Higher-order metametaphysics*

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The recent turn to higher-order languages—languages with quantification into predicate, sentence, and other non-subject positions—promises elegant and more accurate modes of expression, new solutions to old problems, transformation of problem spaces, and generation of new questions: a paradigm shift. The excitement in the peroration of Cian Dorr's agenda-setting paper "To be F is to be G" is typical of the spirit of this movement, and is undeniably infectious:

> And the exploration has barely begun: there is a whole continent of views waiting to be mapped out, and at this point we can only guess which of them will look most believable in the long run. Onwards!

As a community, the best way to handle such transformational ideas is to run with them. Many of us should embrace the new framework, explore it from the inside, and see where that leads. Setting aside the inevitable nay-sayers, that's what we did with logic and linguistic analysis in the early twentieth century, and with possible worlds and modal logic in the 1970s (to take just two examples), in each case with great success. In-depth exploration is needed to tell whether ideas are on the right track; we know them by their fruit. Onwards indeed!

But nay-saying has its place too. Strawson and the other ordinary-language philosophers provided a corrective to Russell and his heirs, important parts of which were eventually assimilated into the mainstream. Quine (at the very least) forced modal enthusiasts to clearly articulate and embrace their metaphysical commitments.[1]

And sometimes nay-sayers are right. Despite the appeal and promise of the higher-order approach, there are metaphysical questions about its foundations. Are higher-order languages in good standing? Do such languages succeed in latching onto reality; is reality such as to be well-represented by them? If so, then it would indeed make sense to stay up nights wondering whether, for instance, $p = (p \,\&\, p)$, for all p. Such questions would concern reality's

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[1] Hirsch (2011), Thomasson (2007, 2015), and others have played a similar role in another context, forcing the dominant "Quinean" tradition in ontology to articulate and defend its foundational assumptions.

higher-order structure. If not, the questions might have "unwanted" answers (if, say, higher-order sentences have first-order, set-theoretic truth conditions), or might not have answers at all (if higher-order sentences fail to be truth-apt), or might fail to have determinate or objective answers, or might suffer some other sort of "discourse failure".[2]

I join higher-orderists in rejecting Quine's (1970, p. 66) claim that higher-order logic can only be understood as "set theory in sheep's clothing". For it can be understood as an attempt to express sui generis higher-order facts. But it *is* a wolf, thus understood, since it is committed to the existence of those facts. And commitments of this sort should be avoided, other things being equal, on grounds of parsimony (section 2). This prima facie consideration would be overturned if higher-order quantification were indispensable, but the usual arguments to this effect are unconvincing (sections 3–6). Although some of the issues surrounding these arguments are familiar, I will shift the debate into more metaphysical territory. A final defense of higher-order quantification, appealing to the "collapse argument", will be considered and criticized (section 7).

1. Higher-order languages[3]

The grammar of first-order logic allows quantified variables to occupy the grammatical position of singular terms (as in, for example, $\exists x\big(F(x) \,\&\, \forall y R(x,y)\big)$), but not the grammatical position of predicates (as in $\exists F F(a)$) or of formulas (as in $\forall p\,(p \rightarrow \sim\sim p)$). The higher-order languages that are now popular *do* allow quantification into predicate and sentence position, and indeed, into the position of arbitrary grammatical categories.

The notion of "arbitrary grammatical categories" is made precise by the device of *types*. Types are conventional entities used to represent, or code up, grammatical categories; thus we speak of expressions in formal languages as *having* or being *of* types. The purpose of representing grammatical categories as entities is to allow us to quantify over them in the metalanguage, in order to make generalizations: "for any type, τ, if an expression has type τ, then …".

Here is one typical development of the idea. We begin with a type, e, which will represent the grammatical category of expressions that stand for

[2] One might stay up nights even given discourse failure. Addressing a question can have value beyond the value of answering it: the value of cartography of logical space.

[3] For further background on these issues, see Bacon (2023); Dorr (2025); Sider (2020*a*).

*e*ntities—that is, singular terms. (It doesn't matter which object the type e *is*; the association between types, construed as entities, and the grammatical categories they represent is purely conventional. We could, for example, simply take e to be the *letter* 'e'.)

The type e is called "primitive" because it isn't constructed from other types. But there are other—nonprimitive—types, which are constructed from simpler types according to this rule, where n may be any natural number (including 0):

$$\text{If } \tau_1, \ldots, \tau_n \text{ are types, then } (\tau_1, \ldots, \tau_n) \text{ is also a type} \tag{T}$$

The type $(\tau_1, \ldots, \tau_n)$ represents the grammatical category of an expression that combines with n expressions, of types $\tau_1, \ldots, \tau_n$, respectively, to make a *formula* (that is, to make an expression that can be either true or false, if none of its variables are free). That is, an expression of type $(\tau_1, \ldots, \tau_n)$ is an *n-place predicate* whose arguments are of types $\tau_1, \ldots, \tau_n$.

An important special case is when $n = 0$; the resulting type () is the type of formulas. (An expression that doesn't need any arguments in order to make a formula is already a formula.) Further examples: (i) (e) is the type of an expression that combines with an expression of type e (a singular term) to make a formula. That is, (e) is the type of the familiar one-place predicates of first-order logic. (ii) $\big((),()\big)$ is the type of an expression that combines with two expressions of type () (that is, with two formulas) to make a formula. That is, it's the type of two-place sentence operators, such as & or $\vee$.

The rule (T) can be applied iteratively, since $\tau_1, \ldots, \tau_n$ may be any types, including complex ones. Since (e) is a type, so is $\big((e)\big)$; but then $\Big(\big((e)\big)\Big)$ is also a type; and so on. There are infinitely many types.

In a typical higher-order language based on this simple type theory, constants and variables of each of the infinitely many types are allowed. Thus in addition to quantifying into singular-term-position (as in first-order logic), or predicate position (as in second-order logic), one can quantify into sentence position (variable of type ()), as in:

$$\forall p \exists q (q \leftrightarrow \sim p)$$

("for every proposition, there exists a proposition that is true iff the first is not true"), or into one-place sentence-operator position:

$$\exists O \forall p \big(O(p) \leftrightarrow \sim p\big)$$

("There exists a property of propositions that is had by a given proposition iff the proposition is not true"), or any other position represented by a type.[4]

In addition to allowing quantification into positions of all types, the currently popular higher-order languages depart from first-order logic in a second way, by allowing "lambda abstraction". The purpose of lambda abstraction (in logic) is to allow for complex expressions of arbitrary type. For example, to represent a conjunctive predicate 'jumps and gallops', we might write $\lambda x.\big(J(x) \,\&\, G(x)\big)$, which is a predicate, read as "is an x such that x is J and x is G". In general, where $v_1, \ldots, v_n$ are any variables, of types $\tau_1, \ldots, \tau_n$, respectively, and ϕ is any formula, then $\lambda v_1 \ldots v_n.\phi$ is an expression of type $(\tau_1, \ldots, \tau_n)$, meaning "are $v_1, \ldots, v_n$ such that ϕ".

