Realism and Lexical Flexibility

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Abstract

Metaphysical investigation often proceeds by way of linguistic meaning. This tradition relies on an assumption about meanings, namely that they can be given in terms of referential relations and truth. Chomsky and others have illustrated the difficulty with this externalist hypothesis regarding natural language meanings, which implies that natural languages are ill-suited for the purposes of metaphysical investigation. In reply to this discordance between the features of natural languages and the goals of metaphysical investigation, metaphysicians propose that we look to the invented languages used to express our best scientific theories as metaphysical guides, in the hope that these languages are better behaved. I argue that this retreat is beset with similar troubles for the metaphysician. In particular, I'll argue that central terms in the field of biology, namely 'gene' and 'species', exhibit a kind of *lexical flexibility* that renders them, much like their natural language counterparts, ill-suited for the metaphysician's aims.

1 Introduction

Contemporary metaphysical inquiry is guided by an appeal to linguistic meaning, and is committed to an *externalist* semantics for the language used in such inquiry.[1] More explicitly, this tradition assumes the following externalist hypothesis:

($\mathcal{E}$) For any expression e (in some language L), the meaning of e determines e's satisfaction/truth conditions.

However, the judgments of natural language speakers are poorly captured by such a semantics, as exhibited in the following:

(1) Kansas is the birthplace of Eisenhower, and it is conservative.

Sentences like (1) exhibit a kind of *lexical flexibility* ubiquitous in natural language. Such flexibility resists externalist treatment, since an externalist semantics requires that for sentences like (1) to be true there must be objects (like Kansas) that are both abstract (by having political views) and concrete (by being the location of birthing events). Whatever one might want to say about locations, they surely do not have political affiliations, even if inhabitants of those locations do.[2] However, given the trouble posed for an externalist semantics by lexical flexibility, metaphysicians should abandon the use of natural languages as tools for metaphysical investigation.

As a result, one committed to metaphysical inquiry might abandon the use of natural languages, and turn to languages that are better-suited for such purposes—namely, those languages invented for the purposes of scientific inquiry. This metaphysical methodology that appeals to the epistemic

[1]It is difficult to overstate how prevalent the appeal to sentences, truth, grammar, translation, and other linguistic properties is in the field of metaphysics. Consider the following non-exhaustive list of canonical works that engage in such inquiry under the guise of metaphysics: Plantinga (1974); Jackson (1982); Lewis (1986); Kim (1998); Smart (1963); Wiggins (1980); Kripke (1980); Hawthorne (2002); Thomasson (1999); van Inwagen (1990); for more contemporary work consider Bennett (2016) who attributes this method to "almost all... philosophers..."(p. 26); see also the majority of articles in (and introduction to) the volume *Contemporary Debates in Metaphysics*: Swoyer (2008); Dorr (2008); Carroll (2008); Schaffer (2008); Bricker (2008); Melia (2008); Thomson (1997); Parfit (2008); Zimmerman (2008); Smart (2008); Sider (2008); Hawthorne (2008); Kane (2008); Vihvelin (2008); van Cleve (2008); Markosian (2008).

[2]I address examples like these in more detail below.

credentials of naturalist inquiry is rooted in Quine, and holds the following tenets:

($\mathcal{T}1$). There is a mind-external world

($\mathcal{T}2$). The mind-external world has the quantificational structure roughly akin to a classical logic

($\mathcal{T}3$). There is a unique language that mirrors the quantificational structure of the mind-external world; call this the language of ontology, $\mathcal{L}_{\mathcal{O}}$

($\mathcal{T}4$). The language(s) used to express our best scientific theories is(/are) the language(s) ($\mathcal{L}_{\mathcal{O}}$) that mirrors the quantificational structure of the mind-external world

If there is a mind external world with the quantificational structure of a classical logic, and there is a language ($\mathcal{L}_{\mathcal{O}}$) that mirrors this structure, then a metaphysician can explore ontological questions by looking at the externalist meanings of expressions in that language. Call this the Quinean Realist[3] methodology. I take the first claim to be unassailable. The second and third claims are less so, and recent debates in the metaontological literature highlight this (see Hirsch, 2002, 2008; Sider, 2009). But the focus of this paper is on the last of these claims ($\mathcal{T}4$), and I'll argue that we have reason to doubt the veracity of this claim. In particular, the languages used in our best biological sciences do not seem amenable to the Realist's methodology. Such languages, I argue, exhibit a kind of flexibility[4] that Chomsky and

[3]Such a Realist is Quinean in the sense that she adopts a certain metaontological view, in contrast to the Pluralism propounded by Carnap (1950). Whether or not Quine himself would endorse this position is beside the point. Such a Realist is a realist because they hold that there is an objective structure to the world. While these two positions seem interrelated, there is reason to think neither entails the other (Hirsch, 2002). For these reasons I've highlighted the distinction here, though from here on, I'll be using 'Realist' and its neighboring forms as short-hand for the more cumbersome and possibly misleading 'Quinean Realist', and this usage applies to any metaphysician that accepts ($\mathcal{T}1$), ($\mathcal{T}2$), ($\mathcal{T}3$), and (some variant or other of) ($\mathcal{T}4$).

[4]Often this type of phenomenon is called 'polysemy' in natural language semantics. I avoid this usage for two reasons; one of them priggish, and the other substantive. The priggish reason is that 'polysemy' indicates (by its roots) that a polysemous expression has multiple meanings. While they do have many meanings, ambiguous/homophonous expressions likewise have multiple meanings—they are polysemous. However, these two distinct semantic profiles should stand in contrast, given the difficulty for externalist se-

others have highlighted as a feature of natural languages, and this feature renders scientific languages, much like their natural language counterparts, poorly suited for ontological investigation.

Understood in this way, the arguments set forth here seemingly target a narrow class of metaphysicians—namely those that accept ($\mathcal{T}1$)–($\mathcal{T}4$). But this is misleading. Any Realist, at least if she hopes to engage in ontological investigation, must accept some form of ($\mathcal{T}4$) above, since some language or other must be used in endeavoring to address ontological dispute. Consider the following variant of ($\mathcal{T}4$):

($\mathcal{T}4$) The language(s) used <u>to express our best scientific theories</u> is(/are) the language(s) ($\mathcal{L}_{\mathcal{O}}$) that mirrors the quantificational structure of the mind-external world.

($\mathcal{T}4'$) The language(s) used <u>by natural language speakers</u> is(/are) the language(s) ($\mathcal{L}_{\mathcal{O}}$) that mirrors the quantificational structure of the mind-external world.

The Realist that accept ($\mathcal{T}4'$) is committed to the truth of ($\mathcal{E}$) as applied to natural languages, since the meanings of those languages will guide their investigation. The troubles pressed below for ($\mathcal{E}$) then apply rather broadly, and should concern any metaphysical practitioner that appeals to a case-based methodology in resolving ontological disputes (Vogel, 2018). This form of argument contends that the use of language in discourse about some particular subject matter has ontological implications, if such talk is to be considered coherent. As a consequence, this methodology uses natural language speaker judgments about individual (typically fictional) cases to adjudicate between various theories about a disputed metaphysical claim in the domain of interest.[5] Considerations of space here forestall any attempt at a substantive survey of the breadth of this class of case-based arguments. But, I take the arguments set forth below to apply to *any* metaphysician that

mantic theories in characterizing "polysemous" expressions, and not homophonous ones. The substantive reason for my usage here is that natural language expressions routinely exhibit lexical flexibility, which manifests with superficially different semantic behavior in a variety of cases, two of which I discuss here. However, these various manifestations of "polysemy" can be unified by internalist proposals; see Pietroski (2005), §3.2. See also Pietroski (2018).

[5]Baz (2017) refers to this as the 'method of cases'. The arguments here against such a methodology, however, are distinct from those Baz offers (sec Vogel, 2018).

(implicitly) embraces ($\mathcal{T}1$)–($\mathcal{T}3$), and ($\mathcal{T}4'$). Such a metaphysician, if she is to maintain her Realist methodology, must either offer a response to the problems posed by the lexical flexibility of natural language expressions, or reject her ($\mathcal{T}4$)-variant in favor of another. I argue that accepting ($\mathcal{T}4$) will not help the Realist avoid the problems posed by lexical flexibility since the languages used to express our best scientific theories also exhibit the lexical flexibility of natural languages.

I'll begin by detailing the Realist's methodology as defined by ($\mathcal{T}1$)–($\mathcal{T}3$) and some ($\mathcal{T}4$)-variant. I'll then review the case of flexibility for natural language to make clear the features relevant to our current inquiry, namely those features that render languages particularly hostile to externalist treatment, and therefore inept for ontological investigation. Put another way, natural languages (or even their precisified counterparts) do not serve as good candidate languages for the ontological purposes of the Realist that accepts ($\mathcal{T}1$)–($\mathcal{T}3$) and ($\mathcal{T}4'$). The purpose of this review is the underscore the Realist's motivation for accepting ($\mathcal{T}4$), which asserts that the language used to express our best scientific theories can succeed where natural languages fail in addressing ontological questions. I will then show that the languages used to express our best biological theories exhibit this same flexibility, rendering them equally ill-suited for the Realist's task. Two central terms in the biological sciences, 'gene' and 'species', embody the flexibility scientific languages exhibit. What I take these considerations to show is that, despite the fact that these terms in the biological sciences fail to be either univocal or apt for externalist treatment in the way the Realist requires, these sciences show all the hallmarks of a good science: they are informative, make generalizations, and have testable predictions. Further, an externalist characterization of those terms can only be adopted at an explanatory cost. Thus, we should be skeptical of any ontological methodology that, prior to empirical investigation, places constraints on the kind of languages scientists can use in constructing their theories. Since there seems to be little justification for such constraints other than their coherence with metaphysical doctrine, this suggests that we should abandon these methodological constraints.

2 Natural Language, Flexibility, and Externalism

Realist commitments to tenets ($\mathcal{T}1$)–($\mathcal{T}3$), and thereby some variant of ($\mathcal{T}4$), require that the languages used for ontological investigation have an externalist semantics. Such a semantics holds (roughly), that for a given language, the meanings of expressions in that language determine the truth conditions for those expressions.[6] If the meanings of expressions in the language ($\mathcal{L}_{\mathcal{O}}$) that guides such investigation cannot be adequately captured by an externalist semantics, this speaks against the use of that language for the purposes of addressing metaphysical questions. I'll begin this section by explicating the Realist methodology in relation to the four tenets above, and explain how a Realist, i.e., a metaphysician committed to ($\mathcal{T}1$), ($\mathcal{T}2$), ($\mathcal{T}3$), and some variant of ($\mathcal{T}4$) is committed to the claim that ($\mathcal{E}$) holds for the language ($\mathcal{L}_{\mathcal{O}}$) indicated in the ($\mathcal{T}4$)-variant she accepts. I'll then close this section by highlighting the lexical flexibility exhibited by natural language expressions, illustrating that a Realist who accepts ($\mathcal{T}4'$) is burdened with accounting for such flexibility. In §3 I'll suggest that central terms in biology are lexically flexible in much the way natural language expressions are, posing a problem for the Realist that accepts ($\mathcal{T}4$).

