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HOW "QUANTITY" DISAPPEARED FROM PHILOSOPHIES OF MEASUREMENT: PERSPECTIVES FROM 19TH CENTURY SCIENCES

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Table of Contents

List of Figures

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Abstract

This dissertation explores how "quantity" was reconceptualized in the context of 19th century experimental sciences. In canonical works of philosophy by Aristotle, Descartes or Kant, "quantity" serves as the link between mathematics and its application in the empirical domain. The conception that "quantities" are composed out of equal, additive units is based on the archetypical geometrical magnitudes and its part-whole structure. Despite the fact that measurement techniques in most experimental sciences already involved much more complex conceptual and practical operations than counting units, the old conception of "quantity" continued to play a crucial role in philosophers and scientists' attempt to explain how mathematics is applied well into the 19th century. In the second half of the 19th century, philosophically minded scientists shifted their attention away from defining the generalized "quantity" to reflecting on the methods and foundation of the measurement process. As they incorporated considerations of experimental work into their philosophical thinking, they highlighted the role of measurement operations, instruments, and the underlying hypotheses and laws in constituting the meaning of every quantitative concept. Even length and time—the most elementary quantities known to mankind—are not a priori quantitative, but rest on certain assumptions underlying the measurement process carried over from experience. This shift of attention led to the disappearance of "quantity" with a fixed, generalized definition from philosophical writings concerning measurement from late 19th century onward.

Specifically, this dissertation contextualizes several sets of discussions among 19th century scientists regarding the foundation of measurement in general or the measurability of specific concepts. The first is Hermann von Helmholtz's discussion of the foundation of measurement in light of his broader experimental work, such as the quantification of electricity and magnetism,

whose concepts were embodiments of dynamic laws in experiments. The second is the discussion of common problems shared by thermometry and psychophysics by Ernst Mach, Gustav Fechner, Wilhelm Wundt, Alexius Meinong and others. The issue boiled down to the possibility of constructing uniform scales for quantities incapable of direct comparison and the complexities involved. The third discussion concerns the implication of non-Euclidean geometry on spatial measurement. In light of special relativity at the beginning of 20th century, space and time both became derived quantities, calculated from laws of physics given a chosen method of measurement. Overall, a few common themes and approaches coalesced through 19th century discussions, which became crucial for later thinkers and scientists to think about measurement through conceptual laws, observation and experiment.

Introduction

Overview

When thinking of measurement, the term "quantity" seems to inevitably come to mind. But what is a quantity? What is it that we measure when we measure? Philosophers from Aristotle to René Descartes to Immanuel Kant devoted significant passages in their works to the term "quantity" when discussing the relationship between mathematics and its applications to empirical experience. But quantity seems to entirely disappear in post-20th century philosophical texts on measurement, which tend to focus on epistemic questions arising from the measurement process, whether these questions concern the conceptual or operational aspects of measurement. Some of these questions might include: how does observation relate to formal theories? To what extent does measurement operations constitute the meaning of the concept?[1] However, almost no one except a few authors in late 19th century seem to be able to speak of "quantity," at least not as definitively as Descartes or Kant did.[2] "Quantity" used to serve as the link between numbers, empirical experiences and the concepts manipulated by the sciences. How, when and why did it disappear from theories of measurement?

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[1] This characterization comes from the entry on "Measurement" from the *Routledge Companion to Philosophy of Science*. Hasok Chang and Nancy Cartwright, "Measurement," in *The Routledge Companion to Philosophy of Science*, ed. Martin Curd and Stathis Psillos (Routledge, 2013), 411–19.

[2] "Quantity" was one of the first things that Descartes, in the fifth meditation and immediately after the proof of the existence of God, could grasp clearly and distinctly about material things external to the thinking subject: "quantity…or 'continuous' quantity as the philosophers commonly call it, is something I distinctly imagine. That is, I distinctly imagine the extension of the quantity (or rather of the thing which is quantified) in length, breadth and depth. I also enumerate various parts of the thing, and to these parts I assign various sizes, shapes, positions and local motions; and to the motions I assign various durations." René Descartes, "Fifth Meditation," in *The Philosophical Writings of Descartes: Volume 2* (Cambridge University Press, 1984), 44 (63-64).

For Kant, providing a mathematical foundation for a concept in science meant constructing this concept as a "quantum." See Chapter 1 for more details.

It is often taken for granted that philosophical questions surrounding measurement only emerged at the beginning of the 20th century, in response to the intellectual revolution brought forth by the theory of relativity. It is to the legacy of Albert Einstein, Henri Poincaré, Percy Bridgman, and so on, that we owe our current, more sophisticated understanding of measurement. Granted, special relativity fundamentally challenged and redefined how we understand the most elementary quantities known to mankind—length and time—and hence opened up entirely new fields of philosophical inquiries about measurement. But there were also significant debates in the second half of the 19th century that paved way for a new conception of the foundation of measurement. Specifically, these late 19th century discussions led to a shift of philosophical attention away from the universal and abstract "quantity" to the methods and processes of measurement, by focusing on foundational issues emerging from the latter in specific experimental contexts. On a closer look, the term "quantity" in most pre-19th century canonical works of philosophy was based on the archetypical geometrical magnitude, with a "self-evident" part-whole structure, equal units, and an unambiguous additive procedure. This scheme was projected onto concepts in the exact sciences whenever they became measurable—time, motion, force, etc., which were obtained by a much more complicated process than "multiplying units to obtain the whole." What 19th century scientists and philosophers exposed was the inadequacy of the unit-based understanding of quantity in explaining how numbers were applied and scales were constructed, in an era when sciences became increasingly inseparable from experimental methods. What kind of unit do we count, for instance, in the case of electricity and magnetism, if quantitative concepts such as charges and current intensities are defined as nothing other than the dynamic regularities exhibited by a certain experimental arrangement? What kind of equal units does a temperature scales have, if the very concept is only a common banner under which heterogeneous behaviors of materials under thermal changes are grouped, and there is no way to directly compare 1° at different locations on the scale? The notion of quantity as divisible into equal units, and the notion of measurement as the addition of units, only applied to concepts that are already measurable by well-established methods, allowing them to reap all the benefits of arithmetical numbers, including divisibility and additivity. These notions do not explain how properties and phenomena become measured in the first place.

It was through 19th century debates that a unit-based conception of "quantity" gave in to a more-or-less common understanding that units and quantities are derived from laws, hypotheses and techniques involved in measurement. The view that quantitative concepts were simply discovered in nature was replaced by the understanding that measurement processes also constituted the meaning of quantitative concepts. Scientists and philosophers realized that even the most basic quantity—spatial extension—was neither inherently mathematical nor a priori, but rested on certain assumptions about the method of measurement, i.e., rigidity of bodies in motion. In a sense, the reconceptualization of space and time at the beginning of the 20th century also stemmed from this shift of perspective, since special relativity itself treated length and time not as primitive quantities, but as derived from the laws describing inertial motion, a natural constant, and the specific operation of measuring time (by traveling light).

The emerging field of inquiries into the foundation of measurement resulted from the interplay between the new quantitative sciences of the 19th century, such as non-Euclidean and projective geometries, electricity, magnetism, thermometry, psychophysics, photometry, and philosophical ideas of the time. In my dissertation, I examine writings on the foundation of measurement from a variety of philosophically minded scientists and scientifically minded philosophers, including Hermann von Helmholtz, Hermann Cohen, Johannes von Kries, Ernst Mach, Gustav Fechner, Wilhelm Wundt, Alexius Meinong and many others, in relation to the specific problems they encountered in experimental sciences. They might be active researchers in multiple disciplines and simultaneously prolific writers in philosophy of science; they might also be concerned with specific problems in a single discipline, such as the construction of scales for newly established concepts or the interpretation of new experimental results. But over again they found themselves tackling broader issues regarding the foundation of measurement drawn out by their specific, either theoretical or experimental work. Collectively, their discussion on issues related to measurement in 19th century sciences shifted the focus of philosophical inquiry away from defining "quantity" to the measurement process.

Overall, developments in late 19th century sciences paved way for 20th century thinkers' consideration of the intellectual and operational processes involved in measurement, by challenging the paradigm of quantity built on the part-whole structure and equal units. While revolutionary ideas like Henri Poincaré's conventionalism and Einstein's special relativity were indeed influential, 19th century discussions laid the groundwork for an emerging field of philosophical inquiries into measurement.

Methods and Literature review

My dissertation discusses 19th century scientists and philosophers' ideas on the foundation of measurement in relation to the specific experimental methods and measurement tasks they were dealing with. As a result, my chapters blend philosophical discussions with narratives in the style of intellectual history and descriptions of the technical aspects of experiments and instruments. While there is extensive literature on different aspects of 18th and 19th century measurement,[3] and on application of numbers in different realms—not just sciences but also

[3] M. Norton Wise, ed., *The Values of Precision* (Princeton University Press, 2020); Jimena Canales, *A Tenth of a Second: A History* (University of Chicago Press, 2010); Peter Galison, *Einstein's Clocks, Poincaré's Maps: Empires of*

social institutions and private practices, policies, governance, cultures[4], etc., my dissertation differs from these existing works by bridging philosophical ideas about what measurement *is* with the specific techniques and tasks in experimental sciences that helped form these ideas. The focus is on how certain philosophical ideas relate to concrete experiments, instruments, and broader philosophical debates. As a result, the role of institutions, political events, social dynamics is at the moment of less concern. Similarly, the expansion of quantitative methods into social sciences, as well as the rise of probability and statistics, are not included in my dissertation, although both very likely did play significant roles in shaping theories on the foundation of measurement and should be incorporated into future research on the topic.

In addition, there are many studies on individual scientist-philosophers' ideas and the interplay between these ideas, often focusing only on contexts related to philosophy and theoretical physics.[5] Different from these works, my focus is on filling in the gaps between

Time (W. W. Norton & Company, 2003); Hasok Chang, *Inventing Temperature:Measurement and Scientific Progress: Measurement and Scientific Progress* (Oxford University Press, USA, 2004); J. L. Heilbron, *Weighing Imponderables and Other Quantitative Science around 1800* (University of California Press, 1993); Timothy Lenoir, "Helmholtz and the Materialities of Communication," *Osiris*, 1994, 24; Kathryn Olesko, "Precision, Tolerance, and Consensus: Local Cultures in German and British Resistance Standards," in *Scientific Credibility and Technical Standards in 19th and Early 20th Century Germany and Britain*, ed. Jed Z Buchwald, Archimedes: New Studies in the History and Philosophy of Science and Technology, 1996; Bruce J. Hunt, "The Ohm Is Where the Art Is: British Telegraph Engineers and the Development of Electrical Standards," *Osiris* 9 (January 1994): 48–63, https://doi.org/10.1086/368729; Bruce J. Hunt, "Scientists, Engineers and Wildman Whitehouse: Measurement and Credibility in Early Cable Telegraphy," *The British Journal for the History of Science* 29, no. 2 (June 1996): 155–69, https://doi.org/10.1017/S0007087400034208; Simon Schaffer, "Late Victorian Metrology and Its Instrumentation: A Manufactory of Ohms," in *Invisible Connections: Instruments, Institutions, and Science*, vol. 10309 (Invisible Connections: Instruments, Institutions, and Science, SPIE, 1992), 27–60, https://doi.org/10.1117/12.2283709; Klaus Hentschel, "Gauss, Meyerstein and Hanoverian Metrology," *Annals of Science* 64, no. 1 (January 1, 2007): 41–75, https://doi.org/10.1080/00033790600964339. John B. Hearnshaw, *The Analysis of Starlight: Two Centuries of Astronomical Spectroscopy* (Cambridge University Press, 2014).

[4] Tore Frängsmyr, J. L. Heilbron, and Robin E. Rider, eds., *The Quantifying Spirit in the 18th Century* (University of California Press, 1990).

[5] Francesca Biagioli, *Space, Number, and Geometry from Helmholtz to Cassirer* (Springer, 2016); Francesca Biagioli, "Cohen and Helmholtz on the Foundations of Measurement," in *Philosophie und Wissenschaft bei Hermann Cohen/Philosophy and Science in Hermann Cohen* (Springer, 2018), https://link.springer.com/chapter/10.1007/978-3-319-58023-4_4; Robert DiSalle, "Helmholtz's Empiricist Philosophy of Mathematics. Between Laws of Perception and Laws of Nature," in *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*, ed.

philosophical ideas with issues in the practical scientific work that authors of these ideas might have in the back of their minds. Ideas are embedded in broader scientific practices and concerns, not merely products of pure thought exchange. Rather than deriving the full philosophical consequences of some of these ideas, I trace the historical changes in the way measurement was conceived. Although many aspects of the 19th century sciences I discuss might not have survived the test of time (e.g., psychophysics, photometry, the "absolute system" of units, conventional thermometry relying on individual thermometric substances) as certain parts of physics did, e.g., special relativity, this does not mean that they played insignificant roles in shaping the discussions surrounding the foundation of measurement. Successful and failed sciences equally helped their contemporaries in understanding measurement and measurability.

I want to emphasize a few works that have significantly shaped my topic, materials and approach. The first is Michael Heidelberger *Nature from Within: Gustav Theodor Fechner and His Psychophysical Worldview*, which pointed to many source materials I end up using in Chapter 2 and 3. In one of his chapters, Heidelberger identifies how controversies over Fechner's psychophysics was a debate about the foundation of measurement and discusses the views of many authors that I cover, including both Helmholtz and Mach. Where I differ from Heidelberger is that I contextualize ideas of Helmholtz and Mach on measurement in other areas of science. For instance, Helmholtz was heavily involved in the creation of an absolute system of units for electricity and magnetism. Although he was on board with some of Fechner's ideas, his stance on

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David Cahan (University of California Press, 1993), 498–521; Karim P. Y. Thébault, "On Mach on Time," *Studies in History and Philosophy of Science Part A* 89 (October 1, 2021): 84–102, https://doi.org/10.1016/j.shpsa.2021.08.001;

Two other major works that may be related to my topic are: Robert DiSalle, *Understanding Space-Time: The Philosophical Development of Physics from Newton to Einstein* (Cambridge University Press, 2006); Julian B. Barbour, *The Discovery of Dynamics: A Study from a Machian Point of View of the Discovery and the Structure of Dynamical Theories* (Oxford University Press, 2001).

measurement incorporated much broader concerns over the quantitative and experimental methods in the exact sciences, as proved by the mention of measurement techniques in electricity, magnetism and photometry in his 1887 "Counting and Measuring." In drawing a comparison between Mach's ideas on thermometry, I also focus more closely on the issues within thermometry, which Mach outlined in his lecture series *Principles of the Theory of Heat*. Specifically, I argue that there is a parallel in the problems in conventional thermometry and psychophysics, and these problems came from the attempt to construct a uniform scale for quantities that could not be directly compared, but must be measured surrogatively, i.e., stood for by other quantities of different kinds. I also ask what Mach would have said about William Thomson's absolute scale for temperature. To Hasok Chang's extraordinary study *Inventing Temperature: Measurement and Scientific Progress* I owe my understanding of the historical development and epistemological problems in thermometry. If I hope to add anything to Chang's analysis of the epistemic circularity involved in constructing a thermometric scale[6], it is that this problem is not unique to temperature but is present with other surrogatively measured concepts. Chang's book also provides significant methodological guidance on integrating philosophical inquiries with historical case studies.

Joel Michell's various articles[7] on the shift from the "classical" to the "representational" theory of measurement has led me to investigate the precise difference in the way "quantity" was

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[6] This is also designated by "the problem of nomic measurement": "1. We want to measure quantity X. 2. Quantity X is not directly observable by unaided human perception so we infer it from another quantity Y, which is directly observable. 3. For this inference we need a law that expresses X as a function of Y, X = f(Y). 4. The form of this function f cannot be discovered or tested empirically, because that would involve knowing the values of both Y and X, and X is the unknown variable that we are trying to measure." Hasok Chang, "Spirit, Air, and Quicksilver: The Search for the 'Real' Scale of Temperature," *Historical Studies in the Physical and Biological Sciences* 31, no. 2 (2001): 249–84, https://doi.org/10.1525/hsps.2001.31.2.249.

[7] Joel Michell, "The Origins of the Representational Theory of Measurement: Helmholtz, Hölder, and Russell," *Studies in History and Philosophy of Science Part A* 24, no. 2 (June 1, 1993): 185–206,

conceptualized in canonical works of philosophy, such as the works of Aristotle, Descartes and Kant. Oliver Darrigol's article "Number and measure: Hermann von Helmholtz at the crossroads of mathematics, physics, and psychology"[8] also points to many sources that proved helpful to my dissertation. Darrigol relates Helmholtz's views on measurement to his contemporary scientists including mathematicians like Paul Du Bois Reymond and Hermann Grassman, psychologists and philosophers (concerned with the possibility of quantitative psychology) like Fechner and von Kries, Wundt and Edward Zeller and physicists like Maxwell and Mach. Darrigol also mentions Helmholtz's reception by mathematicians such as Gottlob Frege and Bertrand Russell. Building on Darrigol's survey of the views on quantity and measurement by the authors mentioned above, I look into the specific scientific issues and measurement tasks some of these authors were faced, and how these specific issues and tasks contributed to their specific views. Francesca Biagioli's *Space, Number, and Geometry from Helmholtz to Cassirer* similarly points to some useful sources on the discussion of measurement. Unlike Biagioli's book, which focuses on the debate on non-Euclidean geometry and neo-Kantianism, I focus more on the experimental practices that informed many of the authors we both discuss.

Chapter Summary

In the first chapter, I present the "classical" united-based conception of quantity that became increasingly out of synch with measurement practices in the sciences. In the writings of

https://doi.org/10.1016/0039-3681(93)90045-L; Joel Michell, "The Logic of Measurement: A Realist Overview," *Measurement*, The logical and philosophical aspects of measurement, 38, no. 4 (December 1, 2005): 285–94, https://doi.org/10.1016/j.measurement.2005.09.004.

[8] Also see David Cahan, *Helmholtz: A Life in Science* (Chicago: The University of Chicago Press, 2018) for the connection between Helmholtz's "Counting and Measuring" and his extensive research and practical work.

In addition, Matthias Neuber's article also helps me solidify my interpretation of Helmholtz. See Matthias Neuber, "Helmholtz, Kaila, and the Representational Theory of Measurement," *HOPOS: The Journal of the International Society for the History of Philosophy of Science* 8, no. 2 (September 2018): 409–31, https://doi.org/10.1086/699015.

most pre-19th century philosophers, quantity means a plurality of units that composed the whole. Since Greek (pure) mathematics (e.g., as in Euclid's *The Elements*) dealt with either numbers or specific geometrical magnitudes—lines, areas and volumes—it was natural for early modern natural philosophers to conceive of the generalized "quantity" in terms of the part-whole relationship exemplified by these archetypical magnitudes. Galileo Galilei's success in representing the laws of motion by geometrical diagrams prompted him to consider quantities like speed to be composed of additive parts. Just like segments of space and intervals of time were additive, one should also follow "the habit and custom of nature herself" and "conceive additions of speed as taking place without complication."[9] Yet in what sense can we understand speed as composed of smaller speeds, in the same way that length is composed of smaller lengths? Immanuel Kant grappled with the same issue in the *Metaphysical Foundations of Natural Sciences*, as he attempted to reconcile his belief that motion as a quantity simply could not be thought as parts constituting the whole, with the fact that it had been successfully quantified in Newtonian physics. This conceptual difficulty, I argue, is symptomatic of the failure of part-whole structure to account for how concepts in natural sciences became quantitative through measurement. In other words, a gap between mathematical theories and the practices of measurement was never bridged since the 14th century, when the concept of motion as an additive quantity was invented by Galileo's own predecessors.

In the second chapter, I discuss how in the second half of the 19th century, new theories to conceptualize the relation between number and quantities emerged, which for the first time accounted for the role of the method of measurement. This was achieved first through challenges

[9] Galilei Galileo, *The Discourses and Mathematical Demonstrations Relating to Two New Sciences*, trans. Henry Crew and Alfonso de Salvio (Macmillan Company, 1914), 197.

to the validity of geometrical measurement. In a series of essays on the foundation of geometry, the German scientist Hermann von Helmholtz popularized non-Euclidean geometries as not just mathematical possibilities, but legitimate candidates to describe physical space. Crucially, he revealed that quantitative comparisons in Euclidean space rest on an empirical presupposition not fully validated. Congruence in superposition, on which every comparison in Euclidean geometry rests, does not in itself guarantee irrefutable results: had one's measuring rod been shriveling up in motion together with one's body, there would be no way to detect it by geometrical means alone. Helmholtz's thought experiment of the "world in a convex mirror" illustrating pseudo-spherical space became a crucial point in Henri Poincaré's 1902 *Science and Hypothesis,* with which the latter leveraged the argument of *conventionalism:* empirical experience is often insufficient in arbitrating two systems of mathematical representation, hence the choice ultimately rests on convenience.

In addition, Helmholtz also proposed a theory of measurement in an 1887 article "Counting and Measuring" to account for his contemporary measurement practices. Referring to how examples in electricity and magnetism, heat, and color science, he argued that notions like unit and divisibility are not useful in understanding measurability. Instead, mathematization depends on whether the *method* of comparison allows for an unambiguous interpretation of mathematical equality and additivity in terms of experimental effects. A quantitative concept embodies physical objects' capacity to bring forth precisely these experimental effects, which render the result of comparison lawful and mathematically describable. In addition to analyzing these ideas in my second chapter, I examine the techniques of measurement in electricity and magnetism, an area which Helmholtz was involved in when he joined the international effort to standardize electrical units in the 1880s. By highlighting the definitions of these units in terms of basic electrodynamic laws and the canonical experiments that embodied the principles of their measurement, I argue that these practices informed Helmholtz's unique views on quantity and measurement. The large-scale, high-profile experiments leading to the determination of electrical units in Germany and Britain underscored the embeddedness of abstract quantitative concepts in empirical procedures.

While Helmholtz brought attention to how the conditions and methods of measurement played a role in constituting quantitative concepts, his contemporaries like Ernst Mach and commentators on psychophysics like Wilhelm Wundt and Alexius Meinong confronted the following question: how could indirectly measured, non-additive quantities be said to have a uniform scale? In the third chapter, I examine the reflection on the foundation of measurement provoked by controversies in thermometry and psychophysics, which shared similar problems. Temperature was traditionally represented by the volume or pressure of a particular substance. Had all substances expanded to the same proportion, one might have been able to simply define a uniform scale for temperature through an 1-1 mapping to volume, but this is not the case: experiments up to mid-19th century showed that all substances behave somewhat differently from each other with temperature change. This means that an 1° increase on different thermometers, and on different parts of the same thermometer, correspond to different quantities of heat. In conventional thermometry there was no way to provide an external reference for the quantity that 1° represented without choosing an arbitrary material as a standard. As Mach pointed out in 1896 *Die Principien der Wärmelehre* (*Principles of the Theory of Heat)* and Hasok Chang does in *Inventing Temperature* more recently, to speak of the possibility of any thermometric substance expanding "uniformly" with "real" temperature in conventional thermometry is an illusion, because it presumes that one already knows how to measure temperature independent of specific thermometric substances. Interestingly, this problem is not unique to temperature. Through a set of experiments showing that humans tend to perceive a difference in two physical stimuli when the physical intensities of these stimuli maintain a constant proportion, the psychophysicist Gustav Fechner proposed that he had found an additive scale for sensation with equal units. These equal units were represented by "just-noticeable-differences" in sensation, which he claimed to be measured every time the experimental subjects reported that they noticed a difference in stimuli. Fechner then proceeded to integrate these "just-noticeable-differences" to obtain an additive scale for sensation. Commentators from Rudolf Elsa to Wilhelm Wundt to Alexius Meinong endorsed the experimental facts, while denying Fechner's arguments for an additive scale, for the reason that "just-noticeable-differences" could neither be proved equal nor be constitutive of the original sensation. They argued that psychophysical "measurement" was analogous to primitive temperature measurement; as such, it provided a quantitative description allowing for fixed points on the "continuum of sensation" without producing equal, additive units.

With the redefinition of absolute temperature by the conversion rate of heat into work (which depended on temperature) in an ideal Carnot cycle by William Thomson, differences between degrees of temperature obtained a common external reference. But concepts like temperature and potential remained "concepts of level," in Mach's words, implying a difference from quantities with additive parts. Noticing that controversies about psychophysics were at its root a debate about the foundation of measurement, Meinong argued that it was crucial to provide a conceptual foundation for non-additive, "surrogatively" measured quantities (i.e., quantities incapable of directly comparison, but must be stood for by other quantities or relationships), which by the end of the 19th century dominated the exact sciences. Most of these quantitative concepts could not fall under the binary categories of extensive or intensive, divisible or indivisible magnitudes. Instead, they represented complexes of relations between other measurable quantities exhibited in experiments.

My fourth chapter discusses how spatial and temporal quantities—the paradigmatic extensive magnitudes supposedly sharing the same part-whole structure with the geometrical length—came under close scrutiny at the turn of the 20th century. Debates over psychophysics, in fact, touched upon some issues with measurement in the exact sciences: Fechner, for instance, pointed out that judgments of equality in the measurement of mechanical quantities boiled down to psychic impressions (e.g., the aligning of ends of two lengths). In the 1898 *Science and Hypothesis*, Poincaré argued that intuition of rigid body motions (on which the "equality" of spatial magnitudes was based) is compatible with multiple criteria for rigidity, depending on the geometry used to describe these motions. Furthermore, experience cannot determine the "correct" geometry of space and, by extension, the "correct" analytic expressions for rigid body motions. As the possibility of spatial measurement was cast into doubt, so did the measurement of time. Mach had previously noted how the measurement of time is ultimately about the interdependence of bodily motions, just as thermometry without an external "absolute" scale is about the mutual coordination of individual substances' behaviors with respect to heat. When regarding time as measurable by equal durations set by uniform motions, uniformity is projected onto an abstract concept of time that actually does not exist independent of individual motions, while individual motions can only be determined as uniform given that one knows how to measure time. As more physicists came to regard Newtonian absolute time as a theoretical hypothesis, Poincaré brought attention to the fact that judgments of simultaneous events across a distance, on which "equal" durations must be based, always lacks self-evidence. Instead of being an immediate judgment, it depends on the method with which one chose to calculate time. With the acceptance of Einstein's special relativity, length and time became derived quantities given the invariance of speed of light and laws of motion in inertial frames, and they must be calculated from the relative motions of reference frames. Spatial and temporal measurement lost their universal validity across reference frames.

Given these intellectual and scientific developments, the traditional conception of quantity based on the archetype of geometric length became increasingly irrelevant. If this generalized quantity served as a link between pure mathematics and its application before the 19th century, then the exclusion of "quantity" from pure mathematics itself, due to the discipline's pursuit of formal precision and rigor, rendered it unnecessary for any theory of measurement to explain what the term meant in pure and applied contexts. The growth of projective geometry proved the possibility of an autonomous, non-quantitative geometry, while the quest to transform mathematical statements into symbolic logical statements eliminated intuitive notions like "continuous magnitude" as variables of mathematical functions. Developments in pure mathematics were taken into account by early 20th century accounts of measurement, which took it as a given that mathematics did not deal with magnitudes, but rather formal structures and relations. Specifically, authors such as Ernst Cassirer and Norman Campbell both echoed the ideas I discuss in previous chapters. For instance, they explicitly noted how traditional understanding of measurement failed to explain the foundation and principles of measurement in scientific practice. Cassirer noted that "what guides us in the choice of units is…always the attempt to establish certain laws as universal." These laws were therefore the true constants in measurement, not the "measuring rods and units."[10] Campbell, on the other hand, was

[10] See Chapter 4 for reference of this quote.

dissatisfied with how measurement had been depicted simplistically as “comparison with a unit of the same kind”: “a student set to measure g with Atwood machine may well wonder at what stage he is comparing the gravitational acceleration with a unit of the same kind; and an engineer may be puzzled when he considers that the unit of force that he employs causes an acceleration in his unit mass of 32.2.”[11] The focus of these accounts therefore shifted to the measurement process, and on the role of physical laws in constituting quantitative concepts. In a sense, both Cassirer and Campbell fleshed out in a more systematic way many inchoate ideas discussed by their 19th century predecessors.

[11] Also see Chapter 4 for this quotation.

Chapter 1 Quantity in the Canons: the Archetype of Geometrical Magnitudes

1.1. Introduction

Measurement was not a problematic concept before the 19th century. Quantities were assumed to exist in nature independent of the measuring process, and measurement was assumed to simply discover their numerical relationships. Once a property or phenomena became measurable, it was considered a quantity, which entailed a part-whole structure typical of geometrical magnitudes. The relationship between number and quantity was likewise not very complex: the ratio between two quantities was equated with the ratio between two numbers or a number, given that one of the quantities served as the unit. In a 1993 paper, Joel Michell labelled the pre-19th century view as "the classical concept of measurement," in juxtaposition with the "representational view of measurement," which became prevalent in the 20th century, regarding measurement as a mapping, or a correlation, between a numerical and an empirical structure.[1]

This chapter examines the "classical" conception of measurement in detail. Characterization of quantity and measurement in canonical works of philosophy, such as the writings of Aristotle, Descartes and Kant, as well as the intellectual roots of such characterization, have not been discussed at length in existing literature concerning the history of measurement theory. There are several common features in the way philosophers writing before the 19th century understood quantity: first, they tended to assume that the mathematical structure of a quantity inhered in nature or in mankind's unmediated experience of nature.[2] Secondly, this

[1] Joel. Michell, *Measurement in Psychology: Critical History of a Methodological Concept* (Cambridge, U.K.: Cambridge University Press, 1999), 25; Michell, "The Origins of the Representational Theory of Measurement,"188.

[2] A similar point was made by Michell and others, namely that numerical properties were assumed to exist in nature as a fact. My point is similar but not restricted to the properties of numbers. Moreover, in this chapter I analyze a number of historical sources that differed from Michell's.

mathematical structure was believed to be geometrical and best represented through straight-line segments, or rectilinear figures in general. Geometrical magnitudes were the archetypical quantities through which all other quantitative concepts came to be conceived. Not only the part-whole structure of geometrical magnitudes, but also their mode of composition, i.e., the successive conjoining of the parts, were projected onto the way all other quantitative concepts were thought to be measured. Kant went as far as taking the "successive synthesis part by part" as one of the basic modes of perception itself. As a result, the philosophical characterization of measurement as an intellectual activity boiled down to counting units: the part or unit taken so many times as to exhaust the whole constituted the plurality (or quantity). This conception remained persistent even if, by the 17th century, highly sophisticated measurement techniques and instruments were widely used for measuring quantities indirectly. When Christiaan Huygens studied the intricate causes impacting pendulum swings in designing extremely accurate mechanical clocks, or when Guillaume Amonton used the constant relationship between air pressure (measured in turn by a mercury column it could support) and volume to measure temperature, what kind of homogeneous unit were they counting? In both cases, the conjoining of units clearly fails to describe the conceptual and practical operations involved. However, philosophical theories of measurement up until Kant's time continued to center on the comparison of units contained in a whole, modelled on the geometrical magnitude.

The relationship between mathematics and empirical sciences[3] was characterized as a relationship between genus and species according to Aristotle and his followers in the 17th

[3] By empirical sciences I mean the "mathematical disciplines" or "mixed mathematics" in premodern contexts: astronomy, optics, music, etc., including those later known as "physico-mathematics" in the 16th and 17th century. For the use of the term "physico-mathematics" see Peter Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (University of Chicago Press, 1995), 168-179. Dear also noted that the Aristotelian distinction between physics (dealing with essences and causes) and mathematics (dealing with quantities alone)

century, and this view further lent support to conceiving quantitative concepts in terms of the part-whole structure of the geometrical line. In Aristotle's theory, quantities in the sciences, obtained by empirical measurement, are particulars, while pure mathematical magnitudes (the discrete and the continuous) are universals. As Aristotle believed, there is no essential difference between the two. Objects have quantitative properties insofar as they are considered measurable, but these properties are also inseparable from the objects. When arithmetic and geometry study "pure" magnitudes and mathematical sciences like astronomy (and, later, physico-mathematics) study quantitative concepts, the distinction is merely a matter of the division of labor. It is not surprising that when natural philosophers first applied geometry to motion, i.e., when speed and acceleration were declared quantities, concepts of motion were represented in geometrical forms and thought to have the same part-whole structure as magnitudes in pure geometry, e.g., the straight line. But this was not entirely uncontroversial—scholastic philosophers from the 14th century did debate whether the intensity of qualities and motion could simply be represented through the addition of parts. That is, before uniform speed and acceleration were accepted as the uncontroversial starting point of a quantitative science of motion, it was actually not clear to many how these concepts could be considered quantities just like the lengths, times, and natural numbers. After all, are speeds composed of smaller speeds? The spectacular success of Galileo's mechanics perhaps overshadowed these philosophical inquiries. Galileo used rectilinear figures to represent accelerated motion without qualms, arguing that the "additions of speed" represented

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began to dissolve through the efforts of 16th and 17th century Jesuit mathematicians, who urged that mathematical demonstrations served as causal explanations as well. However, this shift was not complete. Many German physicists in the early 19th century continued to distinguish physics from other disciplines in terms of "causes" and "the essence of phenomena," as shown by Kenneth L. Caneva, "From Galvanism to Electrodynamics: The Transformation of German Physics and Its Social Context," *Historical Studies in the Physical Sciences* 9 (1978): 63–159, https://doi.org/10.2307/27757377.

by straight lines took place "without complication." The ease and simplicity of such conception, he noted, stemmed from "the habit of nature."[4] In what way concepts like speed shared the same structure as additive geometrical lines was unaddressed—perhaps considered unnecessary in light of the success of the laws of motion.

Newer concepts in the exact sciences were comprehended in similar ways. According to Descartes in *Rules for the Direction of the Mind*, the simplest mode to represent any measurable feature of nature is through the straight line. The mathematician Paul Du Bois Reymond, writing as late as the second half of the 19th century, continued to share this view. However, tension created by the assumed part-whole structure of quantitative concepts also persisted. In Immanuel Kant's *Metaphysical Foundations of Natural Science*, Kant refused to think of speeds as simply an additive "extensive" quantity in the manner of length and space. Speed is an "intensive magnitude," according to Kant, which cannot be grasped through the successive synthesis of part by part, but only as a whole at an instant, as an intensive degree. In his effort to provide a "pure" mathematical foundation for the science of motion in *Metaphysical Foundations*, Kant gave an idiosyncratic and difficult solution to the problem of adding motions. If Kant was motivated to define and construct speed as pure, additive magnitudes in order to explain the apodictic certainty of the mathematical laws of motion, in the end his solution still does not elucidate what components in the empirical experiments are translatable through geometrical representations and relations (in Galileo's or Newton's work), so that mathematical laws are capable of correctly describing and predicting the empirical outcome.

I argue that the difficulties encountered by Kant are symptomatic of the tension between a traditional way of conceiving quantity and explaining the application of mathematics, and the

[4] Galilei, *Dialogues Concerning Two New Sciences,* 197.

emergence of new quantitative concepts and measurement techniques in the sciences. By Kant's time, the default way of explaining the applicability of mathematics in realms beyond geometry and arithmetic was to define magnitudes as direct bearers of quantitative properties, rather than considering the conceptual and practical activities in the measurement process. When the typical definition of quantity through part-whole structure clearly failed in the case of speed and motion, Kant sought to solve the puzzle while maintaining the existing paradigm of explaining mathematical application: to define an immediately additive magnitude with the same kind of intuitive immediacy as the composition of geometric magnitudes. In this sense, his solution in the first chapter of *Metaphysical Foundations of Natural Sciences* appears ad hoc. With the benefit of hindsight, we might argue today that in quantifying any concept, relations between experimental objects and phenomena that stand for measures of space, time, or other quantities come to be described in a mathematical way, with the goal of establishing a universally applicable law. However, Kant's views on mathematical application provided the backdrop against which fledging philosophies of measurement emerged in the 19th century.

In the 19th century, the extensive magnitudes modeled on the geometrical line remained influential to the way many thinkers conceptualized the foundation of measurement and quantification. As quantitative concepts became more and more inextricable from experimental procedures, many 19th century scientists argued that a measurable concept must be reducible to units of length, mass and time. These three mechanical units were still conceived through the part-whole structure, and the reason why they provided a solid foundation for all other physical measurement, for many, was that their equality and addition had indubitably clear meaning. Mathematician like do Bois Reymond, on the other hand, argued along Descartes' line that all measurable quantities can be considered as "linear mathematical quantities." The culmination of the "classical" conception of measurement was perhaps the standardization of length and mass at the 1875 International Metre Convention, when the material standard of a length prototype was literally obtained from congruence of superposition—the method by which geometrical length is ordinarily measured and equal unit length determined.

1.2. Abstraction and Universal Mathematics

Aristotle is the main progenitor of idea that mathematical properties reside in natural objects but are separated out for the purpose of analysis. As Aristotle's natural philosophy and logic formed the core of medieval university education by 1255,[5] his views on the status of mathematics and the definition of its subject matter had a long-lasting impact. Mathematics is not the first science in Aristotle's writings (it occupies an intermediate position between metaphysics and the sciences), but it deals with the universal. In his philosophy, under the heading of pure mathematics are the subordinate mathematical disciplines, such as harmonics, mechanics, optics and astronomy. These subordinate disciplines deal with the particulars. Pure mathematical entities do not exist on their own, independent from the perceptible objects, a claim that Aristotle held specifically against Plato. Instead, perceptible objects are treated *as* geometrical entities, or *as* divisibles or indivisibles, insofar as they share geometrical and arithmetical properties. For instance, if a segment along a straight path is bounded by two extremities, it can be treated as a geometrical line while remaining distinctly as a segment of a road or a wall.[6] In the meantime, Aristotle believed that there are real objects in nature that truly

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[5] Edward Grant, *The Foundations of Modern Science in the Middle Ages: Their Religious, Institutional and Intellectual Contexts* (Cambridge University Press, 1996), 45.

[6] Jonathan Barnes Barnes, ed., *The Cambridge Companion to Aristotle* (Cambridge University Press, 1995), 86-7. Peter M. Distelzweig, "The Intersection of the Mathematical and Natural Sciences: The Subordinate Sciences in Aristotle," *Apeiron* 46, no. 2 (2013): 85–105.

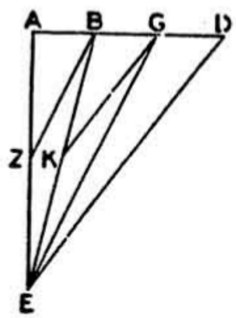

Figure 1 Proposition IV in Euclid's Optics. "of equal spaces located upon the same straight line, those seen from a greater distance appear shorter."

Specifically, node E represents the position of the eye. EA is perpendicular to AG. AB, BG, GD represents three equal distant lengths. EA, EB, EC, ED are visual rays. Z is the midpoint of EA. K is the midpoint of EB. Draw ZB parallel to EG and KG parallel to ED. By proving that angle ZEB > angle BEG, it follows that AB appears longer than BC.

The proof goes as follows: AZ=EZ, AB=BG, so EZ:ZA=GB:BA. BZ>ZA , so BZ>ZE. So angle ZEB> angle ZBE = angle BEG. Therefore, AB appears longer than BC. By a similar proof one can show that CD appears smaller than BC.

Source: Harry Edwin Burton, trans., "The Optics of Euclid," Journal of the Optical Society of America 35, no. 5 (May 1945): 375–372.

instantiate geometrical objects, such as a perfect bronze sphere or isosceles triangle.[7] The Greek sciences provide plentiful examples to illustrate what Aristotle meant. In Euclid's *Optics,* for example, rays of vision were treated as straight lines, while the eye's position with respect to objects' locations was illustrated as a triangle.[8] Theories of visual perception were directly proved by geometrical theorems. (Figure 1)

In Greek music theory, there are similarly extensive use of mathematical language. The Pythagoreans constructed musical scales from arithmetical principles. They discovered that certain numerical relations, e.g., when the strings of an instrument were in the ratio of 4:3 in length, consistently produce consonance. With respect to this discovery, sounds are said to be in ratios with each other. Since higher frequency of motion leads to higher pitches, according to

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A more often used example: When counting a multitude such as a flock of sheep, each sheep is indivisible for the purpose of serving as a unit in counting, regardless whether the sheep, as an animal, is divisible or not.

[7] Jonathan Lear, "Aristotle's Philosophy of Mathematics," *The Philosophical Review* 91, no. 2 (May 1, 1982): 161–92.

[8] Richard D. McKirahan, "Aristotle's Subordinate Sciences," *The British Journal for the History of Science* 11, no. 3 (1978): 197–220.

Euclid, "sounds must be said to consist of *parts*, since they reach their proper pitch by addition or subtraction."[9]

In the sense illustrated above, Aristotle claimed that each mathematical discipline (music, astronomy, etc.) "concern the *same items*" as geometry or arithmetic.[10] Geometrical and arithmetical theorems, which in their own respect only concern pure magnitudes, directly explain phenomena in the subordinate disciplines. In Aristotle's words, "the fact falls under one science, while the reason falls under the higher science (mathematics)."[11] This characterizes the genus-species, universal-subordinate relation between mathematics and its realm of application.

To Aristotle, quantities meant lengths, areas, volumes, sounds, visual rays, times[12], etc.. Each form their own distinct kinds.[13] Magnitudes are not numbers, and arithmetic operations are performed on magnitudes of the same kind. A pair of magnitudes can be in the same ratio as a pair of numbers with the exception of the incommensurables. The theory of proportions in Book V of Euclid's *Elements* (see next section for more details) thus plays the role of interconnecting different realms within mathematics.

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9 Euclid, "Sectio Canonis, Introduction," in *A Source Book in Greek Science*, ed. Morris Raphael Cohen and I. E. Drabkin, 1st ed. (New York: McGraw-Hill Book Co., 1948), 291.

10 Aristotle, *Posterior Analytics*, trans. Jonathan Barnes, 2nd ed., Clarendon Aristotle Series (Oxford: Clarendon Press, 1994), 77b1-10. Distelzweig, "The Intersection of the Mathematical and Natural Sciences."

11 Aristotle. *Posterior Analytics*, 76a10.

12 For Aristotle's conception of time, see Ursula Coope, *Time for Aristotle: Physics IV. 10-14* (Clarendon Press, 2005). Mark Sentesy, "The Now and the Relation between Motion and Time in Aristotle: A Systematic Reconstruction," *Apeiron* 51, no. 3 (2018): 279–323.

13 This was characterized succinctly by Stein in a 1990 article: these specific concrete magnitudes like length, area, volume, time, sound, etc., constitute the "substrate" of number, they were "the bearer of magnitude." "for us, to say that two distinct triangles are equal in area is to say that they have 'the same area.' But on the suggested reading of the Greek terminology, it would be incorrect to speak of 'the area of this triangle': a triangle does not have an area, it *is* an area—that is, a finite surface; *this area* means *this figure*, and the two distinct triangles are two different, but equal, areas." Howard Stein, "Eudoxos and Dedekind: On the Ancient Greek Theory of Ratios and Its Relation to Modern Mathematics," *Synthese* 84, no. 2 (1990): 163–211.

It is unclear whether Euclid himself intended the theory of proportion for things that are not geometrical magnitudes. Book V of Euclid's *Elements,* where the theory of proportion is introduced, is directly continuous from the previous four geometrical books. In the first four books, the quantitative relationships between geometrical objects—lines, angles, rectilinear figures, solids, etc.—have a very concrete foundation, because Euclid painstakingly constructed geometrical figures in various quantitative relationships using a compass and a straight edge without invoking numerical or abstract notions. The compass and the straight edge, as well as their methods of constructing figures, underlie quantitative assertions about geometrical figures arranged in a particular manner—assertions such as "this triangle is double that triangle; this angle can be bisected into these two angles; this rectilinear figure is double the triangle constructed on the same base; or, this rectangle is the sum of two other rectilinear figures."[14] Because Euclid showed that it is possible to construct geometrical figures as same, double, triple, or multiple of each other using the very concrete instruments of compass and straight edge, it makes sense to discuss the quantitative relationships between magnitudes abstractly in Book V: it is a progression from the concrete methods of construction that enable and justify quantitative relationships between geometric figures to the more general and abstract discussion of these various quantitative relationships.

[14] For example, in propositions I.3 and I. 23 in Book I, Euclid showed how equal lines and equal angles can be constructed through compass and straight edge (and through the construction of equilateral triangles). In another two propositions (I.9 &10), he showed how a given rectilinear angle or a given straight line can be cut in half by constructing equilateral triangles through certain relevant points. In several other propositions (I.34, 35, 36, 41), he demonstrated the equality as well as quantitative relationships (e.g., half or double) of rectilinear figures of the same or different kinds, e.g., the equality between a triangle and a parallelogram. In book II, Euclid showed geometrically how a rectilinear figure is the sum of rectilinear figures constructed on its base.

Also see Ian Mueller, *Philosophy of Mathematics and Deductive Structure of Euclid 's "Elements"* (Dover Publications, 1981).

But in Aristotle's writings, there is a sense that the theory of proportion is applicable to all that can be considered magnitudes regardless of how they become quantitative. In *Posterior Analytics*, Aristotle noted that the theory of proportion deals with the universal: "Numbers, lengths, times, solids—do not constitute a single named item and differ in form from one another," he noted, but the theory of proportion proved that ratios alternate unanimously for all quantities: "now, however, *it is proved universally*: what they suppose to hold of them universally does not hold of them as lines or as numbers but as *this*."[15] Insofar as the quantities are considered as "having-such-and-such ratio," the same theories pertaining to ratios apply to the quantities. In this sense, the theory of magnitudes is the universal that governed the subordinate, and these subordinates extend *beyond* geometrical magnitudes like lengths, angles, triangles and cubes, to which Euclid limited himself in Book V of the *Elements*. For Aristotle, the theory of magnitudes also should apply to various magnitudes dealt with by different mathematical disciplines regardless of how they come to be called magnitudes. Hence if magnitudes, abstractly speaking, are represented through the part-whole structure of the geometrical line (see next section), and if all measurable aspects of nature automatically produce quantities subject to the theory of magnitudes, then these various magnitudes are also represented as straight line segments with a part-whole structure.

The classification of sciences in medieval curriculum echoed Aristotle on the status of mathematics and its subject-matter. Scholastic philosophers reaffirmed the idea that mathematics deals with properties abstracted from natural objects by the intellect, as can be found in various accounts. For instance, writing in the 12th century, Hugh of St Victor noted that "[mathematics]

[15] Aristotle. *Posterior Analytics*. 74a18-25. Later he said: "why do proportional alternate? The explanation in the cases of lines and of numbers is different—and also the same: as lines it is different, as having such-and-such ratio it is the same." (Ibid, 99a9-11)

is a branch of theoretical knowledge 'which considers abstract quantity. Now quantity is called abstract when, intellectually separating it from matter or from other accidents, we treat of it as equal, unequal and the like, in our reason alone.'" In analyzing the relationship between pure magnitudes studied by mathematics and physical entities possessing them, the 12th century Domingo Gundisalvo noted that quantitative features such as length, breadth, height are found in all physical things. Pure magnitudes can be abstracted from tangible surfaces or thickness not because mathematical objects "exist or could exist as such in reality, but because the reason often considers actual aspects of things…with respect to reason itself, or, as reason might allow them to be."[16]

The idea that mathematical properties are only separated out from perceptible objects for the purpose of analysis also implies that they inhere in natural objects. As the 17th century natural philosophers elevated the status of mathematics, we see them not only confirming the abstractionist view from Aristotle, but also emphasizing that quantitative features are properties that objects themselves possess rather than products of our interpretation of relationships exemplified through the act of measurement in various ways. For instance, Isaac Barrow's 1664 mathematical lectures notes that the objects of mathematics, magnitude and multitude, are to be understood in two respects: as "abstracted from all matter, material circumstances, and accidents (i.e., are considered generally in themselves, without regard to these Things); or as they *inhere* in some particular subject, and are found conjoined with certain other physical qualities, actions

[16] Edward Grant, ed., *A Source Book in Medieval Science* (Harvard University Press, 1974), 55, 65, 66. Gundisalvo also said that mathematical studies are divided into the continuous and discrete. Geometry and astronomy deal with the mobile and immobile magnitudes such as "[the magnitude] of a tree or a stone"; arithmetic and music deal with the discrete, "like that of a flock or of a people."(Ibid.)

and circumstances."[17] He meant that quantities are either treated by geometry and arithmetic as pure magnitudes, or as a bearer of pure magnitudes, with substantive and qualitative features, be it an object, an "action," or a phenomenon.[18] The difference is merely a matter of perspective: "there is no reason why the doctrine of generals should be separated from the consideration of particulars, since the former entirely includes and primarily respects the latter."[19] More importantly, mathematics ought to apply ubiquitously in the study of nature, because no branch of natural science can exclude the consideration of quantities: "*For magnitude is the common affection of all physical things, it is interwoven in the nature of bodies, blended with all corporeal accidents*, and well-nigh bears the principal part in the production of every natural effect."[20] Since quantities inhere in bodies, accidents, and natural effects, quantification are merely a process of discovery, the reading of the book of nature.

