Christopher Peacocke

Magnitudes: Metaphysics, Explanation, and Perception

Christopher Peacocke: Columbia University, cp2161@columbia.edu

1 Magnitudes: Exposition of a Realistic Ontology

I am going to argue for a robust realism about magnitudes, as irreducible elements in our ontology. This realistic attitude, I will argue, gives a better metaphysics than the alternatives. It suggests some new options in the philosophy of science. It also provides the materials for a better account of the mind's relation to the world, in particular its perceptual relations.

As a preliminary, let us distinguish between magnitude-types, magnitudes, and magnitude-tropes. Magnitude-types are generic kinds of magnitudes. Distance is one magnitude-type, whose instances are more specific distances, such as the distance measured by one meter, and the distance measured by ten miles. Gravitational mass is another magnitude-type, whose instances include the magnitude measured by one gram, and the magnitude measured by ten pounds weight. Magnitudes come in types. Every magnitude is of some magnitude type. As Frege said, "Something is a magnitude not all by itself, but only insofar as it belongs with other objects of a class which is a domain of magnitudes" - "Etwas ist eine Grösse nicht für sich allein sondern nur, sofern es mit andern Gegenständen einer Klasse angehört, die ein Grössengebiet ist" (1998: §161:159).

If we slice more finely than magnitudes, we reach magnitude-tropes. Just as we distinguish between properties and tropes, between the property of being red and this particular thing's redness, to be distinguished from a different object's redness, so we can equally make the parallel distinction for magnitudes. For some purposes, we may want to distinguish, in the case of two equally long meter rods, between this rod's length and that rod's length. My focus here is going to be mainly on the magnitudes, rather than their types or their tropes.

Magnitudes are apparently involved in many explanations and in many counterfactuals, from the humblest to the most sophisticated science. The shadow is a certain size because the flagpole casting it is a certain height. The avalanche flattened the forest because the avalanche's momentum was above a certain threshold. Ceteris paribus, if the flagpole had been shorter, so would its shadow. If the avalanche had had a much lower momentum, the forest would still be standing; and so on. We should take these appearances at face value. An object or an event's have a certain magnitude can both explain and be explained.

In these explanations and counterfactuals, we cannot replace the reference to magnitudes with reference to the extension of a predicate "has such-and-such magnitude". Suppose the flagpole is 10 meters high. The predicate "is 10 meters high" has an extension, one that includes various buildings, watchtowers, fences. These various objects are completely irrelevant to the explanation of the flagpole's casting a certain length of shadow. The explanation of its casting a shadow of a certain length would be the same even if some of these other objects were not in the extension of "is 10 meters high".

Conversely too: consider a world in which the flagpole and these other objects had all had a different height, so that in these other possible circumstances, "is 11 meters high" had the same extension (E, say) as "is 10 meters high" has in the actual world. In this other world, the magnitude of the flagpole (its 11 meter length) that explains the length of its shadow there would be different. It is irrelevant to that explanation that in the actual world, E is also the extension of "is 10 meters high".

This argument is entirely parallel to the argument that one would give for the conclusion that in explanation by properties and explanation of properties, references to properties cannot be replaced by corresponding references to the extensions of those properties. The same points apply whether extensions are conceived as sets, sets of ordered n-tuples, Fregean courses-of-values, or indeed as Fregean functions from entities of one sort or other to truth-values.

Extensions or anything extensional will also not discriminate between two magnitudes with the same extension. Explanation by something's having a certain mass is different from explanation by something's having a certain electrical charge, even for some magnitude of mass M, all and only the things with mass M have a certain electrical charge. The same goes *pari passu* for which magnitudes are explained, as well as for the magnitudes that do the explaining.

I take all these simple points about the irrelevance of certain objects to the explanations in question to be statements of what we ordinarily know about explanations. They do not involve any commitment to, for instance, a non-Humean account of laws. Any philosophical analysis of laws, explanations, and counterfactuals is going to be plausible only insofar as it preserves these obvious points about the irrelevance of certain objects. Nothing here, or in what follows, is meant to imply that a broadly Humean or Ramseyan treatment of laws is incorrect, or could not accommodate these points.

Peter Railton pointed out to me that the inadequacy of attempts to elucidate explanation by magnitudes in terms of extensions is parallel to the inadequacy of attempts to explain the role of objective probabilities in terms of actual frequency of occurrence of events of a given type.[1] For a good explanation of phenomena involving radioactive decay, we need to recognize objective probabilities not definable in terms of actual frequencies, and we need to formulate laws using those objective probabilities. Actual extensions and actual frequencies are heavily contingent features of the actual world that cannot bear the weight of explanation and its associated counterfactuals.

All this seems, or may seem, completely obvious, so why is it even worth saying? Part of the reason is that writers of several different stripes have attempted to elucidate empirical statements about magnitudes in terms that do not involve any ontology of magnitudes, and have done so in ways that contradict these obvious points. Such attempted elucidations characteristically proceed in stages. Suppose that

(i) an object *x* has a magnitude M of a certain magnitude-type T (such as mass, or distance) that measures *n* in units U.

This, it is said as a first step, can be elucidated simply as

(ii) *x* measures *n* in units U for the type in question.

There is then a fork in the road according as one or another kind of reduction is attempted. Note that in moving to (ii), reference to the magnitude M has already disappeared. (This may be a good example of the dictum that an argument is almost always over in the first few sentences.)

The first kind of reductive explanation of magnitudes is found in an earlier approach, which attempts to explain (ii) in terms of what are essentially evidential relations. Here is a quote from Patrick Suppes' and Joseph Zinnes' paper "Basic Measurement Theory":

> 6. This sample of ferric salt weighs 1.679 grams.
>
> But this statement may be replaced by the statement:

1 In conversation, at the Inter-American Congress of Philosophy, Salvador, Brazil, October 2013.

> 7. The ratio of the mass of this sample of ferric salt to the gram weight of my standard series is 1.679, and the manufacturer of my series has certified that the ratio of my gram weight to the standard kilogram mass of platinum iridium alloy at the International Bureau of Weights and Measures, near Paris, is 0.0010000. (Suppes and Zinnes 1963: 9)

This replacement statement numbered 7 does give evidence for the statement of mass in grams. It is, however, not at all plausible that it specifies what statement 6 means, nor that it specifies how the world has to be for statement 6 to be true. It is clear that the evidence provided by the manufacturer may be misleading; that the object originally used as the standard kilogram mass in Paris might have been filed down by a thief; and so forth. If the evidence can be misleading, then we need an account of what the evidence is evidence *for*. A statement of what is clearly both empirical and contingent evidence cannot provide this.

The second kind of attempt at a reductive approach does not speak in quite that way of evidence, but rather, in this example, it speaks of the place of the sample of ferric salt in a system of relations. This needs a little explanation and exposition. Two elements are involved in this second elucidation of "*x* measures *n* in units U for such-and-such type of magnitude". The first element is the notion of an "extensive system" in the sense of Suppes and Zinnes on a domain of non-numerical objects. The second element is the notion of a numerical extensive system isomorphic to that extensive system.

An extensive system $<A, R, \circledast>$ consists of a domain A, a binary relation on A, and a function $\circledast$ whose domain is ordered pairs of elements from A and whose range is A. Suppes and Zinnes write, by way of illustration:

> If A is a set of weights, then the interpretation of aRb is that either a is less heavy than b or equal in heaviness to b. The interpretation of $a\circledast b$ for weights is simply the weight obtained by combining the two weights a and b, for example, by placing both on the same side of an equal arm balance. (Suppes and Zinnes 1963: 42)

The axioms for being an extensive system, as they note, are similar to those given back in by Hölder (1901). The axioms given by Suppers and Zinnes are:

A1 *If aRb and bRc, then aRc* (transitivity)

A2 $((a\circledast b)\circledast c)R(a\circledast(b\circledast c))$ (associativity)

A3 *If aRb, then* $(a\circledast c)R(c\circledast b)$ (adding the same to each preserves R)

A4 *If not aRb, then there is a c in A such that* $aR(b\circledast c)$ *and* $(b\circledast c)Ra$

A5 *Not* $(a\circledast b)Ra$ (magnitudes corresponding to R are always positive)

A6 *If aRb, then there is a number n such that bRna, where na is defined recursively:* $1a=a$*, and* $na=(n\text{-}1)(a\circledast a)$.

