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ON MATHEMATICAL REALISM AND APPLICABILITY OF HYPERREALS

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We argue that Robinson’s hyperreals have just as much claim to applicability as the garden variety reals.

In a recent text, Easwaran and Towsner (ET) analyze the applicability of mathematical techniques in the sciences, and introduce a distinction between techniques that are *applicable* and those that are merely *instrumental*. Unfortunately the authors have not shown that their distinction is a clear and fruitful one, as the examples they provide are superficial and unconvincing. Moreover, their analysis is vitiated by a reliance on a naive version of object realism which has long been abandoned by most philosophical realists in favor of truth-value realism. ET’s argument against the applicability of hyperreals based on automorphisms of hyperreal models involves massaging the evidence and is similarly unconvincing. The purpose of the ET text is to argue that Robinson’s infinitesimal analysis is merely instrumental rather than applicable. Yet in spite of Robinson’s techniques being applied in physics, probability, and economics (see e.g., [70, Chapter IX], [1], [76], [60]), ET don’t bother to provide a meaningful analysis of even a single case in which these techniques are used. Instead, ET produce page after page of speculations mainly imitating Connesian chimera-type arguments ‘from first principles’ against Robinson. In an earlier paper Easwaran endorsed real applicability of the σ -additivity of measures, whereas the ET text rejects real applicability of the axiom of choice, voicing a preference for **ZF**. Since it is consistent with **ZF** that the Lebesgue measure is *not* σ -additive, Easwaran is thereby walking back his earlier endorsement. We note a related inaccuracy in the textbook *Measure Theory* by Paul Halmos. ET’s arguments are unacceptable to mathematicians because they ignore a large body of applications of infinitesimals in science, and massage the evidence of some crucial mathematical details to conform with their philosophical conclusions.

1. Varieties of realism. In a text entitled “Realism in Mathematics: The Case of the Hyperreals” [33], K. Easwaran and H. Towsner (henceforth ET) deal with the following two general questions related to the issue of mathematical realism: (1) Which mathematical claims can be taken as meaningful and true within mathematics? (2) Which mathematical ideas can be taken as applying to the physical world as part of scientific theory?

For the special case of hyperreals ${}^*\mathbb{R}$ in the sense of Robinson’s framework for analysis with infinitesimals and related approaches, these questions are answered by ET as follows:

- (1)* The hyperreals ${}^*\mathbb{R}$ are meaningful mathematically since their existence can be ensured within mathematics by the axiom of choice (**AC**). ET consider **AC** to be true.

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(2)* In contrast to the reals \mathbb{R} , the hyperreals ${}^*\mathbb{R}$ have no real application in science as part of a scientific theory. Hyperreals may only be used instrumentally, as ‘computational tools,’ but they do not correspond to anything that exists in the real world. Real numbers correspond directly to objects existing in the real world and therefore are to be considered as applicable.

ET consider item (1)* as a claim that supports mathematical realism since it ensures a certain kind of existence of certain mathematical entities. Namely, hyperreals exist within the universe of mathematical entities. Complementarily, ET conceive item (2)* as an anti-realist claim, since it denies a certain kind of existence to ${}^*\mathbb{R}$. More precisely, hyperreals do not exist in the real world, since the hyperreals are not part of a (true) scientific theory of the real world. Only real numbers exist, since they are part of (true) scientific theories of the real world.

According to ET, this ‘mixed’ character of their account of (mathematical) realism shows that ET’s brand of realism is more balanced than other purely ‘realist’ or purely ‘anti-realist’ accounts.

In this paper we argue that items (1)* and (2)* actually provide evidence for something quite different, namely, that ET subscribe to an outdated and simplistic account of mathematical realism (and of scientific realism as well) that fails to meet the criteria of current literature in contemporary philosophy of mathematics and philosophy of science. We will first argue for these claims for mathematical realism. Then we show that ET not only miss the point for mathematical realism but for scientific realism as well.

More precisely, ET subscribe to an *ontological realism* in mathematics that is exclusively concerned with the question of the *existence* of mathematical objects, be it the existence within mathematics or outside mathematics. For some time, this ontological question (be it in mathematics or in science) is no longer the only game in town.

1.1. Dummett, Shapiro, Putnam. With respect to mathematical realism it is generally admitted that there are at least two different realist themes in contemporary philosophy of mathematics. According to the first, mathematical objects exist independently of mathematicians, their minds, languages, and so on. This is the only version of realism ET deal with. It may be succinctly characterized as an ontological or *object realism*. A unique feature of ET’s approach is their quest for “numbers that correspond to the world,” not found in, and a likely embarrassment for, any object realist such as Colyvan [23].

The second theme in contemporary mathematical realism centers around the thesis that mathematical *statements* have objective truth-values independent of the minds, languages, conventions, and so forth, of mathematicians. This realism may be called truth-value realism or *objectivity realism*. It can be characterized as a realism that centers around the objectivity of mathematical discourse. According to it, the interesting and important questions of philosophy of mathematics are not over mathematical objects, but over the objectivity of mathematics (Shapiro [73], 1997, p. 37).

Truth-value realism does not center around an *ontological* view thesis. Although truth-value realism claims that many mathematical statements have unique and objective truth-values, it is not committed to a distinctively platonist idea that such truth-values are to be explained in terms of an ontology of mathematical objects. A related challenge to the uniqueness of ontology of mathematical objects was developed by Benacerraf [5].

Philosophers of quite different orientations have argued for objectivity realism in mathematics. Even if they disagree widely on the various issues related to the existence of mathe-

mathematical objects, virtually all converge in the assertion that mathematics is the *objective* science par excellence. Therefore they consider it a central task of philosophy of mathematics to elucidate what exactly is meant by this assertion. As Michael Dummett put it succinctly: “What is important is not the existence of mathematical objects but the objectivity of mathematical statements” [29, p. 508].

A prominent example of a philosopher of mathematics who argued vigorously for a truth-value realism in mathematics is Hilary Putnam. Although Putnam was notorious for changing his philosophical views a number of times, with respect to the issue of a non-ontological realism in mathematics his convictions have remained remarkably stable. For forty years in the course of his entire philosophical career he insisted on a non-ontological realism. Already in his *What is mathematical Truth?* he put forward the thesis that “The question of realism is the question of the objectivity of mathematics and not the question of the existence of mathematical objects” (Putnam [67], 1975, p. 70).

In “Indispensability Arguments in the Philosophy of Mathematics” (Putnam [68], 2012) he insisted that his well-known *indispensability argument* should be understood as an argument for the objectivity of mathematics, and not as an argument for a platonist interpretation of mathematics. The point is reiterated in his posthumously published essay in 2016: “one does not have to ‘buy’ Platonist epistemology to be a realist in the philosophy of mathematics. The modal logical picture shows that one doesn’t have to ‘buy’ Platonist ontology either” [69, p. 345].

1.2. Maddy on varieties of realism. Some years ago, Penelope Maddy in her book *Defending the Axioms. On the Philosophical Foundations of Set Theory* (Maddy [62], 2011) took up the issue of mathematical realism. In *Defending the Axioms* we find a detailed and balanced discussion of the two themes of *object realism* versus *objectivity realism* in mathematics as presented by Shapiro, Dummett, Putnam and others; cf. Maddy (2011, chapter (V.1)).

ET refer to Maddy as well but in a rather unfortunate way. They do not mention Maddy (2011), but seek to use some of her earlier work to render plausible their distinction between real application and merely instrumental use. ET write: “As Penelope Maddy says about physics, ‘its pages are littered with applications of mathematics that are expressly understood not to be literally true: e.g., the analysis of water waves by assuming the water to be infinitely deep or the treatment of matter as continuous in fluid dynamics or the representation of energy as a continuously varying quantity.’ [Maddy, 1992, p. 281]” [33, p. 5]

Yet, as (Maddy 2011) shows, she is no longer interested in arguing for the existence of mathematical objects analogous to the existence of “planets and atoms and giraffes, independently of the human mind” [33, p. 1], as ET put it. Quite generally, the issue of which numbers ‘correspond to the world’ is not one that interests many philosophers of mathematical realism today.

In her book Maddy is mainly interested in the objectivity of set theory. Even if the question of set-theoretic axioms and their ontological status were to be settled to the satisfaction of philosophers, and even if all objects of mathematics could be reconstructed as sets, there is no reason to assume that thereby all problems of philosophy of mathematics would be solved. One may ask: “Do groups, Riemannian manifolds, Hilbert spaces objectively exist?” For instance, did groups as mathematical objects come into being in the second half of the 19th century, or did they exist since times immemorial as may be suggested by the example of finite symmetry groups of geometrical figures (see e.g., Wussing [78])?

It seems plausible that philosophy of mathematics is more than philosophy of set theory.