Descartes' vision of "mathesis universalis," a universal science governing all matters regarding measure and order, quickly became well-known. He also identified extension—length, breadth, depth, easily measurable as geometrical magnitudes—as the essence of substance. This is a convenient solution to the question of how mathematics can be known a priori but highly efficacious in application.[21] In the same vein as Aristotle, and in fact using the language of genus

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[17] My italics.

[18] Here are some examples he used: a straight line is a pure magnitude insofar as divisibility, congruence, proportionality are concerned; it can simultaneously be the path of a light ray in optics, or as a suspended string sustaining a weight that determine the momentum of circular motion.

[19] Immediately after this sentence, he also noted that theorems in geometry and arithmetic "descended to the very lowest species" of mathematical disciplines, reaffirming the genus-species relation between pure mathematics and its application. Isaac Barrow, *The Usefulness of Mathematical Learning Explained and Demonstrated: Being Mathematical Lectures Read in the Publick Schools at the University of Cambridge*, trans. John Kirkby (S. Austen, 1734), 19.

[20] Ibid, 21.

[21] The essence of nature was simply identified with extension—the subject matter of mathematics—therefore mathematics "naturally" applied. Lisa Shabel, "Apriority and Application: Philosophy of Mathematics in the

and species, Descartes argued that there is no essential difference between mathematical entities and the objects of their application. Just as the term "extension" does not mean anything distinct and separate from the extended subject, numbers and geometrical figures do not exist apart from the bodies. Numbers should not be distinguished from concrete multitudes, while geometrical entities are "simply a mode of body."[22] It would be a mistake to think otherwise. Thus, whenever numbers are invoked, one must "imagine some subject which is measurable in terms of a set of units." Although the intellect tends to exclusively fix its attention to this abstract set, the conclusion still fundamentally concerns a concrete group of objects being numbered. "Those who attribute wonderful and mysterious properties to numbers…would surely not believe so firmly in such sheer nonsense, if they did not think that number is something distinct from things numbered."[23] Where mathematics is applied, there is some concrete bearer of the mathematical properties, some magnitude; and the notion that no essential difference between mathematical entities and the object of their application exist is not far away from the notion that certain mathematical features (such as the part-whole structure) exist in nature independent of the measuring process.

In the contexts discussed above, the universal-subordinate relationship between pure mathematics and mathematical disciplines, such as astronomy, optics, and other predecessors of mathematical physics, was not merely a methodological description. Not only were mathematics applied historically in these disciplines as a tool to understand nature, philosophers also thought that mathematical properties reside in natural bodies, accidents, and effects. Hence this view

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Modern Period," in *The Oxford Handbook of Philosophy of Mathematics and Logic*, ed. Stewart Shapiro (Oxford University Press, USA, 2005).

[22] René Descartes, "Rules for the Direction of the Mind," in *The Philosophical Writings of Descartes* (Cambridge University Press, 1984), 446/61.

[23] Ibid, 445-6/60-1.

implies a pre-established harmony between mathematical (primarily geometrical) structures and things investigated by mathematical disciplines, namely magnitudes. Philosophers largely neglected the intellectual and practical activities involved in the act of measurement, as well as the assumptions carried into representing non-geometrical objects geometrically. Measurement was to them merely the discovery of quantities or magnitudes. But what is a quantity or a magnitude? This brings us to the second feature of the pre-19th century conception, which is the tendency to project the part-whole structure of geometrical magnitudes onto other quantitative concepts.

1.3. Part-whole Structure and Non-geometrical Concepts

While the term "quantity" had almost never been explicitly defined before the 19th century (it was often simply assumed to include multitude and magnitude, or the discrete and the continuous), quantities were assumed to be constituted by parts or equal units. Geometrical magnitudes, mostly the straight line, served as the archetype through which the abstract term quantity was conceived. On the surface this seems irreproachable, but certain conceptual problems also emerge when the part-whole structure (or the relation between unit, plurality and the whole) is applied to non-geometrical concepts whenever they are considered quantitative. Can two degrees of intensity or two speeds simply be added to each other, giving rise to a third degree, like two lines added to each other to form a new line? 14th century scholastic philosophers grappled with this issue, in a way echoed by late 19th century psychophysicists who aimed to measure sensation (chapter 3). In light of the success of Galilean physics, this philosophical issue was sidelined.

In Book V of *Elements*, Euclid discussed various theorems that can be proved regarding ratios between magnitudes in general. As mentioned earlier, the abstract theorems of magnitudes have a concrete geometric foundation in *Elements*. Euclid noted that a magnitude can be a part of another magnitude when one measures another, in which case the latter is a multiple of the former. By this definition, measurement is explicitly linked to part-whole relation.[24] Two magnitudes of the same kind can be in a certain ratio, which in turn can be the same as the ratio between two other magnitudes of a different kind (although he distinguished the "sameness" between ratios of magnitudes from the "sameness" between ratios of numbers). Notably, even if the theorems in Book V are about ratios between magnitudes in general, which can potentially apply to any specific type of magnitudes (e.g., lengths, triangles, etc.), Euclid illustrated the relationships between magnitudes and between ratios by straight-line segments, as illustrated by proposition 1 in Book V (Figure 2), as well as all other propositions in the same book:

> "Proposition 1: If there be any number of magnitudes whatever which are, respectively, equimultiples of any magnitudes equal in multitude, respectively, whatever multiple one of the magnitudes is of one, that multiple also will all be of all."[25]

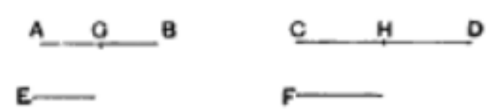

Figure 2 Illustration of Euclid's Proposition 1, Book V. If AB is some multiple of E, and CD is the same multiple of F, then the sum of AB and CD is the same multiple of the sum of E and F. While the proposition itself applies to any general magnitude, the illustration is through straight line segments. (Source: Euclid, The Thirteen Books of Euclid's Elements, Translated from the Text of Heiberg with Introduction and Commentary, trans. Thomas L. Heath, vol. II, 3 vols. (The University Press, 1908), 138.)

Euclid used straight line segments to illustrate the relationship between magnitudes as well as those between numbers (in Book VII). This can be explained by the primacy of geometry

[24] This is specified in the list of definitions in Book V: "a magnitude is a part of a(nother) magnitude, the lesser of the greater, when it measures the greater"; "the greater (magnitude is) a multiple of the lesser when it is measured by the lesser." (Euclid, Definition 1-3, Book V, *The Thirteen Books of Euclid's Elements, Translated from the Text of Heiberg with Introduction and Commentary*, trans. Thomas L. Heath, vol. II, 3 vols. (The University Press, 1908).)

[25] Ibid, 138.

over arithmetic in Greek mathematics as well as convenience—the proofs themselves in fact do not rely on geometrical propositions, i.e., their proofs are not geometric. Nevertheless, Euclid's illustrations might have added to the "naturalness" in representing the abstract notion of quantity by straight-line segments. If the theory of magnitudes is supposed to apply to all things measurable, as Aristotle said, then a glance at Euclid's Book V would suggest that there is a "natural" way of representing quantitative concepts—through geometrical line segments, which have clear part-whole structures.

According to Aristotle in the *Categories*, all quantities are made of parts.[26] As in the Greek mathematical tradition, quantities are either discrete (e.g., number, speech) or continuous (e.g., geometrical figures, time, place), depending on whether the parts have "relative position in reference each to the others." The parts of a straight line either share a common boundary or "each…must lie somewhere, and each can be clearly distinguished." In contrast, the parts of a discrete quantity, like number, are only ordered, but do not have relative positions with respect to each other.[27] More importantly, quantities cannot admit degrees, but can only be said to be as equal or unequal: "what is 'three' is not more truly three than what is 'five' is five." In contrast, qualities (e.g., habits, virtues, natural power, healthiness, sweetness, coldness, color, sensation, etc.) *only* admits variations of degrees and be described in terms of more or less. Action and affection (action and being acted upon), such as heating and cooling, being pleased and being

[26] Aristotle, *Aristotle: The Categories. On Interpretation. Prior Analytics* (Harvard University Press, 1962)., 39. Although all quantities had parts, for Aristotle, the relation between part and whole is not necessarily a numerical relationship. Aristotle's theory of time presented a counterexample: time is a continuous quantity with an order.

[27] Ibid, 6.

pained, also have degrees.[28] Part-whole structure is central to Aristotle's quantity-quality distinction.

In the 17th and 18th century, a number of new quantitative concepts were invented, such as universal time and space, uniform velocity, uniform acceleration, quantity of motion, quantity of matter. They became foundational for modern mechanics. Regardless of how these quantitative concepts were obtained, geometrical magnitudes were the default lens through which they were seen. In *Rules for the Direction of the Mind*, Descartes proposed the notion of the dimensional unit, by which he meant any "mode or aspect in respect of which some subject is considered to be measurable," such as length, breadth, depth, weight, speed, and "countless other instances of this sort."[29] This allowed quantitative concepts to not be restricted by the multitude/magnitude categorization. But he further stressed that quantities of any dimension should be represented as a straight line or a linear geometrical magnitude: "Rectilinear and rectangular surfaces, or straight lines…must serve to represent sometimes continuous magnitudes, sometimes a set or a number. To find a simpler way of expressing differences in relation would be beyond the bounds of human endeavor."[30] Meanwhile, he reaffirmed the classical notion of quantity and measure: measurement is a process where "we regard the whole as being divided into parts."[31] Such parts can even be intellectual. (Equal) units are "basis and foundation of all the relations," the "common nature" of all things being compared, and "if no

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[28] Ibid, VIII-IX.

[29] Descartes, "Rules for the Direction of the Mind." 447. In "Apriority and application: philosophy of mathematics in the modern period," Shabel points out that Descartes invented dimension as a representational format so that quantities could be more flexibly treated by mathematics.

[30] Ibid, 452.

[31] Ibid, 448.

determinate unit is specified in the problem, we may adopt as unit either one of the magnitudes already given."[32]

This view was reiterated in Isaac Barrow's mathematical lectures. Barrow did in fact mention cases of indirect measurement. For instance, time is not directly measurable but must be measured by "the space… run over equably with a certain velocity by some noted moveable body… for it cannot be known how much time is passed, but by estimating the quantity of such a space." In astronomy, indirect inferences based on geometrical laws were used: the earth's diameter for example, was used as a measure of the distance between distant stars.[33] Both cases involved measurement which "expressed its [one quantity's] relation to other known quantities," and this was in fact how their quantitative values were known. Strikingly, while this line of thought in the late 19th century would lead to either the conclusion that measurement is in most cases indirect measurement, or the conclusion that time intervals are never directly compared for time to be measured, Barrow reached a conclusion in the opposite direction. He wrote: strictly speaking, measurement is still defined by enumerating the parts contained in a whole: "a measure is *more strictly* taken for a magnitude, which some number of times taken does constitute and compose another magnitude, or which being some number of times taken from another magnitude leaves no reminder, but entirely exhausts it… a *measure* thus taken will never exceed the thing measured, but either is equal to it, or some aliquot part of it, i.e., which being sometimes repeated according to any number composes the whole."[34] Regardless of how

[32] Ibid, 450.

[33] Barrow, *The Usefulness of Mathematical Learning Explained and Demonstrated*.

[34] Ibid, 261-2.

measurement actually proceeded, he imagined the process of measurement to be the counting of equal units to exhaust the whole.

In Isaac Newton's *Principia*, the relationship between parts and the whole as the defining feature of quantities was not explicitly mentioned but taken for granted.[35] Although he defined absolute time separately, in some of his actual theorems, he used space to measure times.[36] The part-whole structure was not only the precondition of a metric for relative space and time, given through the ratio between two parts of time or space. It also allowed Newton to extrapolate laws of motion drawn from local phenomena on a universal scale. One of the four rules of reasoning in the *Principia* is that

> "the extension, hardness, impenetrability, mobility, and force of inertia of the whole arise from the extension, hardness, impenetrability, mobility and force of inertia of each of the parts; and thus we conclude that every one of the least parts of all bodies is extended, hard, impenetrable, movable, and endowed with a force of inertia. And this is the foundation of all natural philosophy."[37]

The principle that properties of the whole are derived from the properties of parts allowed Newton to define quantity of motion as "velocity and quantity of matter conjointly." As he explained: "the motion of the whole is the sum of the motions of all the parts. And therefore in a body double in quantity, with equal velocity, the motion is double; with twice the velocity, it is quadruple."[38] As the quantity of matter (mass) serves as a universal measure of the "number of parts" contained by matter (this measure of matter was also defined through part-whole relation),

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[35] It seems that Newton's definitions for absolute time and space does not involve a metric with a given unit. But the comparison between parts of space and parts of time gives rise to the metric for relative space and time.

[36] See Barbour, *The Discovery of Dynamics*.

[37] Isaac Newton, *Newton's Principia: The Mathematical Principles of Natural Philosophy*, trans. Andrew Motte (New-York : Published by Daniel Adee, 1846), http://archive.org/details/newtonspmathema00newtrich.

[38] Ibid., Definition III. Ibid, 73.

the quantity of motion is naturally the sum of all the motion possessed by the parts.[39] For my purpose here, it is worth pointing out how theoretical concepts that were either measured indirectly, or only theoretically postulated and in a way substantiated through quantitative laws that predicted motion correctly at the time, they were quite naturally conceived through parts and the whole by the authors I have mentioned. To state a quantitative law expressing an invariant relationship seem to entail that there is some quantity—some concrete bearer of the magnitude with a part-whole structure—even if such quantity ultimately only stands for a conceptual relationship.[40]

1.4. Conceptual Problems in Representing Motion Through Part-Whole Structure

Geometrical representation was an essential tool for modern mechanics in its early days. By favoring geometrical representation, the structure of geometrical magnitudes was projected onto new quantitative concepts. Descartes' call to represent all quantities using straight lines and rectilinear figures was indeed carried out in Galileo's mathematics of motion. In *Dialogue Concerning the Two New Sciences*, Galileo represented the concepts of time, speed, and acceleration by straight lines, rectangles, and other rectilinear figures. Granted, there is an intuitive plausibility in thinking of distances, temporal intervals (if, for instance, equal times are measured by the equal weights of water in a water clock), and speeds that have gone through uniform increase or decrease in terms of equal increments of straight-line segments. But nowhere did Galileo question whether by representing these concepts using the straight line and rectilinear

[39] Ori Belkind, "Newton's Scientific Method and the Universal Law of Gravitation," in *Interpreting Newton: Critical Essays*, ed. Andrew Janiak and Eric Schliesser (Cambridge University Press, 2012).

[40] This part-whole relation served as the condition of numerical representation, since Newton defined numbers as "not so much a multitude of unities, as the abstracted ratio of any quantity to another quantity of the same kind, which we take for unity." Isaac Newton, *Universal Arithmetick: Or, A Treatise of Arithmetical Composition and Resolution*, trans. Joseph Raphson (London, Printed for W. Johnston, 1769), 1.

figures, one would also bring in additional assumptions regarding how these concepts should be understood. Is a speed or a time composed of smaller speeds? Is adding two speeds or times like adding two line-segments? Instead of engaging with these issues, Galileo wrote that uniform addition of speeds is simply an insight taking from "the habit and custom of nature herself," as he employed "only those means which are most common, simple and easy." What he meant is this: "just as uniformity of motion is defined by and conceived through equal times and equal spaces (thus we call a motion uniform when equal distances are traversed during equal time-intervals, so also we may…through equal time-intervals, conceive *additions of speed as taking place without complication*."[41] In retrospect, the reason why motion was successfully mathematized cannot be that speed is composed of smaller, additive speeds—such notion simply makes no sense. But Galileo's quote suggests that he had no alternative to explain why motion can be mathematized other than that speed must be an additive quantity with parts just like spatial distances or geometric line.

Galileo's mathematization of motion using geometric means was indebted to a broader attempt to mathematize qualitative intensities by scholastic philosophers, and it had not been uncontroversial from the start that the intensity of motion, or any other quality, should be regarded as having additive parts. We recall from Aristotle's *Categories* that quality does not admit of parts, only degrees. Aristotle did not specify in what way the variation of qualities should take place, which led to different interpretations among scholastic philosophers. Meanwhile, since Aristotle regarded both variations of qualities and local motion as change or movement, scholastic philosophers also recognized an analogy between varying speed and the varying qualitative intensity. But how precisely the variation of qualitative intensities should be

[41] Galileo, *The Discourses and Mathematical Demonstrations Relating to Two New Sciences,* 197.

conceptualized was one of hotly debated issues among scholastic philosophers since the 12th century.[42] Two dominant theories from the 14th century stated the following: 1. each degree of quality succeeds one another at every instant and destroys the former degree completely, because each degree of quality is indivisible and cannot be said to have parts (the succession theory); 2. In the same subject, degrees of quantity are simply additive, composed of homogeneous parts (the addition theory). A group of Oxford scholars called Oxford Calculators embraced the second view—the addition theory, namely that degrees of intensity are no different from extensive magnitudes. They also treated concepts of speed and acceleration, from the newly invented uniform motion and uniformly accelerated motion, just as additive quantities and succeeded in proving the mean-speed theorem, one of the crucial theorems in Galileo's own mathematical physics.[43] Meanwhile, the geometric representation in Galileo's style was first invented by Nicole

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[42] For instance, Thomas Aquinas claimed that some qualities, like health or motion, goes through intension and remission through the addition and subtraction of parts, while others, like heat, color, or the God-given charity, vary only in the sense that the subject participates in these unchanging qualities by varying extent.

See Marshall Clagett, "Richard Swineshead and Late Medieval Physics: I. The Intension and Remission of Qualities (1)," *Osiris* 9 (January 1, 1950): 131–61, https://doi.org/10.1086/368527; Elzbieta Jung, "Intension and Remission of Forms," in *Encyclopedia of Medieval Philosophy: Philosophy Between 500 and 1500*, ed. Henrik Lagerlund (Springer Science & Business Media, 2010).

[43] Clagett points out that although propositions regarding "uniform speed" appear in the writings of the Greeks, they do not assume the concept to be a magnitude but focus on the proportionality between distance and time. Medieval philosophers first used the notion of "the proportion of the movements (i.e., velocities)" and applied the theory of proportions to it. In this sense the quantity of uniform speed was invented in the 13th and 14th century. Marshall Clagett, *The Science of Mechanics in the Middle Ages* (University of Wisconsin Press, 1959), 218.

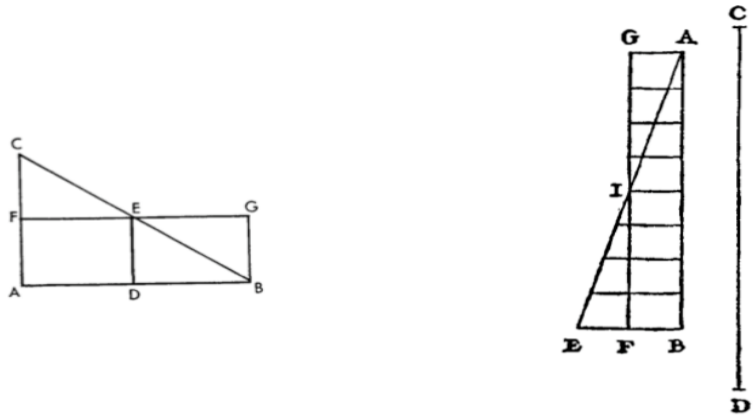

Figure 3 (Left) Nicole Oresme represented the intensity of a quality as a straight-line segment erected on a point. (Right) Galileo's reproduction of the mean speed theorem in Dialogues, arguing that the addition of speeds should be considered straightforward and unproblematic.
Oresme spoke directly of the ratio of intensities of qualities as the ratio of straight-line segments and applied geometry directly to these intensities. This figure (left) proves that "uniformly disuniform" quality (represented by ABC) is "of the same quantity" as the "quality of the same or equal subject that is uniform according to the degree of the middle point of the same subject."
Similarly, Galileo represented the uniformly "increasing values of speed" in terms of the horizontal straight lines in AEB and proved the mean speed theorem in the same manner as Oresme did for "the quantity" of "uniformly disuniform" quality.
Source: Oresme, *Nicole Oresme and the Medieval Geometry of Qualities and Motions*, 409; Galileo, *The Discourses and Mathematical Demonstrations Relating to Two New Sciences*, 173.

Oresme through the "latitude of forms."[44] Oresme argued that "every intensity which can be acquired successively ought to be imagined by a straight line perpendicularly erected on some point of the space." The intensity could belong to color, speed, hotness or coldness, visual

[44] Edith D. Sylla, "Medieval Concepts of the Latitude of Forms: The Oxford Calculators," *Archives d'histoire Doctrinale et Littéraire Du Moyen Âge* 40 (1973): 223–83. Jung, "Intension and Remission of Forms." For more on the topic, see Grant, *The Foundations of Modern Science in the Middle Ages*, 100. Clagett, "Richard Swineshead and Late Medieval Physics."

powers, or anything else capable of varying continuously.[45] The ratios between intensities at different points would simply be the ratios between the straight lines. This analogy was supposed to apply to motion as well: for a point moving along a distance, its intensity of velocity was represented as the altitude of straight lines erected along every point it traversed. Using this representational scheme, Oresme proved the mean speed theorem geometrically, in almost the exact same manner as Galileo did three centuries later (Figure 3).

Before scholastic philosophers first began to treat motion along with other qualitative intensities mathematically, it was not unproblematic to assume that speed or other qualitative intensity is acquired through the addition of parts: objections arose almost at the same time as those quantitative treatments. Perhaps the success of the Oxford Calculators and, later, Galileo, overshadowed issues surrounding these foundational concepts, namely what had been lost or presupposed with certain representational forms. Thanks to the ground paved by his scholastic predecessors, Galileo could declare that the addition of speed should be considered as unproblematic. By ignoring such philosophical problems as *how* mathematization is possible when it cannot be explained by the part-whole structure of magnitudes, Galileo could proceed with his mathematical physics. On the other hand, it is not surprising that the unresolved philosophical issues would resurface in later centuries.

1.5. Kant's Construction of Quantities

In the previous section, I argue that the part-whole structure characteristic of geometrical magnitudes was the primary form through which quantitative concepts were understood. This leads us to Immanuel Kant, the most influential voice on epistemology in 19th century German-

[45] Nicola Oresme, *Nicole Oresme and the Medieval Geometry of Qualities and Motions: A Treatise on the Uniformity and Difformity of Intensities Known as Tractatus de Configurationibus Qualitatum Et Motuum*, ed. Marshall Clagett (UMI, 1968), 167.

speaking philosophy. At the end of the 18th century, the Konigsberg philosopher set the goal of explaining the possibility of knowledge from the point of view of human subjectivity. In the *Critique of Pure Reason*, Kant first identified the forms of receptibility, through which the external world is immediately received by us, which Kant called the pure intuition of space (all possible forms of shape, dimension and spatial relations) and time (all forms of succession, order and simultaneity). External objects cannot possibly be apprehended without being represented in space, while inner states cannot be apprehended without being related through time.[46] Second, Kant identified the pure concepts of understanding, or the categories, as the forms of all concepts pertaining to objects—only when concepts of objects are formed in accordance to these categories can they enter valid judgments.[47] The concepts apply to the content of intuition, namely the (pure and empirical) intuitions in space and time. It is through this application that experience and cognition become possible at all: the immediate impressions presented in space and time must be subsumed under the concepts to be meaningful, and concepts must be instantiated through intuition in order to produce objective knowledge. Both the forms of intuition and the categories are a priori, meaning that their origin lie in the subject, and are imputed to our experience of the external world.

For Kant, mathematics constructs concepts in a priori intuition, which means that it exhibits "a priori the intuition corresponding to it [the concept]."[48] The classical example used in Transcendental Aesthetic is that the concept of "5+7" does not contain the concept of "12," and one must invoke intuition using the fingers of one's hand, the intuition of the successive addition

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[46] Immanuel Kant, *Critique of Pure Reason* (Cambridge University Press, 1999), A23/B38.

[47] Paul Guyer, ed., *The Cambridge Companion to Kant* (Cambridge University Press, 1992).

[48] Kant, *Critique of Pure Reason,* A714/B742. Also see Michael Friedman, *Kant's Construction of Nature: A Reading of the Metaphysical Foundations of Natural Science* (Cambridge University Press, 2013).

one by one, to obtain the sum. Similarly, in (Euclidean) geometry, axioms and theorems must be demonstrated by drawing the figures themselves in accordance with their definitions; they cannot follow from the conceptual definitions themselves: "I cannot represent to myself any line, no matter how small it may be, without drawing it in thought."[49] Geometry therefore demonstrates different possible ways of synthesizing (i.e., putting together; combining) the pure intuition of space.

Instead of attempting to explain Kant's views on mathematics fully, I examine here only the presuppositions concerning the use of the concept quantity in the First Critique. Quantity is among the four categories (the others were quality, relation and modality) applied to the content of intuition, whose origin lies in the understanding. Under its heading are three categories—unity (Einheit), plurality (Vielheit), totality (Allheit).[50] These categories are most clearly illustrated by the structure of geometrical magnitudes, they also correspond with Kant's definition of the extensive magnitudes: those magnitudes whose representation of the whole is achieved through the successive synthesis of the homogeneous part by part. The association with measurement is stronger in *Prolegomena*, where these three subcategories are rephrased as measure (das Mass), the magnitude (die Grösse) and the whole (das Ganze). Furthermore, quantity is a more primitive concept than number. As Kant defined number as the "schema of quantity (quantitas)."[51] What he meant by "schema" is this: intuition must be subsumed under a specific concept according to specific procedures. The schema of a concept is a "universal procedure" for generating a pure image in the imagination to illustrate the concept. So while the pure image generated in pure

[49] Kant, Immanuel. *Critique of Pure Reason*, A162.
[50] "Totality is nothing other than plurality considered a unity." Kant, Immanuel. *Critique of Pure Reason*, B111.
[51] Ibid, B182.

intuition to illustrate the concept quantum (a concrete magnitude) is space (a spatial magnitude), the procedure that generated this image is through number.[52] Number is intertwined with the "the homogeneous in intuition" to produce quantitative cognition; it is "the representation that summarizes the successive addition of one (homogeneous units) to another." In this sense, natural numbers are central to Kant's understanding of numbers.[53]

The concept of quantity is applied to both the empirical and pure intuitions. In Kant's theory, quantitative cognition is a part of the cognition of the objects. Specifically, Kant made this point in the following passage:

> "all appearances contain, as regards their form, an intuition in space and time, which grounds all of them a priori. They cannot be apprehended, therefore, i.e., taken up into empirical consciousness, except through the synthesis of the manifold through which the representations of a determinate space or time are generated, i.e., through the composition of that which is homogeneous and the consciousness of the synthetic unity of this manifold (of the homogeneous). Now the consciousness of the homogeneous manifold in intuition in general, insofar as through it the representation of an object first becomes possible, is the concept of a magnitude (Quanti)."[54]

With this passage, he concluded that all appearances are extensive magnitudes, those "cognized through successive synthesis (from part to part) in apprehension."[55] What he meant in this paragraph is that the representation of an object is not possible unless the mind synthesizes (again, puts together, or combines) its spatial and temporal forms to form a coherent representation. The mind synthesizes the spatial and temporal form of any *particular* object or phenomenon in empirical intuition in the same way as it synthesizes the parts of pure space and

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[52] According to this definition, one can argue that although it is not possible to have an image of a large number such as 1000, it is possible to apprehend this number as the method of its enumeration, the successive addition of units up to 1000 times and the grasping of such aggregate as one.

[53] Ibid, B180-2.

[54] Ibid, B202-3.

[55] Ibid, B204.

time, through the successive addition from part to part.[56] Now the synthesis of the pure intuition of space and time through conjoining or adding homogeneous parts, by definition, exhibits magnitudes, i.e., the composition of the homogeneous. Such synthesis follows the stages from unity to plurality to totality. As a result, any part of space and time constitutes a magnitude, and magnitudes are ingrained in all appearances—all representations of the external object and phenomena—as a constituent part of the appearances. This took Descartes' view that quantitative structure is ingrained in natural objects one step further by locating an explanation for it in cognition, in our mode of perceiving the external world. The three subcategories of quantity have a strong connotation of the part-whole structure, demonstrated paradigmatically by geometrical magnitudes. Applying mathematics means, primarily, applying this structure.

But there is another kind of magnitudes in the First Critique, called the intensive magnitude. As opposed to extensive magnitudes whose "representation of the parts makes possible the representation of the whole (and therefore necessarily precedes the latter),"[57] the intensive magnitudes are those "which can only be apprehended as a unity, and in which multiplicity can only be represented through approximation to negation = 0."[58] This definition initially comes up in Kant's discussion of sensation: sensations have degrees, and this degree has any arbitrary magnitude above zero. The apprehension of this degree, or magnitude, is "not successive but instantaneous."[59] In other words, although intensive magnitudes range continuously from zero to infinity, each magnitude can only be apprehended in an instant,

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[56] "As intuitions in space or time they [appearances] must be represented through the same synthesis as that through which space and time in general are determined." Ibid, B203.

[57] Ibid, A162-3.

[58] Ibid, A169.

[59] Ibid.

instead of proceeding from part to part. Between a given degree and null, there is a "continuous nexus" of intermediate degrees. This applies to not only sensation but also the magnitude of gravity (or "the moment of gravity" apprehended instantaneously[60]), color, and velocity. By invoking the notion of the intensive magnitude, Kant seems to have the views of Descartes in mind in juxtaposition, because the undeniable cognition of intensive magnitude illustrates that extension alone is inadequate in describing the properties of matter, e.g., the same space can be illuminated by different degrees of light, or the same volume of matter can weigh differently.[61]

Notably, the distinction between extensive and intensive magnitudes not only harkens back to Aristotle's distinction between quantity and quality, it also harkens back to the scholastic discussion in the 14th century about the intension and remission of forms. After all, Kant could not accept that qualitative intensities could simply be represented like geometrical magnitudes with parts. The emphasis that each intensive degree must be apprehended in an instant is reminiscent of the medieval "succession theory," the view that each degree of intensity replaces the next one held by some scholastic philosophers in the 14th century. Although Kant did not argue that intensities would replace one another, the similarities between these two lines of thought are prominent, as he claimed that the apprehension of an intensity is "not successive, but instantaneous," and between any two different degrees, there are an "infinite gradation of every smaller degrees," each of which is also to be apprehended in an instant.[62]

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[60] Ibid.

[61] Tim Jankowiak, "Kant's Argument for the Principle of Intensive Magnitudes," *Kantian Review* 18, no. 3 (November 2013): 387–412, 406.

[62] Kant, *Critique of Pure Reason*, A169, B214.

Since intensive magnitudes still invoke the synthesis of the homogeneous, they are still magnitudes—although the synthesis is not from part to part, but a "coalition."[63] Although the categories under quantity (unity, plurality, totality) clearly correspond with the part-whole structure of extensive magnitudes, and although Kant stressed that each intensive magnitude is not composed of parts but can only be grasped as a whole, he still applied those categories to the intensive magnitude. Unity is exemplified in the fact that each intensive magnitude could only be grasped as a unity; plurality in the "approximation towards negation = 0,"[64] presumably in the difference, or heterogeneity, between any two intensive magnitudes arbitrarily close. If we illustrate the intensive magnitude with degrees of sensation like pain or pleasure, this makes sense. Nevertheless, the notion of number, the universal procedure of successive addition of part to part, then by definition does not apply to intensive magnitudes; furthermore, the pure image of quantum, space, clearly does not reflect intensive magnitudes. Finally, it is not easy, if at all, to see how unity and plurality, identified with measure (Mass) and magnitude (Grösse) in *Prolegomena*, retains its connection to measurement when applied to intensive magnitudes. This is especially notable in his treatment of motion in *Metaphysical Foundations of Natural Sciences*.

The intensive magnitude played a crucial role in Kant's effort to reconcile his belief that speed cannot be represented through the addition of parts (as Galileo might claim) with the fact that motion can be mathematized. For Kant, the latter requires clarifying what kind of *magnitude* is involved, just as the empirical validity of the Pythagorean theorem in actual measurement requires the definition of triangles and their properties in Euclid's *Elements*. His answer to what kind of mathematical magnitude is involved in motion is that speed is an intensive magnitude. In

[63] Ibid, B202.

[64] B210.

Metaphysical Foundations of Natural Science, he dedicated the first chapter to explaining how speed can be additive without being composed of parts of smaller speed, namely how it can be constructed as a "quantum" in a priori space and time, through which the composition of the homogeneous is made immediately intuitive. This is what Kant meant when he said in the first chapter of *Metaphysical Foundations* that he intended to provide a "pure mathematical" solution to the quantification of motion.[65] Since geometrical magnitudes served as the archetype of magnitude for Kant and many others, Kant similarly required the same kind of intuitive immediacy of composition for all that qualify as magnitudes. This means that two magnitudes of motion must be composed into a third in both direction and speed without referring to empirical observation or experiment, or any measuring technique, but in a different way from the composition of lengths and angles through parts. To demonstrate that motion is a quantum in a priori intuition is the precondition of even considering the relationship between forces and motions.[66]

The simplest case of adding two motions of the same point is when these two motions are along the same line, in the same direction. From our contemporary point of view, if we want to add two speeds v_1m/s to v_2m/s, we would simply add the two scalars to get $(v_1 + v_2)$m/s, namely in the same second during which the object previously covered v_1 meter, it now covered $(v_1 + v_2)$ meters. If we want to represent this sum geometrically like Galileo, we would simply combine the line segment v_1 meters and the line segment v_2 meters and call the geometrical sum

[65] "The connection of motions by means of physical causes, that is, forces, can never be rigorously expounded, until the principles of their composition in general have been previous laid down, purely mathematically, as basis." Also, "…"phoronomy has first to determine the construction of motions in general…a priori solely as magnitudes." Kant, Immanuel. *Metaphysical Foundations of Natural Science*, 486-7 [original paging], 21-2.

[66] For a more detailed analysis, see Daniel Sutherland, "Kant on the Construction and Composition of Motion in the Phoronomy," Kant: Studies on Mathematics in the Critical Philosophy, December 22, 2017.

of these two line segments the new speed, indicating the distance the object would now cover in a second. As we recall from Galileo, velocity increase was represented as simply an increase in the length of the straight-line segment, since the quantity of velocity was represented geometrically or spatially in an unproblematic way. But Kant denied this method. His objections go as follows (Figure 4)[67]:

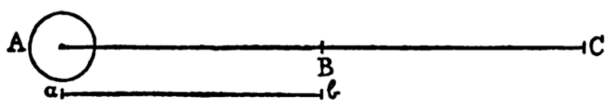

Figure 4 Kant's illustration explaining how two speeds in the same direction cannot be added spatially. Source: Immanuel Kant, Kant's Prolegomena, and Metaphysical Foundations of Natural Science, 162.

Let one represent the first speed by the geometrical line AB, and the second speed by the geometrical line ab, and ab equaled the line BC in length, and for convenience let it be assumed that the geometrical segments AB, BC and ab were all equal. Then each of these three line segments represented an equal speed: AB/unit time, BC/unit time, ab/unit time.[68] Then the sum of two speeds AB/unit time and BC/unit time, which was equivalent as the sum of AB/unit time and ab/unit time, could not be AC/unit time. Why? Because BC was "not traversed in the same time as the line ab" if the object travelled with a speed AC/unit time. In other words, doubling the speed would mean that distance AB was traversed in ½ unit time, and distance BC or ab would also be traversed in ½ unit time. But AB/½ unit time was a different speed from the original addend—AB/unit time—just as ab/½ unit time represented a different speed from ab/unit time. If speed AC/unit time was the sum of those two original speeds, it was not intuitively clear that the original addends composed into the sum, AC/unit time, since neither

[67] Images on the following pages are from Immanuel Kant, *Kant's Prolegomena, and Metaphysical Foundations of Natural Science*, trans. Ernest Belfort Bax (London : G. Bell and Sons, 1883).
[68] Kant did not use "unit time" but he used AB, BC and ab to represent spaces traversed "in equal times." Ibid, 490, 26.

was distinguishable in the sum.[69] One would have to refer to an empirical test to show that this was indeed the case, which violated the condition that the mathematical foundation of motion must be constructed in a priori intuition. Furthermore, if one had referred to the parallelogram of forces that showed how one motion was affected by another, then this construction was mechanical, not mathematical, which "should only make intuitive what the object (as quantum) *is to be*, not how it may be *produced* by nature or art by means of certain instruments and forces."[70] But Kant stated another reason why he refused to represent speed as spatial magnitudes:

> "something is assumed here that is not obvious in itself—namely, that two equal speeds can be combined in precisely the same way as two equal spaces—and *it is not clear in itself that a given speed consists of smaller speeds, and a rapidity of slownesses, in precisely the same way that a space consists of smaller spaces*. For the parts of the speed are not external to one another like the parts of the space, and if the former is to be considered as a quantity, then the concept of its quantity, since this is *intensive*, must be constructed in a different way from that of the extensive quantity of space."[71]

Basically, Kant denied that speed is composed of smaller speeds, i.e., that its "parts" are smaller speeds, just as length or angle are composed of smaller lengths and angles. Therefore, speed cannot simply be represented as a geometrical or extensive magnitude, but can only be conceived as an intensive magnitude, like a degree of a sensation. The composition of speeds does not invoke any numerical notions, nor the concatenation of parts, but must be the "coalition" of two intensities, each of which can only be grasped as a unity, and this coalition must be self-evident in a priori intuition. If this is not difficult enough, the composition must also

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[69] As Kant also noted, "geometrical construction requires that one quantity be the *same* as another or that two quantities in composition be the *same* as the third, not that they produce the third as causes, which would be mechanical construction." (Ibid, 493, 29.) In Sutherland's words, the difficulty here was the "unrepresentability" of the "identity of parts of magnitude with the whole magnitude they compose." Sutherland, Daniel. "Kant on the Construction and Composition of Motion in the Phoronomy."

[70] Kant, Immanuel. *Metaphysical Foundations of Natural Science*, 495, 31-2.

[71] Ibid, 493, 28-9. My italics.

conform to the categories of quantity defined in the First Critique—unity, plurality, totality—a criterion derived, ironically, from magnitudes that have a clear part-whole structure.

Kant's solution to this difficult task goes as follows: to think of the composition of two motions as occurring in two "relative spaces," namely two reference frames. Consider the point object moving with a speed in one reference frame. Consider a second reference frame going in the opposite direction with the same speed with respect to the first reference frame. Then viewed from the second reference frame, the point-object is travelling with double speed. From perhaps God's point of view, both addends are simultaneously identifiable in the sum and the identity of the sum and the addends was intuitively clear. Similarly, when considering the composition of two motions comprising an angle, instead of applying the parallelogram of motion directly, Kant proposed to think through the following scenario (Figure 5):

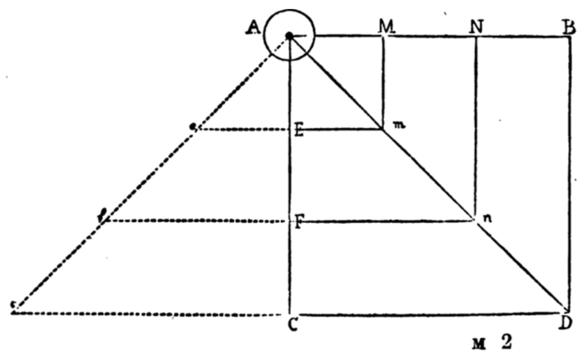

Figure 5 Kant's illustration for the addition of two speeds in directions that comprise an angle. (Ibid, 163.).

Instead of composing AB/unit time and AC/unit time into AD/unit time, consider an object moving with AC/unit time with respect to only one reference frame, let us say in the absolute space. Consider a second reference frame moving with a speed equal in magnitude with AB/unit time, but in opposite direction of AB, with respect to the absolute space. So at the time when the object reaches point E, from the perspective of the second reference frame, which itself is at e, in absolute space, it would seem as if the object has traversed a distance described by Em; when the object reaches F, from the perspective of the second reference frame, which itself is at f, it would seem as if the object has traversed a distance described by Fn, and so on so forth. This allows motion AC and BA to be composed into motion AD.

To correspond with the subcategories of quantity, Kant reinterpreted unity as the "unity of line and direction," plurality as "plurality of directions in one and the same line," and totality as the "totality of directions, as well as lines." Each corresponds with the cases of the composition of two motions in the same direction along the same line, in different directions along the same line, and along different lines.[72] This again deviates from the application of unity, plurality, totality to intensive magnitudes in the First Critique, where unity is interpreted through the fact that each intensive magnitude can only be grasped as a unity, while plurality is interpreted through "approximation to zero." But more crucially, under such interpretation of the subcategories of quantity, the connection to measurement is lost. How precisely did Galileo, for instance, proceed from experimental phenomena to its mathematical description, then? This is not explained by Kant's demonstration in "Phoronomy": in fact, we know for sure that Galileo did not go through the same kind of reasoning but simply assumed that speed was additive. Kant was motivated to build a "pure" mathematical foundation of motion (by constructing it as a "quantum") in order to explain the apodictic certainty of the mathematical laws of motion (by proving its synthetic a priori nature), but paradoxically, his solution did not explain what exactly in the experimental phenomena was in fact mathematically described by founders of mathematical physics like Galileo, or what exactly in such phenomena did conform to mathematical laws (e.g., the mean speed theorem).

[72] Ibid, 495.

Why did Kant go through so much trouble to demonstrate the composition of motion as two intensive magnitudes? What Kant tried to reconcile were the conflicting beliefs that it makes no sense to describe concepts in dynamics by a part-whole structure, and yet dynamics had been successfully mathematized. The task was further complicated by his idiosyncratic definition of quantity grounded in a priori intuition as well as the assumption that mathematical description entails a bearer of magnitude with an additive procedure. If one considers how concepts of motion in fact became mathematized, historically, Galileo and Newton simply did not have anything like Kant's composition of motion in mind when they laid the foundation for mechanics: they simply took the part-whole structure of concepts like distance, time, speed, etc., for granted. The real question, as mentioned above, is then what in the experimental phenomena corresponded with the geometrical representation by Galileo or Newton, so that mathematical derivations were able to predict the experimental outcome. Such miraculous effectiveness of mathematics in empirical sciences motivated Kant to provide an a priori explanation in the first place, but because the measurement process was never part of his concern and Kant focused on the search for an additive "magnitude," in the end we still do not have an account of how motion was actually quantified that is historically and epistemologically convincing.[73]

In the meantime, we recognize that the difficulty stemmed from the deeply grounded belief that quantification entailed directly additive magnitudes and the tendency to characterize magnitudes in terms of part-whole structure analogous to that of geometrical, spatial, and

[73] Also, his construction could not extend to other concepts in mathematical physics. Regarding the quantity of matter, Kant argued that the quantity of matter could *not* be constructed as a mathematical magnitude, but only "manifests itself in experience" through the quantity of motion at a fixed speed. Neither could it be determined by "an aggregate of parts" since matter was "infinitely divisible." Finally, it was not possible to compare every chunk of matter with every other matter through extension if their respective density was unknown. Quantity of matter was not constructible partly also because the concept of matter already contained causality and force. See Friedman, Michael. *Kant's Construction of Nature*, 292.

extensive magnitudes. This conception simply became inadequate in explaining how concepts in empirical sciences were mathematized. Into the 19th century, the increasing use of indirect measuring techniques meant that many quantitative concepts (in particular, those in electricity and magnetism, heat, and the measurement of other "imponderables") were defined operationally and through their mechanical effects; their simply could not meet the immediate visual intuitiveness that Kant required to determine quantities and their composition. Hermann von Helmholtz, for example, regarded the unit as posterior to measurement, additivity as operationally defined, geometrical measurement as no more fundamental or inherently certain than any other form of measurement.

1.6. Mechanical or "Absolute" Units in 19th Century Sciences

The shift from 18th and early 19th century natural philosophy to late 19th century exact sciences was characterized by a number of trends: the quantitative spirit came to predominate, scientific theories became inseparable from technology and instrumentation, a common set of research topics and experimental protocols began to be shared across national boundaries, specialized institutions and professional communication channels emerged, among many others.[74] Problems related to measurement were embedded in these developments. To name one of them: the refinement of experimental instruments allowed the discovery of new phenomena that became quantifiable due to new experimental designs. For example, the torsional force between conductors charged with static electricity could be formulated through an equation analogous to the law governing Newtonian central force, only thanks to an experimental setup that created conditions hardly existing in nature. As the historian John Heilbron articulated: "the

[74] Jed Z Buchwald and Sungook Hong, "Physics," in *From Natural Philosophy to the Sciences: Writing the History of Nineteenth-Century Science*, ed. David Cahan (University of Chicago Press, 2003).

instruments not only made the measurement, they also produced the phenomena to be measured."[75] Sophisticated instruments, such as protractors used in geodetic surveys or astronomical observations, existed long before the 19th century, but in those cases angles measured other angles, lengths measured other lengths, and durations measured other durations.[76] Magnitudes measure other magnitudes of the same kind through geometrical laws. The same could not be said for quantities emerging from experimental sciences of the late 18th and 19th century, such as charge, potential, current intensity, heat and so on.

How did the traditional understanding of quantity in terms of part-whole structure, homogeneity and units, and so on, reconcile with these new quantitative concepts? One solution was to regard measurement simply as reducibility to length, mass and time, which fundamentally did not challenge the conception of quantity based on geometrical or extensive magnitudes such as length and mass. In other words, the extensive magnitudes, analogous to the geometrical archetypes, continued to serve as the foundation of measurement. As mechanics had been the paradigmatic model of mathematical physics since Newton's *Principia*, various new phenomena that later became distinct subfields of physics were initially investigated in terms of their mechanical effects, proceeding from the study of forces—between assumed "particles" in electricity, magnetism, heat, optics, and so on[77]—and dynamics of macroscopic experimental

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[75] Heilbron, *Weighing Imponderables and Other Quantitative Science around 1800,* 3.

[76] Ibid, 65.

[77] The imponderables were thought to carry properties or forces that explained a number of newly discovered electrical, magnetic, or thermal phenomena. For instance, before the acceptance of the field theory of electromagnetism in late 19th century, it was believed that electricity consisted of one or two "rare, subtle, highly elastic" fluids that easily pervaded conducting substances like metals but was obstructed in non-conductors. The imponderables "eluded the universal pull of gravity or escaped from the pressure of terrestrial tourbillion." Just as the distribution of electric fluid in conductors explained their attractive and repulsive forces, the caloric residing in bodies explained such phenomena as the latent heat. (Ibid.) The philosophical implication of the successful measurement of the imponderables did not seem to have been explicitly theorized for the majority of the century.

objects. This approach remained significant in the second half of the 19th century (see Chapter 2 for details). With the advancements in each field, experimental laws continued to find the easiest expression in mechanical terms, which also proved to be the least controversial among scientists subscribing to different high-level explanatory theories (e.g., the field approach versus particle approach to electricity and magnetism) when they had to decide on a common definition.[78] With dimensional analysis and the unifying principle of energy conservation, an interconnected system of algebraically expressed laws brought disparate realms in the exact sciences together.[79] The foundation of this system was naturally the mechanical quantities—length, mass and time. Length and mass were also the first to be standardized internationally through material artifacts in 1875.

