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The Epistemic Privilege of Measurement: Motivating a Functionalist Account

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Abstract

Philosophers and metrologists have refuted the view that measurement's epistemic privilege in scientific practice is explained by its theory neutrality. Rather, they now explicitly appeal to the role that theories play in measurement. I formulate a challenge for this view: Scientists sometimes ascribe epistemic privilege to measurements even if they lack a shared theory about their target quantity, which I illustrate through a case study from early geodesy. Drawing on that case, I argue that the epistemic privilege of measurement can precede shared background theory and is better explained by its pretheoretic function in enabling a distinctive kind of inquiry.

1. Introduction

Philosophical interest in measurement has long been driven by the idea that measurement operations provide scientific concepts with theory-neutral meaning or evidence. This situation changed quite drastically over the last decades. The role of theory in measurement has been one, if not *the*, dominant theme in recent metrological and philosophical work on the subject. Identifying which inferences qualify as measurements, we are told, is "theory-dependent" (van Fraassen 2012; Giordani and Mari 2021), as is the separation between confounding factors and the correct magnitude of a quantity under measurement (Tal 2019). While this work has led to a better understanding of measurement practice, it almost exclusively draws from examples in contemporary metrology. As a consequence, our existing view on measurement is overwhelmingly based on studies of long-established measures within securely established theoretical frameworks (e.g., Tal 2016; Mari et al. 2017).

One of the key questions that theory-dependent accounts of measurements have to answer is why measurement is assigned a special epistemic status in scientific practice—a privilege that has traditionally been linked to its theory neutrality (e.g., Bridgman 1927; Campbell 1928). Throughout the last decade, metrologists Luca Mari, Paolo Carbone, Dario Petri, and philosopher Alessandro Giordani have worked out a theory-dependent epistemology of measurement to fulfil that warrant. In their *structural* account, the epistemic status of measurement is explained by scientists' agreement on specific models and general theoretical assumptions, which allows for "object-oriented" and "subject-independent" inferences (Mari et al. 2017, 55). In what follows, I identify a challenge for that account: Measurement can be pursued in the absence of agreement on a general theory. I motivate that challenge based on a case study of the measurement of planetary figures in early physical geodesy. Geodesists did not have recourse to a shared general theory of their target quantity, from which I conclude that the structural account systematically *overestimates* the role of theory in measurement. While my ostensive target is Mari et al.'s account, I provide some reasons that my argument also has important implications for similar proposals put forward by Eran Tal and Bas van Fraassen.

In response to the problems faced by the structural account, I argue that measurement's special status results from the specific commitments held by measurement agents and their *function* in enabling a distinctive kind of inquiry. I refer to these as *stability* and *nomicity*, denoting agents' commitments to establishing the stability of outcomes under similar conditions and their lawful variation across conditions.

2. The epistemic privilege of measurement

Measurement is generally assigned a privileged role in scientific practice. The *outcomes* of measurement inferences are taken to (at least loosely) constrain new theorizing. This is not an observation about the facts explaining the epistemic status of a particular measurement, but the general importance that is attributed to measurement as a *practice*. Recent work in the philosophy has rejected traditional views that linked this privilege to measurement's theory neutrality (e.g., Bridgman 1927; Campbell 1928). This rejection is motivated by two related observations on the theory dependence of measurement (van Fraassen 2012; Tal 2019; Giordani and Mari 2021):

1. Sophisticated measurement procedures rely on theoretical models of the link between *measurement indicators* and a quantitative *measurement outcome*.
2. Scientists ascribe particularly high reliability to measurement outcomes if several different theories are involved in the measurement process.

Because these two observations undermine theory neutrality, advocates of theory dependence need to offer an alternative explanation of measurement's epistemic privilege. The most sophisticated explanation of that sort is offered by Luca Mari, Paolo Carbone, Alessandro Giordani, and Dario Petri. Their "structural interpretation" asserts that measurement is best understood as a tripartite *inferential activity*, whose structure individuates it from other forms of inference. This claim is supported by the observation that contemporary measurements combine controlled manipulations of measurement indications with several different levels of theoretical framing: (1) a general theoretical model of the target property, (2) a specific theoretical model of its instantiation in a particular object, and (3) a theoretical model of how that object is nomically connected to measurement instruments (Figure 1).