It is a theorem of Suppes (1951), reported as Theorem 17 in Suppes and Zinnes (1963), that for any extensive system, if we take equivalence classes under the relation *xRy&yRx* for that system, there is a numerical extensive system isomorphic to the extensive system formed from those equivalence classes. The proof of the theorem involves taking the class of elements that stand in the equivalence relation (same weight, length, etc., under the various natural interpretations) to some selected element *e* in the domain chosen as unit object. The proof consists in constructing a mapping *f* from equivalence classes of the non-numerical objects to the real numbers, and establishing that it has the properties required of an the relevant isomorphism.

With this apparatus in place, we are now in a position to give the second, less evidentially oriented construal of statements of the form "*x* measures *n* in units U for such-and-such magnitude-type". The proposal is that they should be read as saying:

> For the magnitude-type in question, there is an extensive system whose domain has *x* as an element, and a corresponding isomorphic numerical extensive system, and under that correspondence, there is a selected object *e* distinctive of the units U, whose equivalence class is mapped to 1 under the correspondence, while equivalence class of the object *x* is mapped to the number *n*.

We can call this proposal of equivalence the *extensive system/chosen object claim*.

This extensive system/chosen object claim is vulnerable to the same objections raised against the treatment of magnitudes as extensions. If the extensive system/chosen object claim is meant to be a complete account of what is meant by a statement of particular measurement, then it brings irrelevant objects into explanations. It also (a versions of the same point) does not work in counterfactuals. The properties of the standard gram or meter in Paris have nothing to do with, are irrelevant to, the explanation of why the avalanche flattened the forest or why the flagpole cast a certain length of shadow. This is reflected in the counterfactuals supported by such explanations. Other things equal, if the standard gram in the vault in Paris had been filed down, however much, the avalanche whose momentum had a certain magnitude would still have flattened the forest.

These points, I should emphasize, in no way undermine the value of the Suppes Representation Theorem. That numbers can code for certain systems of empirical relations between objects is both theoretically and philosophically significant. It shows how the assignment of numbers as measurements in a system can be empirically grounded, if the relations and operations in the extensive system are empirically grounded. It shows, in the context of a reasonable epistemology, how these statements of particular measurements can amount to knowledge. Indeed, I will be arguing that the framework can do more than that, taken together with a good metaphysics of magnitudes. But the points so far do show a gap between the theory of extensive systems and the Representation Theorem, on the one hand, and what it is for a statement of a particular measurement to be true. We need an account that can handle explanation, relevance, and the counterfactuals properly.

If we take a straightforwardly realistic attitude to the ontology of magnitudes, we are in a position to give a different interpretation to the Suppes axioms for extensive systems. Instead of construing the variables in the axioms as ranging over material objects and events, or other entities (such as places, times, or pairs of such) that have the measurable magnitudes, I suggest that we construe the variables simply as ranging over the magnitudes themselves. The relations mentioned in the axioms are then taken as relations between magnitudes. The references to functions on objects are reconstrued as references to functions on magnitudes. There is a relation, in the case of mass, of one magnitude M_1 of the mass-type being less than or equal to a magnitude M_2. There is a mass-magnitude M_3 that is the addition of two mass-magnitudes M_1 and M_2; and so forth. So construed, the axioms A1-A6 all hold for the familiar list of extensive magnitudes. Hilbert famously recommended that we regard axioms as implicit definitions. Hilbert said in a letter to Frege in 1899, about the concept *point*, that "… the definition of the concept point is not complete till the structure of the system of axioms is complete. For every axiom contributes something to the definition, and hence every new axiom changes the concept" (Frege 1980: 42). It is not necessary to agree with an unrestricted version of Hilbert's doctrine, or even to agree with him about the concept *point*, to recognize that some concepts are fully implicitly defined by a suitable set of axioms. I recommend that, in this particular case, we see the Suppes axioms A1-A6 as definitive of what it is for a magnitude-type to be extensive. There is nothing more to a magnitude-type being extensive than magnitudes that are instances of the type conforming to axioms A1-A6.

Under this reading of the axioms, the Suppes Representation Theorem has a rather different significance than it was initially portrayed as possessing. When the axioms for extensive systems are construed as being about magnitudes of a given type, the Representation Theorem is, perhaps surprisingly, a contribution to metaphysics. It, and more particularly the correspondence functions mentioned in its proof, provides a systematic link between two domains of entities: magnitudes of a given type, and the real numbers.

On this conception of the importance of the various Representation Theorems involved in the formal theory of measurement, they are completely freed from any suspicion that they have importance only on an evidentialist, operationalist, or verificationist theory of meaning and understanding. The theorems connect types of magnitude, as characterized by the different axioms to which they confirm, with the possibility of certain kinds of mapping to the real numbers. On the approach I am recommending, we distinguish what is to be explained by the nature of magnitudes of a certain type from what is to be explained by features of a procedure for measurement. The significance of the Representation Theorems, on the construal I advocate, neither mentions, nor does it allude indirectly to, any particular procedure of measurement. So where Suppes and Zinnes write that one of the broadly mathematical problems they are addressing is to "*determine the scale type of the measurements resulting from the procedure*", I would say their results are, instead, theorems that determine the scale type from characteristics of the magnitude-type itself, where the characteristics are given in certain axioms to which instances of the magnitude-type conform. The fact that a certain magnitude-type is extensive, for instance, is not in any way tacitly relative to some particular measurement procedure. It is a fact about the magnitude-type itself. Any characteristics possessed by a good procedure for measuring magnitudes of that type are explained by features of those magnitudes them selves, rather than being autonomous characteristics of the procedures.

When an object *x* measures *n* in certain units for a certain kind of magnitude, there is a magnitude M of that kind, such that *x* has magnitude M, and M also measures *n* in those units. The units may be described by reference to some standard objects or events in Paris. When the units are so described, the statement that the object measures *n* in those units, and the statement that magnitude measures *n* in those same units, may involve empirical properties and relations concerning some standard object in Paris. But one should always distinguish between:

> what is explained by x's having the magnitude M itself

on the one hand, and

> what is (or is not) explained by x's standing in certain relations to a standard object in Paris.

It is the flagpole's height having a certain magnitude M that explains the length of its shadow. Its relations to an object in Paris have nothing to do with that explanation. Parallel points apply to the momentum of the avalanche and what it explains.

It can be helpful here, as so often, to distinguish wide and narrow scope of descriptions in relation to statements of explanation. This statement can be true:

> There exists some magnitude M that is in fact ten times the length of the standard meter rod in Paris, and it is because the flagpole's height is M that it casts a shadow of a certain magnitude in respect of length.

That is compatible with the falsity of this statement, in which the material about the standard meter rod in Paris falls within the scope of "because":

> It is because the flagpole stands in a certain relation to the standard meter rod in Paris that it casts a shadow of a certain magnitude in respect of length.

And, to add the obvious remark that the reader is no doubt expecting at this point, Kripke's distinction between fixing the reference of an expression and giving its meaning, which he illustrated by the standard meter rod in Paris, needs an ontology of magnitudes for its most straightforward exposition (1980). The reference fixed by the description "the length of the standard meter rod in Paris" is a certain magnitude, M. That particular rod could have had a magnitude distinct from M as its length. To the best of my knowledge, the only plausible regimentation of this modal truth involves quantifying over magnitudes themselves, or involves notions that have to be explained in terms of an ontology of magnitudes.

