Synthese

BETWEEN STRUCTURE AND FUNCTION: A SYNCRETIC, PRAGMATIST ACCOUNT OF SCIENTIFIC REPRESENTATION

--Manuscript Draft--

Manuscript Number:	
Full Title:	BETWEEN STRUCTURE AND FUNCTION: A SYNCRETIC, PRAGMATIST ACCOUNT OF SCIENTIFIC REPRESENTATION
Article Type:	Original Research
Keywords:	scientific model; scientific representation; structuralism; functionalism
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Funding Information:	Ministerio de Economía y Competitividad (FFI2016-76799-P) — Dr. Jose Diez Sociedad de Lógica, Metodología y Filosofía de la Ciencia en España — Dr. Jose Diez
Abstract:	The goal of this paper is to sketch a syncretic account of scientific modelling and representation that combines some elements of previous, approaches, disregarding other elements that are considered troublesome. The aim is to provide an analysis weak enough to be applied to the whole plethora of different models in empirical science, but strong enough to provide substantive answers to the main problems on scientific representation satisfying some relevant philosophical desiderata. The account is syncretic in being partially structuralist and partially functional. It integrates a weakened version of some structuralist ideas, and an essential functional component. In order for this integration to be possible, the account introduces some strong pragmatic elements in crucial steps.

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Abstract. The goal of this paper is to sketch a syncretic account of scientific modelling and representation that combines some elements of previous, approaches, disregarding other elements that are considered troublesome. The aim is to provide an analysis weak enough to be applied to the whole plethora of different models in empirical science, but strong enough to provide substantive answers to the main problems on scientific representation satisfying some relevant philosophical desiderata. The account is syncretic in being partially structuralist and partially functional. It integrates a weakened version of some structuralist ideas, and an essential functional component. In order for this integration to be possible, the account introduces some strong pragmatic elements in crucial steps.

Keywords: scientific model, scientific representation, structuralism, functionalism

1. Introduction

The goal of this paper is to sketch a syncretic account of scientific modelling and representation that combines some elements of previous approaches, disregarding other elements that are considered troublesome. The aim is to provide an analysis weak enough to be applied to the whole plethora of different models in empirical science, but strong enough to provide substantive answers to the main problems on scientific representation satisfying some relevant philosophical desiderata. The account is syncretic in being partially structuralist and partially functional. It integrates a weakened version of some structuralist ideas, and an essential functional component. Nevertheless, in order for this integration to be possible, the account introduces some strong pragmatic elements in crucial steps.

First, I start in this section with some caveats and listing a variety of examples representative of the different kinds of modelling the account aims at explicate. In section 2, I first characterize what I take to be the four main problems on scientific modelling and single out the three to which the account tries to provide a substantive answer; then I introduce the relevant desiderata to assess the different proposals and a general, six-ary grammar for modelling sentences. In section 3 I summarize structuralist and functional accounts and make a general balance of their pros and cons. In section 4 I present the main features of my account in two steps, first in a simplified version useful to express the main intuitions and I apply it to three of the previously characterized problems; secondly I assess it wrt the relevant

[1] Research for this work has been supported by the research projects…. . I want to thank …. for comments and criticisms to earlier versions of this paper.

desiderata, and, as a result of such assessment, I introduce some complexities a scond, and final, version of the analysis. I conclude with some final remarks comparing the present account *vis a vis* its main competitors.

The proposal presented here, its elaboration and its defence, heavily depend on the crucial distinction between the existence and the success of the representation. Besides its emphasis in such distinction, it does not aim to introduce radically new ideas, but rather to select and combine in a right manner elements of already existing accounts. The proposal, though, aims at being original and providing a valuable contribution in combining such elements in a novel way, preserving the correct insights of both structural and functional accounts without their shortcomings. It also aspires at having the virtue of being unified and substantive at the same time. That is, it does not withdraw towards straight pluralism, nor to explicatoriless minimalism, and proposes a substantive and enough unified account that is nevertheless free of clear counterexamples.

The object of this analysis is scientific modelling, but just in one of its arguably several varieties, namely the *representational use* of scientific models. Two caveats are in place. First, scientific models not only represent. Scientific models can be used to serve other goals. For instance, they may be designed just to test whether certain empirical hypothesis or theory is coherent. If one can build a model exemplifying such hypothesis/theory, then it follows that the hypothesis/theory is consistent. For instance, in Tobin (1970)'s ultra-Keynesian model changes in the money supply are a passive response to income changes generated, via the multiplier mechanism, by autonomous investment and government expenditure. The model, explicitly declared "not believe[d]" by Tobin (1970, 303), is designed to refute the claim attributed to Friedman according to which changes in money supply are the principal (/unique) cause of changes in money income. Though Friedman (1970) disagreed, what matters to us here is that the model has mere consistency/possibility goals (which is related to what in the literature on scientific explanation has often been called how-possible, in contrast with how-actual, explanations).

Other models may have a *merely* predictive function. That is, they are constructed to carry out relevant observational predictions in a given field but without the aim the non-observational components representing anything actually present in the phenomenon. Of course, merely predictive models are representational in a minimal sense, namely they represent the observable phenomena they want to deliver predictions about, but they do not aim at representing any non-observable element in the world. Some computer simulations (Humphreys 2004, 2010; Winsberg 2001, 2003, 2010) may be of this kind.

Arguably, some models are only *partially merely* predictive, i.e. besides the components that aim to correspond to observable predictable phenomena, some of the other components that play a role in the model aim to correspond to something else in the phenomena, but other components *also playing a role* do not. For instance, the Burridge-Knopoff (1967) block model of earthquakes is designed to match the empirically previously known Gutenberg-Richter law $N=10^{b-am}$ that correlates the magnitude m of an earthquake with the number N of earthquakes of equal or greater magnitude in a region/fault (a, b being region-dependent parameters, b close to 1). Although the model has some elements that aim at corresponding to something in the phenomenon, (e.g. the elasticity of the strings) there are others (such as distances between blocks) that are not aimed to correspond to relevant elements of the modelized phenomenon. Another example is the ancient Antikythera Mechanism model of the solar system (Carman and Evans 2014, Jones 2017), a computer machine constructed more than twenty centuries ago and discovered in 1900, with some components with clear representational function (e.g. the relative positions of the moon and inner and outer planets) but others with no aim to have a correspondence in the skies (e.g. the different amplitude of the 360 degrees in which the celestial orb is divided, or the pin/slot mechanism for the Moon). This issue is related to other common feature of many models, namely idealization, for idealized components of the models (perfect spheres, frictionless surfaces, …) do not aim to have an exact correspondence in reality. Nevertheless, both issues should in principle be distinguished, for it is in principle one thing not aiming at having *any* correspondence to reality and other, different thing not aiming at having an *exact* correspondence to reality (though one may propose an analysis of the former as a limiting case of the later).

And there might be other non-representational uses, for instance, some of the uses of models as mediators (cf. e.g. Morgan and Morrison 1999), or as epistemic artefacts (Knuuttila 2011), or as explanatory fictions (Bokulich 2009), or as tools for theory construction (Hartmann 1995) might be (partially) non-representational as well (cf. Peschard 2011 for not-only-representational uses of models). With respect this first caveat, our analysis will then be restricted to representational models, or to the representational components of models;[2] it does not aim to apply to the non-representational elements.

Second caveat: not only scientific models *stricto senso* represent. Other scientific objects (theories, engineering graphs, scientific images/drawings[3], geographical maps, …) represent; and other non-scientific objects as well (drawings, photographs, sculptures, non-scientific scale replicas, metro maps, linguistic sentences and others). With respect this second caveat, the analysis aims to apply to any scientific *representans*, so I will be using 'scientific model' in a wide sense as referring to any scientific construct with representational goals, scientific theories included. As for non-scientific representations, although the analysis might have some application to some of them, it does not aim to apply to non-scientific representations and must then be taken as confined to scientific cases. In particular, though some intuitions might be applicable also to pictures or languages, many of the details could not. The extent to which it might be extended, or modified, to cover non-scientific representations will not be discussed in this paper (nor will be discussed the demarcation problem of what makes a *representans* scientific, a merely sociological criteria will do for our present concerns).

After these caveats, let us just list some paradigmatic examples of scientific *representantia* that, without any aim of exhaustion, are representative of the main kinds of modelling one can found in scientific practice: the double-helix, and the triple-helix models of DNA; the ideal pendulum model; the kinetic theory of gases and its billiard ball analogy; the phlogiston and oxygen theories of combustion; Ptolemy's, Brahe's and Copernicus' models of the Solar system; Newton's and Einstein's theories of gravitation; the Lotka-Volterra predator-prey model; the above mentioned Burridge-Knopoff block model of earthquakes; the Bak-Sneppen model of species replacement; the computational model of the

[2] Models can also be used to explain, but according to us, the explanatory use is a sub-case of the representational use; that is, explanatory models are certain kind of representational models (for alleged non-representational yet explanatoy use of models see e.g. Bukolich (2009), Kennedy (2012); Bukolich draws his account on a general Woodwardian account of causal explanation (Woodward 2003), while I rather favor neo-expectability accounts (Díez 2014)).
[3] See e.g. Elkins (1999), Perini (2005, 2010) for the use of images and drawings in science.

mind; Thomson's, Rutherford's and Bohr's models of the atom; XIX century corpuscular and wave theories of light; the pipes-reservoirs-fluid Philips-Newlyn hydraulic model of economic dynamics; the numerical representation of magnitudes in Representational Measurement Theories; the scale Mississippi River Basin model; the scale aircraft wind-tunnel simulation; the use of Drosophila as a model of general genetic phenomena; the experiments with mice for modelling human pain or reactions to drugs; the Mercator and Gall-Peters two dimensional maps of continental land.

This is only a small sample of the vast plethora of models and representations in scientific practice. They cover a whole variety of scientific models/representations: material models; theoretical models; qualitative models; mathematical models; scale models; phenomenological models; analogical models; idealized models; reductive models; organism models (in the sense of Ankeny and Leonelli 2011); in vivo and in vitro models; etc. It is nowadays common to claim that the modeling activity in scientific practice is too varied to be analyzed by a single, unified notion, and that it is better to treat "model" as a polysemic concept whose different contents scape from a unified analysis and share, at most, a family resemblance. The (ambitious) goal of this paper is to resist this pluralistic tendency and present a unified account that applies to this plethora of scientific modellings explicating what they all have in common in an enough substantive manner that satisfies natural, but demanding, desiderata.