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Model Type	Description
General Model	Quantitative theoretical characterization of **target property** in terms of **other quantities**, derived from **general theoretical laws**
Specific Model	Quantitative description of an **instantiation of that property** in one or multiple **specific objects of interest**
Model of Measurement Process	Description of the interaction between the **specific model's parameters** and one or many **measurement instruments,** theoretically identifying and anticipating **possible sources of error** and (potentially) **quantifying uncertainty**

Figure 1. Illustration of Mari et al.'s structural interpretation of measurement.

These models are used to complete a holistic measurement task: determining the magnitude of a particular instance of a target property. The structural interpretation explains the epistemic privilege of measurement in virtue of the sophisticated structure of this inferential process. The abstraction of the general model allows scientists to define a unit that transcends local context, guaranteeing "subject-independence." The specific and measurement process models, in turn, facilitate measurement's sensitivity to the particular instantiation of the quantity in question—guaranteeing "object-orientedness" (Mari et al. 2017, 65). In jointly fulfilling these two criteria, measurement is individuated from other forms of inference such as observation or simulation.

In what follows, I will focus on the structural interpretation when discussing the epistemic privilege of measurement. In some sense, this is an undetermined choice, which I justify by the fact that Mari et al. offer the most detailed account of the inferential structure measurement. Because this structure is the explanans of measurement's epistemic privilege, I take the structural explanation to be the *strongest* explanation. Moreover, other explanations bear close similarity to their arguments. Eran Tal argues that measurement's objectivity is a result of its *robustness*, where the latter is guaranteed by metrologists successfully linking an abstract theoretical model of a quantity to different modeling assumptions about specific measurement processes (Tal 2017). Bas van Fraassen, similarly, argues that measurements uniquely fulfil their privileged function in "empirically grounding" an abstract quantity in virtue of general theoretical assumptions about what counts as a measurement of that quantity and specific modeling assumptions about how confounding distortions can be predicted (van Fraassen 2012).

3. Measuring planetary figures in early geodesy

In the previous sections, I have outlined how metrologists and philosophers explain the epistemic privilege of measurements, focusing particularly on Mari et al.'s structural interpretation. In what follows, I investigate a historical case study in which a crucial aspect of their explanation is not realized—namely, a shared general theoretical framework to characterize the target quantity. The historical case is offered by early geodetic measurements of the earth's ellipticity, the defining

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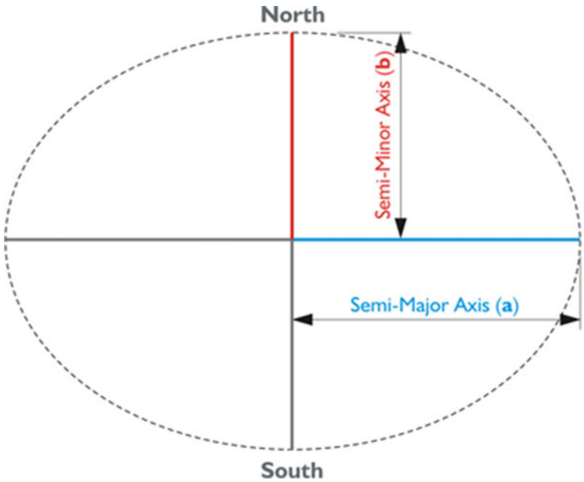

Figure 2. Meridian ellipse of an ellipsoid of revolution, where a and b are parameters in terms of which polar flattening ($f = \frac{a-b}{a}$) is defined.

quantity of the earth's general figure. These measurements began after scientists had observed that surface gravity varies with latitude, indicating that the earth might not be a sphere. In response to this, they tried to determine the degree to which the earth deviates from a spherical shape by being prolonged or prolate—a quantity that can be geometrically represented as an ellipsoid's ellipticity (Figure 2). The focus of what follows is not on whether early geodesists established a unique value for the earth's ellipticity—which they did not—but on the kind of inquiry that was possible in the absence of theoretical agreement.