It is possible to conceive of an explanatory enterprise in which the intended explanandum is in fact the relation of the shadow to the standard meter rod in Paris. I doubt that this is in fact the target of any ordinary empirical investigation of the world, but we can conceive of someone interested in such matters. For that person, the relational characterization "standing in such-and-such relation to the meter rod in Paris", as it features in the *q* position of "It is because *p* that it is the case that *q*" would be within the scope of the "because" operator. This is a different explanandum than is involved in an explanation of the magnitude itself, independent of its relations to what is the case in Paris. For this relational explanandum, it may indeed be necessary to mention the relations of the flagpole to the standard meter rod in Paris. But such an explanation is possible only because there is a core explanation, involving a relation between the magnitudes themselves, an explanation that does not need to mention the rod in Paris. That causal explanation holds, and then the entities it mentions, including the magnitudes involved, may be characterized in terms of their relations to other things – equally to objects in London or New York, as well as Paris. The core explanation does not mention standard objects or events.

An ontology of magnitudes does not have to involve a commitment to their having an absolute character. It is entirely consistent, and in my view correct, to hold that magnitudes are ineliminable, while also being relative. In my view, a fuller statement of the correct form of attribution of a magnitude is:

x has magnitude M of type T, at time *t*, relative to frame *r*.

Frame-relative magnitudes can still be causally explanatory. The fact that the time between two events has a certain temporal magnitude relative to a certain frame of reference can explain the difference between to clock readings made in that frame of reference. When we recognize the frame-relative nature of magnitudes, explananda involving magnitudes will also be frame-relative. The frame-relative magnitudes remain unit-free.

Frame-relative magnitudes of a given type need not exist in splendid isolation from magnitudes of other types. It is consistent with the existence of frame-relative magnitudes that they are individuated in part by their relations to other magnitudes. It is always a substantive question in metaphysics how far such individuative dependence extends. But some individuative dependence on relations to magnitudes of other types is consistent with the causal-explanatory power of frame-dependent magnitudes.

The realism about magnitudes I have been advocating here has consequences for various positions that hold that there are psychological and/or constructivist elements in magnitudes. I mention two such positions.

In his very engaging book on temperature in the history of science, Hasok Chang discusses what he calls "the *principle of single value* (or single-valuedness): a real physical property can have no more than one definite value in a given situation" (Chang 2004: 90). Of this principle, he writes that "it is not logic but our basic conception of the physical world that generates our commitment to the principle of single value". More specifically, he describes it as an 'ontological principle', "whose justification is neither by logic nor by experience" (Chang 2004: 91). "Ontological principles are those assumptions that are commonly regarded as essential features of reality within an epistemic community, which form the basis of intelligibility in any account of reality. The denial of an ontological principle strikes one as more nonsensical than false" (*ibid.*). He adds, "Perhaps the closest parallel is the Kantian synthetic a priori; ontological principles are always valid because we are not capable of accepting anything that violates them as an element of reality" (*ibid.*). These last two quotations suggest that the principle of single value is something to do with our psychological makeup, or the makeup of the minds in an epistemic community. Chang writes that a significant difference between his treatment of his ontological principles and Kant's treatment of his synthetic a priori principles is that "It is possible that our ontological principles are false" (*ibid*.).

From the standpoint of the realism about physical magnitudes I have been advocating, the principle of single value can be demonstrated. If there is a physical magnitude-type whose various real physical magnitudes form an extensive system, then it is a theorem, provable from their conformity to these axioms, that they can be measured by a ratio scale, and there will be at most one number that is their value once a zero and unit have been fixed. (This is Theorem 18, the Uniqueness Theorem of Suppes and Zinnes 1963: 43.) This theorem is not a matter of what, with our human psychological makeup, we are capable of accepting, or of features of our epistemic community. In my view, Chang is right to say that "our basic conception of the physical world" generates our commitment to the principle of single value – but the conception that generates it is one that involves the existence of magnitudes, of a certain type, from which the principle follows.

The realism makes such a difference here, because if one does not make use of the ontology of magnitudes, and speaks only of what, empirically, certain measuring procedures will produce in repeated circumstances, then the principle of single value looks empirical. But the correct principle in this area does not mention any particular measuring procedure at all. Uniqueness of value follows from satisfaction of the axioms characterizing an extensive magnitude (given a fixing of a unit), and any empirical consequences are empirical consequences concerning the nature of one's measuring apparatus and its regular operation. (Temperatures, which are Chang's focus, of course form a difference system, rather than an extensive system, but a similar point applies. There is a corresponding Uniqueness Theorem for infinite difference systems: see Theorem 13 in Suppes and Zinnes 196: 37.) What is genuinely empirical is this question: if there is a magnitude satisfying the relevant axioms, does such-and-such measuring device in fact operate in such a way as to give the value that's uniquely determined (given the zero and unit) by the magnitude we are attempting to measure? If the device in question does not give the same value in apparently relevantly similar circumstances, we may draw conclusions of varyingly radical degrees. We may, conservatively, conclude that circumstances are not relevantly similar. At the next level, we may conclude that they are relevantly similar in respect of what we are trying to measure, but that our device is not a good instrument for obtaining that measurement. At the most radical level, we may conclude that we were under an illusion that there is any such magnitude satisfying the axioms for measurement on a ratio scale of the sort we thought there was. That is what we would say today about those scientists - and philosophers - who believed that cold is as real as heat, a physical and explanatory magnitude in the physical world. (For further entertaining and illuminating discussion, see Chang 2004: 162-8.) I do not see, in a plausible description of our procedures in this area, any need to take the principle of single value as founded in some human epistemic characteristic.

A second kind of view holds that magnitudes, either singly or a group, have no identity outside of holistic groups of hypotheses that are involved or presupposed in any procedures we use for measuring those magnitudes. On views of this kind, magnitudes should be regarded as constructs from those procedures, and no more. In contrast, I would distinguish sharply between holism about the hypotheses on which we rely in thinking that operation of some instrument measures a certain magnitude, on the one hand, from constructivism and holism about the ontology of magnitudes themselves. The former holism, a holism of the epistemic, is evident. By itself, that epistemic holism does not imply constructivism about magnitudes. The epistemic holism is also consistent with various degrees of constitutive involvement of other magnitudes in the individuation of the magnitude being measured. The holism of hypotheses involved in accepting a particular measurement by an instrument is extraordinarily extensive. That particular response of an instrument measures a particular magnitude will involve some physical theory that involves the magnitude; it will involve matters concerning the materials of the instrument; it will involve the mechanisms by which it transmits to a read-out device. It is entirely consistent with this holism that these matters may have nothing to do with the nature, the individuation of the magnitude being measured. It may indeed be the case that some magnitudes are individuated in part only by their relations to other magnitudes. This is obviously an important question, both for any given magnitude, and for the general philosophical question of the principles that distinguish local from more extensive individuation. My point here is simply that, on a realistic conception of magnitudes, these issues cannot be settled simply from the undeniable phenomenon of the epistemic holism involved in accepting an instrument as measuring a particular magnitude.

There are positions in the previous literature that are close to endorsing the ontology I have been advocating, and I particularly want to mention the contributions of Brent Mundy and Chris Swoyer. Brent Mundy, in his papers of the late 1980s, in particular his 1987 paper "The Metaphysics of Quantities" offers a different motivation from that which has driven my discussion above.[2] Mundy treats quantities as properties of objects. There is a simple translation scheme between Mundy's ontology and mine. Where I write "*x* has magnitude M", Mundy would write "$Q_M(x)$", where Q_M is the property of having magnitude M. Where I have an algebra of magnitudes, Mundy has a corresponding algebra of properties. What he calls "rays" of properties are all properties that correspond to magnitudes of some single type of magnitude, in my sense. Mundy says the expression "the size of *x*" refers to a quantitative property of *x* (Mundy 1987: 34). I myself think it less strained to distinguish a size, as a first-level entity, and, what is distinct, the property of having that size. But these are all points of detail in the larger scheme of what ontology we should adopt. Mundy's position and mine are in the same camp.