2. Settling the analysis

In this section I set the framework of the analysis by presenting what I take to be the four main problems of scientific representation to (some of) which the analysis has to provide an answer, the desiderata that it has to satisfy in providing such answers, and finally making explicit the many-ary hidden grammar of representational sentences.

Four problems

I start with the blunt first formulation of the problems and soon after I make some essential clarifications.[4] For this first version, I will use the simple sentence "M represents T", standing 'M' for 'the model' and 'T' for 'the target'. The four problems are the following:

(PD) Problem of directionality: *In virtue of what the direction of the representation is from M to T?*

(PE) Problem of existence: *In virtue of what M represents T?*

(PS) Problem of success: *In virtue of what, if M represents T, it does it successfully/adequately?*

(PO) Problem of ontology: *What kind of entities are models?*

[4] Different versions of these four problems are of course already present in the literature; this formulation is partially similar to the summary of the five problems on scientific representation mentioned by Frigg and Nguyen (2017), but with some essential differences, the main one the essential distinction made here of the problems of existence and of success.

Some important comments are in order. The leading idea that drives the formulation of the first three of these problems is that scientific modelling is analogous to speech acts. In speech acts, to use Austin's *motto*, we do things (asserting, inquiring, commanding, ….) with words. In (representational) modelling, we also do things, sometimes with words, but others with drawings, figures, symbols, material pieces or other items. The most important fact to take into consideration for this analogy, is that in speech acts we have to distinguish between Performance conditions (P-conditions) and Success conditions (S-conditions). Or alternatively, 'success', and 'failure', are ambiguously applied to speech acts: a speech act (an assertion, a command, …) may "succeed" or "fail" in the following two different senses:

In a first sense, we may succeed/fail in *performing* the act. And with respect to performance, mere will or intention does not suffice. Even if one has the intention to perform an assertion, if what she utters is e.g. 'Peter is between Mary and', or 'Lucy is taller than brotherhood', there is no assertion made. In order to perform an assertion, for instance, together with intention some other conditions must be met, uttering a grammatical and meaningful sentence to start with (and maybe other P-conditions, but we will not enter into this debate here). The same applies to other speech acts such as commanding, inquiring and others.

Once the P-conditions have been satisfied and the act has been properly performed, the speech act may succeed/fail in a second sense. It is constitutive of speech acts to have certain goal, in particular to obtain a specific response from the audience. In commands, to perform the behaviour commanded; in inquiries, to deliver the information requested; in asserting, e.g. to justifiably believe what is asserted (although there is lot of discussion about the goal of assertion, see e.g. Pagin 2007, but we will not discuss this here). In this second sense, the speech act succeeds/fails if, *once it has been correctly performed*, its constitutive goal is/is not actually reached. As the italics connote, the success in the first sense is a precondition for the success in the second sense (if, by chance or other alien reason, the audience performs the aimed behaviour without the speech act having been performed, this cannot be considered a success *of the speech act*).

The leading idea is that the same applies to modelling/representing. For the representation to be performed, certain conditions must be met (problems of directionality and existence above). And once the representation has been properly performed, additional conditions must be satisfied for the representation to be successful in the second sense (problem of success above). Models/representations also have certain goals constitutively associated to them, and the model succeeds in this second sense if its constitutive goal is satisfactorily reached. If one accepts this,[5] then this part of the analysis has to come in two subsequent steps. First, one has to provide an answer to PE, and then to PS including as precondition that the performance conditions have already been satisfied:

(Answer to PE) M represents T: ………….

(Answer to PS) M successfully represents T: (i) M represents T & (ii) …………

[5] One might not, for instance defending that no-representation simply is a limiting case of misrepresentation, that is that mere will suffices for representing and if one does it completely bad then we can talk of no-representation. If I understand them well, this is the stance taken by Chackravartty (2009) and Ducheyne (2008), for whom some degree of success is necessary for the existence of representation.

(or better: If M represents T, it does it successfully when: …………)

This distinction between the problem of existence and the problem of success will have a crucial role both in the account presented and defended, and in the assessment of other accounts.

As for the problem of directionality, the proposal is that it is a sub-problem of the problem of existence. That is, one of the clauses in (Answer to PE) must have as a consequence that the direction is from M to T. And, if as in our case directionality is considered necessary yet not sufficient for existence, the answer must include additional clauses:

(Answer to PE) M represents T: (i) [directionality] & (ii) ………….

With regard the problem of ontology (PO), as I will explain in detail later, the account will leave it partially open. The account does not commit itself to any particular ontology of models *but for the ontological constraints implied* by the answers to the other problems. As we will see, some ontological constraints will be so implied, but many other ontological aspects will be left open; in this regard, I will share French (2010) quietism, but only as much as the answers to the other problems allow me to do (for an overview on the ontology of models, see e.g. Frigg and Hartmann 2012; for a more recent and comprehensive one, Gelfert 2017).

Terminology

Before continuing, let me settle some terminology to clarify the (not completely innocent) use I will be doing of some crucial terms. First, I will use 'model-description' to refer to (if any) the linguistic items used by the modeller to describe the model or to instruct its material construction (if material model): ink-words in papers and text-books, lists of instructions, written equations, etc.[6] I say "if any" because some models may come without physical signals/symbols but directly described-instructed by the mind (whether we could then talk also of mind-linguistic items in an alleged *mentalese* language is a topic I will not enter here).

I will use 'model' to refer to what the model description describes or instructs, which is basically an *ensemble* of entities together with a *standing for* relation. There is a lot more to be said about them, and I will do that in the next sections. It suffices by now to say that, for instance, in material models such as the physically constructed double helix DNA model, the ensemble is the complex physical entity constructed. Or in a mathematical model the ensemble (or one subpart of it) is a certain mathematical function with some properties such as being over the real numbers, being differentiable, etc.[7] Material models (including,

[6] If I understand him well, this is similar to Frigg (2010)'s linguistic prompts in his fictionalist theory of models.

[7] These properties that constitute the model (e.g. being differentiable) that are properties of entities that also constitute the model (e.g. a specific function) make natural to distinguish between the model and its linguistic description. One could try to identify the model with types, or equivalence classes, of physical/mental descriptions. But it would be very unorthodox to reconstruct this saying, for instance, that differentiabilty, as a property instantiated by some functions and not by others, is an equivalent class of physical words that is instantiated by other equivalence class of physical symbols (thanks to Roman Frigg and Matteo Plebbani for emphasazing this point).

if one thinks they are so, computer simulations) may have particular practical or educational benefits, but there is nothing that makes them relevantly different to our present analytical concerns, which aims then to apply to material and non-material models alike. For instance, the double helix theoretical model of the DNA was materially constructed by Watson and Crick, while the triple helix was not; both have a model description that describes the model; in both cases we have, together with the model description, a model; but just one model was materially implemented as material model. Our account aims at applying to both cases alike, thus it abstracts from the material/immaterial differences.[8]

I will use 'target system' to refer to the phenomenon in nature that the model aims to represent/account for. Here we have to take a decision. One may individuate the target as (allegedly) constituted by the entities the model attributes to it. If we do this, in case a model attributes to the target an entity that actually does not exists (phlogiston, caloric, ether and other similar cases), then there is no target, the model is targetless (thus, strictly speaking, the model does not attribute the non-existent entity "to the target", since there is no target either). This is the way some (e.g. Frigg and Nguyen 2017) go, but I do not think this is the best talk. Not because the targetless problem and the fictionalist route that it could motivate, for one might instead for instance claim that in these cases there is no physical entity but there is an abstract entity (e.g. phlogiston as *abstracta*) that constitutes the target. The problem is rather that this forces us to use what I think is a counterintuitive talk. I take it that on the most common talk both phlogiston and oxygen models have the same target, they both represent (differently) combustion-calcination. The same in other cases: caloric and kinetic theories have the same target, they both represent heat phenomena; Newton's and Einstein's theories have the same target, they both represent gravitation phenmomena; ether and electromagnetic models have the same target, they both represent light; and so on and so forth. I think this is the most common talk that is worth to preserve and that the alternative option cannot preserve. In any event this is how I am going to use 'target'. On this use, then, the target is the *previously* identified and individuated natural phenomenon (by, say, features t1, .., tn), towards which the model is addressed.[9] Then, though phlogiston model attributes phlogiston to the target, and oxygen model attribute oxygen to the same target, neither phlogiston nor oxygen *constitute* or *individuate* the target. Thus, the model may attribute to the target elements that are not used in individuating the target (nevertheless, we will continue talking of the model attributing entities "to the target", while we should talk better of models "extending" the target with new entities). On this reading, only models *created for exclusively representing* the alleged structure of an alleged entity that does not exist, would be targetless. I do not think there are clear historical cases of this kind,[10] but in any event, I take as uncontroversial that, if any, they are the absolute minority and exceptions that our analysis may leave aside without regret (check our list above).

[8] This said, even in the case of material models, models must be taken as equivalence classes, on pain of implausibly claiming that two copies of the same model actually are two different models.

[9] It follows that as far as a target exists, its individuative features t1, …, tn also exist; how these features are themselves individuated (partially theoretically?, fully observationally?), is left here as an open issue.

[10] Maxwell's model of the ether seems to provide a case in point, but this depends of whether he was modelling "just the ether" without any connection to light phenomena, which is controversial. Other possible cases of targetless models are models such as a bridge model never constructed nor addressed to any specific bridge, or a 5-sex population model, or Norton's Dome, or others (thanks to James Nguyen and Roman Frigg for pointing this to me); I think they must be treated as totally non-representational (e.g. 5-sex model), and thereby out of the scope of our analysis; or partially non-representational, which will be discussed in the last section.

Finally, I will use 'model content' to talk of what the model expresses, the alleged state of affairs that according to the model exists in the world (for this it is crucial to bear in mind that not all the components of the models are intended to be projected to the world, more on this below). Note that I do not use 'refer' here, for it is one important feature of this account that when the model fails (e.g. phlogiston) there is no entity/thing in the world, different from the model, to which 'model content' refers. I'll discuss this at length in section 4, for the present introductory goals this provisional characterization suffices. Notice that, on this terminology, target and content never coincide. [11]

This suffices as clarification of my use of terms that are of general use in the literature on modelling and representation. Other more specific terms, introduced in the analysis, will be discussed later.

Desiderata

Any account of scientific modelling/representation must *prima facie* satisfy, at least, the following three desiderata (prima facie, that is, unless the account provides independent reasons for not doing so); these desiderata will be used in assessing the present account vis a vis alternative ones.