The objectivity of mathematics should not be discussed in the overly abstract realm of set theory alone. Problems of mathematical realism do not only arise for sets, even if, arguably, every mathematical object could be reconstructed as a structured set. Indeed, dealing with the question ‘Are there groups?’ or ‘Are there Riemannian manifolds?’ instead of the analogous, rather wornout questions ‘Are there sets?’ or ‘Are there numbers?’ has some advantages, and not only didactic ones.

1.3. On the dialectics of tool and object. If science is a *Werdefaktum* (‘Fact in Becoming’ as the neo-Kantians such as Cassirer claimed) the objectivity of mathematical discourse may have to be conceived as a ‘fact in becoming’ as well, depending on the historically evolving practice of mathematical discourse (see [63, Section 2.1]). The mathematical practice may change the nature of mathematical objects: they may change from mere tools to full-fledged objects. From this perspective also the history of mathematics (and science) becomes important for the issue of mathematical realism.

1.4. Maddy on history of mathematics and science. (*Defending the Axioms*, pp. 27–29). One clear moral for mathematics in application is that we are not in fact uncovering the underlying mathematical structures realized in the world. Rather, we are constructing abstract mathematical models and endeavoring to make true assertions about the ways in which they do or do not correspond to physical facts. There are rare cases where such a correspondence is something like isomorphism, as for elementary arithmetic and simple combinatorics and there are probably others, like the use of finite group theory to describe simple symmetries. However, in most cases the correspondence is something more complex.

For the relation between the physical and the mathematical, an isomorphism is the great exception. Moreover, ET ignore the most essential aspects of mathematical conceptualisation. The correspondence between the empirical and the mathematical (ideal) is more complex. Cassirer drew the conclusion that not the existence of objects, but objectivity of the method is important; see further in Section 1.5.

ET rely on an outdated model of the relation between mathematics and physics proposed already by Newton and Galileo, namely, that mathematics can and does discover the mathematical structure of the world, or better, that the world has a unique mathematical structure. Such a view is related to the well-known Cantor–Dedekind postulate according to which the real mathematical line actual describes the physical line, a position untenable in view of contemporary knowledge in physics (see Sections 2 and 2.16).

Since the application of mathematical concepts typically amounts to an idealisation, mathematical concepts have no direct correspondence in the ‘real world’. This holds even for a seemingly elementary concept such as the natural numbers \mathbb{N} . After all, it is an essential feature of \mathbb{N} that it is an infinite structure, and clearly, even the ‘small’ infinity of \mathbb{N} has no direct correspondence in the ‘real world’; see further in Section 2.8.

1.5. Cassirer on object and objectivity. Maddy and most other contemporary philosophers of mathematics, having been educated in the Anglo-Saxon tradition of analytic philosophy, tend to attribute the merit of having shifted the attention of philosophers of mathematics from questions of object realism to questions dealing with the objectivity of mathematics to a rather obscure and unelaborated remark of Georg Kreisel’s; see e.g., [62, p. 115, note 4]. However influential Kreisel’s remark may have been in recent analytical philosophy of mathematics, Kreisel was by no means the first to note the relevance of distinguishing between a realism concerning mathematical objects and a realism concerning the objectivity of mathematical discourse.

Already in the first decade of the 20th century, the neo-Kantian philosopher Ernst Cassirer had elaborated in his *Substance and Function* ([20], 1910) a neo-Kantian philosophy of science and mathematics that emphasized (for both mathematics and natural science) the importance of distinguishing between two interpretations of realism, namely, object realism and truth-value (objectivity) realism.

It is not very surprising that Cassirer's philosophy of mathematics has been virtually ignored in the quarters of analytic philosophy. For a long time Cassirer has been classified as a partisan of some stripe of continental idealism, giving analytic philosophers an excuse not to take his work seriously.

The underlying reason for such a lamentable state of affairs may have been that in traditional 20th century Anglo-Saxon philosophy there is a conviction that idealist philosophy on the one hand and serious science and philosophy of science on the other do not go well together. Often, idealism plays the role of a strawman to whom all the vices are attributed that one wishes to criticize. In the 21st century one still finds philosophers such as Susan Haack who propagate virtually the same caricature of idealism that Scheffler put forward almost 50 years ago: "An idealist holds that everything there is, is mental: that the world is a construction out of our, or, in the case of the solipsist, his own, ideas – subjective idealism; . . . or that the world is itself of a mental or spiritual character – objective idealism, as in Hegel." (Haack [44], 2002, p. 70)

Evidently, for Haack idealism is not an option to be taken seriously. For her, idealism is the bogey of realists. For some time, however, such an attitude has turned out to be increasingly untenable due to the fact that Cassirer's neo-Kantian philosophy of science is re-evaluated as a serious competitor to the classical analytical philosophy of science, namely, the logical empiricism of the Vienna Circle and related groups such as Reichenbach's Berlin group; see e.g., [22], [41], [42].

The starting point of Cassirer's account of *critical idealism* is the insight that the 'object' of scientific knowledge - be it mathematical or empirical knowledge - should not be conceived as a kind of Kantian 'thing-in-itself' beyond all possible experience. Rather, "If we determine the object not as an absolute substance beyond all knowledge, but as the object shaped in progressing experience, we find that there is no 'epistemological gap' to be laboriously spanned by some authoritative decree of thought, by a 'transsubjective command.' For this object may be called 'transcendent' from the standpoint of a psychological individual; from the standpoint of logic and its supreme principles, nevertheless it is to be characterized as purely immanent." It remains strictly within the sphere, which these principles determine and limit, especially the universal principles of mathematical and scientific knowledge. This simple thought alone constitutes the kernel of critical 'idealism'." (*Substance and Function* [20], 1910, p. 297)

For Cassirer, knowledge never starts with well-determined objects that are 'given' to a cognizing subject. Rather, objects are the result of an objectifying process: "To know a content means to make it an object by raising it out of the mere status of givenness and granting it a certain logical constancy and necessity. Thus we do not know 'objects' as if they were already independently determined and given as objects, - but we know objectively, by producing certain limitations and by fixating certain permanent elements and connections within the uniform flow of experience. The concept of the object in this sense constitutes no ultimate limit of knowledge, but is rather the fundamental instrument, by which all that has become its permanent possession is expressed and established. The object marks the logical possession of knowledge, and not a dark beyond forever removed from knowledge."

(*Substance and Function*, p. 303)

Perhaps one may say that for Cassirer an ‘object’ is generated by a scientific method of objectification. Thus (valid) scientific concepts do not aim to produce ‘copies’ of pre-existing objects; rather, “Scientific . . . concepts are valid, not in that they copy a fixed, given being, but in so far as they contain a plan for possible constructions of unity, which must be progressively verified in practice, in application to the empirical material. But the instrument, that leads to the unity and thus to the truth of thought, must be in itself fixed and secure. . . . *We need, not the objectivity of absolute things, but rather the objective determinateness of the method of experience*” (*Substance and Function*, p. 322; emphasis added).

This summary of Cassirer’s position may suffice to provide evidence that his ‘critical idealism’ has little in common with the simplistic caricature of idealism as found in Haack’s description of this philosophical current in [44, p. 70]. Moreover, Cassirer offers us a more elaborate account of the interplay of objects and objectivity in the practice of science than Kreisel’s succinct remark that bluntly asserts that for mathematics its objectivity is more important than the existence of its objects. Cassirer’s critical idealism treats mathematical and scientific realism on a par: for both areas he argues for a truth-value realism as opposed to an ontological object-centered realism. This should be considered as a virtue since it enables us to overcome the boundaries between mathematics and the natural sciences that in the age of an ever more mathematicized science are becoming ever more artificial and obsolete.

2. Applicable vs instrumental. As we already mentioned in Section 1, ET pursue a distinction between mathematical ideas meaningful within mathematics and those meaningful within the sciences. Section 1 dealt with philosophical shortcomings of their approach. In this section we will focus on the mathematical shortcomings of their analysis.

ET focus on “the question of which mathematical claims can be taken as meaningful and true *within mathematics*, and which mathematical ideas can be taken as *applying to the physical world* as part of a scientific theory” [33, p. 1] (emphasis added).

They also pursue a related distinction between mathematical techniques that can be described as *applicable* (such as Newtonian mechanics or quantum mechanics) and ones that are merely *instrumentally* useful in modeling. ET take the superior *applicable* techniques to “accurately describe” (see Section 2.4) or “correctly model” the world.

They seek to apply such an *applicable vs instrumental* dichotomy to an appraisal of Robinson’s framework [70] for analysis with infinitesimals. The main thrust of their text is the claim that real numbers are applicable whereas hyperreal numbers are merely instrumental. We argue that the ET argument fails at several levels, as follows.