The case for a realistic view of magnitudes is in fact broadly analogous to the case for a realistic view of properties in scientific explanation, as the latter case was developed in Hilary Putnam's essay "On Properties" (1975). It is a familiar point that many statements about properties in successful sciences have no plausible translation into statements about linguistic predicates. "Selection pressures will favour properties that produce stronger offspring" – the properties may not all be ones identified in our language, by any means. Many of the points that apply to properties apply equally to magnitudes. Putnam discusses the case of a scientist who conjectures that there is a single property responsible for a range of phenomena (Putnam 1975: 316). A scientist might equally make a conjecture about an as yet unidentified magnitude. It would be hard to accept the arguments for a realistic view of properties, but reject a realistic view of magnitudes. In some sense properties, on the Putnam model, are universals, since many different objects may have the same property. A similar point applies to magnitudes as I have been conceiving them. Properties and magnitudes, so conceived, are on a par with respect to the causal realm.

Mundy himself has a distinctive motivation for his own treatment. He says, of approaches that do not employ an ontology of magnitude-properties, that they

> all depend essentially upon at least one strong existence axiom asserting the *existence of sums*, e.g. the existence, for any two objects *x* and *y*, of an object $z=x*y$ whose magnitude is the sum of those of *x* and *y*. It is recognized in first-order measurement theory that this particular assumption is unrealistic because of practical limitations on the process of concatenation, but the only weakened first-order axiom system addressing this point known

2 I thank Hartry Field for mentioning Mundy's position, in discussions on the margins of the Mexico City workshop mentioned in the final note below.

> to me is that of Krantz…which replaces the assumption of universal existence of sums with the assumption that sums exist whenever they are not larger than a certain size, and which then yields existence of a scale only for objects not greater than that size. (Mundy 1987: 32)

This seems to me a good motivation, but it is a somewhat different motivation from mine. The motivation I offered for an ontology of magnitudes would apply even when the existence assumptions Mundy mentions are fulfilled. The metaphysics of what it is for an object to have a certain magnitude of a given type should not mention particular standard objects; and a philosophical theory that appeals to them does not give an adequate account of their role in causal explanation and in counterfactuals. These points apply whether the existence assumptions are fulfilled or not.[3]

In this respect, my position is much closer to the realism of Chris Swoyer (1987), who emphasizes the role of the property of having a certain magnitude in causal explanation. Swoyer is also sceptical, incidentally, of the possibility of purely extensional treatments of magnitude-properties. He treats magnitudes as properties, where the properties are neither extensions nor intensions. As Swoyer notes, the idea of introducing properties and relations into relational structures was earlier developed by George Bealer (1981) and Edward Zalta (1983). In the remainder of this paper, I want to carry this realism further, to develop some applications to our understanding of scientific laws, of the role of the real numbers in science, and of the explanation of some distinctive features of our perception of magnitudes.

2 Laws and Relations between Magnitudes

How are statements of laws to be understood under this realistic treatment of magnitudes? In a law such as Newton's law that f=ma, we normally substitute numerical terms for the letters for force, mass and acceleration, in some units for mass, distance and time. To each of the numerical assignments to a magnitude variable, there corresponds the magnitude itself, the one which has that measure under the given units. So a law such as f=ma codes a relation between magnitudes by specifying a relation between the numbers that measure those

3 Mundy contrasts his theory with "first-order theories", but the account I have offered is first-order, with an expanded ontology of magnitudes. I suspect the crucial contrast in this area is not between the first-order and the second-order, but between the various ontologies that different approaches admit.

magnitudes. We use the numbers to specify the magnitudes, and we use functions on numbers – in this case, simply multiplication – to specify relations between the magnitudes corresponding to those numbers. Under a realistic approach to magnitudes, there is a good case for saying that what is really explanatory is the relation between the magnitudes themselves. The law concerns that relation, and the numbers help merely in picking out that relation in a computationally convenient fashion. We could put it this way. There is some relation R between magnitudes of force, mass and acceleration meeting this condition:

> R(F, M, A) iff the measure of force F is the numerical product of the measure of mass M, in specified units, and the measure of acceleration A, in specified units.

What is on the right hand side of this biconditional obviously mentions numbers and operations on pairs of numbers. But the relation on magnitudes picked out by this numerical condition has a nature and identity entirely independently of the numerical apparatus used to pick it out. To give an analogy: we may similarly pick out a region of the surface of the earth using four GPS coordinates to specify a four-sided region. The region picked out has a nature and identity entirely independent of the GPS coordinate system used to pick it out. The same goes for relations between regions of the surface of the earth. The relevant Newtonian law, under the realistic conception of magnitudes, then concerns the relation R itself. It simply says of this relation R:

> For any object, its force exerted F, its mass M, and its acceleration A stand in the relation R(F, M, A).

This formulation leaves us, however, with at least two questions urgently in need of answers:

Question One: can we give some account of the nature of this relation R that makes clear that its nature and existence is independent of anything to do with measurement by real numbers? Without some such elaboration, the comparison of R with the relations holding between GPS coordinates and relations between places is in danger of being question-begging if we cannot characterize R fundamentally without mentioning the numbers.

Question Two: we can multiply numbers, but it is not at all obvious that we can make any sense of multiplying magnitudes themselves, so what operation involved in the relation R corresponds to multiplication? If we cannot multiply magnitudes themselves, what entities are within the domain and range of this operation, whatever it is? And how do those entities relate to the law f=ma?

It is at this point that the realistic approach to magnitudes I have been proposing here needs to be integrated with the formal treatment of magnitudes, ratios of magnitudes, and the algebra of magnitudes developed in Dana Scott's *A General Theory of Magnitudes* (1963).[4] The Introduction to Scott's essay says that it grew out of a series of lectures he gave on geometry in 1958-9 at the University of Chicago. His essay aims to give clear formal and algebraic articulation of the ideas of Eudoxus' theory of proportions, as later developed by Euclid, and to do so in a way that does not take for granted some prior understanding and reliance on the real numbers. Scott's work is of course of great interest in its own terms. If what I have to say here is moving in the right direction, it is also of wider significance. The abstract general theory of magnitudes, ratios, and operations on ratios Scott developed is, in my view, an essential component of a realistic view of magnitudes in empirical science more generally. I will later be arguing that a realistic view of magnitudes is capable of explaining some highly distinctive features of the perception and mental representation of magnitudes. Scott's account will also be essential to this explanation in various ways.

It makes sense to say, of two pairs of magnitudes (x, y) and (z, w) that the ratio of x to y is the same as the ratio of z to w (that x:y = z:w, in the traditional notation). We can say what this means without using any particular unit for the magnitudes involved, and without using real numbers. To keep matters intuitive, let us start with an illustration. Suppose we have a pair of ratios that are distinct. We will take the ratios 4:7 and 5:8 as our example pair. Then for such a pair of distinct ratios, there always exists what we can call a 'splitting' pair of natural numbers m, n such that the proposition

$$4m < 7n$$

differs in truth value from

$$5m < 8n.$$

In the case of this pair of ratios 4:7 and 5:8, one such splitting pair m, n is 13, 8. With those values for m = 13 and n = 8, we have 4m = 52 and 7n = 56; so we have 4m < 7n. But 5m = 65, and 8n = 64, so we do not have that 5m < 8n. The two inequalities displayed above differ in truth-value.