3.1 Newton

In his *Principia*, Isaac Newton proposed a novel theory of planetary figures, from which he derived the first proposal for the ellipticity of the earth. This theory was deeply embedded in his theory of universal gravitation. Because Newton took the theory of gravitation to generalize to the mutual attraction between *all* celestial and terrestrial bodies, he conjectured that the same force that keeps the pendulum swinging isochronously and planets moving in their orbits also holds the key to understanding the earth's physical formation. Establishing *how* universal gravitation could affect the formation of planets formed the core of Newton's novel theory of the earth's figure. Put very roughly, that theory treated planets as equilibrium figures of rotating fluids, with their shapes being jointly determined by their initial motion, initial density distribution, and a theorized force that acts regularly between all of their constituent particles. Newton concluded that the earth, modeled as a homogenous fluid that rotates with uniform angular velocity, must have the shape of an oblate ellipsoid with an ellipticity of 3/692 to be in a state of hydrostatic equilibrium. Corresponding to the resulting variation of surface curvature with latitude, he then predicted the effective surface gravity—indicated by the length of an isochronous one-second-pendulum—to vary as the square of the sine of the latitude (Newton 1999, 828).

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Newton supported these claims by citing measurements of the variation of surface gravity conducted by several French physicists in the 1670s. Jean Richer first measured such a variation during an expedition for the Paris Académie. From 1671 to 1673, he monitored the daily swings of a pendulum clock. While the clock was closely calibrated to astronomical time in Paris, he found it losing an average of 2 1/2 minutes every day in Cayenne (Richer 1679). In 1682, Jean Varin, Jean Des Hayes, and Guillaume De Glos replicated this basic result in Gorée (off the west coast of Africa) and Guadaloupe (West Indies) but recorded a different variation with latitude (Académie royale des sciences 1703, 357–58).

3.2 Huygens

After reading the first edition of Newton's *Principia*, Christiaan Huygens worked out an alternative treatment of the problem, adding new impulses to Newton's results and challenging the empirical support that they provided for universal gravitation. Like many of his contemporaries, he took the idea that all particles of matter attract each other to be absurd, arguing that gravitational force varies as a function of distance to the center of planets. His work on the matter was published in 1690 as *Discours de la cause de la pesanteur*. Huygens used the problem of the earth figure to level an empirical attack on universal gravitation. Newton's theory would then not only be unintelligible but also fail short of explaining one of its explicitly associated phenomena. To show this, Huygens derived the equilibrium figures of fluids subject to alternative gravitational forces and tried to show that they account better for actual measurement outcomes. Huygens concluded that under inverse-square and linear variation of gravity with the distance to the center of planets, a rotating fluid is in hydrostatic equilibrium if its ellipticity is 1/578 and 1/579, implying that the earth is less oblate than Newton had assumed (Huygens 1690, 156).

While Huygens did not provide new empirical arguments *in favor* of his model, he did offer measurement results that seemed to speak *against* Newton's results. Just a few years before writing his *Discourse*, he had come up with a trial for how well his pendulum clocks might aid nautical navigation. In 1686, the ship *Alcmaer* returned from the Cape of Good Hope to the Dutch island Texel, carrying with it two specifically designed exemplars of Huygens's pendulum clocks. If the pendulum clock should help in finding longitude at sea, it needed to be corrected according to a theoretical model of the earth's figure and the latitudinal variations in the effective surface gravity on such a figure. The performance of different models in this task, therefore, offered a test of their accuracy. As Eric Schliesser and George Smith have reconstructed, this test relied on two astronomical determinations of the longitude at the Cape of Good Hope and Texel. After the *Alcmaer* had returned to Texel, Huygens assessed how much the longitude indicated by the pendulum clock needed to be corrected for it to match Texel's astronomical latitude. Huygens noted that the only corrections required were due to the increase of the centrifugal effect with latitude, leading to longitude values accurate up to "5 to 6 [nautical] miles" (Schliesser and Smith 2000, 16).

Huygens's result implies that Newton's predictions—assuming universal gravitation—led to noticeably wrong determinations of longitude, as the earth would be more flattened at the poles and surface gravity would vary more noticeably with latitude. Notably, Huygens also explained why previous measurements seemed to support the Newtonian prediction. During his experiments with constrained pendulums, he realized that the bob only remains isochronous if its arc is kept sufficiently small. Huygens had advised the Alcmaer mariners to "make the pendulum move ... roughly just 2 or 3 thumb-widths" (Huygens 1686).