Efforts to materialize this interconnected system and standardize newer quantities (many of which were essential for industrial technologies) also relied on their mechanical definition. For example, a unit magnetism (or "magnetic fluid") was defined by Carl Friedrich Gauss in the 1830s and 40s as "that which produces a repelling effect on another [unit quantity] like itself, separated in unit distance, with a motive force = 1, i.e., the effect of accelerative force = 1 for a mass = 1."[80] Gauss and Wilhelm Weber gave the name to the system of electrical and magnetic quantities defined as functions of mechanical units the "absolute system of measurement" (See

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78 See Helmholtz, Prof H. "XLVIII. On Systems of Absolute Measures for Electric and Magnetic Quantities." *The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science* 14, no. 90 (December 1, 1882): 430–40. Hermann von Helmholtz, "XLVIII. On Systems of Absolute Measures for Electric and Magnetic Quantities," *The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science* 14, no. 90 (December 1, 1882): 430–40, https://doi.org/10.1080/14786448208628441. Also James Clerk Maxwell, *A Treatise on Electricity and Magnetism* (Clarendon Press, 1873).

79 Buchwald, Jed Z, and Sungook Hong. "Physics."

80 Cited in Andre Koch Torres Assis, Karin Reich, and Karl Heinrich Wiederkehr, "On the Electromagnetic and Electrostatic Units of Current and the Meaning of the Absolute System of Units — For the 200th Anniversary of Wilhelm Weber's Birth," *Sudhoffs Archiv* 88, no. 1 (2004): 10–31.

Chapter 2 for details). The name continued to be used among later 19th century scientists. When William Thomson, for example, defined temperature scales based on the conversion between mechanical work and heat, he also named it the "absolute" temperature (See Chapter 3 for details). The term "absolute" signified an indubitable foundation. The edifice of exact science rested on the universally accessible and comparable quantities of length, mass and time, whose objectivity were also grounded in the a priori intuition in the Kantian spirit. This conception was reiterated by 19th century such as the neo-Kantian philosopher Hermann Cohen (Chapter 2) and the physicist Heinrich Wilhelm Dove, as the latter wrote in an influential[81] essay *Über Maaß und Messen*:

> "Time, space, matter, which occurred in every perception, were the *only* perception which could vary in relation to itself *quantitatively*. To state these variations, it is required to have a communal unit, a measure, which can be either given through nature or stipulated by convention."[82]

The quantitative structure of the mechanical units—length, mass and time—was self-evident: it was characterized by the part-whole structure typical of geometrical magnitudes. This structure was also embedded in the innate mode of perceptual experience. Their traditional measurement techniques through a(n imagined) process of aggregating or concatenating equal units was considered "natural," only one step away from using body parts or crude senses to estimate lengths, durations, and weights. It is therefore is not surprising that many 19th century practicing scientists regarded reducibility to these mechanical units or to extensive magnitudes as *the* foundation of measurement in general. In the opening chapters of the highly influential 1873 *Treatise on Electricity and Magnetism*, Maxwell noted that

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[81] See M. Norton Wise, ed., *The Values of Precision* (Princeton University Press, 1997), 118.

[82] Heinrich Wilhelm Dove, *Ueber Maass und Messen, oder, Darstellung der bei Zeit-, Raum- und Gewichts-Bestimmungen üblichen Maasse, Messinstrumente und Messmethoden, nebst Reductionstafeln* (Verlag der Sanderschen Buchhandlung, 1835), 2.

"There must be as many different units as there are different kinds of quantities to be measured, but in all dynamical sciences it is possible to define these units in terms of three fundamental units of Length, Time, and Mass…hence, in all scientific studies it is of the greatest importance to employ units belonging to a properly defined system, and to know the relations of these units to the fundamental units, so that we may be able at once to transform our results from one system to another."[83]

On a similar theme, when commenting on the foundation of measurement in light of psychophysicists' attempt to measure sensation (see Chapter 3 for more details), the physiologist Johannes von Kries wrote in a widely cited essay that the reason why sensation could not be measured was because it could not be reduced to extensive magnitudes.[84] Certitude in measurement lay in the clear intuitive sense in which one could declare two magnitudes as equal. Surveying all quantitative concepts in the physical realm, one could observe that magnitudes became measurable as some combinations of length, mass and time.

"…in the final analysis it is always and only values of length, time and mass that are compared one to another. The reduction of all other magnitudes to these values is mediated by a convention that involves an appropriate and pragmatic consideration of empirical relations."[85]

The only major perceived problem within this system of measurement built on mechanical units was error, or, the impossibility of absolute precision due to mankind's crude senses. According to the 1842 *Handwörterbuch der Chemie und Physik*: "since natural phenomena are represented in space and time, a precise understanding according to measure and number is essential for a complete knowledge of them (phenomena)."[86] Error and uncertainty in

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[83] Maxwell, *A Treatise on Electricity and Magnetism*, 3.

[84] J. von Kries, "Ueber die Messung intensiver Grössen und über das sogenannte psychophysische Gesetz," ed. R Avenarius, *Vierteljahrsschrift fur wissenschaftliche Philosophie* 6 (1882): 257–94.

[85] Ibid.

[86] E.F. August et al., "Experiment, Beobachtung," in *Handwörterbuch der Chemie und Physik: A - E* (Simion, 1842), 771.

measurement also stemmed from the crudeness of the visual sense, rather than the conception of measurement based on the way units of length, mass and time were measured.

> "the quantitative conception of phenomena is nevertheless limited by the imperfectness of our senses. Indeed, we attempt to trace the observations, more and more, back to observations through the *visual* sense, which, in itself was the most exact, is still capable of unlimited refinement through the instruments. The history of the sciences shows clearly, how the progress of sciences is dependent on the perfection of optical instruments and in general, the same devices through which visual perception became more accurate."[87]

Indeed, the process of standardizing the first international unit of length and mass in 1875 did seem to boil down to congruence in superposition, to the perfect alignment of two material rods determined by visual means with the aid of microscopes. The original ideas for a metric reform proposed by the French Academié des sciences in 1791 was to ground the standards of length, mass and time in natural constants that were considered immutable, universally accessible by all cultures, and reproducible any time in case that material prototypes were lost. Invariable properties of nature served as the ground of certitude for the units thus defined. This vision was never truly realized, at least beyond France. Unit length, for instance, was supposed to be grounded in either the length of a seconds pendulum or 1/4 of 10-millionth of the earth's meridian. Unit mass was supposed to connect to unit length through the mass of pure H_2O of a given volume, at the temperature of its maximum density. However, the practical difficulties involved in determining the relation of these constants to existing standards in the 19th century meant that it would be at odds with the goal of producing easily reproducible standards from easily accessed resources. The length of a seconds pendulum would vary by geographical location due to the differences in gravitation and air resistance, according to Friedrich Bessel.[88] Similarly,

[87] Ibid.

[88] Friedrich Wilhelm Bessel, "Ueber Mass und Gewicht im Allgemeinen und das Preussische Längenmass im Besonderen.," in *Populäre Vorlesungen über wissenschaftliche Gegenstände* (Penthes-Besser & Manke, 1848), 285.

the meridian could not be directly measured, but only calculated based on the length of an arc subtending one degree in latitude. But the measurement of such large-scale geographic distance required extensive geodetic survey using triangulation, and small inaccuracies in angle measurement would be greatly amplified in the process. The intrinsic irregularity of the shape of the earth further meant that it was impossible to infer the overall dimensions of the earth with a degree of certainty required for the fundamental units of a system of measurement. By mid-19th century it was a consensus that the connection between the Metre produced by France and the earth's meridian was only nominal. As pointed out by the astronomer and later the president of the International Bureau of Weights and Measures, Wilhelm J. Foester:

> "Good measurements with help of a suitably established and carefully maintained length prototype made of metal, perhaps controlled by suitable parts of completely crystalized materials, can therefore serve much better to detect the changes in the relationships of extension and curvature of a surface portion of the earth's body…"[89]

The original French Metre standard produced as 1/4 of a 10-millionth of the earth's meridian, according to Foester, was so unreliable that its foundation was "illusory" and quickly abandoned when the metric system was introduced to Germany. Similarly, pure water in nature was impossible to find, hence to define mass on the density of water meant that the chemical and thermal conditions of the water must be determined as precisely as possible to calculate the deviation of the water used from "pure water," at maximum density and highest purity.[90]

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[89] „Gute Messungen mit Hülfe eines zweckmässig eingerichteten und sorgfältig aufbewahrten Urmaasses von Metall, vielleicht noch controlirt durch geeignete Stücke aus vollkommen krystallisirten Stoffen, werden daher viel eher dazu dienen können, wirkliche Veränderungen in den Erstreckungs - und Krümmungsverhältnissen eines bestimmten Flächenstücks des Erdkörpers zu erkennen, als dass man aus derartigen Maass bestimmungen unmittelbare und allgemein gültige Controlen der Unveränderlichkeit des Urmaasses ableiten könnte." Wilhelm Foerster, "Gemeinsames Maass und Gewicht, und der Pariser Vertrag vom 20. Mai 1875," in *Sammlung wissenschaftlicher Vorträge* (Dümmler, 1887), 61.

[90] Ibid, 55.

When 17 nations signed the first metric convention in Paris in 1875, they also agreed to produce prototypes of unit length and mass based on the material standards in the Paris Archive, rather than on natural constants such as the density of water or the meridian of the earth.[91] This was motivated largely by pragmatic needs. Linking standards to natural constants would be too time consuming, although some scientists had by then advocated a future length standard grounded in the speed of light (See Chapter 4). Still, the production of the Meter and Kilogram prototypes in 1875 was a highly significant, symbolic event. An abstract mathematical usage was ascribed to an arbitrarily chosen artifact by stipulation, which Ludwig Wittgenstein mentioned in *Philosophical Investigations*: "there is one thing of which one can say neither that it is one metre long, nor that it is not one metre long, and that is the standard metre in Paris."[92] The production of the unit length prototypes was literally achieved by laying one metal bar against another and observing the congruence under a microscope. In other words, the whole system of scientific quantities rested on the rudimentary principles of congruence in superposition and concatenation, which mirror the way geometrical length had been measured for millennia.

1.7. Paul Du Bois Reymond: the Centrality of Linear Magnitudes

As mentioned in previous sections, the objects of mathematics had been considered as magnitudes and multitudes abstracted from concrete bodies in nature, since quantitative features were thought to inhere in particular physical bodies, qualities, actions and so on. The distinction between pure mathematics and the mathematical study of nature—the quantitative sciences—was more or less a division of labor, since they concern ultimately the same objects, namely magnitudes and multitudes with concrete embodiments. This conception began to crumble with

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[91] See Terry Quinn, *From Artefacts to Atoms: The BIPM and the Search for Ultimate Measurement Standards* (OUP USA, 2012).

[92] Cited in Robert J. Fogelin, *Wittgenstein: The Arguments of the Philosophers* (Taylor & Francis, 1999), 127.

the development of 19th century mathematics. Formalization and rigorous deductive proofs replaced intuitive certainty, characteristic of Euclid's geometry, as the criteria for valid mathematical theorems. As mathematical entities emerged, which are entailed by logic but could by no means be intuited (e.g., the transfinite numbers or the everywhere continuous but nowhere differentiable function), the connection to concrete magnitudes in nature became increasingly tenuous. Mathematics as a discipline formed its own domain of topics, methods, and professional standards.[93]

Hence, since the second half of the 19th century, mathematicians rarely discussed the term quantity. But not all mathematicians had abandoned the attempt to establish a connection between abstract mathematics, geometrical intuition, and the realms of the application of mathematics, despite the increasing difficulty of such a task. In the opening chapters of Paul du Bois Reymond's 1882 *Die Allgemeine Functionentheorie,* a treatise dealing with a mix of philosophical issues and advanced mathematics such as infinite sequences, he provided an extensive discussion of the meaning of quantity.[94] By quantity, du Bois Reymond meant all quantitative concepts in the empirical sciences hitherto capable of measurement. His views demonstrated the persistence of Kantian influence among 19th century intellectuals' understanding of the relationship between abstract mathematics and quantitative concepts in the sciences.

In first chapter titled "Über mathematische Grössen," du Bois Reymond noted that "mathematical quantities (quantum, quantities, quantité)" in general refer to "a common property of heterogeneous things, in relation to which they are numerically comparable, such as

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[93] See Jeremy Gray, *Plato's Ghost: The Modernist Transformation of Mathematics* (Princeton University Press, 2008).

[94] He was particularly concerned with the existence of certain mathematical limits, such as infinite decimals like 3.1415926…. Ibid, 145-147.

length or weight."[95] While this is a quite ambiguous characterization, he claimed that with any more specific "diplomatic definitions" to capture the essence of mathematical quantities would yield little results: "We obtain through these definitions as little an overview of the extension and content of a concept so delicately and richly ramified, as we can obtain about a new animal species when given only a certain number."[96]

The correct way to approach the question, for du Bois Reymond, is to examine the various realms of knowledge in which the concept of quantity occurred. These include both subjects that studied the external world, e.g., physical sciences, and those that studied the "inner life of the soul," e.g. psychology. A definition of mathematical quantities ought to be based on commonalities among heterogeneous quantitative concepts in these disciplines—here, the genus-species, universal-subordinate relationship between mathematics and the sciences was implicitly assumed. In line with tradition, he categorized quantities into the discrete and the continuous. Numbers are "a discrete kind of magnitude, [which] arise from the representations of separateness of objects in perception."[97] Continuous quantities, on the other hand, seem to encompass most quantitative concepts studied in physical sciences—lengths, areas and spatial

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95 „Unter mathematischer Grösse (quantum, quantities, quantité) versteht man gewöhnlich eine gemeinsame Eigenschaft verschiedenartiger Dinge, in Bezug auf welche sie numerisch vergleichbar sind, wie deren Länge oder Gewicht...Allein es ist, wie sogleich eingeräumt werden soll, mit dergleichen Produkten diplomatischer Definitionskunst überhaupt nicht viel geleistet. Wir erhalten durch sie ebensowenig einen Überblick über den Umfang und Inhalt einese so zart und reich verzweigten Begriffs, als wenn uns eine neue Tierform durch Angaben einer gewissen Anzahl." Paul Du Bois Reymond, *Die allgemeine Functionentheorie* (H. Laupp, 1882), 14-5.

96 Ibid.

97 This way of defining the concept of number in close connection to sensation, perception or counted objects following the Kantian tradition remained influential in late 19th century but received a backlash from advocates of logicism. Famously, Edmund Husserl outlay a similar analysis of number in the 1891 *Philosophy of Arithmetic* in terms of its origin in unmediated perceptual experience, e.g., cardinal numbers arise from the intuition of "something and something and something…" which resulted in severe criticisms from Gottlob Frege. See Gottlob Frege, "Review of Dr. E. Husserl's Philosophy of Arithmetic," trans. E. W. Kluge, *Mind* 81, no. 323 (1972): 321–37.

volumes, weights, time, velocity, force, heat, intensity of light and sound, electric tension, intensity of currents, etc. What these continuous mathematical quantities have in common, according to du Bois Reymond, is that "their measurement and comparison are based on visual perception, and that what they have in common, which can be compared and measured, ultimately always boil down to straight lines, and can be divided and combined like straight line segments."[98] He gave a few examples: the curve traced by the pointer of the clock describes time, while the arm of the scale measures weight; similarly, force is set to be proportional to the energy of pressure or motion, which in turn was expressed in terms of length measurement.[99]

Reducibility to length is central to conceptualizing the nature of all quantitative concepts for du Bois Reymond. Most quantitative concepts in the physical sciences are expressed as functions containing length as a variable, he noted, and they can be reduced to length in their measurement. More importantly, he claimed that the differences, parts, and multiples of parts of these quantities—length, mass, time, velocity, force, heat, intensity of light and sound, electric tension, intensity of currents, etc.—are *quantities of the same kind* just as parts of lengths are again the same kind as the whole. How exactly he arrived at this conclusion is unclear. By what means could concepts like velocity, force, heat, intensity of light and sound, electric tension, intensity of currents, etc., which are measured indirectly through a network of experimental laws, be said to have parts constituting the whole? On this du Bois Reymond did not inquire further. Essentially, du Bois Reymond would characterize any quantitative concept defined through a function with length as a variable as a linear quantity, as long as this concept is bound by zero.

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[98] „Die angeführten stetigen mathematischen Grössen haben, ihre Messung und Vergleichung auf Wahrnehmungen des Gesichtssinnes beruht, dass ihr vergleich- und messbares Gemeinsame schliesslich stets die geradlinige Strecke wird und dass sie sich wie diese theilen und zusammensetzen lassen." (Du Bois Reymond, *Die allgemeine Functionentheorie*, 20.)

[99] Ibid, 22.

Furthermore, he thought that both extensive and intensive magnitudes belong among linear mathematical quantities. It is easy to comprehend why extensive magnitudes are linear for him, according to the criterion discussed just in the previous paragraph. However, intensive magnitudes are also linear magnitudes: "the condition that the sequence of magnitude is a mathematical one requires only that they are sufficiently determinate [in the sequence] in themselves, not that they are determinable for us right now by being assigned to a linear measure."[100] In other words, even if intensive magnitudes are not immediately expressible as a function of linear quantities, the fact that the difference between degrees are again magnitudes of the same kind—again, a claim he did not explain—means that intensive degrees also share the essential characteristic of linear quantities.

Consequently, even temperature and sensation satisfy this criterion, i.e., that their differences are magnitudes of the same kind. Du Bois Reymond seemed to have held the view that since "continuous magnitudes" like temperature could have infinitesimal increments, they are composed out of "arbitrarily small increments with the same property."[101] The reasoning behind this conclusion was not explained. As we will see in chapter 3, the claim that part-whole

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[100] „Die Bedingung, dass die Grössenfolge eine mathematische sei, verlangt nur, dass ihre Individuen an sich hinlänglich bestimmte, nicht dass sie zur Zeit etwa durch Zuordnung zu lineärem Maass für uns bestimmbar seien, oder dass wir auch nur eine Ahnung davon hätten, wie solche Zurückführung je werde bewirkt werden können." Ibid, 25.

[101] „In der That, wenn die Veränderung einer Veränderlichen durch Unterschiede so zu sagen aus demselben Stoff wie die Veränderliche stattfindet, so muss sie, wie schon bemerkt, wenn stetig, mit Null anfangen, weiter muss sie, da sie ganz aus beliebig kleinen Zuwächsen von gleicher Beschaffenheit sich aufbaut, auch Vielfache und Theile derselben Art zu lassen, und dies ist eben unser Begriff von der lineären Grösse." (Ibid, 26.) He also argued that when we speak of "difference in degree," the changes (increase or decrease) of a magnitude are taken to be the same kind as the original magnitude. (Ibid.) By this logic, sensations were also linear mathematical quantities for du Bois Reymond. Sensations that arise from stimuli could be said to have various degrees: "increased feeling of warmth, which corresponds to an increment in temperature, arise from the increment in sensation, which itself is a sensation of warmth, through which the feeling of warmth becomes an inner image of temperature." (Ibid, 29)

structure applied to temperature at all would be challenged by Ernst Mach, drawing from the history of 19th century thermometry.

In sum, du Bois Reymond arrived at linearity as a unifying feature for all quantitative concepts in the sciences:

"It seems that however far away we are now and in future from being able to represent all phenomena of the external world mathematically, there will be no instance of magnitude that cannot be taken as a linear mathematical magnitude…because anywhere we venture into, all the variables will show themselves to be graduated in extension or degree, and such magnitudes, if capable of more precise determination, we consider…as essentially linear, even if we have not yet untangled them in their linear geometrical or mechanical end-variables."[102]

Echoing Descartes' vision in *Rules for the Direction of the Mind*, du Bois Reymond noted "geometrical representations therefore form the origin and constant refuge of our exact thinking, a claim which can encounter little objection."[103]

In this chapter, we have seen that the part-whole structure typical of geometrical magnitudes were projected onto all other measurable quantitative concepts in the canons of natural philosophy. The part-whole structure was contingent upon homogeneous and equal units, and measurement consisted of multiplying such unit so many times as to exhaust the whole. There were several reasons for how deeply entrenched this conception of quantity and measurement had been: 1. This was encouraged by the view that mathematical structures inhere

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[102] „Es scheint überhaupt, dass, wie weit wir davon entfernt sein und in alle Zukunft bleiben mögen, alle Erscheinungen der äußeren Wahrnehmungswelt mathematisch darstellen zu können, kein Beispiel einer Grössenart sich darbieten will, die nicht vermuthlich einmal in den Begriff der linearen mathematischen Grösse wird aufgenommen werden können, oder besser, die wahrscheinlich nie als eine lineäre sich erweisen werde. Denn überall , wohin wir vordringen , zeigt sich alles Veränderliche der Ausdehnung oder dem Grade nach abgestuft, und solche Grössen, falls sie genauer Bestimmung fähig sind, betrachten wir, wie dies soeben näher erörtert wurde, als wesentlich lineär, auch wenn wir sie noch nicht, ähnlich wie ich dies von der Härte bemerkte, in ihre linearen geometrischen und mechanischen Endvariabeln aufgelöst haben." (Ibid, 28)

[103] The reason was that all quantities either have length as a factor in their dimensional unit, or their measurement boils down to reading scales that have length. Expressing quantities in length was "the first step toward mechanical understanding." (Ibid.)

in nature, and that the quantitative concepts subject to mathematical (especially geometrical) methods must not be distinguished from the mathematical objects themselves. 2. In these canons, quantitative relations, which in fact had concrete basis in the method of measurement (i.e., concrete procedures being attached with a numerical meaning), were often represented by geometrical magnitudes. This can be observed in Euclid's Book V, Galileo's *Two New Sciences*, and Descartes' *Rules on the Direction of the Mind*. 3. The conceptual problems with projecting part-whole structure onto non-geometrical objects had been pointed out by many throughout the time, such as by the scholastic philosophers. What does it mean for a speed or acceleration to be composed out of parts? There's no sensible answer to this question. Due to various historical reasons, such as the success of Galileo's mechanics and Kant's "successful" yet highly ad hoc solution to constructing speed as an additive intensive degree, these conceptual problems were overshadowed. Throughout the 19th century, the traditional conception of quantity and measurement remained influential, and evolved into two notions: 1. That all measurable magnitudes were reducible to length, mass and time, which were thought to embody the kind of part-whole structure analogous to geometrical magnitudes. 2. That all magnitudes were ultimately functions of length. This paves way for our discussion of how quantity and measurement came to be reconceptualized in the 19th century, especially as it became questionable whether equal units of spatial and temporal magnitudes were attainable at all.

Chapter 2 Hermann von Helmholtz's Theory of Quantity and Measurement in the Context of 19th century Scientific Practices

2.1. Introduction

In the last chapter, I discussed the set of ideas that characterize the predominant conception of quantity and measurement prior to the 19th century. Measurement did not constitute its own philosophical problem, and quantity was frequently conceived through the relationship between parts (unit) and the whole, characteristic of geometrical magnitudes. Aristotle's dictum "quantity means that which is divisible into constituent parts" held strong sway in this framework. In face of the growing success in mathematical physics, philosophers including Immanuel Kant had to stretch the theory of quantity based on part-whole relation in order to account for the "miraculously efficacious" applicability of mathematics in explaining natural phenomena. The challenge of this task is revealed by the difficult passage in Kant's *Metaphysical Foundations of Science*, where Kant attempted to reconcile the intuition that speed is not an extensive quantity with the fact that it must be conceived as additive in Galilean and Newtonian mechanics. Meanwhile, the idea that quantitative structure is embedded in unmediated cognition or in nature itself grew increasingly out of touch with the variety of measurement techniques and practices, which measured quantities indirectly or generated new quantitative phenomena.

The influential German scientist Hermann von Helmholtz contributed to a different understanding of quantity and measurement in the second half of the 19th century. In Helmholtz's epistemological writings on geometry, measurement is brought to the forefront. Euclidean and non-Euclidean geometry are distinguished in terms of their respective metrical expressions for distances and angles. As a result, which geometry describes space was for Helmholtz an empirical question to be determined through measurements (e.g., stellar parallax). Moreover, he pointed out that the possibility of using analytic geometry to investigate the structure of space already contains an assumption of empirical origin: it must assume the comparability of lengths, derived from the existence of rigid structures in our world. In a less renowned article from 1887, "Counting and Measuring" Helmholtz presented a theory of quantity more pertinent to scientific practices, with an empiricist thesis expanding beyond geometry to virtually all quantitative concepts in physical sciences. According to Helmholtz, to clarify what it means to express the relations between real objects through numbers—what it means to quantify—is to clarify the empirical conditions under which mathematical equality and addition can be interpreted in an experimental context and operationalized through specific procedures. The mathematical relation of equality should be interpreted by identifying a symmetrical and transitive relation in the act of comparison. Mathematical addition, on the other hand, for Helmholtz, ought to be interpreted through a concrete combinative operation (e.g., joining two objects side by side) demonstrating conformity to the associative and commutative laws of addition. The implication of Helmholtz's account is that the method of comparison provides the condition of possibility for discussing any particular quantity concept; it is an integral part of the latter's meaning and definition. Furthermore, he noted that quantity is an "objectification" of objects' capacity to bring forth certain empirical effects in experiments, in such a way that these effects can be mathematically describable. The concept of unit, as well as divisibility, are unnecessary for determining the measurability of a quantity, they are conceptually secondary: once the quantity obtains a determinate meaning fixed by a reliable method of measurement, it would certainly be possible to think of it in terms of unit and division. The techniques of measuring length, mass and time are so ancient that these "fundamental units of physics" appear intrinsically describable by numbers, but this still does not change the fact that their measurability also have an empirical basis, as any other quantitative concept.

While Helmholtz's essays on geometry were highly influential, his theory of measurement was included in an article where he simultaneously discussed the foundation of arithmetic. The article was delivered at a time when many of his contemporary mathematicians, such as Richard Dedekind and Gottlob Frege, came to believe that foundational statements in arithmetic (later extended to the entirety of mathematics) must be proved step by step through logical derivations. As a result, Helmholtz's attempt to "prove" the ordinal number sequence and the additive laws by appealing to counting or intuition of time appeared both old-fashioned and inadequate to his mathematical audience. The 1887 article was poorly received in its time for this reason, even if Helmholtz argued in the article in favor of the thesis that number ought to be defined separately from quantity, and that the application of numbers form a distinct philosophical problem from the foundation of number. While mathematicians led the path towards logicism and formalism, the gap between pure mathematics and quantification in scientific practice only widened, in absence of a consensus on how pure mathematics is applied.

Putting the mathematical concerns aside, Helmholtz's account of measurement was novel in his own time. His key arguments were challenged by his contemporary, the neo-Kantian philosopher Hermann Cohen. Cohen attributed a naïve empiricism to Helmholtz in an 1888 critical review, pointing out Helmholtz's divergence from the Kantian conception, according to which number is derived from quantity. He criticized in particular Helmholtz's attempt to explain physical quantity in terms of the "method of comparison," and for correlating mathematical equality and addition with physical operations before giving any explicit definition of the term "quantity." These criticisms are telling because they reveal the novel aspects of Helmholtz's argument. It appears that Cohen found reprehensible the operationalist undertone of Helmholtz's account, especially Helmholtz's extensive discussion of the "method of comparison" when discussing equality and additivity. For Cohen, the epistemologist's task is to find a universally applicable category of quantity, a certain mode of synthesizing the content of perceptual experience rooted in the transcendental subject, through which individual quantitative facts in science can be grounded. How could Helmholtz go straight to the "method of comparison" without offering a general definition of quantity in the first place? For Helmholtz, however, there simply *isn't* a single definition or cognitive category that unifies all quantitative concepts in science. Quantitative relations are fulfilled by all kinds of methods of measurement. One can only set structural requirements that the method of comparison must meet to be able to express equality and additivity, but not predetermine the meaning of these terms from the outset. The generalized quantity is an empty concept. Mathematical relations are simply coordinated with specific empirical regularities and operations, and the ability to express physical attributes as quantities was contingent upon experimental facts.

Why did Helmholtz propose such a theory of measurement at such a time? What could he be referring to with the notion of quantity? Considering that his idiosyncratic views are not found in Kant or his contemporary philosophers, neither are they fully foreshadowed in his other epistemological writings, the solutions to these problems should perhaps be sought in the history of science and technology. Helmholtz's account of measurement is pertinent to his context of scientific practice. As historians have shown, physics as a discipline took its modern form in the 19th century, characterized by its exact, quantitative and experimental methods.[1] The subject matter and phenomena studied by physicists became increasingly inextricable from their experimental context and instrumentation. Physicists' tendency to avoid metaphysical commitments in the latter half of the 19th century further encouraged them to formulate laws in dynamical and energetic terms. At the time, quantities were often directly defined in terms of

[1] Buchwald and Hong, "Physics," 165, 168.

experimental laws; hence their definitions could be translated into directly observable effects. In this sense, the experimental method became an integral, though implicit, part of the definitions of these quantities. Parameters of the measuring device itself also frequently entered the calculation of these quantities.

Electricity and magnetism provide telling examples to illustrate how quantification of the 19th century physics progressed. Helmholtz was directly engaged in researches in electrodynamics since the 1870s and was familiar with precision measurement techniques by overseeing Europe's first attempt to standardize electrical quantities in the 1880s. The Physikalisch-technische Reichsanstalt under Helmholtz's directorship also performed experimental work in determining the standards. A survey of the history of 19th century study of electricity and magnetism shows that the quantification of these phenomena was initially made possible through certain canonical experiments that lent themselves to mathematical descriptions. The interaction between unspecified "imponderable fluids" or "current-elements" was described through mechanical actions among ponderable objects through equilibrium scenarios, e.g. the balance of torsion with electrostatic or electrodynamic forces. Most instruments for measuring the most common electrical quantities were based on prototype experiments such as those conducted by Charles-Augustin de Coulomb and André-Marie Ampère. When electrical quantities became standardized and their units were determined in the Centimetre-Gramm-Second system in the 1880s, their definitions also simply contained the dynamical terms describing experimental regularities. By examining the experimental work underlying their definitions, I show that a scale for these quantities, with or without material embodiments (e.g., a mercury column of certain dimensions resembling the "Meter" or "Kilogram" prototypes), ultimately referred to regularities brought forth by experimental operations. The addition of these quantities no longer resembled the concatenation of geometrical segments. Hence the distinction between extensive and intensive magnitudes could be reduced to that of additive and non-additive ones.

The standardization of electrical units involved large-scale, high-profile experiments that integrated the measurement of multiple quantities in a single setting, such as those performed by the British Association led by James Clerk Maxwell in the 1860s and by Wilhelm Weber in 1880s. The determination of electrical units highlighted the experimental foundation of quantitative concepts in 19th century physics and the materiality of instrumentation that made theoretical, mathematized reasoning in physical sciences possible. It is no wonder that in Helmholtz's account of measurement, the method of measurement comes to the forefront in characterizing quantities, while the notion of unit—a byproduct of the measurability of electrical quantities—fades to the background, as well as many other Kantian categories, e.g., homogeneity, divisibility and so on. It was the method of measurement that determined if the structural requirements of mathematical equality and additivity can be met, which in turn determined whether a qualitative aspect of physical phenomena could be considered as a quantity.

Apart from showing how Helmholtz's account of measurement and quantity was tied to his context of scientific practices, this chapter has a second purpose. By showing the pattern of quantitative methods in physical sciences, I pave way for my discussion in the next chapter of how such pattern was followed in other traditionally non-quantitative areas. In recent years, scholars have pointed out that Helmholtz implicitly referred to the controversies brought forth by psychophysics in "Counting and Measuring."[2] Yet quantitative methods in physics featured in

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[2] See Heidelberger's section on Helmholtz in Michael Heidelberger, *Nature from Within: Gustav Theodor Fechner and His Psychophysical Worldview* (University of Pittsburgh Pre, 2004); Olivier Darrigol, "Number and Measure: Hermann von Helmholtz at the Crossroads of Mathematics, Physics, and Psychology," *Studies in History and Philosophy of Science Part A* 34, no. 3 (September 1, 2003): 515–73, https://doi.org/10.1016/S0039-3681(03)00043-8.

the broad backdrop against which the philosophical discussion concerning measurability of sensation took place, serving as a benchmark when psychologists reflected on what was measurable.

2.2. Helmholtz's Epistemology Before 1887[3]

Though being a prominent practicing scientist, Helmholtz wrote a few widely influential essays on the epistemology of mathematics, including the 1868 "On the Facts underlying geometry," 1870 "On the Origin and Meaning of Geometrical Axioms," and 1878 "Facts in Perception." This last essay concerns primarily perception, but he also mentions issues related to geometry, since, as we know, the two are intertwined in Kant's epistemology. There is a common thread connecting the empiricist theses of these essays. That is, Helmholtz tended to dissolve any notion of a preestablished harmony between knowledge and the external world by revealing the empirical conditions that make such knowledge possible in the first place. This is shown by the main arguments in his essays on spatial perception and the foundation of geometry: space is not given a priori as a unified intuition but acquired through reinforced associations between tactile and visual impressions. Similarly, Euclidean geometry, taken for granted as the geometry of space, does not have its seat in the transcendental subject so that it is incapable of being refuted by experience; rather, its axioms and validity rest on certain assumptions derived from the observation of everyday objects. Along the same line, in his essay on the foundation of arithmetic and measurement, Helmholtz argued that numbers are not miraculously efficacious in their application in nature; instead, it is we who have chosen to coordinate specific empirical regularities, in the act of measurement, with mathematical relations, in order to interpret the

[3] For more on Helmholtz's epistemology of geometry and perception, see Gary Carl Hatfield, *The Natural and the Normative: Theories of Spatial Perception from Kant to Helmholtz* (MIT Press, 1990). Cahan, *Helmholtz: A Life in Science*. and Biagioli, *Space, Number, and Geometry from Helmholtz to Cassirer*.

former through the latter. The strategies of argument in these various works on epistemological issues are similar: just as local signs in perceptual experience indicate lawful correlations between sensations acquired empirically, the abstract system of geometrical axioms and quantitative concepts in science indicate lawful behaviors of physical objects, which we derive from empirical observation or from directly interacting with these objects.

In his early career, Helmholtz's research focused on the physiology and psychology, where he tackled issues such as the laws of eye movement and mechanisms of depth, distance and locality perception in the visual field. He argued against the "nativist" position, according to which innate anatomical mechanism is responsible for spatial perception. For instance, the nativist might hold that each point on the retina contains its own "spatial values," so that its stimulation can invoke the perception of a corresponding spatial direction.[4] For Helmholtz, the ability to localize is entirely learned. The effect of the excitation of individual nerve fibers on specific locations of the retinal image gives only a "local sign," just as the contraction of eye muscles required to turn the eyes and direct the line of vision to bring a point on the retina into focus is a sign, or as the sensation of voluntary movements required to bring the human body to an object is a sign.[5] These are all signs whose spatial meaning we have learned to interpret through repeated trial and error in our interaction with the environment. The spatial meaning of these local signs consists of precisely the correlations among them, for instance, how certain regular changes in retinal images (indicated by local signs a1, a2, a3, etc.) are always brought forth by certain voluntary movements (indicated by local signs b1, b2, b3, etc.).[6] Thus the child

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[4] R. S. Turner, "Consensus and Controversy: Helmholtz on the Visual Perception of Space," in *Hermann Von Helmholtz and the Foundations of Nineteenth-Century Science* (University of California Press, 1993), 154–204, 175.

[5] Here Helmholtz's "signs" are closer to "signals" than mathematical or linguistic signs with assigned meanings.

[6] Hermann von Helmholtz, "The Recent Progress of the Theory of Vision," in *Popular Lectures on Scientific Subjects*, trans. Edmund Atkinson (Longmans, Green, 1873), 267.

learns to turn its eyes and reach with its hand objects in its vicinity, by playing with whatever object presented to it, by turning it over and over to learn all of the perspectival images and all of the sensory impressions it can afford as a result of the child's own movements. Its eyes learn to trace the outline of objects, and every occasion that they succeed in doing so again to bring forth the corresponding retinal image is a confirmation that the child has executed the correct innervations in the eyes to identify such an object. As an adult, the learning process has become unconscious through innumerous repetitions. Spatial relations are learned and consolidated in the same way, through orienting ourselves in the immediate environment. Every movement and the ensuing perception function are an experiment to confirm that we have learned to read our perceptual signs correctly, namely, how to modify the appearance of things in specific ways by specific innervations or movements. We learn to exert precisely the kind of innervations to bring forth the expected changes in sense impressions.[78]

Helmholtz then extended the argument involving "local signs" in spatial perception to perception in general, as he formulated in the 1878 lecture "The Facts in Perception":

As he formulates in the 1878 essay "The Facts in Perception": "We observe during our own continuous activities, through which we obtain the knowledge of the enduring of a lawful relationship between our own innervations and the bringing into presence of different impressions from the current range of presentables." See Hermann von Helmholtz, *Epistemological Writings: The Paul Hertz/Moritz Schlick Centenary Edition of 1921, With Notes and Commentary by the Editors* (Springer Science & Business Media, 1977).

[7] For instance, as soon as the infant succeeds in seeing the shape of objects by converging both eyes to trace their outlines, it forms a rule for the movement of the eyes for seeing future objects. In carrying out those movements and receiving input in the retinal images as expected, it becomes more convinced of the correspondence between the two processes. It is only because such correspondence can be found in various conditions repeatedly that the spatial intuition becomes solidified. Helmholtz, Hermann von. "The Recent Progress of the Theory of Vision," 266.

The rules for employing the kinds of innervation to bring forth the desired results, for Helmholtz, is "a piece of knowledge which cannot be expressed in words but is the result which sums up my previous successful experience." (Ibid, 271.)

[8] "Each of our voluntary movements, whereby we modify the manner of appearance of the objects, is to be regarded as an experiment through which we test whether we have correctly apprehended the lawlike behavior of the appearance before us, i.e., correctly apprehended the latters' presupposed enduring existence in a specific spatial arrangement." Helmholtz, *Epistemological writings*, 136.

"Inasmuch as the quality of our sensation gives us a report of what is peculiar to the external influence by which it is excited, it may count as a symbol for it, but not as an image. For from an image one requires some kind of sameness with the object of which it is an image…but a sign need not have any kind of similarity at all with what it is the sign of. The relation between the two of them is restricted to the fact that same objects exerting an influence under same circumstances evoke same signs."[9]

The point is that sense perception does not provide a direct copy of the external world. Experience only yields knowledge of a correspondence between the lawfulness within and the lawfulness outside. The configuration of lawful correlations between sensations (the signs) only corresponds with the lawfulness of the external world, since sensations are merely "effects produced in our organs by external causes." Scientific investigation takes the same path, he noted, it must constantly form hypotheses and conduct experiments, in which our active intervention is correlated with each experimental result it affects, and what can be gained is no other than lawful connections in the phenomena.

In a famous 1868 lecture, Helmholtz limited the correspondence between the inner and the outer to mathematical relations:

"only relations of time, of space, of equality, and those which are derived from them, of number, size, regularity of coexistence and of sequence—'mathematical relations,' in short—are common to the outer and the inner world, and in these we may indeed strive for a complete correspondence between our representations and the represented objects."[10]

The strategy of argument developed in Helmholtz's theory of perception is carried over into his epistemology of geometry. In his essays on geometry, Helmholtz argued against Immanuel Kant's claim that spatial intuition is given as a whole and necessarily governed by the axioms of Euclidean geometry. He built his arguments on his contemporary researches on non-Euclidean geometries by mathematicians such as Bernhard Riemann and Eugenio Beltrami.

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[9] Ibid, 122.

[10] Helmholtz, Hermann von. "The Recent Progress of the Theory of Vision," 276. Translation is slightly modified by me based on the German original. Hermann von Helmholtz, *Populäre wissenschaftliche Vorträge: 2* (Vieweg, 1871), 98.

According to Riemann, the distinction between the Euclidean flat n-dimensional manifold, and geometries of constant positive or negative curvature (e.g., spherical and pseudo-spherical geometries) lies in their distinctive forms of expression for measuring distances and angles. By interpreting geodesics as straight lines in Euclid's original postulates for geometry, one can interpret the spherical and pseudo-spherical (or saddle-shaped) geometries as being governed by non-Euclidean axioms, namely sets of axioms modified from Euclid's original ones. In pseudospherical geometry, for example, there is not one, but infinitely many, parallels through a point outside a straight line; in spherical geometry, there is more than one shortest line between two points.

Reflecting on these mathematical results, Helmholtz argued that non-Euclidean geometries are not merely mathematical possibilities. Which set of axioms governs physical space is an empirical matter susceptible to refutation. Since spatial intuition is learned by reinforced regularities in sensory impressions, we are perfectly capable of forming spatial intuition compatible with the axioms of non-Euclidean geometries if exposed to the appropriate experiences. For instance, if an observer who has developed their spatial intuition in a flat space enters a pseudospherical world, they would continue to see straight lines as the lines of light rays, they would see the most distant objects around them as being at a finite distance, but upon approaching them these objects would expand in depth. As a consequence, they would form a non-Euclidean spatial intuition. Space is therefore not pre-determined to be Euclidean, but simply taken for granted as Euclidean. But why has it been taken for granted? It results from reinforced experience with physical bodies encountered in everyday life, such as geometrical similarity between shapes of different sizes, which is only possible in space of constant zero-curvature.[11] Similarity is assumed to be valid regardless of the size and location of objects. Furthermore, astronomical observations showed zero stellar parallax (in pseudospherical space, the parallax is positive even for infinitely distant points) by Helmholtz's time.[12] But this does not mean that future measurements would not refute this result and disprove Euclid's axioms as those actually governing space.

The best way to determine the geometry of physical space is through measurement and analytic geometry, which eliminates all intuition from its methods. The value of curvature that distinguishes different kinds of geometry (of constant curvature) can be extracted from intrinsic measurements (i.e., without referring to external coordinates) to be compared with Euclidean flat space using analytic methods.[13] However, Helmholtz argued, since analytic geometry requires measurement, and measurement in turn relies on the notion of a rigid structure in motion, the notion of rigidity truly must be presupposed, despite being an idealization from experience. Geometrical means (congruence in superposition) cannot prove or disprove what it must presuppose.[14] Had our measuring rods been shriveling up along with our body in motion, analogous to what occurs in a convex mirror, where the person as well as the measuring rod becomes smaller as they move away from the center, there would be no way to discover this by geometrical means alone, because the person in the convex mirror would count up exactly the

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[11] For instance, in geometries of constant but non-zero curvature, enlarging the size of a triangle one also changes the sum of its angles. In geometries of non-constant curvature, shapes distort in motion. Similarity is only possible in Euclidean flat space.

[12] Helmholtz, *Epistemological Writings*, 18.

[13] For instance, the distance expression in Euclidean geometry is given by the Pythagorean theorem. It is different in non-Euclidean geometries. This is also an intrinsic quantity, meaning that it has a fixed form of expression, regardless of the selection of the coordinate system and without referring to a higher dimension in which this geometry is imagined to be embedded.

[14] It is in this sense that Helmholtz argued "space is transcendental without its axioms being so."

same number of centimeters as the person outside without realizing the distortion. If this had been in fact the case, Helmholtz noted, the entire body of physics would have to be rewritten.

In Helmholtz's writings on geometry, measurement emerges as a central theme that cannot be avoided in discussing the foundation of geometry. From his point of view, not only does the choice among different geometries (Euclidean and non-Euclidean ones, in Helmholtz's time) for describing space depend on measurement (e.g., by measuring stellar parallax), the possibility of geometry at all also depends on measurement: as Riemann showed, starting from a n-dimensional manifold in which only positional information is given, one has to presuppose the comparability of small line elements (ds's) to apply the tools of analytic geometry, from which the structure of space can be further investigated. For Helmholtz, geometry rests on certain empirical conditions (e.g., the rigidity of the measuring rod) that make measurement possible, rather than the other way around. He expanded this argument to all of mathematical representation of physical quantities in the 1887 "Counting and measuring from an epistemological standpoint." The effort to dissolve the pre-established harmony between mathematics and the empirical world continues. In this essay, Helmholtz no longer identified arithmetic as the "science of magnitudes," but claimed that it has a separate foundation, while quantity is the name given to empirical attributes and relationships that can be represented by numbers. The possibility of such representation needs to be clarified on a different basis. The essay is split into two parts, each dealing with a different set of issues. In the following sections I examine the two distinct parts of "Counting and Measuring" respectively.

2.3. The Foundation of Arithmetic and Its Reception Among Mathematicians

"Counting and Measuring" was not well received by Helmholtz's contemporaries, especially among the mathematical audience. Mathematicians tended to be hostile to Helmholtz's attempt to prove the basic statements in arithmetic by referring to "psychological facts" or the intuition of time, because it went against an emerging consensus that the foundation of number (as well as algebra and analysis) must be grounded in logical proofs (Chapter 4).

Helmholtz argued in the opening sentences that arithmetic is not the "science of magnitudes (Größenlehre)" but instead "a method built upon pure *psychological* facts, which teaches the logical application of a symbolic system (namely, the numbers) to an unlimited extent and an unlimited possibility of refinement." By "a method built upon pure psychological facts," he meant, first, that the ordinal number sequence originates from the one-dimensionality of the intuition of time, and second, that the axioms of arithmetic, which consist mainly of the laws of addition, can be derived from the fact that the ordinals are fixed in a univocal order. In counting, one finds "a sequence, through which the acts of consciousness succeeded one another in time," and is able to hold this sequence in memory. Arbitrary signs can be assigned to fix this succession, which gives us the numerals in different languages. Going forward and backward in the succession are two essentially different acts, because there is an unambiguous distinction between the present activities of the consciousness, which can be thought together with the past activities, and the future ones, which are non-existent in memory. There is therefore an essential distinction between "1, 2, …n-1," "1, 2, …n" and "1, 2, …n+1." This is the distinction between succession and precession. As a result, the ordinal numbers form an irreversible sequence and every position in the counting sequence is unique (presumably, if one is counting to 4, it is impossible that they in fact have counted to 6).[15] Cardinal numbers result from applying the

[15] Hermann von Helmholtz, "Zählen Und Messen Erkenntnisstheoretisch Betrachtet," in *Wissenschaftliche Abhandlungen*, vol. 3 (Leipzig: Johann Ambrosius Barth, 1895), 356–91. Also in Helmholtz, *Epistemological Writings*, 75.

ordinal numbers: they follow one another as one "throw onto the counted heap" one by one (or, mentally grouping the counted ones) at each step of counting.[16]

The axioms of arithmetic are taken from two mathematicians, the brothers Robert and Hermann Grassmann, and Helmholtz derived them from the univocal ordinal sequence. The symbolism of addition "a + b" essentially means counting to position "a" in the sequence, and then treating the position "a+1" as "1" and counting from 1 to b. From this, he went on to prove the associative law of addition by first proving "(a + b) + 1 = a + (b + 1). The proof again refers to counting, or, the equivalence of the following two acts: counting to position "a," then treat "a+1" as the starting position, counting from "1, …b," then counting one further; and counting to position "a," then treat "a+1" as the starting position, counting from "1, …b+1." The generalized formula a+(b+c)=(a+b)+c can be proved by induction,[17] and the commutative law is proved in a similar fashion.[18] Helmholtz stressed the advantage of his proof, referring to the fixity

[16] In its application to real objects being counted, the order of counting does not change the cardinal number of a group. This is proved in the following manner: in counting, an arbitrary sequence is coordinated with the natural number sequence. Suppose two members ε and ζ are coordinate with natural numbers n and n+1. It is certainly possible to exchange ε and ζ such that ε is coordinated with n+1 and ζ with n, without skipping or repeating any element in the 1-1 mapping between two sequences. Then, it is certainly possible to continually exchange the rest of the elements in the arbitrary sequence to get every permutation without skipping or repeating any letter in the 1-1 mapping with the natural numbers. One would get, for instance, the following coordinations: $\begin{smallmatrix}1&2&3&4&5\\ \alpha&\beta&\gamma&\delta&\varepsilon\end{smallmatrix}$ …. Or $\begin{smallmatrix}1&2&3&4&5\\ \beta&\gamma&\varepsilon&\alpha&\delta\end{smallmatrix}$ …. Or $\begin{smallmatrix}1&2&3&4&5\\ \delta&\varepsilon&\alpha&\beta&\gamma\end{smallmatrix}$ …. , etc.