1. (*Realism à l’ancienne*) ET claim that they adopt the philosophical position of scientific *realism* and pursue the issue of which numbers “correspond to the world”(see e.g., Section). However, the issue as stated is not a relevant issue in contemporary trends in realism in the philosophy of mathematics, as analyzed in Section 1.1. The current literature addresses not the issue of which mathematical *objects* correspond to the world, but rather the issue of the *objectivity* of mathematical discourse; see Section 2.13.
2. (*Representing the world and Cantor–Dedekind*) ET seek to drive a wedge between the applicability of the reals and that of hyperreals on account, in their words, of providing “representation of part of the world” ([33, p. 9]), but their stance is conditioned on the adoption of the Cantor–Dedekind postulate. The latter involves an identification of a

Cantor–Dedekind real line as they understood it with a physical line. Such a stance is untenable in view of contemporary knowledge in physics; see Sections 2.10 and 2.16.

3. (*Elegance über alles*) Well into their argument, ET pull out of a hat a criterion of the *elegance* of a mathematical framework (a fine criterion, to be sure; see Section 3.1). They use it to justify a reliance on a standard uncountable *number system* of cardinality \mathfrak{c} . But ET fail to apply their *elegance* criterion consistently when it comes to adopting a standard *structure* of cardinality \mathfrak{c} over the traditional reals; see Section 3.6. ET thus display a double standard indicating a partiality of their analysis.
4. (*Physics to the rescue*) ET claim that “an applicable use of the hyperreals would require substantial new developments in physics.” However, such a claim ignores the existence of numerous such applications as developed e.g., in Albeverio et al. [1]; see Section 4.1 for additional sources.
5. (*Applicability of \mathbf{AC} -dependent entities*) ET claim that their objections to using hyperreals in mathematical models in physics apply also to other entities whose existence cannot be proved in Zermelo–Fraenkel set theory (\mathbf{ZF}) without the use of the Axiom of Choice (\mathbf{AC}). However, they focus their critique on Robinson’s framework, and ignore the existence of many applications relying on other entities commonly used in traditional mathematics, whose existence cannot be proved without exploiting \mathbf{AC} -related principles.
6. (*Walking back earlier endorsement*) ET’s position is at odds with an earlier endorsement of σ -additivity by Easwaran. For a discussion on the applicability of the Lebesgue measure without \mathbf{AC} and on other consequences of accepting at face value ET’s claim that their objections to the hyperreals apply also to mathematical entities independent of \mathbf{ZF} , see Sections 4.5 through 4.12.
7. (*Undercutting and rebutting*) We will exploit a dichotomy of *rebutting* vs *undercutting* developed by Easwaran in [32]. Briefly, *rebutting* an argument involves showing that its conclusions contradict those reached in other work published in reliable venues, whereas *undercutting* involves finding gaps in the argument itself. We will use the dichotomy to analyze the ET text [33].

2.1. Chimera and dart strategies. To elaborate further on the last item (7), note that a related dichotomy contrasts two possible strategies for challenging the applicability of a particular fragment of mathematics:

- (Co1) (the “chimera” strategy) Argue from first mathematical principles that there are built-in shortcomings in the fragment of mathematics that would disqualify it from applications;
- (Co2) (the “dart” strategy) Analyze a particular attempt to apply the fragment of mathematics to a specific area of science and show concretely how it falls short of the mark.

An example of the *chimera* strategy (Co1) is Connes’ attempt in [25, p. 14] to exploit the Solovay model so as to argue that hyperreals are chimeras. His attempt is evaluated in [50, pp. 263, 278, 279 and Section 8.2, p. 287].

Another example of strategy (Co1) involves the *basic* form of Smooth Infinitesimal Analysis (\mathbf{SIA}). The basic form has only nilsquare infinitesimals, which may hinder applications of the *basic* form of \mathbf{SIA} in more advanced applications in physics and geometry where second-order Taylor expansion and second differentials appear, such as Newtonian mechanics or curvature of curves in differential geometry. Note that there are more advanced versions

of **SIA** possessing nilpotent infinitesimals of higher order than 2 (see [57]); our purpose here is merely to give an example illustrating the chimera strategy (Co1).

An example of the *dart* strategy (Co2) is Connes' attempt in [25, p. 13] to find fault with hyperreals in a specific application involving throwing darts at a target; his attempt is evaluated in [50, Section 8.1, pp. 286–287].

Between 1994 and 2007, Connes used to offer apriori chimera-type arguments ‘from first principles’ against hyperreals. In 2013, two articles appeared analyzing Connesian *chimera* critiques: Kanovei et al. [50] and Katz–Leichtnam [53].

Connes has toned down his approach since then. His most recent piece touching on the nature of infinitesimals no longer seeks to contrast the respective infinitesimals of Connes and Robinson but rather those of Newton and Leibniz. Connes claims that to Newton, an infinitesimal was a variable taking determined values and tending to zero, whereas to Leibniz, it was a number (we will refrain from commenting on the historical merits of such a claim). Thus, Connes writes that his own “new set-up immediately provides a natural home for the ‘infinitesimal variables’: and here the distinction between ‘variables’ and numbers (in many ways this is where the point of view of Newton is more efficient than that of Leibniz) is essential. It is worth quoting Newton’s definition of variables and of infinitesimals, as opposed to Leibniz: [‘]In a certain problem, a variable is the quantity that takes an infinite number of values which are quite determined by this problem and are arranged in a definite order[’]” (Connes [26], 2018, p. 168).

In sum, the difference between Newton/Connes and Leibniz/Robinson now boils down to the fact that Connes leaves a null sequence alone (or more precisely replaces it by a compact operator with the corresponding spectrum), whereas Robinson carries out a quotient space construction involving a nontrivial equivalence relation, resulting in infinitesimals that are numbers and not merely sequences.

Unlike Connes, Easwaran continues his attempts to market Connesian *chimera* arguments from first principles, aimed against Robinson. Since Robinson’s infinitesimal analysis requires **AC** or some weaker form of it in order to develop the hyperreals, Easwaran seeks to undermine the legitimacy of Robinson’s framework by attacking that of **AC** (see further in Section 4.4).

This, however, constitutes philosophical opportunism because in an earlier paper, Easwaran sought on the contrary to defend the real applicability of countable additivity, which depends on **AC** just as a hyperreal field does; see Section 4.5.

2.2. ET vs Lotka–Volterra. ET provide an example of a strategy of type (Co2) in the context of the Lotka–Volterra theory (LV), a model of predator-prey population dynamics. They claim that LV is only *instrumental* rather than applicable, on the grounds that the real number resulting from a solution of the LV differential equations is in general noninteger, and a noninteger obviously cannot literally represent the size of a mammal population.

This is an example of an ET attempt to exhibit a shortcoming of the fragment of mathematics represented by LV. We argue that it is not a successful example in any meaningful sense, since the trivial modification [**LV**] defeats the non-representability objection; see Section 2.5.

In fact ET focus almost exclusively on strategy (Co1) when it comes to challenging Robinson’s framework. Namely, ET seek to argue inapplicability from first mathematical principles (rather than examining specific applications). In this sense, they are committing the same fallacy as Connes before them, who sought to base a refutation on Solovay models, undefinability, etc.

We argue that strategy (Co1) is rarely a legitimate way of refuting the applicability of a mathematical theory (unless one is dealing with pseudoscience like Sergeyev's and Rizza's; see [43], [51], [56], [74]). If so, the ET effort fails on two counts: (a) it misses the mark on what it takes to challenge a fragment of mathematics in a meaningful fashion; (b) the critique is grounded in an obsolete version of the philosophy of object realism.

2.3. Axiom of choice. In connection with the status of the axiom of choice (**AC**), ET write: “[W]hether or not all well-formed mathematical statements have a definite fact, there is a fact about the Axiom of Choice, and the fact is that *it is true*, as well as all of its consequences” [33, p. 2] (emphasis added).

It may be helpful to clarify the context of this remark. There is a spectrum of opinions among mathematicians as to the exact status of **AC**. One of possible positions is to postulate **AC** as being *true*, as ET have done.

Easwaran argued in [31] against the use of hyperreal fields on the grounds of their alleged reliance on **AC**: “My claim is . . . that no physical facts could make one of these infinitesimals rather than another be the credences of a particular agent. Although the Axiom of Choice guarantees that such hyperreal-valued functions exist, and although these functions are quite useful to talk about in mathematical contexts, they have mathematical structure that goes beyond that of credences. None of this rules out a certain *instrumental* use of hyperreals” [31, p. 33] (emphasis added).

We note that declaring **AC** to be *true* may be a good instrumental attitude while doing professional mathematics, but relying on such an assumption while doing professional philosophy tends to *undercut* the authors' credibility, because it confuses syntax and semantics.