3.3 The Cassinis

Both Huygens and Newton supported their theoretical claims by appealing to pendulum measurements of surface gravity variations with latitude. The stipulated link to the earth's figure was based on theories of gravitation. In light of their skepticism toward all such theories Giovanni Domenico Cassini, head of the Paris observatory, and his son Jacques carried out a much less theoretical measurement of the earth's figure from the 1690s onward—explicitly addressing Huygens and Newton (Cassini 1722, 299). Rather than surface gravities, the Cassinis measured the latitudinal variation of the length of an equal arc of a meridian.

They conducted a triangulation survey with calibrated rods and octant and sextant telescopes, the cutting edge of contemporary precision measurement (ibid., 51–61). The Cassinis also corrected for potential errors introduced by altitude variations across the triangulation network by geometrically reducing all stations to sea level. While they measured the altitude of the stations optically, they controlled these results with barometer measurements (ibid., 135–55). They found the meridian's northern section to be 97 toises shorter than the southern section, which the arcs measuring 56960 and 57057 toises. Generalizing this variation gives an ellipticity of –1/95. Contra Huygens and Newton, this seemed to suggest that the earth is an oblong ellipsoid, with an equatorial diameter that is shorter than its polar axis.

In 1720, Jean-Jacques de Mairan also tried to reconcile the existing pendulum measurements with the Cassinis's result. Without invoking any particular theory of gravitation, he defined an empirical "law" of terrestrial gravity that describes the relation between measured effective surface gravity and latitude on Cassini's oblate spheroid model. He argued the only empirically justified claim about terrestrial gravity was that it increases as a function of surface curvature (Mairan 1720, 252). Hence, Huygens's and Newton's measurement results were subject to theoretical errors, introduced by relying on theories of planetary equilibrium figures that postulated an unsubstantiated link between a gravitational force and the earth's ellipticity.

3.4 The Newtonian response

In the second and third editions of the *Principia* (1713 and 1724), Newton explicitly responded to the alternative measurements of the earth's ellipticity. A striking and, so far, unappreciated novelty in the revised propositions 19 and 20 is their explicit appeal to measurement error. Newton offered a reworked analysis of pendulum data, estimating the respective reliability of the available results in light of possible errors. He argued that Jean Richer's measurement results were most trustworthy, appealing to the duration of the observation's series. Newton noted, moreover, that Richer's result fits exactly with his own prediction once it was duly corrected for thermal expansion.

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Given that Richer also replicated his measurement multiple times and had used the same clock in Paris and Cayenne, Newton argued that the agreement with his theoretical prediction was sufficient to favor universal gravitation. Newton also discussed several new and conflicting measurements by other French physicists. He explained these deviations by postulating further errors in relative surface gravity measurements. In its final shape, the *Principia* refers to four different sources of measurement error: "The discrepancy could have arisen partly from (i) [random] errors in observations, partly from (ii) the dissimilitude of the internal parts of the earth and (iii) from the height of mountains, and (iv) partly from the differences in heat of the air" (Newton 1999, 477). In his updated and extended analysis, Newton also responded to the Cassinis' measurement. He offered precise predictions for the length of 1° meridian sections at different latitudes. These predictions correspond to latitudinal distances on the surface of an oblate ellipsoid with an ellipticity of 1/230—a slightly altered value Newton obtained by updating the empirical coefficient for the magnitude of the centrifugal effect at the equator. Contrary to the Cassinis, however, Newton did not base these predictions on arc measurements but simply extrapolated from the gravimetric results.

Newton's protegee John Desaguliers provided a systematic criticism of the Cassinis' results in three papers that were successively published in the Transaction of the Royal Society throughout 1724. He offered a much more extensive error analysis than Newton. The force of Desaguliers's criticism stems from two connected arguments. On a general level, Desaguliers notes the methodological inferiority of comparing directly adjacent meridian sections. The less two arcs differ in latitude, the less they will differ in their length. In a more detailed discussion, he showed that, given their adjacent setup, the Cassinis' measurements are too sensitive to possible errors to support their conclusions. The upshot of Desaguliers's empirical argument was that the geometric leveling is uncertain enough to introduce errors that are larger than the measured length difference between the two arcs (Desaguliers 1724).

