justifying what ? - two basic types of knowledge claims revisited

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Abstract

"It is often assumed that knowledge claims must be justified. But what kind of justification is required for knowledge ? . . ." [1]

presupposition: the kind of epistemic justification depends on the type of the knowledge claim and its respective knowledge claim tradeoff 'vague vs. precise'.

procedere: in two - almost purely logical - case studies I account for this tradeoff and question in each case what (if any) were its general outcome wrt justification

first for basic measurement statements of the form $''\phi\,(\mathbf{x}) = \mathbf{r} \in \mathbb{R} \pm \delta''$ (wrt "measurement accuracy realism" debate)

and secondly predication statements of the form $''\mathbf{x\ is\ (\ a\ case\ of\ )\ P}''$ (wrt "epistemic vagueness" debate).

Contents

[1] I submitted an early draft of this stuff in response to the CFA for the **epijust** conference(held in the end of March 2023 at Munich). While This paper was not selected for the conference, I find it worthwhile to make it publicly available in a more elaborated version. It summarizes and expands my views of basic forms of vagueness and relates them to 'epistemic justification'. My three papers, to which I refer for this, had been dedicated to spotlight some imop misconceived aspects of the interweaving of knowledge and reality in science and humanities.

1 'vague vs. precise' tradeoff wrt basic measurement

1.1 mapping observations to maths

considering formulae expressing[2] a measured value $\phi(\mathbf{x}) = \mathbf{r} \in \mathbb{R} \pm \delta$

1. measurement statements usually are said to be precise within a certain 'margin of error' [3]

2. the presupposition, that physical objects do have precise quantitative properties, which only - due to unfavorable circumstances - can't be measured exactly, has been challenged at least since the late seventies[4], thence arising the question, whether physical objects instead were to be conceived to have vague quantitative properties, and whether accordingly, what had been termed before error of measurement margin [a 'small' interval of reals embedding the measured value] now were to be understood as a zone of 'object vagueness'.

3. my thesis here amounts to contesting both, that neither view is appropriate to describe the situation, instead, that the indeterminacy in question is a relational one. And try to account for it not so much as a lack or deficiency of knowledge, but primarily as a consequence of theory construction, viz. as a consequence of mapping finite sets of observations (which may have been judged as pretty true wrt the corresponding operative rules, the used measurement set and an appropriate measurement situation) to mathematical structures (which, due to some postulated Archimedian property wrt the selected unit) are held to be potentially infinite.

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[2]the argumentation in this section originated with my 1981 "Differences in Individuation and Vagueness" [7], imhop as well of relevance to contemporary discussion

[3]see e.g. in Krantz et alii [10] § 1.5.1 Error of Measurement, p.27f.

[4]at that time (1978 ff.) by Günther Ludwig et alii, see for a short account Michael Drieschner's review of 'A New Foundation of Physical Theories' by Ludwig,Günther and Thurler,Gérald,[[6], pp. 403-406]. For more recent discussion cf. Eran Tal's work [15], [16], [17] and also Paul Teller's [18]. Tal and Teller both use in their respective expositions the word 'accurate' instead of the word 'precise', maintained here from my 1981 paper.

4. Obviously these mappings can never be uniquely determined from the empirical data (finite sets of observation statements)[5] except in the 'ruler case',and this (logico-mathematical) indeteminacy is misconceived as an epistemic, else metaphysical, vagueness.

Figure 1: basic measurement case study

strictly empirical	quantifying in	theory's object domain
measurement operations	idealized structure	mapping $q \pm \delta \in \mathbb{Q} \mapsto \mathbb{R}$
finite number of objects	countably many objects	
comparative concept &	unit selection, postulating	unique up to a scale factor
& measurement ↓	Archimedian property	
↓ observation values ↓	$\mapsto$ $q \in [dense\,order]\ \mathbb{Q}$	
↓ are comparative and ↓	mapping not unique	
↓ cluster round mark 'q'	hence $\mapsto$ $q \pm \delta \in \mathbb{Q}$	

1.2 outcome wrt the epistemic justification of 'basic measurement knowledge claims'

The observation sentences' acceptance as true then seems to be only proper part of the epistemic justification of the basic measurement statement, as the assignment of some $r \pm \delta \in \mathbb{R}$ to an observational result goes in a decisive way beyond any possible observation: it hence is not part of the truth condition of the observation sentence(s), but this assignment is a (vital [6]) part of the truth condition of the basic measurement statement. So, in a sense this assignment thins out the full empirical justification provided by the observation sentences, on which the 'basic measurement statement knowledge claim' is based. Hence epistemic tradeoff is: $\boxed{+}$ more precise, $\boxed{-}$ thinning out empirical content.

[5] the structures paradigm – to which I here refer to – may be found in Krantz et alii [10], §3.2.1 Closed Extended Structures, pp. 72 ff.

