1111/phis.12052.

Bliss, Ricki, and Kelly Trogdon. 2021. "Metaphysical Grounding." In *The Stanford Encyclopedia of Philosophy*, edited by Edward N. Zalta, Winter 2021. Metaphysics Research Lab, Stanford University. https://plato.stanford.edu/archives/win2021/entries/grounding/.

Bogen, James, and James Woodward. 1988. "Saving the Phenomena." *The Philosophical Review* 97 (3): 303. https://doi.org/10.2307/2185445.

Boyd, Nora Mills. 2018. "Evidence Enriched." *Philosophy of Science* 85 (3): 403–21. https://doi.org/10.1086/697747.

———. 2021. *Epistemology of Experimental Physics*. Cambridge Elements. Cambridge: Cambridge University Press.

Bridgman, Percy W. 1909a. "An Experimental Determination of Certain Compressibilities." *Proceedings of the American Academy of Arts and Sciences* 44 (10): 255–79. https://doi.org/10.2307/20022428.

———. 1909b. "The Measurement of High Hydrostatic Pressure. II. A Secondary Mercury Resistance Gauge." *Proceedings of the American Academy of Arts and Sciences* 44 (9): 221–51. https://doi.org/10.2307/20022424.

———. 1914. "The Technique of High Pressure Experimenting." *Proceedings of the American Academy of Arts and Sciences* 49 (11): 627–43. https://doi.org/10.2307/20025490.

———. 1917. "The Electrical Resistance of Metals Under Pressure." *Proceedings of the American Academy of Arts and Sciences* 52 (9): 573–646. https://doi.org/10.2307/20025699.

———. 1921. "The Electrical Resistance of Metals." *Physical Review* 17 (2): 161–94. https://doi.org/10.1103/PhysRev.17.161.

———. 1922. "The Electron Theory of Metals in the Light of New Experimental Data." *Physical Review* 19 (2): 114–34. https://doi.org/10.1103/PhysRev.19.114.

———. 1923. "The Compressibility of Thirty Metals as a Function of Pressure and Temperature." *Proceedings of the American Academy of Arts and Sciences* 58 (5): 165–242. https://doi.org/10.2307/20025987.

———. 1926. "The Effect of Pressure on the Viscosity of Forty-Three Pure Liquids." *Proceedings of the American Academy of Arts and Sciences* 61 (3): 57–99. https://doi.org/10.2307/20026138.

———. 1927. *The Logic of Modern Physics*. New York: The Macmillan Company.

———. 1931. *Dimensional Analysis*. Revised. New Haven: Yale University Press.

———. 1938. "Operational Analysis." *Philosophy of Science* 5 (2): 114–31. https://doi.org/10.1086/286496.

———. 1949. *The Physics of High Presure*. New Impresion with Supplement. London: G. Bell and Sons.

———. 1950. "The Operational Aspect of Meaning." *Synthese* 8 (1): 251–59. https://doi.org/10.1007/BF00485912.

———. 1964. *Collected Experimental Papers. Volume I Papers 1-11 Volume I Papers 1-11*. https://doi.org/10.4159/harvard.9780674287792.

Chang, Hasok. 1995. "Circularity and Reliability in Measurement." *Perspectives on Science* 3 (2): 153–72. https://doi.org/10.1162/posc_a_00479.

———. 2004. *Inventing Temperature: Measurement and Scientific Progress*. Oxford: Oxford University Press.

———. 2007. "Scientific Progress: Beyond Foundationalism and Coherentism." *Royal Institute of Philosophy Supplements* 61 (September):1–20. https://doi.org/10.1017/S1358246100009723.

———. 2017. "Operationalism: Old Lessons and New Challenges." In *Reasoning in Measurement*, edited by Nicola Mössner and Alfred Nordmann, 25–38. History and Philospohy of Technoscience 9. London; New York: Routledge.

Cohen, I. Bernhard. 1985. *Revolution in Science*. Cambridge, Mass.: Belknap Press.

Collins, Harry M. 1985. *Changing Order: Replication and Induction in Scientific Practice*. London ; Beverly Hills: Sage Publications.

———. 2016. "Reproducibility of Experiments: Experimenters' Regress, Statistical Uncertainty Principle, and the Replication Imperative." In *Reproducibility*, 65–82. John Wiley & Sons, Ltd. https://doi.org/10.1002/9781118865064.ch4.

De Clark, Sybil Gertrude. 2017. "Qualitative Vs Quantitative Conceptions of Homogeneity in Nineteenth Century Dimensional Analysis." *Annals of Science* 74 (4): 299–325. https://doi.org/10.1080/00033790.2017.1372519.

de Courtenay, Nadine. 2015. "The Double Interpretation of the Equations of Physics and the Quest for Common Meanings." In *Standardization in Measurement: Philosophical, Historical and Sociological Issues*, edited by Oliver Schlaudt and Lara Huber, 53–68. London; Brookfield: Pickering & Chatto.