4. Measurement without theoretical agreement: Toward a functionalist explanation

Between the 1680s and 1720s, the physical study of the earth's figure began to constitute a cohesive problem, centered around the newly created quantity of planetary ellipticity and two different measurement indicators. Geodesists agreed on what we earlier referred to as the epistemic privilege of measurement: Their mutual engagement and criticism was predicated on the belief that theoretically conceivable models also had to accord with the results of measurements. However, they did not agree on a general theoretical framework to explain their choice of model or assess the relative merit of particular measurement outcomes. As we have seen, attitudes to competing theories of planetary equilibrium figures ranged from aversion (Huygens) and support (Newton, Desaguliers) to qualified agnosticism (Cassini I and II, Mairan). It is striking that early geodesy still experienced a somewhat cumulative and cohesive development, with scientists critically acknowledging each other's measurements and diagnosing potential errors in each other's results.

Returning to the structural explanation of measurement's epistemic privilege, we can immediately recognize that geodesists lacked what Mari et al. call the "general model" of the target property or general theoretical laws from which such a model could be derived (Figure 1). The agreement was limited to the ellipsoid as a specific model of the earth, with (1) surface curvature and (2) gravitational acceleration defined as functions of its latitudinal coordinates. To carry out measurements of planetary ellipticity—geodesists' target quantity—these two quantities (1) and (2) were linked to two accessible quantitative indicators, namely the latitudinal variations in the length of meridional arcs and a one-second-pendulum. As there was no agreement on a general background theory, the exact nature of the theoretical link between the pendulum length and the earth's ellipticity was not generally agreed upon either. The postulated theoretical links from (1) and (2) to pendulum and meridional arc lengths correspond to the basic constituents of what Mari et al. call the model of the measurement process. Hence, early geodesy fails the structural criteria for measurement, not even qualifying as a "candidate measure" (Mari et al. 2017, 55).

While geodesists did not agree on a general theory, they agreed on the ellipsoid as a stipulative model for their specific target quantity: the ellipticity of the earth. Newton and Huygens had different physical arguments for the adequacy of that model and the Cassinis and Mairan accepted it on purely pragmatic grounds—that is, as an empirically useful characterization of the earth's shape. Nonetheless, geodesists agreed that they were engaging in a common measurement problem and that pendulum and arc measurements would be needed to provide evidence for the correctness of alternative background theories as well the magnitude of the model's ellipticity. Most importantly, they were having an intensive discussion about potential sources of measurement *errors*.

Treating significant discrepancies in the inferences from directly accessible indicators to the relevant parameter of their quantity model as errors implies two epistemic commitments on the side of measurement agents. First, they have to be committed to the assumption that measurements of their target property should have the same outcome under the same conditions—a commitment that Hasok Chang dubs the "principle of single value" (Chang 2004). Because I agree with Chang's later assessment that the principle represents an assumption to which agents *commit* by engaging in a certain practice, I will refer to it as *stability commitment*. Building on this basic commitment, measurement agents further need to believe that any variations between several outcomes measured under different conditions should either be (1) lawful, that is, in accord with the modeled relation between indicators and target property or (2) attributable to some external distortion. Call this the *nomicity commitment*. A simple and paradigmatic illustration of lawful variability is found in fulcrum balances, whose use in measuring weight commits agents to Archimedes's law of the lever. As the formal work on uniqueness by Patrick Suppes, Amon Tversky, David Krantz, and Duncan Luce has established, modeling a target attribute with more or less mathematical structure changes the nomic constrains on appropriate measurement systems (Krantz et al. 2006).

Because measurement involves such distinct commitments from its participating agents, treating a system as measurable marks an active choice to participate in a distinctive kind of norm-governed research. In the initial stages of inquiry, a specific measurement problem can be pursued without a shared general model. Yet, it can still be justified by virtue of its *function* in enabling an inquiry with a particular epistemic structure, thereby establishing epistemic coordination between scientists. To

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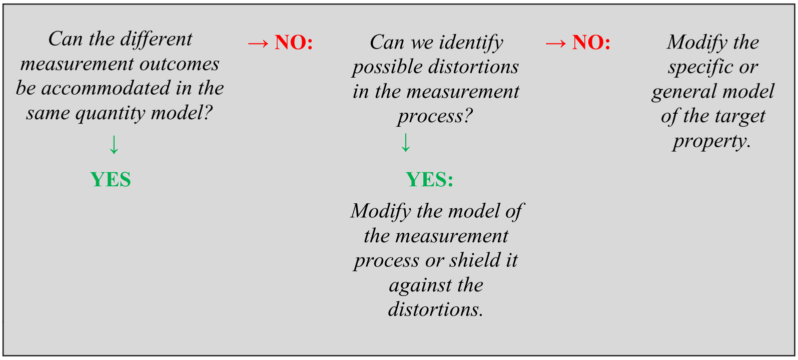