D1 Extension of the analysis. Although the times for analyses providing both necessary and strictly sufficient conditions has passed, the analysis must give necessary conditions that are "nearly sufficient", or at least "as sufficient as possible". This means that the analysis must provide conditions that, as necessary, exclude the cases of non-modelling; and as nearly sufficient, include any paradigmatic case of modelling; i.e. the account must not exclude/include *clear cases* of scientific representation/non-representation. In order to do that, these conditions must be the maximal set of conditions shared by all scientific models. It follows that given two alternative analyses, the strongest one is, ceteris paribus, the best.

D2 Revisionism of the analysis. The analysis must be the less revisionist possible. The common label "scientific model/representation" gives a prima facie reason (i.e. unless independent reasons are provided to the contrary) to look for an analysis as monistic/unified as possible. That is, confronted to extensional difficulties, the immediate reaction cannot be to withdraw towards pluralism. Thus, given two alternative analyses, the most unified is, ceteris paribus, better. On the other hand, the common use of the term 'misrepresentation' gives a prima facie reason to demand that the analysis makes room for unsuccessful representations; that is, for things that are correctly considered bona fide representations but that, nevertheless, do not reach the intended goal.

[11] One could allow model and target to *sometimes* coincide if (contrary to what I have done) one identifies the target system with the phenomenon plus what "the true model" attributes to the world behind the phenomena (thanks to A. Paco and A. Solé for the suggestion). Then the target would be the content of "the correct model", whichever it is. For instance, supposing for the sake of the example that oxygen theory is "the truth" about combustion, phlogiston and oxygen theories would have the same target, namely the real combustion-oxygen systems. In this sense, content and target would coincide for the true models and would not in all others. This would also preserve the above mentioned worth to preserve talk that different models of the same phenomenon have the same target; and wrong models that posit non-existent entities would not be targetless. This option has then the same good consequences that mine has, but given uncertainty about truth we would never know what the targets are, which I find counterintuitive. The only thing we could say with certainty is that the target is the phenomenon "together with whatever is behind it", which I do not think is very illuminating, nor that it provides any benefit that my own, simpler talk does not.

D3 Intension of the analysis. The analysis must be as substantive/elucidatory as possible. In particular, it has to explicate: in virtue of what models succeed (when they do); in virtue of what they fail (when they do), including the different kinds of misrepresentations (if there are several); and in virtue of what successful models allow for "surrogative reasoning" (term first coined by Swoyer 1991), that is, reasoning about the target from information obtained in the model. Some (e.g. minimalists such as Suárez 2004, 2015) may object to this substantive condition, but independent reasons should be argued; minimalism, as pluralism, should not be the first reaction to extensional problems.

As any multifactorial constraint, some desiderata may go against others, so they need to be balanced, and the best analysis is the one that balances them best (although which balance is best is of course disputable).

Grammar for representational sentences

I have been using for the sake of simplification a binary grammar for representation/modelling sentences: "M represents T". Nevertheless, as many have emphasized (prominently Giere 2004, 2010, and van Fraassen 2008) this is an incorrect, and dangerous, simplification. In the account offered here, the correct, hidden grammar of these sentences is not binary but six-ary. Together with the model M and the target T, the following parameters are (many times only implicitly) also present:[12]

Subject S. Representations do not obtain *in abstracto*, they are the result of a scientific practice performed by particular individuals or communities. We use the variable S to refer to the relevant (individual or collective) subject that performs the representation, i.e. for the *representator*: "S uses M to represent T". As will become apparent later, the subject will be determinant in answering the problems of directionality and existence.

Respects R. Given a subject, a model and a target, the assessment of the model as successful or adequate often depends on certain respects, the features/properties of the model that are taken as relevant for the representation. As the bidimensional continental maps example above exemplifies, the same model may be adequate taking into consideration some of its respects or properties but not others. The Mercator model is adequate with respect the shape of the land, but not with respect the area dimension; while for the Gall-Peters the opposite is the case. It is then relative to some of its respects/properties that are taken into consideration, and not others, that a model is assessed: "S uses M to represent T in respects R (of M)".

Purposes P. Respects come with purposes. If the shape-respect of Mercator map is relevant for assessing it, it is so relative to certain purposes, in this case obtaining faithful information about continents real shape; and analogously for the Gall-Peters map and the purpose of obtaining faithful information about continental land area dimensions. And, as it happens in

[12] One could argue that the binary grammar is fine but that the truth conditions involve more than M and T (thanks to Roman Frigg for pointing this to me). I think that then the issue is partially conventional: once we accept that the truth conditions depend on more than two parameters, we can convene in showing this in the grammatical form of representational sentences or not. I think it the former option is better. This is of course compatible with using sometimes, when the context makes clear the implicit form, the simpler binary formulation for the sake of simplicity.

this case of bidimensional continental maps, it may very well be that there is no model adequate for all purposes: If a bidemensional map correctly represents continental shape, it does not correctly represent area dimensions; and the other way around. "S uses M to represent T in respects R for purposes P".

Context C. Finally, the context of use C is determinant for the assessment. Representations are not all/nothing successful; success comes in degrees. Given the same subject, model, target, respects and purposes, a very stringent context may assess the model as unsuccessful if the degree or level of accuracy in which the relevant respects satisfy the purposes is below the demanding limit considered desirable by the context; while in other, less demanding context, the same model may be regarded as enough adequate in the same respects for the same purposes. Context C is then essential in determining the degree of accuracy (and maybe other factors) for the assessment: "S uses M in C to represent T in respects R for purposes P".

Sometimes we can talk as if context C included R and P as well; that is, for the sake of simplification one may talk of the context as determining, together with degree of accuracy, respects and purposes, simply using expressions like "S uses M in C to represent T". Or context may even be taken as implicitly determining the subject ("M represents T in C"). But these are just simplified talks that must not make us forget the hidden six-ary grammatical form. As a last point with regard the grammar, some might feel that an additional parameter is lacking, for as some have suggested (van Fraassen 2008, Elgin 2010), it is essential in representation that the target is *represented-as* something (for a summary of representing-as accounts, see Frigg and Nguyen 2017). I do not include it for I think that the same relevant features may be captured with respects, purposes and context (more in this below).

3. A syncretic account: inherentisms and functionalisms

As I announced, the account presented here shares some intuitions with other existing accounts, although it elaborates the details differently and, more importantly, selects some elements while disregards others in order to be able to integrate altogether in a coherent manner. I group these referred accounts in two main families, which I label "inherentisms" and "functionalisms" connoting what I take to be the main features that characterize them.[13] I summarize them and what I take to be their main pros and cons; and single out, in each case, the elements worth to preserve.

Inherentist accounts

In this family of accounts, the main ingredient in the analysis of scientific representation has to do with the inner natures/structures of model and target and how they relate to each other. According to these accounts, it is in virtue of inherent features of model and target, and their relations, that the representation obtains. I intentionally use the ambiguous expression "representation obtains", without distinguishing between existence

[13] These families correspond, roughly, to what Chakravartty (2009) label "informational" and "functional" approaches; for reasons that become apparent soon, I prefer "inherentist" to "informational". Chakrvartty also claims that these approaches are not incompatible, but does not elaborate a unifying alternative.

and success of the representation, since most of these accounts do not make this distinction explicitly. I take it that, at least in most accounts, the mentioned inherent ingredient is implicitly aimed to analyze the success of the representation, not its existence. Although they typically use phrases like "the model represents the target if and only if the relation … obtains between M and T", I think that they are implicitly referring to the success of the representation, not to the success in having performed a (maybe completely wrong) representation. Or, in any event, they should, for although they usually do not operate explicitly with this distinction, their analysis has some plausibility only as a condition for success (since, if the relevant relation does not apply, then what we have is a misrepresentation, that is, an existing yet unsuccessful representation).

As for the nature of the relation between inherent aspects of model and target that explicates representation, different members of the family propose different relations. We can subgroup these different proposals in two subfamilies: (informal) similarity accounts and structuralist accounts (I qualify 'informal' since all inherentisms belong to the similarity account in a broad sense of "similarity"; I will drop the qualification here after).

According to similarity accounts, prominently defended by Giere (1998, 2004, 2010) and Weisberg (2013) (see e.g. Teller 2001, Suárez 2003, Toon 2012, Frigg and Nguyen 2017 for a discussion), the relevant relation for a representation between M and T is *similarity*. The proposal provides the tools for explicating surrogative reasoning since, if M is similar to T, one can in principle transfer information gathered from M to the similar features in T. And it has the virtue of being extremely flexible so that it may include all kinds of scientific (and non-scientific) representations. But, according to many, this virtue is also its weakness. Interpreted as a proposal about the existence of representation, it would face principled *logical* problems, since similarity is (in whatever construal) a reflexive and symmetric relation, but representation is not (Goodman 1976, Frigg 2003, Suárez 2003) - though it is not ireflexive and asymmetric either.[14] On this interpretation, it would also face a *triviality* problem since everything is similar to everything else in some respects, thus everything would represent everything else. This is so even if we construe similarity as the relation of sharing some property, for, if we do not impose any further constraint, any two things share some property (actually many properties: being a thing, being different to another thing, …).

One can try to avoid triviality adding respects and degrees of similarity, so that, given some respects and degrees, it is now not the case that anything is similar to any other thing in those respects and degrees. This is correct but, although proponents are not always clear, the inclusion of respects and degrees are more natural demands for the success of the representation rather than for its existence. This seems the route taken by Giere himself, who now introduces subjects/agents (and purposes) and makes subject's intentions crucial for the existence of the representation, defending a complex grammar of representation sentences (Giere 2004, 2010): "S intends to use M to represent T for purposes P (with certain respects and degrees)" (see also Maki 2009 for related concerns).

If one interprets similarity as a condition for success and not for existence,[15] then existence (and directionality) is explicated by subject's intentions, and the alleged logical

[14] Yaghmaie (2012), who takes it as an account of the existence of representation, gives examples of reflexive and symmetric representations, but this simply shows that representation is not ireflexive and asymmetric either, not that it is reflexive and symmetric.