4 I am very grateful to Ian Rumfitt for drawing this striking work to my attention.

Scott's treatment generalizes this idea. In the terminology I just used, Scott's point is that ratios are identical when and only when there is no such splitting pair m, n. Where m, n range over integers, Scott (1963: 10) gives the definition:

$x{:}y = z{:}w$ iff for all m, n, $m\oplus x < n\oplus y$ iff $m\oplus z < n\oplus w$.

That is, the ratios between x, y and z, w are the same if and only if there is no splitting pair of integers, that is, no pair of integers m, n such that $m\oplus x < n\oplus y$ differs in truth value from $m\oplus z < n\oplus w$.

Scott notes that we can regard a ratio as an entity abstracted from a pair of magnitudes (Scott 1963, Section 6: 28). A pair of magnitudes always has a ratio, as a pair of parallel lines always has a direction, and as a segment in geometry always has a length. If we want to, we can initially treat the ratio x:y of magnitudes of a given type as the equivalence class of all pairs (u,v) of magnitudes of that type such that $x{:}y = u{:}v$. Note that here we have used only the natural numbers in characterizing the equivalence class, not the real numbers; and the natural numbers have been used only in characterizing repeated additions of a magnitude to itself.

It is also intuitive, and it makes sense, and it is often true, to say of two pairs of magnitudes, where the elements of the first pair are of one type, and the elements of the second pair are of a different type, that they have the same ratio. The ratio of the length that is measured by one meter to the length that is measure by 50 cms is the same as the ratio of the duration measured by 30 seconds to the duration measured by 15 seconds. Ratios are not type-dependent. Scott emphasizes this point too (Scott 1963: 12). The point will be crucial for explaining what laws mean under the realism about magnitudes that I have been advocating.

Scott develops an algebra of ratios in Section 6 of his essay. We can define the product of $a\oplus b$ of two ratios a and b, in the intuitive way:

If $a=x{:}z$ and $b=z{:}y$, then $a\oplus b = x{:}y$.

The unit ratio 1 also has the natural definition: it is the unique ratio c for which there is a magnitude x such that $c = x{:}x$.[5]

5 With these and other definitions, it can be shown that the ratios satisfy the axioms for a commutative field, though without a negative element and without a zero element (Scott 1963: 31).

Now we can return to the Newtonian law that force is the product of mass and acceleration. The crucial first step to answering the question of how it is to be understood on the present view is to note that when we give a numerical value to a magnitude, as when we say that the mass of the object is 7 grams, that is already a statement of a ratio. It is a statement of the ratio of the magnitude of the mass of the object in question to the magnitude that is one standard gram. Though we substitute numerical terms and variables when we compute forces, or acceleration, or masses, from Newton's law, what we are really talking about are ratios, represented by those numbers.

Newton's law then means that the ratio of any two forces is the product of the ratio of their masses with the ratio of their accelerations. This is a law about unit-free magnitudes.

We have seen that multiplication of ratios makes sense. So we have an answer to Question One above. The relation between force, mass, and acceleration asserted by Newton's law is one of multiplication between ratios of magnitudes. This is an account of what the law means without any commitment to any particular unit for any of the magnitudes involved, and without any commitment to the explanation of magnitudes in terms of their relations to particular standard material objects or events. This explanation of what the law means is consistent with the spirit and letter of the realism about magnitudes I have been advocating. If ratios, and their multiplication, can be explicated independently of the ontology of real numbers, as Scott's work shows, then this account of Newton's first law says what it means without any need to mention the real numbers.

Question Two asked how we are to make sense of Newton's law if, as I have asserted, it does not make sense to multiply magnitudes themselves? The question has already been answered, since under this explication of the Newtonian law, it is ratios of magnitudes, not magnitudes themselves, that are being multiplied. As I said, in substituting a numerical value for one of the variables in Newton's law, we are characterizing a ratio between magnitudes, rather than substituting a term for a magnitude itself.

This is a treatment of just one law, but I surmise that the same points can be applied in other cases too. When a law contains multiplication of real-number measures of magnitudes, its import can be taken as involving the multiplication of ratios of the relevant magnitudes. Since exponentiation is defined in terms of multiplication, exponentiation can be handled similarly.

Some proposed laws contain addition, but when they do, the extensive magnitudes being added are of the same type, and so they pose no problem of construal in a realistic theory of magnitudes. In economics, for example, the Keynesian consumption function (which is no doubt an oversimplification) states that the total value of an individual's consumption is the sum of an autonomous element plus the product of the marginal propensity to consume with disposable income ($C = a + cY^d$). Here addition is addition of two magnitudes of the same kind, monetary magnitudes.

It should not be seen as a particularly bold conjecture that no genuine law involves addition of magnitudes of different kinds. It makes no sense to add, for instance, a mass and a distance. A proposed condition that makes no sense cannot even be true; it certainly cannot function in explanations in the way genuine laws do.

Many laws, both of the more basic sciences, and of the special sciences, involve differential equations. The differential of a function is defined in terms of the limit of a sequence of values. To make sense of the limit of a sequence of ratios, we need to be able to make sense of the notion of the relative size, and of the difference between, two ratios. Difference is defined in terms of addition, and addition of ratios has a natural definition, as given by Scott (Scott 1963: 29): if $a = x{:}z$ and $b = y{:}z$, then the sum $a+b$ of the two ratios is $(x+y){:}z$. We can thereby make sense of the limit of a sequence of ratios, given the standard post-Weierstrass, post-Bolzano understanding of the notion of a limit. When the values of the variables in scientific laws are taken to be ratios, as I have argued, then differential equations involving such variables make sense, and can be true and explanatory.

3 A Comparison with Hartry Field's Program

In his book *Science without Numbers* (1980) and subsequent papers, Hartry Field outlines and begins to implement a program aiming to show how mathematics can have a role in scientific theory even if mathematics is not true. Field defends nominalism, the doctrine that there are no abstract entities (Field 1980: 1). What is of interest from the point of view of the theory of magnitudes is the particular way in which Field develops his nominalist program. Field's paradigm is Hilbert's axiomatization of Euclidean geometry in his *Foundations of Geometry* (Hilbert 1971). Hilbert's axiomatization is sometimes called 'synthetic', rather than 'metric', because it does not quantify over real numbers. Field's central idea is that by using the techniques of Hilbert, and in particular the representation and uniqueness theorems that he proved, we can show how mathematics provides a conservative extension of the nonmathematical parts of geometry that are characterized in the synthetic axioms provided by Hilbert. Field's view is that Hilbert's theory "is (or can be made with a little rewriting) a genuinely nominalistic theory of the structure of physical space" (Field 1980: xi). He extends the Hilbertian approach to give a nominalist treatment of Newtonian space-time, to quantities, and to Newtonian gravitational theory, Chapters 6 to 8 respectively of *Science without Numbers*. It is an essential part of this strategy that, for each scientific theory for which a nominalistic treatment is provided, there exist a Hilbert-style synthetic formulation that does not quantify over the real numbers.

I am not a nominalist. Field notes that you do not need to be a nominalist to find his program philosophically significant. It suffices to find attractive theories that do not invoke "extraneous, causally irrelevant entities" (Field 1980: 43), and to think that numbers are extraneous and causally irrelevant to the explanations of phenomena accounted for by scientific theories. Let us call *incompatibilists* those who hold that there is a conflict between mathematics playing an essential role in an empirical science and some real constraint to the effect that an empirical science should not invoke "extraneous, causally irrelevant entities". The concerns of incompatibilists should be allayed in the cases of those explanations by laws that mention only relations between magnitudes, on the model outlined in the preceding section. An object's having a certain magnitude of a given type can be explanatory, and causally explanatory, of spatio-temporal events and states of affairs. Its having that magnitude can also be explained by spatio-temporal events and states of affairs. Magnitudes, as conceived in this paper, are not extraneous entities isolated from the causal realm. Magnitudes are fully involved in the causal realm. They should not raise the same concerns as abstract objects raise for incompatibilists.

