What's Right With a Syntactic Approach to Theories and Models?

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Abstract

Syntactic approaches in the philosophy of science, which are based on formalizations in predicate logic, are often considered in principle inferior to semantic approaches, which are based on formalizations with the help of structures. To compare the two kinds of approach, I identify some ambiguities in common semantic accounts and explicate the concept of a structure in a way that avoids hidden references to a specific vocabulary. From there, I argue that contrary to common opinion (i) unintended models do not pose a significant problem for syntactic approaches to scientific theories, (ii) syntactic approaches can be at least as language independent as semantic ones, and (iii) in syntactic approaches, scientific theories can be as well connected to the world as in semantic ones. Based on these results, I argue that syntactic and semantic approaches fare equally well when it comes to (iv) ease of application, (iv) accommodating the use of models in the sciences, and (vi) capturing the theory-observation relation.

Keywords: syntactic view; semantic view; received view; structuralism; axiomatization; formalization; genetic method; structure; pure structure; embedding

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1 Introduction

The analysis of scientific theories needs some framework—a way in which theories and possibly the world are described and, building on that, a set of tools for analysis. Suppes (1968, 654–656) argues that formal frameworks in particular allow for descriptions and analyses that are explicit, objective, and standardized, abstract from non-essential aspects and help in identifying self-contained, minimal assumptions. More specifically, he argues that these rewards can be reaped by using formalizations in set theory or first order predicate logic (Suppes 1968, 653). Presumably, he would also argue that formalizations in higher order logic and model theory can lead to the same rewards.

The earliest of such formal accounts, developed within logical empiricism and especially by Carnap and Hempel, has been dubbed the 'Received View'. It relies on formalizations of scientific theories in languages of predicate logic and assumes that the non-logical vocabulary $\mathscr{V}$ is bipartitioned into a set $\mathscr{B}$ of *basic terms* (or "observational terms") and a set $\mathscr{A}$ of *auxiliary terms* (or "theoretical terms"), where only the basic terms are directly interpreted. The interpretation of the auxiliary terms is fixed only by the interpretation of the basic terms, the formalization of the theory, and additional sentences (*correspondence rules*) containing both basic and auxiliary terms. The Received View is often regarded as superseded by frameworks in which scientific theories are assumed to be formalized in model or set theory, which I will call *semantic approaches*.[1] Calling frameworks that rely on formalizations in predicate logic of first or higher order '*syntactic*', the Received View is a specific syntactic approach that *additionally* assumes a bipartition of the vocabulary and allows a direct interpretation only of the basic terms.[2]

While the additional assumptions of the Received View have been widely criticized, syntactic approaches in general have often been dismissed with reference to criticisms of the Received View. In this article, I will argue that this was a mistake, and defend syntactic approaches (but not the Received View) *relative to* semantic approaches. Syntactic approaches are widely thought to suffer from a number of shortcomings: Unlike semantic approaches, syntactic approaches (i) cannot avoid unintended models, (ii) are language dependent, (iii) require and have failed to provide an account of the relation between language and the world, (iv) lead to cumbersome descriptions of scientific theories, (v) do not allow the treatment of models, and are therefore at odds with actual scientific practice, and (vi) presume a misleading or false relation between theory and observation. I will argue that if the first three of these problems can be solved for semantic approaches, they can be solved for syntactic ones as well. That the same holds for the last

[1] I will use 'approach' and 'framework' interchangeably in the following.

[2] Although this categorization of syntactic and semantic approaches is standard terminology, it is somewhat incongruous: Set theory and predicate logic can both be used as foundational languages in which to formalize other theories. Model theory, on the other hand, is one of those theories that can be so formalized. I will discuss this point further at the end of §4.

three problems then follows easily. In discussing the relative merits of syntactic approaches, I will ignore the ontological question of whether theories can be identified with either kind of description (or, for that matter, with platonic objects, sets of propositions, thoughts, actions, connection weights in brains, combinations thereof…). Given that scientific theories are typically not formalized in either kind of framework, they are probably ontologically different from both kinds of descriptions.[3]

Given its thesis, this article can probably count as a contribution to the "endless silly, largely unpublished debates over what semantic approaches can do that syntactical or statement approaches intrinsically cannot" that Suppe (2000, S103) laments. There are still justifications for its existence. First, beyond the intrinsic strength of formalizations in predicate logic, my discussion also covers the connection between a theory's description and the world, the relation between formalizations in set and model theory, and the language independence of the approaches. Hence at most some of it is silly. Second, even if the debates are silly, many philosophers of science do hold the view that an analysis of science better use a semantic approach, and sometimes make it sound like semantic approaches are intrinsically superior. After all, Suppe (1974b, 114) himself claims early in the history of the discussion that "if formalization is desirable in a philosophical analysis of theories, it must be of a semantic sort". And looking back at the developments since he made this claim, Suppe (2000, S110, my emphasis) concludes that "by construing theories in terms of families of models, semantic analyses—*and they alone*—have real potential for parlaying such new philosophical wisdom [gained by focusing on models] into enhanced understanding of theories". These claims suggest, at least on the surface, that there is an intrinsic advantage of semantic approaches. Third, if the debates are silly, it might be a good idea to finally put them to rest. This, incidentally, is what I would like to contribute to. As a first step, I may note that this contribution to the debates is meant to be published.

2 Translating between sentences and models

According to van Fraassen (1980, 44),

> [t]he syntactic picture of a theory identifies it with a body of theorems, stated in one particular language chosen for the expression of that theory. This should be contrasted with the alternative of presenting a theory in the first instance by identifying a class of structures as its models. In this second, semantic, approach the language used to express the theory is neither basic nor unique; the same class of structures could well be described in radically different ways, each with its own limitations.

[3]The question what scientific theories *really* are may also simply not be well-defined, so that there is no fact of the matter.

In the semantic approach, a theory is thus formalized by a class of structures, in the syntactic approach by a set of sentences. It seems plausible that every syntactic description of a scientific theory can be captured by a semantic one, because any set Σ of sentences determines the set $\mathbf{S}$ of its models through the mapping

$$\mathbf{\Phi} : \Sigma \mapsto \mathbf{S} := \{\mathfrak{S} \mid \mathfrak{S} \vDash \Sigma\} . \tag{1}$$

Σ has a fixed vocabulary $\mathscr{V}$, containing m_i-place predicates P_i, n_j-place functions F_j, constants c_k, and in higher order logic their respective types. $\mathscr{V}$ does not disappear by the mapping $\mathbf{\Phi}$, since every structure $\mathfrak{S} \in \mathbf{S}$ contains a mapping from every element of $\mathscr{V}$ to an extension with the corresponding arity and type (Hodges 1993, 2–4; Chang and Keisler 1990, §1.3). Call the arities and types of the extensions the *similarity type* of $\mathscr{V}$. $\mathscr{V}$ and its similarity type, sometimes called the '*signature* of $\mathfrak{S}$' can thus be read off uniquely from $\mathfrak{S}$ (Hodges 1993, 4).[4]

However, $\mathbf{\Phi}$ does lose some information because it cannot distinguish between equivalent sets of sentences, that is, if $\Sigma \Dashv\vDash \Theta$, then $\mathbf{\Phi}(\Sigma) = \mathbf{\Phi}(\Theta)$. This can pose problems, for example when modifying a theory: One formulation of a theory can be vastly superior to an equivalent one when it needs to be generalized or adjusted, as van Fraassen (1980, §3.5) has pointed out. Relatedly, the formulation is also relevant when it comes to the inductive support of components of the theory: If the data support one postulate but not another, a formulation that keeps the two postulates separate is arguably better than one that contains a single postulate equivalent to their conjunction. Thus, if irrelevant conjunctions indeed pose a problem for an explicatum of 'confirmation' (cf. Fitelson 2002), the possibility of reformulating a theory allows hiding the conjunction, and thus making the irrelevant conjunct harder to detect. And if the problem of irrelevant conjunctions can *only* be solved by distinguishing between equivalent formulations, then it cannot be solved within a semantic approach.[5]

The loss of distinction between equivalent sets of sentences does not pose a problem, however, if the results of an analysis of a scientific theory are invariant under the theory's equivalent reformulation; and outside of questions of induction, many interesting analyses of scientific theories are so invariant. Conversely, it is often considered a problem if an analysis of the theory is not (witness, for example, Hempel 1965, §§4–5; Carnap 1956, 56; Winnie 1970, 294–295).