2.1 Realism and Externalism

Taken together with ($\mathcal{T}1$)–($\mathcal{T}3$), the Realist that accepts ($\mathcal{T}4'$) is committed to the truth of ($\mathcal{E}$) for natural languages, at least if the meanings of expressions in natural language are to serve as useful guides for ontological investigation. The arguments in this section that highlight the flexibility of natural language expressions tell against such a use. If a suitable language, with an externalist semantics, cannot be found to replace natural languages (like English), the metaphysician's methodology is in trouble. In response to these concerns, the Realist might adopt tenet ($\mathcal{T}4$), insisting that the languages used to express our best scientific theories do not have terms with

[6]The rough qualification here pertains to the role of semantic context in determining the truth-conditions of an expression. That is, context-sensitive expressions like indexicals do not threaten the externalist hypothesis in the manner that lexically flexible expressions do. As I argue elsewhere, treating lexical flexibility as context-sensitivity, of the sort applied by an externalist treatment of indexicals, misrepresents the manner in which speakers treat such expressions (Vogel, 2016).

the flexible character of natural language expressions. Expressions in scientific languages, one might claim, exhibit the precision of terms in a classical logic—precision of the sort required for the Realist's methodology to succeed.

For a classical (invented) language, the meanings of expressions are the satisfaction conditions of those expressions, as satisfied by sequences of objects in a domain. ($\mathcal{T}3$) indicates that there is a language $\mathcal{L}_{\mathcal{O}}$, that has a semantics that can be given in terms of the objects of the world. After all, ($\mathcal{T}2$) asserts that the world has an object-based structure, which is mirrored by some language $\mathcal{L}_{\mathcal{O}}$ according to ($\mathcal{T}3$). If this domain is purported to be populated by the objects of the mind-external world, the meanings of expressions are externalist insofar as they depend on those objects as satisfiers of the relevant expressions. Different Realists disagree about which language(s) can do the work of $\mathcal{L}_{\mathcal{O}}$, and thereby accept different variants of ($\mathcal{T}4$), such as ($\mathcal{T}4'$). But, accepting ($\mathcal{T}2$) and ($\mathcal{T}3$) dictates that $\mathcal{L}_{\mathcal{O}}$ expressions have truth-condition determining meanings, since the object-based structure of reality is mirrored in the structure of $\mathcal{L}_{\mathcal{O}}$'s semantics. If $\mathcal{L}_{\mathcal{O}}$ failed to have a semantics wherein the meanings of expressions determined which objects in the world could serve as satisfiers of $\mathcal{L}_{\mathcal{O}}$ expressions, then the meanings of $\mathcal{L}_{\mathcal{O}}$ expressions would fail to inform us about ontology. In accepting some ($\mathcal{T}4$)-variant or other, the Realist is committed to the applicability of ($\mathcal{E}$) to whatever language they hypothesize as being $\mathcal{L}_{\mathcal{O}}$. The methodological upshot is that, if this proposed language is one whose meanings the Realist grasps, then those meanings can guide metaphysical inquiry. Put another way, for such a language if one knows what an expression in that language means, one thereby knows what the world must be like for that expression to be satisfied; concomitantly, one knows what objects serve as those satisfiers, thereby populating the worldly domain. In this way, the meanings of expression in a language ($\mathcal{L}_{\mathcal{O}}$) can yield verdicts about questions of ontology. But, if one can show that the meanings of expressions for a candidate language $\mathcal{L}_{\mathcal{O}}$ are poorly captured by an externalist semantics, this speaks against using that language for ontological investigation. In §2.2 I will show that the Realist that accepts ($\mathcal{T}4'$) is in trouble, given that natural languages exhibit a lexical flexibility that poses problem for an externalist semantics.

Assuming those arguments are successful, this poses a problem for the Realist that accepts ($\mathcal{T}4'$). The Realist might then abandon ($\mathcal{T}4'$) in favor of ($\mathcal{T}4$), hoping that the invented languages used to express our best scientific theories will exhibit the precision their natural language counterparts lack. After all, these language are invented with the purpose of perspicuously describing the mind external world, in a manner that natural languages are not. One might then expect scientific languages to be more amenable to externalist treatment.

This move has some obvious appeal for the Realist. And for this reason the invented languages used to express scientific theories are the focus of this paper. In particular, we'll be focused on the ontological *bona fides* of such languages. That is, we'll be investigating whether or not scientific languages are a viable option for pursuing the Realist's project. In order for the Realist's methodology to be capable of yielding answers to ontological questions, scientific languages must have an externalist semantics, whereby the meanings of each term in these languages are cashed out in terms of worldly objects. As such these invented languages must not be burdened by many of the properties of natural languages, or at least not by those properties that make natural languages hostile to externalist treatment.

For the Realist adopting this strategy, the goal of the scientific enterprise is to develop a language that "carves nature at its joints." One way of thinking about the language the Realist requires is suggested by Sider (2009), in a discussion regarding the interpretation of the existential quantifier:

> Clearly there are multiple (inferentially and materially adequate) interpretations of quantifiers. As I see it, the real issue is whether any of these interpretations is metaphysically distinguished, whether any of them uniquely matches the structure of the world, whether any carves nature at the joints better than the others. (Sider, 2009, pp. 392)

The picture underlying these claims is of a plenitude of (invented) languages, each with an externalist semantics, and (let's suppose) free from the pitfalls of natural language, with its ambiguities and lexical flexibility. For these languages, Frege's puzzle does not arise, since each term's meaning picks out a unique object. Nor is vagueness a difficulty for users of these languages, as each predicate meets with the precision commensurate with an algebraic model. Somewhere in this vast plenitude of languages there is special one, that "matches the structure of the world." This special language is the one that best describes the world, whose existential quantifier "carves nature at its joints." This supposition, as codified in tenet ($\mathcal{T}3$) above, implies a further claim—namely, that this special language meets the externalist condition indicated in ($\mathcal{E}$).

The privileged language of ontology ($\mathcal{L}_{\mathcal{O}}$) must, as a consequence of mirroring the qualificational structure supposed by the Realist, abide by the externalist hypothesis ($\mathcal{E}$).[7] Any metaphysician that accepts tenets ($\mathcal{T}1$)–($\mathcal{T}4$) above is committed to ($\mathcal{E}$), at least with respect to $\mathcal{L}_{\mathcal{O}}$. Ontologists in possession of this language can pursue the Realist's project, by gathering up the true sentences expressible in $\mathcal{L}_{\mathcal{O}}$, and determining what objects are required by the truth conditions[8] of those sentences. Characterized in this way, the goal of science, for the Realist that accepts ($\mathcal{T}4$), is to *discover* the language of ontology—to journey through logical space, with naturalism as a guide, and arrive at the coordinates that house this special, ontological language.

Two points are worth highlighting at the outset. First, the question probed here is *not* whether humans are capable of constructing an algebra of sufficient complexity and precision to match the "fine structure" of reality assumed by the Realist. If we grant the Realist's implicit assumption that there is a language, an interpretation of the existential quantifier, that best mirrors the structure of reality, the worry probed here is *not* whether humans are (in)capable of penning such a language. The question is whether the process of scientific investigation, in conjunction with the limits of human cognition, will keep us from discovering the language of ontology.

The second point pertains to the role understanding plays in this investigation. Part of the worry is whether humans can *understand* the language of ontology. The scientific journey through logical space envisioned by the Realist would clearly involve the systematic replacement of old theories with new ones, and likewise, old languages will be replaced by new ones. But of course for this to happen, scientists must come to understand the theories they are accepting, rejecting, and revising. As such, if there are interesting

[7]The term 'externalism' has a variety of uses in philosophy of language, mind, epistemology, and metaphysics. Whether ($\mathcal{E}$) adequately corrals all of those various uses is not clear, and is not my task (see Gross, 2015, for discussion). Any view of linguistic meaning that is committed to ($\mathcal{E}$) is subject to the arguments below, and my use of 'externalism' and 'externalist' apply to such views even if they may not apply to other views that some might label 'externalist' (e.g., the use of 'externalism' attributed to Putnam (1975a), that meanings "ain't in the head").

[8]I use 'truth condition(s)' and 'satisfaction condition(s)' interchangeably throughout this paper. Doing so glosses over a technical point, namely that only sentences in a language have truth conditions, while sub-sentential expressions have satisfaction conditions. The difference being that sub-sentential expressions are not truth-evaluable, while (some) sentences are. I trust that adopting this convention here will not be too confusing, since each instance of either expression could be substituted with the much clumsier 'truth-or-satisfaction condition(s)' without a loss of cogency.

limits on the human capacity to understand languages, and the scientific theories expressed by them, there may well be obstacles inherent to the Realist's task. If it turns out that inelminable terms in a scientific language do not admit to externalist treatment or revision (i.e. they do not comport with ($\mathcal{E}$)), this speaks against the Realist's goal of arriving at the unique language of ontology $\mathcal{L}_{\mathcal{O}}$ by naturalist means, since no revision to the theory will eliminate the offending term.

I'll begin by outlining the case I've made elsewhere for thinking that expressions in a *natural* language are hostile to externalist treatment. If these arguments are successful, then the Realist that accepts ($\mathcal{T}4'$) will have to find some suitable $\mathcal{L}_{\mathcal{O}}$ for metaphysical inquiry. In §3 I'll suggest that ($\mathcal{T}4$) fairs no better than ($\mathcal{T}4'$) on this count, by highlighting the lexical flexibility of scientific terms.

2.2 Against Externalism for Natural Language

There are three classes of arguments against externalism in natural language (see Stainton, 2006; Vogel, 2016). The first focuses on the scientific task taken up by the linguist studying natural language—namely, that of proffering an explanation for the development of human children with regard to their (natural) linguistic capacity. The second set of arguments turns to ontological considerations, specifically whether particular views about the ontology of linguistic elements are commensurate with scientific methodology. The third series of arguments are data-driven—they highlight the discord between the meanings speakers assign to natural language expressions, and the would-be externalist treatment for many broad classes of linguistic expressions, demonstrating that natural languages are much more flexible than the apparatus of the externalist's semantic machinery permits. I take these arguments to constitute a formidable case for rejecting the externalist program, at least for natural language semantics.

The first set of arguments against an externalist semantics for natural language highlights the manner in which humans come to acquire natural languages. These arguments stress the importance of accounting for the problem of acquisition, namely the fact that nearly all children in a linguistic environment acquire natural language by the age of four. Addressing the problem of acquisition places limits on the kinds of theories that we can adopt in giving a descriptive account of human linguistic competence. Explaining those facts seems hopeless for construals of natural languages as mind-external objects, thereby forestalling various externalist responses to the problems posed for ($\mathcal{E}$) (Chomsky, 1965, Ch. 1).