A nice perk of lambda abstraction is that it can take over the job of variable binding from quantifiers. For instance, the standard first-order quantifiers $\forall$ and $\exists$ can be treated as predicates of one-place first-order predicates—that is, expressions of type $\big((e)\big)$. Thus instead of:

$$\forall x F(x) \qquad \exists x\big(F(x) \,\&\, G(x)\big) \qquad \forall x \exists y R(x,y)$$

we could write, in official contexts anyway:

$$\forall(F) \qquad \exists\Big(\lambda x.\big(F(x) \,\&\, G(x)\big)\Big) \qquad \forall\Big(\lambda x.\exists\big(\lambda y.R(x,y)\big)\Big)$$

In general, a quantifier over "τ-entities" is a predicate of predicates of τ-entities, and thus has type $\big((\tau)\big)$.

Finally, many higher-order inquiries employ higher-order identity predicates: predicates $=^\tau$, which form grammatical sentences when flanked on each side by an expression of type τ.[5] With them we may raise the question of "fineness of grain" for any type τ: under what conditions are "entities of type τ" the same or different? For instance, are propositions individuated by truth value (the coarsest imaginable grain)?:

$$\forall p \forall q\big((p \leftrightarrow q) \rightarrow (p =^{()} q)\big)$$

Or are they instead individuated by necessary equivalence?:

$$\forall p \forall q\big(\Box(p \leftrightarrow q) \rightarrow (p =^{()} q)\big)$$

[4]Here I am using p and q as variables of type (), and O as a variable of type $\big(()\big)$. Often the types of expressions are represented explicitly by superscripting: $p^{()}$, $q^{()}$, $O^{(())}$.

[5]The identity predicates may be taken as primitive; or, $\alpha =^\tau \beta$ may be defined as meaning that α and β have the same properties: $\forall F\big(F(\alpha) \leftrightarrow F(\beta)\big)$, where F is a variable of type (τ).

Or perhaps they are even finer-grained? Individuation by truth value or necessary equivalence implies that $\forall p\big(p =^{()} (p \;\&\; p)\big)$, which is incompatible with propositions being "structured".

Similar questions of grain may be raised for any type, with the help of lambda abstraction. Are properties identical to their self-conjunctions: $\forall F\big(F =^{(e)} \lambda x.(Fx \;\&\; Fx)\big)$? Is negation the same as triple negation: $\sim\, =^{(())} \lambda p.({\sim}{\sim}{\sim} p)$? And so on.

2. "Innocent" higher-order quantification

By saying that higher-order logic is "set theory in sheep's clothing", Quine meant that a second-order sentence like $\exists F F(x)$ really means (in some sense) that x is a member of some set: $\exists y\, x \in y$. (Or that x instantiates some property, construed as an entity: $\exists y\, I(x,y)$; but Quine regarded talk of properties as being less clear than talk of sets.) Second-order sentences express claims that would be more perspicuously expressed by *first*-order sentences that quantify over distinctive entities such as sets or properties.

It is one of the central claims of higher-order metaphysics that Quine was mistaken about this. Higher-order languages can be used in a sui generis way, to express claims that cannot be expressed in first order languages. A sentence like $\exists F\, F(x)$ does not mean that there exists some entity (first-order quantifier) that x instantiates or is a member of. So what does it mean, then? It means, well, that $\exists F\, F(x)$. Similarly for sentences containing variables of other types. $\exists p\; p$ does not mean that there exists (first-order quantifier) some proposition that is true; it can be true even if there are no such things as propositions. It means, well, that $\exists p\; p$.

(George Boolos (1984) famously defended an anti-Quinean view according to which *plural* quantifiers, such as 'some' in 'some pall-bearers lifted the casket', are sui generis, and are not first-order quantifiers over sets or the like. Much of what I will say also applies to this view, but it is not my main target.[6])

If higher-order quantifiers are sui generis, then the intuitive glosses of higher-order claims that I have been giving (and will continue to give) have the

[6]The current higher-orderists depart from Boolos in two main ways. First, Boolos gives us a way of interpreting the language of monadic, second-order logic, whereas the current higher-order movement embraces quantified variables of arbitrary type, and thus of arbitrary 'adicy and level. Second, plural variables are "extensional", whereas most higher-orderists think that even if it happens to be the case that all and only Fs are Gs, F and G might nevertheless be distinct. (Also they allow properties with no instances.)

potential to mislead. Glossing $\exists F\, F(x)$ as "x has some property", or $\forall p\big(p = (p \mathbin{\&} p)\big)$ as "every proposition is identical to its self-conjunction", can suggest first-order quantification, over properties in the first case and propositions in the second, which is not the intended meaning. Nevertheless, such glosses facilitate comprehension.

The anti-Quinean view is sometimes put by saying that higher-order quantifiers are "ontologically innocent", that they are not "ontologically committing"; but this can seem to mean more than it does. What it does mean is that higher-order quantification does not commit us to there being anything *in a first-order sense*. $\exists F\, F(x)$ can be true without there being some entity (first-order quantifier) corresponding to the predicate variable F. Nevertheless, there is a perfectly good sense in which it is "ontologically committing". $\exists F\, F(x)$ is, after all, an existential sentence. Just as the first-order sentence '$\exists x\, \text{rabbit}(x)$' says that there is, in the first-order sense, a rabbit, so the second-order sentence '$\exists F\, F(x)$' says that *there is*, in the second-order sense, an F had by x; it is false if there is no F that x has (in the second-order sense of 'there is no').

So: is this sui generis understanding of higher-order languages legitimate?

It isn't fully clear why Quine thought that second-order logic must be understood in first-order terms.[7] Sometimes he simply assumes, begging the question, that all quantified variables range over entities (Quine, 1970, pp. 66–7). Sometimes he is insisting that second-order logic is no more part of logic proper than first-order set theory; but the current higher-orderists don't seem to view logicality as an especially important classification.

In my view, the prima facie case against higher-orderism is simply parsimony. Posits that make the world more complex are, other things being equal, to be avoided. And the posit of sui generis higher-order quantification makes the world much, much more complex. Dorr's continent of views about the higher-order realm, exciting as it admittedly is, is exactly the problem. The posit commits one to an ocean of new facts, and the size of the continent suggests the size of the ocean. When I embraced the logical apparatus of first-order logic, using an embeddable negation sign, I didn't sign up for questions such as whether $\sim\, =^{(())} \lambda p.(\sim\sim\sim p)$. Higher-order language's expressive power does indeed yield an exciting research program, but the downside is increased worldly complexity. This is a basic and common sort of recoil from a proposed metaphysical commitment.[8]

[7] See Boolos (1975) and Turner (2015).

[8] An even more common sort is less defensible (in my view): recoil from apparently un-

Higher-orderism might be worth its cost in complexity, just as properties in physics such as charge and mass are presumably worth their cost. But the cost is nevertheless real, not to be paid lightly. We will soon critically examine arguments that the cost is indeed worth paying. But it may be objected right at the start that there can be no parsimony cost to higher-order languages, precisely because they commit us to no distinctive (first-order) ontology.