Conversely, any set of structures $\mathbf{S}$ yields a set of sentences Σ through the mapping

$$\mathbf{\Psi} : \mathbf{S} \mapsto \Sigma := \{\varphi \mid \mathfrak{S} \vDash \varphi \text{ for all } \mathfrak{S} \in \mathbf{S}\} . \tag{2}$$

$\mathbf{\Psi}(\mathbf{S})$ is thus the set of sentences true in all elements of $\mathbf{S}$. For many logics, $\mathbf{\Psi}$ is not the inverse of $\mathbf{\Phi}$. For instance, $\mathbf{S}$ may not be closed under elementary equivalence, while for first order logic, $\mathbf{\Phi}(\mathbf{\Psi}(\mathbf{S}))$ is. Of course, the set $\mathbf{S}' := \mathbf{\Phi}(\mathbf{\Psi}(\mathbf{S}))$

[4]This is discussed at length in §3.

[5]This, of course, assumes semantic approaches that indeed do not incorporate a theory's specific formulation.

of structures is mapped onto itself by $\mathbf{\Phi} \circ \mathbf{\Psi}$ and the set $\Sigma' := \mathbf{\Psi}(\mathbf{\Phi}(\Sigma))$ of sentences is mapped onto itself by $\mathbf{\Psi} \circ \mathbf{\Phi}$. For such sets of sentences and sets of structures, $\mathbf{\Phi}$ and $\mathbf{\Psi}$ are therefore inverses. Call the class of models of a single set of first order sentences *Δ-elementary*. It is known that there are classes of relational first order structures that are not Δ-elementary, but are complements of Δ-elementary classes, or are unions of Δ-elementary classes. All and only classes that are unions of Δ-elementary classes are closed under elementary equivalence, where two structures are *elementarily equivalent* if and only if they are models of the same first order sentences (Bell and Slomson 1974, 141–144). Call two structures that are models of the same sentences of some given logic *syntactically equivalent* in that logic. Higher order logic can often distinguish between elementary equivalent but non-isomorphic structures, and the use of logics that allow formulas with infinitely many quantifiers, conjuncts, or disjuncts further increases the number of classes for which $\mathbf{\Psi}$ is the inverse of $\mathbf{\Phi}$. For simplicity, I will assume in the following that each class $\mathbf{S}$ of structures under discussion either can be captured by a single set of sentences of the respective logic (so that $\mathbf{\Phi}(\mathbf{\Psi}(\mathbf{S})) = \mathbf{S}$) or is not closed under syntactical equivalence.

As a matter of principle (that is, independently of the logic used), $\mathbf{\Psi}$ always loses information because it cannot distinguish between two non-identical, element-wise isomorphic sets of structures, that is, if for each $\mathfrak{S} \in \mathbf{S}$, there is a $\mathfrak{T} \in \mathbf{T}$ with $\mathfrak{S} \simeq \mathfrak{T}$ and *vice versa*, then $\mathbf{\Psi}(\mathbf{S}) = \mathbf{\Psi}(\mathbf{T})$ even if $\mathbf{S} \neq \mathbf{T}$.[6] However, if the results of an analysis of a scientific theory are invariant under isomorphic transformations of theories (a natural demand that is usually fulfilled),[7] this does not pose a problem.

It is sometimes claimed that as a matter of principle, more information is lost in the mapping $\mathbf{\Psi}$ than the distinction between isomorphic structures. Suppe (2000, S104), for example, argues that syntactic approaches are in principle unable to capture some scientific theories, because for syntactic descriptions

> the Löwenheim–Skolem theorem implie[s] that [...] models must include both intended and wildly unintended models. Unintended models provide potential counterexamples.
>
> Blocking them more concerns eliminating syntactical-approach artifacts than dealing with substantive analysis. [...] For example, Kitcher's (1989) unification explanation account has a very simple idea. But he develops it syntactically spending most of the paper trying to block unintended consequences that are artifacts of his formalism. [...]
>
> This is the correct sense of [the] claim symbolic logic is an inappropriate formalism.

[6]Note that this is a weaker condition than the pointwise isomorphism defined by Halvorson (2012, 190).

[7]See, for example, van Fraassen's notion of a theory (van Fraassen 2008, 238; cf. 2002, 22).

First and foremost, Suppe's criticism is not directed at syntactic approaches (or symbolic logic) in general, because the Löwenheim-Skolem theorem does not hold in higher order logic. And while there are structures that even in predicate logic of transfinite order cannot be described up to isomorphism (Enderton 2009, §2, §4), set theory itself can be described up to isomorphism by (and only by) the addition of axioms about the existence of specific inaccessible cardinals (Väänänen 2001, 516), which is enough to capture the analyses in semantic approaches. Since the proof theory of higher order logic is not complete for full semantics (Väänänen 2001, 505), entailment needs to be defined semantically in terms of structures. Luckily, there is no good reason to disallow the use of structures in syntactic approaches, since syntactic approaches presume only that scientific theories can be analyzed by way of their description in predicate logic. They do not presume that the analysis itself must proceed wholly in the object language. In fact, the use of formal models to define entailment was even part of the Received View (Lutz 2012b, 83).

Second, for predicate logic of any order, some structures *can* be characterized in that language up to isomorphism, so that there are no unintended models. The standard examples of structures that cannot be captured in first order logic, e. g. the natural numbers and the reals, can be described up to isomorphism in finite order logic if a full semantics is assumed (Enderton 2009, §2).[8] And if the theory has a finite domain, all elementarily equivalent structures are isomorphic (Hodges 1993, §2.2, ex. 5).

Third, whether the existence of unintended models poses a problem depends on the kind of analysis sought after. The answers to questions that can be phrased in the object language, for example, do not depend on isomorphism, since otherwise they would provide a means of distinguishing between non-isomorphic models. And an analysis that requires isomorphism in only a finite subdomain of the theory's domain (for example in the domain of observations) is immune to the problem even in a first order language. The mapping $\boldsymbol{\Psi}$ of a syntactic descriptions of observations can in general be inverted by $\boldsymbol{\Phi}$, since the number of observations will always stay well below any inaccessible cardinal.

Finally, Suppe's criticism rests on an equivocation of 'unintended model' and 'non-standard model', the latter referring to a model that is syntactically equivalent but not isomorphic to a standard model. Even though Kitcher spends a lot of work blocking unintended consequences, the unintended consequences are not elementarily equivalent to intended ones and can therefore be blocked by syntactic means of first order. Kitcher's account is a good example of how difficult it can be to develop a formalization of an idea, but not of a failure of a syntactic approach because of the Löwenheim-Skolem theorem. Put crudely, if the unintended consequences could be blocked by using a predicate logic of higher order, Kitcher would probably not have shied away from it for lack of a complete proof theory.

[8] Leivant (1994, §3.1, §5.4) and Väänänen (2001, 504–505) discuss the difference between full semantics and Henkin semantics. I will come back to this in §4.

3 Language independence

According to the quote by van Fraassen (1980, 44) above, "the same class of structures could well be described in radically different ways, each with its own limitations", and in the words of Suppe (1989, 4), the semantic approach

> construes theories as what their formulations refer to when the formulations are given a (formal) semantic interpretation. Thus, 'semantic' is used here in the sense of formal semantics or model theory in mathematical logic.

French and Ladyman (1999, 114–115) similarly assume that the structures used in semantic approaches do not contain a vocabulary when they discuss a criticism of semantic approaches they attribute to Mauricio Suárez: If a semantic approach uses models as they are defined in model theory, it is still dependent on a language, since a model "is a structure and an interpretation of a formal language in terms of that structure (that is, a map from the symbols of the syntax to elements of the structure)". If models are taken to involve such a mapping, French and Ladyman (1999, 114) write,

> it is clear that the celebrated claim of the linguistic independence of considering models (and not first-order formalizations of theories), stressed by adherents of the semantic approach as giving it a clear advantage over the syntactic view, is simply not true.

They quote a concurring passage by van Fraassen (1989, 366):

> The impact of Suppes's innovation [switching to models] is lost if models are defined, as in many standard logic texts, to be partially linguistic entities, each yoked to a particular syntax. In my terminology here the models are mathematical structures, called models of a given theory only by virtue of belonging to the class defined to be the models of that theory.