But these criticisms of the externalist program rely on a certain pattern of acquisitional facts, partially characterized by the short time in which children (can) learn a natural language. However, learning the languages of science, and the content of the theories they describe are not beset with the same conditions. Becoming a scientist is hard, and is rarely accomplished in a short four years. Decades of schooling, rote memorization, an abundance of negative feedback, and a bounty of legal stimulants make up the typical course of scientific training—none of which holds for the young natural language learner. As such, the explanatory burden that constrains the space of theories for a natural language has much less bearing on a theoretical account for scientific languages. While such acquisition arguments suggest that natural languages do not have an externalist semantics, these arguments do not seem to apply to scientific languages.

A second set of arguments against an externalist semantics for natural language highlight ontological concerns with such theories of meaning. An externalist semantics holds that the meanings of terms in a natural language are cashed out in terms of relations between those terms and worldly objects. Thus the externalist owes us an account of the *relata* for that meaning bearing relation. If no convincing account can be given for the ontological *bona fides* of these *relata*, this gives us warrant to reject the externalist account. If meanings are word-to-world relations, there had better be an ontologically stable notion of a *word*, such that objects can be related to them (meaning-wise). The point emphasized in these arguments is that there is no clear way to proceed in addressing this ontological demand. The most plausible method would be to delineate words by the sounds humans produce in constructing expressions. But a point familiar to phonologists is that appeals to the characteristics of the sonic waves produced by speakers of a given language do not divide these noises into *sui generis* kinds (Bromberger & Halle, 1995). Put another way, any naturalistic inquiry into the nature of words shows that there is no mind-independent subject matter to be investigated, and no scientifically viable conception of *words*. Thus, if there is no natural kind 'cat'—i.e. no ontologically genuine category for the word 'cat'—then there can be no relation, meaning-bearing or otherwise, between 'cat' and (all of) the (possible) cats. This is troubling for an externalist semantics for natural languages, since the externalist requires that there be such meaning-bearing relations with the requisite *relata*.

There are a few reasons to think these arguments against externalism do not extend to scientific languages. But the most pressing is that such arguments rely on the very Realist methodology they purportedly undermine. The ontological criticism against an externalist semantics for natural language holds that there are no words. Rephrased, the thought expressed is that, because our best (language) sciences tell us there are no words, ontologically there can be no such things to stand in meaning bearing relations as the externalist requires. This criticism against the externalist program relies on the Realist's ontological methodology. Recall, the Realist insist that ontological investigation proceeds by identifying the true expression in a language, and determining from there what kind of objects must occupy the worldy domain that satisfies the truth-conditions of those expressions. When that language is the regimented language of our best (and complete) science, the ontological posits of that language are "what there is." But as indicated above, this methodology only succeeds if the semantics for the relevant language is externalist. So if the criticism leveled here is that our best scientific theories say that there is no ontologically *bona fide* category 'scientific term', and there are therefore no scientific terms to stand in meaning-bearing relations to the mind-external world, the argument is self-defeating. The justification for saying that there are no scientific terms relies on the assumption that the *scientific terms* in our best theories denote all and only the objects in our ontology, because the semantics for such a language is externalist. Thus the criticism that would yield the anti-externalist conclusion about the semantics for scientific languages assumes that such a semantics is indeed externalist. For this reason, ontological arguments against an externalist semantics do not seem to extend to scientific languages.

The final set of arguments against an externalist semantics, I'll argue, do extend to scientific languages. These are data-driven arguments, as they highlight the discord between the pattern of meaning assignments competent speakers of the language give to expressions, and the truth-conditional properties of those expressions. Such arguments are data-driven because they show that externalist models cannot explain the distribution of meaning assignments they are meant to explain. Specifically, natural languages exhibit a kind of *flexibility* that externalist models cannot accommodate.

The cases that speak most strongly against an externalist semantics for natural language are those that have ontological commitments which require a single object to satisfy ontologically distinct types of predicates. Externalist construals of the meanings for such expressions attribute ontologically incommensurate properties to a single object, and thereby have implausible entailments. Consider the following examples:

(2) This book, which John wrote, is five pounds.

(3) The Hirshhorn Museum is bankrupt and it is a cylinder.

For the externalist (and hence the Realist) expressions like these require metaphysically suspect objects to satisfy their purported truth conditional meanings. There is a perfectly sensible meaning ascribable to (2) which entails the existence of a metaphysically bizarre object.[9] On this reading the usage of 'book' would seemingly refer both to some abstract object that John wrote (possibly entirely in his head), and the physical, hefty text that weighs five pounds. But 'this book' cannot have both these referents, and must refer to a single object to satisfy the two predicates.[10] This single object cannot be the physical text, since we can imagine John has never interacted with, much less scribed, the physical object. Relatedly, the single referent cannot be the abstract object that John wrote (in his head, say). While abstract objects can have properties, mass is not one of them. On either of these accounts this sentence would always be false, which belies the manner in which speakers would treat this claim.

More importantly, an externalist semantics will fail to explain the inferences that natural language speakers make, as they pertain to lexically flexible expressions. Consider the following expressions:

(4) John wrote this book.

(5) This book weighs five pounds.

Given a situation where a natural language speaker would affirm (2), that same speaker would also assent to both (4) and (5). On an externalist semantics this inference is explained by modeling such inference as entailment—the

[9]There is another reading of (2) which does not require a metaphysically suspect object, but rather requires that John scribed a fairly long book (in the sense that might lead to a hand cramp). But any semantics must be able to account for all expression meanings, including the one outlined above.

[10]This single object could be some combination of these two objects, maybe their mereological sum. But if it were, the sentence would turn out to be false since such an object does not weigh five pounds, nor is it written by John, even if both of these claims would be true about some part of this combined object.

truth of (2) entails the truth of (4) and (5). On this explanation, (4) and (5) are entailed by (2) because the *same object* that satisfies the predicates in (2) also satisfies the predicates in (4) and (5) respectively. Of course, neither the concrete physical text, nor the abstract contents can satisfy both predicates. The externalist, in order to explain inference patterns of this sort, is committed to the existence of an object that is at once *both* abstract and concrete.

The externalist might reply, insisting the 'book' here is homophonous (i.e. ambiguous)—we have two words 'book', one that refers to abstract contents, and the other that refers to physical texts. Those two different words, the reply insists, appear in (4) and (5) respectively, and likewise satisfy the distinct predicates, without the need for a bizarre hybrid abstract-concrete object. But this move will not explain how the *single* use of 'book' in (2) licenses the inference to (4) and (5), at least not if that inference is modeled as entailment. Whichever word 'book' is used in (2), either referring to a content or to a physical text, the resulting referent will fail to satisfy one of (4) or (5). The externalist appeal to ambiguity will not do, insofar as modeling lexical flexibility as homophony will fail to capture the inference patterns of natural language speakers.[11]

Likewise, the referent of 'the Hirshhorn Museum' in (3), on an externalist semantics, must be of the sort that can be both bankrupt and cylindrical. Given that the anaphoric 'it' must derive its referent from this NP, these phrases must co-refer to some single object, on an externalist semantics. Though abstract institutions can have many properties, they do not seem to have a shape. And while physical buildings can have a shape, they cannot have financial troubles. Yet, for (3) to be true there must be an object of this odd sort.[12]

And even if there are metaphysically bizarre objects, of the sort required by an externalist semantics for these flexible expressions, as noted by Pietroski (2005) such a semantics fails to explain the oddity of

(6) # The Hirshhorn Museum is a bankrupt cylinder.

On a externalist semantics (3) and (6) are truth conditionally equivalent, and thereby have similar (if not identical) meanings according to such theories.

[11] The externalist's homophonous reply does not exhaust the possible responses to these phenomena. For a detailed discussion of available replies see Vogel (2016).

[12] For a more detailed description and exhaustive analysis of examples like these, see Chomsky (1977); Pietroski (2005).

Yet (6) is semantically odd in a way that (3) is not, a fact unexplained by theories that treat meanings as determining truth conditions.

There are numerous replies in the polysemy literature to the particular form of lexical flexibility rehearsed thus far. The so-called *problem of co-predication* has largely focused on the flexibility of nouns like 'book' and 'Hirshhorn Museum.' As such, it is tempting to think that the problem here has a fairly limited scope, suggesting particular kinds of solutions. One might think that examples like (2) or (3) can be resolved by teasing out the various denotations of the relevant noun phrases. The externalist might claim that sentences like (2) are some how deviant, insofar as their noun phrase constituents solicit multiple meanings in a single use, aberrantly demanding a kind of dual denotation quite contrary to the typical usage of such nouns. In effect, one strategy for the externalist is to offer a paraphrase for the deviant sentence—one that an externalist semantics can more readily handle.[13] Put a bit glibly, what is meant by the use of (2) is clarified in

(7) This copy of the book, which John wrote, is five pounds.

The sentence in (7) does not pose a problem for an externalist. Given modest proposals about the meaning of 'copy' as expressing a relation between an abstract content and a physical object, we now have two objects to which the differing predicates can apply. John wrote the content, which has no weight. And the physical object, that John has (let's suppose) never interacted with, weighs five pounds. If what speakers really mean, and what hearers really understand, in using/understanding sentences like (2) is a sentence like (7), this paraphrasing strategy resolves the problem posed by co-predication by offering externalist-friendly paraphrases to troubling sentences.

There are two related problems with this paraphrasing strategy. First, sentences like (2) and (3) are readily comprehended by natural language speakers, and in no need of repair as a result of their purported deviance. They are both grammatically and semantically well formed. Compare (2) to

(8) *The cat seems sleeping.

[13] See Recanati (2010) for an approach along similar lines. Recanati explains these cases by distinguishing between an intuitive meaning of expressions, of the sort that natural language speakers deploy in processing sentences, from the literal meaning of a sentence. But as Asher (2011) notes, a view that permits the modulation of truth-conditional meanings in this way fails to explain the negative data about how phrases can *not* be so modulated.

The sentence in (8) is ungrammatical, and in comprehending the expression a native English speaker is forced to repair the ill-formedness—in particular, by adding a copula:

(9) The cat seems to be sleeping.

Of course, (2) requires no such repair, since the sentence is grammatically well-formed. But for the paraphrase strategy to be plausible, speakers must be doing the semantic analog of repairing an ill-formed sentence in resolving the claimed semantic deviance of the relevant expression. However, sentences like (2) and (3) are not semantically deviant as such. The contrast here between (3) and (6) is instructive:

(3) The Hirshhorn Museum is bankrupt and it is a cylinder.

(6) # The Hirshhorn Museum is a bankrupt cylinder.

The sentence is (6) odd in a way that the sentence in (3) is not. If the paraphrase strategy is designed to explain the phenomenon of polysemy in cases of co-predication by indicating that speakers repair sentences like (3) to those more amenable to externalist treatment, then one wants to know why (6) is demonstrably worse.

The second problem with the paraphrase strategy is that it simply fails to explain the relevant data. Native English speakers *do* understand sentences like (2) and (3). So even if they get somehow repaired because they are somehow deviant (which they seem not to be) the externalist owes us an account of that pragmatic operation. But whatever that complex operation might be, one would expect that native speakers would process sentences with literal uses more readily than those that are purportedly deviant. After all, if hearers pragmatically rescue the meaning of (2), and derive (7) as a result, that should require more cognitive effort. Put another way, processing sentences with literal (i.e., non-deviant) uses of nouns should be easier. But they aren't (Frisson, 2015). So while it might be tempting for the externalist to resolve co-predication problems of lexically flexible expressions with a paraphrasing strategy, such a move poses further problems with little explanatory benefit.