[15] In the similarity camp, Ducheyne (2008) denies this distinction, for he takes some degree of success as a condition for existence.

problems vanish. Whether triviality also vanishes depends on whether we impose additional conditions, together with intentionality, for existence. If we do not, it seems that triviality of existence prevails, since everything could represent anything else just depending on subject's intentions, a situation that some find desirable.[16] I (for the reasons given in section 1 based on the analogy between representations as speech acts) do not, and my account will impose additional conditions for existence. As for success, similarity (in the relevant respects-degrees and for the relevant purposes) now imposes the condition for a given representation -which is a representation independently of similarity- to be successful. This is on the track of what I think is correct, but too weak, or too unspecific. Without the respects-degrees-purposes addendum, it trivializes success, since any representation would be successful for any M and T are similar *in something*. With such addendum, it does not trivialize success, but these parameters are completely determined by the pragmatic context; if the account does not impose any other parameter or condition for success independent from context, it actually says nothing but simply that the context will tell whether a given representation is successful or not, which has almost no, or very little, elucidatory value. My account will impose some constraints that, though still leaving an important role to the pragmatic context, do not leave everything to it.

Structuralist accounts can be regarded as a formal manner of avoiding the unspecifity problems of (informal) similarity accounts by imposing precise formal constraints to the similarity relation. There is a whole plethora of formal relations offered as candidates for this precisification. All are algebraic in nature and thus presuppose an "algebraization" of model and target as relata of the relation; that is, all take model and target as being algebraic structures composed by domain universes and relations and functions over them (or at least "re-describable" as such structures). Some of these relations are also reflexive and symmetric, and some others are also transitive, thus object of the same, or stronger, logical problems criticism *read as conditions for existence* (Frigg 2003, Suárez 2003). For the reasons given above, I do not think these criticisms are well addressed since these relations are not, or at least should not, be presented as conditions for existence but for success. Although structuralists are not always clear in this regard and sometimes the way they talk may cause confusion, the criticism must in any event be answered by distinguishing both kinds of conditions and making explicit that the formal relation is a condition for success, not for existence. Then, this alleged logical problem should not have *any* relevance in choosing the appropriate relation at all; that is, if one does not choose a reflexive/symmetric/transitive relation, one must do so for reasons completely independent of the existence of the representation.

The strongest relation, and very likely the one that first comes to one's mind, is *isomorphism* (or *isomorphic embedding*) between model and target systems (van Fraassen 1980). Since isomorphism between model and target systems as a whole is too strong in many cases, alternative accounts propose alternative relations that weaken isomorphism in the regards considered too strong for the cases in point. Suppes (1970, 1974, 1989, see also Adams 1959) proposes homomorphism in a substructure. Developed after Suppes' track, Sneedian structuralism (Sneed 1971, Balzer, Moulines and Sneed 1987, Díez 1998), proposes

[16] Callender and Cohen (2006) defend this hyper liberal view. Actually, in their case (and contrary to others), not only about existence but also about success, which according to them is only constrained by pragmatic factors not by conceptual constraints.

structural embedding, similar to isomorphism to a substructure. In Mundy (1986), the relation is *faithful homomorphism* or *dihomomorphism*, roughly a homomorphism in both directions. For Swoyer (1991), the relation is the one he calls Δ/Ψ-*morphism*; basically, there is at least one relation in at least one of the systems that is homomorphically projected to the other system. Ibarra and Mormann (1997) talk of *structure preserving maps*. In the partial structures approach (Bueno 1997, Bueno, French and Ladyman 2002, Da Costa and French 2003, Bueno and French 2011, French 2014), the relevant relation is *partial isomorphism*. Van Fraassen (2008) refers to *selective resemblance*, which, as its name connotes, is a kind morphism between selected parts of the systems (see Thomson-Jones 2011 and Pero and Suárez 2016 for review of some of these different mappings).

All these proposals have virtues, to start with their precision and specificity. And each structural relation accounts very well for success in the cases it applies to, since success in some cases is due to partial isomorphism, in others due to homomorphism, etc. For the same reason, each one accounts well for surrogatory reasoning when the obtaining structural relation is the one that the account demands: each one makes understandable that, if the relation obtains, we can transfer information from the model to the target. Nevertheless, none is satisfactory as a general account of success in *all* scientific models.

One problem is related to distortion and idealization, ubiquitous in scientific modelling and something that, in strict, simple versions of these proposals do not seem to make room for. I do not think that this is fatal problem; algebra has in principle the resources to "blur" these relations in the respects necessary for accommodating distortion an idealization.[17] Other problem has to do with the algebraic elaboration of the proposals. According to them, in order for the relation to be able to obtain between model and target, model and target must be algebraic (set-theoretical, or the like) structures. Yet, there are several reasons why, if we want to account for real life scientific representations, it is at least uneasy to claim that target systems in the world are algebraic structures (Frigg 2003). Nevertheless, I do not think that this problem is fatal either. Some options are in place (see Frigg and Nguyen 2017, 2017c for a discussion): that though systems are not structures, structures can naturally be ascribed to them; or that systems are *describable* as structures for the sake of representation; or that systems instantiate formal structures; or, ontologically more committed, to bite the bullet and claim that systems in the world actually are algebraic structures. Every option has pros and cons and none is completely innocent and calls for some reform, but I think this does not rule out structuralist accounts in principle.

What I take to be the main problem of structuralist accounts has to do with the first desideratum, namely extensional adequacy. Isomorphism seems to work in modellings in which each element of the model corresponds to one and only one element of the target (or better, partial isomorphism, for there are elements of the model and target that may not have correspondence in the other system). But in other cases it is too stringent. In representational measurement, for instance, we do not have isomorphic but homomorphic relations; this extends to explanatory representation of data systems when we attribute to them magnitudes that may have the same value for different objects. On the other hand, if the relation in place demands the two systems to be homologous, we will have troubles for instance with theoretical representations, in which the theoretical model is richer than the data system/target; and with reductive representations as well, in cases in which one component of the reduced systems is represented as a combination of different components of the

[17] See, e.g. Balzer et al (1987) ch. VII for an example.

reducing systems. If we weaken these three aspects, we obtain something similar to Swoyer's account (Suppes' and Mundy's imply homologous systems). But this seems too weak, to start with since it has the unwilling consequence that if A successfully represents B then any extension of A also successfully represents any extension of B (Díez 1998). The moral I think one must extract is that specifying any *concrete* structural relation as the one that applies all across scientific modelling is going to be either too strong or too week. Only the pragmatic context may determine which particular relation is demanded in each case (Bueno and Colyvan 2011 make a similar point). Yet, discussing similarity accounts above, I have just said that leaving to pragmatic determination all the constraining parameters of the analysis makes it elucidatorily useless. The account presented here will include a condition that, on the one hand, cannot be trivially satisfied, and on the other, imposes conceptual constraints leaving open some parameters that the context must determine.

Finally, interpreted, as I have argued they should, as proposals about the success of the representation, structuralist accounts simply get silent about the problem of existence (and directionality). There is nothing in structuralism *as such* that gives any clue about in virtue of what something represents (maybe unsuccessfully) something else. Of course, structuralist accounts may be supplemented with additional elements to this effect, but if they do so they will do it independently of structuralist considerations. In this regard, though structuralist do well explicating success and surrogative reasoning, they fall short in elucidating in what representing (including misrepresenting) consists of.

Functional accounts

In functional accounts, the main ingredient in the analysis of scientific representation has to do with the function that the model performs with respect to the target. According to them, it is in virtue of the model facilitating such a function that the representation obtains. I, again, intentionally use the ambiguous expression "representation obtains", without distinguishing between existence and success of the representation, since most of these accounts do not make this distinction explicit either. Although some authors seem to imply that the functional condition characterizes existence and not success, I argue that one must also distinguish here between existence and success, the former in terms of performing the function (either correctly or incorrectly), the latter in terms of performing it successfully.

As in inherentism, we also find different functionalist accounts with different proposals about the function that the model allows to carry over the target. All these proposals, though, share the intuition that the relevant function in (representational) scientific modelling has to do, in one sense or other, with *transferring information* from the model to the target; this is what we called above, following Swoyer, surrogative reasoning. Some representatives of this family are Hughes (1997, 2010), Ibarra and Mormann (1997, 2000), Suárez (2004, 2010, 2015), Contessa (2007, 2011) and Ducheyne (2012); and also, to some extent, some advocates of the representation-as account, e.g. Elgin (2010), van Fraassen (2008), Frigg and Nguyen (2017, 2017b); and some fictionalist accounts as well, such as Frigg's (2010) relevant fictional information account. To our present goals, it will suffice to briefly comment some aspects of Hughes', Suárez's and Contessa's accounts (Frigg and Nguyen' DEKI account will be discussed later).

Hughes presents his DDI (Denotation-Demonstration-Interpretation) account as a "modest" one, in the sense that it does not aim at providing an analysis in a strong sense but just to call attention to three activities involved in scientific representation that, "if we examine a theoretical model with these three activities in mind, we shall achieve some insight into the kind of representation that it provides" (Hughes 1997, 339); the reference to them as "activities" connotes the presence of a subject that performs them. By denotation, a model is addressed to a target. In demonstration, we examine the *internal dynamics* of the model system and demonstrate/infer certain information about it. Finally, we interpret this information gathered from the model as applicable to the target: "the conclusions demonstrated within the model have to be interpreted in terms of its subject" (ibd, 332). Notice that this is simply witnessing that modelling involves surrogative reasoning, yet without providing any specific elucidation of in virtue of what this is so.

Suárez proposes his inferentialist account from an explicitly acknowledged deflationist, or minimalist, perspective (see Suárez 2015 for his discussion of such deflationism): first, he offers necessary conditions that do not aim at being sufficient, or even nearly sufficient; secondly, these conditions are offered as platitudes with no intended deep explicatory role. According to the account, "[Inf] *A* represents *B* only if (i) the representational force of *A* points towards *B*, and (ii) *A* allows competent and informed agents to draw specific inferences regarding *B*." (2004, 773). Suárez implicitly considers [Inf] as giving conditions for the existence of the representation, for explicitly claims that [Inf] makes room for misrepresentation: "(ii) … demands that we correctly draw inferences from the source about the target, but it does not demand that the conclusions of these inferences be all true" (776). There may then be misrepresentations satisfying (i) and (ii); that is, non-successful representations must satisfy (i) and (ii) as successful ones do, which means that (i) and (ii) are conditions of existence, not of success (success that will depend on the truth/correctness of the inferred conclusions). Nevertheless, *taking them alone* as conditions for existence they seem too weak, too insufficient. The reason is that, once we have satisfied (i), i.e. once we actually have that A representationally-points towards B, (ii) seems to exclude very little, if something: every system A allows competent agents to draw *some* inference regarding any other system B. Of course, arguably, the account implicitly refers to some relevant kind of inferences, but, *if nothing else is added*, nothing rule trivial/irrelevant inferences out. It is true that, since [Inf] does not want to provide sufficient conditions, from the weakness of (ii) it does not follow that all these unwanted cases count as representations simply by satisfying (ii). But even if not aimed as sufficient, or even as nearly sufficient, a necessary condition that rules out so little, or nothing, is of little aid; it seems simply saying that models involve surrogatory reasoning, which is true, but was also our *explicandum* not then an acceptable *explicans* (see Contessa 2011 for a similar criticism).