But metaphysical commitments, in the sense relevant to parsimony, need not be to entities. For instance, the adoption of modal operators presumably has no distinctive first-order ontological commitments: the operators do not correspond to new entities, but rather to "new modes of truth", so to speak. For "modalists", reality has a modal aspect, an aspect unrecognized by anti-modalists like Quine. Modalists accept an ocean of facts, resulting in a continent of new questions, such as whether reality might have been exactly as it actually is physically but not mentally, whether I could have been born from different parents, and so on. The world is a more complex place according to modalists than it is according to Quine, despite the fact that modalists don't (or needn't) recognize any new entities. (My quantification over aspects, modes, and facts in this paragraph is inessential, present only because of natural language's preference for nouns, which is well illustrated by this very sentence.) Similarly, the adoption of predicates of 'charge' and 'mass' in physics, which everyone acknowledges as involving an increase in worldly complexity, doesn't (or needn't) involve postulation of new entities. (Of course, some argue that adopting 'charge' and 'mass' *does* commit one to new entities, namely, universals of charge and mass; and similarly for modal operators. But the claim that adopting the vocabulary commits one to worldly complexity does not require this view.)

I myself think of metaphysical commitment in a certain way, as including a commitment to the key expressions "carving nature at the joints" (Lewis, 1983; Sider, 2011).[9] But the point is not tied to this metaphysical baggage. Even those who are skeptical of it should, unless they reject realist philosophy of science in general, agree that the adoption in physics of predicates of 'charge' and 'mass' is "costly" in the Occamist sense, where by 'adoption' I don't mean merely using the predicates meaningfully, but additionally, using them "theoretically", to state laws and give explanations, in the absence of any possibility of reduction. And then, unless they claim some special exemption for metaphysics, they

knowable facts. The epistemic recoil has a quite different source, neo-verificationist rather than Occamist. See Sider (2020*b*, section 3.15) for a discussion of related issues.

[9] See Fine (2001) for another approach in the same quadrant of logical space, though Fine does not particularly emphasize parsimony.

should agree that the adoption of modal operators is costly. And then, unless they claim some special exemption for logic, they should agree that the adoption of higher-order languages is also costly.[10]

I have said that the higher-order viewpoint increases reality's complexity because it recognizes an ocean of new facts. But it might be objected that much of this ocean may not be new at all, in a higher-order sense of 'new'. For if propositions (in a higher-order sense) are sufficiently coarse-grained, many of the allegedly new propositions expressed by higher-order sentences will in fact be identical to old propositions recognized all along, propositions expressed by first-order sentences. In the most extreme case, if propositions are individuated maximally coarsely, by their truth value, then there will be just two propositions, The True and The False, so that no proposition expressible in the higher-order language is new.

This argument makes two mistakes. First, it evaluates a theory's complexity using the *truth* about grain, rather than what the theory *says* about grain. The parsimony argument is epistemic: convinced that the world is probably simple, we give more credence to theories that say that the world is simpler. A higher-order theory that says that grain is coarse might well gain some credibility for that reason, but it doesn't matter whether the theory's propositions are in fact identical to old propositions. Second, the argument evaluates complexity solely on the basis of *propositional* grain. Two theories, each of which says that propositional grain is maximally coarse, might still differ in their complexity in an epistemically relevant way. One might posit more physical properties, or posit more complex structure at higher levels.

Some of these complexity judgments are admittedly fraught. (Not that we have an alternative to relying on them. We must simply draw tentative conclusions.) Which is more complex, a given higher-order theory or an as-similar-as-possible first-order theory of propositions, properties, and relations of arbitrarily high 'adicy and level? The former, I suppose, because of its larger ideology—distinctive variables and quantifiers at each level—but it's hard to place much weight on this. It's somewhat clearer that a given higher-order theory is more complex than a first-order theory of sets—ZF, say. But the judgment is again tenuous if the higher-order theory posits fairly coarse grain, for instance that "logical equivalents are identical", as in Bacon and Dorr (2023).

[10] Timothy Williamson forcefully opposes both exemptions, for instance in the preface to *Modal Logic as Metaphysics*. Other works in the anti-exceptionalist tradition include Almog (1989); McSweeney (2019); Paul (2012); Quine (1948).

I prefer the beautiful austerity of first-order ZF, though I wouldn't bet my next mortgage payment on it.

We are in murky epistemological territory. However, in some dialectical contexts and in much of the remainder of this paper, we can rely on a quite secure judgment: that first-order ZF is simpler than a higher-order theory that embeds that first-order theory, such as second-order ZF. For then the higher-order theory includes all the complexity of the first-order theory, plus more in addition.

Let us turn, now, to arguments for higher-orderism.

3. Argument from natural language

Some defend higher-order quantification by arguing that natural language already contains it. Boolos (1984) famously argued that natural language contains plural quantification; and Agustín Rayo and Stephen Yablo (2001) (following Arthur Prior (1971) and Dorothy Grover (1992)) argue that natural language contains devices tantamount to both monadic and polyadic second-order quantification. Just as Boolos claims that the natural language sentence 'Some critics admire only one another' doesn't carry a commitment to sets of critics, so Rayo and Yablo argue that 'Somehow things relate such that everything is so related to something' (the putative natural-language analog of $\exists R \forall x \exists y R(x,y)$) doesn't carry a commitment to relations (as entities).

But it isn't clear why any of this matters. If natural language doesn't contain higher-order quantification, couldn't we just introduce it, provided reality can support such talk? New concepts in physics (charge, spatiotemporal separation) are rarely introduced by defining them in pre-existing terms; rather, we lay out a role for the concepts, and posit that the role is filled. Conversely, if natural language does contain higher-order quantification, but reality *can't* support it, wouldn't higher-order natural language then suffer either discourse failure or unwanted reduction? The real issue is whether reality can support higher-order talk, not whether natural language already has it. Moral and modal skepticism of various sorts persist (including error theories, expressivist theories, and aggressively reductive theories) despite the presence of modal and moral natural language.

The primordial question, then, is metaphysical: whether reality can "support" higher-order languages. Now, it is difficult to canonically phrase this question without using contentious metaphysical language, such as "carving at the joints”. But the same is true for analogous questions about modal and moral language. These questions’ grip on us does not rely on an antecedent acceptance of any particular inflationary metaphysics.[11] So let us continue with an atheoretical, baggage-free statement of the question: does higher-order talk have the underpinnings in reality needed to be free from either reduction or discourse failure of various sorts?

4. The ZF argument

Boolos gave a second argument, which has nothing to do with natural language. He gave it in defense of irreducibly plural quantification, but it can also be given in support of irreducibly higher-order quantification.