[6] e.g., it's known basically from Zeno's times, that for a consistent account of motion rational numbers, which to some extent may seem operationally addressable, are not sufficient to do the job. And the late solution, real numbers - providing limits for converging sequences - reside totally beyond the world of operational meaning. A nice sketch of the case with Zeno in Russell's (written in popularizing intention) article 'Mathematics and the Metaphysicians' [13]

The epistemic justification reaction wrt this **justification gap** seems to be quite independent of a stance wrt "measurement accuracy realism", a fortiori independent from the here sketched special form of its denial. Such follow on justification will invoke the essential role of measurement availability for designing experiments and for developing quantitative conjectures and theories on the behaviour of physical objects, and in this context, for identifiying physical objects as quantitative objects, in simplest cases as scalars or vectors (e.g. velocity vectors in kinematics).

In this justification endeavour it will also resort to some or other kind of **higher order justification**, e.g.
1. to the intersubjective verifiablity of measurement operations' compliance to the accepted measurement standards and procedures
2. to the reproducibility of the measurements with little variance in observational results (observation sentences), which expectation implies assumptions on the durability, stability and availability of suitable measurement equipment, and of course of the reproducibility of suitable measurement situations, including the absence of 'occult powers' during measurement [7]
3. to the availability of [wrt the 'margin of error'] equal or corresponding or still significantly better results by other, more refined methods of measuring the quantity in question, e.g. optical measurement of length instead of measurement by rods; but presumably no such alternative measurement option would be better off wrt the projection of finite observation data to infinite mathematical structures; e.g. with triangulation you'll have the mathematical projection for observed angels and line segments into an metrical e.g. Euclidean space.
4. to an unclear multiplicity of assumptions as to the uniformity of nature, its conformity to quantitative natural laws, to the truth-conducity of coherence in physics, ...
5. to technological, else industrial, availability and usability of measurement arrangements (practicability proof)
6. to ... [open list] ...
And it is perhaps only wrt the 'conformity to quantitative natural laws', that the refutation of 'measurement accuracy realism' might induce a suspicion wrt an 'accuracy realism of physical objects described by physical theories' [8]

Again, this type of situation (questioning, whether "the book of nature is written in the language of math's") is not pretty new, has a well known predecessor long time before the development of the infinitesimal calculus. In an unarticulated way it occurs in the history of science already, when - purportedly - some unlucky Pythagorean adept (around 450 B.C.) detected the first reported case of a quantity, which can't be grasped as a rational number [9]

[7] standard example: while measuring length by rods, rods be not bent during transport, ...
[8] and hence we are back in Ludwig programme's line of realism attack, as sketched in [6]
[9] $\sqrt{\mathbf{2}}$, cf. Becker[2], p. 41, this case somewhat ironically implicitly cited in Plato's choice of the geometry example for his "Meno" dialogue [unit square's diagonal]

2 'vague vs. precise' tradeoff wrt predication

2.1 relational account of Sorites series

vagueness of predication is typically claimed for sentences S of the grammar syntactical form **x is (a case of) P** , (where **P** is meant to represent a scientific empirical term, else an every day language term), and this grammar syntactical form is supposed to be as well the logical syntactical form, i.e. that **P** then symbolizes always an unary predicate[10]. My thesis here amounts to an elaboration of a contrary view, viz. that this unary predicate stance is at least unrewarding, and might and in certain cases should be replaced by an again relational analysis (see below on 'proper types' with and without definite extension).

Lastly motivated by my search for a **logical**(!) criterion to settle the Whewell-Mill debate on whether 'natural groups are given by type or by definition' [11]

1. I turn to the Sorites series discussion. As a tool for analysis I introduce a concept of 'similarity relation' (any 2-place relation, which is reflexive and symmetric) as a generalization of the well known concept of 'equivalence relation' (any 2-place relation, which is reflexive, symmetric and transitive). Hence wrt this definition equivalence relations prove to be just the transitive similarity relations, the complement are the 'non transitive similarity relations'. Using this latter concept I show the Sorites series structure may well be conceived as a structure for a non transitive similarity relation, ending up my Sorites series analysis for now with a Sorites series formula [**ssf**]

$$\mathbf{P}(\mathbf{a_1}) \wedge \mathbf{S}(\mathbf{a_1}, \mathbf{a_2}) \wedge \ldots \wedge \mathbf{S}(\mathbf{a_{i-1}}, \mathbf{a_i}) \wedge \mathbf{S}(\mathbf{a_i}, \mathbf{a_{i+1}}) \wedge \neg \mathbf{S}\left(\mathbf{a_{i-k(0<k<i)}}, \mathbf{a_{i+1}}\right) \wedge \neg \mathbf{P}(\mathbf{a_{i+1}}) \ldots$$