With regard elucidatory power, then, clause (ii) does not provide much if, as it happens, it does not tell more about the kind of inferences considered relevant. It does not provide explication in other respect either, namely, it does not elucidate in virtue of what these inferences can be drawn, when they can. And clause (i) suffers from a similar lack, for as stated it simply says that when A represents B, the representational force of A points towards B, but gets silent about in virtue of what this is so. Nevertheless, with regard to the lack elucidatory power, deflationist inferentialim such as the one defended by Suárez may remain unaffected, for such lack is an admitted consequence of deflationism/minimalism. The platitudinous character of [Inf] is accepted from the start. Nevertheless, inferentialism and deflationism need not go together, and non-deflationists inferentialists (and non-deflationists in general) should worry about this lack of elucidatory power.

Actually, Suárez himself adds something that tends to mitigate this feeling. He distinguishes between the *constituents* of representation, that define the concept and must be given in the analysis - in his case exhausted by [Inf] (i) and (ii) -, and the *means* of representation, which are the particular means (similarity, isomorphism, …) by which the inference is drawn in particular occasions. And, importantly, there cannot be representation without M and T instantiating some particular properties and standing in certain relation (Suárez and Solé 2006, Suárez 2015). This seems to suggest that even radical minimalist inferentialism acknowledges that some inherent features of M and T, and their relation (maybe different relations in different cases), are always relevant for representation as providing the basis for the inferential function. This, I think, points to a correct syncretic direction, even if Suárez considers -according to me wrongly- that this element must be kept out of the analysis itself.

Contessa, drawing on Hughes and Suárez, proposes an *interpretational* conception that, contrary to Suarez's, aims to be a "substantive account" (Contessa 2007, 48). According to this account, M represents T for a user if and only if "the user adopts an interpretation of the vehicle in terms of the target" (61). Here 'interpretation' is a technical term that, in case of what he labels *analytical* interpretation, refers to the following conditions (58): the user takes M to denote T; the user takes every (relevant) object, every (relevant) n-ary relation and every (relevant) n-ary function in M to denote (respectively) one and only one object, n-ary relation and n-ary function in T; every object, every n-ary relation and every n-ary function in T is denoted (respectively) by one and only one object, n-ary relation, n-ary function in M. Although he thinks that other, non-analytic interpretations are possible, he claims that they would be the exception and remains silent about them. Notice that this conditions -that he exemplifies with the London Underground network- are similar to the formal pre-conditions for an isomorphism, demanding a one-to- one correspondence between objects, relations and functions (preserving logical category); but it does not demand the isomorphism condition that if two objects are R-related in M their images are image-R-related in T (and the other way around). The conditions simply amount to the existence of a correspondence-for-the-user between parts of M and parts of T, without requiring any additional "similar behavior" condition. Contessa introduces isomorphism-like conditions when, talking of surrogatory reasoning, he claims that an analytic interpretation "underlines the following inferences" (61), roughly: if n objects in M are related by a relation or a function in M, then "it is valid for the user to infer" that the corresponding objects in T are related by the corresponding relation or the corresponding function in T; in this regard, I think that Contessa makes a first, though only partial, step in the right syncretic direction.

Balance

I believe that both inherentist and functional accounts contain a grain of truth, but also that so stated they fail. Inherentisms are right in that some "same behavior" relation (similarity, partial isomorphism, faithful homomorphism, …) is always involved in successful modelling, and it grounds, and explicates, correct surrogatory reasoning. But any specific version proposing a *concrete* relation necessary for all models is going to be either too strong or too weak. This is so reading inherentisms, as I defend it should, as proposals for success, not for existence. With regard existence, the most that some inherentists do is to make it dependent on the intentions of the subject, but if our analogy to speech acts above is correct, mere will does not suffice for existence and additional conditions must be sought. Functionalisms, on the other side, are correct in that the facilitation of an epistemic/informational function is essential for the existence of the representation, and that success in modelling/representing is success in reaching the goals of the relevant function. But without explicating in virtue of what the function may be performed, and successfully performed when it is so performed, the analysis lacks the necessary elucidatory power. Adding further constraints with regard constituents of M and T, and their relation, points to what I believe is the correct syncretic direction, but much work remains to be done.

The above assessment of these accounts suggests a syncretic way of preserving their grain of truth without their shortcomings. First, existence must be explicated by subject's intentions together with some additional condition that, on the one hand, is analogous to syntactic-and-semantic meaningfulness for assertions, and on the other, explicates the facilitation of an informational function. Second, success must be elucidated in terms a relevant similarity relation that, on the one hand, may change from context to context, and on the other, grounds in each case that the transfer of information has been correctly done. I contend that the account sketched in the next section meets these desiderata.

4. The ensemble-plus-standing-for account (EPSF)

Some EPSF principles

EPSF starts from the five following intuitions (that may be traced back to Wittgenestein's *Tractatus* –Tr-, though substantially amended, modified or complemented later):

(i) Models/representations (*Bilder*, in Tr) are "interpreted facts". That is: (i-a) Models are facts, *ensembles* of entities behaving/ensembled to each other in specific way (Tr 2.14). For instance –simplified-: two strings of colored wood pieces joined by wire helically rolled towards each other; or a particular binary function assigning to two unextended points a numerical value. And (i-b), models are facts whose parts *stand for* entities in the world (Tr 2.13). For instance, the pieces for molecules, the three color properties for chemical properties, the wire pieces for chemical bonds, the macro spatial torsion for a micro spatial torsion; or, the function stands for the gravitational force, the points for the Sun and the Earth, the numerical value for the intensity of the gravitational attraction.[18] [19]

(ii) The existence of the representation requires some logical/categorical-congruence constraints (Tr 2.18). (For instance, one cannot represent, *not even falsely,* a triadic relational state of affairs by a dyadic relational fact).

(iii) What the model represents, its content, is that the entities in the world for which the constituents of the model stand, behave to each other as the constituents of the model do (Tr 2.15).

[18] Most of the times the ensemble and the standing for relation are created or presented simultaneously, but sometimes the ensemble may be presented first and after the standing for relation "plugged" into it, nothing hinges on this.

[19] If models, mathematical or material, are types of ensembles, then its parts are also types; and may sound unnatural to tell that the relation applies to types. I think one can go both ways, applying it to type-constituents, or to token representatives of the types. Although they might have different consequences for the ontology of models, nothing in what follows hinges on the elaboration of these details, which I would not pursue here.

(iv) If the entities in the world behave so to each other, the model is successful/correct/true, otherwise it is unsuccessful/incorrect/false (Tr 2.21; of course this must, and will, be modified for the analysis to make room for successful yet inexact models).

(v) The "representational relation" of the whole model, and the "standing for relation" of its parts, must be neatly distinguished. *And* the global representation relation reduces to the ensemble plus the standing for relation (Tr 3.12). The representation relation *consists* in the ensemble having its parts standing for other entities. Likewise for model-content: once an ensemble has been projected through the standing for relation, this expresses the content of the model; once the world contains an ensemble and a standing for relation on it, it does not contain *in addition* the content of the model. If the model is true, the world contains in addition other fact; but if the model is false, the world contains no other thing at all.

I take that these ideas contain the inherentist part of the core of a core of a correct conception of scientific[20] modelling/representation. In order to develop such a conception from them, though, they must be elaborated in combination with other elements (many of which would have been surely denied by the author of Tr [21]). In particular, I am going to introduce some strong pragmatic elements that he would have for sure rejected. Be this as it may, I think that these five ideas, adequately pragmatized, provide the correct starting point for developing an account the desiderata summarized at the end of the last section. Let us see these other elements.[22]

(vi) Contextual simplicity-in-use. In Tr, the entities constituents of the picture (individuals, properties, relations) are really "simples". One of the reasons why it is not possible to give real life examples of models that meet the full Tractarian account, is that real life models are not like this. In real life, including scientific models, the constituents of the model are not "simples" (in case there were such things, it would be highly implausible that we had direct access to them for constructing models with them[23]). Our models have parts, but such parts are also complexes (for instance a piece of wood). Nevertheless, what is true is that in modelling, *we take* the (complex) constituents *as being* simple, i.e. we use them as if they were simple, which means that it does not matter for the model what their internal nature is.

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[20] This claim is confined to scientific representation, with no goal of applying it to mental, or linguistic, representations at all. My account may, or may not, completely, or partially, apply to these representations, but we keep silent about this issue here. Recall that, according to this account, scientific models are, or may be, described by linguistic or mental descriptions. This first step from the description to the model is then left out of the scope of this paper.

[21] The particular way in which Tr elaborates in detail the combination of these ideas with other thesis also defended by the early Wittgenstein, is widely acknowledged as a failing project; nothing in what follows commits our account with these additional wittgenstanian theses, and as a matter of fact, most of the pragmatist components I will introduce are incompatible with many of these Tractarian extra theses.

[22] Fort the sake of simplification, when I do not explicitly say the contrary I will refer now to atomic models, not to molecular ones (molecular models are logical combinations of other (sub)models, atomic models are simple models. i.e. models that are not molecular). Nothing in what follows hinges on this.

[23] This is true at least of individuals, for it might be argued that some properties (e.g. being red, or being massive) or relations (e.g. being heavier than, being longer than) are simple despite we have direct access only to their complex instances. It suffices to the point made here that every model has at least un component that, though taken as simple, is complex. This implies a well know conflict with other parts of the *Tractatus*, yet parts that are not endorsed here.

Real life modelling consists in picking up some complex entities and, *taking them as simples*, combining them forming an ensemble.[24]

(vii) Contextual dependence of the logical form/category-in-use. In Tr, not only the constituents of the model are simple, they also have the logical category they have "in-se", the logical category fixed by their true ontology regardless we have access to it or not. Once modelling takes as constituents complexes using them as simples, their logical category is also a logical category in-use. That is, taking them as simples, combining them in forming ensembles, implies treating them, each one, as having a specific logical category (individual, monadic first order property, triadic first order relation, monadic second order property, etc.). In modelling, constructing the ensemble with the taken as simple components, implies *attributing* to each one a specific logical category in-use.