The argument begins by assuming the correctness of the Zermelo-Frankel (“ZF”) approach to set theory. (Thus the argument supports higher-order logic—second order logic, in the first instance—as a supplement to, not a replacement for, set theory; and the parsimony judgment it must overcome is the comparatively secure one that first-order ZF is simpler than higher-order theories that include second-order ZF.) In its standard, first-order form, ZF set theory contains the following schema:[12]

$$\forall z \exists y \forall x \big(x \in y \leftrightarrow (x \in z \ \& \ \phi)\big) \qquad \text{(Separation schema)}$$

An instance of Separation says that if we begin with a set, z, then there exists a subset y of z containing all and only members of z that are ϕ. By replacing the schematic variable ϕ with different formulas in the language of set theory, we thereby are assured of the existence of different subsets y of a given set z.

Separation goes some way toward articulating the idea that any given set z has “arbitrary” subsets. But it doesn’t fully articulate that idea, since it implies the existence of only those subsets that are picked out by formulas in the language of set theory. Since that language has an enumerable vocabulary and its formulas are finitely long, its set of formulas is enumerable, whereas the set of subsets of any infinite set is not enumerable; so many subsets will be left out. That’s not to say that such subsets don’t exist; but the Separation schema doesn’t say that they do.

[11] It is compatible with this methodological point that the best way of understanding the questions in fact makes use of inflationary metaphysics.

[12] The variable y cannot be free in ϕ. A more accurate discussion, but requiring more setup, would focus on the more fundamental “Replacement” schema, whose instances are axioms of ZF.

How, then, *can* we articulate the idea that z has "arbitrary" subsets? By saying that "any collection of members of a given set, z, forms a set"? But what could "any collection" mean, if we wanted to do better than the Separation schema? It's no good for it to mean "any set"; the result would be a triviality. The higher-orderist, however, can say that it means *any property*, in a second-order sense of "any property". Using second-order resources the separation schema can be replaced with a single sentence in the language of second-order set theory:

$$\forall F \forall z \exists y \forall x \Big(x \in y \leftrightarrow \big(x \in z \;\&\; F(x) \big) \Big) \quad \text{(Second-order separation principle)}$$

(Boolos himself understood the second-order quantifier as expressing plural quantification.)

The argument in favor of higher-order quantification, then, is that with it we can give a better formulation of ZF set theory:

> It is, I think, clear that our decision to rest content with a set theory formulated in the first-order predicate calculus with identity… must be regarded as a compromise, as falling short of saying all that we might hope to say. Whatever our reasons for adopting Zermelo-Fraenkel set theory in its usual formulation may be, we accept this theory because we accept a stronger theory consisting of a *finite* number of principles, among them some for whose complete expression second-order formulas are required. We ought to be able to formulate a theory that reflects our beliefs. (Boolos, 1984, p. 441)

Now, the alleged problem with first-order ZF cannot be that there are true propositions about sets that we can't state using its language, since even the language of second-order ZF still has a countable vocabulary and finitely long sentences and hence only countably infinitely many sentences.

The quotation suggests that the problem involves our *beliefs*: their statement requires expressive resources beyond first-order ZF. This is still weak. If the beliefs of some sect of medieval angelologists require distinctive language to state, this is no argument that the language is in good standing.

An initially more promising argument is that second-order ZF is a *better theory* than first-order ZF, which justifies accepting the new conceptual resources needed to state it.

New conceptual resources are justified in an analogous way in the physical sciences. In classical mechanics, for example, we accept properties of charge and mass because they are needed to state simple and powerful laws of motion. Without them, we could only describe motion in list-like fashion: "these things moved in these ways, those moved in those ways, ...".[13] We posit charge and mass because they enable a better theory, a theory that is both simple and strong.

Now, the alleged problem with first-order ZF cannot be that the presence of the Separation schema detracts from *simplicity*. For even though it contains infinitely many instances of the Separation schema, they collectively exhibit a syntactic pattern, since they are all instances of the schema. In fact, since our total theory includes logic, we must already be understanding infinite sets of sentences exhibiting such syntactic patterns as not detracting from simplicity. For one thing, logical axioms are usually stated using schemas, such as this one from propositional logic:

$$\big(\phi \to (\psi \to \phi)\big)$$

More importantly, logical theories are closed under rules of inference, such as modus ponens. This latter sort of simplicity is not a matter of the presence of one particular sort of sentence, but rather is relational and pertains to the total theory: whenever the theory contains, for example, sentences ϕ and $\phi \to \psi$, it also contains ψ.

The alleged problem must instead pertain to *strength*. A good theory must be both simple and strong; and although first-order ZF's infinitely many claims of set-existence are sufficiently simple (because of the syntactic pattern they collectively exemplify), perhaps they lack a certain important kind of strength—a kind of strength which is possessed by the second-order principle of Separation. For as we saw earlier, putting forward the Separation schema only implies the existence of subsets that are picked out by formulas in the language of set theory.

The argument can't be left there, however. In order for second-order logic to be useful, one needs a guarantee that various particular properties exist. For instance, to use the second-order separation axiom to carve out the subset of prime numbers from the set of all numbers, one needs to know that there is such a property as *being a prime number*. Thus typical axiomatizations of second-order logic include the following schema (where ϕ may be replaced

[13] See Sider (2020*b*, sections 4.4 and 4.12) for a discussion of some related issues.

with any formula with no free occurrences of variables other than x):[14]

$$\exists F \forall x \big(F(x) \leftrightarrow \phi \big) \qquad \text{(Comprehension schema)}$$

So although the *set*-existence principles in second-order ZF are nonschematic, the *property*-existence principles are schematic. Thus second-order ZF has a limitation of strength that is parallel to that of first-order ZF. The limitation occurs, to be sure, within the logical part of the theory (if we count second-order logic as logic); but it's hard to see how that matters.

The ZF argument cannot, then, be based on a general complaint about merely schematic strength. Can the complaint against first-order ZF simply be that a properly strong set theory ought to imply the law-like generalization that there is a subset of a given set for each property (with 'each property' understood in the second-order sense)? That accusation of weakness would be unimpressive, for the description of the generalization uses the very vocabulary (namely, second-order quantification) whose legitimacy is at issue. The second-orderist would be trying to simultaneously persuade us of the existence of a gap in first-order ZF and of the means to fill it. It isn't as if first-orderists can be convicted by their own lights of omitting a lawlike generalization from their theory, since the alleged generalization isn't even statable in their language. Secular particle physicists would be similarly unimpressed by the criticism that their theory leaves out lawlike generalizations about "haloed" particles.

To make this crystal clear, consider the accusation that even second-order ZF is too weak, because although it asserts the existence of subsets corresponding to each property, its merely schematic theory of the existence of properties fails to include the generalization that there exists a property for each "super-property". This imagined objector employs a "super-second-order quantifier", a sort of quantifier that is syntactically like ordinary second-order quantifiers, and claims that this new quantifier must be recognized in order to state a nontrivial, nonschematic existence principle for the ordinary second-order quantifiers. This objection is a failure, and not only because its underlying sensibility

[14] And perhaps parameters. A similar point can be made about second-order approaches that replace the Comprehension schema with λ abstraction and rules of universal instantiation and β conversion.