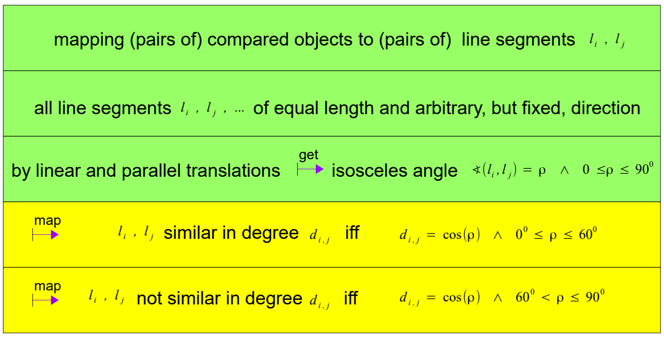
2. But, as the concept of 'non-transitive similarity relation' is a horribly weak and general one and thus seems to evade conceptual imagination, I give an exact but extremely simple **geometrical model**, which allows a visual imaging of the non transitive (as well as the transitive) cases.

Thus the intended use of this model − besides the trivial task of proving consistency of the Sorites series formula [**ssf**] − is mainly a heuristic one, viz. to give a geometrical intuition of the behaviour of similarity relations (again, of non transitive as well as of transitive ones)

[10]argumentation in this subsection drawn from my paper "Note on Sorites Series" of 2020 [8]

[11]the most crucial passages at Whewell [19] Chapt. II,§9 Difference of Natural History and Mathematics, and §10 Natural Groups given by Type, not by Definition, pp. 121 f., and Mill [11],Chapt. VII, § 4, pp. 278ff., especially p.282

Figure 2: visualization model for mapping similarity by degree - for both: transitive and non-transitive similarity, for an example see figure 3



3. preliminary ending up, that supposedly both (the transitive as well as the non transitive) similarity relations perform scientific work, and it seems rather nonsensical to play one off against the other. But although either similarity variant deserves its place, in Whewell's example of the rosaceae group in botany, or else in natural history studies in general, Whewell's defense of using [proper] types[12] seems better substantiated than Mill's criticism.

2.2 types: predicates and proper types

the topic of 'non transitive similarity relations' now gets somewhat refined by an tripartite definition:[13]

1.0. **types** may be viewed as generated by a (**transitive or non transitive**) **similarity relation S** and a non empty set of **set of paradigms PD** belonging to its domain, i.e. $\bigwedge_{\mathbf{x}}(\mathbf{x} \in \mathbf{PD} \rightarrow \mathbf{Sxx})$

1.1. **predicates** then may be viewed as types generated by an transitive similarity relation, viz. - without loss of generality - as the union of some of its equivalence classes, whose representatives form the paradigm set.

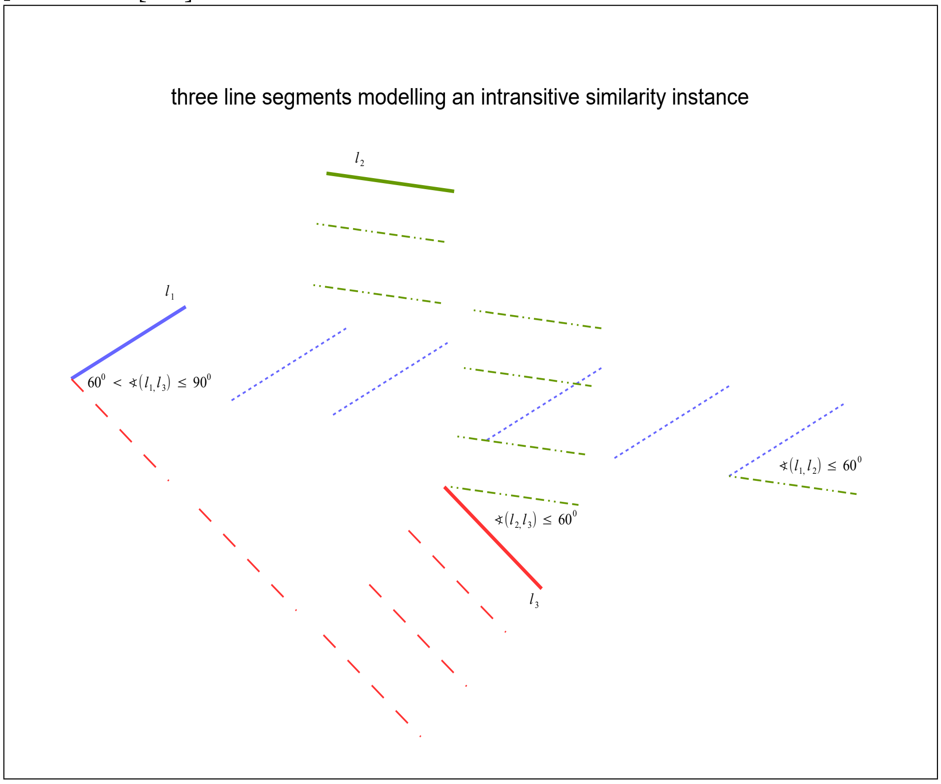
1.2. types generated by an non-transitive similarity relation wrt a freely chosen[14] paradigm set may be called **proper types** .