For the purposes of this paper, we can leave open the question of incompatibilism. Incompatibilism is a problematic doctrine, certainly in the naive formulation given above. It is not clear that fundamental physical science uses, or needs, the notions of causation and the causal at all. Maybe the notion of causation has a role in, and serves the practical purposes of, everyday life in ways that make it both quite unsuitable and quite unnecessary for the more basic sciences. The relevance of the ontology and conception of magnitudes for which I have been arguing does not, however, require any endorsement of incompatibilism (just as it does not require any endorsement of nominalism either). The relevance of the present conception of magnitudes consists rather in its bearing on ways in which we can achieve a theoretical understanding of those respects in which mathematics and its ontology is essential to certain parts of science, and those in which it is not. The enterprise of gaining some understanding of that issue is something in which we should be interested even if we are not nominalists or incompatibilists. Recognition of an ontology of magnitudes expands our resources for characterizing the ways in which mathematics is, or is not, essentially involved in one or another component or property of a scientific theory.

On the approach to scientific laws outlined in the preceding section, it is not necessary to develop a *synthetic* theory in Hilbert's style to show that the real numbers do not play any essential role in the nature of laws that relate various magnitudes, and do not play an essential role in explanations that invoke those laws. The laws, understood and formulated as laws that relate the magnitudes themselves, do not involve any quantification over real numbers, in cases in which the explication of the preceding section is available.

What individuates real magnitudes of a given type (distances, masses, etc.) is something that lies between the two levels of characterization of magnitudes available in Field's treatment. Those two levels available on Field's account are the synthetic characterizations given in a Hilbert-style synthetic axioms; and the statements of real values of magnitude-type in some measurement system, specifying the size of particular magnitudes of distance, mass, and so forth, for material objects, or particles, or pairs of space-time points, and so forth. Let us take each of these levels of characterization in turn.

We can fix on distance as an example in considering the synthetic level of characterization. In Euclidean geometry as axiomatized by Hilbert, there is use of a *same distance* (congruence) relation. Hilbert's Axiom Group III, the Axioms of Congruence, contains axioms such as these:

> III, 1. *If A, B are two points on a line a, and A' is a point on the same or another line a' then it is always possible to find a point B' on a given side of the line a' through A' such that the segment AB is congruent or equal to the segment A'B'. In symbols AB≡A'B'.*
>
> III, 2. *If a segment A'B' and a segment A''B'', are congruent to the same segment AB, then the segment A'B' is also congruent to the segment A''B'', or briefly, if two segments are congruent to a third one they are congruent to each other.* (Hilbert 1971: 10)

Here congruence is treated as a relation between line segments, a relation with properties specified in Hilbert's various axioms (in Group III, in this case). It is not treated as a relation between two line segments and a number.

Hilbert's whole set of axioms for geometry can be satisfied by two different spaces with points e.g. twice as far apart in one as in the other. There is nothing nonempirical in making this point. The distinction between one rather than the other of these spaces being actual can be detectable by forces and other laws. The intelligibility of the distinction may be restricted to distance at a given time (there is no need to insist on Newtonian absolute space to make the point).

Quite generally, magnitudes themselves slice more finely, and are individuated more finely, than the Suppes/Hölder axioms A1-A6, under the reading

Suppes intended. No particular set of magnitudes of a given type is fully characterized by these axioms. It is not at all surprising that more should be involved in their individuation, given that we are concerned here with magnitudes that are conceived of as explanatory entities in the material world. Magnitudes are empirically detectable entities, in virtue of their causal-explanatory relations to other magnitudes (force, acceleration etc.) and to other states, including perceptual states, discussed below. Magnitudes are not individuated solely by the relations of their instances to other empirical instances of the same type. This is a radical contrast with abstract objects for which the idea, as Quine once put it, that they are "known only by their laws" is more plausible, when properly formulated (1969: 44-5). This difference between magnitudes on the one hand, and classical abstract objects on the other, bears both upon the difference between the metaphysics of the two cases, and correspondingly upon what is involved in the ability to think about and represent entities of these two kinds.

Under Hilbert's own conception of the application of his geometrical axioms, the axioms are subject-matter free, and can be applied to any subject-matter for which there are properties and relations that stand in the relations specified by the axioms. See again his letter to Frege, 29/12/1899 in Frege (1980: 40-1). Any realist about magnitudes would expect to insist that there is more to being a set of magnitudes of a given type than is given in a set of subject-matter-free axioms. I earlier endorsed an axiomatic characterization of what it is for a magnitude to be extensive. That is not an endorsement of a global Hilbertian view of purely axiomatic individuation of the magnitudes themselves.

In summary, the characterization of magnitudes of a given type at the synthetic level does not distinguish between two different sets of magnitudes (of a given type) each of which satisfy the synthetic axioms.

The other characterization of a magnitude available in the Field-style approach is the statement of its value in particular system of units and measurement. Here the equivalence would be in the second style of proposed reduction of statements of magnitude that we discussed in the first section above. But these statements bring into characterizations of possession of a particular magnitude objects that are, by any reasonable standard, explanatorily irrelevant in explanations by possession of the magnitude, such items as the standard meter rod, the ticking of a particular atomic clock, and so forth. There is thus some tension for an incompatibilist who aims to explain away the apparent role of mathematics in a scientific theory by following the Hilbertian synthetic paradigm. The same arguments the incompatibilist uses against invoking allegedly causally irrelevant objects in explanations certainly apply equally against the explanatorily irrelevant standard objects, the meter rods and atomic clocks, that have to be invoked in following the Hilbertian paradigm.

I suggest then that when we take a realistic attitude to the magnitudes themselves, we should think of them as individuated at a level different both from the characterization in the synthetic axioms, and from the statement of measurement that relates a magnitude to particular objects or events. The former does not slice finely enough; and the latter involves explanatorily irrelevant objects. If we are realists about magnitudes, there is no pressure to explain what it is for something to have a certain magnitude in either of those two directions. A reasonable ontology of magnitudes allows us to explain how real numbers are not essentially involved in certain scientific laws without incurring either an inadequate statement of what is involved in the attribution of a particular magnitude to an object, and without bringing in explanatorily irrelevant standard concrete objects or events.[6]

I should emphasize that I have not argued, and it is not my aim to argue here, for the thesis that the real numbers play no essential part in science anywhere. All I have aimed to show is that they are dispensable in formulating the real content of certain kinds of law that state certain kinds of relations between magnitudes. There may be other kinds of law that resist this treatment. Moreover, there may be, or there could develop, sciences that state that all first order laws for a certain domain have a particular property, or are related to one another in various ways, and these properties and ways might be characterizable only in essentially mathematical terms. Nothing I have argued here rules out those possibilities. All I have argued is that a realistic conception of magnitudes gives us the resources for elucidating the respect in which certain laws do not involve mathematics essentially, but involve only magnitudes, their ratios, and operations on those ratios.

4 Unit-Free Magnitudes in Perception

I now aim to explain a feature of perceptual content by drawing on the above account of magnitudes. Prima face, we perceive certain magnitudes. The perception of magnitudes is essentially involved in what is arguably the most basic kind of perceptual content, what I call scenario content. The scenario content of a perception is the way the perception represents the world in the immediate

6 Field does briefly consider the introduction of a continuum of temperature properties, each one being the property of having a specific temperature. He says this approach would be "at least arguably a nominalistic one" (Field 1980: 55), and indeed cites Putnam's essay "On Properties" (1975).

spatio-temporal environment of the subject as being (Peacocke 1992: chapter 3). An experience with scenario content represents things and events as at certain distances and directions from, and as at certain angles to the perceiving subject. It represents various objects and events as standing in various spatial and temporal relations to one another. All of these contents involve the perception of magnitudes: distance, height, angle, direction, and so forth.