[17] Helmholtz's proof by induction that $(a + b) + c = a + (b + c)$ goes as follows

a. This is true when c=1
b. Prove it is also true when c= c+1.

Proof: $(a + b) + (c + 1) = [(a + b) + c] + 1$ according to the statement that (a+b)+1=a+(b+1).*

$= [a + (b + c)] + 1$ according to the inductive hypothesis

$= a + [(b + c) + 1]$ according to *.

$= a + [b + (c + 1)]$ according to *.

[18] Basically, Helmholtz proved first that the proposition "1+a=a+1" is true by induction. Then by a similar proof by induction, this was generalized to "a+b=b+a."

of each position in the ordinal number sequence alone, as "it can be obtained without referring to external experience."[19]

Arithmetic, or the "theory of pure numbers" in general, is built on these basic facts, and it investigates "how different combinations of these symbols (the operations of calculation) led to the same result." Subtraction and multiplication are operations based on addition and the univocal ordinal sequence. For instance, the symbolism "a - b" can be understood as the number that must be added to b in order to obtain a as a sum. Taking the number sequence as having no limit in both ascending and descending positions, one obtains negative numbers. Helmholtz did not go into more detail on irrational or imaginary numbers. As he noted earlier that the theory of number teaches the logical application on a symbolic system to an unlimited extent, one might infer that he would consider these newer mathematical entities as created by arithmetical or algebraic operations. Overall, the status of these novel entities does not seem to concern Helmholtz, since he mentioned that in physical sciences, irrational numbers are simply approximated to the best of scientists' ability. After all, he never considered himself a professional mathematician.[20]

In light of a new demand for rigor among Helmholtz's contemporary mathematicians, no arithmetical statement was considered as proved unless it was shown by step-by-step deductions from preliminary notions (depending on the individual, these notions may differ). Hence Helmholtz's appeal to the unexplained "intuition of time" received much criticism. Gottlob Frege, for instance, famously claimed that he had seen "hardly anything…more unphilosophical"

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[19] Ibid, 87.

[20] Cahan, *A Life in Science*, 372.

than Helmholtz's attempt to ground arithmetic empirically.[21] As "Counting and Measuring" became largely ignored for its discussion of arithmetic, so did its discussion of measurement. As historian Francesca Biagioli points out, "partly because the arithmetical considerations against Helmholtz appeared to be so compelling, that the focus of his theory (i.e. the foundation of measurement) was moved to the background."[22]

2.4. Measurement, or the Application of Mathematics Through "the Method of Comparison"

The second half of "Counting and Measuring" deals with the conditions for the "objective application of arithmetic to physical magnitudes," where Helmholtz intended to explain the term "quantity" "in the realm of facts." Arithmetic would remain a "pure game of ingenuity with dreamt up objects" if it could be applied to the real world, that is, if it cannot describe the relations and attributes of real objects using numbers and make predictions by calculation in advance.[23] This point potentially continues to hold well in light of a logicized/formalized mathematics. The foundation of the *application* of numbers requires a separate explanation from the foundation of numbers, as Helmholtz phrased the issue in the following terms: "what is the objective sense of our expressing the relationship between real objects, as magnitudes, through denominate numbers? And under what conditions can we do this?" These questions can be resolved into two other questions, he argued, namely what it means to declare two objects in a given relation as equal, and what conditions must be satisfied for us to regard some kind of physical combination as additive. To clarify the term quantity is to

[21] "Helmholtz wants to ground arithmetic empirically, by hook or by crook. Therefore, he does not ask how far one can go, without drawing on the facts of experience. Rather he asks: how can I most quickly draw in whatever facts of sensory experience?" Frege, Gottlob. *Grundgesetze der Arithmetik*. Verlag von Hermann Pohle, 1893, p.140n. A similar comment was from George Cantor, who compared Helmholtz to one Enlightenment thinker who claimed that numbers were invented by shepherds to count their sheep. See Gray, *Plato's Ghost*, 97.

[22] Biagioli, "Cohen and Helmholtz on the Foundations of Measurement."

[23] Helmholtz, *Epistemological Writings*, 75.

clarify how equality and additivity in arithmetic are interpreted "in the realm of facts."[24] Concerning the notion of unit, he noted: unit is not an essential concept, but appear as an "unnecessary restriction on the realm of valid propositions, if one from the outset treats physical magnitudes as only those that were composed of units." In other words, quantities need not be composed of units (or conceived through a part-whole structure) to be measurable. Helmholtz then commented on the "fundamental units of physics," namely length, mass, and time, to which virtually all other physical quantities were reduced to in the 19th century: "these universally known units are not to be defined by their concept," but "can only be displayed in particular natural bodies (weights, measuring rods) or particular natural processes (day, pendulum beat). The fact that they are more universally known by tradition among men does not alter the business and the concept of measuring, but *appears in contrast as only an incidental feature*."[25] In this sense, Helmholtz thought that length, mass and time are not quantitative on an a priori basis.

Measurement first and foremost depends on fixing an empirical condition to which we attribute equality. Equality was "an exceptional case" occurring in measurement; it expresses itself through the first axiom of arithmetic, namely through its transitivity and symmetry: if a = b, b = c, then a = c; and if a = b, b = a. "In factual observation," he noted, equality is "only exhibited in that two equal objects under a suitable condition meet or concur, letting a particular success to be observed, which does not, as a rule, happen between another pair of similar

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[24] Here I echo Biagioli's claim that Helmholtz emphasizes "the physical *interpretation* of the laws of addition" the conditions for such interpretation are largely empirical, not formal. My main argument however diverges from Biagioli's, as she focuses on geometrical measurement. Biagioli, *Space, Number, and Geometry from Helmholtz to Cassirer*, 100.

[25] My italics. Helmholtz, *Epistemological Writings*, 89. This sets his views on measurement apart from those of his contemporaries, who argued that the absolute units bear the authority of nature. For example, Fleming Jenkin: the absolute system "bears the stamp of authority, not of this or that legislator or man of science, but of nature." Fleeming Jenkin, *Reports of the Committee on Electrical Standards Appointed by the British Association for the Advancement of Science* (E. & F.N. Spon, 1873).

objects."[26] With this most generic characterization, he went on to give a list of examples. For instance, placing two objects on a balance does not in general create an equilibrium. Exceptionally, two objects are found to be in equilibrium, and exchanging them does not disturb the equilibrium. The equilibrium is also transitive: when a and b are both found to be in equilibrium with c, they are found to be in equilibrium with each other.

Such empirical interpretation of mathematical equality as a symmetric transitive relation also underlies the quantitative nature of the concepts of length and time. Specifically of length, congruence in superposition is the "exceptional" case in the act of measurement that is both transitive and symmetric. When the end-points of any two segments coincide, the congruence also "always recurs in any situation and on exchanging the two point pairs in any arbitrary manner."[27] As Helmholtz already argued in his essay on the foundation of geometry, the validity of congruence in superposition depends on the existence of rigid physical bodies in motion anywhere in space. Time, on the other hand, is measured by physical processes that are repeatable. The equality of durations depends on the coincidence of two processes' beginnings and ends. How does one know that the durations of these processes remain unaltered? The only justification is that the measurement of duration by different methods—using pendulum beats or the depletion of sand or water clocks, for example—all yield the same result.[28] If the beginning and end of process a coincide with those of a third process c, then c also proceed simultaneously with b.

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[26] Translation is modified. The original quote is „Gleichheit zwischen den vergleichbaren Attributen zweier Objecte ist ein ausnahmsweise eintretender Fall, und wird also in tatsächlicher Beobachtung nur dadurch angezeigt werden können, dass die zwei gleichen Objecte unter geeigneten Bedingungen zusammentreffend oder zusammenwirkend einen besonderen Erfolg beobachten lassen, der in der Regel zwischen anderen Paaren ähnlicher Objecte nicht eintritt." Helmholtz, "Zählen und Messen, erkenntnistheoretisch betrachtet."

[27] Helmholtz, *Epistemological Writings*, 92.

[28] Ibid, 93.

Other quantitative concepts in physics are also built on "exceptional" occurrence in comparison that can be interpreted as a symmetric transitive relation. For instance, electrical currents are compared through their electromagnetic effects, i.e., the angle of deflection of the magnetic needle of a nearby compass; the (visual) brightness of two lighted areas are compared by placing them adjacent to each other and identifying a recognizable line of demarcation.[29] Across all realms of inquiries in the physical sciences, "the most varied physical means have to be sought out." Ultimately, only experiment can determine whether the observed case qualify as a symmetric transitive relation, i.e., equality. This is entirely an empirical matter for Helmholtz.

Additivity is explained in a similar way. A physical connection (Verknüpfung) could be "regarded as addition, if the result of the connection—when compared as a magnitude of the same kind—is not altered either by exchanging individual elements with each other, or by exchanging terms of the connection with equal magnitudes of the same kind."[30] In other words, the operation can be regarded as addition when it satisfies the axioms of addition, regardless of what the operation consists of, provided that the sums are always determined by the same method of comparison which determines equality for the quantity in question. These axioms are:

II. The associative law of addition: $(a+b)+c=a+(b+c)$

III. The commutative law of addition: $a+b=b+a$

IV. the same added to the same gives the same.

Again, whether a physical act of combination satisfies those additive laws can only be verified experimentally by interchanging the objects (the addends) in comparison. As Helmholtz

[29] The comparison of brightness was a common method in astronomy to compare the distances of stars. In the next chapter, I discuss photometric methods in more detail.

[30] Helmholtz, "Zählen und Messen, erkenntnistheoretisch betrachtet," 383.

explained using the example of weight again, when balancing five 1-gram weights with one 5-gram weight, substituting different copies of the 1-gram weights does not change the result of equilibrium (equals added to equals are equal), neither does changing the order of putting these different 1-gram weights on the balance (commutative law), neither does substituting the five 1-gram weights with a 5-gram weight (associative law).

Examples from physical sciences illustrate the heterogeneity of operations that can be interpreted as addition. As Helmholtz noted, by joining two conductors end-to-end, one adds the quantity of resistance; by joining two conductors side-by-side, one adds the quantity of conductance (both quantities are measured by the same method.) Furthermore, joining two Leyden jars or condensers side-by-side adds the charge, while joining them end-to-end adds the potential. What Helmholtz did not say explicitly is that the precondition for declaring these kinds of combination as the addition of the said quantities is the validity of the empirically discovered laws in electricity and magnetism (e.g., Ohm's law). He did, however, point out that the ability to interpret a combinative operation as additive ultimately depends "empirical knowledge of certain aspects of its [the object's] physical behavior in meeting and interacting with others."[31] To put it in other words, knowing the experimental regularity—how physical objects behave when certain operations are performed under certain circumstances—is the precondition of establishing the additive structure of the quantity in question.

Combining what Helmholtz said about equality—an "exceptional" occurrence contingent upon the method of comparison—we can infer the following synopsis of his arguments: it is not because concepts are quantities that it is possible to map certain aspects of the behavior and interaction of physical objects associated with these concepts onto mathematical

[31] Helmholtz, *Epistemological Writings*, 103.

representation. Instead, it is because we interpret these lawful aspects of their behavior and interaction, exhibited in the act of comparison, as indicating structural features like transitivity, symmetry, commutativity, etc., that they become quantitative concepts. Measurement does not discover quantities in nature but construct them through establishing regularities in experiments, and the units being counted are determined with concrete reference to the experimental method and even apparatus. This can be said about the fundamental units of length, mass and time as well. As Helmholtz wrote, their additive combination and equality are so familiar to us that these concepts seem inherently describable in terms of "greater," "less," "how many," and so on.[32] But this is only because we know the physical operations that underlay their mathematical interpretation all along. These operations are so mundane that length, mass, time have become disembodied quantitative concepts that in themselves guarantee mathematical representation (e.g., as Galileo claiming that it "follows the habit of nature" to represent motion by geometrical lines and diagrams, or Descartes claiming that all quantities should be represented in thought as lines (Chapter 1)). However, their "quantitative-ness" are derived from an empirical foundation just the same. After all, what can be possibly meant by the equality of length, other than the fact that in everyday observation, the congruence between the end-points on the measuring rod and the end-points on some other object is not altered by the order of superposition, and that whenever two pairs of end-points are congruent with a third, they are always found to be congruent among themselves? Despite its seeming triviality, superposing line-segments is an experimental act that involves hypothesis and intervention, just like human perception.

[32] Ibid, 96.

This message is corroborated by the following quote, when Helmholtz discusses how equality of one quantity may be related to the equality of other quantities (for instance, conductors are equal in length and material are equal in resistance). He noted:

> "we are accustomed to express this colloquially, that we objectivise (objectiviren) the capacity for objects to produce the decisive success in the first kind of comparison as an attribute of these objects; we ascribe the equal magnitudes of this attribute to the objects deemed equal, and we represent other effects which preserve the equality as the *effect* of the attribute, or as empirically solely dependent upon the attribute. The meaning of such claim is no more than this: objects, which demonstrate equality in the same kind of comparison that determines the equality of this particular attribute, can also be interchangeable in other conditions (which determine the equality of other attributes) without altering the result."[33]

In other words, a quantitative attribute is not a property of the things being compared, it is simply the objectification of the set of effects brought forth by objects in the act of comparison, to which we attribute equality. It is not *because* two things are "equal in x quantity" that they are in turn "equal in y quantity." It is rather that the set of effects to which we attribute equality in the first kind of comparison always correlate with the set of effects to which we attribute equality in the second kind of comparison. Bodies that have the same gravitational mass also have the same inertia, but this is based on our empirical knowledge "a particular law of nature for this particular connection."[34] Mathematical functions relating different quantities to one another ultimately state empirical correlations of this kind.

In proposing his own theory of quantity and measurement, Helmholtz relegated some of the key conceptual categories in his predecessors' writings on these topics to a secondary status.

[33] „wir pflegen dies sprachlich dann so auszudrücken, dass wir die Fähigkeit der Objecte den bei der ersten Art der Vergleichung entscheidenden Erfolg hervorzubringen, als ein Attribut derselben objectiviren, den gleichbefundenen Objecten gleiche Grösse dieses Attributs zuschreiben, und die anderweitigen Wirkungen, in denen sich die Gleichheit bewährt, als Wirkungen jenes Attributs, oder das erfahrungsgemäss nur von jenem Attribut abhängend bezeichnen. Der Sinn einer solchen Behauptung ist immer nur der, dass Objecte, die sich bei derjenigen Art der Vergleichung als gleich erwiesen haben, die über die Gleichheit dieses besondern Attributs entscheidet, sich auch in den bezeichneten anderweitigen Fällen gegenseitig ohne Aenderung des Erfolges ersetzen können.“ Helmholtz, "Zählen und Messen Erkenntnisstheoretisch Betrachtet," 377.

[34] Helmholtz, *Epistemological Writings*, 91.

For instance, he claimed that homogeneity, unit and divisibility, and the distinction between extensive and intensive magnitudes, are not useful in clarifying the condition and meaning of measurement. Instead, they are conceptually derivative to the conditions of equality and additivity and can be derived once a quantity is found to be measurable. Let us be reminded that in Kant's writings, homogeneity is a term used to describe the manifold of intuition in space and time. When the content of perception is reduced to a homogeneous manifold, it is possible to make quantitative distinctions, as Kant wrote in a 1780 text: "homogeneity is specific identity with numerical diversity, and a quantum consists of homogeneous parts."[35] Quantity, after all, is defined by Kant as the successive synthesis of manifold intuition. This view is inherited by many 19th century philosophers. For example, similar arguments can be found in the writings of the neo-Kantian philosopher Hermann Cohen (next section) and Edmund Husserl.[36] But in Helmholtz's article, homogeneity is understood in operational terms: homogeneous magnitudes are simply those "whose equality and inequality are to be determined through the same method of comparison."[37]

Divisibility and unit are also conceptually derivative notions. As mentioned earlier, Helmholtz considered the notion of unit superfluous in discussing the meaning of quantity and measurement. If we only include those quantities that are composed of units as quantities, then we exclude a vast number of valid propositions in science outside the realm of quantity, he claimed. Divisibility is traditionally tied with the notion of continuity. Continuous magnitudes,

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[35] Cited in Daniel Sutherland, "Kant's Philosophy of Mathematics and the Greek Mathematical Tradition," *The Philosophical Review* 113, no. 2 (2004): 157–201.

[36] In *The Philosophy of Arithmetic*, Husserl discusses the concept of multiplicity, which presupposes abstraction from particular content into "something" that can be then conceived as "one and one and one…etc." This gives rise to number. See Frege, "Review of Dr. E. Husserl's Philosophy of Arithmetic."

[37] „Grössen, über deren Gleichheit und Ungleichheit durch dieselbe Methode der Vergleichung zu entscheiden ist, bezeichnen wir ‚gleichartig.'" Helmholtz, "Zählen Und Messen Erkenntnisstheoretisch Betrachtet," 377.

such as spatial magnitudes, were considered to be infinitely divisible. But the issues concerning infinite divisibility of continuous magnitudes becomes irrelevant in the 1887 article because additive magnitudes can be regarded as divisible in general. If a concept is measurable and can be represented by numbers, then the usual arithmetical operations can also find meaningful empirical interpretations, depending on how this quantity is measured. But for a concept to be expressible numerically in the first place, its method of comparison and physical combination must be capable of instantiating the relation (equality) and operation (addition) of numbers. Similarly, the distinction between extensive and intensive magnitude is no longer meaningful. Helmholtz noted that one can think of natural coefficients as "intensive" magnitudes because they are not obviously additive. But, he wrote, future scientific research may reveal the empirical laws that allow these coefficients to be additive, and they will cease to be "intensive."

It is worth noting that Helmholtz's own views on the issue of quantity changed significantly from 1847 to 1887. An unpublished manuscript titled "On General Physical Concepts" dated from 1847 shows that he subscribed to a characteristically Kantian approach, which proceeds from the faculty of the mind and the structure of perceptual experience, instead of the "method of comparison." In this manuscript, quantity is a "thought-connection" of the "relation of an object to another." Homogeneous units and part-whole relation are central in this characterization of the concept of quantity:

> "[Objects are] homogeneous in a relation if both can be decomposed into simple parts that are equal with respect to this relation…every object which can be *thought* as divided into equal or homogeneous parts can be considered a magnitude. *Measuring means determining the set of these parts*; a determinate set is called number, an individual part, the unit of measure."[38]

[38] Hermann von Helmholtz, "On General Physical Concepts," in *The Neo-Kantian Reader*, ed. Sebastian Luft, trans. David Hyder (Routledge, 2015), 6.

He went on to discuss the distinction between finite and infinite divisibility, noting that continuous magnitudes can be thought of being infinitely divisible, although this cannot be actually carried out. Then he writes: "*the science of the connection of magnitudes according to quantity is arithmetic.*"[39] In the 1887 article, as we have discussed so far, the foundation of arithmetic is separate from the foundation of measurement, and the notions of homogeneity, units and divisibility are reduced and operationalized.

Later in this chapter I will discuss the context of scientific practices that incubated Helmholtz's unique views on measurement. Granted, Helmholtz's epistemological writings on geometry in 1860s, in which he pointed out the empirical presupposition of geometrical axioms and the concept of distance, anticipated his treatment of quantity, as an empty abstraction that stands for empirical regularities conforming to certain structural features, brought out by the act of measurement. However, there are also many aspects of his theory that cannot be fully explained by referring to his earlier philosophical writings. The priority of the "method of comparison" seems to run against the grain of the Kantian tradition, as it has almost an operationalist connotation, namely that the meaning of quantities consists of the operations of measurement alone (although Helmholtz qualified this connotation by referring to the empirical regularities). This tendency was picked up by Hermann Cohen, as we will immediately see. Additionally, from Helmholtz's point of view, committing to an empiricist foundation of the quantity concept does not entail abandoning the part-whole relationship and the centrality of unit, both central to other philosophers' writings on quantity. I shall argue in the rest of the chapter that these shifts in Helmholtz's thought were prompted by the developments in 19th century sciences, where many classic examples of "intensive magnitudes" that cannot be

[39] Ibid.

characterized in the same way as length and angles became quantitative through highly conspicuous, large-scale experiments. The derivation of units (e.g., in electricity and magnetism) as byproducts of experimental laws became simply too apparent to ignore.

2.5. Hermann Cohen's Critique

Helmholtz is frequently associated with the "Back-to-Kant" movement in German philosophy. But not all of his views were compatible with the Kantian tradition. Apart from his empiricist arguments against the a priori status of Euclidean geometry, his arguments on quantity and measurement also appeared foreign to the Kantians. In 1888, the neo-Kantian philosopher Hermann Cohen published a critical review of Helmholtz's "Counting and Measuring." Cohen's objections shed light on how Helmholtz's account of number and quantity diverges from the accepted views of his time (especially in the Kantian tradition).[40] Cohen believed that to analyze the validity of scientific concepts, or, to use the Kantian jargon, to provide the condition of possibility of these concepts, one must proceed from an analysis of the transcendental subject. The validity of concepts should not be established through inductive or empirical proofs but should be sought in their a priori basis. As he wrote in his 1885 *Kants Theorie der Erfahrung*:

> "elements of consciousness…are sufficient and necessary to provide the ground and justification for the fact of science… If one finds, for example, the concept of system necessary and constitutive of science, then it becomes necessary to *find out an element of consciousness* which in its generality corresponds with this feature of science…the element of consciousness must serve as the foundation of science, and the presuppositions of science must be asserted as the principles of the cognizing consciousness."[41]

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[40] As Cahan notes, the separation of number from quantity also "gave [Helmholtz] a fresh occasion to distance himself—as he had already done in regard to human perception (1855, 1866-67, and 1878) and geometry (since 1868)—from what he here referred to as the 'strict adherents of Kant,' thus implying he was not one of them." (Cahan, *Helmholtz: a Life in Science*, 629.)

[41] „solche Elemente des Bewusstseins seien Elemente des erkennenden Bewusstseins, welche hinreichend und notwendig sind, das der Wissenschaft zu begründen und zu festigen. Die Bestimmtheit der apriorischen Elemente richtet sich also nach dieser ihrer Beziehung und Competenz für die durch sie zu begründenden Thatsaschen der wissenschaftlichen Erkenntniss. Findet man z.B., dass der Begriff des Systems für die Wissenschaft notwendig, für

This is the basic spirit in which Cohen proposed his own views on quantity against Helmholtz's. First and foremost, Cohen argued, Helmholtz never provided a clear definition of the term quantity: "It must be considered as a defect in the clarity of this work, that Helmholtz did not proceed from a definition of quantity or something along the lines of "first, it is a case of the definition of quantity…" By expressing instead his 'standpoint' 'from the outset,' he started from number. Only then did he move on to the question of quantity. Cohen quotes Helmholtz: "'Then we must ask: what is the objective sense, that we express the relation between real objects through denominate numbers, as quantities?' Only 'express'? Not 'produce' and 'generate'? Are the relations between real objects accessible or thinkable in any other way than through the expression of quantity? Is the concept of quantity not rather an irreplaceable instrument for the determination of objects? Must one not, therefore, start from this instrument?"[42]

With this exclamation, Cohen questioned the separation of number and arithmetic from quantity as well as Helmholtz's overall approach to the quantity concept, namely, clarifying the meaning of quantity through clarifying the criteria for determining empirical equality and additivity. His objections will be unpacked in the next few paragraphs. First of all, Cohen raised a valid objection to Helmholtz's account of number based on "pure psychological facts." Helmholtz claimed that the ordinal number sequence is based on the intuition of time, on the capacity to distinguish past acts of consciousness from the present act and keep them in memory. Nevertheless, Cohen argued, all acts of consciousness presuppose this distinction, without which everything will simply blend into a blur. Then what distinguishes the ordinal number sequence

dieselbe constitutiv sei, so wird es nothwendig sein, ein Element des Bewusstseins ausfindig zu machen, welches in seiner Allgemeinheit diesem Merkmal der Wissenschaft entspricht...die Element des Bewusstseins müssen als Grundlagen der Wissenschaft wirksam sein, und die Voraussetzungen der Wissenschaft sind als Grundzüge des erkennenden Bewusstseins geltend zu machen." Hermann Cohen, *Kants Theorie der Erfahrung* (F. Dümmler, 1885), 77-8.

[42] Hermann Cohen, "Jubiläums-Betrachtungen," *Philosophische Monatshefte* 24 (1888), 262.

from the product of any other psychic activity? Contrary to Helmholtz's views, Cohen maintained that ordinal number sequence is more complex than temporal succession.

More importantly, for Cohen, the concept of quantity must be tackled from a transcendental point of view—quantity is neither pure intuition nor pure concept but stems from the interaction of pure concept and intuition. Counting is a process through which units in a multiplicity are generated and fixed. Real objects are reduced to "mere comparability," and only then is comparison possible, and only through a comparison of the number of units in one multiplicity with another can there be any sense to talk about equality. "Equality always presupposes comparability, additivity, namely the unit in multiplicity, namely quantity."[43] Number is simply the possibility to form multiplicity out of units, through addition. When the difference between magnitudes in comparison cannot be fixed by a number, then we call these magnitudes extension. This cognitive order from homogeneity to quantity to equality had roots in the stages in the "constitution of the object," by which Cohen meant that quantitative conception is already embedded in the grasping of a determinate object by the conscious mind[44]: "without thinking of the object as quantity, we could not think of them as object at all."[45]

But Helmholtz completely reversed this cognitive order, as Cohen noted:

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[43] Ibid, 269.

Also, „Diese Reduction des sogenannten reellen Objects auf die blosse Vergleichbarkeit mit der Einheit der Mehrheit ergiebt die Gleichartigkeit, als die erste und fundamentalste Art einer Art...Die instrumentale Gleichartigkeit bedeutet nichts anderes als die Summirbarkeit, die Möglichkeit und Befugniss, Einheit der Mehrheit zu bilden, das sind Zahlen. Die Gleichheit dagegen setzt den Begriff der Grösse voraus...dass in den verglichenen Grössen durch Zahlen ein Unterschied nicht festgestellt werden kann, oder dass dieselben, in Zahlen bestimmt, dieselbe Ausdehnung beschreiben."

[44] „Es fügen und ordnen sich demgemäss die Begriffe des Gleichartigen, der Gleichheit und der Grösse. Die Ordnung dieser Begriffe entspringt aus ihrer kritischen Begründung, welche durch die Aufgabe geleitet wird: den Gegenstand zu constituiren. Von dieser Rücksicht wird auch Helmholtz geleitet, aber nicht in kritischer Reinheit und Sorgfalt. Er unterscheidet nicht Stufen in der Bildung des Objects." (Ibid.)

[45] „Dass wir, ohne den Gegenstand als Grösse zu denken, ihn nicht als Gegenstand denken können." Ibid.

"Helmholtz…did not distinguish the stages in the formation of the object, and thus the fundamental questions concerning number and quantity immediately became the question concerning 'physical combination,' according to which we establish the equality relation. The elementary meaning of equality was therefore lost. Plato said: [by equality] I do not mean equal rocks and equal sticks, rather I mean equality in itself. The elementary meaning of equality lies in homogeneity."[46]

The reference to Plato suggests that Cohen attributed a naïve empiricism to Helmholtz, according to which mathematical equality is mistakenly taken as particular instances of equal collections of concrete objects. Helmholtz's equality is therefore only a "relative" equality. It seems that Cohen found the operationalist undertone of Helmholtz's claim particularly reprehensible—the claim that in the context of scientific practice, equality is an empirical occurrence predicated on some specific method of comparison. Furthermore, Cohen continued, the concept of unit is fundamental for its "constitutive character" in the formation of the object-concept, but this layer of significance is also lost by Helmholtz. More blasphemous is Helmholtz's claim that "quantities, whose equality and inequality are determined by the same method of comparison, we call 'homogeneous.'" Identifying homogeneity with "the method of comparison," as Helmholtz did, seems to indicate nothing other than philosophical confusion. As Cohen clarified: the root of quantity lies not in "the method of comparison 'under a suitable circumstance,' but in the Urmethod of ordering through unit as the building block of multiplicity." He continued: "the reduction of so-called real objects to mere comparability with the unit in multiplicity, yields homogeneity, as the first and the most fundamental kind of a kind."[47]

Basically, Cohen suggested that the concept of quantity can only be obtained from an a priori source, from the "transcendental subject" without reference to properties of any particular

[46] Ibid, 268.

[47] Ibid, 270.

body, any technique of measurement, or any empirical input whatsoever. He upheld a conception of quantity passed down from Aristotle, which I have explored in detail in the previous chapter. For Helmholtz, however, quantitative expressions have a hidden empirical basis. Not only does a claim about quantity assume certain lawful behavior of physical bodies, but it also seems that the *method* of measurement is a prerequisite for discussing the meaning of quantity and measurement in the first place. This is why in clarifying the "objective meaning" of our expressing physical relations and attributes through numbers, Helmholtz chose to clarify the general conditions for operationalizing mathematical equality and additivity, i.e., conformity to the structural features of equality and to the laws of addition. But he also admitted that the specific interpretation of equality and addition for different quantities are different, and they are left to the measurer's empirical judgment. Ultimately, the possibility of this interpretation depends on natural laws, because one has to know how physical objects would behave in the act of comparison before identifying certain configuration of their behavior as equality or addition. By the time Helmholtz was writing, most quantitative concepts in physics were not the kind that can be "constructed in intuition" without the mediation of measuring instruments and techniques. Quantitative concepts even in mechanics are not chunks of perceptual experience in space and time, but situated in a conceptual space where reference to experimental practices is an integral part of theoretical definitions and vice versa. This point I hope to illustrate in full detail with a case study of the measurement of electrical units in Helmholtz's time. Helmholtz's aim in "Counting and Measuring," I believe, is to explain how quantitative concepts in physical sciences are created, defined and measured in his time. The stipulation of a unit or a scale is only a by-product of their measurability, as was prominently featured in the determination of electrical units.

2.6. A Theory of Measurement in the Context of 19th Century Scientific Practices

Helmholtz's views on measurement and quantity did not emerge out of a vacuum, nor should it be plausibly understood as a response to purely intellectual problems raised by his philosophical predecessors, especially those in the Kantian tradition. Despite his frequent excursions into philosophy, he was first and foremost a practicing scientist. He also had little interest in appealing to his contemporary academic philosophers. In private letter to Rudolf Lipschitz in 1881, he spoke of academic philosophers with scorn, most likely referring those who had vehemently defended Kant's doctrine of a priori spatial intuition against his own essays on the epistemological foundation of geometry.[48] Helmholtz said that he took comfort in the fact that mathematicians and physicists increasingly aligned with his epistemological views.[49] Given this antipathy towards academic philosophy, Helmholtz's intended audience was most likely his contemporary scientists or scientifically minded philosophers.

Quantification is a hallmark of late 19th century sciences.[50] The expansion of quantitative methods into the realm of the psychic through Gustav Fechner's psychophysics, as many scholars have pointed out, incited much debate among psychologists and philosophers concerning the foundation of measurement at the time. Helmholtz probably had these debates in the back of his mind as he wrote the 1887 paper.[51] But psychophysics in turn grew out of emulating the

[48] They were "impotent bookworms who have never generated new knowledge…everyone reads to himself and is incapable of reflecting on other peoples' thoughts." Rudolf Lipschitz, ed., "Helmholtz to Lipschitz, March 1881," in *Briefwechsel mit Cantor, Dedekind, Helmholtz, Kronecker, Weierstrass und anderen* (Springer-Verlag, 2013), 131.

[49] As he wrote in the same letter: "..dann empört mich immer, so oft ich mir auch vorgenomen habe mich nicht empören zu lassen, die Unverfrorenheit, mit der Leute, die nicht den kleinsten geometrischen Satz zu fassen vermögen, in der sicheren überzeugung überlegener Weisheit über die schwierigsten Probleme der Raumtheorie absprechen.)" (Ibid.)

[50] See Kathryn Olesko, "The Meaning of Precision: The Exact Sensibility in Early Nineteenth-Century Germany," in *The Values of Precision*, ed. M. Norton Wise (Princeton University Press, 1997).

[51] Cahan. *Helmholtz: A Life in Science*, 629; Michael Heidelberger, *Nature from Within: Gustav Theodor Fechner and His Psychophysical Worldview* (University of Pittsburgh Pre, 2004); Olivier Darrigol, "Number and Measure: Hermann von

quantitative methods in physical sciences. Its founder, Fechner, had worked in electrodynamics in his early career. He translated Jean-Baptise Biot's work into German and experimentally confirmed Ohm's law. Furthermore, many psychologists commenting on psychophysics frequently and self-consciously referred to measurement practices in physical sciences, in their effort to either deny or support Fechner's thesis (Chapter 3). For instance, rejecting Fechner's thesis, the psychologist Johannes Von Kries invoked the Kantian distinction between intensive and extensive magnitudes. Von Kries claimed that intensive magnitudes must be reducible to extensive (i.e., mechanical) ones, i.e., length, mass and time, in order to be measurable. In supporting his argument, he referred to successful precedents in physics—the new "thermal units, amperage, voltage, electromotive force, electrical resistance, magnetic flux, etc"—all of which are reducible to mechanical units.[52] Physicists had also generally believed that these base units constitute the foundation of mathematical representation. Wilhelm Weber and Carl Friedrich Gauss's "absolute system of measurement," reducing electrical and magnetic quantities to mechanical units, was decisive in shaping the trajectory of 19th century physics. James Clerk Maxwell wrote in the first chapter of his 1873 *Treatise on Electricity and Magnetism* that "in framing a mathematical system we suppose the fundamental units of length, mass and time to be given, and deduce all the derivative units from these by the simplest attainable definitions."[53] On the other hand, Helmholtz's own views on this point, as I have already shown, is subtly different: he did not consider reducibility to mechanical units in itself the most crucial factor in quantifying physical concepts. There is nothing a priori about the concepts of length, mass and time, he

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Helmholtz at the Crossroads of Mathematics, Physics, and Psychology," *Studies in History and Philosophy of Science Part A* 34, no. 3 (September 1, 2003): 515–73.

[52] Kries, "Ueber die Messung intensiver Grössen und über das sogenannte psychophysische Gesetz."

[53] Maxwell, *A Treatise on Electricity and Magnetism*, 1.

argued in "Counting and Measuring." Rather, their quantitative structures are derived from experimental regularities (e.g., the transitivity and symmetry of congruence in superposition) that are given mathematical interpretations, just like any other quantity.

While the controversies surrounding quantitative psychology catalyzed a systematic reflection on the nature of measurement among late 19th century scientists, the broader backdrop against which such reflection took place was the emergence of new quantitative methods in physical sciences. The quantification of what had been known in the 18th century as "imponderable substances" was conspicuous in particular. Heat, electricity, magnetism, light—these were thought to be substances that have neither extension nor weight. Temperature representing the sensation of hot and cold, or the phenomenon of transient, spectacular electric discharge, initially appeared to be classic examples of intensive magnitudes. 18th century natural philosophers like Henry Cavendish and Alexander von Humboldt literally shocked themselves to compare the intensities of electric currents via the intensities of pain.[54] Nevertheless, by the end of the 19th century, quantities like charge, current, resistance, temperature, and so on, were among the ranks of extensive magnitudes, as their units came to be expressed in mechanical units. In this sense, the older categories of extensive versus intensive, and divisible versus indivisible magnitudes in pre-19th century philosophy of quantity, lost their significance. Further conceptual issues emerged from the fact that most experimental phenomena crucial in the quantification of heat, electricity and magnetism seem to have been artificially produced by the experimental setup and the instruments involved.[55] Granted, one could argue that highly

[54] Arthur Schuster, *A History of the Cavendish Laboratory 1871-1910: With 3 Portraits in Collotype and 8 Other Illustrations* (Longmans, Green, and Company, 1910), 33. Andrea Wulf, *The Invention of Nature: Alexander Von Humboldt's New World* (Alfred A. Knopf, 2015), 24.

[55] Heilbron, *Weighing Imponderables and Other Quantitative Science around 1800,* 66, 3.

sophisticated apparatuses (e.g., the sextant) had been prominently used in science since the antiquity, but in these cases, angles measured angles, weights measured weights, hence the "homogeneity between parts and the whole" in intuition is in this sense intact. The instruments that measured the imponderables in the 19th century seem entirely different in nature: in what sense could the intensity of "electrical conflicts" inside the conductor be expressed in terms of length, mass and time? The homogeneity between parts and the whole no longer seems obvious. How do these quantities relate to mechanical units exactly, and why did their quantification convince some people that reducibility to mechanical units is the foundation of measurement, but not others? What was the implication of the quantification of the "imponderables" for a general philosophy of measurement, and specifically for Helmholtz's views on measurement? He did, in fact, invoke examples of quantities in electricity, magnetism, heat, in his 1887 article. It seems that an examination of the meaning and definition of these quantitative terms have become inevitable for us to understand the historical context of the theoretical debates concerning quantity and measurement.

In the next section, I focus on the quantities in electricity and magnetism mainly by examining how they were defined and measured up till the first International Electrical Congress in 1881, which represented Europe's first attempt to standardize units of electrical quantities, based on the most uncontroversial definitions of these quantities in the international scientific community at the time. Their definitions were documented in various reports of the 1881 Congress, including a report given by Helmholtz himself. The experimental techniques and instruments in which these definitions were embedded can be found in various experimental reports published in scientific journals such as Poggendorff's *Annalen der Physik und Chemie*. Electrical measurement was also quite familiar to Helmholtz himself. Although he never performed the related experimental work, he attended the international electrical congresses to serve on the Electrical Standards Committee in 1881, and again in 1882, 1884 and 1893 to negotiate further issues based on the experimental work underway.[56] These conferences involved many high-profile figures in the European scientific community, such as William Thomson, John Éleuthère Mascart, Helmholtz, G. H. Wiedemann, and so on, and were major scientific events at the time.[57] When Helmholtz became the director of the Physikalisch-Technische Reichsanstalt in 1887, determining electrical standards according to the agreements made at the international congresses became a central task at its Electrical Laboratory, headed by one of Helmholtz's student, Wilhelm Jaeger.[58]

Helmholtz became a professor of physics in 1871 at the University of Berlin, and he began publishing on electricity and magnetism through the problem of open currents in animal physiology.[59] His work on this topic intervened in two major competing paradigms at the time: the action-at-a-distance approach predominantly accepted by continental physicists, which explained electrodynamic/-magnetic phenomena in terms of the force between two quantities of electric or magnetic fluids in motion, and the field approach shared among British physicists, which explained the same phenomena by the intervening medium, or the field. Helmholtz adopted a unique methodology by focusing on the energy states of dynamic systems formed by

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[56] Cahan, *Helmholtz: A Life in Science*, 577.

[57] For more on standardization of electrical units, see Larry Randles Lagerstrom, "Constructing Uniformity: The Standardization of International Electromagnetic Measures, 1860-1912" (Ph.D., United States -- California, University of California, Berkeley, 1992), http://search.proquest.com/pqdtglobal/docview/303998652/abstract/FED093B2B478419CPQ/1; Olesko, "Precision, Tolerance, and Consensus: Local Cultures in German and British Resistance Standards."

[58] Before 1887, experiments had only been carried out by individual scientists in their own labs. David Cahan, *An Institute for an Empire: The Psysikalisch-Technische Reichsanstalt, 1871-1918* (Cambridge University Press, 2004), 104.

[59] Cahan. *Helmholtz: A Life in Science*, 378.

laboratory objects.[60] He mainly relied on the principle of energy conservation rather than the interaction between potentially hypothetical entities like the "electric fluid" or "the field." It was within this framework that many German physicists in the 1890s assimilated Maxwell's field theory. For our purposes, it is worth pointing out a significant parallel between Helmholtz's approach to the study of electrodynamics overall and his approach to the theory of quantity and measurement: both approaches exemplify his commitment to phenomenological regularities (consistent with the law of energy conservation) among directly observable objects. This philosophical principle is encapsulated in a set of lectures "Introduction to Theoretical Physics," given in the last few years of his life, where he discussed his broader views on physical laws and concepts. For example, he spoke of terms like "attractive force" in Newtonian mechanics as a linguistic abstraction, which boiled down to a specific set of observable effects:

> "In colloquial expression, we mostly deviate from the formulation of natural laws given so far, in that we form abstractions and use nouns instead of verbs, for example, we express the former of the above-stated rules in the following form: between any two heavy bodies at a finite distance from each other in space, there is an attractive force of a certain magnitude. We introduce an abstraction instead of an easy description of the phenomenon of motion—the attractive force. We designate with it not anything, anything that has a factual sense, more than what is contained in the mere description of the phenomenon…we know nothing more factual about such a force than that, as often as it works, or [that when] conditions for its effectiveness arise, the phenomenon concerned will be observed."[61]

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[60] However, he favored Maxwell's theory as opposed to Weber's. See Olivier Darrigol, *Electrodynamics from Ampère to Einstein* (OUP Oxford, 2003), 233-234. Cahan, *Helmholtz: A Life in Science*, 443.

[61] "in dem sprachlichen Ausdruck weichen wir nun meistenteils von der bisher angegebenen Formulierung der Naturgesetze ab, indem wir Abstracta bilden und statt der Verba Substantiva einsetzen, .z.B. das erste der oben angeführten Gesetze in der Form aussprechen, dass zwischen je zwei schweren Körpern, die sich in endlicher Entfernung von einander im Raume befinden, fortdauernd eine Anziehungskraft von bestimmter Grösse besteht. Wir haben damit statt der einfachen Beschreibung des Phänomens der Bewegung ein Abstractum, die Anziehungskraft, eingeführt. Wir bezeichnen damit in der That weiter nichts, wenigstens nichts, was noch einen factischen Sinn hat, als was auch in der bloßen Beschreibung des Phänomens enthalten ist. Nun weiss man über eine solche Kraft weiter nichts Thatsächliches anzugeben, als dass, so oft sie wirkt, oder die Bedingungen eintreten für ihre Wirksamkeit, das betreffende Phänomen beobachtet werden kann. Es ist also ein in gewissem Sinne leeres Astractum, welches aber, wenn es richtig verstanden wird, in der That die wirklich vorkommenden Phänomene beschreibt." Hermann von Helmholtz, *Vorlesungen über theoretische Physik: Einleitung zu den Vorlesungen über theoretische Physik* (Barth, 1903).

Just as he had argued that the general concept of quantity "objectified" directly observable effects when comparing objects through experiments, he argued that the concept of force as a noun is more-or-less an empty abstraction, a linguistic sign that ultimately stands for specific empirical laws. But in order to fully flesh out this parallel by clarifying in what sense quantities were mere objectification of experimental phenomena, we must now turn to specific cases in 19th century scientific measurement practices.

In the next section, I proceed from the content of Helmholtz's report on electrical units defined at the 1881 International Electrical Congress, to the experimental methods for measuring these quantities both in their original contexts of discovery and in the "absolute system" that integrated different units in a single experimental setup. The latter guided the experimental work that determined electrical units before and after 1881. I argue that these quantities were defined in terms of phenomenal laws exemplified by certain canonical experiments. Therefore, the experimental method, through which the electrodynamic, -static and -magnetic phenomena in question were compared, became an inseparable component of the definition and meaning of these units. This called for a conception of quantity different from one based on unit and part-whole relationship. Helmholtz's account of measurement that characterized the term quantity by heterogeneous "methods of comparison" and considered their mathematical structure as interpretations of "exceptional occurrences" brought forth by specific operations in experiments, reflected the overarching approach to measurement practices in his contemporary science.

2.7. How Electrical Quantities Were Determined in 1881

In a report to the Union of Electricians, Helmholtz reviewed the conversations that took place in the first International Electrical Congress in Paris. He had travelled there earlier in 1881 and served on the Electrical Standards Committee (with the title of "Foreign Vice President") to negotiate with representatives of other European nations on the matter of electrical units, including the choice of theoretical definitions,[62] nomenclature, the appropriate experimental methods, and the material instantiation for resistance unit. Standardization of electrical quantities had become an urgent matter in recent years, he noted, due to a rising electrical industry and the tremendous amount of capital at stake: the manufacturers of cables and different electrical appliances and components (e.g., the dynamo and electrical lighting) had to agree on their measurements, while scientists could benefit from standardized instruments.[63]

He then went on to explain the principles by which standards were defined in the "absolute" system of measurement. There had been two major systems of interconnected units in the past few decades: the electrostatic and electromagnetic systems, which defined units in terms of electrostatic and electromagnetic laws, respectively. Because electrical technologies like the dynamo and telegraphy largely depended on electromagnetic effects generated by currents, the electromagnetic system was a natural choice for the first standardized international system of electrical units. In the electromagnetic system, three units were connected with each other and determined together: electromotive force, current and resistance. The experimental methods for executing these two systems of measurement, he noted, had been devised by two German scientists earlier in the century: Carl Friedrich Gauss and Wilhelm Weber. Gauss formulated the laws of interaction between two quantities of magnetism, and Weber extended this law to the interaction between two current-carrying conductors based on Ampère's work in

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[62] There were some controversies regarding whether Gauss's definition of unit magnetic pole should be preserved. Helmholtz believed that it should, because without Gauss's phenomenological definition, there would be disagreements over the dimension of the magnetic pole in Weber's and Maxwell's frameworks. See Helmholtz, "XLVIII. On Systems of Absolute Measures for Electric and Magnetic Quantities."

[63] Hermann von Helmholtz, "Ueber die Berathungen des Pariser Kongresses, betreffend die elektrischen Masseinheiten.," *Elektrotechnische Zeitschrift*, no. Zehntes Heft (Oktober 1881).

electrodynamics. It was through these laws that unit magnetism and current were defined. Now, how was unit current defined according to Ampère's laws? Helmholtz gave a quick description of the means of calculating the quantity of current: when calculating the electromagnetic force between two electrical circuits with currents in them, placed at arbitrary distance and location with respect to each other, one would first integrate the mechanical effects between pairs of infinitesimal segments composing the circuits, and then integrate these effects on a macroscopic scale. For an arbitrary pair of such infinitesimal segments, the "currents" in them should be broken down to two components: one along the line of their connection, the other vertical to that line. The attractive or repulsive force between the two segments would be proportional to the vertical components of the two "currents" and inversely proportional to their separation, depending on whether they flowed in the same or opposite directions. A unit current, Helmholtz noted, was defined *directly* in terms of this effect, that is, by equating the interaction between two current-carrying conductors with a mechanical force: $k = 2\, i \cdot j\, \frac{\lambda\, l}{r^2}$. Because force k was conventionally measured by gravitational force—the product of mass and the gravitational factor, an acceleration, the product $\mathrm{i} \cdot j$ was "*a magnitude of the same kind* (eine Grösse gleicher Art) as force"[64] and each current element i, j had the dimension $\frac{\sqrt{ml}}{t}$ in units of lengths, mass and time. The unit quantity of magnetism could be defined by precisely the same principle (since the attractive or repulsive force between two quantities of magnetism is also proportional to the product of their "quantities," and inversely proportional to the distance of separation). Therefore, unit magnetism had the same dimension as unit current as they were both measured by mechanical force.

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[64] Ibid.