While ET adopt the position of mathematical realism, theirs is presumably intended to be a sophisticated kind of realism publishable in respectable philosophical venues like *Philosophical Review* where [31] was published. A philosophically sound position (whether realist or not) would have to account for the difference between theory and model. Meanwhile, **AC** is part of the former and therefore can't be declared *true* in any but an instrumental sense. Philosophical realists like Putnam and Maddy don't subscribe to naive notions of the kind involved in declaring this or that axiom to be true, but rather argue for the objectivity of mathematical discourse; see Sections 1.2 and 2.13. Furthermore, what ET write here about **AC** is at odds with what Easwaran wrote elsewhere about the real applicability of σ -additivity; see Section 4.5.

2.4. Accurately describing the world. ET write: “The question is about the extent to which the theories we build using various purported mathematical entities *accurately* describe the world, or are merely instrumentally useful in modeling the world and making observational predictions” [33, p. 2] (emphasis added).

This passage is found in their Section 1.2 entitled *Applicability*. The unique reference ET provide in this section is Chakravartty [21].

However, Chakravartty's Stanford entry is about *scientific realism*. For him, scientific realism means realism concerning the empirical sciences, i.e., physics, chemistry, biology etc. He explicitly asserts: “Scientific realism is a realism about whatever is described by our best scientific theories.” In passing, he mentions other kinds of realisms, for instance ‘external world realism,’ ‘sense datum realism,’ ‘mathematical realism.’ None of these varieties of realisms are treated by Chakravartty. Thus, none of the terms used by Chakravartty in the Stanford article are directly applicable to the realism ET are interested in, namely, mathematical realism. Thus, the ET reference to Chakravartty's article is gratuitous.

Furthermore, the ET quest for aspects of mathematical models that would “accurately describe the world” is arguably a quixotic one (see Section 1.4). We will interpret the ET position charitably as a search for a dichotomy between *applicable* (rather than *accurate*) fragments and merely *instrumental* fragments. However, ET don’t provide any convincing examples to motivate their dichotomy. Their example of mammal-counting (see Section 2.5) fails to deliver on its promise. The absence of well-motivated examples *undercuts* the seriousness of their thesis.

2.5. [Lotka–Volterra]. In connection with the study of predator-prey population dynamics, ET write: “As a simple example, the structure of the natural numbers seems to accurately represent counts of predators and prey (at least, when talking about mammals or birds or other macroscopic animals with clear individuation). But while the real-valued differential equations of the Lotka–Volterra model can often be useful in predicting or understanding the ways the predator and prey populations will change, the infinite precision of various non-integer values that show up are not taken to represent the actual numbers of predators or prey in the ecosystem” [33, p. 2].

Here ET seek to establish a contrast between mammal-counting and Lotka–Volterra modeling of a mammal population in an ecosystem. However, their pastoral example is unconvincing because one can simply modify the outcome of the Lotka–Volterra (LV) solution by applying the integer part (floor) function. With respect to the resulting model, that we will denote [LV], one achieves both of the following objectives:

1. The modified model [LV] yields an integer for an answer; and
2. the mathematical essence of the LV model is unchanged in passing from LV to [LV].

The modification we introduced in no way affects the issue of the alleged inadequacy of the Lotka–Volterra model in describing “actual numbers of predators or prey in the ecosystem” as ET put it. This *undercuts* their argument since it highlights ET’s failure to exhibit any meaningful shortcoming on the part of the Lotka–Volterra model that would make it *instrumental* rather than *applicable*.

ET return to the LV theme later in the article: “[B]iologists often represent populations of predator-prey systems with the Lotka–Volterra differential equations, using real numbers (rather than integers) to count populations” [33, p. 5]. This recurring example is not helpful if their goal is to convince the reader that they propose a meaningful dichotomy, because the modified [LV] solution does have integer values, defeating their pastoral distinction.

2.6. Vague predicates. While it is true that the number of mammals in a given ecosystem or habitat will be a natural number, the number assigned can not be warranted as an accurate description. Indeed, a predicate like “in a given habitat” is not an exact predicate, since it admits borderline cases.

The observer doing the counting has to make conventional decisions about which individuals to count as satisfying the predicate. His decisions constitute the first step of idealisation. Animals move in and out of habitats just as people move in and out of a crowd. When one starts counting species, as ecologists do, things get more complicated. Thus, idealisation is involved regardless of whether \mathbb{N} or \mathbb{R} is used as a basic number system.

The question of accurate modeling of population dynamics is addressed in a more subtle manner than what is found in [33] in both undergraduate textbooks [61] and research articles [18].

2.7. Are infinitesimals added to \mathbb{R} or found inside \mathbb{R} ? In connection with number

systems used in scientific modeling, ET write: “The hyperreals are an extension of the real numbers to include infinitesimal numbers, etc.” [33, p. 2].

Such a viewpoint is a common one and accurately describes many applications of infinitesimals. Significantly, ET’s philosophical objections are contingent upon such an *extension* view. However, there is another approach that does not view the theory incorporating very small numbers as such an *extension*, namely Edward Nelson’s approach [64] as summarized in Section 2.8.

2.8. Nelson’s view. In Nelson’s framework, one works within the ordinary real line and finds numbers that behave like infinitesimals there via a foundational adjustment. Such an adjustment involves an enrichment of the language of set theory through the introduction of a one-place predicate **st** called “standard,” together with additional axioms governing its interaction with the axioms of Zermelo–Fraenkel set theory (**ZFC**); see Fletcher et al. [38], Katz–Kutateladze [52], Lawler [58] for details.

The predicate **st** can be viewed as a formalisation of Leibniz’s distinction between assignable and inassignable numbers; see [55], [3]. Arguably, Nelson’s formalisation of mathematical analysis as practiced from Leibniz until Cauchy is more faithful than the formalisation developed by Weierstrass and his followers, who had to discard infinitesimals since they were unable to account for them in a satisfactory fashion.

In Nelson’s framework *Internal Set Theory* (**IST**), an infinitesimal ϵ is a *real* number such that $|\epsilon|$ is smaller than every standard positive real number. Similarly, a nonstandard natural number $n \in \mathbb{N}$ is greater than every standard natural number.

The possibility of viewing infinitesimals as being found within \mathbb{R} itself pulls the rug from under ET’s philosophical criticisms anchored in the *extension* view. Indeed, whatever argument ‘from first principles’ ET can put forth against hyperreal fields, it must apply against the real field, as well, because Nelson’s approach takes place within the real field itself, and is therefore immune to ET’s objections.

For instance, *since the only order-preserving automorphism of \mathbb{R} is the identity*, nontrivial automorphisms of ${}^*\mathbb{R}$ in the *extension* approach are not internal, and therefore don’t exist in Nelson’s approach, dissolving ET’s objection based on automorphisms.

In sum, what ET present as arguments from first principles turn out to depend on technical choices in set-theoretic foundations, namely whether to work with a language limited to $\{\in\}$ or the richer language $\{\in, \mathbf{st}\}$ of Nelson’s **IST**, a conservative extension of **ZFC**.

2.9. Cardinality fallacy. ET write in a parenthetical remark: “We assume, however, that the language is countable, since we are concerned with theories which will ultimately be used to describe physical situations” [33, p. 3].

It becomes clear in [33, Section 3.4, p. 9] that when ET mention countable languages, they refer to countably infinite languages, as opposed to uncountably infinite ones; see Section 3.6 below for details.

There is a curious assumption here involving an implied connection between the countable cardinality on the one hand and what ET refer to as “physical situations” on the other. The bland assumption that infinity (even countable) can have literal meaning with respect to “physical situations” seems unwarranted. Have ET enumerated countably many physical entities? The assumption ties in with the unique aspect of ET object realism discussed in Section 1.1.

2.10. Actual real number. ET’s view concerning the comparative reality if the reals as compared to the hyperreals is as follows: “[T]he hyperreals extend the reals with infinite

numbers: there is a real ω such that $r < \omega$ for every actual real number r ” [33, p. 3].

Note that in this passage, ET refer to ω as a “real number”. Such an approach is consistent with Nelson’s viewpoint (see Section 2.8), one that ET apparently don’t wish to adopt, *undercutting* their argument. Furthermore, what exactly is an “actual real number r ”? The passage seem to suggest that ET subscribe to the Cantor–Dedekind postulate, involving an identification of a Cantor–Dedekind real line (as Cantor and Dedekind understood it) with a physical line; see [38] for a discussion. Such a stance is untenable in light of contemporary knowledge in physics; see Section 2.16.

Furthermore, ET “think there are some features that mathematical models of physical phenomena need to have to be taken in a realist way, and there are principled reasons to think that hyperreal models will usually lack these features” [33, pp. 5–6].