Figure 3. Simple sketch of the epistemic structure of a measurement problem.

understand what this epistemic coordination looks like, we must look at the kinds of inference that are licensed by the stability and nomicity commitments. As we have seen, these two commitments allow agents to identify *errors* in numerical discordant measurements of the modeled property, which can then be traced back to an insufficient ability to (1) model the target system or (2) detect and anticipate distorting influences (Figure 3). If a group of scientists engages in a measurement problem, they thus adopt a template for identifying discrepancies and anticipating their sources in theoretical mistakes or unaccounted distortions. Thus, after the first pendulum experiments and Newton's initial theoretical work in the *Principia*, we can observe an evolving and interactive commentary on measurement errors. In his reflection on the voyage experiment, Huygens's implied that earlier pendulum measurements were unreliable because researchers did not sufficiently constrain the arc of the pendulum. Initially, Newton reacted to the extant pendulum data by correcting his model of the earth's internal density distribution, later by using experiments with thermal expansion to correct Richer's value. Cassini and Mairan, in turn, accused Newton and Huygens of introducing errors by relying on unconfirmed theoretical assumptions about gravitation. As Desaguliers's work exemplifies, participants in a measurement problem can also employ the inverse strategy and use theoretical assumptions about external distortions to call into question particular measurement outcomes. Thus, when Desaguliers systematically developed Newton's criticism of Cassini's arc measurement, he estimated its sensitivity to known distortions (irregular altitude), undermining its inferential link to their model's ellipticity and, ipso facto, the physical earth's figure.

Taking stock, we can note that geodesists organized their collective inquiry around a measurement problem, implicitly subscribing to what I dubbed the epistemic privilege of measurement. Rather than embedding their measurements in a shared theoretical framework, they constrained their theorizing according to their ability to identify and explain measurement errors. In the absence of a shared background theory, I have traced back the ability to identify measurement discrepancies as errors to two implicit commitments to the stability and nomicity of measurement. While the structural interpretation assumes that measurement needs general theoretical frameworks to attain a privileged epistemic function in science, my historical analysis

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challenges this assumption. The practice of measurement is rooted in the commitment of epistemic agents to stabilize (stability) and control (nomicity) the inferential relations between models and directly accessible indicators of a target system. These commitments have an autonomous *function* in rendering empirical discrepancies as measurement errors, permitting a self-corrective inquiry into external distortions and shortcomings of modeling assumptions. It has been widely noted that such iterative revisions of measures and models can drive the subsequent construction of general theories (Chang 2004). Hence, measurement's epistemic function in licensing a distinctive form of inquiry offers a better and more general explanation of its epistemic privilege.

While my view breaks with the dominant explanations of measurement's epistemic privilege, it should be read as further specification, rather than a refutation of recent epistemological work on measurement. It is one of the key assumptions in recent studies that "measurement is a quasi-autonomous activity" deserving of an internal epistemology (Mitchell et al. 2017)—an assumption very much at the heart of Mari et al.'s account of measurement's general inferential structure. Rather than rejecting this line of thought, I propose to further *extend* it to cases without secure theoretical foundations. Even quantity models without unequivocal theoretical support can facilitate the autonomous kind of inquiry we call measurement. Artificially restricting measurement to theoretically secure cases risks misunderstanding the basic position measurement occupies in the general architecture of scientific inference.

5. Conclusion

Recent explanations of measurement epistemic privilege overestimate the importance of scientists' agreement on general theories for characterizing target quantities. Using a case study from early physical geodesy, I have shown that measurement can occupy its privileged role in scientific inquiry even in the absence of agreement on such theories. Instead, measurements can be established based on stipulative and contextually specific quantity models and agents' commitment to the stability and nomicity of the inferential link between modeled quantities and their measurement indicators. Because these commitments license the identification of measurement errors in numerical discrepancies, the explanation of such errors allows scientists to subsequently articulate and test theories.

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