[12]'proper types' in my terminology, see below

[13]argumentation in this subsection refers to and expands stuff from my paper "on the epistemological significance of arguments from non transitive similarity" of 2021 [9]

[14]freely chosen from the purely logical point of view. Of course, in epistemic relevant cases as in - say - an e.g. 'megalopolis'-type example from some social science, the selection of

Figure 3: example visualization in the line segments model: three line segments in 3D space. $\angle(\mathbf{l_1}, \mathbf{l_3}) > \mathbf{60^0}$ here corresponds to the $\ldots\neg\mathbf{S}\left(\mathbf{a_{i-k(0<k<i)}}, \mathbf{a_{i+1}}\right)\ldots$ part of the $[\mathbf{ssf}]$ formula



2.2.1 non transitive instances don't vanish, when the subsumption problem for a proper type is solved

2. The line segments model (for mapping similarity relations) specified above allows for the mapping of a sharp **demarcation** between 'similar [in degree d]' and 'not similar [in degree d]'. And this might seem to reveal an epistemic advantage of predicates (generated by transitive similarity relations), which are supposed to have a fixed extension, against proper types (generated by non transitive similarity relations), which may show Sorites series structure with its allegedly 'grey zone' of borderline cases. I contest, that in this respect there were a decisive difference between proper types and predicates, by claiming, that **in both cases we have a like task of subsuming x under P**.

objects for the paradigm set will be motivated by some subject-specific and expert reasoning

By 1.0 above both, predicates and proper types, may be viewed as ordered pairs of paradigm set and similarity relation (both non empty), viz. $\mathbf{P} = \langle \mathbf{PD},\ \mathbf{S} \rangle$.

Subsuming an object $\mathbf{x}$ under a predicate or proper type $\mathbf{P}$, i.e. asserting $''\mathbf{x\ is\ (\ a\ case\ of\ )\ P}''$ (grammatical form) then amounts in both cases to asserting

$$\boxed{\bigvee_{\mathbf{y}} [\ \mathbf{y} \in \mathbf{PD}\ \wedge\ \mathbf{S}(\mathbf{y},\mathbf{x})]} \qquad [\textbf{subsume}]$$

which hence is a common logical form
for subsuming whatever object $\mathbf{x}$ under (a proper type or predicate)$\mathbf{P}$

By solving the subsumption problem for all objects in the respective range[15], a similarity relation may change from being transitive to being non transitive (depending on decisions wrt borderline cases) - but not vice versa; this latter asymmetry caused by: the transitivity being negated, if at least one counterexample (as sketched in **figure 3** above) exists. But what - by solving the subsumption problem for all objects in the respective range - changes anyway, is, that **completing subsumption fixes the extension** of the predicate or proper type in that range, hereby **abolishing any grey zone**, if it existed before.

Returning to [**subsume**]: An intransitive instance of a similarity relation $\mathbf{S}$ is always given by a triple of objects $\langle \mathbf{x},\mathbf{y},\mathbf{z} \rangle$ of the domain of $\mathbf{S}$. Now for proper types $\mathbf{P} = \langle \mathbf{PD},\mathbf{S} \rangle$, regardless whether the subsumption problem for $\mathbf{P}$ (e.g. by clearing borderline cases) is completely solved, i.e. regardless whether the extension of $\mathbf{P}$ is uniquely determined, we can distinguish two cases, of which at least one obtains at least once for any proper type $\mathbf{P}$:

case 1 (extra – type) : the domain of the similarity relation $\mathbf{S}$ exceeds the extension of the type $\mathbf{P}$, i.e. there exists an object $\mathbf{x}$, belonging to the domain of $\mathbf{S}$ but not belonging to the extension of the proper type $\mathbf{P}$, because it's not similar to any paradigm $\mathbf{u} \in \mathbf{PD}$. Nevertheless, it may be related to such a paradigm by a similarity chain, this then were the Sorites series case as described by **ssf**. In other words, Sorites series is a subcase of **case 1** .

case 2 (intra – type) : there is an intransitive instance $\langle \mathbf{x},\mathbf{y},\mathbf{z} \rangle$ of $\mathbf{S}$, where all three objects $\mathbf{x},\mathbf{y},\mathbf{z}$ belong to the extension of $\mathbf{P}$, i.e. each of them is similar to some paradigm $\mathbf{u} \in \mathbf{PD}$. But this **case 2** is a misfit otherwise, compared with the transitive case; because in this case the existence of an intransitive instance shows, that not every path between objects in the extension of $\mathbf{P}$ is a similarity chain wrt $\mathbf{S}$.