Other cases of flexibility, while less decisive, serve as evidence against an externalist semantics for languages exhibiting such flexibility. These are cases in which a term expresses a constellation of concepts with distinct satisfaction conditions, without being of different ontological types. Color predicates exhibit this kind of flexibility. Consider the following:

(10) This house is blue.

(11) This marker is blue.

(12) This ink is blue.

(13) This iris is blue.

(14) The sky is blue.

The predicate ‘is blue’ attributes a similar property in all these instances, related to human phenomenological experiences of color. But the extension of this predicate will vary depending of contextual aspects of its use. For (10) to be true, the exterior of the house must be (mostly) blue, but the interior can be any color. Contrastingly, (11) is still true even if the marker’s exterior is completely white, so long as the contents of the marker are of the right sort. And while (13) can be true even if the majority of the plant is not blue, the ink in (12) can appear completely black, so long as it can be used to scribe blue orthographs. Further, (14) is true even if the chunks of the air that constitutes the sky are themselves completely void of color.

This seems to indicate that, while uses of the terms ‘blue’ are deeply related, since they all rely on human visual experiences of color, they express different concepts with conflicting satisfaction conditions. These differing conditions suggest that we should be pluralists about the concept BLUE, all expressible by the expression ‘is blue’. Of course, we can treat these uses as cases of homophony, or even a kind of indexicality (Rothschild & Segal, 2009). But because these uses of ‘blue’ are deeply related, treating these various uses as cases of homophony is disingenuous to the phenomena.[14] Importantly, accepting a kind of pluralism about the meaning of terms like ‘blue’ undermines the Realist’s methodology, which requires that meanings of expressions determine their extensions. After all, if we are forced to be pluralist

[14]The paradigm cases of homophony treat distinct, and typically unrelated, concepts as expressible with the same phonological form. In the following expressions

(15) The frogs are on the bank

(16) They robbed the bank

the term ‘bank’ expresses two distinct and unrelated concepts: BANK_r which relates to the earth around rivers, and BANK_f which relates to a financial institution. These two concepts are clearly unrelated, but the same simply cannot be said for uses of ‘blue’.

about the BLUE concept (which, lets suppose connects with the world in a referent-fixing way), the meaning of the *term* 'blue' will not determine the truth conditions for any expression in which it is used, absent the plethora of contextual information needed to determine which concept is meant by the usage.

The externalist might respond the lexical flexibility of expressions like 'book', 'Hirshhorn Museum', and 'blue' by indicating that we ought to accept the bizarre consequences of the arguments above. There really are such hybrid abstract concrete objects that expressions like 'book' denote. They are after all the consequence of accepting the fruitful externalist hypothesis ($\mathcal{E}$). That is, an appeal to parsimony suggests that the explanatory benefits of accepting ($\mathcal{E}$) outweigh the ontological costs posed by lexically flexible (polysemous) expressions. Rejecting ($\mathcal{E}$), so the reply goes, is worse than accepting abstract concrete objects.[15]

Adjudicating the aptness of this reply would require more space than I can devote here.[16] But three points are worth underscoring. First, this reply requires that the claimed successes of the externalist program are real successes. However, it is not clear that externalism has met with as much success as one might assume. Expressions with vague, context-dependent, and fictional elements all pose their unique problems for ($\mathcal{E}$) (Jackendoff, 2002). But even the most pedestrian sentences prove to be difficult for the externalist:

(14) The sky is blue.

Given that nearly every English speaker by the age of four can understand the meaning of (14), the externalist claim that the meaning the four-year-old understands determines the conditions under which the sentence is true does not sit well with the inability of the externalist to articulate what those truth conditions are. The appeal of semantic externalism in explaining acquisitional facts is supposed to fall out of the close relationship between meaning, reference, and truth. But then one wants to know how the four year-old can understand (14) without being able to discern which thing in the world has

[15]A reply of this sort is suggested in Stanley (2007). Williamson (1994) offers a similar argument for an equally bizarre consequence of accepting ($\mathcal{E}$) with respect to vague expressions.

[16]For some attempts with more devotion see Pietroski (2003, 2005, 2018).

the professed hue.[17] If even the most basic facts prove troubling for a theory of meaning, its not clear how an appeal to parsimony should be convincing.

Second, the costs here are not merely born out by accepting bizarre hybrid objects. The polysemy literature has focused on the co-predication problem as applied to nouns, because these cases put the Chomskyan point most starkly. But natural languages are replete with polysemy, and not limited to special cases of particular nouns, as indicated by the following:

(17) Sara codes[/writes/paints/cooks. . .] brilliantly, but sloppily.

(18) Indy found the artifact, his spouse, and his calling.

One might aptly summarize an episode in the life of an archaeologist with the sentence in (18), making flexible (polysemous) use of the verb 'find'. Despite this single use, there seems to be no *single* relation of *finding* that relates Indy to the relevant artifact, spouse, and career. In finding an artifact one seems to discover the location of an object whose location was previously unknown. But finding one's spouse is decidedly not a matter of discovering the space-time location of a misplaced individual that was antecedently one's romantic partner. And while finding one's calling is a matter of discovery, it never involves uncovering the location at which one's career resides. If the meaning of (18) determines a truth-condition, there seemingly must be some single relation expressed by the verb 'found' that relates Indy to these three things, despite the manifest diversity in the relevant relations. Thus in accounting for cases of lexical flexibility, the externalist that takes the bullet biting strategy while appealing to considerations of parsimony, will have to admit into their ontology not only abstract concretea, but some rather peculiar relations.[18]

Lastly, even if one accepts the required motley crew of hybrid entities and relations into their ontology, propounding the parsimony reply above, the final point to underscore is that this does not constitute an objection to the problems posed here. After all, the indicated externalist reply is that the

[17]See Pietroski (2015); Carston (2012) for discussion. See Wellwood (2019) for an analysis of 'more' and the limits of truth conditional semantics in explaining children's semantic competence. See Pietroski (2018) for an alternative to externalism.

[18] There are many responses to the co-predication problem and a complete survey of those responses is not possible here. See Falkum & Vicente (2015) for a review. But it is worth highlighting that even those proffering solutions to the co-predication problem with respect to nouns admit that the case for verbs proves more vexing (Vicente, 2019).

problems posed for ($\mathcal{E}$) are theoretical *costs*. The purpose of this paper is to show just how costly these problems are, insofar as the Realist has no easy way of avoiding them in rejecting ($\mathcal{T}4'$) in favor of ($\mathcal{T}4$). One might contend after the score is tallied that the costs are worth bearing, since alternatives to ($\mathcal{E}$) are inadequate. I'm skeptical of such a claim. But, even the externalist (and Realist) ought to be aware that the price they pay in accepting ($\mathcal{E}$) is higher than one might suspect, even if they think that higher price is still a bargain.[19]

The relevant inquiry for the purposes of this paper then is whether our scientific languages exhibit the kinds of flexibility (or polysemy) rehearsed in this section. If the languages used to express claims or generalizations in the sciences are flexible, then these languages are infelicitous candidates for ontological investigation. I argue that there is sufficient reason to hold that expressions used in the biological sciences exhibit this kind of flexibility. In particular, uses of terms like 'gene' and 'species', while they serve an invaluable explanatory purpose within the biological sciences, exhibit lexical flexibility. Insofar as biological explanations cannot be reduced to physio-chemical explanations, the Realist's methodology requires that the languages used in biological explanations have externalist meanings. Thus, if the terms of biological languages are indeed lexically flexible this speaks against the Realist's use of such languages for ontological investigation.

3 Lexical Flexibility of Scientific Terms

In this section I review two cases of terms in the biological sciences that exhibit lexical flexibility, and thereby cannot be assimilated into an externalist semantics for the scientific languages of biology, as the Realist requires.

3.1 Gene

Explanations in evolutionary biology makes use of the term 'gene' in ways that are indispensable. The study of biology has, from its conception, been concerned with reproduction, and specifically with the means by which

[19] Many balk at the plenitude of concrete possibilia proposed by Lewis (1986), whose central argument is the same appeal to parsimony. And while positing an infinitude of concrete possibilia seems implausibly fantastic, one can at least clearly imagine what such things are like. The same cannot be said of concrete abstracta (nor abstract concretea).

features of an organism are passed on to their progeny (Aristotle, *On the Generation of Animals*). Aristotle's concern was to explain how an organism's form could be transferred from one generation to the next. Since the work of Darwin, interest in heritability took on a particular importance, insofar as explanations of fitness require that individual organisms can pass on their features to their kin. If a particular trait was adaptationally advantageous, that characteristic must (typically) be inheritable by the organism's offspring.[20]

Early conceptions of 'gene' in post-Darwinian biology were meant to provide such an explanation. Mendel's cross-breeding experiments with peas led to an initial conception of the gene, characterized by Mendel's followers as the "unit-character" that is both responsible for the ratios seen in Mendel's Laws, and carried in the gametes of parenting organisms. Around the same time that Bateson coined the use of 'unit-character', Johannsen made explicit the use of the term 'gene' as a means of distinguishing between the characteristics an organism exhibits, or its *phenotypic* traits, and "whatever it is that determines an organism's properties and is passed down through the gametes" (Weber, 2005, p. 195). So construed the manifestation of a gene was left free of any particular physical commitments, but the thought was that an "organism's properties are determined by special, separable and therefore autonomous [units]" (Johannsen, 1909, pp. 143–144) [as translated in Weber, 2005]. Thus, there was thought to be a one-to-one mapping between a gene and a particular organism-trait.

Work on *Drosophilia* (a species of fruit fly) over the next decades, and later with *E. coli* (a rod-shaped bacteria) would prove that this relationship was actually quite complex, admitting to a many-to-many character. Not only are phenotypic traits the consequence of many genes interacting in intricate ways, but so too, a given gene can be involved in multiple phenotypic traits. Around the time of Watson and Crick's published double helix model, the identification of the gene with a sequence of DNA emerged. This conception identified genes as sequences of DNA that code for the production of

[20]This is not necessarily the case. For example, a random mutation for an individual organism can yield an advantage despite never being inherited from its parents. And given some unique circumstances, the mutation could lead to that organism surviving a plague that destroys the rest of the species. Assuming this trait is not passed on to this individual organism's progeny, the trait plays an essential role in explaining why the organism and its lineage survived while all others did not. And this is true, despite the fact that the trait was never inherited, nor heritable. But such a case is surely at the periphery.

proteins.