"Thus," French and Ladyman (1999, 115) conclude, "van Fraassen should be interpreted as talking about *structures* by those who wish to understand model in the sense of the 'standard logic texts'".

However, it is not exactly clear what French and Ladyman mean by 'structure', except that it contains the extensions of symbols, and not the symbols itself. They do claim that van Fraassen's "emphasis on structure is compatible with this definition of model theory from a contemporary textbook", according to which model theory "is the study of the construction and classification of structures within specified classes of structures". Of course, everything in this quote from Hodges (1993, ix) depends on his definition of 'structure'. A few pages further on (Hodges 1993, 1), there is evidence that his definition might not be what French and Ladyman think it is:

> Model theorists are forever talking about symbols, names and labels. A group theorist will happily write the same Abelian group multiplicatively or additively, whichever is more convenient for the matter in hand. Not so the model theorist: for him or her the group with '·' is one structure and the group with '+' is a different structure. Change the name and you change the structure.

One of the reasons that Hodges (1993, 2) gives for this focus on symbols is that

> we shall often want to compare two structures and study the homomorphisms from one to the other. What is a homomorphism? [...] [A] homomorphism from structure A to structure B is a map which carries each operation of A to *the operation with the same name in* B.

But, of course, it is the *definition* of a structure that shows whether symbols play a role in model theory. Here is, for example, the part of his definition that deals with relations (Hodges 1993, 2, my notation):

> For each positive integer n [a structure contains] a set of n-ary relations on $|\mathfrak{A}|$ (i. e. subsets of $|\mathfrak{A}|^n$), each of which is named by one or more n-ary **relation symbols**. If R is a relation symbol, we write $R^{\mathfrak{A}}$ for the relation named by R.

It is clear that the symbols play an important role in a structure: They identify the extensions by naming them.

French and Ladyman would have found a definition better suited to their position in a less contemporary textbook (Bell and Slomson 1974, §3.2) in which

> a *relational structure* is an ordered pair
>
> $$\mathfrak{A} = \langle A, \{R_n \mid n \in \omega\}\rangle ,$$
>
> where [...] for $n \in \omega$ R_n is a finitary, say $\lambda(n)$-ary relation on A. [...] The relational structure $\mathfrak{A}$ will count as an interpretation of the language L [$= \{P_n : n \in \omega\}$] if the degrees of the relations R_n correspond to the degrees [$\delta(n)$] of the predicate letters P_n. That is, for $n \in \omega$, $\delta(n) = \lambda(n)$. In this case we say that the relational structure $\mathfrak{A}$ is a *realization* of the language L and that L is *appropriate* for the structure $\mathfrak{A}$.

This definition of what one could call '*pure structures*' (Smith 2008) seems to be used by many proponents of the semantic approach (e. g., da Costa and French 1990, French and Ladyman 1999). It is free of any specific vocabulary up to the vocabulary's similarity type, that is, any appropriate vocabulary can be used with the structure.

A structure $\mathfrak{A} = \langle A, \mathscr{I}\rangle$ contains an interpretation $\mathscr{I}$ mapping, say, the relations $\{P_i\}_{i\in I}$, from a vocabulary $\mathscr{V}$ to their extensions, $\{P_i^{\mathfrak{A}} \mid i \in I\}$. A pure structure, on the other hand, contains instead an indexed set $\{P_i^{\mathfrak{A}}\}_{i\in I}$ of extensions.[9] But this introduces a vocabulary through the back door: The mapping from I to the set $\{P_i^{\mathfrak{A}} \mid i \in I\}$ that is needed to define such an indexed set is is the same as an interpretation with the vocabulary I. Any claim that I is not a vocabulary but an index set has to rely on commitments (or rather declarations) outside of the formalism.

Suppes (2002) writes structures as tuples with the domain as first element, which can avoid this hidden dependence on a specific vocabulary: Tuples may, for example, be introduced as primitives, with axioms like $\langle a_1,\dots a_n\rangle = \langle b_1,\dots,b_n\rangle \Leftrightarrow a_1 = b_1,\dots,a_n = b_n$. They may also be defined through sets, as in Kuratowski's definition. Of course, one can still introduce a vocabulary by assigning, for example, natural numbers to the positions of the tuple, but this assignment is not unique. In fact, any mapping from a well-ordered index set with the right cardinality would do. (The common method of representing a tuple $\langle a_1,\dots,a_n\rangle$ as a set of pairs $\{\langle 1,a_1\rangle,\dots,\langle n,a_n\rangle\}$ again introduces a specific vocabulary, the natural numbers $\{1,\dots,n\}$.[10]) The possibility of assigning a vocabulary to a tuple cannot be avoided, since the ordering of the extensions is essential. One could not just drop the ordering by, for example, using a multiset of extensions instead of a tuple, as an example by Halvorson (2012, 192) makes clear: Let

$$\Theta \rightleftharpoons \{\exists_{=1}x(x=x)\} \tag{3}$$

and

$$\Lambda \rightleftharpoons \{\exists_{=1}x(x=x)\} \cup \{\forall x(Q_0x \to Q_ix \mid i \in \mathbb{N}\} \tag{4}$$

be theories described in the vocabulary $\mathcal{V} = \{Q_i \mid i \in \mathbb{N}^0\}$. The two theories are not logically equivalent and thus do not have the same class of models. However, if structures were defined *without* any order of their extensions, then the two theories *would* have the same models: Every model of Λ is trivially a model of Θ. Conversely, any model of Θ in which the extension of every predicate contains the one element of the domain is a model of Λ. If in a model of Θ some extension is empty, make it the extension of Q_0 (which is possible, since the extensions are not ordered), so that the model is a model of Λ as well. Thus every model of Θ is a model of Λ and *vice versa*. Therefore, if the extensions in a structure are not ordered, semantic approaches in principle cannot distinguish all those theories that can be distinguished syntactically.

Thus even when trying to avoid a specific vocabulary, something like a tuple is needed for the definition of a pure structure. But since structures can have infinite vocabularies and tuples are finite, pure structures cannot be tuples if they are

[9] It is clear from their use of ω as an index set that Bell and Slomson (1974) intend '$\{R_n \mid n \in \omega\}$' to be an indexed set, in my notation '$\{R_n\}_{n\in\omega}$' (rather then the set of the indexed set's elements). For if $R_2 = R_3$, then $\{R_i\}_{i\in\{1,2,3\}} \neq \{R_i\}_{i\in\{1,2\}}$, while $\{R_i \mid i \in \{1,2,3\}\} = \{R_i \mid i \in \{1,2\}\}$; Bell and Slomson thus need indexed sets to allow for different names with identical extension.

[10] This, incidentally, is the vocabulary that Carnap (1958, 242) chooses to name physical objects.

to be able to express anything that structures can express. In line with the possibility of assigning any finite well-ordered index set to tuples, I therefore suggest to let a pure structure $\hat{\mathfrak{A}}$ contain a domain A and a mapping from an arbitrary and not fixed well-ordered index set to a set of extensions on the domain. That the index set is not fixed can be captured by identifying any two mappings that only differ in their index sets:

Definition 1. A *representative of a pure structure* $\hat{\mathfrak{A}}$ is a triple $\langle A, a, \prec\rangle$, where $a : I \longrightarrow \mathscr{I}(\mathscr{V})$ is a mapping from the index set I to the image of an interpretation $\mathscr{I}$, and $\prec$ is a well-ordering of I. Two triples $\langle A, a, \prec\rangle$ and $\langle A, a', \prec'\rangle$ *represent the same pure structure*, $[\langle A, a, \prec\rangle] = [\langle A, a', \prec'\rangle]$, if and only if there is an order isomorphism $f : I \longrightarrow I'$ and $a \circ f = a'$.

The use of an interpretation $\mathscr{I}$ is simply a way to ensure that a maps only to set theoretical objects that can be extensions of predicate, function, or constant symbols. The definition determines a tuple if and only if the index sets of the representatives are finite. In both the finite and infinite case, the order of the objects in $\mathscr{I}(\mathscr{V})$ is preserved as in an ordered set, and additionally, elements of $\mathscr{I}(\mathscr{V})$ can occur repeatedly as in a multiset.