It seems, we could compare the percentage(s) of extra and intra cases in samples of a single and of multiple intransitive similarity relation(s) in a research field, and use them as an indicator for their (empirical) significance.

Using a proper type $\mathbf{P}$ as a (unary) predicate then may hide the weakening of its conceptual strength by the intransitive instance(s). Nevertheless, such use as a predicate is from the purely logical point of view legitimated by the equivalence

$$\boxed{\mathbf{PD} \neq \varnothing \Vdash\ \mathbf{P}(\mathbf{x})\ \leftrightarrow\ \bigvee_{\mathbf{y}} [\ \mathbf{y} \in \mathbf{PD}\ \wedge\ \mathbf{S}(\mathbf{y},\mathbf{x})]} \qquad [\textbf{predication}]$$

for predicates $\mathbf{P}$, as well as for proper types $\mathbf{P}$.

[15] say, by processing an appropriate catalogue of measurements and/or expert decision

2.2.2 basic calibration, a rare(!) unproblematic case of solving the subsumption problem for a predicate

The calibration of basic measurement devices, e.g. weight pieces of allegedly equal weight for a beam balance[16], may serve here as an example of an unproblematic solution of the subsumption problem: wrt, say, an industrially produced set of balancing weights: any weight suspect of belonging to the grey zone or of precipitating an intransitive instance is winnowed. Though, from the logical point of view, to exclude intransitive instances with certainty, it would be necessary to test the weight pieces not only against the calibration standard but also mutually each against each other, which is hardly done often or at all in reality. But within the context here it is sufficient, that it is possible to extend the calibration procedure that way. More important: from an epistemic point of view, the subsumption procedure seems unproblematic, because in this kind of calibration both, the calibration standard (which serves as the respective paradigm) and the similarity relation ('equal weight', implemented by the calibration process) are operational, and there is no respective operational interest in the winnowed pieces.

2.2.3 concerning proper types in general

3. There has been apparent **underestimation** of non transitive similarity relations within Logical Empiricism, but their ubiquity, their varieties and their usefulness in science, humanities, and technology are obvious - I broadly mention and partially discuss examples ⟨ in [9], §3 ⟩

4. Application of the **proper type – predicate** distinction to a well known philosophy example ⟨ summary of [9], §4 ⟩ :
applying the distinction to the start of the enduring "universals debate", in order to advocate Plato's theory of Forms against the criticism, Aristotle raises in his 'metaphysics' in some text passages against 'the advocates of the theory of forms' [17]. Analysis along these lines shows: Plato's theory of forms may well be understood as a special case of 'proper type' reasoning, but of proper type without definite extension[18]. Epistemic tradeoff: while Aristotle's universals qua 'predicates' fit as syllogistic terms for valid syllogistic reasoning; Plato's forms qua proper types without presupposed definite extension would fail for that purpose; this is, because syllogistic reasoning presupposes a definite extension of the syllogistic terms [19]. Nevertheless Plato's theory of forms constitutes

[16] also the example in the already cited Krantz et alii [10] §1.5.1 p.27f.

[17] Aristotle M13 1086a32-35 "... For they not only treat the Ideas as universal substances, but also as separable and particular. That this is not possible has been argued before ..." Ross translation [1], pointing thence to M4 1003a 7-17, important also M1, 987b1-b14[1] ending "... but what the participation or the imitation of Forms could be they left an open question ..." A detailed and instructive discussion of Aristotle's part in this case may be found in Laura Castelli (2013) [5], 'Universals, Particulars and Aristotle's Criticism of Plato's Forms'

[18] see above point 2 wrt the subsumption problem for proper types. It's probably fair, to assume Plato's theory of forms being far from claiming or presupposing a definite extension for (a proper type given by) a Platonic form (as its sole paradigm); at least this is the view adopted here without further reference

[19] as a short glance at the decision procedure for syllogisms by Venn diagrams shows

an independent and usable account of 'knowledge', quite different from Aristotle's, but not contradicting it.

2.3 is then here any general outcome wrt epistemic justification of 'predication knowledge claims' ?

2.3.1 basic justification ?

In simple cases, in which the subsumption problem for ″**x is (a case of) P**″ gets solved, as in the calibration example above, we get a like outcome as in the case of mapping observational data to math structures in basic measurement, considered in the first section: by solving the subsumption problem for type **P** we get a definite extension of **P**, therewith the applicability of deductive logic for **P**, thus an epistemic gain [+] in preciseness, for the prize [−], that we include or exclude borderline cases - perhaps only by (expert) decision, and maybe exclude some or all clearly intransitive instances as well.