(viii) The taken-in-use simplicity and category is not determined by intrinsic features of the complexes. Of course, if we had access to simples they could not be taken as complex. But, confronted, as usually, with a complex, several options for using it in modelling are open; the "same thing" may be differently used in different models. A "white dot" may *be taken* as a simple-individual, or as a simple-property (whiteness), or as other simple property (circularity), or as one compound (an individual being white), etc. An arrow "→" may be seen as an individual or as a relation. This thing "O→I" may be seen (as molecular complex) as one individual being round and other being thin, and the round being in relation → to the thin, or as the individual → starting O and ending I (but notice that "the same thing" could also be taking as simple in certain models).

(ix) This simplicity and category in-use also applies to the targets. That is, in individuating the target we also describe it as formed by certain simple-in-use constituents with specific logical category in-use.

With these elements in hand, we can start formulating the EPSF account. I do it in the form of answers to the problems of existence, directionality and success introduced in section 2. In doing so, I`ll introduce some other elements in each step that will deserve discussion, and will be discussed, either in this or in the last section.

Directionality and existence

Starting with the problem of existence, and its sub-problem of directionality, EPSF explicates the directionality of the model towards the target in terms of subject's intention, the *intention-of-representing T with M*. I write 'intention-of-representing' as a united expression in order to connote that this is a primitive notion, for it cannot be compositionally analyzed in terms of "intention" and "representation" (representation in the EPSF sense) on

[24] Wittgenstein himself uses the example of the car accidents reconstructed by scale models in court, and of course he was aware that the parts of such models were not simple, so my liberalization has some coherence to the intuitions behind his own example. Nevertheless, he also had an argument against the non-simplicity of model components, namely that in such a case, that some intended *bild* actually had sense would depend on that other being true (Tr 2.0211, 3.23), which would ruin his explication of logical truth. Of course, my account is not committed to such argument.

pain of circularity. Whether this primitive intention makes the account non-naturalizable (cf. Putnam 2002, van Fraassen 2002, Suárez 2003 for this issue) will be discussed later. Intention is then necessary and sufficient for directionality. And thus it is at least necessary for existence. Yet, as I have argued above, it is not sufficient. One may try to perform a representation, and fail. There are conditions, besides intention, that must be met for the representation to be performed.

The first additional condition has to do with the standing for relation that takes as its domain the constituents of the model that are *respects* of the representation. The condition has also a Tractarian motivation, namely, that the standing for relation must preserve logical category. A thing taken as individual in the model must stand for a taken as individual in the target; a taken as a binary first order relation in the model must stand for a taken as binary first order relation in the target; etc. (notice that this condition is part of Contessa's analytical interpretations). The idea behind this is that you cannot, for instance, represent (not even wrongly) the dyadic fact that the Earth is more dense than Mars by a model consisting in an individual exemplifying a monadic property, for instance Peter being male.[25] One could object that this should be a condition for success, not for existence. I do not think so. It would be as arguing that in uttering 'Peter is between Mary and' one actually performs an assertion, but an unsuccessful one. Of course, we can stipulate talks at will, but I think that it is uncontroversial that the grammar failure is of a different kind than asserting falsely; then I think it is better described as ruining the intention of performing the assertion. One could stipulate otherwise, but then one should also distinguish between two essentially different kind of unsuccess, which I think is only verbally different and potentially misleading. Likewise for grammar/logical failures in representing.

This said, it might be that the strict "same category condition" is too stringent, that another, weaker condition suffices to this effect. For instance, an individual in the model could stand for a monadic first order property, if I want to represent a monadic first order property instantiating a monadic second order property and the ensemble of the model consists in such individual instantiating a monadic first order property, and then the individual in the model stands for the monadic first order property in the target, and the monadic first order property in the model stands for the monadic second order property in the target. Let us call 'logical congruency' this condition imposed on the standing for relation. The subject constructing the ensemble determines the logical category in-use of the (taken as simples) ensemble's constituents. Then, the standing for relation satisfies the *logical congruency* condition if and only if the things for which the parts of the model stand, have categories that make logically possible to ensemble them as the parts of the model that stand for them are ensembled in the model. One might object that this is a very weak condition if, as in our case, logical categories are not in-se but only in-use, for this condition would be violated only in case of logical incoherence of the modeler. Then we cannot expect interesting real life examples of such failure in scientific practice. I fully agree. And in this regard it is like the grammar constraint for assertions, there are no interesting real life examples either. Admittedly, logical/grammar incoherence is not very interesting, but this does not mean that logical/grammar coherence is not a condition for modelling/asserting.

[25] One could argue that we could always take Peter being-male as representing Earth being-more-dense-than Mars, that is, "monadizing" the dyadic relation and making being-male standing for being-more-dense-than Mars. But in such a case it is arguable that we are not really taken being-more-dense-than-Mars as a simple monadic property.

Logical congruence, though, is not the only existence condition besides intention. A second condition is needed for making sense of the fact that we may fail in performing the representation if the entities for which the parts of the model stand have nothing to do with the intentionally selected target. For instance, let us suppose that I intend-to-represent the killing of Caesar. For doing that, I construct on top of my table an ensemble made of two medium pieces of wood and a little one of plastic, taken as individuals, and certain spatio-temporal relations. But then, in my standing for relation, one of the wood pieces stands for Maradona, the other for the 1986 English goalkeeper, the plastic piece for the ball in the quarter-final, and the spatiotemporal relations on the table for spatiotemporal relations in the Mexican stadium (this standing for relation satisfies the logical congruency constraint). I do not think that, *given that* I have intentionally addressed as target the killing of Caesar; I do not think that we can say that I nevertheless am representing it, although very wrongly. If the objects that the representator intends to be stood for do not belong to representator's intentional target, the representator is "objectually" incoherent and, even if logically congruent, there is no representation at all. Again, we can talk at will, but I think that the situation is better described as a failure in performing the representation.

We then must add a condition connecting the standing for relation with the intentional target selected. The weakest option is that at least one of the stood-for entities is part of the intended target as it has been individuated. Maybe this is too weak and reasons could be given for a more demanding condition. For instance, if I want to represent Trafalgar's battle by an ensemble made of pieces and spatiotemporal relations on a board, and I make one of the pieces to stand for Nelson, but the other pieces stand for Real Madrid and Barcelona football players and the spatiotemporal relations on the board stand for spatiotemporal relations in the Nou Camp stadium, it is arguable that I have not represented my target, not even wrongly. This is a substantive issue that should be discussed carefully, but for lack of space I will not do this here and will choose the more conservative option, namely, that all elements that individuate the intentionally selected target T are "stood-by" elements of the model (remember that individuating features of T do not include all what the model attributes to it, e.g. in the phlogiston case, phlogiston is not an individuating feature of T). Very likely, again, there are not going to be very interesting, real life examples of such failures; representators are not usually so incoherent in combining the standing for and the selecting target relations. But, again, this does not mean that this kind of condition is not in place.[26] We arrive then to the following analysis:

(EPSF-E) Existence

S uses M to represent T in respects R with purposes P in context C:
(i) S has in C the <u>intention-of-representing</u> T (individuated by features t1, .., tn) with M in respects R, for purposes P;
(ii) in C, M is an ensemble of entities, those of which are respects in R stand, according to S, for other entities satisfying the logical congruence constraint;
(iii) in C, all target individuating features t1, …, tn are stood for by respects R of M.

[26] One could perhaps go for a more liberal option, dropping the first, intentional condition, and let the target be whichever system for whose pieces the standing for relation stand for. But we would have similar troubles with conceivable representators that make the parts of the models stand for completely unrelated thing, I think the same intuition applies.

EPSF-E naturally generates a surrogatory reasoning with the following conclusion: stood for elements behave to each other in T C-like the respects-elements that stand for them behave in M (C then determines the kind of alikeness relation relevant in the context, and its corresponding degrees of alikeness). Notice that we could not add as a new condition that its content obtains as a fact, for its satisfaction would make all existing representations successful. What we could add is that the subject has a belief with such content, but since it is controversial that such believe is necessary for representing (see e.g. Nguyen 2016), I will not add this further condition.

Although, admittedly, EPSF-E does not have very interesting real life examples of failure in scientific practice, it has interesting implications compared to other accounts. For instance, it clearly opposes Callender and Cohen (2006) account, that does not distinguish the parts of the model standing for from the model as a whole representing, and according to which anything, such as salt shaker, can represent anything else, e.g. Madagascar, depending only on subject's will. No. *In a model*, the salt shaker, taken as a simple, may stand for any other thing also taken as simple, but only as much as the standing for relation holistically satisfies the logical congruence constraint. But it cannot stand for other thing taken as complex; the salt shaker cannot stand for the London Undergound network *taken as a complex* (nor for Madagascar taken as a complex). And, *as a model*, its representational potential depends on how one takes it as a complex, i.e. as an ensemble. According to some use of the salt shaker as an ensemble, it could represent Madagascar as a complex; according to some other uses, it cannot.

Let us compare this account with the previous functional ones of existence. Compared to Hughes' DDI account, EPSF-E also includes: denotation, in clause (i); demonstration, implicitly in the construction of the ensemble and the specification of the respects that are relevant for the representation; and interpretation, via clauses (i) and (iii) as is made explicit in the associated surrogative reasoning. But EPFS-E is stronger than DDI, including conditions that DDI does not, which, if free of counterexamples, makes it better. Compared to Suárez', EPSF-E fares also better. On the one hand, it does not take the facilitation of drawing inferences as constitutive brute platitude in the analysis. The possibility of drawing inferences, or surrogative reasoning, is now a consequence of the analysis. On the other hand, EPSF-E is much stronger than [Inf], and then, if free of clear counterexamples, correspondingly much better. Compared to Contessa's, EPSF-E improves it in three ways. First, it weakens the identity of logical form constraint imposing just holistic logical congruency, which remedies possible counterexamples that strict categorical identity might generate. Second, it introduces purposes and degrees of satisfaction (although this will become more apparent when success is analyzed below). Third, but for the features of the target used in its individuation, EPSF-E permits that some alleged element attributed to the target actually does not exist (more on this below).