Definition 1 does not introduce a specific vocabulary, and is as language independent as the use of tuples: Even if not represented as a mapping from natural numbers to extensions, a tuple *can* be assigned a well-ordered index set, and this definition of pure structures only makes this possibility explicit. To stress the point: The infinity of vocabularies in definition 1 is not artificially introduced. Both indexed sets and tuples assume some specific set or multiple sets of entities that provide a vocabulary. Index sets can be used as vocabularies immediately, since they are already mappings from some set to a set of extensions. Tuples are either defined as a mapping from a range of natural numbers to a set of extensions or allow the introduction of such a mapping without any further assumptions except about the elements of the index set. In definition 1, the identification of structures with different index sets avoids this one further assumption.

Muller (2010) has suggested a conception similar to definition 1 for the use in semantic approaches, but he considers his conception an extension of semantic approaches, and justifies it with the need to connect theories to the world with its help. I will briefly revisit Muller's suggestion for connecting theories to the world below; at this point I only want to stress (again) that definition 1 is not an extension of the concept of a structure used in semantic approaches, but rather an acknowledgment of the infinity of vocabularies implicitly contained in it.

In the definition of 'structure' given by Bell and Slomson (1974), the index set I plays the role of the set of relation symbols $\{P_i \mid i \in I\}$ used by Chang and Keisler (1990) and Hodges (1993). For examples relevant in the following, see the definitions of 'reduct', 'isomorphism', and 'substructure' by Chang and Keisler (1990, 20–23) and by Bell and Slomson (1974, 153, 73), respectively. Definition 1 requires a somewhat more elaborate modification of standard definitions, of which I will only give the modification for the notion of embedding. Since

$$\begin{tikzcd} I \arrow[r, "a"] \arrow[d, "g"] & \mathscr{I}(\mathscr{V}) & A \arrow[d, "h"] \\ J \arrow[r, "b"] & \mathscr{I}(\mathscr{V}') & B \end{tikzcd}$$

Figure 1: Functions between arbitrary representatives of two structures $\hat{\mathfrak{A}} = [\langle A, a, \prec\rangle]$, $\hat{\mathfrak{B}} = [\langle B, b, \prec'\rangle]$ so that $\hat{\mathfrak{A}}$ can be embedded in $\hat{\mathfrak{B}}$.

pure structures are given by classes of mappings with well-ordered index sets, embeddings between pure structures can be defined via a bijection between the index sets of their representatives and a function from one pure structure's domain to the other's. In this definition (which I give only for pure first order structures), the position in the ordering of the index sets plays the role of the element of the vocabulary.

Definition 2. A *pure first order structure* $\hat{\mathfrak{A}}$ can be *embedded* in a pure first order structure $\hat{\mathfrak{B}}$ if and only if for any two representatives $\hat{\mathfrak{A}} = [\langle A, a, \prec\rangle]$ with $a : I \longrightarrow \mathscr{I}(\mathscr{V})$ and $\prec$ an ordering on I, and $\hat{\mathfrak{B}} = [\langle B, b, \prec'\rangle]$ with $b : J \longrightarrow \mathscr{I}(\mathscr{V}')$ and $\prec'$ an ordering on J, there is an order isomorphism $g : I \longrightarrow J$ and an injective mapping $h : A \longrightarrow B$ such that

1. for all $c \in I$ mapped to constants by a, $h(a(c)) = b(g(c))$,
2. for all $F \in I$ mapped to n-ary functions by a and all $x_1, \ldots, x_n \in A$, $h\big(a(F)(x_1, \ldots, x_n)\big) = b\big(g(F)\big)(h(x_1), \ldots, h(x_n))$, and
3. for all $P \in I$ mapped to n-ary relations by a and all $x_1, \ldots, x_n \in A$, $\langle x_1, \ldots, x_n\rangle \in a(P) \Leftrightarrow \langle h(x_1), \ldots, h(x_n)\rangle \in b(g(P))$,

h is called an *embedding* of $\hat{\mathfrak{A}}$ in $\hat{\mathfrak{B}}$. If h is surjective, $\hat{\mathfrak{A}}$ and $\hat{\mathfrak{B}}$ are called *isomorphic*.

Embeddability of pure structures only has to be shown for one representative of each pure structure:

Lemma 1. *Pure first order structure $\hat{\mathfrak{A}}$ can be embedded in pure first order structure $\hat{\mathfrak{B}}$ if and only if there are two representatives $\hat{\mathfrak{A}} = [\langle A, a, \prec\rangle]$ with $a : I \longrightarrow \mathscr{I}(\mathscr{V})$ and $\prec$ an ordering on I, and $\hat{\mathfrak{B}} = [\langle B, b, \prec'\rangle]$ with $b : J \longrightarrow \mathscr{I}(\mathscr{V}')$ and $\prec'$ an ordering on J, an order isomorphism $g : I \longrightarrow J$, and an injective mapping $h : A \longrightarrow B$ such that conditions 1–3 of definition 2 are fulfilled.*

Proof. The proof from left to right is immediate. For the other direction, assume $[\langle A, c, \prec^*\rangle] = \hat{\mathfrak{A}}$ with $c : K \longrightarrow \mathscr{I}(\mathscr{V})$ and $[\langle B, d, \prec'^*\rangle] = \hat{\mathfrak{B}}$ with $d : L \longrightarrow \mathscr{I}(\mathscr{V}')$. By definition 1, there are order isomorphisms $i : K \longrightarrow I$ and $j : J \longrightarrow L$ (see figure 2). Then there is an order isomorphism $k : K \longrightarrow L$ and a one-to-one mapping $h : A \longrightarrow B$ such that

1. for all $m \in K$ mapped to constants by c, $h(c(m)) = h \circ a \circ i(m) = b \circ g \circ i(m) = b \circ j^{-1} \circ j \circ g \circ i(m) = d(j \circ g \circ i(m)) = d(k(m))$,

$$\begin{tikzcd}
K \arrow[d, "i"] \arrow[dr, "c=a\circ i"] \arrow[ddd, bend right, "k=j\circ g\circ i"'] & & \\
I \arrow[r, "a"] \arrow[d, "g"] & \mathscr{I}(\mathscr{V}) & A \arrow[d, "h"] \\
J \arrow[r, "b"] \arrow[d, "j"] & \mathscr{J}(\mathscr{V}') & B \\
L \arrow[ur, "d=b\circ j^{-1}"'] & &
\end{tikzcd}$$

Figure 2: Functions between four representatives of two structures $\hat{\mathfrak{A}} = [\langle A, a, \prec\rangle] = [\langle A, c, \prec^*\rangle]$, $\hat{\mathfrak{B}} = [\langle B, b, \prec'\rangle] = [\langle B, d, \prec'^*\rangle]$.

2. for all $F \in K$ mapped to n-ary functions by c and all $x_1, \ldots, x_n \in A$, $h\big(c(F)(x_1, \ldots, x_n)\big) = h\big(a \circ i(F))(x_1, \ldots, x_n)\big) = b \circ g \circ i(F)(h(x_1), \ldots, h(x_n)) = b \circ j^{-1} \circ j \circ g \circ i(F)(h(x_1), \ldots, h(x_n)) = d(k(F))(h(x_1), \ldots, h(x_n))$, and

3. for all $P \in I$ mapped to n-ary relations by c and all $x_1, \ldots, x_n \in A$, $\langle x_1, \ldots, x_n\rangle \in c(P) \Leftrightarrow \langle x_1, \ldots, x_n\rangle \in a \circ i(P) \Leftrightarrow \langle x_1, \ldots, x_n\rangle \in a \circ i(P) \Leftrightarrow \langle h(x_1), \ldots, h(x_n)\rangle \in b \circ g \circ i(P) \Leftrightarrow \langle h(x_1), \ldots, h(x_n)\rangle \in b \circ j^{-1} \circ j \circ g \circ i(P) \Leftrightarrow \langle h(x_1), \ldots, h(x_n)\rangle \in d(k(P))$

$\square$

The definition of embedding for pure structures respects the standard definition of embedding for structures (Hodges 1993, 5) in that there is a connection between structures and pure structures, and under this connection, the two definitions of embedding are interchangeable. More precisely, any structure $\mathfrak{A}$ gives rise to a representative of a pure structure $\hat{\mathfrak{A}}$ by a well-ordering of its vocabulary, and any pure structure $\hat{\mathfrak{A}}$ contains a structure $\mathfrak{A}$ among its representatives. From the definition of embedding for pure structures, it follows that a pure structure $\hat{\mathfrak{A}}$ can be embedded in a pure structure $\hat{\mathfrak{B}}$ if and only if they are represented by some structures $\mathfrak{A}$ and $\mathfrak{B}$ with the same vocabulary and the same ordering of the vocabulary, and $\mathfrak{A}$ can be embedded in $\mathfrak{B}$:

Claim 2. *Pure first order structure $\hat{\mathfrak{A}}$ can be embedded in pure first order $\hat{\mathfrak{B}}$ if and only if there are first order structures $\mathfrak{A} = \langle A, \mathscr{I}\rangle$ and $\mathfrak{B} = \langle B, \mathscr{J}\rangle$ and a well-ordering $\prec$ such that $\hat{\mathfrak{A}} = [\langle A, \mathscr{I}, \prec\rangle]$, $\hat{\mathfrak{B}} = [\langle B, \mathscr{J}, \prec\rangle]$, and $\mathfrak{A}$ can be embedded in $\mathfrak{B}$.*

Proof. For the proof from left to right, choose any representatives $\langle A, a, \prec\rangle$ of $\hat{\mathfrak{A}}$ and $\langle B, b, \prec'\rangle$ of $\hat{\mathfrak{B}}$. Now choose for $\mathscr{V}$ the index set of a, $\mathscr{I} = a$, and $\mathscr{J} = b \circ g$, where g is the order isomorphism from I to J. The claim follows from definition 2 because for any $Q \in \mathscr{V}$, $h(Q^{\mathfrak{A}}) = h \circ a(Q) = b \circ g(Q) = \mathscr{J}(Q) = Q^{\mathfrak{B}}$ in the domain of $\hat{\mathfrak{A}}$, which is also the domain of $\mathfrak{A}$.

For the proof from right to left, choose $g = \mathrm{id}$. The claim follows with lemma 1 because for any $Q \in \mathscr{V}$, $h(\mathscr{I}(Q)) = h(Q^{\mathfrak{A}}) = Q^{\mathfrak{B}} = \mathscr{J}(g(Q))$. □

Putting it slightly differently, $\hat{\mathfrak{A}}$ can be embedded in $\hat{\mathfrak{B}}$ if and only if for *any* two structures $\mathfrak{A}$ and $\mathfrak{B}$ that represent, with a common well-ordering of their vocabulary, $\hat{\mathfrak{A}}$ and $\hat{\mathfrak{B}}$, $\mathfrak{A}$ can be embedded in $\mathfrak{B}$:

Corollary 3. *Pure first order structure $\hat{\mathfrak{A}}$ can be embedded in $\hat{\mathfrak{B}}$ if and only if for any structures $\mathfrak{A} = \langle A, \mathscr{I} \rangle$ and $\mathfrak{B} = \langle B, \mathscr{J} \rangle$ and any well-ordering $\prec$ such that $\hat{\mathfrak{A}} = [\langle A, \mathscr{I}, \prec \rangle]$ and $\hat{\mathfrak{B}} = [\langle B, \mathscr{J}, \prec \rangle]$, $\mathfrak{A}$ can be embedded in $\mathfrak{B}$.*

Proof. Immediately from claim 2 and lemma 1. □

Conversely, a structure $\mathfrak{A}$ can be embedded in a structure $\mathfrak{B}$ if and only if, under some well-ordering of their vocabulary, they represent pure structures $\hat{\mathfrak{A}}$ and $\hat{\mathfrak{B}}$, respectively, such that $\hat{\mathfrak{A}}$ can be embedded in $\hat{\mathfrak{B}}$. The proof assumes the axiom of choice.

Corollary 4. *First order structure $\mathfrak{A} = \langle A, \mathscr{I} \rangle$ can be embedded in $\mathfrak{B} = \langle B, \mathscr{J} \rangle$ if and only if $[\langle A, \mathscr{I}, \prec \rangle]$ can be embedded in $[\langle B, \mathscr{J}, \prec \rangle]$ for some well-ordering $\prec$.*

Proof. Note that $\mathscr{I}$ and $\mathscr{J}$ must have the same vocabulary. The proof from right to left follows immediately from theorem 2. For the proof from left to right, note that any vocabulary $\mathscr{V}$ can be well-ordered (assuming the axiom of choice). The claim again follows immediately from theorem 2. □

Again, putting this slightly differently, $\mathfrak{A}$ can be embedded in $\mathfrak{B}$ if and only if under *any* well-ordering, they represent pure structures $\hat{\mathfrak{A}}$ and $\hat{\mathfrak{B}}$, respectively, such that $\hat{\mathfrak{A}}$ can be embedded in $\hat{\mathfrak{B}}$:

Corollary 5. *First order structure $\mathfrak{A} = \langle A, \mathscr{I} \rangle$ can be embedded in structure $\mathfrak{B} = \langle B, \mathscr{J} \rangle$ if and only if $[\langle A, \mathscr{I}, \prec \rangle]$ can be embedded in $[\langle B, \mathscr{J}, \prec \rangle]$ for all well-orderings $\prec$.*

Proof. Immediately from corollary 4 and lemma 1. □

These results show that language independent pure structures and typical model theoretic operations between them are well-defined. It also shows that nothing is lost by discussing embeddings only for structures or only for pure structures. Language independence can be achieved at any point by introducing an ordering for the vocabulary, turning the structures into representatives of pure structures. Conversely, the ordering can be eliminated at any point in the discussion by choosing, for all pure structures at once, one of the possible vocabularies. It is clear that other model theoretic notions besides embedding have similar analogues for pure structures.

The independence from a specific vocabulary that French and Ladyman (1999) call 'much celebrated' seems indeed important, since language is inherently conventional, or at least relative to a group of speakers. Understanding 'propositions' "in the medieval sense of the term", that is, as "interpreted sentences of some particular language" Suppe (1974a, 204–205), argues that therefore theories cannot be the propositions in which they are formulated:

> Suppose a theory is first formulated in English, and then is translated into French. The English formulation and the French formulations constitute different collection of propositions; if theories were collections of propositions, then the translation of the theory into French would produce a new theory; but, of course, it does not—it is the same theory reformulated in French.

Suppe's point is clear: The ideal gas law does not change only because one uses the names 'pression', 'volume', 'constante de Boltzmann', and 'températur' instead of 'pressure', 'volume', 'Boltzmann constant', and 'temperature'. The use of structures as defined above avoids the use of any specific vocabulary, and thus supports the claim by van Fraassen (1989, 222) that "in discussions of the structure of theories [language] can largely be ignored".

The simultaneous switch of all structures to pure structures indeed leads to a freedom to choose between an infinity of vocabularies, and thus the freedom from any specific one. So assume that two models $\mathfrak{A}$ and $\mathfrak{A}'$ of two sets of sentences Θ in $\mathscr{V}$ and Θ' in $\mathscr{V}'$, under some order of their vocabularies, represent the same pure structure $\hat{\mathfrak{A}}$. Then the models are reducts of a structure $\mathfrak{B}$ in $\mathscr{V} \cup \mathscr{V}'$ which is a model of both Θ and Θ'. But $\mathfrak{B}$ is also a model of identity claims between any two elements of $\mathscr{V}$ and $\mathscr{V}'$ that have the same position in the pure structure. In the vocabulary $\mathscr{V} \cup \mathscr{V}'$, each symbol of $\mathscr{V}$ can thus be used as the definiens of the symbol of the same position in $\mathscr{V}'$ and vice versa. These identifications therefore allow the definitional extension of either set of sentences to the other. Since explicit definitions are non-creative, neither set of sentences gains content by the addition of the definitions, and thus it is possible to go from, say, Θ to Θ' by first extending Θ through the definitions and then eliminating the $\mathscr{V}$-symbols from the resulting theory. The effect, of course, is a simple renaming of the symbols.

However, the semantic view is too liberal in its neutrality with respect to the vocabulary, for not every renaming makes sense. In the ideal gas law, for example, the translation should allow the renaming of 'températur' into 'temperature', but not into 'pression'. For syntactic approaches, this restriction on renamings can be expressed by introducing analytic sentences. Any renaming not entailed by analytic sentences is not allowed. Then, 'temperature' can be renamed as 'températur', but not as 'pression'.

There is another problem with arbitrary renaming, for it is not only too liberal in some respects, but also too restrictive in others. It does not, for example, avoid the language dependence referred to by Suppe (1974a, 204–205), who notes, relying on the medieval notion of 'proposition', that

> quantum theory can be formulated equivalently as wave mechanics or as matrix mechanics; whichever way it is formulated, it is the same theory, though its formulations as wave mechanics will constitute a collection of propositions which is different from the collection of propositions resulting from its formulations as matrix mechanics.