Now, given the unit magnetism and unit current, one could establish the relationship between these two units by measuring the magnetic effect of a current. A common way to do this was to put a compass needle at the center of a current-carrying coil, and the deflection of the compass needle was proportional to the net force exerted on it by both the current and terrestrial magnetism. The ratio between terrestrial magnetic force and the electromagnetic force exerted by the current was equal to the tangent of the angle of deflection. Now, given these, it became possible to calculate the resistance of any conductor in principle. The development of heat in conductors had been found to be proportional to the square of current intensity, time and resistance, namely, the product i^2wt. The cause of this heat, Helmholtz noted, was lost mechanical work and should be measured as such. That is, if i^2wt had the same dimension as mechanical work, and i^2 had the same dimension as force, then resistance w had the same dimension as velocity, $w = \frac{l}{t}$. Alternatively, he noted, one could obtain the same result for resistance by considering the induced current (by changing magnetic flux), since the latter is converted from mechanical work.[65]

So far, Helmholtz was recapitulating some of the most long-standing methods to define and measure electrical quantities that survived changes in high-level theories in electromagnetism in the 19th century. Although he described an interconnected system of measurement that seemed perfectly neat and free-floating, the "quantitative-ness" of these concepts had been discovered separately. These discoveries were the result of various experimental methods to exhibit the effects of various charge-to-charge, current-to-current, and current-to-magnet interactions in mechanical frameworks. The definition of quantities, the electrostatic, -dynamic

[65] Ibid, 484-5.

or -magnetic laws in which they occurred, and the experiments which became the model of measuring instruments, were all inextricable from one another in their contexts of discovery. To illustrate this point, I review some of the most elementary electrical quantities in their context of discovery, since the so-called "absolute" system can only make sense given that the experimental work behind the definition of these quantities are made clear.

Take the definition of "electrical quantities" (charge) for example. Charles-Augustin de Coulomb's statement of the force law between two "quantities of electrical fluid," inversely proportional to their distance of separation, was both enabled and physically embodied by his torsional balance. The instrument balanced the attractive or repulsive force between two equally charged (the "quantities of electrical fluid" were equally distributed between two identical spherical conductors by direct contact) spheres—with the torque in the thread suspending one of them. With this equilibrium of force, it became possible to formulate an equation outputting the unknown "quantity of electricity" as a function of the variables in the experiment. This original setup became the model for most modified versions of the electrometer, which remained as one of the most accurate measuring instruments throughout the 19th century. Similarly, as already mentioned earlier, unit magnetism was also based on the attractive or repulsive force between the "quantities of magnetism" (it therefore had the same dimension as unit charge). Its official definition at the first international congress was precisely "free magnetism that exert unit force at unit distance on the same quantity of magnetism."[66] The technique of measurement was a bit more complex, requiring two separate experiments invented by Gauss in order to separate terrestrial magnetism, which varied by location, from calculation, but the idea was the same.

[66] F. Neesen, "II. Verhandlungen der Ersten Sektion und der Kommission für die Elektrischen Einheiten," *Elektrotechnische Zeitschrift*, no. Zehntes Heft, 2. (Oktober 1881): 399–409.

André-Marie Ampère's force law between "current elements"—in his original experiments—was materialized by his experimental setup as well, which allowed the torque between two linear or twisted conductors, resulting from their electrodynamic interaction, to be balanced by gravity or other mechanical forces. This equilibrium condition allowed him to establish equations that had mechanical units on one side, and the factors that were proportional (the current itself) or inversely proportional (distance) to this force on the other (though his equations concerned infinitesimal elements, it was integrated on a macroscopic scale to compare with the experimental effects).[67] The quantity of "current intensity" was defined in no other terms than this force equation. Ampère was well-known for his agnosticism towards the nature of the electrical and magnetic "fluids" and avoided hypotheses that were not grounded in directly observable effects.[68] As he proposed in his famous 1826 memoir, to describe the so-called "current" in terms of *numbers*, one should only compare different currents in terms of the "actions which they exert at the same distance on a similar element of any other current" placed in a direction perpendicular to the straight lines joining the mid-points of the currents. The ratio between the *forces* was the measure of the intensity of one current, supposing the other is the unit.[69]

An instrument that directly instantiated Ampère's scale for current intensity was Weber's bifilar electrodynamometer, which Weber built to both verify Ampère's mathematical laws and measure currents accurately. The device consisted of a rotatable coil suspended by the two wires

[67] James R. Hofmann, *André-Marie Ampère: Enlightenment and Electrodynamics* (Cambridge University Press, 1996); Jed Z. Buchwald and Robert Fox, eds., *The Oxford Handbook of the History of Physics* (OUP Oxford, 2013).

[68] Jed Z. Buchwald and Robert Fox, eds., *The Oxford Handbook of the History of Physics* (OUP Oxford, 2013), 288.

[69] André-Marie Ampère, *Théorie des phénomènes électro-dynamiques, uniquement déduite de l'expérience* (Méquignon-Marvis, 1826).

feeding the current, placed at the center of a static coil, corresponding directly to Ampère's own equilibrium experiments.[70] The electrodynamic torque would cause the suspended coil to rotate, and the torque was found to be inversely proportional to the square of current intensity.[71] The galvanometer—a more commonly used device for comparing currents since it was simpler than the electrodynamometer (also invented by Ampère, though Biot and Félix Savart are credited for the better version of the mathematical law)—compared currents by their magnetic effects or force on unit magnetism as well.[72] The principles of comparing currents by their magnetic effects, embodied by every galvanometer used in the 19th century, directly translated into the official definition of unit current at the 1881 electrical congress: given a wire of length l with current i passing through it, and μ free magnetism in distance L perpendicular from the current, the force between them would be expressed in the proportionality $k = const. \frac{l \cdot i \cdot \mu}{L^2}$. The unit current would be, therefore, that which exerted $\frac{l}{L^2}$ force on a unit of magnetism (determined by Gauss's method) according to the above-mentioned relationship.[73]

[70] Darrigol, Olivier. *Electrodynamics from Ampère to Einstein*. (OUP Oxford, 2003), 58.

[71] Ibid, 59.

[72] Weber's delicate bifilar electrodynamometer was difficult to reproduce, and not suitable for practical use. An electrodynamometer that could be manufactured for industrial purposes—suitable for measuring larger currents than those at Weber's laboratory—only came into existence in the 1880s, designed by O. Frölich for Siemens & Halske. F. Kohlrausch developed a similar electrodynamometer around the same time. See O. Frölich, *Die Entwickelung der Elektrischen Messungen*, die Wissenschaft. Sammlung Naturwissenschaftlicher und Mathematischer Monographien.5. Hft. (Braunschweig: F. Vieweg und sohn, 1905), https://catalog.hathitrust.org/Record/005693992.

[73] Ibid.

It should be noted that proponents of the field theory, such as James Clerk Maxwell, thought that the "absolute" definitions were only provisional, but since he was unable to measure the "density, elasticity, &c., of the magnetic medium" required for his field theory at the time, he adopted the conventional formulae, noting that they could be susceptible to future modifications. See Daniel Jon Mitchell, "What's Nu? A Re-Examination of Maxwell's 'Ratio-of-Units' Argument, from the Mechanical Theory of the Electromagnetic Field to 'On the Elementary Relations between Electrical Measurements,'" *Studies in History and Philosophy of Science Part A*, The Making of Measurement, 65–66 (October 1, 2017): 87–98, https://doi.org/10.1016/j.shpsa.2016.08.005.

In both of the cases just discussed, 19th century instruments for measuring static electricity or current recreated the experimental conditions that enabled discovery of these quantities. They embodied the associated laws in electrodynamics and -statics. The scales of these

Fig. 28.

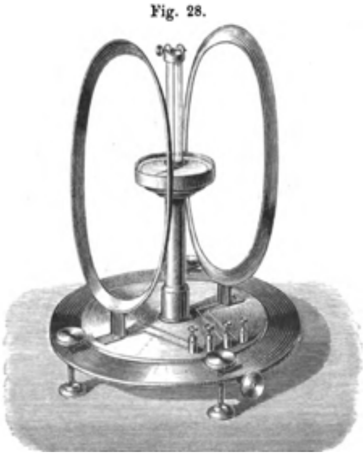

Fig. 52.

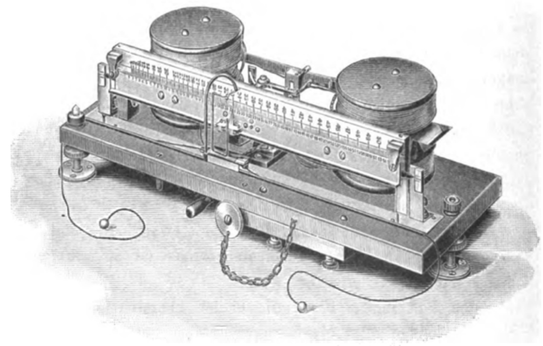

Figure 6 (Left) Helmholtz's 1849 improvement of a tangent galvanometer. (Right) An electrodynamic balance based on Helmholtz's design in 1881.
The magnetic needle of the tangent galvanometer is not placed at the center of a single coil, but between two parallel coils on both sides of the central plane in which the magnetic needle swings.
To use the electrodynamic balance, weights are placed on the pans to balance the electrodynamic forces until an equilibrium was restored.
Source: Frölich, O. *Die Entwickelung der Elektrischen Messungen. Die Wissenschaft. Sammlung Naturwissenschaftlicher und Mathematischer Monographien*.5. Hft (Braunschweig: F. Vieweg und sohn, 1905), 41, Fig. 28; Ibid, 78, Fig. 52.

quantities were not the visible scale on the face of the measuring instrument. Instead, they were embodied by the entire apparatus and the lawful effects exhibited by the measuring process, in a very literal sense, as the physicist J. C. Poggendorff complained in 1842: it was difficult to establish a scale for current independent from each instrument, because the scale had to be specifically computed from each individual component of the device: the dimensions and conditions of the coils, the magnets, and so on.[74]

[74] J. C. Poggendorff, "Von dem Gebrauch der Galvanometer als Messwerkzeuge," *Annalen der Physik* 132, no. 6 (1842): 324–44. Electrometers provide another telling example, which compare currents by chemical (rather than mechanical) means based on an immediate experimental effect (the decomposition of electrolytes, the extent of which only depended on time and current intensity). At the same time when Michael Faraday discovered the

Upon examining the origin of other quantitative concepts in electrodynamics and -magnetism, one finds similar stories: they came into existence when a certain regularity in a particular experimental arrangement was discovered, in a way that connected to previously known regularities. For instance, the concept of resistance was formed when the Gymnasium teacher Georg Ohm varied the length, form and material of conductors and found the magnetic action of the needle placed nearby to be impacted accordingly. In order to establish the quantitative relation, he controlled the increments of the source of power (Ohm called this the "exciting force") using a thermopile, an instrument that represented another set of experimental regularities previously discovered by T. J. Seebeck. The thermopile consisted of alternating plates that would produce current when being heated. When heating one, two, three, four…pairs of these plates, Seebeck had found that the magnetic effect of the current produced increased proportionally (Figure 7).[75]

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phenomenon, he already proposed to use it as a means to measure currents: "I endeavored upon this law to construct an instrument which should measure out of the electricity passing through it, and which, being interposed in the course of the current used in any particular experiment, should serve at pleasure, either as a comparative standard of effect, or as a positive measurer of this subtle agent." A scale, according to this method, corresponded to the weight of metal deposits or the volume of gas released, depending on the electrolyte.

Michael Faraday, *Experimental Researches in Electricity*, vol. 1 (London : Dent ; New York : Dutton, 1922), http://archive.org/details/experimentalrese00faraiala.

[75] Christa Jungnickel and Russell McCormmach, *Intellectual Mastery of Nature: The Now Mighty Theoretical Physics, 1870-1925*, vol. 1, 2 vols. (University of Chicago Press, 1986).

Fig. 76.

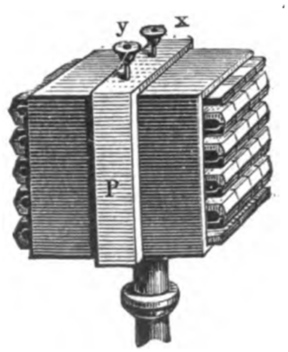

Figure 7 Nobili's thermopile, consisting of alternating metal plates. Heating the conjunctions produces a current proportional to the temperature difference.

Source: Frölich, O. *Die Entwickelung der Elektrischen Messungen*, 111.)

The definitions of electrical quantities I have discussed so far were mostly given in the early 19th century. How did they come to be measured in the second part of the 19th century, especially around 1880? To answer this question, one must turn to the so-called "absolute system" of measurement, according to which electrical quantities and standards were determined both in the years leading up to the first International Electrical Congress and according to the agreements reached at the 1881 congress itself. From a pragmatic point of view, the standardization of cables and appliances across the electrical industry could be achieved simply by having a single material standard serve as the resistance unit for as many manufacturers as possible. For instance, Siemen's mercury standard, as Helmholtz mentioned in his report, had very much dominated the Germany and Austrian market, as well as parts of Russia and eastern nations, thus the lack of standardization was more of a problem for the British than the Germans.[76] However, for scientific purposes, the "absolute" system more faithfully instantiated the original definitions of these quantities by defining their units in terms of the physical laws they occurred in, thereby expressing units of current, electromotive force, and resistance in units of length, mass and time. The "absolute system" safeguarded against the potential variation of a

[76] He noted this also because the British standard „Ohm" created by the British Association in the 1860s and the Siemens's unit were the two main competitors for the European market.

material standard of resistance (usually a coil), which always remained a possibility however stable the chosen metal was thought to be.[77] In this case, the laws of electrodynamics and electromagnetic induction, on which the experiment depended, were the true invariant in the measuring process.

First proposed by Weber in an 1851 paper, the schema of the experiment was as follows (Figure 8): a malleable conductor is shaped into two connected circles, A and B, placed in the same plane (the line through their centers was parallel to the direction of earth's magnetic field). C is a magnetic needle measuring the strength of any current in the conductor. When A is twisted to be perpendicular to AB (and thus terrestrial magnetism) at a uniform angular speed, a current is induced in the conductor and is measured by C. The unit electromotive force, according to the laws of induction available then, was defined directly in terms of this method: it was that which "unit of measure of earth's magnetism exerts upon a closed conductor, if the latter is so turned that the area of its projection on a plane normal to the direction of the earth's magnetism increases or decreases during the unit time by the unit of surface."[78] In other words, unit electromotive force was that which was induced by this experimental setup, when the conductor was twisted at unit angular speed in a given terrestrial magnetic field. The unit current, on the other hand, was defined as that which exerted unit force on the bar-magnet when circulating through unit area. If unit resistance was "that which a closed conductor possesses when a unit measure of electromotive force produces a unit measure of current intensity," then the value of a particular resistance standard serving as the induction coil in this experiment could

[77] See Olesko, Kathryn. "The Meaning of Precision: The Exact Sensibility in Early Nineteenth-Century Germany."

[78] Wilhelm Weber, "On the Measurement of Electric Resistance According to an Absolute Standard.," *The London, Edinburgh and Dublin Philosophical Magazine and Journal of Science* 22, no. 146 (1861): 226–40.

be calculated as a function of the induced current (measured by the force on the compass needle) and the electromotive force (measured by the dimension of the apparatus).

Based on this schema, there were a few adaptations that had been carried out by various scientists or scientific groups between 1851 and 1881. As I mentioned earlier, the British Association led by Maxwell and Fleming Jenkin had already performed a set of experiments in the 1860s based on Weber's design, though with slightly modified apparatus (see below). They produced a standard called the "Ohm," which nevertheless was found to deviate from its "true" theoretical value by various experimenters. In addition, there were several other experiments conducted by various individual scientists. In an 1880 report to the Royal Saxon Society for the Science, Weber gave an overview of all of the major adaptations based on his original schema in the past three decades. There were four main adaptations. The first method used terrestrial magnetism for induction by turning the coil by ninety degrees in the manner specified earlier, just as Weber described in the 1851 paper. It measured the induced current by measuring the oscillation period of a magnetic compass needle, much smaller than the dimension of the coil. This method need not measure the value of local terrestrial magnetism itself, because the latter was a factor in both the extent of induction by rotating the conductor and the needle's oscillation

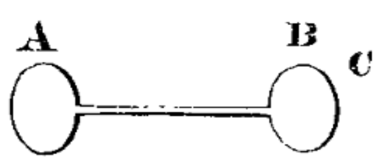

Figure 8 Weber's description of the apparatus for determining resistance in absolute measure. Source: Weber, Wilhelm. "On the Measurement of Electric Resistance According to an Absolute Standard," 230.

in earth's magnetic field. The factor was thus canceled out in the calculation. Weber conducted the experiment himself in 1880 at the old, iron-free observatory at Pleissenburg, Leipzig, accompanied by several physicists and a mechanist (among those present, he named Eduard Riecke of Göttingen, Heinrich Weber at Braunschweig, and a Dr. Weinek from the Hessen

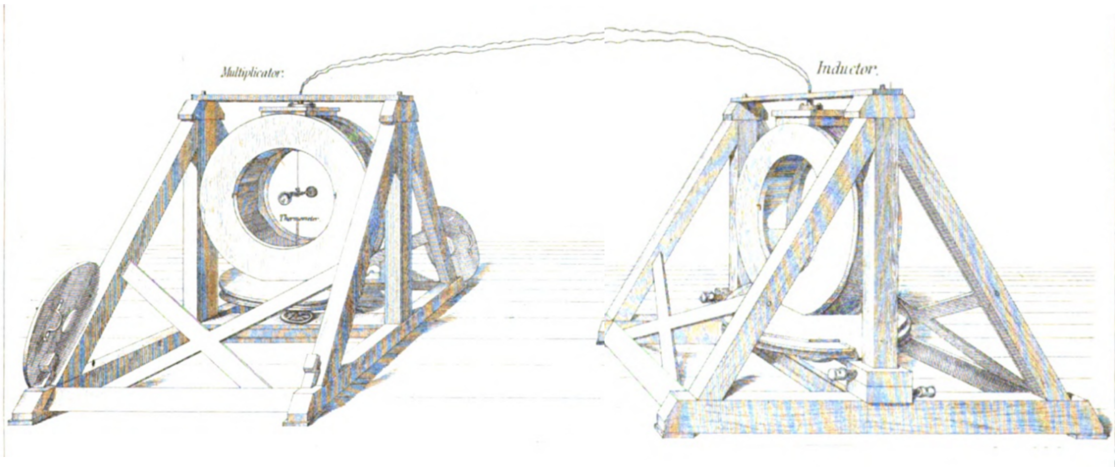

Figure 9 Weber and Zöllner's 1880 apparatus for absolute determination of resistance. The inductor on the right rotates in terrestrial magnetic field to generate a current; the multiplicator on the left measures the induced current by a magnet suspended at the center of the coil.
Source: Weber, Wilhelm, and F Zöllner. „Ueber Einrichtungen zum Gebrauch Absoluter Maasse in der Elektrodynamik Mit Praktischer Anwendung," Tafel. II.

observatory). The apparatus was constructed by the Hamburg firm A. Repsold & Söhne, which relied on a weight-releasing mechanism to generate uniform rotation of a large induction coil too heavy to be handled manually—made from a copper wire of 414.95 kilograms in weight and 3.33 millimeter in thickness, constructed by Werner Siemens, for both the induction coil and the multiplicator (Figure 9).[79]

The second method used the same apparatus but a multiplicator tightly enclosing an astatic needle, whose deflection was calculated by a method called damping. Measurement of the local terrestrial magnetism and the initial moment of the needle was required for this experiment. Friedrich Kohlrausch used this method in 1870 and conducted the experiment at the Göttingen

[79] Wilhelm Weber and F Zöllner, "Ueber Einrichtungen zum Gebrauch Absoluter Maasse in der Elektrodynamik mit Praktischer Anwendung," *Berichte über die Verhandlungen der Königlich Sächsischen Gesellschaft der Wissenschaften zu Leipzig. Mathematisch-Physische Classe.* 32 (1880): 77–143.

magnetic observatory using an apparatus older than Weber's. He compared the Siemen's mercury unit and the B.A. unit in the "absolute" units and reported that the B.A. "Ohm" deviated from its true value (supposedly 10^7millimeter/second) by almost 2%.[80] The third method was almost identical to the first one but measured the deflection of the compass needle through damping, instead of the period of oscillation. The fourth method had been used by the British Association under the recommendation of William Thomson in 1862. It required the uniform rotation of a multiplicator of known dimensions in the earth's magnetic field and the observation of the deflection of a small magnetic needle suspended at the center of the coil.[81] An adaptation of this method was made in 1873 by H. Lorentz and was later reproduced in Lord Rayleigh and H. Sidgwick's experiment in 1883, when the British Association performed further experiments to determine the value of the "Ohm" and the mercury unit. Rayleigh and Sidgwick used a circular disc of metal, instead of a coil, to uniformly rotate in a magnetic field created by a battery current circulating through a coil coaxial to the disk. Instead of having a galvanometer measure the induced current, they balanced the galvanometer at zero-degree deflection, by connecting it to both the induction coil and the external battery, thus cancelling the external battery current with the induced current.[82]

The above discussion summarizes the kinds of quantity and their methods of measurement that a late 19th century practicing scientist like Helmholtz faced on a regular basis. It was within these experimental contexts that the following list of definitions, given at the 1881

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[80] Ibid, 426.

[81] Ibid.

[82] Lord Rayleigh and H. Sidgwick, "Experiments, by the Method of Lorentz, for the Further Determination of the Absolute Value of the British Association Unit of Resistance, with an Appendix on the Determination of the Pitch of a Standard Tuning-Fork," *Philosophical Transactions of the Royal Society of London* 174 (1883): 295–322.

international congress as the guiding principles for determining electrical units by the "absolute" methods, became concrete and meaningful:

1. "Unit current is that which exerts a force of $\frac{l}{L^2}$ on unit magnetism" given a conductor of length l and current i and μ free magnetism at a distance L, in the direction perpendicular to the direction of the current.
2. Unit of electromotive force is "that which is induced in a straight conductor of unit length at a location with unit magnetic force everywhere, if the conductor is moved with unit speed to be perpendicular to the plane in which conductor and magnetism exerting unit magnetic force."
3. "Unit resistance is the same resistance, with which unit electromotive force gives rise to unit current."
4. "Unit quantity of electricity is that which in unit time moves through the cross-section of a conductor of unit resistance."
5. "Unit capacity is that of a condenser, which, charged by unit electromotive force, contains unit quantity of electricity."[83]

…

As we can see, these units were not so much parts constituting the whole, as derived from the electrodynamic and -magnetic laws associated with each electrical quantity. Their definitions were grounded in the experimental methods for their determination. Units were *products* of the measurability of the concepts in electricity and magnetism. Upon examining the experimental work that underlay the definitions of each electrical quantity, we find that none of the notions in

[83] Neesen, F. "II. Verhandlungen der Ersten Sektion und der Kommission für die Elektrischen Einheiten," 400.

traditional philosophy of quantity—part-whole relationship, homogeneity, divisibility, or the distinction between extensive and intensive magnitudes—really played any primary role in explaining how the concepts discussed so far became quantitative. What mattered was the empirical work uncovering regularities in experiments and the interconnectedness of these regularities. The nouns "charge," "current" "resistance" and so on only stood for what would occur in particular large-scale experiments. Displaying many characteristics of modern "big science," involving enormous amount of labor and scientific attention, the experimental determination of electrical quantities highlights the embeddedness of abstract quantitative concepts in empirical procedures. The materiality of instrumentation that made theoretical concepts in physics possible in the 19th century was hardly a thing that could be ignored. No wonder that in Helmholtz's 1887 "Counting and Measuring," the method of measurement is at the forefront in characterizing the abstract concept of quantity. Quantities and their scales are only "objectifications" of object's capacity to bring forth certain effects in the act of comparison. On its own, the generalized "quantity" is only the collective name for all individual quantitative terms measured separately and in different ways, instead of a synthetic a priori concept on whose basis empirical measurements need to be grounded.

As Kant's dictum that science must be grounded in mathematics was actively pursued in the 19th century, the scope and method of mathematization expanded significantly since Kant's time, to the point that the traditional account of quantity seemed almost irrelevant to explain some of the most important quantitative concepts in 19th century physics. In effect, Helmholtz's theory of quantity filled a gap between theory and practice left by Kant. When many of his contemporary philosophers continued to rely on the notion of unit and part-whole relationship to conceptualize quantity and measurement, these notions became increasingly out of synch with scientific practices.

Chapter 3 Measuring without Units: Temperature, Sensation, and other Non-additive Quantities

3.1. Introduction

What do temperature (in practical thermometry), stellar magnitudes (measured by photometric methods), and sensory discrimination (as described by Weber's law) have in common? In the context of late 19th century sciences, these concepts posed major challenges to existing philosophical theories of quantity and measurement. As shown in chapter 1, the conception of quantity through part-whole relationship, based on the archetype of geometrical magnitudes and reinforced by the Kantian definition of quantity in terms of "the composition of the homogeneous in intuition," held strong sway among many 19th century authors. The case of temperature, brightness intensity, discernibility in sensation, and potentially other concepts that were products of the expansion of quantitative methods in the 19th century sciences, complicated the way quantities were understood. Strictly speaking, these concepts cannot be considered additive quantities composed of equal units. Take temperature as an example: what exactly does it measure? It does not quite make sense to say that 10° contains twice the degrees of heat as contained in 5° (though temperature does indicate degrees of heat), neither does it make sense to add one temperature to another. Even with the thermodynamic definition of a uniform temperature scale, it does not make sense to speak of "adding 5° to 10°" universally as one can speak of "adding 5 cm to 10 cm," without specifying the specific heat capacity of the individual bodies whose temperatures are being measured.

Partly, the non-additivity of quantities like temperature is related to the fact that their intervals cannot be directly compared. Temperature is not directly accessible but must be represented indirectly by something else. In thermometry prior to William Thomson's absolute scale, it was represented by the change in volume or pressure of a chosen thermometric substance—mercury or hydrogen, for instance. While it might be a straightforward matter to construct a uniform scale for length or volume, it was not so with making a uniform thermometric scale, because there was no way to directly compare temperature intervals. Had all substances exhibited the same pattern of expansion with temperature increase, it might have been possible to define a uniform scale for temperature through its 1-1 mapping with volume, but this is not the case. Each candidate for thermometric substance exhibited more-or-less different behaviors with regard to thermal changes. One might ask, as researchers in thermometry did, which particular gas or liquid expanded in proportion to the "real" temperature—when early 19th century experiments discovered that gases expanded in proportion to *each other* with thermal changes, this did lead scientists to believe that gases expanded uniformly with the "real" temperature. But on a closer look, the logic is circular: by what standard can we say that gas expansion is uniform with respect to temperature, if temperature is defined by none other than the behavior of individual substances, including gases?[1] This circularity was identified by Ernst Mach in the *Principles of Theories of Heat*, and more recently discussed by historian of science Hasok Chang in a series of publications. Interestingly, the circularity was not unique to temperature measurement. One of the most controversial topics in 19th century sciences is Gustav Fechner's psychophysics, in which a uniform scale was similarly projected onto sensation as it was projected onto gas expansion with respect to temperature. Fechner based his formulation of a functional law between "just-noticeable-differences" in sensation and physical stimuli on the discovery that experimental subjects almost always noticed a difference in stimuli (e.g., differences between two

[1] The solution to this problem, as shown by William Thomson's absolute temperature scale, is to find an external reference that is not an arbitrarily chosen thermometric substance. To this end, Thomson defined a new scale of temperature whose ratio is fixed by the ratio of work generated to heat input in an ideal Carnot cycle. See section 3.4 "Absolute temperature as a concept of level."

weights, between the intensities of two light sources, etc.) only when the physical intensities of these stimuli (measured by physical weights, or photometric methods) increased by a fixed ratio. That is, given S as a measure of the physical intensity of a stimulus and ΔS its variation, a difference between stimuli was noticed when $\Delta S/S$ = constant. Fechner therefore concluded that he had found an additive scale for sensation composed of equal units, and these units were none other than the just-noticeable differences $\Delta E = \Delta S/S$. But how could one know that the differences in sensation on different locations of the scale were equal to one another, if there was no way to directly access sensations other than by marking their intervals by $\Delta S/S$? Fechner himself defended the equality of perceived differences by claiming that such equality was proved by facts of observation—those very facts that led him to formulate the scale for sensation in the first place. But according to the empirical facts Fechner drew on, differences were *noticed* given constant ratio of physical stimuli, this is not equivalent with the claim that those noticed differences in sensation were equal. The situation is analogous to primitive temperature measurement. In both cases, what were measured could not be directly accessed, but must be represented by something else; in both cases, the proportionality between that something else was mistaken by historical actors for the proportionality between the thing to be measured. Many of Fechner's contemporaries like Wilhelm Wundt acknowledged the validity of Weber's law but not Fechner's construction of an additive scale for sensation. Many others, like Rudolf Elsa, Ernst Mach and Alexius Meinong, explicitly drew a parallel between the psychophysical scale and conventional thermometric scales, since they suspected both of being merely definitional.

For Ernst Mach, the circularity involved in projecting uniformity onto scales that must be represented by other measured quantities could also be extended to Newton's absolute time, since time was thought to be measurable only by uniform motion, but no uniformity could be proved without knowing how to measure time. Mach argued that both temperature or universal time are abstract ideas falsely granted a concrete reality; in fact, they only represent the interdependence of bodies: in measuring temperature we coordinate the behavior of one body in relation to thermal change by that of another, just as in measuring time we gauge the motion of one body by the motion of another.

Many of Fechner's contemporaries agreed that even if the uniformity of the scales could not be proved, this would not render Weber's law, or by extension, the photometric scale for brightness (which inspired Fechner by showing that errors in brightness perception did conform to his law) useless. The photometric brightness scale was widely used in 19th century astronomy independent of psychophysics, although the scale was neither additive nor necessarily uniform. Similarly, conventional thermometry did play a crucial role in offering precise knowledge and control over natural phenomena despite being "definitional." Indeed, the logarithmic law Fechner discovered continues to be a part of contemporary cognitive science today.[2] These scales were not entirely arbitrary products of the minds of their authors, but often established on experimental regularities. Among late 19th century authors, the prevalent use of non-additive, semi-quantitative scales pushed the boundary between measurement and numerical coordination. In 20th century measurement theory, ordinal scales without equal units would be admitted as a legitimate form of measurement, especially in social sciences.[3]

What psychophysics did succeed in doing was to bring attention to the inability of unit-based conception of measurement to explain the measurement of basic quantitative concepts in

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[2] Larry Hardesty, "What Number Is Halfway between 1 and 9? Is It 5 — or 3?," MIT News, October 5, 2012, https://news.mit.edu/2012/thinking-logarithmically-1005; Lav R. Varshney and John Z. Sun, "Why Do We Perceive Logarithmically?," *Significance* 10, no. 1 (2013): 28–31, https://doi.org/10.1111/j.1740-9713.2013.00636.x.
[3] See S.S. Stevens, "Measurement," in *Scaling: A Sourcebook for Behavioral Scientists*, ed. Gary Maranell (Routledge, 2017).

the exact sciences. Direct comparison with units independent of anything external is impossible even in the case of spatial and temporal measurement. This was Fechner's defense for using external stimuli to stand for sensation. Wundt, for instance, noticed that "the absolute constancy of natural laws has emerged the actual condition of every time measurement," implicitly referring to existing contention regarding the role of Newton's first law as a disguised definition of time.[4] For Alexius Meinong, indirect or "surrogative" measurement like that of temperature or sensation deserved its own philosophical analysis. The different values on the scales do not represent additive units, but rather a relation of difference ("dissimilarity"), whose values can be correlated with an external function involving other variables, and this difference can only be considered additive in the sense of relational composition. In *Principles of Theory of Heat*, Mach used the notion of "concept of level" to designate non-additive quantities like temperature (even for Thomson's absolute temperature) or electric potential. He noted: "the conception of temperature is a conception of *level*, like the height of a heavy body, the velocity of a moving mass, electric and magnetic potential, and chemical differences."[5] Implicitly, the scales of these concepts of level could become uniform, but not strictly speaking additive like length or mass.

In a sense, some of the issues with non-additive quantities born out of functional relationships discovered in an experimental context harken back to Kant's argument that speed could not be conceptualized through additive parts. In late 19th century, those questions were particularly brought to the attention of scientists, because of a combination of long-lasting difficulties in thermometric measurement and widespread controversies raised by psychophysics. Considering the fundamental role some of those concepts played in entire fields of research and

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[4] See the section "Theories of indirect measurement" for the reference.

[5] See the section "Mach's analysis of the temperature concept" for the reference.

the prevalent use of instruments for their measurement in practice, the lack of clarity on what precisely was measured and how it was measured within the existing philosophical framework led some commentators to reflect on the foundation of measurement in general. The resulting discussions further proved how the conventional understanding of quantity through the lens of length and other geometrical magnitudes was incapable of addressing many key issues in the foundation of measurement.

3.2. 19th Century Thermometric Research

The fact that bodies and gases dilate with heat had been known and experimentally demonstrated since the antiquity, and the first air- or liquid- in-glass thermometers with numbered marks to represent degrees of hot and cold were constructed in Galileo's time based on the volume expansion of bodies and gases. The demand for more precise instruments called for a better understanding of the relationship between volume expansion and degrees of heat stood for by temperature. Nevertheless, how heat was determined and measured depended on hypotheses. For instance, during the 18th century it was believed that the temperature of a mixture of 1 portion of boiling water and 9 portions of freezing water would be 10°. Mercury, whose volume expansion was close to being proportional to the temperature values predicted this way, was the superior thermometric substance.[6] In contrast, the caloric theory popular near the end of the 18th century argued that the heat substance (caloric) interacted with matter on a microscopic level; some portions of the caloric were responsible for exerting an impact on the thermometer, while others were responsible for overcoming or acting on intermolecular forces

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[6] This was challenged by the observation that heat can be absorbed without raising the temperature, as in the case of melting ice), temperature values could no longer derived by the method of mixture, as heat might be absorbed by the mixture without impacting its temperature, or impacting it in unexplained ways.

Hasok Chang, "Spirit, Air, and Quicksilver: The Search for the 'Real' Scale of Temperature," *Historical Studies in the Physical and Biological Sciences* 31, no. 2 (2001): 249–84.

between the material particles, thus becoming latent. According to this theory, since gas molecules were widely separated and therefore exerted a weaker force among themselves, almost all caloric absorbed were expended in raising its temperature, hence the behavior of gas under heating or cooling most accurately reflected the "real" quantity of caloric absorbed.[7]

The explanation offered by the caloric theory was compatible with the experiments conducted by early 19th century researchers such as Joseph Lois Gay-Lussac and John Dalton, who separately demonstrated in 1802 that all gases largely expanded to the same extent between the melting point of ice and the boiling point of water under constant pressure. Gay-Lussac experimented with atmospheric air, oxygen, nitrogen, hydrogen and found that all expanded about 137.5% in volume *within the above-mentioned range*.[8] In a subsequent set of experiment, he showed that within this range, the expansion of the air was close to being proportional to the indications of the mercury thermometer (Figure 3.1).[9] If the mercury thermometer indeed marked the "real" temperature "out there in nature," then this would indicate that gas expanded proportionally to degrees of temperature as the caloric theory predicted. While this was indeed Dalton's conclusion, namely that his experimental results showed that "all elastic fluids under the same pressure expand equally by heat," Gay-Lussac explicitly pointed out that since the precise nature of the connection between heat and temperature indicated by any particular thermometer was not known, their results only facilitated the very urgent practical need to calibrate different gas thermometers: "We believe, it is true, in general, equal divisions of its [gas thermometer's]

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[7] Ibid. See Hasok Chang, *Inventing Temperature*; Buchwald and Fox, *The Oxford Handbook of the History of Physics*.

[8] J.B. Biot, "Extract from Biot's Treaties on Physics, Chapter IX of Volume I.," in *The Expansion of Gases by Heat*, ed. Wyatt William Randall (American book Company, 1902), 45; Jed Z. Buchwald and Robert Fox, eds., *The Oxford Handbook of the History of Physics* (OUP Oxford, 2013), 483.

[9] J.B. Biot, "Extract from Biot's Treaties on Physics, Chapter IX of Volume I.," in *The Expansion of Gases by Heat*, ed. Wyatt William Randall (American book Company, 1902).

scale correspond to equal increments of caloric; but this view is supported by no very positive fact."[10]

The disclaimer did not prevent widespread misunderstanding among Gay-Lussac's

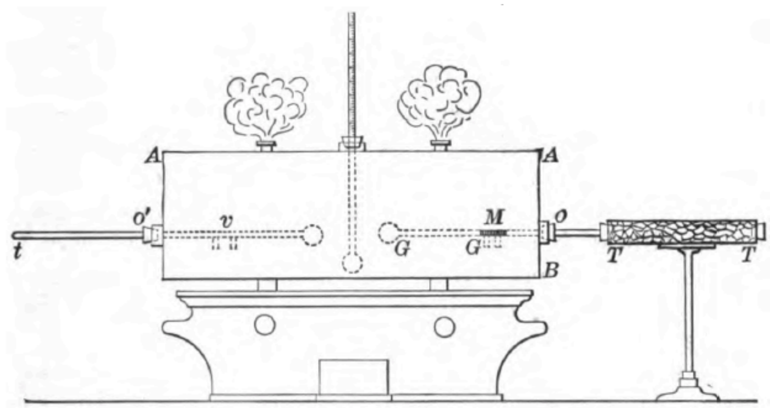

Figure 10 Gay-Lussac's apparatus. By Mariotte's (1679) law, at constant temperature, volume and pressure of a gas are inversely proportional to each other. In this experiment pressure is kept constant by keeping GG at atmospheric pressure (air is introduced and dried through TT, a tube filled with calcium chloride to absorb moisture). M is a small column of mercury, acting as a piston that slides along the tube to indicate the volume expansion of gas contained in GG. AB is a vessel placed on a furnace, filled with water and can be heated to different temperatures, measured by a mercury thermometer t. Source: Biot, J.B. "Extract from Biot's Treaties on Physics, Chapter IX of Volume I," 54.

contemporaries, who interpreted his and Dalton's experiments as showing that the all gases expand uniformly with the "real" degrees of heat under constant pressure, while they in fact only showed that gases largely expanded proportionally to each other, and to the readings of the mercury thermometer at best, all within the range between the freezing and boiling points of water.[11] As Mach pointed out, the misunderstanding was shared by even Rudolf Clausius in his

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[10] L. J. Gay-Lussac, "Researches upon the Rate of Expansion of Gases and Vapors.," in *The Expansion of Gases by Heat: Memoirs by Dalton, Gay-Lussac, Regnault and Chappuis*, ed. and trans. Wyatt William Randall (American book Company, 1902), 25.

[11] Ernst Mach, *Principles of the Theory of Heat: Historically and Critically Elucidated* (Springer Science & Business Media, 2012), 54. Images on subsequent pages are from the German edition, Ernst Mach, *Die Principien der Wärmlehre: historisch-kritisch Entwickelt* (Barth, 1896).

1863 *Mechanical Theory of Heat,* where Clausius stated the equivalence of heat and work and proposed the first law of thermodynamics. In a passage quoted by Mach himself, Clausius wrote that due to the weak attraction among gas molecules, the pressure of gas under constant volume

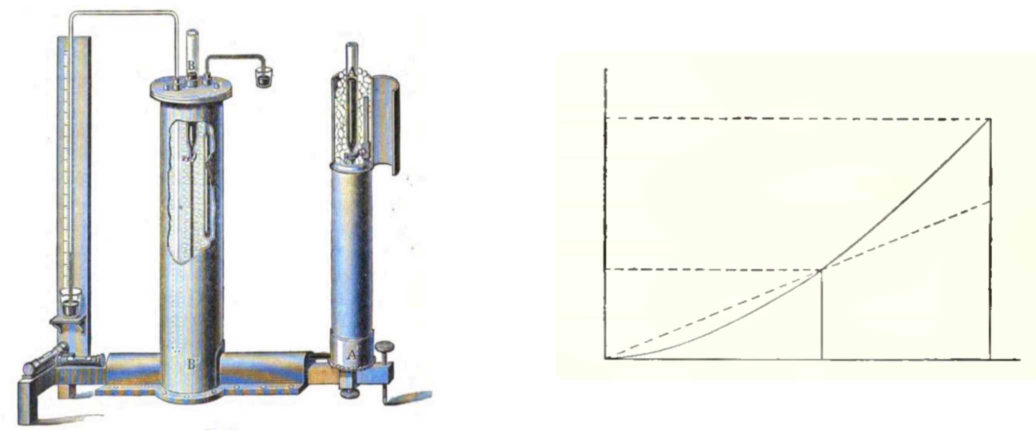

Figure 11 (Left) Illustration of Dulong and Petit's apparatus; (Right) Mach's illustration of Dulong and Petit's finding: the coefficients of expansion of different thermometric substances all differ from each other.

Dulong and Petit's experiment compared the coefficients of expansion of mercury, glass, air and several metals. From the height differences between the mercury in A and B, Dulong and Petit calculated mercury's coefficient of expansion between 0 degree and the temperature recorded by the air thermometer. The mercury weight thermometer's reading was determined both by the expansion of mercury and that of glass vessel, so knowing mercury's coefficient of expansion gave glass's coefficient of expansion. Knowing both the expansion of glass and mercury, they inserted a small rod of iron in a glass thermometer, filled the rest of the vessel with mercury, and calculated the expansion of iron, and repeated the same with copper and platinum. Setting up two tubes filled with mercury, one plunged into a bath of melting ice and the other into a hot oil bath, whose temperature could be continuously raised to 100°, 200°, or 300° (determined by the air thermometer) by heating, Dulong and Petit measured the temperature of the hot oil bath using both an air thermometer and a mercury weight thermometer (which expels mercury from expansion) to compare their scales.

As Mach's illustration shows, if we regard the expansion of air as the standard, the coefficients of expansion (the volume expansion responding to 1° increase on the thermometer) of all other bodies increase with temperature; if we regard the expansion of iron as the standard, the coefficients of expansion of all other bodies decrease with temperature; if we regard the expansion of mercury as the standard, the coefficients of expansion of iron increase while that of air decrease.

Source: Thomas Preston, *The Theory of Heat* (London: Macmillan, 1894), 166; Mach, Ernst. *Die Principien der Wärmlehre*, 45.

"must be approximately proportional to the absolute temperature. The correctness of this inference has, indeed, so much intrinsic probability that many physicists since Gay-Lussac and

Dalton have assumed it outright, and based upon it their calculations (!) of the absolute temperature."[12] Like many of his contemporaries, Clausius had interpreted Gay-Lussac and Dalton's experiments as indicating that the pressure of gas under constant volume indicated *the* temperature, independent of the particular matter measuring it.

Except for the gases, it was discovered that all promising candidates for thermometric substances exhibited different patterns of expansion. Pierre Dulong and Alexis-Thérèse Petit's classic experiment in 1817, comparing mercury, glass, iron and various metals, was cited by Mach to illustrate this point (Figure 11). This confirmed the central dilemma in 19th century thermometric research: there was no external standard for accessing the "real" temperature other than by the variety of individual bodies, gases, fluids that had hitherto been used as thermometric substance. While by the mid-19th century, gas thermometers appeared to be the most promising due to their largely uniform increase in pressure or volume with temperature among themselves within a range, this still could not lead to the conclusion that gas expanded in proportion to temperature, since temperature values were indicated by none other than gas expansion.[13]

[12] Cited in ibid, 55.

[13] This is best phrased by Chang's article: we cannot claim that Y = f(X) is a linear function of X if we don't already know the values of Y independent of X. (See Chang, Hasok, *Inventing Temperature*, 59.)

Furthermore, divergences among gases were also revealed: when the increase in pressure and reduction in volume reaches beyond certain point, vapors begin to liquefy, and for different vapors, the temperature upper limits below which this occurs are different. This means that the Mariotte's law, by which temperature was measured (by either volume or pressure when keeping the other variable fixed), could only be applicable within a limited range, and for each gas this limited range would appear to be different. (Figure 12) If all bodies exhibit different behaviors in response to the same changes, then none can claim an exclusive right to define temperature. The 10º increment on one thermometer (defined by the behavior of its thermometric substance) would not correspond to the 10º on another, or at a different location on the same thermometer. Using any particular scale would only result in a definitional scale. In William Thomson's words, in designating temperature values, "it appears then that the standard of practical thermometry

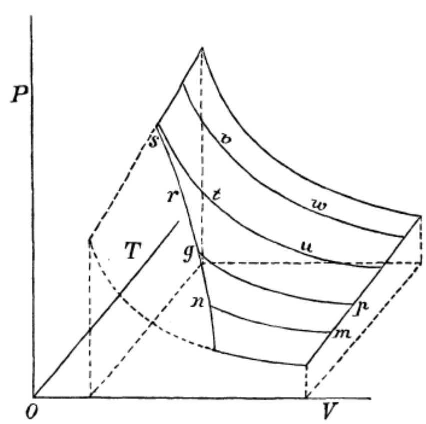

Figure 12 Mach's illustration of isothermal curves for gases: the pressure of a vapor at a given temperature ascends in the curve mn, but at n liquefaction begins. At a higher temperature, when pressure ascends along pg, the liquefaction starts at a higher pressure g. At the critical temperature, no liquefaction every occurs (wv). Source: Mach, Ernst. *Die Principien der Wärmlehre,* 30.

consists essentially in the reference to a certain numerically expressible *quality* of a particular substance," that is, its volume, pressure, or other properties.[14]

The problem is analogous to the one in spatial measurement discussed in Helmholtz's papers on the foundation of geometry (chapter 2): if spatial magnitudes were only measurable by congruence in superposition, there would be no way to confirm the invariance of the measuring rod in motion; if the measuring rod altered through motion, then the result of measurement, and the very concept of spatial distance, would be meaningless. In both cases, the quantities of space and temperature would be contingent upon certain properties of physical bodies (e.g., the rigidity of the measuring rod in motion, or the proportionality of gas pressure or volume with temperature) defining them. But there was no way of validating that these properties independent of the very concepts that they define. The dilemma in both cases stems from the fact that the concepts in question were constituted by the method of measurement, but "abstracted" and bestowed an entirely independent existence outside these methods, like the Platonic ideas, as Mach would say.

The acceptance of thermodynamics in the mid-19th century brought forth a new framework in which temperature was defined and measured. Heat was no longer viewed as a conserved quantity or an imponderable substance but a form of energy. The definition of temperature was accordingly based on the interconversion between mechanical work and heat, rather than the behavior of specific substances. Still, in connecting these theoretical concepts to

[14] L. J. Gay-Lussac, "Researches upon the Rate of Expansion of Gases and Vapors.," in *The Expansion of Gases by Heat: Memoirs by Dalton, Gay-Lussac, Regnault and Chappuis*, ed. and trans. Wyatt William Randall (American book Company, 1902).

concrete measurement operations, the behaviors of specific gases at different temperatures inevitably entered the calculation of work and heat.

The development of thermodynamics in the 19th century was very much indebted to engineering, in particular the study of steam engines and their efficiency. As Mach noted, while it was commonly held that thermodynamics began with the conception of heat as motion, those who entertained the kinetic theory of heat (the view that heat is the result of "unperceived motions of molecules of matter") often also believed that heat was a conserved quantity. The kinetic theory of heat in these earlier accounts therefore remained largely as "wholly contemplative, philosophical, and passive," i.e., they did not play as constructive a role in pushing forward thermodynamics as the investigation of steam engines and the behaviors of various gases and vapors.[15] The connection between mechanical work and heat was investigated in detail by the French engineer Sadi Carnot, whose ideas laid the groundwork for thermodynamics. Carnot was interested in investigating whether the performance of work by heat (given a temperature difference) is independent of the materials, i.e., different gases or vapors, and ways to attain the maximum amount of mechanical work given a quantity of heat.[16] To achieve such maximum, Carnot invoked a thought experiment, a 4-step cyclic process

[15] Mach, *Principles of Theory of Heat*, 199, 200. Mach noted: "the intuitive notions by which we obtain and facilitate our grasp of the facts are of far less importance than the accurate study of the facts themselves. By this study the notions spoken of adapt themselves and develop themselves to such an extent that they then attain a rich constructive power. Even the material theory of heat would not ultimately have hindered the full development of thermodynamics." (Ibid., 200.)