When all is working well, it seems that the perception of an object's having a certain magnitude (or magnitude in a range) is explained by the object's having that magnitude (or magnitude in the range). More precisely, a magnitude relative to a certain frame of reference causally explains the subject's perception of that magnitude, relative to that same frame.

The fact that some objective property causes a feature of perceptual experience does not imply that the perceptual experience so caused represents that objective property. Perceptual representation of a property goes beyond mere causal sensitivity. What more is involved is a substantive question of great interest. Burge (2010) argues that in the spatial case, and in a large range of other kinds of case, it involves the presence of certain constancies in the perceiving subject's psychology. The same spatial properties, and relations, are perceived as the same (constant) over a variety of spatial orientations and spatial relations of the perceiver to the object with the spatial properties, and in various ambient conditions. Here it is important that a crucial, and arguably most basic, set of examples of constancy are the spatial magnitudes themselves. The same objective length, angle, height is perceived as such in a variety of conditions and a variety of properties and relations of the perceiver. The perception of properties, such as shape, whose instantiation is constitutively dependent upon the presence of certain magnitudes, depends on the presence of at least some cases of constancies exhibited for distance and angle perception.

The point about the causal dependence of the perceptual experience of magnitudes on the presence of certain magnitudes in the perceived world is a point that applies beyond the case of the spatial magnitudes. The point applies also to temporal magnitudes. It applies to duration, to frequency, and to properties derivative from temporal magnitudes, such as rhythm and meter in the perception of music. The correct model for the perception of time must in certain ways overlap with the model of perception more generally; though of course temporal perception has its own special features.

The feature of perceptual experience that I aim to explain is illustrated by the fact that we do not perceive distance in centimeters, inches, or any other units. We don't perceive weight in grams, or duration in seconds, or any other units. This is a phenomenon I noted almost thirty years ago in "Analogue Content" (Peacocke 1986), but I failed there to ground it in a general philosophical account of magnitudes. Some experts may be capable of knowing, on the basis of their visual experience, and even without any counting, that a particular edge is 10 cms long, approximately. But even for that expert, there is a magnitude M such that the expert sees the object as having magnitude M in respect of length. What this expert has in addition is the cognitive capacity to identify that seen magnitude M as 10 cms, approximately. The seeing of the length as M and the capacity to know that the magnitude M is 10 cms are distinct. The same kind of seeing of the length as M exists in other subjects without the capacity to know, simply on the basis of visual experience, that it is 10 cms. Acknowledging the existence of such experts does not allow us to dispense, even in their cases, with the notion of seeing something as having a certain unit-free magnitude.

The basic form of the content of magnitude perception is:

> object *x* as given under a certain mode of presentation has magnitude M, where M itself is both: given in a certain way; and given as a magnitude of a certain kind (height, weight, etc.).

One and the same magnitude may be given in different ways when, for instance, the heights of two things widely separated in the visual field are both perceived. When widely separated, they are not given in perception as having the same height; but it may nevertheless be the same height that each mode of presentation picks out. The magnitudes given in the scenario content of perception are also given under modes of presentation. (That is an instance of the entirely general thesis that any object is given to a mind only under some mode of presentation, whatever the mental state or event in which it is given.)

The central component in the explanation of the unit-free character of perception of magnitudes is that the explaining magnitudes are themselves unit-free. Perception represents the magnitudes in the way they in fact are. If magnitudes were, *per impossibile*, always to involve some relation to some empirical object or event as designated unit, we would have to treat the explanation of the perception of magnitudes as somehow involving explanation by something that throws away, by some kind of factoring out, the alleged unit in the magnitude in explaining the roughly veridical perception that is unit-free. On the treatment of magnitudes proposed here, there is no such diversion through a unit that is factored out in the explanation, and does not feature in the content of the perceptual state. The magnitudes themselves are not ontologically dependent upon any particular unit defined in terms of relations to an empirical object. Nor do the perceptual states implicate any such relationship of the magnitude as perceived to some particular material object or event.

Similar points to these about the perception of magnitudes apply equally to the mental states and events involved in action. The contents of intentions, tryings, and action-awareness, can also involve magnitudes. These contents are equally unit-free. Sometimes the states and events involved in action inherit their content from the contents of perception. You may intend to stretch your arm *that* far, level with the end of the bookcase you see. But sometimes the content is not dependent upon current perceptual content. In the dark you may intend to reach up to a familiar height to pull on the hanging cord that turns on the light. There is a certain height H such that you intend to reach up to H above your shoulder. The way in which you think of this height need not involve any of the standard units of measurements. (You may, though you need not, think of H as standing in certain approximate spatial relations to the size of parts of your body, in particular your arm, in this case. I return to that in a later section.)

These points have consequences for how we think of the contents involved in content-involving computation that underlies perception and action. Just as we have a different conception of what the real laws are under a unit-free ontology of magnitudes, so a similar point applies to the content-involving transitions involved in subpersonal computation. It is unit-free magnitudes themselves that are computed by a perceptual system. Computational systems leading to the formation of an intention, or an action-plan, must also use unit-free magnitudes. I note this as a corollary task for further theory, rather than following a diversion off our main path.

The fact that magnitudes in the world are unit-free and that perception of them is also unit-free is not founded in some magical connection. There is a significant computational process, of interest both empirically and philosophically, underlying the perception of a magnitude as it really (or approximately) is in the world. My point in this section is that the general form of explanation that permits us to perceive a range of properties and relations as they really are is something that will apply to unit-free magnitudes also. Perceptual content concerning magnitudes is also plausibly content for which it is true that for a perceptual experience to possess it, perceptual experiences as of magnitudes must be causally explained, in a range of central cases, and when all is functioning properly in the perceiver, by those magnitudes themselves (Burge 2003, Peacocke 2004).

There is a view according to which we do not perceive magnitudes at all, contrary to the arguments I have given so far. This opposing view has been proposed and defended by Brad Thompson (2010) and by David Chalmers (2010, and forthcoming). Suppose that overnight, unknown to you, all your linear dimensions doubled. Suppose too the world around you also doubled in its linear dimensions. Wouldn't everything look the same to you? Suppose too that this state of affairs continued throughout your life, say for another thirty years. After thirty years, wouldn't experiences that are now of lengths of two meters be phenomenologically identical with experiences that were of lengths of one meters, before the night of the great shift? Thompson and Chalmers argue that the subjective representational content of spatial experience is not really of magnitudes at all, but of something common to phenomenologically identical experiences before and long after the great shift. This is a view of spatial experience that is similar to certain views of the representational content of colour experience. Those are views according to which there is a common phenomenology across different cases in which different reflectance properties produce the same subjective experience of colour. The different reflectance properties do not enter the representational phenomenology, on one natural understanding of that notion, in colour experience. The proposal is that perception of spatial magnitudes be treated similarly to the perception of the objective properties that cause colour experience.

This rival to the view I have been advocating has ontological and epistemological dimensions. Ontologically, it naturally treats phenomenology as supervenient on properties within the body, or even the brain, of the perceiving subject. It also has many epistemological ramifications for scepticism, as Chalmers notes. The content of beliefs reached by taking perceptual experience at face value is much less committal than one might have thought. Such beliefs are neutral on the actual magnitudes of the objects the perceiver sees around her.

Because of these multiple ramifications, the Thompson-Chalmers view deserves extended discussion, which would take us off our main track. Here I just want to indicate how I would respond to the considerations offered in support of the position, and to indicate the elements of the competing conception I would defend.