Dixon, Scott. 2023. "Metaphysical Foundherentism." *Synthese* 201 (3): 86. https://doi.org/10.1007/s11229-023-04053-1.

Ellis, Brian. 1968. *Basic Concepts of Measurement*. Cambridge University Press. https://books.google.com/books?id=LsQVmwEACAAJ.

Feest, Uljana. 2016. "The Experimenters' Regress Reconsidered: Replication, Tacit Knowledge, and the Dynamics of Knowledge Generation." *Studies in History and Philosophy of Science Part A* 58 (August):34–45. https://doi.org/10.1016/j.shpsa.2016.04.003.

Fourier, J. 1878. *The Analytic Theory of Heat*. Translated by A Freeman. London: Cambridge University Press.

Franklin, Allan. 1997. "Calibration." *Perspectives on Science* 5 (1): 31–80. https://doi.org/10.1162/posc_a_00518.

Haack, Susan. 1993. *Evidence and Inquiry: Towards Reconstruction in Epistemology*. Reprinted. Oxford: Blackwell.

Hemley, Russell J. 2010. "Percy W. Bridgman's Second Century." *High Pressure Research* 30 (4): 581–619. https://doi.org/10.1080/08957959.2010.538974.

Hoddeson, Lillian, Ernest Braun, Jürgen Teichmann, and Spencer Weart, eds. 1992. *Out of the Crystal Maze: Chapters from the History of Solid State Physics*. New York: Oxford University Press.

Isaac, Alistair M. C. 2019. "Epistemic Loops and Measurement Realism." *Philosophy of Science* 86 (5): 930–41. https://doi.org/10.1086/705476.

———. 2022. "Escape from Zanzibar: The Epistemic Value of Precision in Measurement." *Philosophy of Science*, June, 1–14. https://doi.org/10.1017/psa.2022.67.

Jalloh, Mahmoud. 2022. "Scientific Polysemy, Semantic Detoxification, and Sophisticated Operationalism." http://philsci-archive.pitt.edu/id/eprint/20230.

———. Forthcoming. "Metaphysics and Convention in Dimensional Analysis, 1914-1917." *HOPOS: The Journal of the International Society for the History of Philosophy of Science*, Forthcoming.

Kemble, Edwin C., and Francis Birch. 1970. "Percy Williams Bridgman 1882-1961." *National Academy of Sciences*, 21–67.

Koch, Sigmund. 1992. "Psychology's Bridgman Vs Bridgman's Bridgman: An Essay in Reconstruction." *Theory & Psychology* 2 (3): 261–90. https://doi.org/10.1177/0959354392023002.

Kyburg, Henry Ely. 1984. *Theory and Measurement*. Cambridge Studies in Philosophy. Cambridge; New York: Cambridge University Press.

Lange, Marc. 2009. "Dimensional Explanations." *Noûs* 43 (3): 742–75. https://doi.org/10.1111/j.1468-0068.2009.00726.x.

Lawson, A. W. 1956. "The Effect of Hydrostatic Pressure on the Electrical Resistivity of Metals." *Progress in Metal Physics* 6 (January):1–44. https://doi.org/10.1016/0502-

8205(56)90003-X.

Mari, Luca, Paolo Carbone, Alessandro Giordani, and Dario Petri. 2017. “A Structural Interpretation of Measurement and Some Related Epistemological Issues.” *Studies in History and Philosophy of Science Part A* 65–66 (October):46–56. https://doi.org/10.1016/j.shpsa.2017.08.001.

Mari, Luca, Mark Wilson, and Andrew Maul. 2021. *Measurement Across the Sciences: Developing a Shared Concept System for Measurement*. Springer Series in Measurement Science and Technology. Cham: Springer International Publishing. https://doi.org/10.1007/978-3-030-65558-7.

Mitchell, Daniel Jon. 2017. “Making Sense of Absolute Measurement: James Clerk Maxwell, William Thomson, Fleeming Jenkin, and the Invention of the Dimensional Formula.” *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics* 58 (May):63–79. https://doi.org/gr6p4c.

Morgan, Mary S., and Margaret Morrison, eds. 1999. *Models as Mediators: Perspectives on Natural and Social Sciences*. Ideas in Context. Cambridge ; New York: Cambridge University Press.

Morrison, Margaret. 1999. “Models as Autonomous Agents.” In *Models as Mediators: Perspectives on Natural and Social Sciences*, edited by Mary S. Morgan and Margaret Morrison, 38–65. Ideas in Context. Cambridge ; New York: Cambridge University Press.