Though, to be honest, from a realistic point of view, in general the subsumption problem for ″**x is (a case of) P**″ cannot be presupposed to be solved, and hence we won't have the (strict) applicability of deductive logic to predicates or proper types easily available.

But, as centuries of scholastic reasoning might perhaps convince us, the use of unary predicate logic, without the extension to n-place relations and -functions [, and without the arithmetization of logic in metatheory], has not been great support for promoting scientific knowledge in the past. This observation may give us some relaxation wrt the many cases, especially in humanities, where the subsumption problem does not seem solvable, at least not easily or without serious loss [20].

2.3.2 higher order justification ?

It seems then that wrt predication in sciences and humanities we mostly deal with proper types with indefinite extension, and these are often inter se categorically different, hence e.g. the justifications of claims like 'x is close to an inertial system', 'x is a megalopolis of the late 20th century', 'x is of phylogenetic type α', 'x is a case of anxiety neurosis', ... are not likely to share common justification criteria.

The justification of 'predication knowledge claims' therefore seems to remain nearly exclusively at the command of the respective science or humanities disciplines. I say, nearly exclusively, as some PoS topic like 'construct validity' (discussed mainly in psychology) may have the power of extending to say sociology and economics; besides there are issues with hermeneutics in the humanities, and across disciplines with 'emergence' cases.

To end with, wrt 'predication knowledge claims' it seems from the foregoing discussion, that it is not of highest importance to have completely solved the subsumption problem for **x is (a case of) P**, but whether the predicate or proper type **P** allows for significant (while not necessarily strict) generalizations.

[20] more important for scientific progress in the humanities seem e.g. quantifiable elements from statistics, experiments, and surveys, next formalizable structures (e.g. using graphs, structure generating algorithms, ...), next ... open list ...

Whewell's respective statement wrt natural history branches, already cited in [9] should be reconsidered seriously, it goes :

"11. It has already been repeatedly stated, as the great rule of all classification, that the classification must serve to assert general propositions. It may be asked what propositions we are able to enunciate by means of such classifications as we are now treating of. And the answer is, that the collected knowledge of the characters, habits, properties, organization, and functions of these groups and families, as it is found in the best botanical works, and as it exists in the mind of the best botanists, exhibits to us the propositions which constitute the science, and to the expression of which the classification is to serve. All that is not strictly definition, that is, all that is not artificial character, in the description of such classes, is a statement of truths, more or less general, more or less precise, but making up, together, the positive knowledge which constitutes the science. ..."
([19] Chapt. II,§11, pp. 122f.)

3 some clarifications and afterthoughts

3.1 on presenting basic measurement

3.1.1 taking a detour across the rationals

My 1981 basic measurement presentation [7] maps - perhaps somewhat unexpectedly - operational measurement results in a first step only to the field of rational numbers, and maps only in a second step the rational measurement values by homomorphic embedding from the rationals to the field of real numbers (see above **figure 1** in **section 1**)

While this last step is mathematically most trivial (kind of identity mapping), imop epistemically it's an important second step, because in an obvious sense, the rational numbers with their addition and multiplication operations and the respective inverses as well as the Archimedian property can - however rudimentarily - be pictured by measurement-operations-arrangements (for additive measurement structures), starting already with counting operational objects. E.g., the 'ruler case', as I term it in my 1981 paper, is simply the case, that some object length measured by a ruler (implementing fragmentarily the Archimedian property of a selected length unit, say inch) matches by operational scrutiny exactly for some n the length of n inch on a calibrated ruler, so we have in this case an operationally exact measurement value.

Postulating the existence of a limit for each converging sequence of rational numbers doesn't have in a like way an operational picture. But, on the other hand, the mapping to the reals is needed in order to enter the world of (mathematically modeled) physical objects, perhaps most basically - as already suggested above - in kinematics acceleration and motion.

3.1.2 knowledge gap denied - justification gap denounced

the conclusion in my 1981 paper had been, that the alleged vagueness of measurement should not be conceived as a knowledge gap or lack of knowledge, my

respective phrasing was:
" ... , it would be systematically misleading to say that "we do not know, which mathematical statements should be 'equated' with the empirical statements". There is nothing to know here. What is to be explained, is the way in which empirical statements are correlated with mathematical ones. That this correlation is not unique, and in principle cannot be unique, is simply a consequence of theory construction. ..." [7] p. 120
"... From the epistemological point of view: It turned out that there are no 'vague statements', but contrary to common belief measurement statements are not applications of mathematical terms to empirical objects. There is no such application, and no such application is needed to describe measurement. There is only a correlation of empirical statements with mathematical statements, which is not unique. ..." [7] p. 121

And while today I keep this view 'no knowledge gap' in full, now wrt epistemic justification I stress the point that but the above construction proves clearly kind of a justification gap, which is a lot bigger, when mapping the operational results to the real numbers, and imop not as big, as long as the mapping is to the rationals only. As already stated in other words, remaining justification of this seeming justification gap - as far as it can be done at all - is to be sought in the respective scientific enterprise as a whole, to whose foundation it belongs.