However, whatever aspect of idealisation ET identify in hyperreal fields, will also be present in the real field (see Section 2.8). Therefore, while their “principled reasons” may be valid, they fail to come to their aid in driving a wedge between applicability of reals and applicability of hyperreals.

2.11. Reals are ideal like hyperreals. In a similar vein, ET write: “We don’t deny that hyperreals could figure in models of this sort - we merely assert that when they are so used, we should recognize them as idealizations that *don’t correspond to the world* in the way that other parts of the models do” [33, p. 5] (emphasis added).

The ET comment about the idealisation aspect of hyperreal fields is true enough, but it is equally true about the real field, unless one adopts the Cantor–Dedekind postulate (see Section 2.10). This observation *undercuts* the ET argument.

3. Mathematical realism, automorphisms. The authors’ stated pursuit of the philosophy of mathematical realism fails to meet the criteria of current literature in the field.

3.1. Maddy to the rescue. In a passage quoted in Section 1.2, ET rely on Maddy’s 1992 text concerning idealisation in mathematical physics. However, Maddy’s position has evolved significantly since 1992. Particularly, in 2011 Maddy published *Defending the axioms* that analyzes important distinctions for the discussion of realism in mathematics, such as *objectivity* of mathematical discourse versus realism concerning mathematical *objects*; see [62, pp. 115–116]. The issue of which numbers “correspond to the world” (see Section) is not one that interests many philosophers of mathematical realism today.

3.2. The realist/antirealist Potemkin village. ET write: “An *anti-realist* who claims that all scientific theories are merely models of this sort, with no clear distinction between the representational and the fictional parts of the theories, may deny the *cogency* of the distinction we are interested in. But if one accepts a distinction between the realism of counting mammals with integers and the instrumentalism of counting mammals with real numbers, then one accepts a distinction of the form we would like to use” [33, p. 5] (emphasis added).

Here ET are attempting to hide behind a figleaf of a traditional realist vs antirealist dichotomy in an attempt to score a point, but the figleaf is transparent. One needn’t be an antirealist to doubt that ET have formulated a “cogent” distinction (as they put it) between applicability and instrumentalism, as we argued in Section 2.5. There may well exist a cogent distinction “between the representational and fictional parts of [scientific] theories” but ET have not given us any cogent reason to “accept . . . a distinction between the realism of counting mammals with integers and the instrumentalism of counting mammals with

real numbers.”

3.3. Automorphisms. ET first raise the issue of nontrivial *automorphisms* of ${}^*\mathbb{R}$ in [33, p. 6]. They seek to capitalize on the existence of models of hyperreals with nontrivial automorphisms, in contrast with the reals that are *rigid*, i.e., lack such automorphisms (see Section 2.8 for the dissolution of their objection in Nelson’s framework). The argument is that because of the existence of such automorphisms, no specific infinitesimal can be taken to have “physical meaning” any better than its image under an automorphism.

The main fallacy of their argument is that *any* model using an infinite number, say H , will necessarily involve a certain degree of arbitrariness, since substituting $2H$ for H would typically do just as well in such a model.¹ No automorphisms are needed to argue this type of indeterminacy. Their focus on the issue of nontrivial automorphisms misses this basic point.

Furthermore, if as they declared earlier (see Section 2.9), ET wish to work with countable theories, then the quotient field of Skolem’s non-Archimedean integers (see e.g., [50, Section 3.2]) provides a *rigid* non-Archimedean field as can be proved using Ehrenfeucht’s lemma [35].² The ET “automorphisms” argument is thus based on massaging the evidence.

As Skolem’s integers can be naturally embedded in a hyperreal field (see [4, Section 2.4]), the latter can be viewed as the kind of elegant “completion” of the former that ET allow for the real completion of countable fields of computable reals (see Section 3.2), *undercutting* the ET argument.

3.4. Physical segments. ET write: “For the example of the numerical representation of distance, the physical relation is ‘longer than’ and the operation is the ‘concatenation’ of two segments . . . Given that ‘longer than’ and ‘concatenation’ can be applied to any pair of distances, that every distance can be extended and divided, etc.” [33, p. 6].

Here ET are postulating both indefinite extension and indefinite divisibility of physical segments, in line with the Cantor–Dedekind postulate (see Section 2.10) but contrary to established physical theory. ET assume that they can find “physical” segments that can be indefinitely divided. Such an assumption is dubious and goes contrary to basic principles of quantum mechanics, where infinite divisibility breaks down at quantum levels. Continuum understood as *matter* is grainy by quantum mechanics, in the sense of violating indefinite divisibility at scales below Planck.

What about continuum understood as *space-time*? Einstein field equation $R_{\mu\nu} - \frac{1}{2}Rg_{\mu\nu} + \Lambda g_{\mu\nu} = \frac{8\pi G}{c^4}T_{\mu\nu}$ shows via the stress-energy tensor $T_{\mu\nu}$, that the graininess of matter necessarily affects that nature of the metric and curvature terms in the left-hand side. Thus according to general relativity, the metric and the curvature can only have physical meaning at scales where matter itself has meaning. This indicates that the indefinite divisibility of models of space-time used in relativity theory is necessarily a mere idealisation as far as very small scales are concerned.

3.5. Archimedean circularity. Concerning the issue of physical representation of real numbers, ET write: “Given that ‘longer than’ and ‘concatenation’ can be applied to any

To give an elementary example, in calculus the integral of a Riemann-integrable function f on a compact interval $[a, b]$ can be computed as follows. One chooses an infinite hypernatural number H , partitions $[a, b]$ into H subintervals of length $\Delta x = \frac{b-a}{H}$, computes the corresponding infinite Riemann sum, and applies the shadow (standard part): $\int_a^b f(x)dx = \text{sh}(\sum_{i=1}^H f(\xi_i)\Delta x)$. Replacing H by $2H$ throughout would not affect the value of the integral.

More precisely, the model S given by Skolem’s non-Archimedean integers is rigid by Ehrenfeucht’s lemma. Rigidity of this structure is preserved when passing to the quotient field $Q(S)$ due to a theorem of Julia Robinson [71] that ensures that S is definable in $Q(S)$ by a first-order formula.

pair of distances, that every distance can be extended and divided, that ‘concatenation’ and ‘longer than’ commute appropriately, and the *Archimedean principle*, one can show that a numerical representation of the conventional sort chosen must in fact exist, etc.” [33, p. 6] (emphasis added).

Here ET seem to postulate that there is an Archimedean principle for the physical relation “longer than” but ET don’t give a source for such. Without a source, what the ET are doing is to *assume* the conclusion they want to reach, namely that a “representing” number system is necessarily Archimedean because the target is assumed to be Archimedean, revealing a logical fallacy known as *vicious circle*. This observation *undercuts* the ET argument.

ET’s fallacy is not uncommon. In a recent article, Walter Dean discusses “a tradition within measurement theory which questions whether it is always appropriate to assume that the mathematical structures employed as scales for length measurement must be Archimedean” [28, p. 336]. In footnote 75, he provides sources in the literature on measurement theory that have expressed “similar concerns about the empirical status of the Archimedean axiom.”

3.6. Changing the subject. ET write: “These examples [of how geometry motivates the introduction of square roots into the number system] so far only motivate the use of quadratic extensions of the rationals, but *adequate theorizing* about physical laws motivates the use of more complete subfields of the real numbers” [33, p. 8] (emphasis added).

In this sentence ET have attempted to change the subject of the discussion. Until now ET have spoken about physical reality and what constitutes “accurate” (or *applicable*; see Section 2) representation thereof. Now they have switched to discussing what constitutes “adequate theorizing.” However, their discussion of “adequate theorizing” involves an equivocation on the meaning of *adequate*, since adequate theorizing is not the same as accurate representation. Their changed focus becomes transparent by the end of the same paragraph; see Section 3.1.

4. Elegant scientific theorizing.

4.1. Adequacy and elegance. ET write: “[A] physical system that used only algebraic numbers could not be phrased in such an *elegant* way as the differential equations traditional for Newtonian and later theories” [33, p 8] (emphasis added).

Here ET change the focus of what their qualifier *adequate* refers to. The adjective no longer refers to (i) existence of an accurate fit with physical reality, but rather (ii) the *elegance* of the scientific theory involved (in this case, the appropriate ordinary differential equations). Falling back on the criterion of elegance undermines their own argument, as we argue in Section 3.5.

4.2. Real world quantities, anyone? ET write: “It may be that the full set of real numbers is unnecessary — perhaps the computable real numbers . . . suffice. However using a superstructure causes no harm to the applicability of the theory so long as the values we attempt to assign to observable quantities belong to the substructure” [33, p. 8].