[16] Carnot held that heat was a conserved quantity, and the passage of heat gave rise to an amount of mechanical work without destroying the heat. In Mach's description, he thought that the performance of work by heat was analogous to the performance of work by a waterfall. Carnot's theory was modified by Thomson, Joule, Clausius and others. See Mach, *Principles of Theory of Heat.*

involving a piston performing work or having work performed on it with gas expansion or compression. The "Carnot cycle" has now become a standard introduction to thermodynamics:

1. A cylinder containing gas below a piston stands on an object A serving as the "heat reservoir" (being able to confer heat indefinitely) at constant temperature t_1. The piston is loaded with weight equal to the pressure of the gas. Gas expands and is kept at the same temperature t_1 by continuing to take heat from A.[17]

2. The cylinder is removed from A and prevented from heat transfer. Gas continues to expand, pushing the piston, while the temperature of the gas sinks to t_2.

3. The cylinder stands on an object B serving as the "cold reservoir" (being able to absorb heat indefinitely) at constant temperature t_2 ($t_2 < t_1$). Gas is compressed (having external work performed on it) and kept at the same temperature t_2 as heat continues to be transferred to B. The piston is pushed down to such an extent that

4. when the cylinder is removed from B, further compression to the original volume before step 1 would also restore the original temperature t_1.

Carnot concluded that the mechanical work in this process was entirely independent from the choice of the "working material"—air, steam, or alcohol vapor.[18] Instead, the maximum

[17] The process is an ideal scenario and cannot be actually carried out, because if the temperature of the gas is the same as the temperature of A, no heat transfer would occur. The temperature of the gas is therefore imagined to be infinitesimally smaller than the temperature of A. Similarly, if the pressure of the gas is equal to the weight loaded on the piston, no motion would occur. Again, the weight is imagined to be infinitesimally smaller than the pressure. (Mach, *Principles of Theory of Heat*, 203)

[18] The argument is by reductio ad absurdum: given two such cycles with two different working substance, with the same quantity of heat input, the same temperatures of cold and heat reservoirs, if one process produces a higher net work W' than the other, then this latter process could be inverted to expend W, which is smaller than W'. Then W' - W will be the excess work that supplies a perpetuum mobile. (Ibid, 204)

work performed by this process was only dependent on the temperatures of the two reservoirs, and the transferred heat (the difference between heat taken in from A and heat taken out by B).

The precise quantitative relations in the Carnot cycle were subsequently studied by B. P. E. Clapeyron, James Joule, William Thomson, Rudolf Clausius and so on, and modified based on the principle of energy conservation. While Carnot himself assumed that quantities of heat was conserved while providing mechanical work as it sank to a lower temperature, the works of Julius Mayer (1842), James Joule (1843) and Helmholtz (1847) showed that heat vanished when performing work. In a sequence of experiments, Joule found that the amount of heat produced by mechanical work was a constant value. He believed there was a constant ratio between mechanical work performed (or expended) and quantity of heat gained (or lost).[19] (Figure 13) This was designated through the "mechanical equivalent of heat," a constant J.

[19] See James Prescott Joule, "XI. New Determination of the Mechanical Equivalent of Heat," *Philosophical Transactions of the Royal Society of London* 169 (1878).

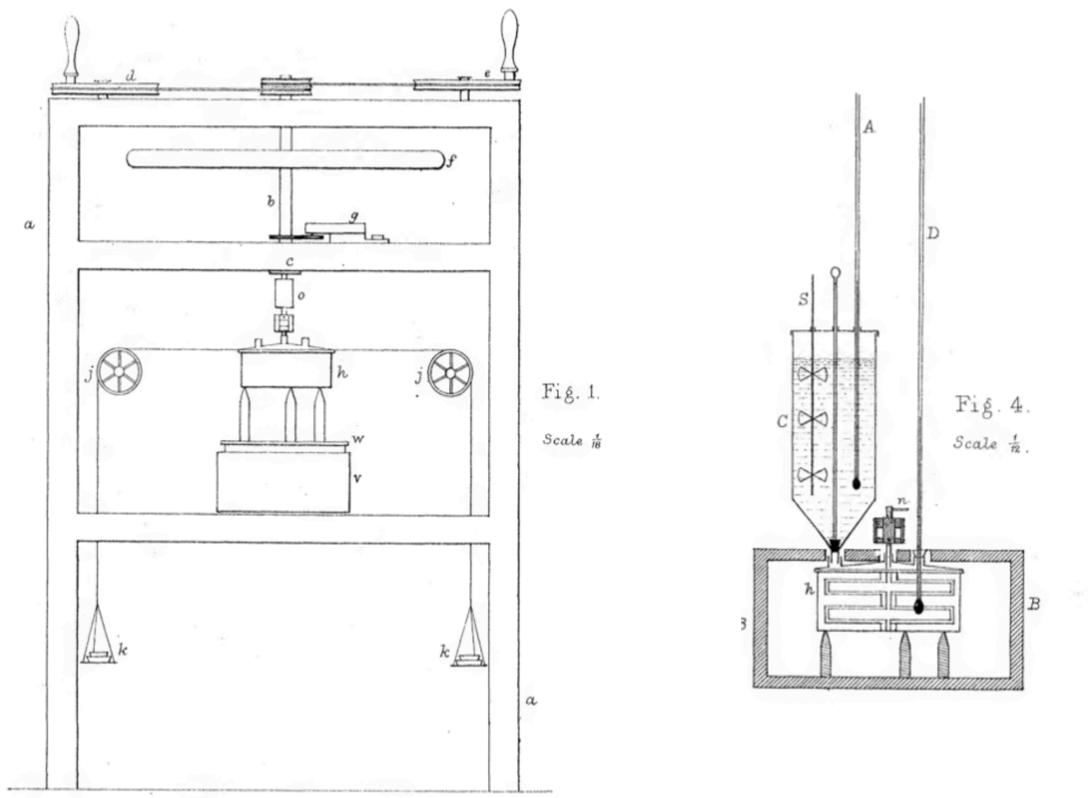

Figure 13 James Joule's 1878 device for determining the mechanical equivalent of heat. The basic idea was to "revolve a paddle in a suspended vessel of water, to find the heat thereby produced, measuring the work by the force required to hold the vessel from turning, and the distance run as referred to the point at which the force was applied." Rotation was produced by weights on the scales connected to two pulleys, and the heat was measured by the small changes of temperature of water in a delicate calorimeter on the right. The loss of mechanical work in the rotation should be equivalent to the increment of heat in the calorimeter.
Source: Joule, James Prescott. "XI. New Determination of the Mechanical Equivalent of Heat," Plate 26.

Thomson's investigation of the efficiency of the Carnot cycle (the engine outlined through step 1-4) took Joule's findings as a given, namely that work and heat were interconvertible. Specifically, if Q is the heat absorbed in step 1 of the Carnot cycle from the hot reservoir kept at temperature t_1, and Q' is the heat conveyed to the cold reservoir kept at t_2 in step 3, then Q-Q' depends on t_1 and t_2. Q - Q', the "vanished heat" in the process, is transformed into mechanical work W, and W = J(Q - Q') according to Joule's theory about the mechanical equivalent of heat. The ratio between W and Q then expresses the efficiency of the cycle—the amount of net work produced in the cycle given initial heat input Q, is itself a function of the temperatures t_1 and t_2. Thomson designated this ratio as Carnot's coefficient. Specifically, he expressed the efficiency μ in terms of $W = Q \cdot \mu \cdot dt$, where dt is the differential in temperature. This efficiency played a crucial role in the definition of the absolute temperature.[20] Earlier, Joule had discovered that the efficiency factor μ was inversely proportional to temperature measured by conventional thermometers. That is, $\mu = J \cdot \alpha \cdot (1 + \alpha t)$, where α is a constant and J is Joule's mechanical equivalent of heat. Instead of taking the temperature measured by a thermometer—the t in the equation—as the indefinable and measuring μ by it, Thomson defined a new scale of temperature, T's, whose values were determined by μ. μ was supposed to be empirically measurable in terms of the work and heat input, both universal quantities independent of temperature, in principle. That is, he defined absolute temperature "arbitrarily," in Mach's words,[21] in terms of $T = J/\mu$. This was indeed an arbitrary but a convenient definition, in the sense that it was suggested by Joule's formulation of μ, which at the time was not confirmed. It was convenient because the new T's would not deviate too far from existing air thermometer scales. This definition is also equivalent to defining the temperatures of the cold and heat reservoir in terms of $T_1/T_2 = Q/Q'$.[22]

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[20] Mach, *Principles of the Theory of Heat*. Hasok Chang and Sang Wook Yi, "The Absolute and Its Measurement: William Thomson on Temperature," *Annals of Science* 62, no. 3 (July 1, 2005): 281–308, https://doi.org/10.1080/00033790410001712246.

[21] Mach, *Principles of the Theory of Heat,* 288.

[22] Alternatively, T_1/T_2 can also be expressed in terms of $W/Q = 1 - T_2/T_1$, or in terms of the efficiencies $T_1/T_2 = \mu_2/\mu_1$, if a Carnot cycle (or any reversible process) is established for each of them with a third fixed temperature.

For any two temperatures, in this definition, their ratios have a fixed reference independent of any particular substance, namely the ratio between quantities of heat absorbed and emitted if a Carnot cycle is established between these two temperatures.[23]

3.3. Mach's Analysis of the Temperature Concept

The 1896 *Principles of Theory of Heat*, born out of the lectures series of Ernst Mach as the professor of the "History and Theory of Inductive Science" at the University of Vienna, was intended to review the development in theories of heat up to the end of the 19th century and expel "superfluous ideas and unwarranted metaphysical views" from a narrative of what had actually been achieved in the field.[24] During this period, his works gained international renown and the *Principles of Theory of Heat* was published in two German editions and translated into English in four years.[25] Like Helmholtz and others philosophically inclined scientists of his time, Mach was concerned with the relationship between mathematical conceptions and how they came to represent concepts and phenomena in the empirical sciences. Mach agreed with Helmholtz that numbers originated from ordinals—a collection of symbols with a fixed order to designate and distinguish counted objects. More complex entities and theorems in mathematics were created by the unlimited application of arithmetical operations to the infinitely extended ordinal sequence. By conventional wisdom, numbers express the differences between

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[23] Mach, *Principles of Theory of Heat*, 284; Chang and Yi, "The Absolute and Its Measurement."

[24] Mach, "Author's Preface to the First Edition," *Principles of the Theory of Heat.*

[25] Ibid, "Editor's Note to the English Edition."

"continuums" (continuous quantities) in equal units by means of measurement.[26] But this was not true with temperature:

> "…temperature is nothing but the characterization or designation of a thermal state by a number. This temperature number has exclusively the properties of an inventory number, by means of which the same thermal state can again be recognized, and if necessary sought for and reproduced. This number likewise informs us in what order the designated thermal states succeed one another and between what other states a given state is situated…the conception of temperature is a conception of *level*, like the height of a heavy body, the velocity of a moving mass, electric and magnetic potential, and chemical differences."[27]

Mach started his analysis of the temperature concept by discussing its forgotten origins in perceptual experiences. Before the concept of temperature, there are initially sensations of warm and cold. As we observe changes in physical objects concomitant to the changes in sensation, such as the "glowing, the melting, or the evaporation of a certain body, the hissing noise made by a drop of water on a hot plate, its freezing on a cold plate," and so on, we use these phenomena instead of direct sensations as a guide to the thermal states of other objects. Volume expansion is a conspicuous and convenient indicator concomitant with our ordered thermal sensations, hence we assume that the more intense a thermal state is, the greater the volume expansion (1). Because thermal sensations tend to equalize after prolonged contact with objects, we assume that thermal states themselves (of objects) also equalize after prolonged contact if objects do not cause volume alterations in each other (2). This is interpreted as the equality of thermal states, allowing us to use the volume of one body after reaching equilibrium to designate not just its own thermal state, but also that of another object. For the equality of thermal states to have general meaning, we assume that physical equality is transitive, that is, if (2) occurs between A and B, and between B

[26] By "continuums" he meant any "system of terms," where each term indicated the degree of some kind of property, and between any two terms infinitely many other terms could be interpolated that still preserve the order.

[27] Ibid, 60-61.

and C, then it also occurs between A and C (3). Finally, due to the continuity of thermal sensations, we assume that thermal states are continuous (4).

It is these sedimented inferences and assumptions originating from unscientific daily experiences that allow the concept of temperature to become an abstract scientific concept in the first place. From Mach's point of view, there are arbitrary definitions or conventions in each of the steps from (1) to (4). (1) is contradicted by the case of water between +3º and +5ºC, which diminishes in volume when combined, (2) by the fact that heat movement occurs when the thermometer does not indicate, and (3) and (4) are only good insofar as experience has not proven otherwise.[28] Once we have an instrument for marking and fixing various thermal states, we use numbers—"an orderly system of *names*" capable of indefinite extension—instead of lengthy physical descriptions such as "the melting point of ice" to mark and order thermal states.[29] The number that is uniquely coordinated with a thermal state is called the temperature. For Mach, this relationship between number and thermal states in practical thermometry is just that—all that has been accomplished is essentially designation that preserves the order.

In reference to temperature prior to Thomson's absolute definitions, the claim that temperatures are merely ordinal is validated by several reasons. Temperature in conventional thermometry is represented by the volume or pressure of a particular thermometric substance. Had all substances exhibited the same pattern of change in volume or pressure under thermal changes, it might have been possible to simply use either parameter to stand for temperature. But this is not the case—all candidates of thermometric substances exhibit different patterns of

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[28] Ibid, 45-48.

[29] Ibid, 69.

expansion—and the choice of any particular substance would be largely arbitrary.[30] Whether one chooses the volume of air, iron or mercury to designate temperature, there is no other substance that expands in a linear relationship to it. No substance can therefore claim to represent the "true" temperature. The discovery that all gases exhibits similar patterns of expansion led many to believe that gas expand "uniformly with temperature." But the observation that gases expand largely in proportion to *each other* does not lead to the conclusion that they expand in proportion to "true" temperature, because "true" temperature in conventional thermometry is defined precisely by various individual expanding objects. Quoting Dulong and Petit, who noted that their own experiments showed "how greatly the expansion of glass departs from uniformity," Mach responded: "We ask in astonishment: 'by what criterion is the 'uniformity' or 'lack of uniformity' of the expansion of glass to be estimated and measured?"[31]

The ratio between any two temperatures on a thermometric scale calibrated by the volume of a substance, therefore, is designated by numbers by an arbitrary convention. It is certainly possible to divide the volume between, say, 0º and 100º by 100 parts equally, but the same degree fails to refer to the same quantity—if it refers to an external quantity, like increment of heat or difference between two thermal states, at all. In a similar vein, the ratios between degrees are also not coordinated with any external reference. The difference between 1° and 2° is not the same as the difference between 2° and 4°, because it's always possible to argue that the

[30] Ibid., 40-41.

[31] Ibid, 55. He further cited Clausius who noted that the observable pressure of gas was "an approximate measure of the dispersive of the heat contained in the gas; and therefore, …this pressure must be approximately proportional to the absolute temperature." Clausius also noted: "In view of Gay-Lussac's discovery…that all gases suffer, under the action of heat, like expansions for like increases of temperature, the hypothesis is well justified that the expansion in question is uniform for all degrees of temperature, inasmuch as it is more probable that the expansion should be uniform than that all gases should exhibit the same variability." (Ibid.)

thermal difference corresponding to the "doubling" on the air thermometer does not correspond to the "doubling" on the mercury thermometer. Mach summarized this dilemma as follows:

> "objects counted, which are alike in some particular respect, and which may replace one another in this respect, are called units. What is it that is counted, for example, by the number representing a temperature? In the first place it is the division of the scale, the real or apparent increments of volume or of tension of the thermometric substance. Geometrically or dynamically regarded, the objects here counted may be substituted for one another indifferently; but with reference to the thermal state these objects are signs or indices merely of that state, and not equivalent, enumerable parts of a universal property of the thermal state itself."[32]

Choosing pressure of a particular gas or vapor instead of volume to designate temperature suffers from the same problem. While thermometric scales based on air pressure had in fact been extensively made and put in use, the coefficient of pressure for gases is approximately the same with the coefficient of volume.

Choosing any particular thermometric substance and choosing either volume or pressure to represent thermal states, both involve arbitrary conventions. Therefore, the relation between number and thermal states on the thermometric scale is also conventional. That is, temperature numbers can be by different functions of a chosen physical property of a chosen thermometric substance. The resulting scales have different features, and none is superior to any other in principle. For instance, there is no upper limit to the temperature scale determined by gas expansion, but gas pressure or volume cannot fall below zero. Defining temperature as proportional to pressure arbitrarily through the equation $p = p_0(1 + \alpha t)$, where p_0 is the pressure of unit gas under constant volume, then temperature has a lower limit at -273° C, since p cannot fall below zero. But replace air by mercury and employ the same reasoning, then -5000°C is the absolute zero. On the other hand, if one defines temperature through a logarithmic function of volume, as John Dalton had in fact done, then there is no lower limit or

[32] Ibid, 71.

"absolute zero" in principle. Dalton marked the indices of -20, -10, 0, 10, 20… on the thermometer corresponding to $v_0/1.0179^2, v_0/1.0179, v_0, 1.0179v_0, 1.0179^2 v_0$ (v_0 is the initial volume of a gas). These values can never reach zero.[33] Defining temperature as a linear function or a non-linear function of volume or pressure is entirely a matter of convention.

The pressure or volume here are merely symbols that stand for a particular temperature level, and temperature numbers "the symbols of the symbols." When the symbols disappear, the thing symbolized need not disappear along with them; the properties of the symbols cannot be outright identified with the properties of the symbolized.[34] If one further asks what the symbols stand for, the answer would have to be that temperature markings in conventional thermometry allow us to recognize if the same thermal states have been reproduced in different scenarios and in different objects, the "same" states being the collection of the mutually coordinated behavior of objects that occur concomitantly—initially concomitantly with our thermal sensations, later with phenomena that designate in place of sensations.

Mach's main argument with regard to temperature is captured in the following quote: "thermal states exist in nature, but the conception of temperature exists only by virtue of our arbitrary definitions, which might very well have taken another form." Temperature is merely the collection of behaviors of objects ordered in a way that roughly mirrors our thermal sensations. Volume or pressure changes of objects are merely one (and the most easily noticed) phenomena that occur under thermal changes that mirror the order of our sensations. But as volume or pressure indications replace thermal sensations to stand for thermal changes, we project a uniform relationship between volume or pressure changes and the "real" temperature.

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[33] Ibid, 58.

[34] Mach, *Principles of the Theory of Heat*, 58.

Although different thermometric substances' behaviors deviate from each other, we still seek a substance whose volume or pressure most approximates a proportional relationship with the "real" temperature. But independent of the behavior of individual bodies under thermal changes, there is no such "Platonic ideal of temperature, of which the temperatures read from the thermometric scales were only the imperfect and inexact expression."[35]

The exact same logic infects the measurement of time. Mach argued that Newton's absolute space and time "originated in a similar manner," as "the sensation of duration plays the same part with regard to the various measures of time as the sensation of heat" in the case of temperature. In the 1883 edition of *The Science of Mechanics*, Mach noted:

> "when we say a thing A changes with the time, we mean simply that the conditions that determine a thing A depend on the conditions that determine another thing B. The vibrations of a pendulum take place *in time* when its excursion depends on the position of the earth. Since, however, in the observation of the pendulum, we are not under the necessity of taking into account its dependence on the position of the earth, but may compare it with any other thing (the conditions of which of course also depend on the position of the earth), the illusory notion easily arises that all the things with which we compare it are unessential. Nay, we may, in attending to the motion of a pendulum, neglect entirely other external things, and find that for every position of it our thoughts and sensations are different…time is an abstraction, at which we arrive by means of the changes of things; made possible because we are not restricted to any one *definite* measure, all being interconnected."[36]

As temperature is born out of the need for ordering behaviors of bodies under thermal changes largely in accordance with thermal sensations, time is similarly born out of the need for grouping and distinguishing changes in external bodies based on the sensation of duration. Volume or pressure of individua substances replaces thermal sensations in temperature measurement, i.e., the thermal states of other objects are compared with the thermal states of the object serving as thermometric substance and quantified by the changes in the latter. Likewise,

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[35] Ibid, 54.

[36] Ernst Mach, *The Science of Mechanics* (Open Court, 1893), 233.

the durations of one (recurring) motion replaces the sensation of duration in temporal measurement, i.e., the durations of other motions are compared with the durations of a fixed motion and quantified by the latter. In both cases, the knowledge actually obtained is the interdependence of things, i.e., how changes in A can be described by changes in B, but a uniform scale is projected onto both temperature and time, postulated as the real standard against which all observed behaviors of individual bodies or motions need to be compared, although such standard has never been discovered.

Many of Mach's contemporaries were already suspicious of Newton's absolute time, especially as they were reminded by astronomers' discovery that the earth's rotation is not uniform in the mid-19th century that no perfectly uniform motion had yet been validated. Some, such as Carl Neumann and Ludwig Lange, attempted to redefine time through inertial motion, that is, durations could only be traced out by the displacement of a body undergoing inertial motion.[37] As Mach rephrased: motions could only be uniform with regard to another motion, but not uniform on their own.[38]

3.4. Absolute Temperature as a "Concept of Level"

How does Mach's arguments hold against Thomson's absolute temperature based on the universal relationships between work and heat, and the conservation of energy? The question concerns how Mach conceptualized indirectly measured quantities like the absolute temperature, which is uniform but not additive. In the *Principles of Theories of Heat*, Mach thoroughly examined the work of Thomson, Joule, Clausius and others, including the ideas and experiments leading to a thermodynamic scale. But he did not extensively discuss whether these developments would

[37] See Barbour, *The Discovery of Dynamics*.DiSalle, *Understanding Space-Time*., Olivier Darrigol, *Relativity Principles and Theories from Galileo to Einstein* (Oxford University Press, 2021).

[38] Mach, *The Science of Mechanics*, 223.

have challenged his arguments on temperature in the opening chapters of the book. The absolute temperature scale is based on the fact that given a temperature difference, heat transfer gives rise to work. Establish a Carnot cycle on two objects with a temperature difference and designate their temperatures to be T_1 and T_2, then the ratio between these absolute temperature values has a *fixed reference*: one can now speak of the ratio between any two temperatures on the absolute scale meaningfully because such ratio had a fixed reference—$T_1/T_2 = Q/Q'$, where Q is the heat input (provided by the object serving as heat reservoir) and Q' the heat dissipated (emitted to the object serving as cold reservoir and was not transformed into work), or $T_1/T_2 = \mu_2/\mu_1$, where μ's are efficiencies of Carnot cycles if established between each of these temperatures and a third one. Previously the efficiencies were measured by temperature readings of thermometers, now the relation is inverted: temperatures are measured by the efficiencies.

Mach briefly noted that in constructing the absolute scale for temperature, the goal is to achieve "universal validity analogous to the potential scale."[39] The change in the electrical potential of a charged body, as Mach illustrated, when it sinks from 51 to 50 or from 31 to 30, for instance, is equivalent to the rise in potential of another body having the same capacity from 10 to 11 or from 24 to 25. In other words, differences between potential can be universally compared and substituted for one another. The absolute temperature, according to Mach, achieves something analogous. The ratios between any two temperatures were fixed to the ratios between universally comparable quantities of heat (or the ratios between heat and work), and one can speak of "doubling" "tripling" this ratio meaningfully because it is tied with a set of phenomenological relations in experiment. The ratios between temperatures designate a kind of qualitative relation, which refers to specific measurements in an experiment. Still, temperatures

[39] Mach, Ernst. *Principles of the Theory of Heat*, 72.

themselves are symbols of thermal states of bodies, the banners under which phenomena resulting from differences between thermal states are grouped, coordinated with a universally comparable scale. In this sense, Mach's claim still holds true—"the question is always one of a scale of temperature that shall be universally comparable and that can be constructed with accuracy and certainty, and never one of 'real' or 'natural' scale."[40]

Furthermore, measuring temperature by the absolute scale does not amount to counting units and the difference between two temperatures is not a temperature. This is different from how the difference between two lengths can be expressed as a magnitude of the same kind. The measurement of temperature is not obtained by the addition of units and there is not a universally valid operation to "add" two temperatures. It is in this sense that "the conception of temperature is a conception of *level* (Niveau), like the height of a heavy body, the velocity of a moving mass, electric and magnetic potential, and chemical difference."[41] Elsewhere in the book, Mach consistently spoke of "potential levels," "velocity levels," and so on, harkening back to the argument that speeds are not composed of smaller speeds, which dates back to at least the 14th century.

It is noteworthy that Thomson's definition of the absolute scale is based on the proportion of mechanical work done by an ideal gas (absorbed heat is converted into mechanical work without contributing to the internal energy of the gas) given a definite amount of heat in a Carnot cycle, and such work and heat were still calculated in Thomson's time by pressure and volume of a(n ideal) gas and the heat capacities of particular substances used to measure heat. In

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[40] Ibid, 56.

[41] Ibid, 61.

other words, although in theory, absolute temperatures are defined through universal quantities independent of any particular substance, in practice, the calculation of both work and heat still depends on the properties of specific substances.[42] Since hydrogen's behavior most approximated an ideal gas, its pressure under constant volume form the international standard for temperature measurement from 1887 onward. The primary task of practical thermometry, as seen from the publication concerning the standardization of thermometers at the Physikalisch-Technische Reichanstalt in 1894, was to calibrate various existing practical scales with each other and with the scale provided by Thomson.[43] For higher temperatures, gas thermometers were useless since glass vessels soften, hence they had to be calibrated against pyrometers that used metals like platinum in porcelain vessels. For other purposes, such as for measuring the maximum and minimum temperature over time, alcohol thermometers were superior.[44] Temperature represented as the "level" indicated by the idiosyncratic state of a particular thermometric substance remained palpable in late 19th century practical thermometry.

3.5. Stellar Magnitudes and Brightness: Steinheil and Pogson

We have seen that the primary difficulty in conventional thermometry is that there is no fixed unit or ratio between different temperatures. The equal increments in volume or pressure of two expanding thermometric substances do not correspond to the same quantity; neither do equal increments in volume or pressure of the same substance on different parts of the scale. There is no fixed, external reference independent of all bodies expanding in different proportions to each other. There is a surprising parallel between the difficulties encountered in conventional

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[42] This circularity is noted in Chang, *Inventing Temperature*, 192.

[43] J Pernet, W Jaeger, and E Gumlieh, "Herstellung und Untersuchung der Quecksilber-Normalthermometer," *Zeitschrift für Instrumentenkunde* fünfzehnter Jahrgang, no. 1 (January 1895): 2–13.

[44] Neues Handwörterbuch and Hermann Christian von Fehling, *Neues Handwörterbuch der Chemie: Vol. I-*. (F. Vieweg & Sohn, 1905), 626-7.

thermometry and in psychophysics, both stemming from the fact that both quantities must be stood for by something else—incapable of direct comparison, they must be determined indirectly through functional dependency on something else. Though he had been an early advocate of Fechner, Mach agreed later in his career with many critics of Gustav Fechcher, noting the similarities between the problems encountered by psychophysics and conventional thermometry:

> "to speak of an actual measurement of the sensations makes no sense; instead, the most that can be achieved is to exactly characterize or make an inventory of [sensations] by numerical means. Compare what I have said about thermal states in Principien der Wärmelehre."[45]

In other words, Mach implied that Fechner's scale for sensation was an "inventorial" or "definitional" scale just like temperature scales based on individual thermometric substances, for the reasons discussed in the previous section.

Despite the amount of criticism Fechner's work received, it was not entirely ungrounded in facts. He saw photometric measurements in 19th century astronomy as a precursor to his psychophysical research, claiming that the fundamental psychophysical law was "already contained" in the magnitude-intensity scales proposed by his contemporary astronomers Carl August von Steinheil and Norman Pogson.[46] He was not alone in drawing a close connection between the two areas of research. Charles Sanders Peirce's 1878 report on his comparison of various stellar magnitude scales at the Harvard Observatory also contained a lengthy discussion of quantities of sensation according to the "approximate truth" of Fechner's law.[47] Before the use of photoelectric cells and the analysis of light composition through photographic spectroscopy,

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[45] Ernst Mach, *Die Analyse der Empfindungen und das Verhältniss des Physischen zum Psychischen* (Jena: G. Fischer, 1922), 67, n.3,

[46] Fechner, Gustav Theodor. *Elements of Psychophysics*. Edited by Davis H. Howes and Edwin G. Boring. Translated by Helmut E. Adler. Vol. 1. Holt, Rinehart and Winston, 1966, xxviii.

[47] Peirce, C.S. "Photometric Research." Annals of the Astronomical Observatory of Harvard College. 9 (1856), 5.

19th century astronomers measured the apparent brightness of stars using a combination of visual judgment and instrumental techniques. The apparent brightness of stars is influenced by a variety of factors: actual quantity of light, distances of the stars, atmospheric conditions, individual discrepancies of astronomers, and the eye's sensitivity to brightness and color differences, among others. Hence human perception inevitably entered the equation in the determination of stellar magnitudes during this time, whether through pure visual estimates or photometric methods. Astronomers showed that there was an approximately constant relationship between the ability to perceive a difference in brightness by the human eye and the ratios between quantities of light measured by physical parameters. These results inspired Fechner to pursue a functional dependency between perceived and physical intensity of stimuli, which essentially generalized this relationship to all sensation.

How were stars assigned magnitudes? In the time of Hipparchus and Ptolemy, thousands of stars were observed by naked eye and classified into six different magnitudes and ranked from the brightness to the faintest. John Herschel grouped stars in descending order based on the equality and differences between brightness. Stars were assigned different magnitudes based on, for instance, a small but readily discernible difference, or a difference that "upon longer inspection of them we always return to decide it in favor of the same [star]." A more programmatic protocol to rank stars by brightness and assign them magnitudes was introduced by F. W. Argelander, later the director of Bonn Observatory, in an 1844 publication. Using this method, Argelander then compiled massive, multi-volume catalogues containing hundreds of thousands of stars (known as the Bonn Durchmusterung) down to the ninth magnitude in the following decade, which was later widely referenced in Germany and North America and served as the foundation of most stellar magnitude classification systems until the 1890s.[48] The method was described as follows in a 19th century handbook on astronomy:

> "…one beholds the two stars to be compared, a and b, alternatively: if they are constantly equal, then one notes a · b; in contrast, one notes b · 1 · a, if b now and again appears brighter than a (first step); —b · 2 · a if one finds b always brighter than a (second step), —and b · 3 · a if one finds b somewhat brighter than a at first glance (third step), — and b · 4 · a if one finds b always noticeably brighter than a [presumably at first glance]…"[49]

As can be seen, stars were ranked and distinguished from one another by minute perceptual differences. Number 60 was assigned to Arcturus, the normal star for the first magnitude, and 0 was assigned to the weakest star of the sixth magnitude. Other stars fell in between these numbers after being compared with each other using the step-method. Estimates were no longer reliable for more than 4 steps or when the stars were widely separated, hence an intermediate star would be chosen. 10 steps amounted to one magnitude-class.[50] Roughly speaking, 1 step corresponded to the smallest discernible difference which could only be recognized after some reflection, 2 step with a more noticeable difference, 3 step, with an unquestionable difference at first glance, and difference beyond half-magnitude or 5 steps are not reliable.[51] Despite some noticeable fluctuations, Argelander's scale exhibited approximately uniform gradation along the brightness intensity scale measured by photometric methods (see below) and therefore was not subjected to significant changes when photometric scales were adopted.[52]

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[48] G. Müller, *Die Photometrie der Gestirne* (Leipzig: W. Engelmann, 1897), 455.

[49] Rudolf Wolf, *Handbuch der Astronomie, ihrer Geschichte und Literatur*, vol. 1, 2 vols. (Druck und Verlag von F. Schulthess, 1890), 581.

[50] Ibid.

[51] Simon Newcomb and Rudolph Engelmann, *Newcomb-Engelmanns Populäre Astronomie*, ed. H.C. Vogel, dritte Auflage (W. Engelmann, 1905), 247.

[52] Ibid, 491, 494. But they also noted in a later section that none of the existing brightness scales, including Argelander's, completely conform to the uniform scale achieved in photometry. (Ibid, 491.)

Photometry evolved from 18th century experiments to determine the brightness of artificial light sources in the laboratory setting. Brightness of an artificial light source was calculated by how much the quantity of light need to be diminished until the light source appeared equally bright with another light source or until it appeared completely extinguished. The judgment of equality was made by the eye, while the quantity of brightness was expressed in mechanical terms based on how much light was reduced.[53] Usually the ratio between two brightness values was expressed as a ratio between two values of the same variable related to the measuring device, such as the aperture of telescopic lens, the distance between the eye and the light source, and so on. The method can be illustrated by the first comparison photometer designed by Carl August von Steinheil.

In 1837, Steinheil designed an apparatus capable of comparing the brightness intensities of two stars simultaneously held in the visual field and measuring this intensity relationship with considerable accuracy. The instrument was described in a prize-winning essay submitted to the Royal Bavarian Academy of Science and was later used to classify some two hundred stars in the first photometrically determined catalogue.[54] It was a telescope with an objective lens split into

[53] As Newcomb and Engelman described (my translation):

"The chief principle on which all photometric observations are based is given through the physiological peculiarities of our eyes: comparisons between two different brightness's can only be done with fair accuracy when the difference is small. Every eye sees immediately that the sun is extraordinarily brighter than the moon; but it is not impossible to determine how many times [the former is] brighter [than the latter], be it thousand times or million times. In contrast, an experienced observer can determine with considerable accuracy if two stars are equally bright…the task of a photometer…therefore consists in enabling the eye to make precise determination of greater brightness difference, by diminishing the light from the brighter of the two objects to be compared in a measurable way, to such an extent that it appears equal to the weaker of the two."(Ibid, 247.)

[54] J. B. Hearnshaw, *The Measurement of Starlight: Two Centuries of Astronomical Photometry* (Cambridge University Press, 1996), 59.

The first "completely objection-free index of photometrically determined stars" is attributed to Seidel, who determined 208 brighter, fixed stars with fairly accurate measurements. Seidel's catalogue had served as the basis of other "modern astrophotometry." (Müller, *Die Photometrie der Gestirne,* 444).

two halves, with a reflecting prism installed in one of them, so that the images of two separate stars could be simultaneously observed on each side of the visual field. Instead of comparing the brightness of point-images of the stars, Steinheil observed their out-of-focus images—surface areas illuminated by the starlight—for the reason that judgment of brightness equality was easier and more accurate with surface comparison than with point-comparison.[55] The brightness of the out-of-the-focus images could be adjusted: as the total quantity of light coming from a star remained fixed, its brightness could be varied by projecting this quantity onto a smaller or larger area by shifting the objective lens. Normally, the larger the area on which the same quantity of light is distributed, the less bright it appears. The brightness intensities of two stars would then be inversely proportional to the sizes of illuminated areas, when the objective lens was shifted until the two images looked equally bright.[56] Thus the brightness intensity of two different stars could thus be determined through parameters easily measurable from the device itself. (Figure 14)

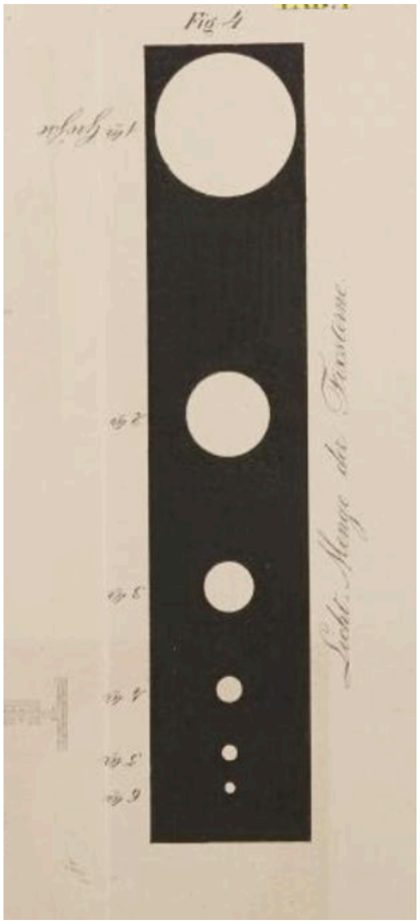
Crucially, Steinheil used his photometric measurement technique to investigate the magnitude class system previously determined by naked eye observation, i.e., to determine the difference between magnitude classes in photometric values. He did so by measuring the brightness difference between some thirty stars of known magnitudes in photometric values. (Figure 14) The result was a logarithmic relation between the difference in stellar magnitude and the difference in intensity. In other words, if d_1, d_2 represent the intensities (proportional to the area of the out-of-focus images) measured by the photometer, while k_1, k_2 represent the

[55] To do so, the objective lens on each side could shifted along its axis, so that the ocular was positioned not exactly at the focal point of the objective lens, but in front of or behind it.

[56] The ratio between the sizes of the illuminated areas, in turn, was proportional to the distance between the ocular from the focal point of the objective. Carl August von Steinheil, *Elemente der Helligkeits-Messungen am Sternenhimmel* (Akademie der Wissenschaften, 1836), 9-10.

magnitude class of the stars, then$log\frac{d_2}{d_1} = m(k_1 - k_2)$, where m is a constant to be calculated from the data. Since stellar magnitudes before the photometric methods were often determined by just discernible differences in the brightness of stars by the naked eye, this logarithmic law was also in a sense a relation between brightness perception and quantity of light.

The logarithmic relation was also confirmed by the British astronomer Norman Pogson in 1856, who used a slightly different method, by varying the aperture of his telescope to control the quantity of light until the stars could no longer be observed. The brightness intensity was thus measured in terms of the physical parameters of the apparatus in a similar way as by Steinheil's method. In the 1856 paper, Pogson tried to determine the relationship between stars separated by different magnitudes and their brightness intensity in photometric terms. The magnitudes of known stars were taken from the observations of Radcliffe Observatory as well as from Argelander's, Bessel's, and a number of other catalogues, and their brightness intensity in photometric terms were subsequently measured. The result was tested under the formula $(logA - loga)/(M - m) = 1/2logR$, where R is a constant (corresponding to the brightness ratio A/a given a M-m magnitude separation), A and a are the photometric intensities of two stars, M and m are the magnitude classes. Pogson obtained an estimate of R as 2.512, which means that a star of any magnitude contains 2.512 approximately times the quantity of light of that of the next smaller magnitude. This result was "remarkably accordant" with the photometric measurements of other astronomers, such as the 2.83 obtained by Steinheil, and 2.519 by



Name und Bezeichnung des Gestirns.	Grösse. Licht-flächen-Durchmesser.	Grössen-Schätzung.	Mittelwerthe; Normalbestimmungen. Messung.	Schätzung.
α Canis majoris (Sirius)	3. 15	1		
α Lyrae (Wega)	2. 82	1		
α Bootis (Arcturus)	2. 52	1		
α Can. min. (Procyon)	2. 11	1		
α Aurigae (Capella)	1. 91	1		
α Virginis (Spica)	1. 88	1		
β Orionis (Rigel)	1. 70	1		
α Orionis (Beteugeuze)	1. 60	1		
α Leonis m. (Regulus)	1. 58	1		
α Cygni (Deneb)	1. 58	1	2. 085	1. 000
α Tauri (Aldebaran)	1. 46	1		
α Scorpii (Antares)	1. 22	1		
γ Orionis (Bellatrix)	1. 15	2		
γ Cassiop.	1. 04	3	1. 2175	1. 750
η Ursae maj.	1. 01	2. 3		
α Polaris (Cynosurus)	1. 00	2. 3		
β Polaris (Cocab)	0. 90	3		
χ Orionis (Saiph)	0. 90	3		
β Cassiop.	0. 86	2. 3		
α Cephei (Alderamin)	0. 85	3		
α Hydrae (Alphard)	0. 81	2		
α Cassiop. (Schedir)	0. 75	3	0. 885	2. 688
η Aurigae	0. 57	4		
ε Aurigae	0. 56	4		
ρ Aurigae (Hädus primus)	0. 39	4		
δ Polaris (Jldun)	0. 37	3	0. 474	3. 750
Piazzi XVIII^h. 227. 24 Urs. min.	0. 201	6. 7		
Piazzi 0^h. 177. Ursae min.	0. 130	7		
Piazzi XX^h. 424. 24 Hev.	0. 106	5. 6	0. 1456	6. 333
Anon. (Z.) Polaris	0. 0658	7. 8		

Figure 14 (Left) "Light quantities of fixed stars," one of Steinheil's plates illustrating how his device works. (Right) Data that Steinheil used to determine the relationship between magnitude class and intensity by drawing from the photometric measurements of 30 stars and their magnitudes.
The areas of the circles on the left correspond with the sizes of out-of-focus images seen from Steinhei"s split telescope, presumably at equal intensity. The brighter the star, the larger the area on which its light is distributed to appear equally bright as other images. From top to bottom: images of stars of the first to the sixth magnitude.
Source: Steinheil, *Elemente der Helligkeit-Messungen am Sternenhimmel,* Figure 4; *Ibid*, 28.

Argelander in a recent paper, and the values obtained by a number of others.[57] Pogson's scale ended up being adopted by both the Potsdam and the Harvard Observatories, the leading two observatories at the time.[58]

[57] Norman Pogson, "Catalogue of 53 Known Variable Stars, with Notes," *Astronomical and Meterorological Observations Made at the Radcliffe Observatory, Oxford, in the Year 1854* XV (1856): 281–98, 296-7.

[58] Hearnshaw, J. B. *The Measurement of Starlight*, 74.

Both Steinheil and Pogson suggested that although magnitude systems compiled by various astronomers in the 18th and 19th century were based on somewhat arbitrary protocols and often did not agree with each other, they exhibited a similar relationship to physical quantity of light. That is, the difference between stellar magnitudes estimated by directly perceived differences in brightness was roughly a logarithmic function of the ratio between the objective quantities of light emitted by the stars, measured by different photometric techniques. One-degree difference in magnitude corresponded to an approximate 2.5-fold brightness in terms of quantity of light, consistently across different catalogues compiled by different authors. Although the photometric technique was later replaced by photoelectric methods, utterly eliminating the human eye from judgment of equal brightness. Pogson's scale was found to deviate little from the photoelectric magnitudes according to an extensive study in 1969.[59] To Gustav Fechner writing in the late 1850s, the logarithmic relation between magnitudes and intensity coincided with a separate set of experiments performed by E. H. Weber on the tactile senses, which provided important clues to quantifying human sensation.

3.6. Fechner Extending Weber's Law

Fechner's psychophysics was one of the most controversial topics in 19th century sciences. The claim to quantify sensation challenged the Kantian dictum from the *Metaphysical Foundations of Natural Sciences* that there can never be a scientific doctrine of the inner sense (empirical psychology).[60] While many critics still based their response to Fechner on Kant's conception of quantity (chapter 2), others provided more complex reflection on existing measurement

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[59] Ibid, 78.

[60] Kant claimed because mathematics could be applied to inner phenomena, which he understood in the specific sense that concepts in a science must be constructible in the a priori intuition of space and time through the composition of the homogeneous (Chapter 1).

techniques in the 19th century sciences. Debates over Fechner's work prompted many to develop new conceptions of measurement to adapt to existing scientific practices that was increasingly experimentally focused.

In the 1850s, Fechner proposed the psychophysical law as a corollary to his philosophical doctrine of psychophysical parallelism.[61] The logarithmic relation between stellar magnitudes and photometric measurements discovered by Steinheil and Pogson struck a chord with him and suggested a connection to a completely separate set of experiments on tactile, visual and tonal senses, conducted by the Leipzig physiologist E. H. Weber.[62] Weber was interested in investigating various senses. In one set of experiment, he used several subjects to quantify the sensitivity of human tactile senses to weight differences. Two different weights of identical appearance were placed on the subject's two hands, or on their lips, forehead, back of the head, shoulders, arms, stomach and so forth. The subjects were asked to report any perceived difference between the two weights, when the experimenter would alter the actual difference between these weights. Weber reported that his subjects tended to detect a difference when the ratio between the two weights was constant, alternatively, when the ratio between weight increment ΔG from G was constant, regardless of the value of G, within a particular range.[63]

Indeed, just as weight differences became "just-noticeable" when $\Delta G/G$ was roughly constant regardless of G, one stellar magnitude difference roughly corresponded with 2.5 times the quantity of light, photometrically determined, regardless of the magnitude. This suggested to

[61] The basic belief was that the physical and the psychic worlds were not two separate realms but two sides of the same coin, and consciousness extended beyond human life to inanimate objects. See Heidelberger, *Nature from Within* for more details.

[62] Fechner mentioned that both Steinheil's and Pogson's scales in the Elements of Psychophysics confirmed his own psychophysical law. Gustav Theodor Fechner, *Elements of Psychophysics*, ed. Davis H. Howes and Edwin G. Boring, trans. Helmut E. Adler, vol. 1 (Holt, Rinehart and Winston, 1966), 54.

[63] Fechner, *Elements of Psychophysics*, 54-7.

Fechner a common pattern of discernibility in sensation. He further noticed that Steinheil's study of error statistics in brightness judgment also confirmed Weber's results. Letting light from the same source (thus of equal intensity) enter both sides of his apparatus, Steinheil had asked the subjects to adjust the out-of-focus images in the visual field until they appeared equally bright. The error in this kind of matching, in photometric values, turned out to be consistently less than 1/38 of total brightness, regardless of the actual value. In Weber's terms, the barely perceivable increment in brightness was approximately always a constant proportion of the total intensity.

Fechner believed that Weber's law, i.e., $\Delta R/R$ = constant, could be extended to the formula $\Delta R/R = \Delta E$, where ΔE is the "just-noticeable-difference" in sensation. In words, this meant "the difference between two stimuli appears equally large to sensation, when the ratio between stimuli remains the same. [Both stimuli] may both increase or decrease in absolute values, as long as they change in the same ratio [the differences between them still appear the same]."[64] He also believed that just-noticeable-sensations should be added or subtracted from each other. This new formula would then be extended to infinitesimal increments in sensation and stimuli. That is, a constant infinitesimal increment in sensation $d\gamma$ would always correspond to the same relative difference, $d\beta/\beta$, $d\beta$ being an infinitesimal increment from stimulus β. Consider stimuli as simply a summation of increments in stimuli and consider sensation as simply a summation of increments in sensation, then one could justifiably integrate the formula $d\gamma = k\, d\beta/\beta$ to get a function between sensation and stimuli: $\gamma = k(log\beta - logb)$ (k and b are constants related to the threshold of noticeable stimuli), similar to Steinheil and Pogson's

[64] This is a direct quote from Fechner in Adolf Elsas, *Über die Psychophysik. Physikalische Und Erkenntnisstheoretische Betrachtungen* (Marburg: N.G. Elwert, 1886), 5.

logarithmic relation between stellar magnitudes and brightness. dγ and dβ were both regarded as direct measures, or homogeneous units, of sensations and stimuli.[65]

It is noteworthy that underlying this reasoning, Fechner still believed that quantities must be defined through the composition of homogeneous units. Otherwise, he would not have felt compelled to sum up and integrate the ΔE's. Implicitly, Weber's law ($\Delta S/S$ =constant) itself was not a "sufficiently quantitative" description of sensation on its own to Fechner. Instead, intensive magnitudes like degrees of sensation must be "composed" of additive units to claim the status of a quantity. When differences between intensive magnitudes were correlated with a physical constant, such differences could simply be considered as equal and additive for Fechner. Take the case of stellar magnitudes as an example, he believed that the logarithmic function discovered by Steinheil did not merely describe a relation between stellar magnitudes and physical intensity, but rather implied that perceptual differences were additive:

> "The perceived psychological difference between the second and fourth stellar magnitude can be broken down into the two equally perceived differences between the second and third and between the third and size class, and can be assumed to be twice as large as each of these."[66]

One must be able to assert this, Fechner argued, because otherwise an entirely analogous case in weight measurement would be equally impermissible: ordinarily speaking, if the weight difference between A and B is equal to the weight difference between B and C, then the difference between A and C is twice the difference between A and B (or B and C).[67] Differences between weights is thus constitutive of weight as a quantity. By extension, Fechner believed that

[65] Gustav Theodor Fechner, *Elemente der Psychophysik*, vol. 2 (Breitkopf & Härtel, 1888), 13.

