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MEASURABILITY AND PHYSICAL LAWS

ABSTRACT. I propose and motivate a new account of fundamental physical laws, the Measurability Account of Laws (MAL). This account has a distinctive logical form, in that it takes the primary nomological concept to be that of a law relative to a given theory, and defines a law simpliciter as a law relative to some true theory. What makes a proposition a law relative to a theory is that it plays an indispensable role in demonstrating that some quantity posited by that theory is measurable. In Section 1, I motivate the project of seeking a philosophical account of fundamental physical laws, as opposed to laws of nature in general. In Section 2, I motivate seeking an account with the distinctive logical form of the MAL. In Section 3, I present the MAL and illustrate the way it works by applying it to a simple example.

This paper has three parts. In the second part, I will discuss a novel logical form that a philosophical account of laws of nature might take, which I'll call the *meta-theoretic form*. In the third part, I will sketch and motivate a new account of laws – which I'll call the *Measurability Account of Laws* – that takes the meta-theoretic form. Accounts of meta-theoretic form are more plausible when considered as accounts of physical laws than as accounts of laws as found in all the sciences. In fact, they are only really plausible as accounts of laws found in *fundamental* physics, as opposed to phenomenological physics. I think that's all right; my primary goal here is to give a good account of what fundamental physical laws are. But that's an idiosyncratic goal to have. Most philosophers who take laws of nature seriously think that laws are found across the sciences (or at least across the natural sciences) and that an important desideratum for an account of laws is that it give a *unified* explication of laws as they are found in all branches of science. In the first part of the paper, I'll explain why I pursue this idiosyncratic goal.

1. WHY FOCUS ON FUNDAMENTAL PHYSICS?

My ultimate aim here is to give an account of what fundamental physical laws are. So, what do I mean by "fundamental physical law"? Eventually I'm going to answer that question, but here at the beginning, all I can do is present a set of paradigm cases that bear a family resemblance. Fundamen-

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tal physical laws include things like Newton's Laws of Motion, Newton's Law of Universal Gravitation, Coulomb's Law, Maxwell's Equations, and Einstein's Field Equations. Or rather, those are all *putative* fundamental physical laws; each of those *would be* a fundamental physical law if the theory to which it belongs were true. In general, fundamental physical laws are the things that foundational theories in physics – such as classical mechanics, quantum mechanics, and general relativity – are trying to state. That's what I want to give an account of.

Much philosophical work on laws of nature (e.g., Armstrong 1983; Carroll 1994) starts out by assuming that we have a very general concept of a law of nature, independent of the details of any particular theory or branch of science, and then tries to explicate this very general concept. Before one can begin to explicate this general concept, though, one needs an initial characterization of it, that makes it reasonably clear what the target is. Most philosophers start with something more or less like what I'll call the Common Philosophical Characterization, or CPC:

CPC: A law of nature is a true, (logically and mathematically) contingent generalization that supports counterfactuals, plays an important role in scientific explanation, and can be inductively confirmed by its instances.

Presumably, all of the sciences are concerned with counterfactuals, explanations, and inductively confirmable generalizations. So if the CPC is what gives us our initial grip on the notion of a law, then surely any satisfactory philosophical account of laws ought to be applicable to any branch of science.

It is a familiar point that the CPC is not something that one can just read off of scientific practice. If it were, then it ought to match, fairly closely, the way scientists use the label "law". But it doesn't. Many scientific claims that seem to satisfy the CPC are not customarily referred to by scientists as laws – for example, correct formulas of stoichiometry. Also, many claims that are commonly referred to as laws in scientific discourse do not satisfy the CPC – for example, the Hardy-Weinberg Law and the Laws of Large Numbers, which are all theorems of statistics and so are not mathematically contingent. The CPC is thus a stipulative way of fixing a usage for the term "law of nature". There's nothing wrong with that, of course; scientists' usage of the word "law" does not appear to be uniform or principled, so if we want to talk about laws in a way that will lend itself to interesting philosophical theorizing, then we have to stipulate a use or sense of the word "law".