The logic of their argument forces ET to admit that the real numbers are merely a convenient idealisation; in fact they acknowledge it repeatedly (see Section 3.3). However, the same is true of the hyperreals, underscoring the fact that their argument is on shaky ground. ET write: “[E]ven if some field between, say, the algebraic numbers and the computable reals is the correct model of length, we can work in the full model of the reals even though, when assigning values to actual *real world quantities*, we only ever use values from the subfield” [33, p. 8] (emphasis added).

In this passage, ET admit that some numbers in the model may not have an “adequate” referent in their original sense of “adequate” (see Section 2.18). In other words, some real numbers may not be what they refer to in this remarkable sentence as “real world quantities,” in a unique form of object realism (see Section 1.1).

4.3. Hypercomputation beyond “actual quantities”. ET write: “[E]ven if the computable real numbers (or some other substructure of the reals) appear to suffice for all current physics, the discovery of some means of hypercomputation could change the correct choice of substructure” [33, p. 8].

All systems ET mentioned so far as potentially corresponding to “real world quantities”, such as algebraic and computable numbers, are countable. Where do the reals come in then? ET write: “[W]hen an applicable model is a substructure of a rigid larger mode[l], that larger model can remain applicable: at worst it includes theoretical states that can never appear in reality, and which are therefore never assigned to *actual quantities*” [33, p. 8] (emphasis added).

If some real numbers are never assigned the “actual quantities” as ET acknowledge, then it clearly follows that \mathbb{R} is a convenient idealisation.

Their comment on rigid models seems to endorse the real field and imply a criticism of hyperreal fields. However, hyperreal fields without automorphisms can also be constructed, making the system rigid. This is true both with regard to the usual hyperreal fields relative to suitable structures of cardinality \mathfrak{c} (see Section 3.6), and with regard to countable models with respect to countable structures (see Section 2.15). ET have again failed to drive a wedge between the applicability of the reals and the hyperreals in a meaningful way.

4.4. More “actual representation”. ET write: “To justify the claim that the hyperreals are an actual representation of some part of the world (rather than merely a useful computational tool), the representation must be unique, or at most involve a small number of arbitrary choices” [33, p. 9].

In the context of the attempt by ET to drive a wedge between reals and hyperreals, this comment appears to assume that the reals are an “actual representation of the world” or that at any rate a suitable subfield of \mathbb{R} provides such a representation. This amounts to accepting the Cantor–Dedekind postulate; see Section 2.10. In line with our policy of interpreting the ET thesis charitably (see Section 2.4), we note that, even if one assumes that what they seek is an *applicable* theory, hyperreal fields are adequate to the task; see Section 3.3.

4.5. Countable languages. ET write: “[I]f the continuum hypothesis holds, the hyperreals (viewed as an ordered field) have many automorphisms which fix the reals. This continues to hold in any *countably infinite* expansion of the language of ordered fields – for instance, we might want to add many functions arising as solutions to various differential equations, like exponential and trigonometric functions, in addition to countably many units and coordinates” [33, p. 9].

Earlier in their text, ET do admit an uncountable number system for reasons of *elegance*, so as to be able to defend the use of \mathbb{R} as the basic number system. But their insistence on trimming the language to countable size does not deliver the desired disqualification of non-Archimedean systems, since such countable systems can be constructed that admit no automorphisms, i.e., are rigid; see Section 2.15.

If one insists on keeping all of the uncountably many real numbers, for similar reasons of *elegance*, symbols for all functions $f: \mathbb{N} \rightarrow \mathbb{N}$ should be included in the structure. In such case suitable hyperreal fields do become rigid; see Section 3.6.

A standard tool in quantum mechanics is the Hilbert space ℓ^2 of square-summable sequences. This space has the same cardinality as $\mathbb{N}^{\mathbb{N}}$, namely \mathfrak{c} . Nonetheless, quantum mechanics is generally thought of as an *applicable* theory, *undermining* the ET thesis.

4.6. Uncountable languages and rigidity. ET finally admit the following: “In a *uncountable* language, the situation is more complicated – indeed, in a large enough language, the hyperreals can become rigid [Enayat, 2006] – but such an infeasible language is, to say the least, an unusual setting for a scientific theory” [33, p. 9] (emphasis in the original).

ET’s emphasis is on the distinction between countable (see Section 3.5) and uncountable languages. Their emphasis makes it clear that they view a countably infinite language as a suitable setting for a scientific theory, whereas an uncountably infinite language is “infeasible” and “to say the least . . . unusual” to such an end.

Since ET don’t reveal the details, it may be helpful to point out that a standard construction of a hyperreal structure over a countable index set, together with the language including all n -ary functions on \mathbb{N} and all 1-place predicates on \mathbb{N} , already gives a rigid model of ${}^*\mathbb{R}$.³ Such a structure has cardinality \mathfrak{c} .

Such a setting is not an unusual setting for a scientific theory and on the contrary is the standard setting routinely used in physics; see e.g., Section 3.5.

5. New developments in physics. After making some preliminary remarks concerning the continuum hypothesis, ET proceed to claim the following: “There is a slim road here through which new information could change our view: new physical discoveries could demonstrate the falseness of the continuum hypothesis and find some way to uniquely distinguish a particular, rigid, hyperreal structure which has an observable physical significance. Because of this possibility, we cannot claim that the hyperreals couldn’t *possibly* be applicable; we can only claim an applicable use of the hyperreals would require substantial new developments in physics” [33] (emphasis in the original).

ET’s claim that “an applicable use of the hyperreals would require substantial new developments in physics” is a non-sequitur even by their own criterion of *rigidity*, since reasonable rigid hyperreal systems exist regardless of the status of the continuum hypothesis; see Section 3.6.

5.1. Sources on applications of Robinson’s framework. The monograph by Albeverio et al. ([1], 1986) contains five hundred pages of meaningful applications of the hyperreals in physics, *rebutting* the ET thesis. ET mention the work [1] briefly as an “actual proposal . . . for the use of the hyperreals in science (as in, for instance, [Albeverio et al., 1986])” but provide no substantive evaluation of its merits. Many applications to physics, probability theory, stochastic analysis, mathematical economics, and theoretical ecology appear in the books Robinson ([70], 1966, chapter IX), Nelson ([65], 1987), Capiński–Cutland ([19], 1995), Arkeryd et al. ([2], 1997), Faris ([37], 2006), Van den Berg and Neves ([76], 2007), Loeb and Wolff ([60], 2015), Lobry ([61], 2018), and in many other sources.

5.2. Why elementary equivalence? ET acknowledge the possibility “that some physical quantity has non-Archimedean behavior” [33, p. 10] but question the need for elementary equivalence with \mathbb{R} . Such a need could be argued as follows. If science tells us that some quantity grows exponentially, we would want “exponential” to have the usual meaning in the whole non-Archimedean domain. In other words, we would want the domain to be elementarily equivalent to \mathbb{R} with respect to the properties of the exponential.

More precisely, due to the availability of a first-order definable pairing function in $(\mathbb{N}, +, \cdot)$ (Cantor’s pairing function), all n -ary functions on \mathbb{N} can be first-order simulated once we have all 1-place predicates.

Furthermore, we want elementary equivalence for elegance and there is no need necessarily to have reasons from physics (even though such reasons are available). This is in line with the ET admission of full \mathbb{R} , rather than using just some countable subfield of it, for reasons of elegance; see Section 3.1.

5.3. Quantifier structure. ET write: “[W]hile the hyperreals are meaningful objects worthy of their own study, there are other contexts (particularly dealing with real analysis, and the physical theories using it) where they are mere tools for avoiding some complexity in dealing with quantifier structure” [33, p. 12].

In mathematical pedagogy particularly at freshman level, one of the main advantages of the hyperreals is providing a simplification of such “quantifier structure” (see e.g., [54]). However, ET’s reference to physical theories makes it clear that they are not limiting their sweeping “mere tool” claim echoing Connes (see [50]) to pedagogy. As such, their claim has little basis.

5.4. Connes and first mathematical principles. ET proceed to relate to the rebuttal as developed in [50] of Connes’ critique, which involves two separate points:

1. the issue of undefinability, where Connes claims to be thoroughly familiar with the “Polish school of logic” through a seminar he participated in, and drops hints related to Solovay models that indicate that he is talking about a purely mathematical issue that he claims to be a shortcoming of the hyperreals.
2. the issue of “constructiveness” which (unlike Bishop) he interprets as applicability to physics. Sanders has analyzed the difference between Bishop’s and Connes’ take on “constructiveness” in [72].