[66] Gustav Theodor Fechner, "Ueber die Psychischen Massprincipien und das Weber'sche Gesetz," *Philosophische Studien* Bd. 4 (1888): 161–230, 183.

[67] Ibid.

differences in sensation must also sum up to a quantity of sensation when they were shown "equal," and thus provide a measure for sensation itself.

3.7. Two Sticking Points in Making an Additive Scale of Sensation

While it is beyond the scope of this chapter to thoroughly examine the responses to Fechner's work, I focus on two sticking points in his attempt to construct a scale for sensation. Fechner himself had acknowledged these difficulties, and his commentators also focused on them as the main obstacles preventing Fechner's extension of Weber's law from being legitimate. Meanwhile, these two points are also generalizable problems applicable to the measurement of quantities incapable of direct comparison. The first point concerns on what basis one could declare the difference between sensations A and B as equal with the difference between points C and D, elsewhere on the scale (1). The second point concerns on what basis one could declare that differences among sensations, indirectly measured by an external function, could be considered constitutive of sensation itself. alternatively, it concerns if the difference between degrees in an ordered sequence in general is a degree of the same kind (2). Specifically, in Fechner's case, the experimental subject perceived a sensation of difference (Unterschiedsempfindung) given constant $\Delta R/R$, and one could easily protest that this sensation of difference was entirely different in kind from the original sensation—the sensation of weight, brightness and so on. The sensation of difference would also be different in kind from the arithmetic differences between the original sensations (Empfindungsunterschied). Fechner's attempt to directly add or integrate the sensation of difference to get the original sensation, as if integrating infinitesimal lengths to get total lengths, was unjustified. These two sticking points motivated much of criticisms against Fechner. However, even some of the harshest critics acknowledged that Weber's law itself ($\Delta R/R$ = constant for the differences in stimuli to be noticed) depicted facts, without acknowledging Fechner's extension from Weber's law to an additive scale of sensation.

These two questions were also addressed by Fechner himself. Regarding the first point, he noted in his 1887 reply to critics: "the entire question as to whether a mental measure is totally impossible revolves around this very impossibility," i.e., of claiming that the differences between perceived sensations—characterized by "just-noticeable-differences" ΔE's—could be said to be equal on different parts of the scale of sensation.[68] He seems to believe that just-noticeable-differences were proven equal by the equality of relative difference in stimuli (ΔR/R) accompanying each just-noticeable-difference. As he noted: the claim that differences between sensations were equal on different parts of the scale of sensation was proved by "facts of astronomical determination of equality of such differences"[69] and by other laboratory experiments such as those conducted by the Belgian psychologist Joseph Delboeuf.[70] But the fact that experimental subjects almost always noticed a difference in two sensations when ΔR/R was constant, which those empirical results did indeed show, could not actually amount to the conclusion that the just-noticeable differences ΔE's were equal. Instead, they merely showed that a judgment (e.g., the "noticing" of a difference) was made when the relative differences between stimuli ΔR/R was constant. It would be circular to claim that equal ΔE's correspond to equal

[68] Ibid.

[69] "die Möglichkeit, den empfundenen Unterschied zwischen Helligkeiten in einem Theile der Helligkeitsscala dem empfundenen Unterschied zwischen Helligkeiten in einem anderen Theile derselben gleich zu finden, wird nun aber eben durch die Thatsache der astrononomischen Gleichschätzung solcher Unterschiede bewiesen." (Gustav Theodor. "Ueber die Psychischen Massprincipien und das Weber'sche Gesetz," 181)

[70] Ibid, 181. Although he had no decisive evidence against the notion that those magnitude systems were established purely by arbitrary protocols, he argued that it would be "a most unlikely coincidence" if psychophysical law was not true, because the brightness-intensity relation agreed so well with Weber's law.

$\Delta R/R$'s when each ΔE's was defined by equal $\Delta R/R$.[71] The situation is analogous to temperature measurement before the absolute scale—to claim that the volume of a thermometric substance was a linear function of temperature, one must be able to determine the equality of temperature changes from A to B and from B to C independently of volume. But temperature intervals were defined through volume changes. Similarly, to claim that each increment by $\Delta R/R$ corresponded to equal difference in sensations (more specifically, the sensation of brightness, weight, and other stimuli, not the sensation *of a difference*) would also require one to already know how to measure sensations independently. But this was not the case—for Fechner, increments in sensations (the just-noticeable differences) were precisely defined by stimuli.

Regarding the second point, Fechner argued that the "just-noticeable-differences"—the noticing of a difference given constant $\Delta R/R$—could simply be considered the absolute differences in sensation and of the same kind with the original sensation. Fechner's own arguments were quite muddled on this point. His reasoning went as follows: imagine an ideal case where maximum sensitivity could be achieved, that is, every difference between sensation, however small, could be noticed. Then the sensation of difference (the noticing of a difference) would be identical with the difference in sensation. Now, according to Fechner, Weber's law could be considered valid for infinitesimal increments in sensation (a questionable claim in itself), thus we could consider the differences in sensation as the same as the sensations of difference.[72]

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[71] The circularity in Fechner's reasoning, without referring to the analogous case in temperature measurement, has been made by Heidelberger in *Nature from Within*.

[72] "Man kann bemerken, dass in dem Falle, wo jeder kleinste Unterschied zwischen zwei Empfindungen wirklich empfunden würde, die Unterscheidung zwischen Empfindungsunterschieden und empfundenen Unterschieden müssig sein, vielmehr der empfundene Unterschied mit dem Empfindungsunterschiede zusammen fallen würde. Nun kann man sich unter allen möglichen Weisen, wie ein Unterschied empfunden werden kann, auch den Fall als Gränzfall denken, dass wirklich schon der kleinste Unterschied, der besteht, auch empfunden würde, welches den grösstmöglichen Grad der Unterschiedsempfindlichkeit bezeichnen würde. Insofern kann ein Empfindungsunterschied stets mit einem solchen Gränzfalle identificirt werden, und

Among Fechner's critics, many rejected an affirmative answer to (1) based on the conviction that sensations could not be compared quantitatively in principle, even with indirect measurement, either because it was impossible to compare them directly and achieve the same kind of intuitive certainty as in length comparison, or because sensations could not be reducible to mechanical units—length, mass and time. Jules Tannary and Johannes von Kries, for instance, held this view. I believe that Fechner did in fact have an effective argument against those objections, as I will elaborate in the next section. But many others acknowledged the validity of Weber's law (that humans tend to perceive differences in external stimuli at intervals defined by a constant ratio between stimuli) without accepting Fechner's extension of it. The physicist Rudolf Elsa, who overall condemned Fechner's efforts, noted that "no one would object to the admissibility of these [the original Weber's law] formulation. We have, so far, only found a measure formula for the difference between sensation (Empfindungsunterschied) and the sensation of difference (Unterschiedsempfindung), but not for sensation as a function of stimuli."[73] Weber's law was simply a description of facts:

"experimental confirmation of [Weber's] law has been sought by Fechner himself and numerous other researchers with great acumen and often with admirable diligence…the proportional relationship was not found everywhere and constantly, but for various sensations

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Gesetze und Verhältnisse bezüglich der Abhängigkeit der Empfindungswerthe von den Verhältnissen der Reize, welche für jeden Grad der Empfindlichkeit gleich gültig bleiben, auch wenn sie nur an empfundenen Unterschieden constatirt werden konnten, doch eine Uebertragung auf Empfindungsunterschiede gestatten, da wir uns die Empfindlichkeit blos bis zu ihrer Granze gesteigert zu denken hätten, um die Grösse des empfundenen Unterschiedes mit der des Empfindungsunterschiedes zusammenfallen zu sehen. So hat das Weber' sehe Gesetz nur an empfundenen Unterschieden bewährt werden können; aber diese hindert nicht, es auch für Empfindungsunterschiede im engeren Sinne triftig zu halten, und unter Zuziehung des mathematischen Hülfsprincips die Fundamentalformel für kleine Empfindungsunterschiede daraus abzuleiten…" (Fechner, Gustav Theodor. *Elemente der Psychophysik. Vol. 2,* 85)

[73] "Niemand wird gegen die Zulässigkeit dieser Formulierungen etwas einzuwenden haben. Wir haben bis jetzt aber nur Maasformeln für den Empfindungsunterschied und die Unterschiedsempfindung gefunden, nicht aber für die Empfindung als Funktion des Reizes." Elsas, *Über die Psychophysik. Physikalische und Erkenntnisstheoretische Betrachtungen,* 15.

the validity of the law cannot be denied…in this judgment one can confidently agree with Fechner."[74]

Elsa compared Weber's law to Mariotte's law, on which temperature measurement based on gas expansion was based: the more experiments were conducted, the more deviations from the law were found, but the fundamental significance of the law remained undeniable, and this was true to Mariotte's law as much as to Weber's law.[75] Similarly, the Leipzig psychologist Wilhelm Wundt distinguished Weber's law from Fechner's law, endorsing the former while holding reservations on the latter:

> "According to Weber's principle, noticeable differences in sensation, in terms of the degree of their noticeability, can be equated with one another; whereas after the Fechner's law, equally noticeable differences [i.e. by me, equally large sensation of difference] can be considered equally large differences in sensations."[76]

The main arguments of Wundt's article are the following: Weber's law stated that differences in sensations of stimuli were noticed given constant ratios of stimuli, and therefore was a statement about the *noticeability* of difference. Equal noticeability of difference (Ebenmerklichkeit der Unterschiede) is about degrees of noticeability (Grad der Merklichkeit), not about increments in sensation. This is an explicit negative answer to both (1) and (2): the judgment of a difference cannot yield an absolute difference in sensation; they are two different kinds of sensations. One could only conclude from Weber's experiments, Wundt argued, that the degrees of noticeability (of differences in sensation) were the same because experimental subjects

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[74] Ibid, 46.

[75] Ibid, 45-46.

[76] „nach dem Weber'schen Princip können eben merkliche Unterschiede der Empfindung in bezug auf den Grad ihrer Merklichkeit einander gleich gesetzt; wogegen nach dem Fechneräschen gleich merkliche Unterschiede [d.h. nach mir gleich große Unterschiedsempfindungen] als gleich große Unterschiede von Empfindungen…betrachtet werden können. (Cited in Gustav Theodor. "Ueber die Psychischen Massprincipien und das Weber'sche Gesetz," 200.)

would often not notice a difference in two stimuli until they increased or decreased by a constant ratio; alternatively, this meant differences in sensation were *equally noticeable*, given constant ΔR/R. While this degree of noticeability could be equal, it did not mean that differences in sensation (Empfindungsunterschiede) were equal.[77] On its own, degrees of noticeability could be a valid "measure" of sensation, and the only thing that could be "measured" about sensation, even if it was not constitutive of sensation itself.[78] Degrees of noticeability was nevertheless not an additive quantity:

"A chief objection…[consists in] that the units obtained [for the degrees of noticeability] cannot be added at will like parts of a measuring rod, and as a consequence, the gauging of any given concrete sensation through a chosen unit appears to be a hopeless problem for now and perhaps for good…[but] even those who raise this kind of objections themselves would by no means dismiss the measuring experiments of Weber and his researches as completely pointless and empty. It is indeed in and itself clear that we are dealing with facts of observation, whose interpretation one could have very different opinions about, which cannot be discarded by a theoretical discussion."[79]

This is a crucial step beyond the conventional understanding of quantity: Weber's law might be the only quantitative description of sensation we could have, even if it would not result in an additive scale of sensation. Its psychological significance could be understood as "a law of apperception"—or a general description about how humans perceive: we perceive intensities not by their absolute magnitude, but in relation to each other.[80] Wundt answered negative to both (1)

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[77] Wilhelm Wundt, "Ueber das Weber'sche Gesetz," *Philosophische Studien* 2 (1885): 1–36, 18.

[78] Wundt claimed that ΔR/R could serve as a unit for this degree of noticeability.

[79] „Ein Haupteinwand gegen derartige Feststellungen besteht nun aber noch darin, dass sich die so gewonnenen Einheiten nicht beliebig addiren lassen wie die Theile eines Maßstabes, und dass daher vorerst und vielleicht für immer die Ausmessung einer beliebigen concreten Empfindung mittelst der gewählten Einheit ein aussichtsloses Problem zu sein scheint… In der That werden nun aber selbst von denjenigen, welche derartige Einwände erheben, die messenden Versuche Weber's und seiner Nachfolger keineswegs als völlig zweck- und inhaltslos verworfen. Es ist ja auch an und für sich klar, dass es sich hier um Thatsachen der Beobachtung handelt, über deren Interpretation man sehr verschiedener Meinung sein kann, die sich aber nicht durch eine theoretische Discussion aus der Welt schaffen lassen." (Ibid, 20)

[80] "Eine psychologisch verständliche Deutung lässt sich nämlich dem Weber'schen Gesetz dann geben...dass wir alle in gegenseitiger Beziehung stehenden intensiven Zustände des Bewusstseins ihrer Größe nach nur in

and (2). Despite the fact that Wundt believed that strictly speaking, measurement required the multiplication of units, he simultaneously accepted a different kind of measure as conveying substantial information—a non-additive scale.

The Graz psychologist Alexius Meinong agreed with Wundt in that the content of Weber's law was about a (qualitative) relation of difference among sensations, not about sensation itself: "equally noticeable dissimilarity (Verschiedenheit) are as such not equal, not even equally noticeable" as Wundt claimed.[81] He made a distinction between what he called "difference (Unterschied)" and "dissimilarity (Verschiedenheit)." In his theory, the former expresses a numerical difference between magnitudes (values of already measurable quantities), with the connotation of "arithmetical difference," while the latter indicates an irreducible relation of difference. The *relation* of difference is not divisible into parts: A and B are either different or not different; this judgment is irreducible. On the other hand, the arithmetical difference between two lengths can be another length of a certain magnitude. Now, Meinong asked: if one affirms or denies a dissimilarity between two things, are they making a judgment about the arithmetical difference between them? The answer is obviously no.[82] Meinong did not deny that the qualitative difference, the "dissimilarity," might be correlated with measurable quantities (for instance, the "dissimilarity" between two points—their distance—might be correlated with length; and he later even argued that the "dissimilarity" between sensations could be expressed as

Relation zu einander bestimmen." Alexius Meinong, *Über die Bedeutung des Weber'schen Gesetzes: Beiträge zur Psychologie des Vergleichens und Messens* (L. Voss, 1896), 32.

[81] „Ebenmerkliche Verschiedenheiten sind als solche nicht gleich, nicht einmal gleich merklich." (Ibid.,162.) The so-called "just-noticeable-difference" merely indicated a threshold below which the "noticing" could not happen; it contained no information on the "how much" of a noticeability. (Ibid, 55)

[82] „Wenn ich vergleiche, genauer, wenn ich auf Grund einer Vergleichung Verschiedenheit affirmiere oder negiere, urteile ich da über Differenz? Und aus dieser direkten Empirie heraus, ohne Vor- oder Nachgedanken, mufs ich darauf mit entschiedenem 'Nein" antworten." (Ibid, 95.)

a ratio and correlated with the "dissimilarity" between stimuli), but the judgment made in Weber's or Fechner's experiments was not a judgment about the arithmetical difference between two sensations, but only an affirmation of a qualitative difference. Hence Fechner could not be justified to identify $\Delta R/R$ = constant with the additive difference between sensations. The irreducible difference between two sensations could not be said to constitute sensation itself.

Direct measurement of sensation, for Meinong, was out of the question— "tonal intensities can neither be added or subtracted; neither can intensities of emotion." However, it was still possible to establish fixed points in the "scale" of sensation as reference points. For instance, a fixed threshold of stimulus could serve as the fixed "null-point" of sensation. As was reminded by primitive temperature measurement, he noted, at times the measurement process could be applied beyond the realm of quantities. The comparison of dissimilarities between fixed points in a qualitative continuum could also deliver reliable results.[83] Fixed points on the thermometer could still be useful for comparing the thermal states of things although they did not indicate determinate quantities. Similarly, fixed point in sensation, for example the "null-point" indicated by a threshold stimulus below which no sensation occurred, could also potentially be useful. But this would not be properly considered measurement: "…in my opinion this is not a measurement, but only an assignment (Zuordnung)." Meinong further noted that it could be considered a case of surrogative measurement, just as primitive temperature measurement. The different scales embodied by the behaviors of different thermometric substances corresponded in

[83] „Je mehr sich einer durch ein Verfahren dieser Art an die primitive Temperaturmessung mittelst Thermometer erinnert finden mag, um so weniger wird es ihn befremden, damit vor den schon oben berührten Fall gelangt zu sein, wo das Masssverfahren sogar über das Gebiet der Grössen hinaus anwendbar ist. Auch die verschiedenen Punkte eines Qualitätscontinuums bieten ja Distanzen, deren Vergleichung, wie das Experiment gelehrt hat, nicht minder zuverlässige Ergebnisse liefern kann, als die Vergleichung von Intensitätsdistanzen." (Ibid., 119)

their function with the "permanent or completely fleeting conditions of the difference-sensitivity (Unterschiedsempfingdlichkeit)." In both cases, "[one is] incompletely informed about changes in this regard."[84]

Regarding the assignment of numbers to intensities in general, Meinong also claimed:

> "the boundary between the realm of measurement and mere fixation without measurement is fluid, and the thought of numerical determination without measurement cannot be completely rejected."[85]

As can be seen from above, the comparison with thermometry appeared frequently among Fechner's commentators. While most of them would deny that such ordinal assignment creates quantities, strictly defined, many still acknowledged the value of such numerical description as Weber's law. Even Tannery, noting that the just-noticeable-differences could only lead to a scale in which each sensation was *defined* to correspond to constant $\Delta R/R$, wrote: "…I will not deny that such definition can be of good use…Let this definition be as useful for such studies [psychophysics] as the thermometer was, which serves to define temperature, to the investigations of physics!"[86] A 20th century social scientist, on the other hand, might simply accept such definitional scale as proper measurement. It was perhaps only until mathematicians confirmed that the paradigmatic quantity—spatial magnitudes—only obtained equal units based

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[84] Here Meinong was specifically talking about Lipps, who argued that a given sensation was composed of just-noticeable-differences, but the argument also applies to Fechner: "Das ist…meines Erachtens nicht Messung, sondern nur noch Zuordnung. Vielleicht köntte man sagen: es ist der Fall der surrogativen Messung, wie er auch in der 'Wärmemussung' durch das Thermometer vorliegt...und über diesbezügliche Veränderungen meist recht unvollkommen unterrichtet ist." (Ibid, 119)

[85] "Doch ist hierin das Gebiet der Messung gegen blosse Fixierung ohne Messung nur fliessend abgegrenzt, übrigens ist auch der Gedanke einer zahlenmässigen Bestimmung ohne Messung nicht völlig abzuweisen." (Ibid, 162)

[86] Jules Tannery and Wilhelm Wundt, "Streitschriften über die Psychophysik: aus der Revue scientifique de la France et de l'Étranger," in *Die Quantifizierung der Natur: Klassische Texte der Messtheorie von 1696 bis 1999*, ed. Oliver Schlaudt (Brill mentis, 2009), 127–43, 143.

on arbitrary assumptions, that ordinal quantities became fully justified. Still, the attitudes of Fechner's commentators were more of ambivalence than outright denial.

3.8. Theories of Indirect Measurement

What Fechner did succeed in doing, I believe, was bringing out a re-examination of the foundation of physical quantities among his contemporaries, forcing them to address the flaws in existing ways of thinking about measurement, especially the measurement of quantities incapable of direct comparison. I believe that his arguments against those (e.g., von Kries, Tannery) who argued that measurability meant reducibility to length, mass and time, were successful.

Against the claim that in principle, sensations could never be measured because (a). they could not be directly compared; (b). equality determinations between sensations could never be obtained with an intuitive, "a clearly understood sense" like length equality, in Johannes von Kries' words, Fechner questioned whether those two criteria were met in physical measurement of his time at all: physical measurement often boiled down to the fact that "an equal number of equally strong psychical impressions" were made by an equal number of physical causes. Equality of physical quantities, such as length and mass, was determined by a judgment in human perception—the alignment of points, congruence in superposition, etc. "The number of…physical units is determined by the number of psychical impressions, where the magnitude of the cause of the single impression, or any multiple thereof, serves as a unit."[87] If the equality of quantities like length and mass would boil down to a psychic judgment, then nothing could be said against using a psychic criterion to determine the equality of sensation. Meinong, although he disagreed with many aspects of Fechner's work, echoed this diagnosis of physical measurement: measurement operations "have value only insofar as their results are given a

[87] Fechner, Gustav Theodor. *Elements of Psychophysics*. Vol.1, 51.

significance that can never be grasped in any sense other than through a psychic fact. What would the 'laying on top of each other' mean, other than to bring the stretches concerned to 'cover' [each other]?"[88] It was through psychic facts that operations in physical measurement obtain their significance.[89] If this was the case, how could psychophysical measurement be denied from the outset?

In fact, Fechner did put his finger on an issue that caused great anxiety among mathematicians and physicists of his time, namely that all spatial measurement must boil down to the validity of congruence in superposition, but whether the measuring rod remains invariant in motion cannot be proved (see chapter 2 and 4). Similarly, the measurement of time has also never been based on direct comparison of parts but must be based on assumptions of uniform motions. However intuitive a quantity might appear, its comparison with and relation to equal units still hinges on a concrete measurement process, requiring something that is not the thing to be measured. Direct comparison of magnitudes only exists in imagination or pure geometry. Fechner asked:

> "Do we count periods of time directly in terms of time, when measuring time, or spatial units directly in terms of space, when we measure space? Do we not rather employ an independent yardstick, a measuring rod, which for time does not consist of pure time, nor space of pure space, when we measure space? Measuring any of these three quantities demands something else as well. Why should the case not be the same in the mental or psychological sphere?"[90]

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[88] "Denn sind auch die Messungsoperationen, wie berührt, zumeist physischer Natur, so kommt ihnen ihr Wert eben doch nur insoweit zu, als ihren Ergebnissen eine Bedeutung beizulegen ist, die sich in einem anderen Sinne als dem einer psychischen Thatsache nun und nimmer erfassen lässt. Was hätte auch das Aufeinanderlegen zu besagen, wäre es nicht das Mittel, die betreffenden Strecken eventuell zur "Deckung" zu bringen?" Meinong, Alexius. *Über die Bedeutung des Weber'schen Gesetzes*, 63.

[89] Heidelberger pointed out that Mach also made this claim. Heidelberger, *Nature from Within*. p.245.

[90] Gustav Theodor Fechner, *Elements of Psychophysics*, ed. Davis H. Howes and Edwin G. Boring, trans. Helmut E. Adler, vol. 1 (Holt, Rinehart and Winston, 1966), 47.

Wundt built on this argument about the prevalence of indirect measurement in the sciences. In an effort to endorse measurement of sensation through a functional dependency on external stimuli, Wundt argued that the most basic quantities of time and space are not obtained from the successive synthesis of homogeneous parts, as this criterion fails immediately with the measurement of time: "we are not in and of itself in the position to measure two periods of time which belong to different parts of an unending course of time…the equality of temporal stretches can only always be inferred under certain assumption about *immutability of natural laws over time*." The true foundation of its measurement lies in the validity of certain natural laws:

> "We make use of an assumption with every measurement of time, which cannot be verified by direct observation at all, the validity of we can only infer from those applications that have not led to contradictions…This assumption is the invariance of time durations of certain lawfully recurring natural phenomena."[91]

The earth's rotation had by then approximated such lawfully recurring phenomena and was practically convenient to serve as the standard of time. However, Wundt noted, this assumption of the invariance of durations of lawfully recurring phenomena "cannot be realized with any possible physical motion in the absolute sense" and the results of measurement were only calibrated by the "ideal norm" of motion, against which corrections were made. An "absolutely constant, objective measure of time" is "theoretically unrealizable," Wundt continued, "in its place, the presupposition of the *absolute constancy of natural laws has emerged as the actual condition of every time measurement.*"[92] Likewise, for all basic quantities in physical sciences, from

[91] Wundt, Wilhelm. "Ueber das Weber'sche Gesetz," 15.
[92] "Bei der Messung der Zeit kehren nämlich keineswegs... die nämlichen Verhältnisse wie bei derjenigen des Raumes wieder, sondern jede Zeitmessung ist eine wirkliche Messung nur insofern, als bei ihr eine räumliche Messung stattfindet. Außerdem bedienen wir uns aber bei jeder Zeitmessung noch einer Voraussetzung, die durch unmittelbare Beobachtung gar nicht verificirt werden kann, sondern auf deren Gültigkeit wir nur aus der widerspruchslosen Anwendung schließen, die sie in der Wissenschaft sowohl wie in der praktischen Anwendung zulässt. Diese Voraussetzung besteht in der Annahme der Unveränderlichkeit der Zeitdauer gewisser regelmäßig wiederkehrender Naturerscheinungen, unter denen die Umwälzung der Erde um ihre Axe, der sogenannte Sterntag, um ihrer relativ großen Annäherung an jene Voraussetzung und um ihrer

mass, speed, to force, their measurement rely on "presuppositions apart from intuition." In making this argument, Wundt anticipated many writings in early 20th century on the foundation of measurement (Chapter 4), especially those of Ernst Cassirer.

Meinong pointed out in general that divisibility into units on its own could no longer explain how many concepts in science were measured, returning to Kant's question regarding the additive composition of speeds a century ago:

"if all measurement, as we have learned so far, is the comparison of parts, then certainly only those quantities can be measured, that can already be broken into parts with the same name, namely the so-called divisible quantities … Now one would not object at all to measure distances or dissimilarity, although…dissimilarities are not composed out of dissimilarities. Also, temperature heights and speeds are measured, but no temperature is composed out of temperatures, no speed out of speeds. We are obviously dealing with an *extension of the concept of measure*."[93]

Apart from quantities like temperature or speeds, the categories of divisible or indivisible magnitudes do not apply to most physical concepts that are defined as functions of other quantities. They do not have a basis in intuition as Kant might require for mathematical

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praktischen Brauchbarkeit willen den Vorzug erlangt hat. Wir wissen heute, dass die genannte Voraussetzung bei keiner physisch möglichen Bewegung in absolutem Sinne verwirklicht ist. Um so mehr gestatten wir uns unter Umständen sogar sehr ungleichförmige Bewegungenzur Messung der Zeit zu verwenden, sobald wir nur in der Lage sind, die Resultate der Messungen durch die erforderlichen Correctionen auf die von uns angenommene ideale Norm zurückzuführen. Auf diese Weise ist allmählich die Voraussetzung eines absolut constanten objectiven Zeitmaßes als im strengsten Sinne unrealisirbar theoretisch wenigstens in den Hintergrund getreten, und an seiner Stelle hat sich die Voraussetzung der absoluten Constanz der Naturgesetze als die eigentliche Bedingung jeder Zeitmessung dargethan." (Wundt, Wilhelm. "Ueber das Weber'sche Gesetz," 15) My italics.

[93] "Ist alle Messung, so wie wir sie bisher kennen gelernt haben, Teilvergleichung, so können selbstverständlich nur solche Grössen messbar sein, die in gleich benannte Teile zerlegbar sind, also die bereits oben im be sonderen so genannten teilbaren Grössen. Nun nimmt man aber bekanntlich gar keinen Anstand, etwa Distanzen oder Verschiedenheiten zu messen, obwohl, wie schon einmal zu berühren Gelegenheit war, alle Relationen einfach, insbesondere Verschiedenheiten jedenfalls nicht aus Verschiedenheiten zusammengesetzt sind. Auch Temperaturhöhen und Geschwindigkeiten werden gemessen, obwohl keine Temperatur aus Temperaturen, keine Geschwindigkeit aus Geschwindigkeiten besteht. Wir haben es hier also offenbar mit einer Erweiterung des Massbegriffes zu thun, und es gilt, nun auch die Klasse von Messungsvorgängen zu charakterisieren, in welcher diese Erweiterung zur Geltung kommt." Meinong, Alexius. *Über die Bedeutung des Weber'schen Gesetzes*, 68. My italics.

expressions, rather they are shorthanded representation of relationships among quantities. These concepts play the role of thought-objects, what Meinong called the "objects of higher order," because they are one step removed from immediately intuitable magnitudes. These thought-objects are never numerically expressed through division into parts but through what he called "surrogative measurement." Consider temperature measurement—what does it actually mean to measure heat? Meinong argued:

> "In the truest sense of the word, it is only the mercury column that is measured, albeit on a scale that is constructed in a special way. The connection with temperature is only established by the fact that a certain temperature corresponds to a certain height of the mercury column, and that with the increase and decrease in the height of the column, the temperature of the surrounding area also increases or decreases somewhat…the heat can be said to be 'measured,' insofar as another thing is measured, *whose different states coexist with the states of heat in an empirical lawful manner*."[94]

The similar can be said about the quotient of distance and time *standing for* the notion of "speed." This is different from measuring the length of Rhine indirectly by another length or calculating the altitude by means of an astrolabe. Strictly speaking, the quantity to be measured in surrogative measurement is not measured at all; it is simply represented by what is actually measured (e.g., the height of the mercury column, the quotient of space and time).[95] Even distance, he claimed, a relation of dissimilarity between two points, is not directly divisible and is

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[94] „Gemessen im eigentlichsten Wortsinne wird hier doch nur die Quecksilbersäule an einem allerdings in besonderer Weise angefertigten Massstabe; der Zusammenhang mit der Temperatur wird nur dadurch hergestellt, dass einer bestimmten Höhe der Quecksilbersäule eben ein bestimmter Temperatur zustand entspricht, und dass mit der Steigerung und Herabsetzung der Länge dieser Säule auch am Temperaturzustande ihrer Umgebung sich etwas steigert resp. herabsetzt. Die Annahme eines Parallelismus in den Veränderungen muss dabei nicht einmal sogar wesentlich sein; sonst müsste es dem Alltagsdenken, dem bei "Wärme" doch jederzeit die sensible Qualität vorschwebt, mehr Schwierigkeit bereiten, mit dem 'Sinken' des Quecksilbers eventuell auch ein "Steigen", das der Kälte nämlich, in Verbindung zu bringen. Jedenfalls kann man also sagen: die Wärme gilt hier für "gemessen", sobald ein anderes gemessen ist, dessen verschiedene Zustände mit den Wärmezuständen in empirisch festgestellter Regelmässigkeit koexistieren. (Ibid, 69.) My italics.

[95] Ibid, 72.

measured surrogatively. The distance between two points is a relation of difference between two points, but to every such relation is assigned a length (Strecke), and it is by means of this length that the indivisible relation of distance is measured. We speak unhesitatingly of a distance or velocity being equal to a number, or 10 times more of another distance or speed, because ordinary language does not distinguish direct and surrogate measurement, or simply omitted the complexities involved in the measurement process.[96] This does not change the fact that these quantities are not measured by direct comparison of parts. For Meinong, surrogate measurement also explains how intensities in general can become measurable and acquire the benefits of numerical comparisons. The intensive degrees themselves are never measured; instead, points of the "qualitative continuum" are mapped onto a surrogate that stands for the former. The dissimilarity between degrees of such "qualitative continuum" is coordinated with measurable differences of the surrogate (e.g., the "dissimilarity" between two thermal states is correlated with an external reference, such as the relation between work and heat). Nothing prevents intensive magnitudes from being measured this way in principle and reap all the benefits of numerical measurement. But often times, the coordination does not yield a uniform scale and results only in numerical assignment. Regarding this, Meinong admitted that "the idea of numerical determination without measurement cannot be completely rejected."[97]

[96] Ibid, 76.

[97] „Doch ist hierin das Gebiet der Messung gegen blosse Fixierung ohne Messung nur fliessend abgegrenzt; übrigens ist auch der Gedanke einer zahlenmässigen Bestimmung ohne Messung nicht völlig abzuweisen." (Ibid, 162.)

Chapter 4 Spatial and Temporal Measurement as Problematic Concepts

4.1. Introduction

As shown in the first chapter, space and time were believed to be self-evidently quantifiable. Spatial extension could be measured by concatenating rigid bodies serving as the unit in an unproblematic way, while time was presumed to be composed out of equal durations. Kant classified space and time as the paradigmatic "extensive" magnitudes, based on the understanding that both could be measured by homogeneous, additive parts. All "appearances," or perception of objects, have an extensive magnitude—an argument that might have served to incorporate Descartes' claim that the essence of material objects is extension.[1] The very definition of "extensive" magnitudes, according to which the whole presupposed the cognition of antecedent parts, is modelled on the spatial and temporal magnitudes measured by additive parts: imagine any small parts of space or lines, Kant argued, one must first "successively generating all its parts" before one could imagine a whole extension, and similarly, one must successively generate the temporal parts of the smallest duration in order to imagine it.[2] Partly, the notion that quantity must be composed out of equal units had such longevity into the 19th century because it seemed applicable to space and time on an a priori basis under the framework of Newtonian physics and Kantian epistemology.

For many 19th century scientists occupied with the conditions of measurability, the problem was to explain how the counterpart of "extensive" magnitudes—"intensive" magnitudes—could be reduced to "extensive" magnitudes. Physical quantities became

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[1] See Descartes, "Fifth Meditation."

[2] Kant, *Critique of Pure Reason*, A162-3.

measurable when they were reducible to the units of length, mass and time, because there was a clear, intuitive meaning to the equality of two lengths, two durations, and two masses. The measurability of "extensive" magnitudes, apart from issues related to precision and error, was never cast into doubt except by a few. But through several influential developments at the turn of the century, it became clear that some serious problems infected the measurement of space and time all along. For instance, under what conditions can spatial or temporal intervals be declared *equal* and their scales uniform? For the measurement of space conventionally understood, it is the rigidity of the measuring rod. If one's measuring rod is not fixed, there is no "equal" units and no sensible measurement. For time, durations are never directly compared; the judgment that two durations are equal rests on the assumption that identical phenomena must have the same duration (i.e., that a motion is "uniform"), and in practice must rely on the determination of simultaneity of events (whether the beginning and end of one motion coincide with the beginning and end of another serving as standard). It turns out that "equal" spaces or "equal" times are no straightforward matter, but contingent upon specific assumptions and methods of the measurement process. Some of these assumptions are never fully warranted.

The first line of attack against the self-evidence of "equal" space came from philosophy of geometry, as non-Euclidean geometry came to be more widely accepted and the foundation of determining the structure of physical space came under close scrutiny. As mentioned in Chapter 2, Helmholtz and Riemann were the first to take the non-Euclidean geometries as serious candidates to describe physical space. In analytic geometry, different (Euclidean, spherical, hyperbolic) geometries are distinguished from each other by their curvature. Helmholtz believed that the only way to determine the curvature of the physical space (for Helmholtz, space must have constant curvature) was through measurement, e.g., of stellar parallax. But he also saw a potential problem: all geometrical measurement must rest on congruence in superposition, which in turn must rest on the assumption of the rigidity of our measuring rod. Should our measuring rods, along with all our surrounding bodies including our own bodies, distort in the same proportion as the measuring rod, then measurements thought to be performed in Euclidean space would not truly be so. Intuition alone is helpless in determining whether such distortion actually occurs, but Helmholtz argued that laws of mechanics would provide a way to guarantee the rigidity of bodies.

Helmholtz's thought experiments were taken up in Henri Poincaré's 1902 *Science and Hypothesis* in almost the exact same form. However, Poincaré arrived at the opposite conclusion, namely that measurement is incapable of determining whether space is Euclidean or non-Euclidean. More specifically, if we are informed that objects do not actually go through Euclidean displacements that preserve their Euclidean metrical properties (by which we determine the "rigidity" of bodies by Euclidean metric), i.e., if bodies go through the kinds of distortion observed in non-Euclidean geometry, we would face the option of either choosing non-Euclidean geometry as the description of space, or Euclidean geometry plus specific physical laws describing the behaviors of bodies under distortion in motion. There is no absolute standard for rigidity, because Euclidean and non-Euclidean geometry have different standards for rigidity. If the "correct" geometry is a matter of conventional choice, then so is the criterion for "true" rigid body motions. If there is no absolute criterion for rigidity, then what does "equal" space obtained from congruence in superposition mean? It could be equal in one geometry and not equal in another. Poincaré further argued that behaviors of physical bodies cannot be used to prove the metrical structure of space and geometry ultimately describes the motion of bodies (more specifically, the motions of "ideal" rigid bodies) instead of properties of space. Suppose we are able to construct bodies that distort in a non-Euclidean manner in motion, which coexists along with “rigid” bodies conventionally understood, would we thus conclude that space was both non-Euclidean and Euclidean? The only conclusion we can reach is that one body moves in a Euclidean manner while the other moves in a non-Euclidean manner.

Newtonian absolute time had been questioned by 19th century scientists, since “truly” uniform motion, the standard for time measurement, cannot be proved—the uniformity of motion presupposes that one already knows how to measure time. Through the works of Poincaré and Einstein, attention was also brought to the meaning of simultaneity, which the comparison of durations must rely on. In actual practice, determining the simultaneity of two events separated by a geographic distance must rely on a chosen method of measurement (e.g., sending a signal through telegraph) from which time must be calculated based on the laws of physics involved. Taking this as a starting point, Einstein fleshed out the consequences of taking the speed of light as constant and comparing the measurement of both time and length across different reference frames. With the consequences of “length contraction” and “time dilation” he revealed that their measurements in any particular reference frame are not universally valid. Instead, length and time are both derived quantities, calculated from laws of motion, speed of light, as well as the relative motions of the reference frames, rather than privileged quantities whose quantitative structure is self-evident from intuition.

Mathematics used to be the “science of magnitudes” prior to the 19th century, hence to explain how mathematical techniques could be used in empirical realms was equivalent to explaining how concepts could acquire structures mirroring the properties of magnitudes—capable of division, addition and measuring and being measured by other magnitudes through part-whole relation, and modelled on the geometrical line. However, in the 19th century the urge to instill more rigor into their reasoning process and basic definitions led mathematicians to exclude quantity based on geometric intuition from mathematics. Two most prominent examples include the definition of real numbers based on set theory notions, which replaced "continuous magnitudes" as variables of mathematical functions, and the growth of projective geometry, containing no quantitative measurement but allowing certain properties of metrical geometries (Euclidean, hyperbolic, spherical) to be derived from within the projective framework. Both these developments facilitated the reformulation of mathematical propositions in terms of symbolic logic. Since "quantity" became increasingly irrelevant to mathematics, it no longer served as the link between pure mathematics and quantitative sciences.

The disappearance of "quantity," with its implicit part-whole structure exemplified by the geometric line, from measurement theory had many reasons. Apart from the fact that it no longer applied to the measurement of space and time in a post-relativistic worldview, and apart from its exclusion from pure mathematics, there are at least two other related reasons. Kantian epistemology had to abandon its doctrine of mathematical cognition based on intuition, and geometry in the style of Euclid was replaced by algebraic methods as the dominant form of mathematical representation in the sciences. Both these forces made it inevitable that the old notion of quantity was no longer relevant in explaining the application of mathematics in the empirical sciences. It is not surprising that a brief look into a few early 20th century philosophers who wrote on measurement shows that they did not continue the long tradition of speaking of quantity. Instead, they shifted toward a law-based conception of measurement, in the sense that scientific theories and numerical laws replaced the notion of unit as the real constant in measurement. However, many of their views are continuous from the inquiries on measurement raised by their 19th century predecessors, especially the claim that every measurement is enabled by scientific hypotheses and the view that additive and non-additive measurement deserve separate foundations.

4.2. Rigidity and the Measurement of Spatial Magnitudes

In our conventional understanding, rigidity is the precondition of measuring space. Two spatial magnitudes can be compared by congruence in superposition, if we overlap the measuring rod with one and move it to overlap with another. Without the rigidity of our measuring rods, such procedure is meaningless. But in the writings of Henri Poincaré, one of the most influential and prolific philosophers of science at the turn of the century, both the notion of an absolute criterion for "rigidity" and the notion that "rigid body motions" can measure space are problematic.

In *Science and Hypothesis*, Poincaré used the same thought experiment from Helmholtz's 1868 "On the Origin and Significance of the Axioms of Geometry" to support an entirely different argument, namely that empirical experience cannot determine the metrical properties of space at all. He agreed with Helmholtz with regard to the centrality of rigid bodies in the foundation of geometry: "if there were no solid bodies in nature, there would be no geometry." This is not merely a claim about congruence in superposition as a method of proof in Euclid's geometry, such as in Proposition 4 of the Book I of *Elements* where Euclid explicitly used the overlapping of one triangle onto another to prove the identity of their sides and angles.[3] The notion of rigidity is much more deeply intertwined with geometry as a whole. Consider the subject matter of Euclid's geometry. Various geometrical entities are constructed from fixed lengths and angles: without a fixed notion of "equal" distances between two points, one cannot

[3] Euclid, *The Thirteen Books of Euclid's Elements, Translated from the Text of Heiberg with Introduction and Commentary*, trans. Thomas L. Heath, vol. I, 3 vols. (The University Press, 1908), 247.

construct a circle, or distinguish equilateral triangles from other triangles, or make any claims about one angle, length, or quadrilateral figures being twice or thrice another. The notion of "equal" distance undergirds the construction of equal angles as well (see Proposition 23 of Book I, where equality of angles is proved through the equality of two triangles with equal sides), as well as difference types of geometrical entities in various quantitative relationships. For Euclid, "equality" might have simply been guaranteed by the rigidity of the compass and the straight edge. In the context of 19th century analytic geometry, the meaning of "equal" distance is tied with the analytic expression for distance—the shortest path between two points with coordinates in a manifold—which differs for Euclidean, spherical or hyperbolic geometries. When thinking about a "rigid" body or fixed geometrical length (which is the basis of defining other geometrical shapes) in Euclidean geometry, we think about two points retaining their distance relation (shortest path) in motion: if a straight edge, with its two ends at coordinates x and y, moves to a different place, with its two ends at new coordinates x' and y', the least condition that must be satisfied for us to claim it is rigid is that $\sqrt{x^2 + y^2} = \sqrt{x'^2 + y'^2}$ =the shortest path between the two end points. "Rigid body motions," for us, are those functions that map coordinates to new coordinates but preserve this distance relation between any pair of points—the shortest path between them. This is what our experience has led us to believe. However, for Poincaré, nothing would prevent us from recognizing that in non-Euclidean geometry, a different criterion for rigidity might equally be accepted. In other words, different "rigid body motions" that preserve a different "distance" relation between points can be accepted as valid and compatible with everyday experience, which I will explain shortly.

This way of thinking about rigid body motions coincides with how the mathematician Felix Klein categorized different kinds of geometries in the 1870s. Klein argued that different geometries ought to be categorized through the transformation groups (i.e., functions mapping coordinates to coordinates that form a "group") that each focuses on, because these transformations preserve different invariant relationships between points in different geometries.[4] Euclidean geometry is distinguished from other geometries by functions that preserve its distance (expressed by Pythagorean formula): translation, rotation, reflection. Hyperbolic geometry, on the other hand, studies other functions (e.g., Möbius transformations in the upper-half plane model) that preserve the hyperbolic "distance"—the shortest path between two points in the hyperbolic space—and angles. For Poincaré, these functions could potentially be considered as "rigid body motions" in non-Euclidean geometries.[5]

What is our intuition of rigidity, anyway? According to Poincaré, our intuition of rigidity originated from a combination of tactile and perceptual experiences with everyday objects. No analytic expressions or metrical constraints are actually forced upon us in our selection of "rigid bodies." Changes in our sense impressions related to an object are distinguished into two kinds: those that only involve a change of position for the object and those that involve both changes in position and state for the object. We distinguish the former—the "*rigid*" bodies—by the fact that the total sense impressions involving them and their surrounding objects can be restored by our voluntary movement of our own bodies. For instance, a body x passes from position α to position β. At position α, it causes a total sense impression A, and at β, a total sense impression A'. Suppose there is a second body y, which differs from x in all kinds of respects. y also passes from α to β, causing a change in total sense impression B and B', respectively. There is nothing in

[4] This was studied by both Klein and Sophus Lie, and Lie concluded that there are at most eight possible transformation groups that could be considered candidate for describing space, but Poincaré narrowed them down to the familiar three: Euclidean, spherical and hyperbolic ones, for the reason that others contain counterintuitive properties. Gray, *Plato's Ghost*, 127.

[5] Also discussed in Jeremy Gray, *Henri Poincaré: A Scientific Biography* (Princeton University Press, 2013).

common between A and B, or between A' and B', except that both changes (from A to A', and from B to B') can be corrected (i.e., A and B can be restored from A' and B') by the same bodily movement. It is by virtue of this commonality that x and y are both regarded solid bodies. In contrast, if an object z passes from α to β but the change in sense impression regarding it cannot be corrected by a simple bodily movement in the manner mentioned above, but requires more complex actions, then z is distinguished from x and y as changing in both state and position in displacement.[6]

Therefore, the notion of rigidity in direct perceptual experience is just this: changes in our sense impressions as an object changes in position could be corrected by the same kinds of bodily movements. Nothing can prevent us from imagining a different kind of "rigid body motion" incompatible with our Euclidean notion of rigidity. According to our judgment of rigidity by intuition, bodies undergoing Euclidean motions (those that preserve the Euclidean distance) can be regarded as rigid to the same extent that bodies undergoing non-Euclidean motions (those that preserve the non-Euclidean distance) can be regarded as rigid, if in both cases the original sense impression of those bodies (and their relation to their surroundings) can be restored by the same kind of correlative motion of our body. But the mathematics describing the latter kind of motions and the invariant components in such motion would differ from the mathematics describing rigid body motions in the Euclidean framework.

Now suppose one observes living beings in a world enclosed in a great sphere, with a temperature law that causes everything to contract in size in proportion to distance away from the center. Suppose all bodies in this world have the same coefficient of dilation and light travels

[6] Henri Poincaré, *Science and Hypothesis* (Science Press, 1905), 47.

in arcs. As a living being departs from the center and passes towards the boundary, the measuring rod they carry, along with their bodies and every surrounding object, contract by the same proportion. But all this is what we observe by mapping the paths and angles from this world to our own Euclidean coordinate system. The intelligent beings in this world would not be able to detect the deformations in their measuring rods, since everything contracts by the same extent (appearing to us). But they too would be able to restore the totality of sense impression of their objects in relation to the surrounding objects, etc., through a correlative movement of their bodies. They would develop their own class of rigid bodies, i.e., objects that have the same angle and length measurement (based on their own formula for calculating distances and angles). They would consider the mappings that preserve their length and angle measurements as rigid body motions, even if these mappings would appear to distort bodies from our Euclidean point of view. In other words, these beings would likely develop non-Euclidean geometry. Or they could adopt Euclidean geometry, acknowledge their displacements as actually distorting the measuring rods, and describe the behaviors of their world by specific physical laws, e.g., those prescribing the specific physical deformation according to temperature. The two choices are equivalent and a matter of convention.[7] In Poincaré's words: "no experience will ever be in contradiction to Euclid's postulate; nor, on the other hand, will any experience ever contradict the postulate of Lobachevsky."[8]

While the main point of this story is that experience cannot determine fully which geometry to choose as the one that describes a space, it also entails that there is no absolute rigidity. Rigidity can be associated with the Pythagorean formula if we believe that our Euclidean

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[7] Ibid., 49-51.

[8] Ibid., 57.

space with its metric is correct. But rigidity can also be associated with the non-Euclidean formula if alien beings enter our world and convince us that our "rigid body motions" (translation, rotation, reflection) actually distort bodies in a way we cannot detect and the correct metric is the non-Euclidean one. Pythagorean formula as the description of the shortest path is obtained from measuring bodies that we think are rigid, but there are not ways to ascertain they *really* are rigid. We cannot even ascertain that our "rigid bodies" actually conform to the fixed Pythagorean formula between two points on it without using other supposedly rigid bodies to measure it. Euclidean geometry is therefore based on the behavior of ideal solid bodies—ones suggested but never definitively proved by our experience. Our idea of rigid body motion merely serves as our standard according to which we measure natural phenomena, according to Poincaré. This standard is a convenient choice but by no way forced upon us by experience.[9]

One further argument from Poincaré's *Science and Hypothesis* is that the notion of an absolute space with a fixed metrical structure independent of specific bodies is only an ideal, because all our measurements pertain only to bodies, not to space—"experiments can only teach us the relations of bodies to one another; none of them bears or can bear on the relations of bodies with space, or on the mutual relations of different parts of space."[10] Space independent of the motion of individual bodies cannot be measured. Suppose one compare the geometrical properties of bodies by bring them to contact. Suppose that when moving one body to another, thereby bringing the sides and angles of two bodies into congruence, we observe that this motion is not like translation or rotation. Specifically, Poincaré used an example of bringing a right angle of a triangle to congruence with the angle subtending between the sides of a double hexagon

[9] Ibid., 53.