Despite its similarity to the principle of naïve comprehension for sets that is refuted by Russell's paradox, the Comprehension schema does not lead to Russell's paradox. Although $\sim x \in x$ is a grammatical formula of first-order set theory, $\sim F(F)$ is not a grammatical formula of second-order logic; so the paradox doesn't get off the ground.

would also call for recognizing super-duper-second-order quantification, super-duper-schmooper-second-order quantification, and so on (each to provide a nonschematic comprehension principle for the previous). It's a failure for the reason mentioned above: the alleged generalization that is missing from the second-order theory of properties is only recognized by someone who already accepts the ideology of super-second-order quantification.

Might the defender of the ZF argument somehow argue that, although merely schematic strength is not inherently problematic, and although first-order ZF has no failure of strength that it is dialectically appropriate to insist on, nevertheless there is still some sense in which second-order ZF is a better theory—is somehow more explanatory? It's hard to see what sense that would be. It is perhaps fruitful to compare our dialectic with a similar one involving laws of nature. Consider the (toy) law that like-charged particles repel one another. "Deflationists" about laws of nature, such as defenders of the Humean or best-system theory, think that this amounts to nothing more than the regularity that all like-charged particles in fact repel, plus some bells and whistles.[15] "Inflationists" about laws, on the other hand, think that there is some kind of further fact, the law, which explains the regularity that like-charged particles repel.[16] But even the inflationists reject an extreme inflationism according to which a good explanation of the regularity requires a still further fact, a **Meta-Law** governing the law; for the **Meta-Law** seems to be explanatorily superfluous.[17] Further, although deflationists regard inflationists' robust law as explanatorily superfluous, many of them reject the extreme deflationism of someone like Michael Esfeld (2020), who thinks that the posit of charge is superfluous since we could just as well state the law of motion by saying (roughly) that particles move in certain ways, namely the ways they would move if there were a property of charge; according to Esfeld, adding that the differences in motion are due to differences in charge does not improve the explanation. It is not obvious who is right in this dialectic; the answer turns on difficult questions about explanation.[18] But I suspect that most will agree either with the standard deflationist or the standard inflationist, and will reject both extreme deflationism (on the grounds that eliminating charge from physics

[15] See, for instance, Lewis (1994).

[16] See, for example, Armstrong (1983).

[17] Some inflationists, such as Lange (2007), accept laws governing other laws in certain cases, but this doesn't affect the point. Lange doesn't think that laws cannot govern without meta-laws, or that his meta-laws need further meta-meta-laws.

[18] See Dorr (2007, section 3; 2010, p. 160–3) and Sider (2020*b*, section 4.12).

incurs explanatory loss) and certainly extreme inflationism (on the grounds that the **Meta-Law** is explanatorily superfluous). What seems particularly objectionable about extreme inflationism is the parallelism between its explanation of regularities and the inflationist's explanation: the two are exactly alike except for an added layer in the former case. The idea that second-order ZF is somehow explanatorily superior to first-order ZF is similarly objectionable, since the strength of its existence-principles is parallel to that of first-order ZF, but with an additional layer.

5. The argument from model theory

Another argument for higher-order languages is that they are needed for a good model-theoretic account of unrestricted quantifiers.[19]

A model for a language is normally defined as an ordered pair $\langle D, I\rangle$, where D, the domain, is a set, and where I, the interpretation function, is a function that assigns to each nonlogical expression in the language some appropriate set-theoretic construction based on D, such as members of D to names, and sets of n-tuples of D (extensions) to predicates. Using methods from Tarski, one can define what it means for an arbitrary sentence of the language to be "true in" such a model.

Thus the standard approach defines the domain of any model, and the extension of any predicate in any model, as sets. But that is limiting. In the intended interpretation of the language of first-order ZF set theory, for instance, the quantifiers range over all sets and '$x \in y$' means that x is a member of y. So the domain of a model corresponding to this intended interpretation should be a set containing all sets, and the extension of '$\in$' should be a set containing all and only ordered pairs $\langle x, y\rangle$ where x is a member of y. But there are no such sets according to ZF set theory (which is assumed in the metalanguage), since either would lead to Russell's contradiction.

Some think of model theory as a theory of meaning, an account of how sentences come to be true and false. Others have more limited aspirations, for instance merely to give an account of logical consequence.[20] Either way, there is a concern that models as standardly defined can provide only a distorted representation of unrestricted quantification over sets, by treating the quantifiers

[19] See, for instance, Williamson (2003).
[20] For the latter viewpoint see Burgess (2008).

as being restricted to a mere part of the set-theoretic hierarchy.[21]

Against this backdrop, the higher-order outlook becomes attractive. For one can, in a second-order language, define a sort of "model" in which the "domain" can contain all the sets, and in which the "extension" of '$\in$' contains all and only the ordered pairs $\langle x, y\rangle$ where x is a member of y. The trick is to not view models as entities, and instead to treat quantification over models as being second-order.[22] We define 'R is a model', for R a second-order dyadic variable, in such a way that when R is a model, '$R(x, y)$' can be thought of as meaning that y is a semantic value of the linguistic expression x. So if π is a two-place predicate, then "$R\big(\pi, \langle u, v\rangle\big)$" can be thought of as meaning that $\langle u, v\rangle$ is "in the extension of π in R", although this is misleading since we are not accepting the existence of an entity, the extension of π in R. In one of these second-order "models" the "extension" of the two-place predicate '$\in$' will consist of all and only the ordered pairs whose first coordinate is a member of the second coordinate. That is: for some model R, for any z, $R(`\in\text{'}, z)$ if and only if z is an ordered pair $\langle u, v\rangle$ such that u is a member of v. No paradox results because we do not recognize an entity as the extension of '$\in$'. One can then define the notion of an arbitrary sentence in the language of first-order set theory being true in such a "model" R.

By adopting a second-order language, then, it is alleged that we can state more adequate model theories for first-order languages. But we will then naturally aspire to state model theories for second-order languages, such as the metalanguage just used in the model theory for the first-order language. And as Øystein Linnebo and Agustín Rayo (2012) argue, if we want to acknowledge the full range of "models" for second-order languages, a third-order metalanguage will be needed. Let Π be some one-place second-order predicate constant in the second-order language. For any property, $\mathscr{F}$, of properties (variable of type $\big((e)\big)$), it would be possible to interpret Π so that it applies to exactly the properties G (variable of type (e)) such that $\mathscr{F}(G)$. So for each such $\mathscr{F}$, there must be a new model R. But it can be shown that there are more such $\mathscr{F}$s than there are second-order relations R. (The argument is analogous to the usual

[21]There are subtle arguments that this limitation does not affect which sentences the standard approach counts as valid or implying one another. But such arguments are less clearly correct when the languages move beyond the first order, and break down if the language contains certain sorts of expressions. And even if it always gives the right answers, the standard approach would seem still to be mis-modeling the facts. See Boolos (1985); McGee (1992); Rayo and Uzquiano (1999) for discussion.