3.2 on handling Sorites series

My analysis of the Sorites series effect amounts to accounting for a striking dissimilarity of start and end of a similarity chain, proving similarity relations, which do such chaining, to be intransitive.

From this point of view there are at least three topics, which imop regularly get (imop undue) intermingled in one or other way in paradox suggesting descriptions of Sorites series [21], these are
(1) the non transitivity of the similarity relation, which does the similarity chaining
(2) the borderline cases
(3) the assumption of decreasing similarity in Sorites series
My clarification aim here amounts to claiming that while (1) describes a necessary condition, (2) and (3) both describe neither necessary nor sufficient conditions.

3.2.1 non transitivity of the similarity relation

According to my account of Sorites series (shortcut by my Sorites series formula **[ssf]** , and expanded by my line segments model for similarity relations), from these three a Sorites series description needs only "(1) the non transitivity of the similarity relation ...", starting the series by a paradigm example, closing the series by a clear counterexample, being clearly not similar to the starting paradigm, and a similarity chain which connects them. And this is roughly, what **[ssf]** expresses. While I do agree, that for full discussion of the Sorites phenomenon the issue of 'non transitive similarity' alone may be too poor, I'd

[21] reference for many is to e.g. Bobzien(2019) [3],Parikh(2020) [12], to Sorensen(2022) [14]

like to insist, that it belongs to the kernel of the topic, while imhop 'borderline cases' and 'monotonicity' do not. Some details :

3.2.2 the (ir)relevance of borderline cases

wrt the borderline cases, there is a switch from my account in "note on Sorites series"[8], where I did not definitely exclude borderline cases from the [**ssf**] items $\mathbf{a_i}$, which I do but now. As stated in the discussion of 'subsumption' above, as far as I can see, borderline cases are not a peculiarity of Sorites series, but may be part of the subsumption problem for any kind of type (i.e. predicates and proper types, else for transitive as well as for non transitive similarity relations. So, I think, they are in no way a proprium of cases which may suggest a 'Sorites similarity chain' or even a 'Sorites induction'). And borderline cases may also occur in basic measurement, e.g. wrt a measuring decision of "clustering around mark 'q' "[22].

Now, debating that way but does not mean to ignore or to not appreciate the discussion of borderline cases. In the opposite, their discussion is, of course, proper and important epistemic work[23], and I understand there is valuable access to this field of epistemological research with use of epistemic modal operators, e.g. in Bobzien 2019 [3] [cf the 'tolerance principle' in section 2, p.4f., and the whole of section 3 pp. 7 ff.]. Only, this discussion imop is not peculiar to Sorites series topic. The more, I 'd like to contest strongly a view, that borderline cases " ... precipitate ... the Sorites ... " , as e.g. has been suggested in the SEP entry for 'Vagueness', viz. "... There is wide agreement that a term is vague to the extent that it has borderline cases... [[14] p. 2]... Vagueness, in contrast, precipitates a profound problem: the sorites paradox..." [[14] p. 14].

In the end we may have Sorites series which do not contain a single borderline case, and which perhaps may be coped with by adjusting paradigm set and/or similarity relation.

Anyway, in my papers [8], [9] I did not focus on borderline cases and respective epistemic modalities, because my target area here is and has been the distinction between predicates and proper types, thus resuming - and systematically expanding on - the discussion that originated with Whewell and Mill in the $\mathbf{19^{th}}$ century.

3.2.3 decreasing similarity

Sorites series has also been characterized as a strictly ordered finite sequence (e.g. ordering by height of objects, number of grains in a heap of grains, ...) starting with clear examples, ending with clear counterexamples and (potential) borderline cases inmidst between[24]

Perhaps any author referring to Sorites series monotonicity would agree that this may be a somewhat idealizing assumption. I want to point out here, why I hold such monotonicity to be a not only a somewhat but an unduly strong idealizing assumption, and hence why I decided for my account of Sorites series to dispense completely with.