But we should note that if we use the CPC to fix our usage of the term "law", then we have no a priori guarantee that there is anything interesting to say about all laws that goes beyond the CPC itself. To speak loosely, we have no a priori guarantee that the CPC picks out a natural kind, or carves nature at a joint. If counterfactual discourse, explanatory discourse, and the canons of confirmation are as context-sensitive as many philosophers take them to be, then it may be that the kind of thing that satisfies the CPC varies widely from one scientific context to another.

Another fact we should notice is that in most branches of science (as opposed to most branches of philosophy of science), the term "law" doesn't seem to carry much weight. Biologists and psychologists occasionally apply the tag "law" to something, but they don't generally get too worked up about whether something should be called a law or not; nothing of importance seems to turn on whether we use the L-word. This has prompted some philosophers, notably van Fraassen (1989, 8) to suggest that the concept of a law is just a plaything of philosophers, a metaphysical holdover from a bygone era, with no real relevance for contemporary science. However, it is a striking but seldom-noted fact that things are different in fundamental physics. Fundamental physical laws, or rather, *putative* fundamental physical laws, have a strikingly large amount in common with each other, even when we look across theories that differ dramatically in their content. This makes it inviting to think that those laws do form a natural kind. More importantly, the concept of a fundamental law does real work in physics, of a kind that would not necessarily be done by just anything that satisfied the CPC, and of a kind that is not obviously done by the putative laws of the other sciences. So in the case of fundamental physical laws, we have good reason to believe that there is a privileged class of propositions, playing a special role in scientific practice, that physicists pick out using the term "laws", without having to make a stipulation like the CPC. Hence, it seems that in fundamental physics, there is a specific law-concept in play, that may or may not also be in play in the other sciences, that it would be nice to have a philosophical account of.

To make good on my claims, I need to identify the commonalities that I claim ought to lead us to believe that the fundamental physical laws form a unified, well-defined kind. We can see the special work done by the concept of a law in fundamental physics in at least two places. The first is in the importance that physicists place on symmetry principles, including relativity principles. A symmetry principle is a principle that quantifies over fundamental laws; it is a principle that says that *all of the fundamental laws* are invariant under some class of transformations. It doesn't say that *everything* is invariant under those transformations: A theory can make particular claims about nomologically contingent facts that are not preserved under the symmetries – for example, a cosmological model can be asymmetric with respect to time reversal, even if time-reversal is a symmetry transformation of the laws. So the concept of a symmetry transformation presupposes the concept of a fundamental physical law. Symmetry principles, in turn, are useful e.g., in classifying quantities as observable or unobservable: In Newtonian mechanics, absolute velocity is not an observable or measurable quantity precisely because absolute velocity is not invariant under the symmetries of the theory. Thus the practice of physics assigns special work to the concept of a fundamental physical law. (Moreover, here we see a hint that the concept of a fundamental law has something important to do with the concept of an observable or measurable quantity, an idea that will come up again below.)

A second place where we can see special work done by the concept of a fundamental physical law is in statistical mechanics. Typical statistical-mechanical explanations purport to explain laws or regularities by showing that almost all dynamically possible micro-trajectories conform to them. "Dynamically possible micro-trajectories" does not, of course, mean *logically* possible micro-trajectories. Rather, it picks out those possible micro-trajectories that conform to some set of laws. But this set of laws cannot include the very laws (or regularities) that statistical mechanics is used to explain, on pain of circularity. Hence, it cannot include the laws of phenomenological thermodynamics, or any other laws or regularities that depend on them (and this includes most of the putatively law-like regularities in biology and earth science, and many of those in chemistry and phenomenological physics). So the laws in terms of which the dynamically possible micro-trajectories relevant to statistical-mechanical explanations are defined are apparently fundamental physical laws; at any rate, they are certainly more fundamental than any laws that can be explained using statistical mechanics.