Mott, Nevill F., and Harry Jones. 1936. *The Theory of the Properties of Metals and Alloys*. London: Oxford University Press.

Ohnesorge, Miguel. 2021. “How Incoherent Measurement Succeeds: Coordination and Success in the Measurement of the Earth’s Polar Flattening.” *Studies in History and Philosophy of Science* 88 (August):245–62. https://doi.org/10.1016/j.shpsa.2021.06.006.

———. 2022. “Pluralizing Measurement: Physical Geodesy’s Measurement Problem and Its Resolution.” *Studies in History and Philosophy of Science* 96 (December):51–67. https://doi.org/10.1016/j.shpsa.2022.08.011.

Pexton, Mark. 2014. "How Dimensional Analysis Can Explain." *Synthese* 191 (10): 2333–51. https://doi.org/10.1007/s11229-014-0401-x.

Siscoe, Robert Weston. 2022. "Grounding and the Epistemic Regress Problem." *Erkenntnis*, May. https://doi.org/10.1007/s10670-022-00561-7.

Smith, George E. 2014. "Closing the Loop: Testing Newtonian Gravity, Then and Now." In *Newton and Empiricism*, edited by Zvi Biener and Eric Schliesser, 0. Oxford University Press. https://doi.org/10.1093/acprof:oso/9780199337095.003.0011.

Smith, George E., and Raghav Seth. 2020. *Brownian Motion and Molecular Reality: A Study in Theory-Mediated Measurement*. Oxford Studies in Philosophy of Science. New York, NY: Oxford University Press.

Soler, Léna. 2015. "Calibration in Scientific Practices Which Explore Poorly Understood Phenomena or Invent New Instruments." In *Standardization in Measurement: Philosophical, Historical and Sociological Issues*, edited by Oliver Schlaudt and Lara Huber, 95–110. History and Philosophy of Technoscience, no. 7. London: Pickering & Chatto.

Sterrett, Susan G. 2021. "Dimensions." In *The Routledge Companion to Philosophy of Physics*, edited by Eleanor Knox and Alistair Wilson, 666–78. London: Routledge.

———. 2023. "How Mathematics Figures Differently in Exact Solutions, Simulations, and Physical Models." In *Working Toward Solutions in Fluid Dynamics and Astrophysics: What the Equations Don't Say*, edited by Lydia Patton and Erik Curiel, 5–29. SpringerBriefs in History of Science and Technology. Cham: Springer International Publishing. https://doi.org/10.1007/978-3-031-25686-8_2.

Stroud, Barry. 1968. "Transcendental Arguments." *Journal of Philosophy* 65 (9): 241–56. https://doi.org/10.2307/2024395.

Tal, Eran. 2017a. "A Model-Based Epistemology of Measurement." In *Reasoning in Measurement*, edited by Nicola Mössner and Alfred Nordmann, 233–53. History and Philospohy of Technoscience 9. London; New York: Routledge.

———. 2017b. "Calibration: Modelling the Measurement Process." *Studies in History* *and Philosophy of Science Part A*, The Making of Measurement, 65–66 (October):33–45. https://doi.org/10.1016/j.shpsa.2017.09.001.

———. 2019. "Individuating Quantities." *Philosophical Studies* 176 (4): 853–78. https://doi.org/10.1007/s11098-018-1216-2.

"The Nobel Prize in Physics 1946." n.d. *NobelPrize.org*. Accessed December 8, 2021. http://www.nobelprize.org/prizes/physics/1946/summary/.

Uebel, Thomas. 2019. "Verificationism and (Some of) Its Discontents." *Journal for the History of Analytical Philosophy* 7 (4). https://doi.org/10.15173/jhap.v7i4.3535.

Vessonen, Elina. 2021a. "Conceptual Engineering and Operationalism in Psychology." *Synthese*, July. https://doi.org/10.1007/s11229-021-03261-x.

———. 2021b. "Respectful Operationalism." *Theory & Psychology* 31 (1): 84–105. https://doi.org/10.1177/0959354320945036.

White, Frank M. 2015. *Fluid Mechanics*. 8th ed. New York: McGraw-Hill Higher Education.

Woodward, James F. 2011. "Data and Phenomena: A Restatement and Defense." *Synthese* 182 (1): 165–79. https://doi.org/c7w5gc.

Zhao, Kino. 2023. "Measuring the Non-Existent: Validity Before Measurement." *Philosophy of Science* Forthcoming. (January). http://philsci-archive.pitt.edu/21626/.

Zwicky, F. 1927. "Phenomena Depending on the Change of Elastic Frequencies in Solid Bodies with Pressure." *Physical Review* 29 (4): 579–87. https://doi.org/10.1103/PhysRev.29.579.