ET treat the issue as follows. They mention Connes’ critique and the rebuttal that appeared in [50], and then point out that “the real issue” is not the abstruse one of the details of the mathematical definitions, but rather an alleged inapplicability in physics: “One major line of debate focuses on the claim that no non-standard hyperreal can be ‘named’. Connes tries to establish this claim by arguing that a given non-standard integer in a hyperreal field can be used to generate a non-principal ultrafilter over the natural numbers, or a non-measurable set of reals. Since these complex entities seem to be beyond some limit of complexity for physical beings like us to grasp, this is said to raise problems for any appeal to hyperreals. Kanovei, Katz, and Morman dispute Connes’ claims about the association of a non-principal ultrafilter with a given hyperreal field ... We claim that *definability is not the real issue* here. These things are ‘definable’ in the strict mathematical sense, but the ‘definitions’ don’t serve the purpose we need definitions to serve in *physical models*, of making it possible to uniquely measure quantities” [33, p. 12] (emphasis added).

In this passage, ET are equivocating on the meaning of Connes’ criticism and more precisely conflating two separate issues. Namely, as argued in [50], Connes places himself on the purely mathematical plane (the *chimera* track of Section 2.1) when he voices his *undefinability* critique. The article [50] argues that his critique is incoherent. ET misrepresent the picture by changing the subject to physical applications (the *dart* track of Section 2.1). Now the *dart* objection is also an objection Connes formulated, but it is a different one, and the response to that in [50] was different, as well. Thus the ET criticism of the rebuttal of Connes in [50] involves the fallacy of moving the goalposts.

Connes claims that he can establish from first mathematical principles, having to do with Solovay’s model, that something is amiss with Robinson’s infinitesimals (and therefore shift the focus to Connes’ own noncommutative infinitesimals; see also the analysis given in [53]

of Connes' critique). That is the myth debunked in [50]. This particular theoretical issue has nothing directly to do with applications to physics (though Connes seeks to apply his contention so as to claim that allegedly no such applications are possible).

The ET approach suffers from the same shortcoming as Connes', in that they ignore the concrete applications of Robinson's framework as for instance in [1] (see Section 4.1 for other sources), and instead attempt to argue from first mathematical principles that Robinson's framework is not applicable to physics. Their attempt misses the target as did the earlier salvos of both Connes [25] and Easwaran [31].

5.5. Applications, σ -additivity of Lebesgue measure. The ET text contains the following remarkable passage: "Note that our worry in application is about existence proofs, and not all uses of the Axiom of Choice. [Bascelli et al., 2014] point out (pp. 851-2) that the countable additivity of Lebesgue measure requires the Axiom of Choice to prove. It is consistent with **ZF** that the set of all real numbers be a countable union of countable sets. Rejection of the Axiom of Choice would surely cause problems for those who want to say that Lebesgue measure is countably additive" [34, p. 13].

Having stated the dilemma, ET attempt to resolve it: "But we don't reject the Axiom of Choice - we reject the realist applicability of mathematical entities whose existence is independent of **ZF**. Thus, even if someone were to convince us to accept some alternate mathematics on which there is a countable collection of countable sets whose union is all of \mathbb{R} , we would say that in practice it is safe to assume countable additivity of Lebesgue measure, because these counterexamples would have no applicability in practice" (ibid.).

Here ET allude to models of set theory where \mathbb{R} is a countable union of countable sets, e.g., the Feferman–Levy model (**FL**). The pertinence of the ET claim that such models "have no applicability in practice" needs to be understood.⁴ In the passage quoted above, ET attempt to change the subject from

1. the applicability of σ -additivity of Lebesgue measure, to
2. the properties of the Feferman–Levy model.

Their strategy is based on the fallacy of moving the goalposts. What is important in applications is not the somewhat paradoxical decomposition properties of item (2) (see [49] for an interesting consequence), but rather the convenient tool of σ -additivity of Lebesgue measure as in item (1). It may be interesting to note that Lebesgue himself required his measure to be countably additive; see [59, p. 236] and [46, p. 122]. In fact, the endorsement of σ -additivity in (Easwaran [30], 2013) creates an awkward situation where the author wants to eat the cake ("reject the realist applicability of" **AC**) and have it, too (namely, assume σ -additivity of the Lebesgue measure).

ET claim to "point out serious problems for the use of the hyperreals (and other entities whose existence is proven only using the Axiom of Choice) in describing the physical world in a real way." They also remark that "other entities dependent on the Axiom of Choice

The **FL** model of \mathbb{R} admits a decomposition

$$\mathbb{R} = \bigcup_{n \in \mathbb{N}} C_n \text{ where each } C_n \text{ is countable.} \quad (1)$$

Note that **FL** cannot support a countably additive Lebesgue measure (because countable sets are Lebesgue measurable and their measure is 0, while countable additivity and decomposition (1) would then entail that every subset of \mathbb{R} is a null set). One can still define a Lebesgue measure in **FL**, but it will only be finitely additive (see Section 4.9). Thus it is consistent with **ZF** that the Lebesgue measure is not σ -additive.

don't lend themselves so naturally to physical theorizing, but we think the points we make apply generally" [34, page 1]. Meanwhile, the σ -additive Lebesgue measure is another entity dependent on **AC** that is widely used in physical theorizing (see Section 4.10). Thus the ET denial of applicability to hyperreals would apply equally well to the σ -additive Lebesgue measure, contrary to much evidence that points in the opposite direction, namely to the usefulness of the σ -additive Lebesgue measure in scientific applications, *rebutting* the ET thesis.

In sum, the real issue is not, as ET imply, whether the decompositions as in (2) have "applicability in practice", but rather whether σ -additivity mentioned in (1) has applicability in practice.

Recall that one of the applications of the Lebesgue measure is the definition of the spaces L^p and of the Sobolev spaces $W^{k,p}$; we recall that if $1 \leq p \leq \infty$ these are Banach spaces, and if $p = 2$ the spaces L^2 and the space $W^{k,2} = H^k$ are Hilbert spaces.

Sobolev spaces are widely used for the study of Partial Differential Equations, that in turn are applied to the mathematical description of physical phenomena. The essential point here is that these applications depend upon properties of the Sobolev spaces that are independent of **ZF**, and typically require **AC** for their proof; see Sections 4.9 and 4.10 for more details.

Faced with the ubiquitous reliance on the axiom of countable choice (**ACC**) in the foundations of analysis and topology, many mathematicians opt to incorporate **ACC** (or the stronger axiom of countable dependent choice) as part of the basic foundational package, and adopt **ZF+ACC** as their philosophical credo. However, this option is not available to ET since they ground their *real applicability* thesis (on behalf of mathematics based on **ZF**) in the allegedly constructive nature of **ZF** foundations (a claim that would be even less plausible for **ZF+ACC** than for **ZF**). ET's constructive track is analyzed in Section 4.10.

5.6. Connes & Katz versus ET. ET seek to oppose Connes and Katz on the issue of scientific applications: "In recent years, there has been much debate about the value of the hyperreals, with two main views exemplified by Alain Connes and Mikhail Katz, and various coauthors of each" [33, p. 11].

However, the salient point here is that Connes and Katz are on the same side as *against* ET's denial of applicability of **AC** and/or related principles. Thus, Connes routinely exploits **AC** in developing the objects he needs to work with his (Connes') infinitesimals, such as the Dixmier trace (which is a Connesian version of integration of noncommuting infinitesimals), and exploits the Čech–Stone compactification $\beta\mathbb{N}$ of \mathbb{N} in [24, ch. V, sect. 6.δ, Def. 11]; for details see [50].

5.7. Measures without AC. Few authors have focused on the study of the properties of Sobolev spaces in **ZF** without **AC**, i.e., in a setting where the Lebesgue measure is only finitely additive.

Terry Tao in [75, Definition 1.2.2] uses a definition of Lebesgue measure that works in **ZF**. When extended to **ZFC** this definition gives the standard σ -additive Lebesgue measure. Therefore Tao's definition is preferable to definitions that have σ -additivity built into the definition and therefore don't make sense over **ZF**. In view of the above, the measure as defined by Tao is not merely a Lebesgue-like measure but arguably the Lebesgue measure itself.

We note that the measure theory textbook by Paul Halmos mentions **AC** only in the context of the construction of a nonmeasurable set. Halmos' proof of σ -additivity makes no mention of **AC**, and is therefore inaccurate. The gap is in [45, p. 42, line -3], where **ACC** is

relied upon implicitly. Further gaps in Halmos (mainly of a philosophical type) are analyzed in [11].

The book [9] contains a thorough study of the properties of finitely additive measures and of the corresponding L^p spaces. The main drawbacks of using finitely additive measures instead of the Lebesgue measure are that

1. the L^p spaces are not complete with respect to convergence in measure;
2. their completions, the so-called V^p spaces, are Banach spaces; however, it is not mentioned whether V^2 is a Hilbert space.