Obviously, the difference between matrix mechanics and wave mechanics goes beyond a mere renaming of the predicate-, function-, and object-symbols. And this is a problem for semantic approaches, because if more than the names of the extensions changes, the pure structures change as well. Hence when Hendry and Psillos (2007, 137, my emphasis) in connection with matrix and wave mechanics state that "a (semantic) model of one *could be turned* into a model of the other", they effectively point out that the classes of pure structures are not the same. Halvorson (2012, §4.2) gives much more precise examples.

The use of explicit definitions can again provide a solution to this dependence on language and structure. So far, I have used only the simplest kind of definition, an identification of symbols. But the comparison of two theories can be generalized to include any kind of explicit definitions. Two theories Θ, Θ' are *definitionally equivalent* if and only if both can be extended by explicit definitions such that they become equivalent. Then their models can be turned into each other by a procedure analogous to the one described above: First expand the model of one theory to include the defined symbols of the definitions. This expansion is unique. Then reduce the resulting interpretation to the vocabulary of the other theory (cf. Hodges 1993, 61). This allows the identification of theories that differ not only in the vocabulary they use, but also in the structures of their models. The procedure therefore also allows for structural differences in the description of theories, and thus can not only be used to extend syntactic approaches, but also semantic ones.[11] This move would also not obviously tether semantic approaches to syntactic ones because the notion of definitional equivalence can be defined without reference to sets of sentences (de Bouvère 1965).

4 Sentences, structures, and the world

A set Σ of sentences of predicate logic is not enough for applying a theory to the world, for if $\mathfrak{A} \vDash \Sigma$, any set of the same cardinality as A can be made into a model of Σ as well:

Claim 6. *Let* $|\mathfrak{A}| = A$ *and* $\mathrm{card}(A) = \mathrm{card}(B)$. *Then there is a* $\mathfrak{B} \simeq \mathfrak{A}$ *with* $|\mathfrak{B}| = B$.

[11] There is no obvious reason why the identification of theories could not be relaxed even further. One could, for example, identify theories Θ in $\mathscr{T}$ and Λ in $\mathscr{L}$ if and only if Θ can be definitionally extended to entail Λ and vice versa. Once this step is taken, one could consider two theories identical when they are mutually reducible in one or another sense of reducibility.

Proof. If $\text{card}(A) = \text{card}(B)$, then there exists a bijection $g : A \longrightarrow B$. For each relation $P_i^{\mathfrak{A}}$ in $\mathfrak{A}$, define $P_i^{\mathfrak{B}} := g(P_i^{\mathfrak{A}})$, for each function $f_j^{\mathfrak{A}}$ and each tuple $\bar{b} \in B^{n_j}$ (or arguments for functions of higher order), define $f_j^{\mathfrak{B}}(\bar{b}) = g(f_j^{\mathfrak{A}}(g^{-1}\bar{b}))$, and for each constant $c_k^{\mathfrak{A}}$, define $c_k^{\mathfrak{B}} := g(c_k^{\mathfrak{A}})$. It is straightforward to show that $\mathfrak{B} \simeq \mathfrak{A}$. □

Corollary 7. *Let* $\mathfrak{A} \vDash \Sigma$ *and* $\text{card}(A) = \text{card}(B)$. *Then there is a* $\mathfrak{B}$ *with* $|\mathfrak{B}| = B$ *and* $\mathfrak{B} \vDash \Sigma$.

Proof. From claim 6, because sets of sentences can determine structures at most up to isomorphism. □

Considering that any bijection between a set and the domain of $\mathfrak{A}$ will do for the proof of claim 6, it is clear that any element of A can be exchanged for any other element. In light of claim 7, it is clear that a set of sentences can be connected to the world (beyond statements about the number of its objects) only if there is a way to distinguish between isomorphic structures. One way of solving this problem is by introducing a set of *possible structures* $\mathbf{M}$, which is determined by the extensions of the $\mathscr{V}$-terms given their meaning. $\mathbf{M}$ allows Σ to make more than cardinality claims about the world by defining the *possible models* of Σ as the possible structures that are models of Σ. The use of possible structures does not have to trivialize syntactic approaches (by allowing all semantic tools): One can postulate that the set $\mathbf{M}$ of possible structures only distinguishes between isomorphic structures, so that the isomorphic closure of $\mathbf{M}$ is always the class of all $\mathscr{V}$-structures. Then it is easy to show that the isomorphic closure of the possible models of Σ is always the class of all models of Σ—in other words, in syntactic approaches, non-isomorphic structures have to be distinguished by sentences of the object language.

In the Received View, an interpretation maps the basic ("observational") vocabulary to extensions that contains objects of the world. This gives a general way of connecting syntactic descriptions to the world: The interpretations given by their possible models map at least one symbol to an extension that contains an object of the world—a *worldly extension*, as I will call it from now on. Interpretations with a worldly extension are accordingly *worldly interpretations*, and the analogous holds for models and structures.

If there are worldly structures, syntactic descriptions can be given a formal connection to the world by relying on worldly possible structures. These can be given by actually showing (pointing to) members of worldly sets and relying on the psychological fact of intersubjective agreements about similarity between experiences (Przełęcki 1969, 35–38). More commonly, the worldly sets are described in a metalanguage (thereby leaving out the question of how the terms of the metalanguage are connected to the world). As van Fraassen (1989, 222) puts it: "Any effective communication proceeds by language, except in those rare cases in which information can be conveyed by the immediate display of an object or happening". Either solution assumes that it is possible to build sets out of worldly objects. Since an interpretation maps to such sets, it is also assumed that it is possible to have a function from a vocabulary to worldly extensions. I will discuss the presumptions of these assumptions below.

The connection to the world has sometimes been claimed to be a strong point of semantic approaches, because they escape the problem of connecting linguistic entities to the world (Chakravartty 2001, 327). However, the connection between pure structures and the world is not completely straightforward either:

Corollary 8. *Let* $|\hat{\mathfrak{A}}| = A$ *and* $\mathrm{card}(A) = \mathrm{card}(B)$. *Then there is a* $\hat{\mathfrak{B}} \simeq \hat{\mathfrak{A}}$ *with* $|\hat{\mathfrak{B}}| = B$.

Proof. From claims 6 and 3. □

Thus just as syntactic approaches must have a means to distinguish between isomorphic structures, semantic approaches must have a means to distinguish between isomorphic pure structures. This suggests a more precise distinction between syntactic and semantic approaches:

Definition 3. *Syntactic approaches* describe theories with sentences in the object language and with structures. Non-isomorphic structures are only distinguished by sentences in the object language.

Definition 4. *Semantic approaches* describe theories with pure structures.

Semantic approaches thus distinguish between isomorphic and non-isomorphic pure structures by a (set theoretic) description of the pure structures.

How the connection between pure structures and the world is to be envisaged in semantic approaches depends on whether the pure structures are meant to be worldly or non-worldly. Suppe's statement quoted in section 3 that semantic approaches construe theories as the formal referents of the theories' formulations (see page 7) suggests that the pure structures used in semantic approaches contain worldly extensions, that is, are worldly themselves. Da Costa and French (2000, fn. 2) also seem to assume that the pure structures are worldly when they state that "the set-theoretic models are constructed in set theories with Urelemente (individual[s], systems, portions of the universe, real things,...)."

A pure structure has as one of its representatives a structure (with a vocabulary), and accordingly the existence of a worldly pure structure entails the existence of a worldly structure. This structure then provides the connection of the linguistic entities with the world. Therefore, if a semantic approach successfully connects theories to the world by using pure worldly structures, syntactic descriptions can be connected to the world as well.[12]

[12]For pure structures taken as indexed sets along the lines of Bell and Slomson (rather than equivalence classes of indexed sets), the modification of the discussion is straightforward: The index set I can be used directly as a vocabulary, providing the interpretation $a : I \longrightarrow a(I)$. If a specific vocabulary $\mathcal{V}$ is desirable, any isomorphism $g : \mathcal{V} \longrightarrow I$ leads to the interpretation $\mathcal{I} = a \circ g : \mathcal{V} \longrightarrow a(I)$. The discussions below can modified analogously.