I contend that EPSF-E fares also better than the recent Frigg and Nguyen (2017, 2017b, 2017c)'s new DEKI account. According to it, *M* represents *T* if and only if: *M* denotes *T* (and possibly parts of *M* denote parts of *T*); *M* is a *Z*-representation (that is, *M* represents *T* as *Z*) exemplifying properties *P*1, …, *Pn* (I take *Pi* to be my *respects*); *M* comes with a key, *K*, specifying how *P*1, …, *Pn* are translated into a set of features *Q*1, …, *Qm* (i.e. K(<*P*1, …, *Pn*>) = <*Q*1, …, *Qm*>); the model imputes at least one of the properties *Q*1, …, *Qm* onto *T*. DEKI is much weaker than EPSF-E. First, it does not distinguish between model denotation, and its elements denotation. Second, K is a kind of standing for relation, but a holistic one, with no need for each relevant piece of the model (model's R-respects in my terminology) to stand for an element in the target. Third, it does not impose any logical congruency constrain. Finally (although I argued above that this is controversial), it allows for an almost massive mistargeting in the model's respects since just one is demanded to be imputed to T. In these regards, DEKI is unnecessarily much weaker than EPSF-E. But in other aspect DEKI is unnecessarily stronger, namely the above referred stipulation that the target is constituted by everything postulated/imputed by the model, with the unwilling consequence that when an imputed entity does not exist then the model is targetless. Unless there are paradigmatic cases that satisfy DEKI but do not satisfy EPSF-E, I take that the latter fares better.

Success, misrepresentation and inaccuracy

As announced, after answering the problem of existence we are in a position to answer the problem of success, and its consequences for misrepresentation. The inherentist intuition preserved here is that success (as surrogatory reasoning above) involves relevant similarity or alikeness of behavior. Roughly: if the parts of T for which the relevant respects of M stand, behave in T (to the degree that C admits) like the original respects in M, and so that in virtue of it the purposes (e.g. inferring about continental land shapes) are achieved, then the model is successful. For this to be the case, an obvious requirement is that the entities postulated by the model, i.e. that its respects aim to stand for, exist. We arrive then to the following analysis (with some necessary comments immediately after):

(EPSF-S) Success

S successfully uses M to represent T in respects R with purposes P in context C:
(i) S uses M to represent T in respects R with purposes P in context C
(ii) all entities that respects in R (aim to) stand for, actually exist(-as-aimed)
(iii) entities that respects in R (aim to) stand for, behave to each other in T C-like their corresponding respects in M
(iv) purposes P are achieved, and are so in virtue of (iii)

The necessary comments are the following. First, as announced, EPSF-S incorporates an essential inherentist condition in clause (iii). Yet, the similarity condition is very minimally formulated, just as "components of the target behave C-like their correspondent components in the model". The idea is that every context determines the alikeness relation (including its degrees of satisfaction) that is relevant for the assessment of M in C. We saw that no specific similarity relation (isomorphism, faithful homomorphism, …) applies to all scientific models, across all contexts, but rather some apply in certain contexts and others in other contexts. Yet, (iii) aims at capture the minimal inherentist aspect that all these relations share, namely they all are, for the case in point, the specific *alikeness relation* that the context determines in such case. So, the characterization "the-determined-by-the-context-kind-of-alikeness-in-behavior" apply to all of them. Admittedly, this general characterization is quite imprecise, but given the multiplicity of different instances in different contexts, it is the strongest possible. And notice that, though imprecise, introduces a real constraint *in the analysis* (contrary e.g. to Suarez's analysis), since contexts must determine particular instances of this general kind. That is, modelling admits different *styles* (Frigg and Nguyen 2017), and every context determines the style in point, but all styles have to be some specific version or other of this general *alikeness in behavior*. Then, it is little, true, but not nothing;

actually, this is all one may impose at the general level of analysis, the pragmatic context does the rest. One could wonder then how this analysis does better than previous similaritivist ones. It does so in the following respects: it makes clear that alikeness in behavior is condition for success, not for existence; it makes explicit that the context determines the specific alikeness in behavior relevant for assessing the model in C (so the charge of *general* easiness of success due to always existing some similarity, does not apply); and, more importantly, it adds a further condition, (iv), not present, at least not explicitly, in such previous accounts. One may still object that these are not radical differences, but I already announced that our analysis was not going to offer radical novelties but rather to put together, in the right way, the worth preserving elements of previous accounts.

Second, it is a seek consequence of the analysis that one reason of model failure is that some entities postulated by the model do not exist (phlogiston, caloric, ether and the like). Nevertheless, to adequately formulate this with complete precision is anything but easy, and (ii) is only a first approximation, useful for expressing the intuition but rather inadequate. If we use "entities that respects in R stand for", the problem is that 'standing for' (like 'hit' or 'brake') is factive: if something stands for something else then the former *and the latter*, both, exist. And respects R of M always exist, but what they aim to stand for may not[27] (only target individuative features t1, …, tn are granted to exist). We then have two options. Either (a bit counterintuitively in my view) we grant that when the aimed object, e.g. phlogiston, does not exist-as-aimed (i.e. physically in this case) it nevertheless exists as an *abstracta*;[28] in which case we could continue using "standing for" here and in EPSF-E. Or (departing dramatically from the Tractarian origin) we use, here and in EPSF-E, the non-factive "*aim to* stand for" instead of the factive "stand for". This latter option I think suits more intuitively scientific practice. Moreover, notice that in the first option we also need to add "-as-aimed" for, without that addition, and since phlogiston would exist as abstracta, the phlogiston model would not fail to satisfy clause (ii). All in all, then, it seems better to talk of "aiming to stand for" instead of "standing for", and revise EPSF-E accordingly (see below). To conclude with (ii), some might be tempted to transfer it to EPSF-E as a condition for existence, but I do not think this is a good option, since the common talk is that, for instance, the phlogiston model, which fails due to the inexistence of phlogiston, nevertheless represents.

Third, clause (iv), its second part included, is central to the analysis. This is the crucial syncretic element since it adequately connects the inherentist and the functionalist grains of truth that we saw in section 3 that were worth to preserve *and to adequately combine*. That the C-alikeness relation actually obtains is the inherentist part. And the achievement of the C-relevant purposes is the functional bit. But they must be adequately integrated, and the correct formulation of such integration must make sense of the intuition, implicit in several previous accounts, that the alikeness in behavior *grounds* the achievement of the function: it is *in virtue of* the fact that the aimed relevant alikeness obtains, that the function-purposes are reached. In modelling, we implicitly "claim" that the relevant entities in the target behave alike the corresponding entities in the model. If this is actually the case, clause (iii) is satisfied. For the representation to be successful, the surrogatory reasoning function with purposes P must be complied, but not by chance or in virtue of other causes, it must be so in virtue of the relevant alikeness obtaining, i.e in virtue of the C-correctness of the conclusions

[27] Notice that we cannot say "what they stand for may not exist" for it is self-contradictory.
[28] This needs a complexization when the postulated entity that does not exist as aimed is an aimed abstract entity, e.g. some specific universal. I will not deal with this difficulty here.

of the surrogatory reasoning. It is difficult, again, to find interesting real life cases of unsuccess due to failure of (iv) in scientific practice, for scientist, when they satisfy the relevant purposes, they do that exploiting the obtained alikeness;[29] but, as the congruence condition in EPSF-E, that this condition is usually satisfied does not mean that the condition is not in place.

Fourth, once success is analyzed, misrepresentation is immediately characterized as unsuccessful representation: *S misrepresents T with M in respects R with purposes P in context C if and only if: (i) S uses M to represent T in respects R with purposes P in context C; and (ii) S does so unsuccessfully*. We then have three different sources of misrepresentation. The representation may fail due to the non-existence of a postulated entity (as postulated). Or the all postulated entities (together to features t1, …, tn of T) exist, but they do not behave to each other C-like the corresponding constituents of the model behave in the model. Or, finally (and admittedly unusual), the postulated entities exist, they behave C-as the constituents of the model in the model, but either (due to unexpected factors) the purposes are not reached, or they are reached but not in virtue of the communality of behavior.

Finally, as many have emphasized (e.g. Elgin 1996, 2010, Jones 2005, Godfrey-Smith 2006, Morrison 2008, Bokulich 2009, Cartwright 2010, Toon 2012, Frigg and Hartmann 2012), representations, even when successful, are almost always inaccurate. Inaccuracy is an essential feature of successful representations that any analysis must account for. There are two main kinds of inaccuracy. On the one hand, inexactness, distortion or idealization: perfect spheres, infinitesimal distances, frictionless planes, point-masses, infinite particle collections, perfectly isolated populations, and the like. Models include these individuals and properties or relations, but the modeler does not aim that the parts of the target for which they stand have exactly these features; it is aimed only that they *approximate them to a certain degree* (determined by C): almost perfect spheres, very small distances, very small massive bodies, indefinitely large finite collections, almost isolated populations, etc. The analysis must do two things in this regard. First, to include an element that makes room for this essentially gradual approximative feature of successful models. Second, to show that there are formal tools for (re)constructing models that make models the kind of entities for which the degrees of satisfaction talk formally makes sense. With regard the former, as we advanced in section 1 introducing the context of modelling, in EPSF-S one of the roles of the context is to determine the degree of accuracy required for the purposes in point. For instance, if we model parabolic shot, the degree of accuracy sufficient in one context may be insufficient in other, more demanding context. In this respect, there is no substantive conceptual constraint applicable across all models; the only general, and almost empty conceptual constraint, as in our analysis, is that every context determines its relevant degree of representational accuracy. With regard the latter, EPSF-S simply assumes in each case the formal tool that the philosopher considers the best for reconstructing idealized models, either topological (e.g.

[29] But imagine, for instance, a crazy geographer that constructs a Gay-Peters map of America with the purpose of getting the proportions of regional land areas, but does not calculate them using the relevant similarity of the map but obtains them using a list of quantities that a magician left attached to the same map-type (and that the magician obtained not correctly using a map but, for instance, consulting the gods who gave him information coincident by chance with the derivable from the map).

Balzer et alt. 1987), or analytic (e.g. Novak 1992), or algebraic (e.g. Ducheyne 2012), or other.