The result of this history is that contemporary biologists make use of the term 'gene' in two distinct ways.[21] The first pertains to the form of an organism, and the manner in which this form is passed on in reproduction. Such a notion is in this sense "preformationist", which we can call Gene-P:

> To speak of a gene for a phenotype is to speak as if, but only as if, it directly determines phenotype. It is a form of preformationism but one deployed for the sake of instrumental utility. I call this sense of the gene—Gene-P, with the P for preformationist. (**?**, p. 45)

The other use of the term 'gene' is related to the discovery of DNA, and the research that followed this discovery. On this use of the term, a 'gene' is a sequence of DNA that encodes for a protein product. Call this the Gene-D use of 'gene':

> *Gene-D is defined by its molecular sequence.* A Gene-D is a developmental resource (hence the D) which in itself is *indeterminate* with respect to phenotype. To be a Gene-D is to be a transcription unit on a chromosome within which are contained molecular template resources. These templates typically serve in the production of various gene products—directly to the synthesis of RNA and indirectly on the synthesis of a host of related polypeptides. (**?**, p. 46)

Given this distinction, Gene-P uses of the term 'gene' are meant to denote whatever is passed on generationally that manifests a particular phenotypic trait. Understood this way, Gene-Ps cannot be sequences of DNA, since most phenotypic traits are the result of the complex interaction between multiple sequences of DNA, development, and the organism's environment. The very same sequence of DNA in different environments will produce different polypeptide products (more on this below). So not only is the relationship from DNA sequences to products a one-to-many relationship, but so too is the relationship between DNA sequences and phenotypic traits. Were the relationship between DNA sequences and phenotypic traits one-to-one, then biologists could adopt a univocal conception of 'gene', which would be good

[21] **?** presents a historicity of the "gene concept" tracing the usage of the term 'gene' and its historical analogs, noting a shift is usage marked by the "phylogenetic turn" stemming from the work of many, including Darwin and Mendel. There's some evidence that contemporary working biologist conceptualize genes in the ways marked by this distinction (Stotz & Griffiths, 2004).

news for the Realist. That the form of this relationship is not one-to-one, coupled with the need for an explanation of how characteristics of form can be passed on from generation to generation, yields the distinction between Gene-P and Gene-D uses of 'gene'.

There are instances where the mapping of DNA sequences to phenotypic traits is well understood, even if not one-to-one. In these cases explanatory generalizations and claims in biology (seem to) exhibit a kind of flexibility. For instance, when scientists talk about 'the gene for breast cancer' such usage seems to be of the type related to Gene-P, insofar as the property of having-breast-cancer is a phenotypic trait (or perhaps more precisely, the property of having-high-susceptibility-to-breast-cancer is phenotypic). The use of the term 'gene' is useful in these contexts, insofar a breast cancer admits to a certain degree of heritability (Pharoah et al., 1997). Biologist and oncologist are interested in studying such an entity, at the level of phenotype since discoveries in this domain might contribute to understanding environmental risk-factors, patterns of infection, and thereby new treatments. But 'the gene for breast cancer' is not a particular DNA sequence responsible for some collection of protein products. Rather, such a 'gene' is characterized by the absence of a DNA sequence responsible for the production of a particular class of proteins in breast tissue and other tissues in the human body. More specifically:

> The normal resource at the breast cancer locus (BRCA1) is not a gene for healthy breasts, but a template for a large and complex protein which is present in many different cell types... capable of binding to DNA and influencing cell division in context specific ways. (**?**, p. 48)

So, in breast tissue cells that lack a particular DNA sequence at a particular location (or locus) on a particular chromosome, a human is more likely to acquire breast cancer. This is because the normal DNA sequence at that locus is absent, as are the multiple protein products it produces which promote cellular stability (and hence stave off cell mutation) during reproduction. Further, this absence can be filled by many divergent sequences of DNA, not some single aberrant sequence. Thus, when someone has 'the gene for breast cancer', what they posses is not a particular sequence of DNA that is responsible for a protein product—they do not have some particular Gene-D. The sequence of DNA they have, among the many aberrant sequences they could have, at the relevant locus fails to produce any product at all. To say someone has the gene for breast cancer is to say that they lack a particular gene. That is, the breast cancer Gene-P is the absence of a Gene-D.[22]

Two points are worth emphasizing here: first, the uses of 'the breast cancer gene' cannot be reduced to uses of 'gene' as a sequence of DNA; and second, uses of 'the breast cancer gene' only makes sense by making use of the conception of genes as DNA sequences. That is, one of these notions is not reducible to the other, yet their meanings are intimately related. Clearly, a Gene-P cannot be reduced to a Gene-D conception, precisely because in diseases like breast cancer there is no Gene-D responsible for breast cancer. None of the DNA sequences involved in explaining the manifestation of breast cancer are Gene-Ds, because they do not produce polypeptide products. Relatedly, any description of a Gene-P in cases like breast cancer requires an appeal to the "normal" Gene-D present at the appropriate place on the relevant chromosome. These uses of 'gene' then seemingly exhibit the feature that renders natural languages hostile to externalist treatment, namely *lexical flexibility*.

These uses of 'gene' have distinct, though (inextricably) related meanings, as exhibited by the following expressions in the language of biology:

(19) The breast cancer gene is multiply realizable, and it prevents the production of a class of proteins originating from the BRCA1 locus on chromosome 17 in breast tissue.

(20) Jill and Jan both inherited the breast cancer gene from their mothers, and it prevents them from producing a class of proteins originating at the BRCA1 locus on chromosome 17 in their breast tissue, making their breast tissue more susceptible to mutation.

These expressions seem to be claims biologists would accept as true (given the obvious assumptions about Jan and Jill). Further this seems to hold despite the fact that the use of 'gene' in both expressions cannot be univocal. If we take the expression 'the breast cancer gene' to refer to the absence of the Gene-D that "normally" appears at the BRCA1 locus, then clearly both notions of 'gene' are being expressed by the single use of 'gene' in these sentences. Such an absence cannot enter into a causal interaction with the

[22]Breast cancer is not unique here. Many other heritable conditions have a similar structure, where the Gene-P for those diseases is marked by the absence of a Gene-D: for example Huntington's disease, sickle-cell disease, and possibly Leprosy.

production of a protein product, even if an alternate DNA sequence can. Yet, on an externalist reading of (19), this is precisely how the world must be for it to be true. There is some object denoted by ‘the breast cancer gene’ that interrupts the production of a protein. But as we’ve seen, ‘the breast cancer gene’ refers to some abstract object, an absence of some particular DNA sequence that is normally found at a particular locus. Just like the case of the Hirshhorn Museum, an externalist treatment of (19) would require the existence of a metaphysically suspect object.

Not only does ‘gene’ exhibit the flexibility of words like ‘book’, it exhibits a kind of flexibility that suggests a pluralist treatment, much like we saw with color terms. Gene-D uses of ‘gene’ suggest that genes are molecules, and that the extension of such a conception is precise—a gene is a DNA sequences that codes for polypeptide products. In the paradigm case, a sequence of DNA encodes the production of an RNA sequence, which is then used by the cell to produce a protein product. But this simple paradigm admits to exceptions, in multiple complex ways. For one, in many viruses DNA plays no role in the production of polypeptides, as RNA plays this role directly, not requiring the presence of any DNA sequence in the virus. But presumably, all the reasons biologists have for holding that DNA sequences are genes due to their role in the production of polypeptides and their ability to be passed on in reproduction, also holds for RNA in the context of a virus cell. But if we identify RNA as viral genes, there must be some good reason not to consider sequences of RNA as genes in other environments. Shifting the extension of ‘gene’ to only include RNA sequences would have the consequence of denying that genes are heritable for many organisms, as in the paradigm case. In most organisms RNA is not inherited in reproduction, though DNA is.

In other cases, a single DNA sequence can encode for the production of an RNA sequence that, in some cells, is only *partly* used to produce particular polypeptides. Yet in other cells, different parts of the same RNA sequence are selected by different processes to produce other polypeptides. In these environments, the RNA sequence yielded by a sequence of DNA is separated by processes further downstream in the production of the resulting polypeptide. In some cellular environments one part of the RNA sequences is used to produce the resulting protein, while in other environments different (sometimes overlapping) parts of the RNA sequence are used to produce different proteins. Thus, the same sequence of DNA is responsible for different protein products, depending on the environment in which the DNA (and its RNA pair) is utilized. In such cases, only part of the DNA sequence that encodes for an RNA chain is responsible for the production of a protein, since the other parts of the RNA sequence the DNA chain begot are not utilized in the production of the protein product. This leads to indeterminate answers as to what (part of) the DNA sequence is in fact the gene (Wilson et al., 2007, p. 203).

Further, because the expression of a gene depends on the other *regulatory* DNA sequences in its environment, sequences that govern the order in which genes are expressed within a cellular environment, the very same sequence of DNA in one cell will produce a protein, yet fail to do so in a different type of cell in the same organism (because transcription factors like regulatory proteins or RNA sequences are present/absent). In terms of pinning down the extension of 'gene', even bracketing concerns about the difference between Gene-P and Gene-D uses of the term 'gene', whether a particular sequence of DNA is considered a gene or not will depend on the environment in which that DNA resides (see Griffiths & Stotz, 2013, Ch. 4 for discussion). In some contexts that DNA sequence produces a protein and counts as a gene, while in others that same sequence will fail to produce a protein (and concomitantly fail to count as a gene). Much like our color predicates in natural language, the predicate 'is a gene' in the language of molecular biology seems to be context dependent is ways that suggest a kind of pluralism about gene concepts.

The upshot is that 'gene' seems to exhibit both kinds of flexibility, even though the discussion thus far has ignored further complexities with the use of 'gene.' That is, the discussion of the term 'gene' so far has been fairly limited to the uses highlighted by Moss. But the use of 'gene' in biology has not been limited to these particular uses. Of note, there's been no mention of a more contemporary notion of genes as units that encode information (e.g. Williams, 1992; Rosenberg, 2006). These uses invite further complexities that spell trouble for the Realist (Griffiths & Stotz, 2013, Ch 4, §2; Ch. 6).[23]

[23] The information theoretic notion of 'gene' immediately raises polysemy concerns analogous to 'book', since much like books that can have the same abstract content conveyed by different physical media, the same information can be encoded in multiple ways. But further worries loom. Much information talk is metaphorical (Sarkar, 1996), raising questions about the inextricability of metaphor in naturalistic inquiry (Hesse, 1966). But even for those that take genes to literally be units of information, differences between a causal notion of information (i.e. Shannon information) and a semantic (i.e. intentional) notion

As we will see with 'species' below, these further competing conceptions get put to use for different purposes by working biologists. Much of what can be said in the following section regarding 'species' could be said of the 'gene' given these various uses.

But the narrow focus of this section to just Gene-P and Gene-D uses of 'gene' is intended to show that, as with natural language expressions like 'book', scientific terms can be used to generate co-predication problems pursuant the Realist's shift to ($\mathcal{T}4$). In natural language semantics, externalist take the co-predication problem seriously because those troubling cases of lexical flexibility have clear ontological commitments that many externalist are unwilling to accept. Concrete abstracta are unwelcome ontological posits. The narrow focus on Gene-P and Gene-D uses in this section is meant to show that the Realist who rejects ($\mathcal{T}4'$) in favor of ($\mathcal{T}4$) does not avoid the co-predication problem. Insofar as 'gene' is lexically flexible, not reducible, and importantly explanatory for the biological sciences, this suggests that a semantics for the scientific languages used to express biological claims is not externalist. This calls into question the Realist's insistence that the languages of our best science must have an externalist semantics to meet her methodological demands.