Mostly, however, the pure structures discussed in semantic approaches are not supposed to be worldly (see, e. g., French and Ladyman 1999). They are simply abstract set theoretic entities—no dogs, observations, or electrons are members of the sets. Theses non-worldly pure structures then have to be connected to the world somehow. I will discuss four ways to do so.

In the first way, the theory described by pure structures is supposed to be isomorphic to some worldly pure structure.[13] But because of corollary 6, if the theory is about more than just the cardinality of its domain, there has to be one distinguished isomorphism (with an order isomorphism g between the index sets) or a set of distinguished isomorphisms that connect each non-worldly pure structure $\hat{\mathfrak{A}} = [\langle A, a, \prec\rangle]$ with those worldly pure structures that the theory structure is supposed to refer to. The result is again a worldly pure structure, since for each g and worldly pure structure $\hat{\mathfrak{A}}$, $[\langle A, a \circ g, g^{-1}(\prec)\rangle]$ is again a worldly pure structure, and thus the previous discussion applies.

As another way to connect a non-worldly pure structure to the world, Muller (2010) suggests taking a pure structure (which he takes to be a tuple), and allowing it to be assigned any compatible vocabulary. The vocabulary can hence be chosen according to expedience. The connection to the world is then given through an interpretation of the vocabulary. It is clear that this approach to connecting semantic descriptions to the world presumes that it is possible to connect syntactic descriptions to the world. Note also that Muller is basically describing a non-worldly pure structure, since the index set of the structure can be assigned any vocabulary, that is, any other index set. It is thus doubtful that Muller in fact provides a solution to the problem.

A third way to connect non-worldly pure structures to the world is to claim that the pure structure and the system it is meant to represent are similar. French and Ladyman (1999) argue successfully that the similarity relation, left largely unexplicated, is too vague to be of much use. Thus even if, within the boundaries of its vagueness, theories can be connected to the world by similarity, the success of this approach has been achieved only by significantly lowering the precision of the analysis. Assuming that both syntactic and semantic approaches aim for more precision, this solution therefore at best achieves a different goal.

French and Ladyman (1999, 115, cf. 113) argue instead that the connection to the world is simply not a problem to be solved by semantic approaches:

> The theoretical models [the pure structures of the theory] are held to relate to models of the phenomena and these are just other structures. That these represent real events and processes cannot be determined by the content of the theory, but is a pragmatic fact about our language [. . .] and it is unreasonable to demand that the semantic view explains the nature of representation in general.

[13] In semantic approaches, isomorphisms are often used as analogues to Tarski's definition of truth.

This clearly falls short of the idea that semantic approaches are *easier* to connect to the world than syntactic approaches. Without an account of the relation between theories and the world, French and Ladyman's stance also completes a terminological confusion: Because of their reliance on possible structures, syntactic approaches are semantic, and now, stripped of their connection to the world and relying on pure structures, semantic approaches stay on the level of the language of set theory, and hence are purely syntactic.

In French and Ladyman's approach, structures are simply set theoretic constructs, and thus their view is very close if not identical to that of the structuralists following Sneed (1971) and Stegmüller (1979), whose formal core commitment is to the expression of scientific theories in terms of set theory. In such a structuralist approach, there is simply one set of set theoretic sentences (those describing the phenomena) that are distinguished as representing real events. It is of historical interest to note that the idea to connect the sentences of a language to the world by determining their relations to a distinguished subset of the whole set of sentences of the language goes back to Neurath (1932), who postulated the translatability of all scientific sentences into protocol sentences (cf. Carnap 1932). If neither predicate logic nor set theory are given a formal connection to the world, they are simply languages that are assumed to describe the world in some not further specified way. Then, for example, the question of non-standard models does not even arise (neither for first order nor higher order logic, nor set theory) unless an additional metalanguage is artificially introduced (Väänänen 2001). If there is a metalanguage, however, set theory and higher order logic are equally expressive (Väänänen 2001, 506–507). The discussion in §2 about the possibilities to capture structures up to isomorphism therefore stacks the deck against syntactic approaches: It rests on the heroic assumption that set theory is not a language that describes the world, but *is* the world (more precisely: the world is a worldly structure), and the task of syntactic approaches is to describe the set theoretic world. If one instead treats, with French and Ladyman, set theory as just another language (which it is), it cannot describe the world more precisely than higher order logic.[14]

Without a direct relation to the world, structuralism's one plausible advantage is that it unifies scientific theories: Many mathematical concepts can be defined through the membership relation, which becomes the only non-logical mathematical constant. Many claims about the defined constants thereby become theorems of the definitional extension of the axioms of set theory. If furthermore non-mathematical concepts are also identified with set theoretical entities (e. g., time identified with the reals), whole scientific theories can be definitional extensions of the axioms of set theory. The difference between structuralism and syntactic approaches *excluding* the semantics of syntactic approaches then amounts to structuralism's restriction to set theory, and no such restriction on the side of

[14]Väänänen (2001) further argues that neither set theory nor higher order logic can describe the world more precisely than first order logic.

syntactic approaches. If a theory is formalized in structuralism, switching to a syntactic approach would thus allow the abstraction from all terms that do not originally occur in the formalized theory, including the membership relation. A structuralist formalization, on the other hand, would cease to be structuralist if one abstracted from the membership relation. In the terms of Hilbert (1900, 1092–1093), syntactic approaches apply the axiomatic method of formalization, while structuralism applies the genetic method.[15]

In conclusion, then, the connection to the world is equally problematic for syntactic and semantic approaches. And a solution for one approach, for example the pragmatic one suggested by van Fraassen (2006), also provides a solution for the other.[16]

5 Corollaries

I have so far argued directly against the position that syntactic approaches are untenable. I now want to discuss briefly why not all syntactic approaches must rely on first order logic or exhaustive axiomatization, use partial interpretations, or ignore the role of scientific models. These features are commonly associated with the Received View, but do not have to be presumed by syntactic approaches in general.

5.1 The use of higher order predicate logic

Second order logic is already enough "to capture *directly* most all mathematical practice" (Leivant 1994, 260; cf. Väänänen 2001, 515), and capturing mathematical practice only gets easier for higher orders. On the other hand, it is impossible to capture mathematical practice directly (that is, without significant reformulation) in first order logic (Leivant 1994, 279). It would thus be very problematic if syntactic approaches were in general restricted to first order logic. But this restriction is already historically inaccurate with respect to the Received View (Lutz 2012b, §2), which should be a hint that it is an inappropriate restriction for syntactic approaches in general. A possible justification of the restriction may be the absence of a complete proof theory in higher order logic, which leads to a loss of the nice features of first order logic described by Rantala (1978), and also makes it necessary to use model theory and thus structures when determining which statements are entailed by a set of sentences. However, there is no good

[15]This fits also with the claim of Sneed's adviser that "the basic methods appropriate for axiomatic studies in the empirical sciences are not metamathematical (and thus syntactical and semantical), but set-theoretical" (Suppes 1954, 244). Incidentally, in structuralism the genetic method is clearly understood as a reductive explication in the sense of Benacerraf (1965, III.B), since, for instance, no-one would assert that time *is* the set of reals.

[16]I have argued that solutions for semantic approaches are also solutions for syntactic approaches. The argument for opposite direction can rely on the fact that every structure represents a pure structure and, if needed, corollary 5.

reason to disallow the use of structures in a syntactic approach. As argued in section 4, syntactic approaches need possible worldly structures for making more than cardinality claims about the world. But the use of a set of worldly structures that is not closed under isomorphism is, if anything, more problematic than the use a set of non-worldly structures closed under isomorphism. Thus, given that model theory only needs such non-worldly structures, there seems to be no further problem for using entailment as a means of inference. As pointed out in §2, semantic entailment was also used in the Received View.

5.2 The impracticality of syntactic axiomatizations

Sometimes, syntactic approaches are considered to be unable or at least to cumbersome to capture mathematical practice because they are assumed to demand exhaustive axiomatizations, which require explicitly writing out all axioms for all terms that occur in a theory (cf. Stegmüller 1979, Suppes 1992). Semantic approaches, on the other hand, are taken to allow non-exhaustive axiomatizations. But of course, both an axiomatization in a syntactic approach and an axiomatization in a semantic approach can be non-exhaustive. Within the Received View, Carnap explicitly argues for the necessity of non-exhaustive axiomatizations, and also gives guidelines on which axioms to include and which steps of an inference to spell out in detail (Lutz 2012b, §3). (Carnap does insist that in principle, well-developed theories can be exhaustively axiomatized, but there is no reason that syntactic approaches have to insist even in principle on exhaustive axiomatization.) Conversely, a scientific theory may well be given in an exhaustive axiomatization of set theory like ZFC or an exhaustive axiomatization of model theory.