[22]See Boolos (1985); Rayo and Uzquiano (1999); Williamson (2003).

Cantorian diagonal argument showing that a set has strictly lower cardinality than its power set, but here the "cardinality comparison" and argument are made in a higher-order language.) The models for the second-order language must be third-order relations.

In fact, Linnebo and Rayo show how to generalize this argument into the transfinite. For each language L_α in a certain transfinite hierarchy, stating its model theory requires a still higher order metalanguage L_{α^+}.

This fact weakens the argument from model theory, if that argument is taken in the metaphysical spirit of this paper. We began with an explanatory ambition: to give a certain sort of theory for the language of first-order set theory. If we take that ambition to justify recognizing second-order quantification, with its attendant worldly complexity[23], then we are saddled with a new explanatory ambition, the satisfaction of which requires a new language, which results in a new explanatory ambition; and so on. The explanatory demand is insatiable, in that there is no language we could speak in Linnebo and Rayo's hierarchy in which we could state a model theory for all languages in that hierarchy.

Will we eventually resist the explanatory demand, and say that for some final language L_α, no explanatory model theory can be given (after having recognized all the worldly complexity of the preceding higher-order languages)? Embracing explanations up to L_α but no further seems akin to embracing, in addition to robust laws, also meta-laws, meta-meta-laws, and so on, up to a certain point and then stopping; or embracing, in addition to sets, second-order quantification, super-second-order quantification, super-duper-second-order quantification, and so on up to a certain point, and then stopping. If we must eventually resist such explanatory demands, wouldn't it have been better to do so right at the start, with the language of first-order set theory? Doing so wouldn't mean saying that this language is meaningless, or contains no logical consequences, or doesn't quantify over all sets after all; it rather means that we won't give a certain sort of *theory* of that language. Nor does it mean that no theory *whatsoever* is available. The usual models of model theory can still be models in the philosophy-of-science sense, albeit imperfect ones, of meaning or logical consequence; we could still give other sorts of semantic theories, such as a Davidsonian (1967) theory of truth for the language of set theory; we could give theories that are nonDavidsonian and not mere models, but which

[23]Here again we rely only on the most secure sort of complexity judgment from section 2: that a higher-order theory that includes an ontology of sets is more complex than first-order set theory.

are not fully comprehensive; and so on. It's a bit sad, but not the end of the world.

The alternative would be to continue to acquiesce to the explanatory demand, formulating more and more theories in increasingly higher-order languages, limited only by patience and lifespan. There is no vicious regress here, even if the theories we are giving are semantic ones, since the theory we formulate at a given stage does not *convey meaning on* the preceding language; rather, the meaningfulness of the preceding language is taken as a pre-existing fact, which the subsequent language is used to explain. The series of semantic explanations might be compared to a series of causal explanations of what occurs at some time in terms of what occurs at some preceding time—a series that we might legitimately continue indefinitely.[24] I don't wish to deny that explanatory gains could indeed be made at each stage in this series of semantic explanations. But each explanatory gain comes with a cost in complexity; and when we look ahead at the looming indefinite series, the appeal of its first step is diminished, in comparison with cutting the whole thing off at the start and sticking with first-order logic and imperfect model theory.

6. Arguments from categoricity

Second-order logic has distinctive model-theoretic properties.[25] For instance, using a second-order language containing expressions for 'zero', 'successor', 'plus', and 'times', one can state a theory of the arithmetic of natural numbers that is "categorical": any two of its models are isomorphic. (Each model has the familiar "shape" of the natural number line: all elements are arranged in a line in which each element is reachable from an initial element by some finite number of discrete jumps.) But in a first-order language using these nonlogical expressions, no theory has this feature. If a theory has a model of the familiar sort, it will also have models of arbitrarily large cardinality, and even "nonstandard" models which fail to be isomorphic to the familiar sort despite having the same number of elements.

Facts like these weigh heavily with some fans of second-order logic. But they don't add up to a convincing argument that second- (or higher-) order logic is in good standing.

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[24]Thanks to Timothy Williamson here.

[25]For the formal results alluded to in this section, see Shapiro (1991, chapter 4) and Väänänen (2019).

One argument that might be based on the facts is metasemantic: without second-order resources, arithmetic language could not have the determinate interpretation that it in fact has. If our theory were merely first-order, according to this argument, nothing would rule out intended or correct interpretations based on nonstandard models.

This argument appears to presuppose an "interpretationist" (Williams, 2007) approach to metasemantics, according to which the only constraint on the correct interpretation of arithmetic language is that our theory of arithmetic come out true under the interpretation. But interpretationism isn't true for *all* language.[26] If it were, then any consistent theory of physics (for instance) would have a correct interpretation under which it comes out true, provided there are enough entities in the world. So if the argument is to even get off the ground, it must provide a reason to think that interpretationism is true of mathematical language despite being false generally. For instance, it might be thought that in physics, the additional constraint on correct interpretation is causal—that our usage of physical predicates must bear certain causal relations to their semantic values—whereas causal constraints are inapplicable to mathematical language.

Thus understood the argument relies on the contentious assumption that the only additional constraint on correct interpretation is causal. That premise would be rejected, for instance, by David Lewis (1983, 1984), whose proposed additional constraint *is* applicable to mathematical language: correct interpretations must, other things being equal, assign "natural" properties and relations as semantic values.

And that isn't the argument's only contentious assumption. For although the models of second-order arithmetic are all isomorphic, they are not identical. If nothing constrains the correct interpretation of arithmetic language beyond that our arithmetic theory must come out true, then there are no constraints on *which* particular entities count as natural numbers. There will be correct interpretations, for instance, in which Julius Caesar is the denotation of the symbol 'zero'. Thus the argument cannot be: "arithmetic language has an *absolutely* determinate interpretation, and only with second-order resources can this be secured", since even with second-order resources, arithmetic would not be absolutely determinate (given interpretationism for arithmetic language).[27] It must rather be: "arithmetic language, although not absolutely determinate,

[26] This is the (unintentional) lesson of Putnam's "model-theoretic argument (1978, part IV; 1980; 1981, chapter 2).

[27] Compare Weston (1976, section V).

is determinate up to ismorphism, and only with second-order resources can this be secured". Thus the argument must rely on a sort of "structuralism".

When we turn from arithmetic to set theory, even more structuralism will be required, so to speak. For in that case, the second-order mathematical theory—namely, second-order ZF—does not quite constrain its models up to isomorphism. Rather, all that is guaranteed is that in a certain sense, any two models have isomorphic initial segments. The argument in this case would thus need to rely on the premise that set-theoretic language is that determinate, but no more.