Moreover, putative fundamental physical laws show striking similarities of mathematical form. Typical laws from fundamental physical theories take one of three forms: (1) Differential equations that give rise to well-posed boundary-value problems (e.g., Newton's second law of motion, Maxwell's equations, Einstein's field equations); (2) equations from which such differential equations can be derived, and whose primary use is the derivation of such equations (e.g., Newton's law of universal gravitation, the Lorentz force law); and (3) equations that are mathematical consequences of such differential equations (such as conservation laws, which are typically derived by integrating differential equations, yielding constants of the motion which are the conserved quantities). This is so in theories as diverse as Newtonian mechanics, general relativity, and quantum mechanics. Furthermore, these laws play a characteristic role in the solution of practical problems. They are used to set up well-posed boundary-value problems, where the boundary values may be given by results of measurement or preparation. The solution of the resulting boundary-value problem is the better part of the mathematical work that needs to be done in typical applications of physical theories. Again, this feature remains constant across theories that are quite diverse with respect to their substantive content; we find it in quantum mechanics as well as classical mechanics and classical electrodynamics, though the forms of the boundary-value problems are rather different in different theories.

The common features of putative fundamental physical laws, and the special work done by the concept of a fundamental physical law, appear to be constant across otherwise diverse physical theories, but they do not seem to extend to all of the putative laws found in science. Moreover, it seems clear that the CPC does not entail that, or explain why, laws should have the special features that fundamental physical laws do. For this reason, it seems that there is specific law-concept at work in fundamental physics that is worthy of philosophical attention, though it is far from clear that this concept must have any role to play in other sciences. This justifies seeking an account of laws that is tailor-made to fit fundamental physical laws. In what follows, whenever I refer to laws, it is this more specific kind of law that I have in mind.

2. META-THEORETIC ACCOUNTS OF LAWS

Here is one form that a philosophical account of laws of nature could take:

(1) P is a *law relative to theory T* iff P is implied by T and plays role R within T.

(2) P is a *law of nature* iff it is a law relative to some true theory.

"Theory" here means *scientific* theory. A "law relative to a theory T" means something that would be a law of nature if T were true, and it is presupposed that the role R can be explicated independently of the concept of a law. On any account of this form, the concept of a law relative to a theory is the primary nomological concept, and the concept of a law *simpliciter* is derivative. If some proposition P is a law, on such an account, then this is not best understood as an intrinsic feature of P itself; it is best understood as a feature of a theory in which P figures. For this reason, I'll call accounts of this form *meta-theoretic* accounts of laws.

There is an obvious objection to the very idea of a meta-theoretic account: "A meta-theoretic account gets things backwards. For it uses the concept of a *true theory*. But the relevant theories here are theories of fundamental physics; such theories are supposed to tell us what the most basic laws of nature are. So such theories are, or at least contain, sets of claims of the form 'It is a law that P'. The truth condition of such a theory is just the condition that the laws are as the theory says they are. Hence, we can't even understand what it is for such a theory to be true, unless we already have an independent understanding of what a law is. And if we do have such an independent understanding, then any meta-theoretic account is trivial. For the laws relative to theory T just are the propositions that T asserts to be laws. All that a meta-theoretic account can tell us, then, is that P is a law just in case some true theory says that P is a law".

This is a serious objection. But there is a reply to it that not only thwarts the force of the objection but also brings out something that makes the idea of a meta-theoretic account a plausible one. The objection presupposes that we have a theory-independent understanding of the operator "It is a law that _", and that a proposition P is a law relative to a theory just in case that theory says or implies that it is a law that P. This presupposition can be challenged. Consider the case of Molly, a typical competent undergraduate physics student. Last semester, Molly mastered classical mechanics, and this semester she is beginning to study classical electrodynamics. She encounters the chapter of the assigned textbook where it is revealed that Maxwell's Equations are the heart of this theory. The textbook author bluntly presents the four equations, and does not pause to place the operator "It is a law that _" in front of them. If the intuition we are considering is correct, then Molly could justly complain that the author's presentation of the theory is elliptical: "All he has said is that according to classical electrodynamics, these four equations are true. But there are numerous theories that are consistent with that claim. For example, there is the theory that all four of these equations are laws of nature. Then there is the theory according to which the two divergence equations are laws, but the two curl equations are merely accidentally true. Further, there is the theory according to which the only real laws are the Lorentz force law and Newton's laws of motion; the four Maxwell equations are all accidentally true. And so on. Which of these many theories is the theory of classical electrodynamics? Curse this cagey textbook author for not being more explicit!"