5.3 Models

Unlike semantic approaches, syntactic approaches are often considered to be inhospitable for scientific models (cf. Frigg and Hartmann 2008, §4.1). This view is typically based on, first, a confusion of the Received View with syntactic approaches in general, and second, a misunderstanding of the Received View (Lutz 2012b, §4). Here, I want to point out that semantic approaches cannot simply be assumed to incorporate scientific models on the basis that semantic approaches involve model theory and 'model theory' contains the word 'model': Scientific models like the liquid drop model of the atomic nucleus are, without reformulation, not the same as the structures of model theory.[17] With this ambiguity resolved, it becomes clear that syntactic approaches are as hospitable to models as semantic ones under *either* interpretation of 'model'. For if 'model' is understood model theoretically (that is, synonymously to 'structure'), then the possibility of using higher order logic allows capturing structures up to isomorphism (*modulo*

[17] The influential argument to the contrary by Suppes (1960) relies on reformulations of scientific models and the assumption that only the formal parts of models are important (cf. Lutz 2012b, 97–99).

an assumption about inaccessible cardinals), and the use of possible structures allows distinguishing isomorphic structures. Furthermore, semantic approaches relying on pure structures involve structures only indirectly, as representatives of pure structures (semantic approaches relying on indexed structures involve structures directly, with the index set as the vocabulary). If, on the other hand, 'model' refers to scientific models, then the question is whether semantic approaches can describe the world (or anything that is not a pure structure, for that matter) better than syntactic approaches. I have answered this question in the negative in §2 and §4.

5.4 Partial interpretations and correspondence rules

Suppe (1974a, 114) concludes that

> it is amply clear from the discussion of the observational-theoretical distinction and correspondence rules above that many of the epistemically relevant distinctions concerning theories cannot be drawn syntactically, and thus that the Received View's insistence on axiomatic canonical reformulation is untenable. Hence, if formalization is desirable in a philosophical analysis of theories, it must be of a semantic sort.

But Suppe's argument does not support his conclusion. For Suppe only criticizes the *Received View's* assumptions about the relation between theory and observation. And the partition of $\mathscr{V}$ into basic and an auxiliary terms, the direct interpretation of only the basic terms, and the interpretation of the auxiliary terms through the interpretation of the basic terms and the correspondence rules are specific to the Received View and obviously independent from the decision between syntactic and semantic approaches. For there is no reason why a syntactic approach cannot rely on possible structures that directly interpret *all* non-logical constants. As I have argued in §4, this does not trivialize the distinction between syntactic and semantic approaches. Historically, Feigl (1950, "personal postscript"), for example, did not endorse the view of a partial interpretation of the vocabulary of theories, even though he clearly was a proponent of syntactic approaches, and even something very close to the Received View.

Not only is Suppe's argument invalid, Suppe himself shows that his conclusion is also wrong: His criticism of correspondence rules (Suppe 1974a, II.E) is based on his presentation of the hierarchy leading from observations to theories developed by Suppes (1962). His description, however, is itself phrased in syntactic terms (cf. Suppe 1974a, 108, n. 225), thereby establishing the possibility of capturing Suppes's hierarchies syntactically. Similarly, there is no reason why, for example, van Fraassen's concept of embedding cannot be captured in syntactic terms. One attempt to this effect has been undertaken by Turney (1990), who introduces the concept of *implanting* as the syntactic counterpart to van Fraassen's *embedding*, that is, isomorphism to empirical substructures of a theory.[18] Finally, if the model or set theoretic formalism of semantic approaches does not receive a formal interpretation at all, the formalism already *is* syntactical, as I have argued in §4. Trivially, the relation between theory and observation can then be captured syntactically if it can be captured semantically.

That, conversely, the use of a subvocabulary is not restricted to syntactic approaches can be seen from the model theoretic concept of a reduct of a structure, in which all those extensions not belonging to the terms of a subvocabulary of $\mathscr{V}$ are eliminated (Hodges 1993, 9). The definition of a reduct for a structure can be extended to also cover pure structures by using the set of positions in a structure rather then the set of terms in a vocabulary. In fact, within a semantic approach, Suppes (1959) has suggested a criterion of meaning that relies on such a bipartition (cf. Lutz 2012a, §6.5).

There is, however, the possibility that the relation of a theory to the observations cannot be captured at all in any formalism, for example because the relation is achieved through completely implicit, contextual knowledge of the scientists. In this case, however, neither syntactic nor semantic approaches can capture the relation.

6 Combining semantic and syntactic approaches

In light of the discussion, semantic and syntactic approaches appear to be on a par with respect to their connection to the world, their language independence, and their treatment of models. In light of the inter-translatability of analyses based on pure structures with model theoretic analyses, and their inter-translatability with syntactic analyses, this is unsurprising. The role of model theoretic approaches as a mediator is interesting, and its close relation to both of the approaches may explain the conviction on both sides that little is amiss—in a syntactic approach, model theoretic results are easy to have through Tarski's definition of truth in a structure, and in semantic approaches, even a structure understood as a tuple can easily be fitted with a vocabulary, making available all the model theoretic concepts and results. Finally, without formal interpretation, either approach can be directly interpreted in the other—set theory can be formalized in predicate logic and *vice versa*.

These intertranslatability results are nothing but positive: They allow comparing more specific versions of the approaches, for example approaches in first order predicate logic and approaches in first order model theory. Here, standard model theory has already led to major results, which only had to be put to work, for example by Przełęcki (1969) and van Benthem (1982, 2011). Taking the close relation seriously also allows identifying a problem in either approach if the prob-

[18]Unfortunately, in his definitions of 'isomorphism' and 'embeddability' Turney (1990, def. 2, 5) allows a re-ordering of a structure's extensions, thereby making examples like that of Halvorson (2012, 192) and the translation of 'temperature' into 'pression' possible.

lem has already been identified in the other. In general, those problems of syntactic approaches that do not stem from formal interpretations of axiomatizations in first order logic have their analogues in semantic approaches, as the above examples of the connection to the world and language dependence show. Another case in point is the conclusion by Chakravartty (2001, 326, §1) that "[r]ealism on the semantic view is by no means impossible, but faced with precisely those familiar, perennial difficulties of reference and correspondence that some semanticists think their approach does without". As a final example, consider theories that cannot be fruitfully formalized. Typically, those theories are considered a problem for syntactic approaches (see, for example Suppe 1974a, 63 and Beatty 1980, appendix 1), but the above results show that if a theory is not fruitfully formalizable in a syntactic approach, neither is it so formalizable in a semantic one. More constructively, the results also show that if a theory is fruitfully formalizable semantically, it is also fruitfully formalizable syntactically.

A related constructive use of the close relation between the approaches is the transfer of solutions from one view to the other. One example is the definitional expansion of theories to allow the identification of theories with formally different structures in semantic views (§3). As noted, this goes far beyond the pure avoidance of assigning different names to the same sets. Other examples are the concept of a substructure in model theory, which captures the syntactic notion of a subvocabulary, the use of the Ramsey sentence by Sneed (1979), which mirrors its use in the Received View, and the syntactic description of empirical embeddings. With respect to the relation between the Received View and van Fraassen's conception of scientific theories, Turney (1990, 449) concludes:

> We see now that there is a syntactic method, which is equivalent to his semantic method. The moral is this: The relevant distinction here is not between syntax and semantics. [...] It is between two ways of linking theory and observation: Correspondence rules versus embedding/implanting.

Turney assumes in this quote that van Fraassen's notion of embedding cannot be captured by correspondence rules, but by his notion of implanting. Neither assumption is true (Lutz 2012a, §4.2.1, §4.2.3), but the importance of Turney's point is this: If the difference between syntactic and semantic approaches is seen as one of formulation, it is possible to search for commonalities between the views and to transfer solutions from one approach to the other. On a meta-level, I therefore do hold the position of the critics of syntactic approaches: The language in which an analysis is phrased, whether it uses pure or indexed structures, structures alone, or possible structures and an object language, matters very little.

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