[22] see **figure 1**

[23] Wrt borderline cases also a lot of interesting material from a psychological point of view is presented and discussed in Nicolao Bonini et alii [4] "On the Psychology of Vague Predicates"

[24] reference here again to Bobzien 2019 wrt principles of polarity and monotonicity, cf. [3] pp. 3-4

To expand on this, again a short logical case study; for sake of transparency wrt 'monotonicity' it's constructed for 'similarity by degree'.
Suppose, we have a proper type $\mathbf{P} = \langle \mathbf{PD}, \mathbf{S} \rangle$ given by some special non transitive similarity relation $\mathbf{S}$ and a non empty paradigm set $\mathbf{PD} = \{\mathbf{z_1}, ..., \mathbf{z_l}\}$, where $\mathbf{S}$ allows for similarity by degree, $\mathbf{Sxy}$ then meaning more explicitly $\bigvee_{\rho}(\mathbf{0,5} \leq \rho \leq \mathbf{1} \wedge \mathbf{S}\rho\mathbf{xy})$, hence the relation properties of $\mathbf{S}$ (reflexivity, symmetry, non transitivity) are more explicitly given by

reflexivity:
(t1)$\bigwedge_{\mathbf{x}}(\bigvee_{\rho}(\mathbf{0,5} \leq \rho \leq \mathbf{1} \wedge \mathbf{S}\rho\mathbf{xx}))$

(t2) symmetry:
$\bigwedge_{\mathbf{xy}}[\bigvee_{\rho}(\mathbf{0,5} \leq \rho \leq \mathbf{1} \wedge \mathbf{S}\rho\mathbf{xy}) \rightarrow \bigvee_{\rho}(\mathbf{0,5} \leq \rho \leq \mathbf{1} \wedge \mathbf{S}\rho\mathbf{yx})]$

(t3) non transitivity:

$\bigvee_{\mathbf{xyz}}[\bigvee_{\rho}(\mathbf{0,5} \leq \rho \leq \mathbf{1} \wedge \mathbf{S}\rho\mathbf{xy}) \wedge \bigvee_{\rho}(\mathbf{0,5} \leq \rho \leq \mathbf{1} \wedge \mathbf{S}\rho\mathbf{yz}) \wedge \neg \bigvee_{\rho}(\mathbf{0,5} \leq \rho \leq \mathbf{1} \wedge \mathbf{S}\rho\mathbf{xz})]$

(t4) Then in any model of the proper type $\mathbf{P} = \langle \mathbf{PD}, \mathbf{S} \rangle$ and for any paradigm $\mathbf{z_{i(1 \leq i \leq l)}} \in \mathbf{PD}$ exists(is definable) the partial ordering relation $\mathbf{O_i}$

$\mathbf{O_i xy} \leftrightarrow \bigvee \rho_{\mathbf{x_i}} \bigvee \rho_{\mathbf{y_i}} [\mathbf{S}(\rho_{\mathbf{x_i}}, \mathbf{z_i}, \mathbf{x}) \wedge \mathbf{S}(\rho_{\mathbf{y_i}}, \mathbf{z_i}, \mathbf{y}) \wedge \rho_{\mathbf{x_i}} \geq \rho_{\mathbf{y_i}}]$

inheriting reflexivity and transitivity from $\geq$

(t5) and exists(is definable) the equivalence relation $\mathbf{E_i}$

$\mathbf{E_i xy} \leftrightarrow \bigvee \rho_{\mathbf{x_i}} \bigvee \rho_{\mathbf{y_i}} [\mathbf{S}(\rho_{\mathbf{x_i}}, \mathbf{z_i}, \mathbf{x}) \wedge \mathbf{S}(\rho_{\mathbf{y_i}}, \mathbf{z_i}, \mathbf{y}) \wedge \rho_{\mathbf{x_i}} = \rho_{\mathbf{y_i}}]$

inheriting reflexivity, symmetry and transitivity from =

(t6) Then each pair of relations $\langle \mathbf{O_i}, \mathbf{E_i} \rangle$ forms a comparative concept for 'similarity to paradigm $\mathbf{z_i} \in \mathbf{PD}$' in the model, showing

$\bigwedge \mathbf{x} \bigwedge \mathbf{y}[(\mathbf{O_i xy} \wedge \mathbf{O_i yx}) \rightarrow \mathbf{E_i xy}]$
weak antisymmetry for the ordering relation $\mathbf{O_i}$ [25].

But for a Sorites series in the model there is no guarantee, that for some such comparative concept $\langle \mathbf{O_i}, \mathbf{E_i} \rangle$ the members of the Sorites series share one of the chains ordered by descending degree of similarity wrt $\mathbf{O_i}$. As noted above, a single Sorites series always starts from a single paradigm, not from multiple paradigms, hence, wrt the likelihood for a Sorites series to be ordered by degree of similarity, we are done.
But of course it's the general case, considering similarity ordering wrt any non empty set of paradigms, which were of interest, if one would intend to study similarity orderings in say some humanities discipline example (as considered in [9]) .

[25] Of course, the equivalence relation $\mathbf{E_i}$ might be replaced by a more liberal one, by selecting the union of some equivalence classes of $\mathbf{E_i}$

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