But this story does not ring true. If Molly is truly a competent physics student, and she has already achieved fluency with Newtonian mechanics and its laws, she will have no trouble recognizing that Maxwell's Equations are laws of classical electrodynamics. (If there is any doubt about this, consider that it is a truism among philosophers of science that Maxwell's Equations are paradigmatic examples of laws of nature, while it seems doubtful that most philosophers of science have actually checked the standard textbooks on electrodynamics to see whether these equations are explicitly called "laws".)

This cries out for explanation. One possible explanation is that when Molly learns that Maxwell's Equations are all true if classical electrodynamics is, she goes through the following reasoning, perhaps unconsciously: "Suppose classical electrodynamics were true. Then, Maxwell's Equations would all be true. It would then be metaphysically possible for none of them to be laws, or for only some of them to be laws, but the best explanation of the truth of all four would be that all four are laws. Hence, if classical electrodynamics, as presented by this deplorably non-thorough textbook author were true, then all four Maxwell Equations would be laws". That is one explanation of why we have no trouble reaching the conclusion that Maxwell's Equations are laws of classical electrodynamics, even without the benefit of being told explicitly which of its claims are supposed to be laws.

But a simpler explanation is available. It is that when Molly learned classical mechanics, she learned (implicitly) to recognize the special role played in that theory by the propositions that are called the laws of classical mechanics. Even if she could not give an explicit, detailed account of what that role is, she grasps it well enough to recognize when the same role is being played by some other equation in some other context. So, when she learned the theory of classical electrodynamics, she didn't need to be told which propositions were its laws – she could see for herself which propositions play the same role, within classical electrodynamics, that the laws of classical mechanics play within classical mechanics. And in general, the laws of a physical theory are not the propositions to which the theory appends the operator "It is a law that _", but rather the propositions of that theory that play a certain role within the theory. This is a role that one can acquire the practical ability to recognize by studying physics, but to give an explicit analysis of it would perhaps require a lot of work.

Summing up: If one accepts a meta-theoretic account of laws, then a statement of a theory that does not explicitly flag the laws of that theory is not elliptical, but if one rejects all such accounts, then it is. Whichever view you take, you must give some account of why it is that once one has mastered one physical theory and the role played within it by its laws, one can usually tell, just by looking at a non-nomic presentation of another physical theory, what its laws are. On the meta-theoretic view, this is easily explained in a way that is true to the phenomenology: Once one has experience with one physical theory, one can, so to speak, "just see" what the laws of a new theory are, by recognizing an analogy or family resemblance between certain propositions of this new theory and the laws of the theories one already knows. If one rejects meta-theoretic accounts, then one has to explain this phenomenon by positing that a sophisticated, unconscious epistemic inference is taking place. This, I suggest, is a more complicated explanation, and one less true to the phenomenology. This is certainly not a knock-down argument in favor of a meta-theoretic account of laws. But, I suggest, it does deflect the force of the objection, while providing an intuitive motivation for meta-theoretic accounts.

On a meta-theoretic view of fundamental physical laws, there is a more illuminating way to define "fundamental physical theory" than as a theory which purports to state the fundamental physical laws, and a more illuminating way to define "fundamental physical law" than as a law posited by a fundamental physical theory. For a meta-theoretic account gives us an independent standpoint from which to give a non-circular characterization of both of these notions: Some, but not all, scientific theories contain certain propositions that play a certain, special role within those theories. The propositions in a theory that play that role within that theory are called the *fundamental laws* of that theory. A theory all of whose propositions are laws of it, is a *fundamental physical theory*. This allows us to distinguish between mixed-level theories – such as the complete theory presented in Newton's *Principia*, which includes fundamental laws as well as nomically contingent information about the structure of the solar system – from theories that belong wholly to fundamental physics – such as the theoretical core of Book I of *Principia*, consisting of the three laws of motion and the law of universal gravitation. The former *contains* a theory of fundamental physics, but only the latter *is* a theory of fundamental physics, and all of its claims are (putative) fundamental physical laws.