But all this skirmishing is beside the main point, which is the argument's uncritical stance toward the second-order quantifiers.[28]

The claim that the models of second-order arithmetic are all isomorphic relies on certain definitions from (standard, set-theoretic) model theory. In the "standard" (or "full") definition of model for second-order logic, the monadic second-order quantifiers are treated as ranging over all subsets of the domain; it is this definition that was presupposed above. But if the second-order quantifiers are instead treated as ranging only over a restricted range of subsets of the domain, with the range varying from model to model (as in "general", or "Henkin" second-order models), nonisomorphic models of second-order arithmetic reappear. The standard definition of model is appropriate given the interpretation of the second-order quantifiers that the higher-orderist advocates, namely as ranging over absolutely all properties (to put it intuitively); but what is the metasemantics of that interpretation? Its correctness cannot be secured solely by our putting forward the theory of second-order logic (including all instances of the comprehension schema), since that theory has non-full Henkin models.

The second-orderist seems to be exempting the second-order quantifiers from interpretationist metasemantics. But if they are exempt, then why not exempt arithmetic or set-theoretic language as well? "The second-order quantifiers are part of logic" is no answer without a specification of the scope of "logic" under which the second-order quantifiers but not arithmetic or set-theoretic vocabulary count as logical, and without an argument that logical, and only logical, vocabulary thus understood is exempt from interpretationism. The metasemantic argument is a dialectical failure, since the higher-orderist is in no position to object to a first-orderist who denies interpretationism, and claims that the languages of first-order arithmetic or set-theory have determinate

[28]Again compare Weston (1976).

interpretations (whether absolutely or in some restricted sense) despite having nonisomorphic models.

Instead of the metasemantic argument, the higher-orderist might simply argue that the virtue of second-order logic is that it allows us to single out, by purely logical means, the intended class of structures in arithmetic, and to nearly single out the intended class of structures in set theory. But what is so special about singling out these structures by "logical" means? Assuming that the determinacy of set-theoretic vocabulary is no longer in question (we have left the metasemantic argument behind), even without second-order logic we can single out the arithmetic structures by *set-theoretic* means, by saying (for example) that they are the ones that are isomorphic to the von Neumann finite ordinals; and we can of course single out the unique set-theoretic "structure" by set theoretic means, by saying simply that it is the structure exhibited by the sets.

7. The collapse argument

Our question has been whether higher-order quantifiers are in good standing. But suppose we construe "not in good standing" as vagueness, and suppose vagueness requires multiple inequivalent precisifications, or candidate meanings, no one of which is determinately meant. (This needn't be tied to the supervaluationist approach to vagueness; all that is assumed is that indeterminacy requires precisifications.) One might then use a "collapse argument" to argue that the higher-order quantifiers could not possibly have multiple precisifications, and thus could not possibly fail to be in good standing.

Collapse arguments aim to show that if putatively distinct candidate meanings for a logical constant obey the standard rules of inference, they must be provably equivalent to one another. For example, let '$\&_1$' and '$\&_2$' express two putatively distinct meanings, but suppose that each expression individually obeys conjunction introduction and elimination. Then $A \&_1 B$ implies A by conjunction elimination for $\&_1$; and similarly it implies B; but A and B together imply $A \&_2 B$ by conjunction introduction for $\&_2$. A similar argument (using conjunction elimination for $\&_2$ and conjunction introduction for $\&_1$) shows that $A \&_2 B$ implies $A \&_1 B$. Thus $\&_1$ and $\&_2$ always generate mutually derivable sentences, and so are in this sense equivalent.

The state of the art on collapse arguments is Dorr (2014), who construes them a little differently. Instead of using a language enhanced by logical constants for putative precisifications (compare '$\&_1$' and '$\&_2$'), Dorr uses a higher-order language to quantify over precisifications, and characterizes their logical features algebraically. To apply Dorr's strategy in the case of quantifiers, we treat quantifier-meanings, and hence precisifications of quantifiers, as properties of properties (recall the end of section 1). Let Q_1 and Q_2 be any two such precisifications of some higher-order quantifier '$\exists$' over "entities of type τ".[29] Dorr formulates the idea that these precisifications have the "standard logical features" of existential quantifiers by saying that they obey existential introduction and existential elimination ("intro" and "elim" for short) in the following senses:[30]

Q obeys intro $=_{\text{df}}$ Any property F property-entails the property *being such that* $Q(F)$

Q obeys elim $=_{\text{df}}$ For any proposition p, if a property F property-entails the property of *being such that p is true*, then $Q(F)$ proposition-entails p

where 'F' is a predicate of τ-entities (type (τ)), 'p' is a sentential variable (type $()$), 'property-entails' is a two-place predicate whose arguments are predicates of τ-entities (type $\big((\tau),(\tau)\big)$), and 'proposition-entails' is a two-place predicate whose arguments are sentential (type $\big((),()\big)$). (As Dorr explains, there are different ways that entailment, in various types, can be understood.) The collapse argument then runs as follows. For any F, since Q_2 obeys intro, F property-entails the property of *being such that* $Q_2(F)$; but since Q_1 obeys elim, the proposition $Q_1(F)$ proposition-entails the proposition $Q_2(F)$. A similar argument (using intro for Q_1 and elim for Q_2) shows that $Q_2(F)$ proposition-entails $Q_1(F)$. Thus Q_1 and Q_2 generate mutually entailing propositions from any property F.

There is a certain pitfall for reasoning about precisifications, which is illustrated by the following argument:

> (i) The sentence 'for any x, x is bald iff x instantiates the property of baldness' is definitely true. So (ii) for each precisification, b, of 'bald': for any x, x is b iff x instantiates the property of baldness.

[29]Thus $\exists$ is of type $\big((\tau)\big)$; the variables 'Q_1' and 'Q_2' are therefore of this type. The quantifiers binding 'Q_1' and 'Q_2', or rather, attaching to λ abstracts for properties of entities of this type, therefore have the type $\Big(\Big(\big(((\tau))\big)\Big)\Big)$.

[30]Elim is analogous to the $\exists L$ sequent rule.

So (iii) any two such precisifications, b_1 and b_2, must be coextensive: for any x, x is b_1 iff x instantiates baldness iff x is b_2.

The conclusion of the argument is obviously false, but what went wrong?

The problem involves the fact that the expression 'baldness' is also vague, and moreover is "penumbrally connected" (Fine, 1975) to 'bald'. That is, the precisifications of 'bald' and 'baldness' are coordinated: each expression can be precisified in many ways, but whenever one is precisified, the other must be precisified in a corresponding way. So what follows from (i) is not (ii), but rather that a given precisification b_i of 'bald' is had by all and only those things that instantiate *the corresponding precisification of baldness*, baldness$_i$. And this does not imply that b_i and b_j are coextensive when $i \neq j$.

The moral is that instances of the following schematic inference are not in general valid, where $S(T)$ is some sentence containing a term, T:

'$S(T)$' is definitely true

Therefore, for each precisification, t, of 'T', $S(t)$

The definite truth of a sentence does not require that the precisifications of a given one of its vague terms must individually satisfy that sentence (so to speak); rather, it requires that any sequence of coordinated precisifications of *all* the sentence's vague terms must collectively satisfy that sentence.