3.2 Species

The term 'species' plays a crucial role in biological explanation. As biologists investigate a particular organism, the usefulness of their findings only gains traction under the assumption that the individuals they study are in someway representative of a larger group. In this way species membership is informative. Knowing that an individual organism is a member of a species enables us to predict a host of other properties associated with that class (Dupré, 1999; Griffiths, 1999). For the Realist, then, the term 'species' must have an externalist meaning if the languages of our best biological sciences are to be apt for purposes of ontological investigation. Relatedly, if 'species' fails to admit to externalist treatment, this speaks against the Realist methodology that insists the languages produced by naturalist investigation are useful tools for settling ontological disputes. However, 'species' seems to be flexible, in the way natural language color predicates are, suggesting a pluralist treatment of the species concept.

of information pose unique problems.(Griffiths & Stotz, 2013)

One way for the term 'species' to be univocal is if individual species terms denoted natural kinds. If there are natural divisions in the world between species, and the terms we use mark those distinctions, the term 'species' can thereby have a univocal meaning as referring to such natural kinds. The work of Kripke (1980) and Putnam (1975b) invited a resurgence in essentialist thinking about species as natural kinds. The idea promoted by Kripke and Putnam is that uses of natural kind terms denote natural kinds, irrespective of speaker knowledge. Putnam in particular argues that there is an implicit convention to natural kind denoting terms that, while individual users of those terms might be ignorant or confused about their denotations, such terms track the essential properties constitutive of the denoted natural kind.

For example, all objects composed of gold share features in common, many used to identify which things are gold and which are not. Gold is shiny, yellow, and malleable. However, according to Putnam, the implicit convention pervading uses of 'gold' dictates that their uses do not merely denote objects that are shiny, yellow, and malleable. Rather, such uses track the microstructural properties that underwrite these superficial properties, and this is true even if users are ignorant of what those microstructural properties are. Uses of 'gold' denote in accordance with the underlying (possibly unknown) *essence* of the natural kind. In the case of gold, of course, we are no longer ignorant of this essence. The atomic number of gold atoms both unifies the natural kind and determines the superficial properties often used to identify bits of gold. That is, what makes an engagement ring and The Hand of Faith both essentially gold is that both objects have certain microstructural properties, abstractly characterized by their atomic number. And uses of 'gold' have, according to Putnam, *always* denoted those properties.

Such an essentialist conception of natural kinds is conceivably at the heart of analytic metaphysics, at least for the Realist. If natural kind terms intrinsically refer to essential natural divisions, the meaning of such terms can be fruitful for ontological investigation. That the world has a certain "fine structure" and can be "carved at its joints" implies that there are natural divisions between objects, divisions that are intrinsic to those objects. The work of Kripke and Putnam invites conceiving of metaphysical inquiry generally along Realist lines. The examples offered by both Kripke and Putnam extend beyond physics and chemistry, to purported biological natural kinds. For Kripke, 'tiger' denotes a natural kind, ostensibly some animal species, members of which share some common essential properties that mark them as members of that species kind (Kripke, 1980, p. 121). In much the way some particular engagement ring and The Hand of Faith are members of the same natural kind in virtue of their essential atomic number properties, Sita[24] and Champawat[25] are members of the same species in virtue of their (yet unknown) essential properties. Putnam makes similar remarks regarding uses of 'lemon', holding that the underlying essential properties are genetic (Putnam, 1975b, p. 147).

As I've suggested, if the Putnam-Kripke line of thought is correct, this is good news for the Realist. If species terms like 'tiger' and 'lemon' track essential properties, those that make an object a lemon or a tiger, then the meanings of such terms are fruitful for ontological investigation. Likewise, the term 'species' has a univocal meaning, namely the numerous classes picked out by those biological kind terms that track the relevant essential properties. However, this essentialist line of thought is troubled.

On essentialist construals, membership in a natural kind is determined at the level of individuals. That an object is a piece of gold, and not a piece of silver is explained by the essential atomic number properties of the relevant bits of matter. A difference in two individuals' atomic number yields a difference in natural kind. But, as Sober (1980) argues, appeal to individual differences in codifying species kinds is incompatible with evolutionary theory. Rather, in post-Darwinian explanations in evolutionary biology, individual differences in organisms within the same species are required to make sense of selection. Intrinsic differences in the reproductive workload between genders in mammalian species are required to explain how traits of parents are passed on to kin, and underwrite explanations of fitness. More starkly, in many insect species, the differing roles of intrinsically different organisms explain why a *population* of insects is more adaptive than others. The role of asexual worker bees is essential for the reproductive success of the hive *via* those members of the population that do produce offspring. For this reason, species *populations* and not individuals are often the units of selection in evolutionary biology. Thus, to explain why a particular species has evolved, the term 'species' must identify a population of organisms, not some set of essential properties some organisms share. The essential differences be-

[24]The mother of most of the tigers that currently populate Bandhavgarh National Park in India, and is thought to be the most photographed tiger in history.

[25]A tiger thought to be the most dangerous to humans, estimated to have hunted 437 humans in the plains of Northern India before she was trapped and killed in 1907.

tween members of the same species population explain why that species has survived, where others have not.

For the essentialist, empirical difference between organisms are facts that need to be explained away. On an essentialist understanding, differences between individual organisms are, in a relevant sense, superficial to their membership in a given species. Sober's point is that these differences are not immaterial, and indeed important to explanations of natural selection. That worker bees differ from their reproductive conspecifics is not a trivial difference for the species. This difference is *required* for the explanations of species fitness, not a feature about the individuals that, from the perspective of species membership, can (or should) be explained away. As such, the existence and membership of a species is understood *because* of the intrinsic differences in its membership, not delineated by appeal to these differences as the essentialist would conclude.[26]

While the failure of essentialism regarding biological natural kinds is not good news for the Realist, such failure does not entail that the term 'species' in necessarily pluralist (and thereby lexically flexible). For all that's been shown there might be a univocal externalist meaning to the term 'species', albeit not one that appeals to intrinsic properties of individuals. Members of a population might well all share certain *relational* properties that ground their natural kind membership. Consider the astronomical term 'moon'. Many diverse celestial objects are moons, composed varyingly of ices, gases, and metals. Even though these objects do not share any particular intrinsic properties unique to moons[27] they are all members of the same kind. Being a moon is a relational property, pertaining to the movement of a celestial body in relation a *planet*, as opposed to (say) a star. Such a property might well ground the kind 'moon' despite its relational character. For all that's been said, 'species' membership might admit to this kind of treatment.

[26] To maintain the essentialist line, one would have to argue that, despite the focus on populations and the differences in members of a population that there is still some intrinsic property these individuals all share. As Ereshefsky (1992) argues, such prospects look grim. Putnam's appeal to some underlying essential genetic properties will not do, for reasons related to the flexibility of the term 'gene'. For more on why intrinsic essentialism fails for biological kinds see Okasha (2002).

[27] They share some intrinsic properties of course: they have spherical shapes, are composed of matter, they move in geometrically predictable orbits, etc. However, none of these properties are unique to moons.

Unsurprisingly, working evolutionary biologists have focused on different aspects of populations in understanding and explaining their evolutionary success, yielding a plurality of relational species concepts (Ereshefsky, 1992). This suggests that 'species' is lexically flexible. These various uses of the term 'species' serve various purposes in explanation, but none of them seem up to the task of serving as the univocal meaning of the term 'species'.

One view, the so-called *Biological Species Concept* (BSC), holds that species boundaries are marked by facts about interbreeding. Most famously Mayr (1970) argues that a species is an interbreeding population that is isolated from other populations according to various mechanisms. This view maintains that an individual is a member of a species based on the individuals it (can possibly) mate with. The thought behind this view is that evolutionary biology is primarily concerned with the transmission and exchange of genes throughout the living world. In a world in which there were no restrictions on gene transmission, species boundaries would be too continuous to be useful. Thankfully for biologists, there are significant barriers to gene transmission, marking discrete boundaries between species. Grouping species based on their (potential) mating partnerships maintains the spirit of this idea.

However, even ignoring worries with counterfactual[28] claims about breeding partners, such views fail to classify organisms that do not reproduce sexually. And given that the history of organisms on the planet is radically skewed toward asexual reproduction, such a view will (counterintuitively) classify most organisms as not members of a species.

Dupré (1999) argues that such a species concept is committed to other empirically unattractive conclusions about species taxa as well. For one, some seemingly distinct species groups exchange genetic information. In particular "different species of oaks have remained coherent and distinct vehicles of evolutionary change and continuity for long periods of time" despite the fact that these "various species of oak appear to have coexisted . . . exchanging significant amounts of genetic material through hybridization" (Dupré, 1999, pp. 7–8). Thus, not only does the BSC fail to delineate asexual species, but the view even misses the mark regarding sexually reproducing species.

[28]Other views of this kind focus on mate selection mechanisms to determine species boundaries. That is, on this view an individual falls into a specific species category based on the mechanism it deploys in finding a mate (Paterson, 1985). Such views however will likewise fail to classify asexual reproducers as members of a species, since they lack such mechanisms.

As such, the essentialist relation proposed by the BSC cannot serve as the univocal meaning of 'species' that the Realist requires.

Other views regarding the meaning of 'species' focus on environmental factors to determine species boundaries. On these *ecological* views, the selection pressures that an environment places on the development of an organism determine which species it belongs to—see for example Andersson (1990). One primary difficulty with such views is that two distinct populations, with distinct selective pressures could evolve to be identical regarding all their intrinsic properties, yet fail to be the same species simply because the set of forces guiding their selection are distinct. At the limit, two organisms could be genetically identical, yet (on this view) fail to be members of the same species.

A third group of theories determine species boundaries based on ancestral relations. Put crudely, on this view an organism's species membership is determined by the species membership of its parents, or in the case of asexual reproduction, its parent organism. The primary concern with this view is, on a Darwinian conception of the evolution of life, all organisms have a common ancestor traced back to the origins of life. And while not all organism are members of the same species, such a hereditary view must posit some criteria for determining taxa changes. Put metaphorically, if the tree of life branches, there must be some criteria by which we can determine where it branches. If an organism's species membership is determined by what branch it occupies on the tree, more needs to be said as to why the tree branches as it does, and these further "diagnosable" details seem to be what a conception of 'species' requires. And often identifying so-called 'speciation events' on the phylogenetic tree appeal to morphological, ecological, and reproductive facts, which represent the same features that competing views hold are the appropriate conditions for species membership.