3. THE MEASURABILITY ACCOUNT OF LAWS

3.1. *Some Motivation*

I've already said a bit about the distinctive role played by fundamental physical laws. For one thing, they often take the form of differential equations that have well-posed boundary-value problems. That suggests the idea of defining the laws of a theory just to be the propositions of that theory that take this mathematical form. But if we accepted this idea, then we would be stuck with the consequence that every true proposition of the right mathematical form would be a fundamental physical law. But surely, there can be propositions of the same mathematical form as the laws that just happen to be true. So if we accepted this idea, we would fail to respect the distinction between laws and contingent regularities. What is distinctive about fundamental laws is not their mathematical form. A more promising idea is that it is something about their function which explains why they typically take the forms they do.

Fundamental physical theories typically make assertions about previously undreamt-of physical quantities: Newton's mechanics introduced the modern concept of mass (as distinguished from weight); classical electrodynamics introduced field strength and displacement current; quantum mechanics introduced spin; and so on. All were unfamiliar quantities that no one ever encountered or observed as such before the introduction of the relevant theory. But the theories themselves provided us with the background knowledge needed to measure these new quantities, by setting up situations in which they are guaranteed to become correlated with old familiar quantities that we already knew how to measure, paradigmatically the displacement of a pointer on a meter. If there are particular theoretical principles that guarantee these correlations, they evidently play a special role in the practice of physics. For the logical positivists, this guarantee was supplied by "correspondence rules" or "coordinative definitions". On some versions of logical positivism, such principles are constitutive of the meanings of theoretical terms; hence they are necessary truths. What I want to try out is the related but distinct idea that the correlations on which measurability depends are guaranteed, not by meaning-constitutive principles, but by laws of nature. This is not to invoke a primitive notion of law. Rather, the proposal is that *what it is* to be a law of a theory is just to play an indispensable role in showing that the theoretical quantities posited by that theory are measurable.

This idea goes partway towards explaining why the laws of fundamental physical theories typically generate systems of differential equations with well-posed boundary-value problems. For, solving a well-posed boundary-value problem is a good way of demonstrating that a theoretical quantity is reliably measurable. Consider a measurement procedure, in which an apparatus A, with pointer variable P, is used to measure a quantity Q. Typically, a measurement procedure requires that certain enabling conditions be satisfied: The apparatus has to be set up in the right way, and the process must be shielded from outside interference in some way, or at least it must be assumed that there is no external interference. To show that the measurement procedure is ideally reliable is to show that if these enabling conditions are met, then the final value of the pointer variable P is a certain invertible function of the initial value of Q. (I'll stick to ideal measurements to save time and complication.) But the details of the way the measurement procedure is set up, the shielding or non-interference conditions, and the final value of the pointer variable constitute a set of boundary conditions. If we can show that the value of Q must be consistent with a solution to a certain boundary-value problem for these boundary conditions, and that this boundary-value problem has a unique solution, in which the final value of P is an invertible function of the initial value of Q, then we can show that we have an ideal, reliable measurement of Q. So if laws are principles from which we can derive systems of differential equations that give rise to well-posed boundary-value problems, then they are well-equipped to do what I am suggesting it is their nature to do, namely, guarantee the measurability of theoretical quantities.

3.2. *A Sketch of a Meta-Theoretic Account of Laws*

Here's a first stab at meta-theoretic account of laws that uses this idea:

(MAL): P is a law relative to T iff: (i) T implies P, (ii) P is logically and mathematically contingent, and (iii) P belongs to the weakest sub-theory of T that implies the measurability of everything that T implies is measurable.

("MAL" stands for "Measurability Account of Laws".) This formulation is designed to ensure that the laws of T include just as much of T's content as is required to guarantee the measurability of every theoretical quantity that T guarantees the measurability of, and no more. (This formulation should be regarded only as a first approximation. Technical refinements may be necessary; they are a topic for future work.)