I suggest that the doubled-earth hypothesis, in the form that involves doubling the linear dimensions of a given individual (and the relevant part of the world around him) in a given, single world, is not nomologically possible. What goes on in the brain of the doubled individual? If we are not changing the laws, then chemical and electrical signals have double the distance to travel, they will arrive later. But also some of their paths will be of different relative length in the doubled world, for the mass/volume ratios of some neurons will change, there will be corresponding shifting around in the brain, some paths will be more than double in length, some shorter… this will yield not smooth computation of representational contents. It will instead involve a gradual and increasing divergence from simply a doubling in size from what goes on in the subject's brain in the actual world. In brief, the spatial contents of our actual experiences are rooted in all sorts of nomologically based constraints.

Correspondingly, the epistemic landscape and the relation of perceptual content to scepticism look different on this view too. The class of possible worlds consistent with a subject's perceptual experience is much more restricted on the view for which I have been arguing than on the Thompson-Chalmers conception. Similarly, the content of the beliefs reached by taking perceptual experience at face value is much more specific. Those beliefs will include beliefs about the particular magnitudes of the objects perceived. The response to scepticism will, on the view I endorse, have to be very different from that available on the Thompson-Chalmers treatment. Scepticism is prima facie more challenging on my more externalist view. For what it is worth, the resources for addressing it are also richer in various respects.

I was just arguing that it is not nomologically possible for there to be a great shift in size, and perceptual content to remain constant over decades, for a given individual in the actual world. That case is to be distinguished from the case of two individuals in worlds that are different throughout their history, and in which one individual has a counterpart in a second world, where that individual, and the world around him, are both doubled in linear dimensions. The perceivers in such a second world would, if the above arguments are correct, either have to have different kinds of brain, and/or the laws would have to be different. The experiences of persons in this second world could have correct representational contents concerning the spatial magnitudes of things. The so-called long-arm functional characteristics of the experiences of those in the second world would also match these correct representational contents. A subject perceiving a thing two meters away that he wanted to touch would move two meters towards it; and so forth. The fact that such a second world is possible, one in which brains and/or the laws are different, does not show that a great overnight shift, of a sort that would establish that we do not perceive magnitudes, is also possible.

The unit-free character of the perception of magnitudes is not the only feature of such perception that can be explained by the distinctive properties of the ontology of magnitudes. I would argue that we can also define, and explain the applicability of, a notion of the analogue character of certain perceptual contents in the framework I have been advocating. But that must be a topic for another occasion.

5 Real Magnitudes and Merely Defined Quantities

We can specify, for any continent, a number thus: if the continent is divided into countries recognized by the United Nations, take the sum of the height in meters of each of the highest mountains in each of those countries, and divide that number by the number of cars owned in that continent. For any continent divided into countries, this is a well-defined number (I just defined it). But there is a sense in which it does not specify a genuine magnitude of any explanatory significance. A continent's being associated with a certain number as specified by this condition does not correspond to its having certain explanatorily significant powers. In the case of properties, Sydney Shoemaker has suggested that "properties are clusters of conditional powers" (Shoemaker 2003: 213). Genuine magnitudes in the spatio-temporal world have explanatory powers. There is considerable plausibility in a condition for identity of magnitudes framed in the spirit of Shoemaker's treatment of properties. Shoemaker writes, "Let us say that an object has power *P* conditionally upon the possession of the properties in set *Q* if it has some property *r* such that having the properties in *Q* together with *r* is causally sufficient for having *P*, while having the properties in *Q* is not by itself causally sufficient for having *P*" (Shoemaker 2003: 212-3). Shoemaker uses causal notions here, but we can substitute "explanatorily sufficient" for "causally sufficient" in the characterization of conditional powers if, for some of the reasons alluded to earlier, we want to place more weight on explanation than on causation. A condition for magnitudes corresponding to Shoemaker's claim is then that magnitudes M and M' are identical if they have the same conditional powers. So, under this criterion, it is plausible that having a certain magnitude of electric current is the same magnitude as this: the number of charged particles per unit volume, times the drift velocity of the charged particles, times the charge on each particle, times the cross-sectional area of the carrier of the current. This last complex description of a magnitude, and the simpler description "the magnitude of the electric current", are descriptions of one and the same magnitude, a magnitude with a single set of conditional powers in Shoemaker's sense. The conditional powers of any magnitude of a certain type will include powers to explain other magnitudes of the same type possessed by other objects and events. So the condition for identity of magnitudes should not be seen as any kind of eliminative reduction of the notion of identity as applied to magnitudes.

It is sometimes a real scientific issue whether a description picks out a genuine magnitude, or whether it is merely a defined quantity. I suggest that the issue of whether there is a genuine magnitude picked out by the description should be equated with the issue of whether the putative magnitude has some conditional powers. In the twentieth century, there was a dispute, discussion of which continued at least into the previous decade, of the significance of what the statistician and psychologist Charles Spearman (1904) called 'the *g* factor', which he sometimes characterized as 'general intelligence'. In Spearman's work, the number *g* was well-defined statistically: it was computed by performance on a number of different tests. But it was and remains a substantive issue whether it is a real magnitude capable of any explanatory significance at all. This is not the place to take a stand on the issue. My point at present is merely that evidence of its explanatory power is what is required if it is to move beyond the status of merely a defined quantity.

It is one thing to say that identity of conditional powers suffices for identity of magnitudes. It is another, and further claim to say that magnitudes are individuated by their conditional powers. The former claim takes the background of our actual laws for granted. But it is arguable that we have a conception of distance, for instance, under which we can make sense of the same distance holding between two events in worlds in which the laws involving distance are different. At any rate, I should emphasize that grounding the idea of a genuine magnitude in explanatory potentialities prima facie leaves open apparently further issues of whether there is a legitimate notion of magnitudes of a certain type that have an identity and nature that outruns the particular laws in which magnitudes of that type feature in the laws of our actual world.

6 Wider Significance of this Conception of Magnitudes

The position I have been developing in this paper is naturally married with, and is at certain points also an instance of, a highly general position in philosophy. The general position is one that gives priority, in the order of philosophical explanation, to the metaphysics of a domain over the theory of intentional contents concerning that domain, and the theory of meaning for the part of language concerning that domain. The particular instance of the general position supported here is the priority of the metaphysics of magnitudes over the theory of intentional contents and meanings concerning magnitudes. This *metaphysics-first* position can be argued for on highly general, abstract grounds that have not been my topic here. But however successful those general, abstract grounds may be, the metaphysics-first position is never fully convincing until one sees it elaborated in more detail for various different kinds of subject matter. What I have been doing here can be taken as a special case of the task of carrying out that more general project for various domains, for the special case of magnitudes and their perception. What I have been doing can be seen as what in other activities is called "proof of concept". For those doubtful that we can make sense of the idea that the metaphysics of a domain is philosophically prior to a theory of the intentional contents of mental states concerning that domain, this treatment of magnitudes is offered as a working example of a case in which we can show that that philosophical priority indeed obtains.[7]

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7 Earlier versions of this paper were presented at the 21st Annual Meeting of the European Society for Philosophy and Psychology in Granada, Spain, in July 2013, as a keynote address; at the Kirchberg Symposium in Austria in August 2013; at the Inter-American Congress of Philosophy in Salvador, Brazil, in October 2013; at the Second Metaphysics Seminar in Mexico City (UNAM and the Instituto de Investigaciones Filosoficas) in November 2013; and at the Work-in-Progress Discussion Group in the Philosophy Department at Columbia University in April 2014. Special thanks are due to Tyler Burge, for his extensive written comments and advice on an earlier version, and to Hartry Field, for several discussions in Mexico City. Their very different reactions have affected both the substance and the presentation. I have also learned from, and taken into account, valuable comments from David Albert, David Chalmers, Michael Devitt, Paul Horwich, Philip Kitcher, Peter Railton, and Robert Stalnaker.

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