If V^2 is not a Hilbert space, then many results on H^k spaces relying on the inner product given by the Lebesgue integral might not hold for finitely additive measures. As a consequence, the study of relatively simple problems, such as the weak formulation of the Laplace equation, might not be practical, since the existence of a weak solution to its Dirichlet problem relies on the Riesz Representation Theorem for the Sobolev space H_0^1 and on the weak sequential compactness of this space. We recall that the Laplace equation is used as a model of various physical phenomena, since its solution can be interpreted as the density of a physical quantity in a state of equilibrium. Some physical laws, such as Fourier's law of heat conduction or Ohm's law of electrical conduction, can be formulated by means of the Laplace equation; see Evans [36] for further details.

We remark that, by working with the algebra of Borel-coded subsets \mathcal{B} of a topological space X instead of the whole algebra of Borel subsets, it is possible to refine some finitely additive measures defined over X to *Borel-coded measures*, that are σ -additive over a suitable σ -algebra $\mathcal{E} \subseteq \mathcal{B}$. The details of the construction are discussed by Fremlin [40, Chapter 56]; we refer also to the appendix of Foreman–Wehrung [39] for a gentle introduction and further references. If the underlying topological space X is second countable, the spaces $L^p(X)$, whose elements are (equivalence classes) of Borel-coded functions whose p -th power is integrable with respect to a codably σ -finite Borel-coded measure, are norm-complete whenever $1 \leq p < \infty$; under the same hypotheses L^2 is also Hilbert space [40, Chapter 56, pp. 204, 212].

Note that this construction applies to the finitely-additive Lebesgue measure over \mathbb{R} . Since second countability of \mathbb{R} can be proved in **ZF** alone (see Herrlich [48]), even in the Feferman–Levy model of the real line there is a (Borel-coded) Lebesgue measure which is σ -additive on a nontrivial σ -algebra of subsets of \mathbb{R} and whose L^p spaces are complete. However, to the best of our knowledge, these spaces of Borel-coded Lebesgue measurable functions have not yet been successfully applied in the sense advocated by ET. See Section 4.10 for further limitations on the applications of the L^p spaces without **AC**.

5.8. ET's worry and the Dirichlet problem. In the passage quoted in Section 4.5, ET express a “worry” concerning existence proofs that make use of **AC**. They advocate the use of “constructive or computable approximations” [34, p. 13] of some results depending on it, such as the Hahn–Banach theorem. Their position echoes the more radical rejection of indirect proofs by constructive mathematicians following Bishop and Bridges [10], [15].

We recall that the approach of Bishop and his school is claimed to be consistent with classical mathematics without **AC**, since at its core it consists in the rejection of the law of excluded middle and of those principles of classical mathematics that imply it, such as **AC** or the Hahn–Banach theorem. Existence results obtained without these principles can and have been turned into algorithms; however, their scope does not yet include some central areas of mathematics such as the theory of partial differential equations.

Consider for instance the constructive Laplace equation, discussed by Bridges and McKubre-Jordens in [16]. The authors admit that it is not always possible to define constructively a solution (since the Riesz Representation Theorem is not constructively valid for every function in H_0^1 and since weak sequential compactness does not entail constructively the existence of a limit). Thus, they write: “We prove the (perhaps surprising) result that the existence of solutions in the general case is an essentially nonconstructive proposition: there is no algorithm which will actually compute solutions for arbitrary domains and boundary conditions” [16, p. 1].

In a similar vein, Bridges and McKubre-Jordens write: “In this section we prove that the existence of a weak solution of the general Dirichlet problem [for the Laplace equation] for a domain $\Omega \subset \mathbb{R}^2$ cannot be proved constructively” [16, p. 6] (in fact their article may have been more accurately titled “Failing to solve the Dirichlet problem constructively”).

As a consequence, this relatively simple equation is not yet tractable by means of their approach. Meanwhile, the non-constructive proof techniques that are used in classical mathematics to prove the existence and uniqueness of a solution to the Laplace equation are applied systematically to other PDEs arising in physics and in engineering (see Pinchover–Rubinstein [66]). To the best of our knowledge, no applicable alternative to the use of **AC** in the theory of PDEs has been proposed yet.

5.9. Applications of finitely additive measures. Some mathematicians have argued against the exclusive use of σ -additive measures. For instance, de Finetti suggested that fair lotteries over infinite sets should be modelled by finitely additive measures [27]. With different motivations, Nelson proposed an approach to probability theory based solely on hyperfinitely additive measures in the context of Internal Set Theory [65]; see also the more recent axiomatic approaches by Benci et al. [7], [8].

The advantages of relinquishing σ -additivity are not limited to probability theory: a well-known result in Robinson’s framework entails that any measure, be it σ -additive or simply finitely additive, can be represented by a hyperfinite counting measure (see Henson [47]); in addition, it is possible to require many degrees of compatibility between the standard measure and its hyperfinite representative [6], or even construct a single hyperfinite counting measure that is simultaneously compatible with all of the Hausdorff measures [77].

Hyperfinite counting measures enable one to define functional spaces that are expressive enough to represent not only the Sobolev spaces $W^{k,p}$, but also other generalized functions commonly used for the study of PDEs. For instance, in [14] it is shown that all linear PDEs and many nonlinear ones can be given an equivalent nonstandard formulation in the space of *grid functions* of nonstandard analysis, thus providing a unifying framework for the study of problems that in standard mathematics require different approaches. For a more precise statement on the advantages of grid functions in the theory of PDEs, we refer to [14], [13], and [12].

5.10. Effective concepts. Brunner et al. open their article with the following illuminating passage: “A concept is *effective* in the sense of Sierpiński if it does not require the axiom of choice **AC**. Here we show by means of examples that fundamental notions of quantum theory are not effective. For instance (see Section 1.2) there is an irreflexive Hilbert space L , constructed from Russell’s socks in the second Fraenkel model \mathcal{M}_2 . Hence the very notion of a self-adjoint operator as an observable of quantum theory may become meaningless without the axiom of choice” (Brunner et al. [17, p. 319]).

The authors go on to hedge their bets with the following: “Nevertheless we identify a

nontrivial class of observables, the intrinsically effective Hamiltonians, which is compatible with L in the following sense, etc.” (ibid.). We should note that a typical physicist is not very interested in limiting the scope of applicability of mathematical results by introducing foundational restrictions (such as banning **AC**), nor in introducing technical complications necessitated by such restrictions. On the contrary, he seeks constantly to “push the envelope” by applying mathematical methods somewhat beyond their ‘official’ domain of applicability. Well-known examples of such attitudes are Dirac’s delta ‘function’ and the Feynman ‘integral.’

6. Conclusion. Easwaran and Towsner argue that the hyperreal number system of Robinson’s infinitesimal analysis is a good instrumental theory, i.e., a theory that is “useful for proving theorems about the real numbers,” but at the same time it is not suitable for the description of physical phenomena, since the infinitesimals of Robinson’s framework are “idealizations that don’t correspond to the world.” However, these claims rest upon an outdated conception of mathematical realism and on the identification of the physical continuum with the Cantor–Dedekind continuum of real numbers as understood by these classical authors. By abandoning the idea that the relation between mathematics and physical reality must be that of an isomorphism, one sees that the arguments proposed by ET against the applicability of the hyperreals, such as the distinction between *instrumental* and *applicable* theories, are inadequate.

While ET concede that new developments in physics could lead to a different model of the physical continuum, they seem to ignore the fact that this approach has already been proposed by many authors. In fact, there are many applications of Robinson’s framework to physics, economics and other sciences, that are unfortunately not discussed in any detail by ET, who choose instead to argue that the hyperreals are inapplicable from first principles and with ad-hoc arguments.

In an attempt to show that their critique is not meant to single out Robinson’s framework, ET suggest that the flaws attributed to the hyperreal numbers are shared also by other mathematical entities whose existence cannot be proved in **ZF**, being dependent on the axiom of choice (**AC**). Yet, they quickly conclude that “other entities dependent on the Axiom of Choice don’t lend themselves so naturally to physical theorizing” and “they won’t generally play any role in application,” thus focusing their attack on Robinson’s infinitesimals. However, their stance on the applicability of mathematical entities dependent upon **AC** is inconsistent, since E argued in an earlier publication in favor of σ -additivity, a property independent of **ZF**. Moreover, ET do not address the fact that many other applicable mathematical theories require some form of a choice principle. As an example, in Section 4 we showed that even simple PDEs become intractable without σ -additivity of the Lebesgue measure.

Thus, if mathematical entities dependent upon **AC** should cause worry and ultimately are deemed not truly applicable, then according to ET many areas of mathematics, such as the theory of PDEs or the formalism of quantum mechanics, would suffer from the same drawbacks attributed to the hyperreals. Being that these theories are widely applied also outside of mathematics, the ET position is hardly defensible.

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