3.3. *An Example*

In order to illustrate how the MAL works, I'll present a simple example of a theory and apply the MAL to it. The example is a theory called TE ("Toy Example"), which will be formulated in wholly non-nomic terms. The MAL is supposed to be able to take a physical theory formulated wholly in non-nomic terms, and then correctly classify the claims of that theory as laws or non-laws. TE will be developed in such a way that we have clear intuitions about what parts of it ought to count as its laws, and what parts of it ought to count as nomically contingent. This will allow TE to pose a non-trivial test for the MAL – if the MAL is acceptable, then it ought to classify all and only the things that seem intuitively to be laws of TE as laws relative to TE.

To be more specific, TE is essentially a truncated version of classical mechanics, with no forces other than spring forces governed by Hooke's law, with some extra information thrown in that is physically contingent by the lights of classical physics. Intuitively, then, the laws relative to TE ought to be Newton's first two laws of motion and Hooke's law; the extra information ought to be nomically contingent. TE is just a simple toy example, and if the MAL is ultimately to be vindicated, it would have to be shown that it gives the right answers when we apply it to richer examples of real physical theories. I think that that can be shown, but not without a much longer discussion than I have space for here. Though it is a toy example, TE is not a completely arbitrary, made-up example; it's laws are a fragment of a real theory, and the way the MAL works when applied to it provides a nice illustration of the way it works when applied to more richly fleshed-out theories.

I'll define TE by describing its models. These models contain only simple springs and cube-shaped objects. Each cube c has a color as well as a mass m(c). One cube is designated as the standard which defines the unit of mass. Each spring has one end that is fixed; its other end can be attached to a cube. On each cube, one or more forces may be impressed at any time. Each spring s is characterized by a spring constant k(s). In each model, the following axioms are true:

TE1: For every cube c and time t, **F**(c, t) = m(c) **A**(c, t) (where **F**(c, t) and **A**(c, t) are the total impressed force and the acceleration of cube c at time t.)

TE2: For every time t, cube c, and spring s, if c is attached to the free end of s, then s exerts a force on c equal to **Fs**(t) = –k(s) **D**(c, t) (where **D**(c, t) is the displacement of c at time t, relative to the equilibrium position of s, which I won't pause to define.)

TE3: There are no other forces than those mentioned in TE2.

TE4: Each cube is blue iff its mass is 4.

Intuitively, TE1–TE3 are all laws of TE, but TE4 is not. The relevant intuitions here are those pumped by considering the real physical theory to which this one is a crude approximation. TE4, if true, is only an accidental regularity, according to classical mechanics.

A problem must be addressed at this point. In order to apply the MAL to TE, we need to be able to tell what TE entails about what is measurable and what is not. The term "measurable", of course, occurs nowhere in the axioms of TE. So it is hard to see how there could be any such entailments. In many ways, TE differs from real examples of physical theories, but this is not one of them. With the possible exception of quantum mechanics, fundamental physical theories don't explicitly say anything about measurement. So it's hard to see how such theories can imply anything about what is measurable and what is not.

This problem can be overcome. We must supplement TE by adding an axiom concerning what is within the means of human observers. TE is intended as a rough sketch of the classical mechanics of springs; in dealing with springs, we have ways of measuring the positions and accelerations of the bodies involved. These measurements are not all made via "direct observation", but nonetheless we are pretty sure they are reliable and legitimate. Furthermore, it is natural enough to assume that human observers are capable of manipulating the springs, and attaching or detaching objects from their free ends. We need to add to TE an axiom that captures all this:

TE5: Human observers can reliably detect displacements, accelerations, and colors, and they can manipulate cubes and springs, and attach springs to cubes and detach them.

This probably seems like cheating. TE is supposed to represent a part of classical mechanics, the part dealing with springs, and that theory does not include human observers within its subject matter. But adding TE5 is not really a cheat. For we can stipulate that for the purposes of discussing the theory TE, the term "human observers" will be defined functionally: A human observer is just any physical system that can do what a human observer can do. In the case at hand, TE5 seems to capture the relevant things that human observers can do. Subject to this local, stipulative definition of "human observer", TE5 is analytic. So its addition to TE does not stretch that theory out of its proper subject matter, because it adds no substantive content.