The second type of inaccuracy is not due to idealization or distortion, but to absence of intended correspondence. It is not that *some* aspects of the model are aimed to correspond to aspects of the target only up to a certain degree, they are not aimed to correspond to any aspect in the target *at all*, as happens for instance in the Burridge-Knopoff model for earthquakes or the astronomical Antikytera Mechanism mentioned in section 1. We seem to have two options here. Either we take them as representational and treat them as limiting, degenerated cases of idealization in which the context accepts zero degree of correspondence. Or we take them as non-representational, i.e. merely predictive, or calculatory, with no intended correspondence at all. Yet, I doubt that the former substantively differs from the latter, since the difference between zero degree of intended correspondence and no intended correspondence seem merely verbal. We then take this source of inaccuracy as connoting the presence of non-representational elements in the model (see section 1). Yet, if we take into consideration clause (iv) in EPSF-S, things are not that simple.[30] We are talking of elements in T that, though not intended to correspond to anything, are nevertheless used in (in Hughes' terminology) demonstrating the information we want to transfer to/interpret in the target. Then, they are relevant respects of M in the sense of being utilized in the demonstration, but not in the sense of intended to correspond to something in T. And EPSF-S(iv) demands that the purposes are obtained in virtue of the behavior of the T-elements being C-like the behavior of the M-elements (deployed in gathered the information) that stand for them. Now, if in successful models, some M-elements that are essential for the relevant projected M-behavior do not aim to have a correspondence in T: How can the purposes be reached in virtue of a correspondence that at least partially does not obtain? How can the outcome behavior be successfully imputed to T without intending that *every respect* that generates such behavior correspond to something in T? This in a nutshell is the problem that the presence of non-representational elements in representational models poses for EPSF-S (it seems, then, that (iv) was not so innocent after all)

The only manner I think this problem may be faced is going fictional in some way, and accepting the consequence that these models have something that makes their representational success partially mysterious. But I think this is simply the way things are: if a representational model includes *and uses* essential elements that are not representational, then its representational status is partially undercut, and its success *as a representation* partially mysterious. The model does not aim the non-projected element(s) to stand for anything (not even approximately), nevertheless it proceeds *as if* it were projected. It does not stand for; it is not even intended that it stands for; the model just proceeds as if the element were aimed to be projected although it is explicit that it is not so aimed. In this sense its projection is just a *pretense*, a *fiction*. And, admittedly, the representational success of the whole model, based on the modelized behavior of this fictional element with the other elements that are really intended to stand for things in T, is somehow mysterious. The "in virtue of" part of clause EPSF-S(iv) must be read, with some oddity, counterfactually, including this fictional reading of the non-representational respects of M. I do not think this is a deficiency of the analysis, but rather the analytical reconstruction of how things are in these quite unusual cases of non-projected respects. Every analysis has to face these odd cases with (not only idealized but completely) fictional components; and either you get silent

[30] Thanks to James Nguyen for pressing on this point.

on them but *by getting also silent on what grounds success in general* (Nguyen 2016), or you explicate the grounds of success and witness these rare cases as partially meeting the conditions only fictionally. I do not see a *tertium datur*.

Notice, to conclude, that this fictional as-if component has nothing to do with the *representation-as* component of Elgin (1991, 2010), van Fraassen (2008) or Frigg and Nguyen (2017). In these cases, it is claimed that M represents T as F, for instance a drawing represents Margaret Thatcher as a bull-dog. The as-F phrase connotes that M makes particularly salient some elements of T "summarized" as F-likeness. I think my analysis, with respects, purposes and context, makes room for this phenomenon. There are two sense of representing something as-F. In one, very broad sense, every model represents T as-something, in particular as-its model content; that is, attributing to T additional components, together with the ones used in individuating T, that behave so and so to each other. This "behave so and so" is the model content, the F such that that every model represents T as F. In other, more strict sense, like in the Margaret Thatcher example, the as-F phrase expresses a particular purpose, for instance to highlight the aggressive character of a person. I take that the inclusion of respects, purposes and contexts in my analysis allows for a similar function.

With the above qualifications, the improved version on EPSF is summarized thus:

(EPSF-E*) *S uses M to represent T in respects R with purposes P in context C:*
(i) S has in C the intention-of-representing T (individuated by features t1, .., tn) with M in respects R, for purposes P;
(ii) in C, M is an ensemble of entities, those of which are respects in R are such that S intends, actually or fictionally (at least one respect actually), that they stand for other entities in T satisfying the logical congruence constraint;
(iii) in C, all target individuative features t1, …, tn of T are stood by respects R of M.

(EPSF-S*) *S successfully uses M to represent T in respects R with purposes P in context C:*
(i) S uses M to represent T in respects R with purposes P in context C;
(ii) all entities that respects in R actually aim to stand for, exist;
(iii) entities that respects in R actually aim to stand for, behave to each other in T C-like their corresponding respects in M;
(iv) purposes P are achieved, and are so in virtue of (iii) (respects, if any, that only fictionally aim to stand for, matter for demonstration in M but not for behavior alikeness in T).

To conclude this final version of the proposal, let me clarify some possible misunderstanding about its seeming epistemological commitments. One might argue that EPSF is too obviously committed with a realist stance in the scientific realism debate to be acceptable as a neutral analysis of modeling: if (as usual) the model attributes to the target unobservable entities, and its success, according to EPSF-S, consists in such entities existing and (approximately) behaving such as the model claims, this looks pretty much like taking a successful theory with unobservable content as being (approximately) true, i.e. its unobservable posits existing and behaving (approximately) as the theory claims. In this regard, the criticism could continue, the account is defective since an admissible account should not bias in favour of a specific position in a related, though arguably orthogonal debate. I sympathize with this concern, but disagree that EPSF-S is committed to a realist position. EPSF-S is an *analysis* that aims at elucidating the concept *model M successfully represents T (in C, with respects R and purposes P)*, or *model-representational success* for short. In in this regard it is analogous to an analysis of the concept (to simplify) *theory T is true*. The problem of scientific realism applied to scientific models presupposes that we have a clear concept of representational success and assumes that models are or are not representationally successful, as the problem of scientific realism applied to theories presupposes that we have a clear concept of theoretical truth, and assumes that theories are or are not true (alethic realism). Epistemic model realists, then, would claim that predictive success justifies to believe in representational success; while antirealist would deny this and defend, for instance (analogously to constructive empiricism), that predictive success justifies only to believe in empirical adequacy/observational success. EPSF, then, does not presuppose epistemic model realism, as alethic realism does not presuppose epistemic theoretical realism. It simply clarifies the concept relative to which epistemic model realists and antirealist take a stance.[31]

5. Concluding remarks

EPSF meets the desired desiderata we were pursuing for an analysis of *representational* models. It:

- Distinguishes three different kinds of intended representations: the intention-to-represent a particular target; the intention of parts of the models standing for parts of T; the holistic representation of M, which reduces to the ensemble plus the (intended) standing for relation.
- Neatly separates, and answers, the problems of directionality, existence and success. As for the problem of ontology, imposes some ontological constraints but is compatible with any ontology that is compatible with such constraints.
- Provides conditions that are necessary and almost sufficient for existence in the sense that EPSF-E* does not include paradigmatic cases of non-representational models, and does not exclude paradigmatic cases of representational models (or so it is claimed). Likewise with EPSF-S* and successful representational models. It is then the strongest analysis known: stronger than others without counterexamples, and there is no other known stronger without counterexamples.
- Is a non-pluralistic, enough unified account that provides strong and *substantive*, elucidatory conditions for existence and success for all kinds of scientific representational models.
- Is syncretic in preserving and combining the correct insights of previous inherentist and functional accounts. And it does so not by mere conjunction, but making one component grounding the other (EPSF-S*(iv)). Against functionalism, it *explicates* functional success. Against standard inherentisms, it does not impose a specific similarity in behaviour for success, but just sets a general inherentist-alikeness constraint that every context must specify.

[31] Of course, this sense of (epistemic) model realism is independent of the sense of model realism in the discussion about the ontology of models.

- Accounts for surrogative reasoning as implicit in model existence, and for successful surrogative reasoning as the main function successfully performed by successful models, but compatibly with the presence of other, additional more specific purposes.
- Incorporates degrees and respects allowing for distortion and idealization in the standing for relation and, consequently, in success.
- Distinguishes no representation from misrepresentation, and accounts for different kinds of misrepresentation.
- Makes room, with some inescapable oddity, for representational models with components that do not have representational import.

I take all them be desirable features for an account of scientific modelling, and EPSF meets them. The analysis, though, has other features that some may find more controversial:

- It is irreducibly intentional in making use of the conceptually primitive (not necessarily metaphysically primitive) "intention-to-represent a target". This, according to some, makes it non-naturalizable, but I do not think that this notion of "non-naturalizable" is incompatible with a physicalist/materialist stance.
- It takes "ensemble" as conceptually (not metaphysically) primitive. Then, its advantages with respect set-theoretical structures (which are set-theoretically definable) are at the cost of accepting this conceptual primitiveness of "ensemble".
- It is irreducibly, and strongly, pragmatist: Respects, purposes and degrees are context dependent. In modelling, there are no logical categories in-se, but only in-use. It is the modeller, in constructing an ensemble, who takes some complexes as simples and treats them as having specific logical categories. The target is pragmatically individuated as the target of the intention-to-represent act, using in C some T features as individuating T (no further explanation of that). The context determines the specific style, the specific alikeness in behaviour, including its acceptable degree, relevant for assessing the correctness of the representation.
- It is fictionalist in several, different senses. First, in the sense that the model-content does not exist, independently of the ensemble and the intended standing for relation; there is no extra-thing in the world besides the ensemble and the standing for relation; of course when the model is correct there is a fact that corresponds to the model, but when not correct, there is nothing, i.e. no complex ensemble, material or abstract, that is the model-content. Second, the modeller may aim some parts of M to stand for elements in T that actually do not exist. Third, it is fictionalist in the sense that some idealized features of M are fictional in not being aimed to have correspondence in T "exactly like that". Finally, in the strongest, game of make-believe sense that in some models the modeller takes some parts as "representational fictions", with no aim to take them to stand for anything at all, not even approximately.

I admit that some of these features may seem controversial, in particular the strong pragmatic elements. But I think they are unavoidable. I think that some of them have made meta-modellers to renounce to a unified analysis and withdraw to minimalist or pluralist positions. I have tried to show that, these pragmatic elements notwithstanding, the correct insights of previous accounts may be put together getting an analysis weaker than the extensionally inadequate, inherentist ones, but stronger than their pluralist or minimalist alternatives. The outcome is an account that is as strong as possible in the respects it can be, but irremediably pragmatic in other respects that do not allow for *general* stronger conceptual constraints. EPSF is enough strong and unified as to provide a substantive monistic explication of scientific representation, and in any event stronger than any other known account free of counterexamples; and at the same time, it also highlights the variety of parameters that the pragmatic context must fix in every specific act of modelling. If free of counterexamples, I then take it that EPSF fares better than any other monistic account, and well enough to resist minimalist or pluralist withdrawals.

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