As Ereshefsky (1992) observes, these three views are mutually incompatible.[29] Each view divides up the space of organisms into different species taxa, yielding conflicting pictures about species membership. And while none of these views seems to draw the boundaries in ways that are commensurate

[29]This review of the various uses of 'species' is not meant to be exhaustive. For a more complete list of such views, numbering in the dozens, see De Queiroz (2007). De Queiroz uses this diversity to argue for a more general species concept that species are "separately evolving metapopulation lineages, or more specifically, . . . segments of such lineages" (*ibid*, p. 880). However, the problem with this view is that it fails to distinguish species from other kinds of lineages.

with how all working biologists make sense of species, they are all informative:

> A taxonomy of monophyletic taxa provides a framework for examining genealogy. A taxonomy of interbreeding units offers a framework for examining the effect of sex on evolution. A taxonomy of ecological units provides a structure for observing the effect[s] of environmental selection forces. (Ereshefsky, 1992, p. 678)

The point is that various biologists use 'species' as a means of identifying different aspects of organisms that guide selection, each with a different explanatory purposes in mind. While my treatment here is certainly not exhaustive, what is clear is that the Realist is committed to holding that one of these (or some other) conceptions is somehow better than the others, and that some single species concept is the most natural. But even our brief discussion here indicates that this commitment is troubled, since "for biologist who adopt a multidisciplinary approach, or those who can step back from their own personal investments and research interests, all of the concepts seem to have some merits" (De Queiroz, 2007, p. 880). The various lineages identified by these distinct usages of 'species' provide useful insights into the ways in which life has emerged with such manifest diversity, with no conception receiving any obvious priority.[30]

The worry then is not merely epistemological for the Realist. Given evolutionary theory, the forces at work in driving the creation of different lineages are distinct forces. The morphological properties of organisms impact the creation of such lineages, as do the properties related to interbreeding and environmental adaption (among the many others). As Ereshefsky (1992) makes clear:

[30] Notice that the problem here is not simply that epistemic values creep into decisions about matters of fact, blurring a distinction that some staunch realists might find objectionable. The Realist targeted here has no problem with epistemic values yielding verdicts about ontology, given that the move to ($\mathcal{T}4$) is motivated by the epistemic credentials of naturalistic inquiry. The point here is that there seems to be no obvious victor on epistemic grounds. Ludwig (2016) highlights a similar point, before pressing a further concern that the appeal to non-epistemic values in concept selection is unavoidable. Conservation biology serves as a stark example, since preservationist/conservationist values inform where the boundaries of the relevant biological entity to be preserved/conserved are to be drawn. Whatever one might make of such arguments, the arguments from the polysemy of 'gene' and 'species' grant the kind of fact-value distinction a Realist requires, in manner Ludwig would deny. My thanks to an anonymous reviewer for bringing this point to my attention.

> The forces of evolution segment the tree of life into a number of different type of lineages, often causing the same organisms to belong to more than one type of lineage [...] So the forces of evolution segment the tree of life into a plurality of incompatible taxonomies [...] Of course this picture of evolution could be wrong; perhaps some of the above-mentioned forces do not exist, of those forces lack the ability to produce stable taxonomic entities. These are, after all, empirical matters. But given what current evolutionary biology tells us, the forces of evolution segment the tree of life into different and incompatible taxonomies. (Ereshefsky, 1992, p. 676–7)

Unless one of these kinds of taxa can be granted priority over the other, none can serve as the denotation of the term 'species'. Ereshefsky was writing nearly thirty years ago. Matters look all the more bleak for the Realist if we were to consider the entire contemporary array of species concepts identified in the biology literature.[31]

At this juncture, the Realist might insist that our discussion merely indicates that there are a multiplicity of species terms, each denoting a unique extension. Much like the English word 'bank', the biological term 'species' is simply homophonous, used to express a variety of distinct species "concepts." This treatment, she might claim, is consistent with an externalist semantic analysis for the various homophonous terms picked out by 'species'.

But, treating 'species' as a homophonous expression implies that the various 'species' terms indicated by the orthograph bear no semantic relationship to one another. After all, 'bank_f' and 'bank_r' (on the analogous natural language proposal) bear no interesting conceptual relations. They are distinct words, denoting distinct extensions. The Realist's response to the apparent plurality of species concepts would treat the term 'species' as expressing a constellation of distinct concepts, bearing no more interesting a relationship than that posed by the two words 'bank'. For the Realist, this is somewhat odd, given that the differences between these various "concepts" can be attributed to the various explanatory aims of different biologists (Ereshefsky, 1992, p. 678). As we've seen, biologists that study sexual reproduction analyze species in terms of mating, while those interested in the environmental influences on natural selection will understand species as ecologically

[31] Wilkins (2017) identifies 22–28 "conceptions" of "the species concept" including six "basic" conceptions, some of which are covered in my discussion above (Wilkins, 2017, pp. 305–8, 371–6). Whatever the definitive number of indispensable concepts/conceptions is, as that number of conceptions grows, the matter seems increasingly troubling for the Realist.

determined. Since the Realist's methodology requires that these different (pragmatically chosen) terms track the structure of reality, the ontic divisions in the world (according to this Realist move) are dependent on human interests. This belies the Realist's insistence that the nature of reality is not dependent on the way humans conceptualize the world.

For the Realist, "it is really, really hard to believe that [facts are]. . . merely a reflection of something about us" like the context sensitivity of the terms we use (Sider, 2011, p. 18). One of the aims of evolutionary biology is to offer explanations for the distributions of traits manifest in organisms, and the term 'species' occupies an integral role in accomplishing that aim. In particular, evolutionary biologists want to explain whether and how a particular property of an organism played a role in the organism's survival. The explanatory role of the term 'species' is to unify findings about the selection of traits across individual organisms. If there are multiple, distinct species concepts, there are multiple ways of grouping individual organisms. Likewise, there are multiple ways to generalize findings about individuals across groups of them. Because of this, these different ways of grouping individual organisms offer different explanations for the way a particular organism has the traits it does. So on this proposal the phenomena to be explained will admit to multiple, equally adequate explanations. Insofar as these explanations quantify over the exact same set of things, they yield *prima facie* competing natural divisions. They attempt to carve up the world in different ways.

Put in different terms, these various conceptions of 'species' model the world differently. But what the Realist wants is a *single* model, that cleaves to the structure of the world. To quote (Sider, 2011):

> According to the [Realist] picture, the point of human inquiry . . . is to *conform* itself to the world, rather than to *make* the world. The world is "out there", and our job is to wrap out minds around it. (p. 18)

Competing models in evolutionary biology, with different characterizations of species, offer distinct indications about what is "out there." Assuming these different models offer successful explanations, as our review here has indicated, they are all equally viable candidates for the unique description of the world. But, to accept that they are all equally valid is to accept that they are all *true*. If that's correct (as the Realist's homophonous suggestion indicates), there is no "comprehensive theory that provides the *single*, correct way to represent the causal structure of the world" contrary to the Realist sentiment echoed in Sider[32] above (Waters, 2005, p. 312). To deny this is just to accept a kind of pluralism about the term 'species', and while many philosophers accept this position (cf. Kitcher, 1984; Stanford, 1995; Dupré, 1999; Ereshefsky, 1998), this does not seem welcome to the Realist.

Further, the Realist that insists on pushing the homophonous reply, in the hopes of these diverse species concepts might converge in the future into a univocal term 'species', will have trouble explaining aspects of scientific practice. As Bzovy (2016)[33] notes, in the applied work of differentiating lineages into species, practicing biologists not only make use of different species concepts, but apply those notions in unison. That is, within a single research program multiple species concepts get used to arrive a species classifications that no *single* concept would have successfully uncovered. As we saw, Ereshefsky (1992) underscores the incompatibility of the variety of species concepts, which posit ontic divisions that are in tension. The cases Bzovy (2016) highlights are those wherein various concepts are applied together to reach conclusions about species membership that no single conception would have uncovered. Generalizing over the details, the case studies he details concern a species of large-spored yeast (of the *Metschnikowia* clade), wherein different species concepts were applied. In some instance, different concepts were used as confirming the results of a previous conceptual deployment; e.g., where a genetic/molecular species concept (which identifies species based on degrees of overlap in molecular genetic material) saw species divisions within the clade, and those results were confirmed by testing whether yeast in the proposed grouping would interbreed—an application of the Biological Species Concept (Lachance et al., 2005). In other cases, divergent results in applying a genetic/molecular species concept and a phylogenetic species concept (that looks to clade structure and history in determining monophyletic groupings) yielded species determinations that neither concept alone would have justified (de Oliveira Santos et al., 2015; Lachance & Fedor, 2014). The importance of these cases for our purpose here is that these biologists studying yeast seem to be uncovering genuine ontic divisions, "carving nature at its joints."

[32]Sider himself might balk at the idea that the world has a single *causal* structure (cf. Sider, 2011, p. 16), but insofar as the sciences describe the structure of reality, this requires a denial of the kind of pragmatism that typifies pluralist approaches in the philosophy of science (cf. Kellert et al., 2006).

[33]I am indebted to an anonymous reviewer for highlighting this work, of which I was not previously aware.

But that adept butchering was not the result of using a *single* privileged conception of 'species' that tracks the fine structure of reality more closely than any other. Uncovering those natural divisions *required* multiple *related* conceptions of the term 'species', not mere homophones that share a lexical entry by historical accident.

All this suggests that the term 'species' is flexible, in much the way 'book' is. Just as we might use 'book' to highlight different properties of an object based on our interests—as when writing a literary essay, instead of packing a moving box—biologists use 'species' to explain different aspects of an organism's evolution, depending on whether they are focused (say) on the impact of sexual reproduction on selection as opposed to the availability of resources. If 'species' is indeed pluralistic, then the Realist's methodology will fail to yield conclusive results about the ontology of biological kinds. And barring the possibility of reducing biological kinds to chemical or physical kinds, which is problematic for familiar reasons (cf. Griffiths, 1999; Fodor, 1975; Lycan, 1990), the Realist would have to deny that the findings of evolutionary biologists are scientific. The Realist would seemingly be forced to conclude that biologists do not speak a scientific language; i.e. that biology is not a science. I take this to be a reason to reject the Realist's methodology, not the scientific *bona fides* of evolutionary biology.

3.3 Realist Replies

The Realist might respond to the worries discussed here by indicating that lexically flexible terms are somehow deficient. She might claim (for example) that Gene-P uses of 'gene' will be jettisoned as biology progresses. Indeed, she might insist, there is some sense in which biology is incomplete—*a fortiori*, the flexibility of terms like 'gene' is indicative of this incompleteness. Once scientific investigation in the domain of biology is complete, the language used by biologists will be free of the flexibility seen with 'gene'. Highlighting the "loose" nature of Gene-P uses of 'gene', the Realist could claim that the *benchmark* of a completed science is the elimination of ambiguity and lexical flexibility. Sider (2011) makes a claim along these lines:

> I hold that the fundamental is determinate... First, no special-purpose vocabulary that is distinctive of indeterminacy ... carves [nature] at the joints. Second, fundamental languages obey classical logic. (p. 137)

Classical languages, with a Tarskian logical structure, do not exhibit properties like lexical flexibility. Since, for the Realist, the structure of the world admits to a classical Tarskian structure, a science whose theory is written in a language not amenable to classical treatment must be an incomplete science—as it does not carve nature at the joints. As such, the theories of such a science cannot be written in the privileged language of ontology ($\mathcal{L}_{\mathcal{O}}$) since "only the propositions in [$\mathcal{L}_{\mathcal{O}}$] are cast in joint carving terms" (Sider, 2011, p. 19). Put another way, if the flexibility of terms in biology cannot be purged, this alone is reason to reject such languages as describing reality's structure. Indeed, Sider insists that "concepts of physics, logic, and mathematics" are the "fundamental ones" made use of in the language of ontology (Sider, 2011, p. 7). The non-classical character of biology's flexible terms is, the argument claims, evidence of the non-fundamentality of such a language.