It is not obvious that there exist, in any of the models of TE, any physical system that satisfies this functional definition of "human observer". But if we assume that the models of TE represent the physically possible worlds allowed by TE, then there have to be observers in some of the models of TE, if any measurements are going to be possible relative to TE. So we also need to make a further assumption:

A: There exist human observers in some of the models of TE.

A is not an additional axiom of the theory TE. It is, rather, a metatheoretical claim *about* TE. Its content is that among the models of TE, *as* *already defined*, there are some containing physical systems that function just as human observers do. Since there is nothing in any model of TE but cubes and springs, this assumption is a little implausible. (Though it is not a priori false; perhaps one can build a machine that is the functional equivalent of a human observer out of cubes and springs, given enough of them!) But TE is just a toy example; real physical theories, such as the whole of classical mechanics, have much more complex and diverse models, and it is far less implausible that the analogous assumption holds for them. Furthermore, since a fundamental physical theory like classical mechanics purports to be a correct fundamental theory of our world, anyone who seriously accepts classical mechanics is committed to the meta-theoretic assumption that classical mechanics has models that (represent possible worlds that) contain human observers. An analogous commitment seems to be implicitly undertaken by anyone who takes any theory of fundamental physics seriously as an account of the actual world. Again, this discussion of TE is intended only to illustrate, in as simple a way as possible, how the MAL works when applied to real physical theories. So although A is not very plausible itself, it is not unreasonable to pretend that it is true for the sake of the present discussion.

Given A, TE guarantees that all forces, masses of cubes, and spring constants are measurable. Here's a sketch of a proof: Take the cube that defines the unit of mass, attach it to any spring s, displace it, and observe the displacement and acceleration at some later time. The acceleration serves as a pointer variable for a measurement of the force impressed on the cube, and the ratio of the acceleration to the displacement serves as the pointer variable for a measurement of k(s). (This fact depends on all three of TE1, TE2, and TE3.) Once k(s) is measured, any other cube can be attached to s, and the operation can be repeated to measure the mass of this cube and the forces impressed on it.

There is another way of measuring the mass of a cube, however – at least in the case of blue cubes. For TE4 guarantees that the color of a blue cube can serve as the pointer variable in a measurement of its mass, by showing that this mass must be 4. However, TE4 does not thereby count as a law relative to TE. For even if we truncated TE by removing TE4, the procedures just described still suffice to show that the mass of blue cubes is measurable – for they show that the mass of *any* cube is measurable.

On the other hand, there is clearly no way of measuring forces in general, or of measuring spring constants, or of measuring the masses of non-blue cubes, that does not rely on TE1–TE3 in the way the procedures described above do. Hence, each of these propositions counts as a law relative to TE, according to the MAL; if we weakened TE by removing one of these propositions, the remaining theory would not suffice to show the measurability of all the things that are measurable according to TE. TE4 and A are also indispensable in any demonstration that anything is measurable given TE. But the MAL does not count TE4 as a law because, given the local, functional definition of "human observer", it is analytic. It does not count A as a law of TE, because A is not a proposition entailed by TE – it is a meta-theoretical statement about TE.

Hence, the MAL produces the right verdicts in the case of the toy example: It implies that TE1–TE3 are laws of the theory TE, but TE4 is a nomically contingent truth relative to TE4. The toy example is an extremely unrealistic and simplistic theory, of course. But the success of the MAL with respect to it provides hope that there is a way of distinguishing between the laws and the contingent propositions of a physical theory, otherwise than by just looking to see where that theory uses the operator "It is a law that ...". It also affords some hope that the MAL correctly explains what that way is.

4. CONCLUSION

I have explained a new form that a philosophical theory of fundamental laws of nature could take, put forward one particular theory of that form (the MAL), provided some motivation for it, and argued that at least in the case of a simple toy example, it produces the right verdicts. Obviously there's much more to be done by way of defending the MAL. In particular, I haven't addressed the question of whether the MAL can account for the apparent ability of laws to support counterfactuals, or their role in explanation. Those are important topics for future work.

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