Return now to the collapse argument. Provided we grant its premise that any precisification of '∃' obeys intro and elim, the argument succeeds in showing that any two such precisifications are equivalent (in the sense described above). But what is the justification of that premise?

Suppose the justification is the (plausible) claim that the sentence '∃ obeys intro and elim' is definitely true. (We will consider another way of supporting the premise at the end of the section.) The derivation of the premise from this claim then relies on the problematic inference. All that the definite truth of '∃ obeys intro and elim' tells us is that precisifications Q_i of '∃' obey corresponding precisifications intro$_i$ and elim$_i$ of 'intro' and 'elim'. Thus in the collapse argument, instead of the claim that Q_1 and Q_2 each obey intro and elim, we would instead have the claim that Q_1 obeys intro$_1$ and elim$_1$, and that Q_2 obeys intro$_2$ and elim$_2$. Since the argument relies on applying intro and elim in a *single* sense to *both* Q_1 and Q_2, the argument would fail.

Now, the argument would be reinstated if 'intro' and 'elim' aren't vague, since then intro$_i$ = intro and elim$_i$ = elim for all i. It would also be reinstated if 'intro' and 'elim' are vague but penumbrally unconnected to '∃'. For in that case (in which the vaguenesses of '∃' and 'intro'/'elim' are "orthogonal"), for any ways that '∃' and 'intro'/'elim' can be precisified individually, they can simultaneously be precisified in those ways, which would yield a single precisification of 'intro' and 'elim' that is obeyed by both Q_1 and Q_2.[31] But if 'intro' and 'elim' are vague and penumbrally connected to '∃', then we are left with each of Q_1 and Q_2 obeying its own precisifications of 'intro' and 'elim', and no guarantee of there being any precisifications of 'intro' and 'elim' obeyed by both.

Are 'intro' and 'elim' vague and penumbrally connected to '∃'? The crucial expressions in the definitions of 'intro' and 'elim' are 'property-entails' and 'proposition-entails'.[32] So the question is whether those expressions for entailment are vague and penumbrally connected to '∃'.

The answer may well be yes. For example, here is one natural picture of how the higher-order quantifier '∃' could be vague: the entire higher-order apparatus, including all higher-order quantifiers and all entailment predicates, has multiple precisifications corresponding to different conceptions of grain: one for modal individuation, another for extensional individuation, and so on. This would require coordinated variation in the precisifications of entailment predicates and higher-order quantifiers of all higher-order types.[33]

To be sure, anyone who is antecedently committed to 'entails' (of all types) *not* being vague could use the collapse argument to conclude that higher-order quantifiers aren't vague either. This might well be a reasonable position for a higher-orderist to take. But a higher-order skeptic might not share the

[31]Applying the orthogonality principle to the group of coordinated precisifications $\langle Q_1$, $\text{intro}_1, \text{elim}_1\rangle$ and the group $\langle Q_2$, $\text{intro}_2, \text{elim}_2\rangle$ yields a third group $\langle Q_3$, $\text{intro}_3, \text{elim}_3\rangle$ where $Q_3 = Q_1$, and $\text{intro}_3 = \text{intro}_2$ and $\text{elim}_3 = \text{elim}_2$. Given the claim that each Q_i obeys intro_i and elim_i, Q_3 (i.e., Q_1) obeys intro_3 and elim_3 (i.e., intro_2 and elim_2); and, applying that claim again, Q_2 obeys intro_2 and elim_2; thus both Q_1 and Q_2 obey intro_2 and elim_2.

[32]Those definitions also contain propositional and property quantifiers, which are also plausibly penumbrally connected to '∃'. However, Dorr pointed out a way of eliminating those quantifiers from the definitions. Where '$\leq$' expresses entailment, 'Q obeys intro' can be defined as meaning $(\lambda X p.Q(X) \leq p) \leq (\lambda X p.X \leq \lambda y.p)$, and '$Q$ obeys elim' as meaning $(\lambda X p.X \leq \lambda y.p) \leq (\lambda X p.Q(X) \leq p)$. (Compare sequent quantifier rules. The variables and symbols '$\leq$' are to be understood as having appropriate types.) Given certain natural assumptions governing '$\leq$' one can then argue that for any Q_1 and Q_2 obeying intro and elim, $Q_1 \leq Q_2$ and $Q_2 \leq Q_1$.

[33]This argument for coordinated variation in the meanings of '∃' and the predicates of entailment cannot be made in the context of Dorr's 2014 paper, since the quantifier there at issue is the *first*-order quantifier.

commitment, and thus could reject the collapse argument as we are currently understanding it.

Our response to the collapse argument has been based on the supposition that its premise—that all precisifications of the quantifier in question obey intro and elim—is justified by the claim that the sentence '∃ obeys intro and elim' is definitely true. But that premise might be supported in some other way. For example, it might be supported by the claim that the facts about entailment (in various types) play a central metasemantic role, a central role in determining what logical expressions mean.[34] Roughly put, there is metasemantic pressure to assign intro- and elim-obeying meanings to any expressions, in any language, that are used in anything like the way in which we use quantifiers.

Even this alternate way of supporting the collapse argument's premise might be undermined if the entailment predicates are vague and penumbrally connected to the quantifier in question. However, the issue is complex, since it depends on difficult questions about the relationship between meaning and precisifications. All too briefly, here are two pictures.

Picture 1: metasemantic pressure applies directly to the precisification-of relation: there is metasemantic pressure toward assigning intro- and elim-obeying precisifications to any quantifiers in any language remotely like ours. This, I take it, is Dorr's picture, since on his view, multiple precisifications are simply meanings: meaning is multiple.[35] Then even if the entailment predicates and hence 'intro' and 'elim' are vague, it would still be the case that all precisifications of a given quantifier '∃' would obey intro and elim, in which case, by the collapse argument, all such precisifications would be pairwise equivalent. Thus '∃' would not be vague. (It would, however, presumably be subject to higher-order vagueness, if 'intro' and 'elim' are vague. It might be, for example, that all precisifications$_1$ of 'higher-order entities are individuated modally' are true, whereas all precisifications$_2$ of that sentence are false, where precisification$_1$ and precisification$_2$ are two precisifications of 'precisification'.)

Picture 2: metasemantic pressure applies only indirectly to precisifications. An expression's precisifications result from indeterminacy in the determination of that expression's meaning. So if there is metasemantic pressure toward assigning intro- and elim-obeying meanings to a quantifier '∃', and if 'intro' and 'elim' (via entailment predicates) are vague—as regards grain, perhaps—

[34] Compare Lewis's naturalness-based metasemantics for nonlogical expressions, mentioned in section 6. Thanks to Dorr for helpful discussion here and elsewhere in this section.

[35] See his forthcoming book *The Multiplicity of Meaning*.

then '∃' will have a corresponding range of precisifications, varying in grain perhaps, and thus would be vague.

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