But this rejoinder to the proposed complications for the Realist's methodology, namely the complications ensuing from the flexibility of terms like 'gene', seems self-defeating. Recall, the reason the Realist embraces scientific languages for use in her ontological investigations pertains to the epistemic credentials of naturalistic inquiry. As Sider indicates

> We should believe generally what good theories say; so if a good theory makes an ontological claim, we should believe it. The ontological claim took part in a theoretical success, and therefore inherits a borrowed luster... [But] the conceptual decisions ... also took part in a theoretical success, and also inherit a borrowed luster. (Sider, 2011, p. 12)

For the Realist, the terminological (or "conceptual") decisions made by a research program, and the language that is the product of those decisions, are given a privileged joint-carving status in accordance with the success of the theory that makes use of them. But in the case of biology, these choices of terminology have lead to the lexically flexible term 'gene' described above. Insofar as the progression of biology has been successful, this success has largely been ascribable to matters related to genetics. So if the epistemic credentials of naturalistic investigation imbue languages with ontological luster, the language used by evolutionary biology should shine quite brightly. Thus to claim that, despite biology's explanatory success, the language of biology is deficient simply because it does not have a classical structure, undermines the motivation for using scientific languages *simpliciter*. Concisely, if the languages of biology lack the luster needed for ontological investigation, despite being developed using (successful!) naturalistic methods, then there is no reason for using *any* scientific language as an ontic tool. Denying the usefulness of the language of biology in ontological investigation because of a pre-theoretical commitment to certain metaphysical doctrines undermines the Realist's appeal to scientific languages in the first place. The Realist owes us a reason for thinking that the biological sciences are somehow epistemologically suspect, despite what seems to be a history of explanatory successes. In the absence of such a reason, the 'book'-like flexibility of 'gene' suggests the Realist's methodology is troubled.

In light of these considerations the Realist might yield some ground. She could admit that terms like 'gene' and 'species' will not be jettisoned from the scientific languages of biology, yet still hold that flexible terms are still aberrant. She might protest that we are conflating distinct notions of scientific languages. Uses of 'gene' and 'species' as described here might be essential for the practices of biologists, but their centrality is of a purely instrumental kind, for use by scientists in the practice of their craft to aid in communication. But these terms are not canonical in the sense that they will find a place in specifications of the generalizations that constitute the theories of (say) molecular biology. That is, we have good reason to mark a distinction between *pragmatic* terms, and *theoretical* terms. Only the latter, the Realist might insist, are genuine constituents of the language of biology—likewise, only the latter find their way into $\mathcal{L}_{\mathcal{O}}$.[34]

The first point to note about this protest is that it commits the Realist to particular (though possibly plausible) views about scientific languages, and how they might be distinguished from natural languages. More importantly though, this requires that the Realist offer a means for making the distinction between pragmatic and theoretical terms. Let's suppose *arguendo* that there is a distinction between pragmatic and theoretical terms in scientific languages. What reason do we have for thinking that 'gene' (at least in the uses outlined above) falls into the former category and not the latter? What are the criteria for determining whether a term used by scientists is theoretical (and thus ontologically trenchant) or not?

The answer most readily on offer from the Realist, that theoretical terms are those terms that can be given externalist meanings, will not do. Such a response begs the question at issue. Recall that the Realist's retreat to scientific languages is motivated by concerns about the semantics for natural languages. In response to the problems posed by the lexical flexibility of

[34] A representative view along these lines can be found in van Inwagen (1998).

natural language expressions, the Realist (rightfully) abandoned those languages as ontological guides. She turns to scientific languages in the hope that they will be better behaved, given that such languages are invented for the purpose of perspicuously describing the world. Essential to this move is the epistemic appeal of naturalist inquiry. To quote Sider again:

> But in trying to decide how much structure there is in the world, I can think of no better strategy than this extension of Quine's criterion: believe in as much structure as your best theory of the world posits. (Sider, 2009, p. 417)

The assumption that the world has a structure of a Tarksian logic garners support when the terms adopted by the sciences, as the result of naturalistic inquiry, can be given a semantics to cohere with an object-based structure. Thus, the reason we should think that the world has an object-based structure is that scientific methods, with their privileged epistemic credentials, produce languages whose semantics reflect that structure.

The justification for the Realist's retreat to scientific languages renders the appeal to the structure of reality as a means of marking the pragmatic/theoretical distinction circular. Naturalistic inquiry has led to the invention of the term 'gene', which given the lexically flexibility it exhibits, does not easily admit to externalist treatment. If the Realist's response to dealing with this counterexample to her general claim—namely, that we should think the world has an object-based structure because scientific languages have a semantics that reflects this structure—is to mark a distinction between pragmatic and theoretical terms, she cannot appeal to the object-based structure of reality to draw the distinction. To indicate that theoretical terms are those terms that result from naturalistic inquiry *and* have an object-based semantics undermines the epistemic credentials of naturalistic inquiry in identifying reality's structure. If the thought is that such inquiry results in terms that cleave to the structure of the world, the case of 'gene' indicates that this structure in not (obviously) object-based. If the Realist insists that 'gene' is no good for ontological investigation, despite the fact that the term is the result of successful naturalistic inquiry and central to producing explanatory generalizations in biology, there must be some property, beyond its resistance of externalist treatment, that makes 'gene' ill-suited for ontological investigation.

The Realist would be able to leverage the object-based structure of reality in marking the pragmatic/theoretical distinction if there was some independent reason to think the world has such a structure. Sider offers such a reason for assuming there is an object-based structure of the world, which amounts to a denial of the closest alternative:

> Suppose... that a suitable stuff-ontology could be constructed. Why should we accept it? One reason for moving to a stuff-ontology is inherently unstable. This reason begins with the epistemological skepticism about metaphysics considered earlier—questions about composition are unanswerable. It then adds some sort of prohibition against questions that are, in principle, unanswerable.
> ... But upholding stuff-ontology just substitutes one unanswerable question for another: is a stuff- or thing-ontology correct? This question seems no more answerable than questions that face the thing-theorist. (Sider, 2002, p. xviii)

The criticism offered against "epistemological skepticism" referenced in this passage is the following, which I offer in its entirety:

> Skeptics often ask too much of metaphysical arguments. A priori metaphysical arguments should not be faulted for not being decisive. For suppose the evidential support conferred by such arguments is fairly weak, though non-zero. Then the support for a typical metaphysical theory, T, will be weak. But the only support for T's rivals will also be from a priori metaphysical arguments. Thus T may well be better supported—albeit weakly—than its rivals. One would then be reasonable in giving more credence to T than to its rivals. Metaphysical inquiry can survive if we are willing to live with highly tentative conclusions. Let's not kid ourselves: metaphysics is highly speculative! It does not follow that it is entirely without rational grounds.
> I will proceed assuming that reasonable belief in metaphysics is indeed possible, and that something like the [Realist] methodology is legitimate. (Sider, 2002, p. xv)

These remarks suggest an argument pertaining to the question of reality's structure, and might legitimize the pragmatic/theoretical distinction deployed to deal with lexically flexible scientific terms:

1. Reality has a structure.
2. Questions about the nature of this structure (e.g. that it is thing-based) have determinate answers.
3. Evidence for various theories that answer these questions, even if minimal, make these debates substantive.

∴ Thus a wholesale skepticism about the substance of such debates is unwarranted.

An anti-Realist might take issue with any of these premises. But even if one accepts each of these premises, these considerations do not amount to a distinct argument for the thing-based structure the Realist assumes—or if they do, the evidence for the position bears the burden of scrutiny. The force of highlighting examples like ‘gene’ and ‘species’ is that they offer evidence against the proposal that reality’s structure is of the classical sort that the Realist proposes. Such counterexamples hold sway even if one assumes that reality has a structure and that proposals about that structure constitute a substantive area of theorization. If the Realist hopes to rebuff the force of these counterexamples by insisting there is a marked distinction between pragmatic and theoretical terms, the criteria for marking that distinction cannot merely rest on the very supposition the counterexamples undermine. The Realist cannot merely insist that the flexibility of terms like ‘gene’ and ‘species’ mark them as merely pragmatic, bereft of the metaphysical merits of theoretical terms.

If the Realist’s proposal puts constraints on the kinds of terms that count as genuine theoretical terms of the sciences, there must be quite powerful reasons to place such constraints on the practices of scientists prior to investigation. Reality’s structure may very well be thing-based. But by insisting that any scientific language that fails to reflect this structure is somehow metaphysically deficient, the Realist is burdened with providing quite powerful reasons for injuncting the offending terms. But there seems to be little to say in defense of this position. Arguments for this view simply propound metaphysical intuitions, or recapitulate the “best theory” argument above. Apart from the (possibly pronounced) intuition that the world has a thing-based structure, or Sider’s professed inability to imagine a better option, there seems little support for the injunction in the case of ‘gene’ and ‘species’. Neither of these appeals should be particularly convincing, given that these terms play a quite central role in the success of biology as a scientific discipline. In the absence of a convincing account of the distinction between pragmatic and theoretical terms, and a means for identifying which terms fall in which camp, the trouble posed by terms like ‘gene’ and ‘species’ remains.

4 Implications

Our cursory examination of these terms in the scientific languages used in biology is not decisive, nor can we conclude that the terms ‘gene’ and ‘species’ will *never* have univocal meanings that determine a single precise extension. Further a reduction of biological notions like ‘gene’ might be possible (cf. Waters, 2000). However, both the terms ‘gene’ and ‘species’ play a valuable explanatory role in biology, and the evidence considered here suggests that the meanings of these terms exhibit a kind of lexical flexibility. Insofar as there are multiple distinct, but related uses of the term ‘gene’ (cf. Weber, 2005; Beurton et al., 2000) and ‘species’ (cf. Mayden, 1997), uses that attribute properties of ontologically distinct types, these terms seem hostile to externalist treatment. At the very least these findings suggest that we should not settle at the outset of scientific investigation that the languages needed to express scientific theories *must* have a particular semantics. Insofar as the Realist requires that we make such stipulations, we have reason to doubt that the Realist’s ontological methodology should be adopted. Maybe the semantics for a language of biology will prove to be externalist, but given the arguments here, this metaontological assumption is at least contentious. To the degree that other terms in the sciences are like ‘gene’ and ‘species’ metaphysicians should abandon the Realist position.

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