[10] Ibid., 60.

pyramid and its center—impossible if both are absolute rigid bodies in Euclidean space. If we observe this kind of behavior, it's hard to imagine that we will thereby conclude that space itself has been proved to be non-Euclidean, because it is entirely possible to imagine that a skilled mechanic has created a special kind of body that morphs in this way as it moves. Suppose that we have both bodies that move in this manner and bodies that move in conformity to Euclidean motions, then if space can be determined by the motion of bodies, we would have both the proof that space is Euclidean and that it is non-Euclidean, which would be a contradiction.[11]

While it was possible to argue that the threat to our conventional understanding of the measurement of space posed by Poincaré is only a theoretical one, the reconceptualization of both space and time by Einstein's special relativity drastically complicated the issue. According to special relativity, the determination of length across geographic distance is dependent on the measurement of time, while the measurement of time is dependent on judgments of simultaneity. However, simultaneity of two events is in turn dependent on the reference frame—two events that are simultaneous in one reference frame would not appear simultaneous in another, and thus time durations of a body's motion as well as length measurement also differ. Whether the measurement of length and time are equal is now contingent upon the reference frame from which measurement is made, calculated from laws of motion presuming that the speed of light is constant. Both space and time become derived quantities that are indirectly measured. The idea that spatial extension and temporal durations are quantities measured through the concatenation of units or parts constituting the whole, appears irrelevant by contrast.

[11] Ibid., 62-3.

4.3. "Equal" Times (and Lengths)

19th century scientists widely acknowledged that time had never been measured through direct comparison. As mentioned earlier, psychologists like Fechner justified using external stimuli as the measure of sensation by arguing that the scale of time was also determined by an external reference, i.e., to some particular uniform motion. How do we know that the motions serving as the standard of time is truly uniform, if we have to rely on time to determine such motion? Scientists were reminded of the fact that no truly uniform motion had been verified with the discovery of tidal effect on the rotation of the earth in the 1850s. The discovery had caused an uproar (since the previous explanation for the moon's observed acceleration offered by the great Pierre-Simon Laplace was proven wrong) and left a deep impression among some, especially those who were already suspicious of Newtonian absolute time.[12] The earth's rotation is slowing down, and sidereal days are not truly equal. What else could humans latch onto as an approximate measure of the true, mathematical time that Newton described?

The mathematician Carl Neumann noted in 1869 that equal times should be defined in terms of equal distances.[13] Neumann's idea was a part of his critique of Newton's first law of motion, which was accepted by many as a disguised definition of time. The first law states that a material point, if set in motion and left entirely to itself without being exerted on by an external force, travels in a straight line, covering equal distances in equal times. Neumann saw the law as

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[12] The moon had been observed to accelerate during the previous centuries, which would have contradicted Newton's law of mechanics without the presence of additional forces to explain its acceleration. Pierre-Simon Laplace had offered an explanation in terms of the moving eccentricity of the earth's orbit around the sun, but in the 1850s errors in Laplace's calculation were pointed out by Cambridge astronomer J. C. Adams, who instead pointed to the effects of tidal friction in slowing down the earth's rotation.

David Edgar Cartwright, *Tides: A Scientific History* (Cambridge University Press, 2000).

[13] There is extensive literature on 19th century critiques of the law of inertia, see Barbour, *The Discovery of Dynamics*; Darrigol, *Relativity Principles and Theories from Galileo to Einstein*.

incomprehensible because both "straight line" and "equal times" were undefined—"straight" with respect to what? "Equal" by what standard, if there is no truly uniform motion and time can never be directly compared? So long as we do not know the meaning of "equal time," it would be impossible for determine the meaning of "covering equal distances in equal times" in the law of inertia. The solution is to define "equal" times by means of equal distances: "two material points, each left to themselves (not subject to external forces), move along in such a way that equal sections of the path covered by the first body *always correspond to* equal sections of the path covered by the other."[14] Ludwig Lange, reformulating Neumann's ideas, echoed that "To determine the temporal relations of a given movement means to compare this movement spatially with another movement which is taken as a basis at all times."[15]

In Poincaré's 1898 "the Measure of Time" (later a part of the 1905 *Value of Science*), he argued that astronomers' conclusion about the tidal effects on the earth's rotation down revealed that time is actually defined in such a way as to allow the laws of physics to retain their simplest forms.[16] The moon's apparent acceleration has been known for a long time. Why conclude from this observation that the daily rotation of the earth does not actually represent equal durations?

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[14] „Zwei materielle Punkte, von denen jeder sich selbst überlassen ist, bewegen sich in solcher Weise fort, dass gleiche Wegabschnitte des einen immer mit gleichen Wegabschnitten des andern correspondiren." (Carl Neumann, *Ueber die Principien der Galilei-Newton'schen Theorie: Akademische Antrittsvorlesung gehalten in der Aula der Universität Leipzig am 3. November 1869*(B. G. Teubner, 1870), 18.)

To reformulate the law of inertia, Neumann proposed to define "traveling in a straight line" as traveling in a straight line with respect to a postulated body Alpha, which served the same function as Newton's absolute space.

Also see Barbour, *The Discovery of Dynamics* and Darrigol, *Relativity Principles and Theories from Galileo to Einstein.*

[15] Ludwig Lange, "Über das Beharrungsgesetz.," in *Königlich Sächsischen Gesellschaft der Wissenschaften zu Leipzig. Mathematisch-Physische Klasse. Berichte über die Verhandlungen*, vol. 37, 333–51, 335.

[16] It is unclear whether Einstein had read Poincaré's paper, "The Measure of Time," which was originally published in 1898. The same paper was contained in the 1905 *Value of Science*, published just a few months earlier than Einstein's "On the Electrodynamics of Moving Bodies," where special relativity was proposed.

Why not instead reform laws of physics into approximately true laws and maintain the standard of time (uniform rotation of the earth)? The latter is less convenient.

Overall, Poincaré believed that we do not possess an intuition of "equal times" except for psychological events (e.g., two motions perceived at the same time by the same person). What do we mean when we say that "from noon to one the same time passes as from two to three"? In practice, we use recurring phenomenon such as the pendulum swing to measure other durations. Implicitly assumed is that identical phenomena have the same durations. But the pendulum swing is only uniform in theory, without factors of temperature, air friction, pressure, etc. exerting any effect on it. Certainly, no real pendulum operates in ideal conditions, and real pendulums must be corrected regularly by referring to sidereal time. Now, again, sidereal time is not uniform, as astronomers showed, since it cannot satisfy the ideal conditions stated in the law of inertia. By extension, can there be any two completely identical phenomena in the universe at all? Technically speaking, even a distant star like Sirius has an effect on the timekeeping device like the pendulum on earth. No two phenomena can be under entirely the same circumstances and share identical initial conditions, hence we lose our justification for using identical phenomena to measure time. But rather than claiming that the laws we have for describing pendulum motion as only approximately true and taking this approximately uniform motion as the basis of time, we maintain the laws in its simplest form and correct the "errors" of time measurement, deviations from its ideal values. Time is "so defined that the equations of mechanics may be as simple as possible."[17]

[17] Henri Poincaré, *The Value of Science*, trans. George Bruce Halsted (Science Press, 1907)., 30.

Crucially, as Poincaré pointed out, not only do we lack an intuition of "equal durations," we also lack an intuition of "simultaneity" of event that are not confined in our consciousness, the key judgment with which durations can be compared indirectly. As soon as we try to transcend one person's consciousness (in which the simultaneity of two psychological events, e.g., the entering of two light signals into the visual field, is still possible) and talk about the simultaneity of two physical events separated by a distance, we inevitably have to measure or calculate time. Two lightening strikes are simultaneously observed, but to determine if they struck at the same time we would have to take into account their distances from the observer. The measurement of time in turn depends on the technique of measurement and the laws of physics involved. As Peter Galison argued, here Poincaré was speaking from personal experience when invoking the timekeeping practices of navigators and geographers, as he had served in the Bureau of Longitudes in an era where large-scale geodetic surveys and telegraphic networks required extensive clock coordination. When navigators and geographers traveled to distant locations and need to determine the time difference from Paris, they had to, for instance, send a telegraphic signal to Paris, in which case the lag in time would be calculated based on the speed of the signal. Alternatively, they could observe the same astronomical event as observers in Paris, which in principle cannot be strictly simultaneously observed at different locations on earth. For all available options time must be calculated by presupposing something else—the speed of the signal, the distance between the two locations, and so on.[18]

As can be seen, by Einstein's 1905 paper "On the Electrodynamics of Moving Bodies," Newtonian absolute time was widely regarded as a hypothesis since it was unclear by what means such absolute time could be accessed through measurement. The 1905 paper made it clear that

[18] Ibid., 233-4.

there is no such thing as fixed time or length across different reference frames. Einstein did fix one method for the measurement of time as a convention, i.e., the traveling light signals, and took the speed of light as constant and laws of physics as the same in all inertial reference frames. Given the extensive literature on the topic[19], it suffices here to explain how time and length are calculated from traveling light signals obeying Newton's first law.

Differences in measuring time and length stems from the differences in the judgment of simultaneity. In a "stationary" system (in which observers believe themselves to be stationary), two clocks across distance can be synchronized by the following criterion: at A, one sends a signal to B when the clock at A reads t_A, and receives the returning signal immediately reflected from B when the clock at A reads t'_A; if the signal is received at B when the clock at B reads t_B, and if $t_B - t_A = t'_A - t_B$, then the two clocks are synchronized. In other words, clocks at A and B are synchronized when t_B is the mid-point between t_A and t'_A. This is a result of assuming constant speed of light and Newton's first law. Now consider observing this synchronizing procedure being performed in a moving system. Suppose that this moving system (a measuring rod, for instance, with A and B as its two ends) moves away from the stationary observer at constant speed v. If a person in the moving system at A sends a signal to B, the time it takes for the signal to reach B is $\frac{r_{AB}}{c-v}$ (r_{AB} is the distance from A to B, c the speed of light) and the time it takes for the signal to return to A is $\frac{r_{AB}}{c+v}$, seen from the observer from the "stationary" system. For this observer, the clock at B reads correctly if $t_B - t_A = \frac{r_{AB}}{c-v}$ and $t'_A - t_B = \frac{r_{AB}}{c+v}$. But the person in the moving system, with no reason to believe themselves to be moving, would still synchronize their clocks at A and B by assuming that the signal reaches B at the midpoint between t_A and t'_A. For them, the

[19] A few examples include: Galison, *Einstein's Clocks, Poincaré's Maps*; Jungnickel and McCormmach, *Intellectual Mastery of Nature*; Barbour, *The Discovery of Dynamics*; DiSalle, *Understanding Space-Time*.

clock at B reads correctly (is synchronized with A) if $t_B - t_A = t'_A - t_B$. Hence the two system—neither capable of proving themselves to be truly stationary—would not agree on how their clocks are synchronized, and hence cannot agree on whether two events are simultaneous.

If two systems in relative motion cannot agree on synchrony, then their measurement of time and length will also differ—both become relative concepts across reference frames. If a light signal traveling from one clock to another travels perpendicular to the moving system, then the total distance it covers, seen from a "stationary" system, is larger than the total distance, seen from within the moving system. Then, what is, say, one hour measured within the moving system, is actually more than one hour seen from the "stationary" system. Time is slower in that moving system judged from the "stationary" one. Length measurement, on the other hand, is dependent on simultaneity and turns out shorter in a moving system. In a "stationary" system, measuring a length (across great distance) means subtracting the coordinates of one end point from the coordinates of another. Observers from the "stationary" system record the coordinates of the two ends of a moving rod at the same instant, then send these coordinates to the observers in the moving system. The observers at two ends of that moving rod will receive the two signals at different times and assume that the coordinates of one end (the one further away from where signals are sent) is sent later than the coordinates of the other end, and in this lag the coordinates of the first end has changed. Upon receiving the signals, the observers in the moving system therefore concludes that the coordinates they receive in fact indicate a distance longer than the actual one. Their measurement of length is therefore shorter than the measurements in the "stationary" system.

In light of the way non-Euclidean geometry and special relativity complicated the way length and time are conceptualized and measured, the idea that these quantities have a simple quantitative structure capable of measurement by fixed units appears irrelevant. In special relativity, both length and time are indirectly calculated from speed of light and laws of motion that are invariant in each reference frame. Instead of being primitive quantities, whose measurement lead to the establishment of laws and the discovery of new quantities, length and time themselves become derived quantities—derived from natural constants and laws taken as primitive. This did not merely happen in theory: during the 1880s attempts had been made by researchers like C. S. Peirce and A. Michelson to calibrate the length standard against the wavelength of light emitted by certain elements using interferometry. Michelson and Morley's experiment essentially simplified the task down to counting the number of fringes resulting from the superposing of two wavelengths of the same kind (and therefore the number of fringes was the number of wavelengths) contained within a distance measured by existing standards of length. This allowed the length standard to be calibrated against the wavelength by means of the laws of optics involved in establishing the relationships between experimental objects in an experimental arrangement.[20]

In a sense, the intellectual breakthroughs at the beginning of the 20th century themselves resulted from an inversion of perspective: instead of taking a given definition of space and time as a given, Poincaré or Einstein saw them as derived from stipulated laws and constants underlying the measurement process. The process and method of measurement, which had been invisible to

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[20] Michelson, Albert A., and Edward W. Morley. "LIX. *On a Method of Making the Wave-Length of Sodium Light the Actual and Practical Standard of Length*." *The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science* 24, no. 151 (December 1887): 463–66.

Also see Canales, *A Tenth of a Second*.

Unit time continued to be defined by solar days until the 1960s, when the BIPM switched to the atomic clock (the frequency of transition in caesium-133 atom).

natural philosophers for centuries, now became determining factors of how a quantitative concept ought to be understood and determined.

4.4. Why Mathematics No Longer Needed Quantity

As seen from Chapter 1, mathematics used to be intimately tied with its realm of application, as it was the "science of magnitudes" prescribing a priori truths to the studies of sensible objects instantiating them. In Aristotle's words, arithmetic and geometry "concern the same items" as astronomy, music, and other "mathematical disciplines," making theorems demonstrated in geometry or arithmetic directly applicable to objects and phenomena in applied mathematical disciplines. Natural philosophers of the 17th century who argued for more extensive application of mathematics into physics (traditionally the study of causes) similarly regarded the resulting science as "mixed mathematics" or "physico-mathematics"—this science shared the title of "mathematics" precisely because both concerned quantities.[21] For Kant, the construction of quantities (the successive synthesis of the homogeneous in intuition) was by definition mathematical cognition.[22] Despite the historical centrality of the notion of quantity to mathematics, by the end of the 19th century it was clear that quantity must be excluded from the foundation of mathematics. An explicit declaration can be found in Bertrand Russell's 1903 *Principles of Mathematics*, which claims that the notion of quantity divisible into equal units "does not occur in pure mathematics" for not being "amenable to mathematical treatment."[23]

The exclusion of quantity from mathematics was part of an effort to instill more rigor into its basic notions as mathematics became a more professionalized, autonomous discipline, with

[21] Dear, *Discipline and Experience*.

[22] Kant, *Critique of Pure Reason*, B203.

[23] Bertrand Russell, *The Principles of Mathematics* (University Press, 1903), 158, 419.

the liberty to determine its own subject matter, methodology and evaluation criteria. Several steps were taken towards that end during the 19th century: redefining the foundation of calculus, redefining numbers, separating analysis and geometry from intuition and mechanics, and axiomatizing the entirety of mathematics.[24] Taking the foundation of calculus as an example: the calculus of Newton and Leibniz (based on improvements by Euler, who introduced the idea of a function) was about the dependency of one varying quantity on another. Functions took "continuous magnitudes" as its variables and the continuity of the functions was proved by invoking the "infinitesimal," an arbitrarily small quantity in which any continuous magnitude could vary.[25] This kind of definition allowed Hermann Cohen to argue in his 1883 *Das Princip der Infinitesimal-Methode* that the infinitesimal was grounded in (and justified by) Kant's a priori category of "reality" in *The Critique of Pure Reason*.[26] For Cohen (and for Kant), it was still possible to provide an epistemological foundation for the applicability of mathematics by establishing a direct link between fundamental notions in mathematics, such as the infinitesimal, and the form of direct perceptual experience. It would be increasingly difficult as mathematicians continued to eliminate intuitive notions from definitions, to whom the urgent task was to replace these ambiguous terms with precisely defined structures. For instance, Richard Dedekind spoke of the term "continuous magnitude" in his "Continuity and Irrational Numbers":

> "The statement is so frequently made that the differential calculus deals with continuous magnitudes (Grössen), and yet an explanation of this continuity is nowhere given; even the most rigorous expositions of the differential calculus do not base their proofs upon continuity but, with more or less consciousness of the fact, they either appeal to geometric notions or those suggested

[24] See Gray, *Plato's Ghost*.

[25] See Cauchy's definition of continuous functions. Ibid., 62-65.

[26] See Marco Giovanelli, "Hermann Cohen's Das Princip der Infinitesimal-Methode: The History of an Unsuccessful Book," *Studies in History and Philosophy of Science Part A* 58 (August 1, 2016): 9–23, https://doi.org/10.1016/j.shpsa.2016.02.002.

by geometry, or depend upon theorems which are never established in a purely arithmetic manner."[27]

Dedekind and Karl Weierstrass were responsible for substituting the notion of continuous magnitudes with real numbers in analysis.[28] Furthermore, Dedekind defined real numbers through the relations and operations governing rational numbers, which simultaneously captures the intuition of the continuous line. Specifically, every real number is defined as a partition of rational numbers into two sets, such that all members of the first set is smaller than all members of the second set, and the first set has no greatest member (or the second set has no smallest number). If there is either a greatest member of the first set (or a smallest member of the second set), then the partition is created by a rational number. If there is neither, then the partition corresponds to an irrational number. Irrational numbers thus "fill the gaps" between the rational ones.[29] By reducing the real numbers to relations between sets of rational numbers, the notion of continuous magnitude is rendered superfluous.

During the second half of the 19th century, projective geometry underwent prominent growth and its significance to mathematics contributed to the view that geometry need not be quantitative. As mentioned earlier, geometry according to Klein was the study of invariant structures preserved by different groups of transformations. In projections (think of e.g., mapping the points and shapes from the surface of a sphere to a flat paper from a point, or mapping points

[27] Richard Dedekind, *Essays on the Theory of Numbers: I. Continuity and Irrational Numbers, II. The Nature and Meaning of Number*, trans. Wooster Woodruff Beman (Open court publishing Company, 1901).

[28] Gray, *Plato's Ghost.* 129-135

[29] Just as between two points on a line there are always infinitely more points, between any two rational numbers there are infinitely more rational numbers that can be inserted. But for any length measured by any unit, there are always infinitely many incommensurable lengths. Hence if rational numbers are mapped onto points on the straight line, then there are infinitely more points on the straight line that cannot be mapped to rational numbers. In this sense, the irrational numbers "fill the gap" between rational numbers and can be constructed through rational numbers by means of set-theory operations. Dedekind, *Essays on the Theory of Numbers*.

from a horizontal to a vertical map, etc.), lengths and angles are not preserved. Therefore, projective geometry does not study these quantitative notions. But other properties, such as the colineation of points (points lying on the same line still lie on the same line under projection) and a relation between 4 points on a line called the cross-ratio, are preserved (Figure 1).[30] If the cross-ratio is equal to -1, then the 4 points form a unique relationship called the "anharmonic range." This is a numerical relationship that does not require the measurement of the distance between any two points but can be determined by algebraic means and geometrical construction. Given any 3 points and their coordinates, it is capable of determining the unique 4th point that forms an "anharmonic range" with them, by geometrical construction that only relies on the incidence relations (e.g., if a point lies on a line, or if a line goes through a point) and the assumption that projective transformation preserves the cross-ratio (Figure 15). Because of this unique relationship, one could construct a coordinate system without any metrical notions: given any 3 fixed points on a line, assign them the coordinates of 0, 1, and ∞, it is then possible to determine the position of 2, 3, 4… uniquely by the iterative construction of the anharmonic range. Just as how points on a Euclidean line are assigned numerical coordinates through the iterative concatenation of a unit "distance" assuming that the "distance" is invariant, here the iterative construction is one of projections by assuming the invariance of cross-ratios, without invoking actual measurement of the segments involved (Figure 15). Although there are no pre-determined notions of length and angles, it is nevertheless possible to build up the metric of Euclidean and

[30] On this topic see Bertrand Russell, *An Essay on the Foundations of Geometry* (University Press, 1897); Jeremy Gray, *Worlds Out of Nothing: A Course in the History of Geometry in the 19th Century* (Springer Science & Business Media, 2011); Felix Klein, *Elementary Mathematics from an Advanced Standpoint, Geometry*, trans. E. R. Hendrik and C. A. Noble (Dover Publications, 1939), http://archive.org/details/elementary_mathematics_geometry.

non-Euclidean geometries within this new coordinate system.[31] Specifically, if for any 4 points in a cross-ratio, 2 are fixed points (real or imagined roots of a quadratic function), then the cross-ratio becomes the expression of distance in Euclidean or non-Euclidean geometries.[32] In this sense, it is possible to define metrical concepts within projective geometry. For this reason, Felix Klein argued that "the projective method embraces the whole of geometry."[33]

[31] See Sebastien Gandon, *Russell's Unknown Logicism: A Study in the History and Philosophy of Mathematics* (Palgrave Macmillan, 2012).

[32] Ibid.

[33] Felix Klein, "On the So-Called Noneuclidean Geometry," in *Sources of Hyperbolic Geometry*, ed. John Stillwell (American Mathematical Soc., 1996), 69–112.

When Russell wrote the *Principle of Mathematics* at the turn of the century, which aimed to

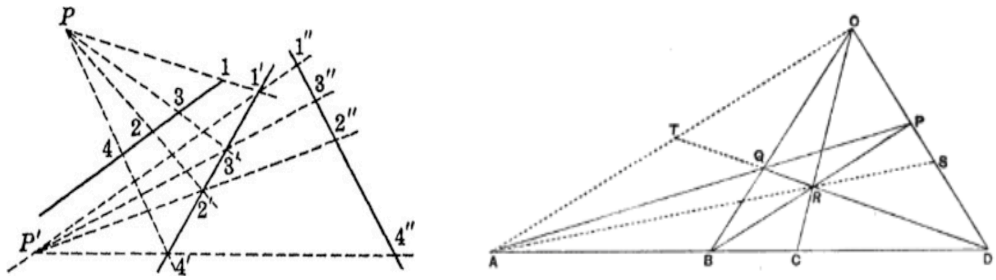

Figure 15 (Left) Cross-ratio in projective geometry; (Right) Karl von Staudt's quadrilateral construction.

The cross-ratio between points 1,2,3,4 (D = $\frac{x_1-x_2}{x_1-x_4}:\frac{x_3-x_2}{x_3-x_4}$ where $x_1 \ldots x_4$ are the coordinates of points 1,2,3,4) is equal to the cross-ratio between 1',2',3',4', which is in turn equal to the cross-ratio between 1'',2'',3'',4''. The numerical value of D is invariant through projective transformations, which are linear fractional functions taking the coordinates of 1, 2, 3, 4 as variables.

Karl von Staudt's quadrilateral construction shows why the anharmonic range is a unique relation that does not depend on measurement of lengths, but only depends on algebra and geometric construction. Consider any 4 points in a harmonic range. If any A, B, C, D with cross-ratio = -1 (not necessarily referring to the A, B, C, D in the image above), then AB/AD: CB/CD = -1 = CB/CD:AB/AD. That is, the cross-ratio between A, B, C, D is equal to the cross-ratio between C, B, A, D. Notably, here AB…CD need not indicate lengths between points, but can just represent the differences between the coordinates of these points. Suppose we are only given A, B, D on a straight line, as seen in the image above, and want to find the 4th point C such that A, B, C, D have cross-ratio = 1 (*). This task is equivalent to finding a 4th point such that there is a projective transformation that maps A, B, C, D to C, B, A, D (**). Von Staudt proved that the following construction satisfies (**) and thereby (*): take a point O outside A, B, D. Join OB, OD. Through A draw a straight line cutting OD, OB at P and Q. Join DQ, BP, and let their intersection b R. Join OR, meeting ABD at C. C is the point required, because A, B, C, D are projected onto S, P, O, D through R, and S, P, O, D are projected onto R, Q, T, D through A, and R, Q, T, D are projected onto C, B, A, D.

Source: Klein, Felix. *Elementary Mathematics from an Advanced Standpoint: Geometry*, 7; Russell, *Essay on the Foundation of Geometry*, 125.

deduce the entire mathematics from a small number of premises through propositional logic, he similarly regarded projective geometry as more fundamental, for two reasons: it requires the smallest number of axioms, and metrical geometry can be derived from within projective geometry, while the reverse is not so easy.[34] The proclamation that projective geometry is more fundamental was very much motivated to encompass all geometries into a system of deductive, logical statements. Metrical geometries, including the Euclidean and non-Euclidean ones, were regarded by Russell as a branch of empirical science. The reason was that quantity, or distance, which requires divisibility into equal parts, is an empirical rather than logical notion: "Quantity, in fact, though philosophers appear still to regard it as very essential to mathematics, does not occur in pure mathematics, and does occur in many cases not at present amenable to mathematical treatment."[35] This view was later embraced by the neo-Kantian philosopher Ernst Cassirer, who came to adopt Russell's vision of mathematics in the 1910 *Substance and Function* and noted that the development of projective geometry realized Leibniz's vision, namely that before space is defined as a quantum, it must first be grasped "in its qualitative peculiarity as an 'order of coexistence.'"[36] In light of 19th century developments in mathematics, the discipline is no longer merely an instrument of measuring and comparing magnitudes, as Descartes' "mathesis universalis" outlined, but rather the study of relations.[37]

If the generalized quantity (implicitly based on the archetype of geometrical magnitudes found in Euclidean geometry) is no longer a relevant notion in pure mathematics, then theories explaining how mathematics is applied do not need to appeal to the notion either. Quantity used to be the link between mathematics and empirical disciplines, but for authors writing at the turn of the century, this link no longer existed. The application of mathematics can be as much the application of its methods (e.g., the differential equation) and its formalization techniques as the

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[34] Russell, *The Principles of Mathematics,* 426-8. Gandon, *Russell's Unknown Logicism*.

[35] Ibid., 426.

[36] Ernst Cassirer, *Substance and Function, and Einstein's Theory of Relativity* (Open court publishing Company, 1923).91.

[37] Ibid., 95

application of particular "quantitative structures" to a particular concept. The quantity concept based on part-whole structure is no longer the only model through which mathematical application is conceptualized.

4.5. Early 20th Century Philosophical Discussions of the Measurement Processes

As discussed in the previous chapters, 19th century authors already began to turn their attention away from giving definitions of quantity to discussing the conditions and processes of measurement. This led, for instance, Helmholtz to discuss how mathematical relations and properties like equality and additivity are translated in terms of empirical observations or operations. Meanwhile, Mach and Fechner's numerous commentators confronted the question of how uniform scales for quantities that must be measured "surrogatively," incapable of direct comparison, can be constructed. To what extent do lawful experimental phenomena define a new quantity with a uniform scale? In their reflections, notions of unit, homogeneity, parts and the whole, extensive and intensive magnitudes, fade into background. Too often these notions are used to project a fixed structure onto measurable aspects of nature, without clarifying how these aspects become quantifiable in the first place or what quantification means. In philosophers' discussion of measurement at the beginning of the 20th century, we see a more decisive and explicit shift away from those terms in traditional philosophy of quantity. Early 20th century writers fleshed out the implications of conceiving measurement through the experimental laws involved, or through the conceptual and operational procedures in measurement. Units of length, mass and time no longer occupy a privileged status apart from other measurable quantitative concepts in these discussions.

One notable trend in early 20th century discussion is the law-based conception of measurement (as opposed to the unit-based conception). That is, measurement is conceptualized as a process of establishing quantitative laws of nature. For some philosophers, measurement constitutes not so much a standalone topic as integral to broader epistemological questions regarding the relationship between theory and observation, between formal symbolic knowledge and "raw" experience. For instance, in Morris Schlick's "Form and Content" (originally delivered as a 3-part lecture in 1932), he mentioned measurement in his discussion of how formal systems are interpreted in an empirical context through observation:

> "In the case of the physicist, observation always takes the strict form which is called measurement. The relationships between what is actually observed or measured and the quantities which finally appear in the equations expressing the laws of nature are extremely complicated, but we do not have to concern ourselves with them. It is sufficient to remark that the whole process leads to the establishment of a one-one-relation between a particular value of a certain physical quantity and a particular fact of observation. In other words: it is stipulated — lastly by arbitrary agreement—that the proposition 'Under such and such circumstances (here the apparatus and the whole procedure have to be exactly described) such and such a fact is observed' shall be equivalent to the proposition: 'The quantity so-and-so has the value so-and-so.' *This is simply the definition of the quantity*: it is the way in which the sign denoting the quantity is connected with reality."[38]

Quantitative concepts are therefore placeholders for conditional statements in observation, which organize the immediately "given" in perceptual experience into formal knowledge through quantitative descriptions. Schlick's law-based conception of measurement is quite explicit in his claim that quantities are just statements of experimental effects under specific conditions.

An earlier explicit declaration of the law-based conception of measurement can be found in the writings of Ernst Cassirer, a prominent spokesperson for the Neo-Kantian school of philosophy. As a student of Hermann Cohen, Cassirer's views on the topic of number and measurement diverged significantly from his teacher. Notably, in Cassirer's 1910 *Substance and*

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[38] Moritz Schlick, "Form and Content," in *The Norton Anthology of Western Philosophy: After Kant*, ed. Richard Schacht et al. (W.W. Norton, 2017),732-733. My italics.

Function, Cassirer took the logicism of his contemporary mathematicians as his starting point, quoting Russell's claim that "quantity" does not belong to pure mathematics. Hence he treated measurement as a different topic from mathematics, requiring different epistemological foundations. Cassirer highlighted the theory-ladenness of measurement. Measurable or numerable phenomena are never self-evidently given in sensations, but created through certain conceptual operations, which according to Cassirer are theoretical hypotheses taken to be universally valid.[39] Consider how the unmediated sensation of heat transforms into the measurable concept of temperature, he noted. We would assume that differences between temperature are directly proportional to the volume of mercury serving as the thermometric substance, but the assumption of the functional dependency between extension and temperature is "nothing but a hypothesis suggested by empirical observation" that never forces itself upon us. Indeed, using a different thermometric substance, the formula of proportionality immediately calls for modification: "the simple quantitative determination of a physical fact draws it into a network of theoretical presuppositions, *outside of which the very question as to the measurability of the process could not be raised*."[40]

Measurement for Cassirer, as for Schlick, is synonymous with establishing experimental laws. Defining quantity is not making an ontological assertion, i.e., asserting a type of "thing," a bearer of magnitude, a substratum in nature that shares the properties of some generalized,

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[39] This, I argue, is where Cassirer differs from Kant. While both agree that quantities are the result of applying conceptual procedures to the immediately given, Kant identified such conceptual procedures with the construction of magnitudes in the same way geometrical magnitudes are constructed in Euclid's Elements, while Cassirer identified them with theoretical hypotheses. While Cassirer would not deny that there are certain commonalities in forming different theoretical hypotheses, the specific hypotheses are ultimately susceptible to theory changes.

[40] Ernst Cassirer, *Substance and Function, and Einstein's Theory of Relativity* (Open court publishing Company, 1923), 142.

Platonic idea of magnitude. Instead, the measurable concept stands for a general principle, a functional dependency between parameters observed.

The law-based conception of measurement is also clear from his critique of the concept of unit: the constancy of the unit for any quantity, for Cassirer, is conferred by hypotheses—the "hypothetical assumption of a universal principle, which first make possible the unit of measure." He used the example of time measurement. Equal times has been defined by various principles: the "uniform" rotation of the earth, or the law of inertia (equal distance is covered by an object in equal times in inertial motion), or the exponential law of decay when using radioactive decay of radium as the measure of time. The question of whether equal units are possible are in fact about whether certain laws are valid:

> "What guides us in the choice of units is thus always the attempt to establish certain laws as universal…the real constants are thus fundamentally…not the material measuring-rods and units of measurement, but these very laws, to which they are related and according to whose model they are constructed."[41]

If special relativity has taught us anything, according to Cassirer, it is the lesson that measurement process is a crucial part of theory making—"the physicist does not have only to hold in mind the measured object itself, but also always the particular conditions of measurement."[42] Every experimental object is not a mere material body but an instantiation of some conceptual process. A small magnetic needle in a tangent compass and a conducting wire are symbolized as an infinitesimally small magnetic axis and a strict geometrical circle. Every experimental instrument embodies some theories and performed its function only insofar as these

[41] Ibid., 146.

[42] Ibid., 358.

theories are valid.[43] Each is idealized as the perfect instrument, based on which the "errors" from measurement can be corrected.

The physicist and philosopher of science Norman Campbell developed a theory on measurement in 1928 of *Principles of Measurement and Calculation*, which fleshed out his ideas on measurement from an earlier publication (the 1920 *Physics: The Elements*). One of Campbell's motivations was that physics textbooks frequently opened with a chapter on "Measurement and Units," simplifying this complex activity to merely the "comparison with a unit of the same kind"—the classical conception of measurement that I discussed in the first chapter. Campbell considered this treatment as "perfunctory" and at times "false," since it fails to reflect basic measurement practices in experimental sciences: "a student set to measure g with Atwood machine may well wonder at what stage he is comparing the gravitational acceleration with a unit of the same kind; and an engineer may be puzzled when he considers that the unit of force that he employs causes an acceleration in his unit mass of 32.2."[44] The conceptual processes involved in measurement, the principles under which properties become quantified, for Campbell, deserve a systematic discussion on their own.

There is much continuity between Campbell's approach to the topic and his 19th century predecessors. His book presents a rare case in early 20th century philosophy of measurement in which principles of measurement are derived from surveying existing quantitative concepts in the exact sciences. As shown in Chapter 1, this approach seems more common with 19th century authors. Although Campbell classified different kinds of quantitative concepts, his classification

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43 Ibid., 365, 144.

44 Norman Robert Campbell, *An Account of the Principles of Measurement and Calculation* (Longmans, Green and Company, Limited, 1928). vi.

focuses not so much on the quantities themselves as their measurement methods. No generalized characterization of "quantity" is found in the book.

Specifically, Campbell made a crucial distinction between "fundamental" and "derived" measurement echoing Meinong's distinction between directly and surrogatively measured quantities. "Fundamental" measurement according to Campbell relies on an additive procedure for the elements to be measured, while "derived" measurement depends on the discovery and formulation of laws expressing relations between quantities that are already measurable. Quantitative concepts like length, mass, periods of time, electrical resistance, area, volume, force, energy, momentum, current, potential difference, charge, conductance, capacity, thermo-capacity, monochromatic illumination, etc., are capable of being "fundamentally" measured, given that there are operations that could be interpreted as direct addition for these quantities. Concepts like density or pressure, defined as mass per volume or force per area, are defined through other measurable concepts and not directly additive.

Quantities obtained from "derived" measurement are those tied with the "discovery and formulation of a certain class of laws," which he called numerical laws.[45] Consider numerical laws like $m = \rho V$, which defines density in relation to mass and volume. These kinds of laws assert some uniform association between already measurable quantities under certain conditions, e.g., between two properties of chemically similar liquids at the same temperature. Oftentimes the quantity defined by such laws might have no meaning apart from the law, but they can also be associated with an observed property originally thought to be incapable of measurement. For instance, before density is defined, there is undoubtedly some notion of a property ranking different liquids by floatation: if an object that floats in A sinks in B, then A > B. As Campbell

[45] Ibid., 57. Examples of these numerical laws: $m = \rho V$, $E = R \cdot i$, $I = I_0 \cdot e^{-\lambda t}$, and so on.

noted, while this ordinal sequence is not capable of being uniquely coordinated with numbers (as we have also seen with the case of temperature), its order might later coincide with an ordinal sequence of ρ *defined* through some relation between two other quantities, such as $m = \rho V$. $m = \rho V$, on its own, was merely a definition. But the property of liquids defined by flotation can be *identified* or coordinated with the property defined by $m = \rho V$. As Campbell argued, scientists might invoke further theoretical explanations to justify that the two properties should be regarded as identical, but more often "the purely experimental discovery of a uniformly identical order is sufficient to justify the assertion that a previously known but immeasurable property has been measured."[46] That is, a purely experimental observation often allows definitions through numerical relationships to be merged with some previously known, qualitatively defined property. The discovery of experimental laws, as Campbell pointed out, is the key to defining new quantities that could stand for previously immeasurable aspects of phenomena:

> "This procedure is of the greatest importance, especially for the sciences bordering on physics. The extension of the 'physical methods' (physics, it must be remembered, is the science of measurement) to other sciences, such as chemistry or physiology, consists mainly in the establishment of numerical laws relating magnitudes already known and the identification of the constants in them with properties already recognized by those sciences, but in the absence of measurement, recognised somewhat vaguely…in fact, the history of any branch of science which has become quantitative within recent times would be nothing but a variation of the steps in this procedure."[47]

The quantification of temperature, as showed in the last chapter, has also gone through a similar conceptual process. Campbell's account of how qualities become quantitative is compatible with the history of thermometry described in the previous chapter. Temperature (or density, which Campbell often used as an example) are not quantities that "exist" out there in nature. The actual algebraic formula is artificially defined, but it is defined as some relation

[46] Ibid., 82

[47] Ibid., 83.

between other quantities such that the numerical values of this formula have the same order as some previously observed, qualitatively described ordinal sequence. In the case of temperature this ordinal sequence used to be thermal sensations, in the case of density the order associated with floatation. Temperature scientifically defined is not thermal sensation, and density not the order of floatation. Language does not distinguish the two, since the same concept are still used. But the quantitative concepts defined through numerical laws have thoroughly replaced the previous notion associated with perceptual experience, in exchange for precision and control.

Notably, Campbell also addressed the view that measurement in physical sciences is equivalent to reduction to length, mass and time, for the reason that these quantities are more fundamental. This view, as discussed in the first chapter, was shared among many 19th century scientists:

> "…It is quite common to find in the writings of those who ought to know better, and not only of philosophers, such statements as that it is the practice in physics 'to reduce all measurements to those of space and time.' This is, of course, an *absurd error*."[48]

The reason for such error is that the reading of instrument, which involves finding that the pointer coincides with the marks on the scale in space, is not equivalent to measuring space or time. The process of reading from a measuring instrument is conflated with the circumstances under which a concept became measurable in the first place. The latter involve often an additive process for some quantities, or a process through which relations among additive quantities are experimentally established for some others. These are absent in the act of reading from an instrument, where only the equivalence relation "=" is required. Reading from an instrument is

[48] Norman Robert Campbell, *An Account of the Principles of Measurement and Calculation* (Longmans, Green and Company, Limited, 1928), 40-1.

not equivalent to the measurement of a quantity, even if it involves the reading of a linear scale. The conflation resulted from the division of intellectual labor in modern science:

> "the result of the widespread use of calibrated instrument of one kind or the other is that most of us leave all true measurement to instrument-maker and standardizing laboratories. It is they, and not the users of their products, who assign numerals to represent properties, and are therefore concerned with the rules which determine the assignment. This division of labour …becomes dangerous only if we allow ourselves to forget that the labour has been divided, that the operations to which laboratory practice is confined are not complete measurement, and that an understanding of the principles of measurement must be based on an investigation of laws and operations which the most experienced and accomplished physicist may never have occasion to establish or to perform for himself."[49]

Hence even if the measurement of a quantity in laboratory practice boils down to reading scales of length, mass and time on instruments, this does not necessarily mean that the measurement of the quantity concerned is none other than reducing concepts to length, mass and time. This latter conception originates from examining the measurement process after a quantitative concept is well-established, when there is an agreed-upon method of measurement for it. The problem remains under what condition such concept renders itself to quantitative descriptions in the first place.

Both Cassirer and Campbell showed that early 20th century philosophical discussions no longer need to rely on the unit-based conception of measurement. They attempted to reveal the conceptual activities, hypotheses, and laws involved in the measurement process. While their views are not remembered as radical breakthroughs in philosophy, their focus on the role of theories and laws in measurement represent a common understanding among early 20th century intellectuals, which shifted significantly from late-19th century.

[49] Ibid, 10-11.

As mentioned above, Campbell's establishing order as the first principle of measurement, more primitive than additivity and physical laws, reflects the conceptual and historical process of quantifying intensive magnitudes like temperature. Let us be reminded that, as I discussed in Chapter 3, a few late 19th century scientists recognized the kind of numerical coordination that preserves the order of the states to be measured and produces fixed points for reproducing such non-additive states. The measurement of sensation and primitive temperature measurement are among the ranks of such quasi-measurement. Although 19th century scientists did not recognize this as proper measurement, they acknowledged its usefulness. When Campbell took order instead of unit as the starting point of measurement, it encouraged some 20th century philosophers to simply regard ordinal measurement as proper measurement. Ordinal scale also provides an intermediary stage between arbitrary designation and measurement that establishes a uniform scale. For instance, Ernst Nagel spoke of the prevalence of ordinal, non-additive quantities in physical sciences in a 1931 article: "no science, certainly not physics, can dispense with qualities that are incapable of addition in the fullest sense, and the progress of modern science has consisted very largely in bringing nonadditive qualities like density, temperature, hardness, viscosity, compressibility, under the sway of numerical determination."[50] Ordinal scale is classified along interval and ratio scales in S. S. Stevens's table of scales of measurement and in the 1971 *Foundations of Measurement* by Krantz et al., regarded as a foundational text in social sciences. The justification of ordinal scales from a theoretical standpoint confers legitimacy and mathematical status to the use of ranking in social sciences. Granted, the fact that order appeared more fundamental to 20th century authors than notions like unit, homogeneity and division into

[50] Ernest Nagel, "Measurement," in *Scaling: A Sourcebook for Behavioral Scientists*, ed. Gary Maranell (Routledge, 2017). Stevens, "Measurement," in Ibid.

parts, typically found in older texts on quantity, has much to do with the formalization of mathematics and that of the quantity concept.[51] However, 19th century discussions that revealed the prevalence of non-additive ordinal measurement in the sciences certainly opened up the possibility of extending the criterion of measurement.

[51] Starting from Hilbert's *Foundation of Geometry*, originally a set of lectures given between 1898-1899, the geometrical line was defined as entities (points) governed by formal axioms that mimic geometrical relations. The axioms of order form one of the first sets of axioms that describe the relation between points and straight lines. See David Hilbert, *The Foundations of Geometry* (Open Court Publishing Company, 1902).

Closing Thoughts

In November 2018, member states of the Bureau International des Poids et Mesures (BIPM) voted to adopt a revision of the international system of units. The decision has been celebrated as changing the foundation of measurement in a fundamental way—"a landmark moment in scientific progress" in the words of BIPM's director—because it ended the use of tangible artifacts to define measurement units, a practice in use since antiquity. Henceforth the seven base units[1] for all scientific measurement are defined in terms of physical constants: these physical constants are now taken to be primitive and invariant, safeguarded by the state-of-art knowledge in physical sciences. Some of the more familiar constants include the speed of light, the Planck constant, the frequency of oscillation of a microwave beam to excite the transition of energy level of cesium-133 atoms,[2] the elementary electrical charge, and so on. The base units such as length and time, which were previously defined by tangible artefacts like the Meter rod from the 1875 International Metre Convention, from which physical constants used be calculated, are now derived from and calibrated by the latter.

The most dramatic change in the 2018 convention was the replacement of the kilogram. The unit length had previously been redefined through the speed of light in 1993, and unit time through the frequency of microwave radiation exciting cesium-133 in 1967. For about 130 years, unit mass had been *the* alloy cylinder locked under three bell jars in an archive in Paris, a

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[1] "Kilogram, Ampere, Kelvin and Mole Redefined: International System of Units Overhauled in Historic Vote," *ScienceDaily*, November 16, 2018, https://www.sciencedaily.com/releases/2018/11/181116115556.htm.

[2] Edwin CartlidgeMar. 1, 2018, and 12:00 Pm, "With Better Atomic Clocks, Scientists Prepare to Redefine the Second," Science | AAAS, February 28, 2018, https://www.sciencemag.org/news/2018/03/better-atomic-clocks-scientists-prepare-redefine-second.

remnant of Europe's first attempt at standardization, an emblem of the conventional basis of all modern, quantitative, and scientific knowledge.[3] The new definition drew an end to defining scientific concepts based on these tangible units. Instead, unit mass is now some function of the Planck constant, which it used to measure, balanced by electromagnetic force that in turn is calculated from the other units, such as voltage and current, derived from natural constants that determine them.

Doesn't the shift from unit-based conception of quantity to law-based or experiment-based conception of measurement that occurred during the 19th century anticipate this new way of thinking about measurement? The term "quantity" disappeared from conceptions of measurement because it has largely served to project a uniform structure onto concepts that are already measurable by well-established methods. The well-defined quantitative concepts thus reap all the benefits of the arithmetical number, including divisibility and additivity. However, the conjoining of, or the comparison with, units does not explain how concepts become measurable in the first place, neither does it distinguish different ways of quantification. The unit-based conception of quantity owed much of its longevity to the "self-evident" quantitative structure of length and time, and to the use of geometrical representation in natural philosophy. It led natural philosophers to treat all quantitative concepts through terms like unit, homogeneity, plurality, divisibility, and so on, as if underneath each concept there were some "bearer of magnitude" mirroring the kind of structure outlined in Euclid's Book V on the theory of proportion. But measurement is a much more complicated conceptual and practical process.

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[3] For instance, Wittgenstein discussed the meter prototype a lot in his Philosophical Investigations. For instance: "there is one thing of which one can say neither that it is one metre long, nor that it is not one metre long, and that is the standard metre in Paris." W. J. Pollock, "Wittgenstein on The Standard Metre," *Philosophical Investigations* 27, no. 2 (April 1, 2004): 148–57.

Wittgenstein compared the unit with rules of grammar or deduction, which were neither true or false. Possibly, Jorge Luis Borges's short story "On Exactitude in Science" also fits here.

What 19^{th} century scientists discovered is precisely measurement as an object of philosophical inquiry and generalization. They became interested in questions such as what in general allows properties and phenomena to be meaningfully expressed by numbers in the first place, and how numbers and scales actually relate to the property and phenomena gauged by them. Perhaps this was inevitable, due to the increasingly experimental nature of the exact sciences. Through interrogating the specific intellectual or practical activities involved in the process of measurement, they exposed the unspoken and unjustifiable assumptions concerning the unit-based conception of quantity. The focus on how measurement process itself constitutes the meaning of quantitative concepts paved way for the reconception of the measurement of space and time.

Overall, developments in late 19th century sciences paved way for the collapse of paradigms in many realms. Among them, the paradigm of quantity based on geometrical magnitudes and its concatenation procedure is a crucial one. The intellectual developments I examine in my dissertation formed the basis of many 20^{th} century themes that are commonly associated with revolutionary ideas concerning the nature of measurement. As a historical investigation, my dissertation shows the productivity of cross-disciplinary conversations in spawning new paths in individual sciences. Philosophically, it challenges the conception that nature is inherently mathematical, by showing how quantitative methods have expanded into different realms of inquiry by design and argument, and by the invention of new techniques and justifications for relating numbers or mathematical tools to the empirical world. This process calls for a constant revision in how we conceptualize the process of measurement, as the historical figures in my narrative strove to do.

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