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Programme Overview

9:00–10:30
Registration

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11:00–12:00
Plenary lecture

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Plenary lecture

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Parallel sessions

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PLENARY LECTURES

The modal μ-calculus: How games, fixed points, and automata met each other

Giovanna D'Agostino
University of Udine

-Fixed points of operators are intensively studied in mathematics, as well as in Computer Science.

-The mathematical theory of games is a beautiful theory with applications in Economy, Politics, Sociology, Biology, Computer Science

-Automata are finite object widely used in Computer Science to express some sort of regularity for languages over finite/infinite words, trees etc.

In this talk we present a logical formalism, the modal μ-calculus, that embodies all these three aspects in a beautiful theory.

Two bad arguments against naturalism in the philosophy of social science

Francesco Guala
University of Milan

Naturalism is still facing a strong opposition in the philosophy of social science from influential scholars who argue that philosophical analysis must be autonomous from scientific investigation. The opposition exploits philosophers' traditional diffidence toward social science and fuels the ambition to provide new foundations for social research. A classic antinaturalist strategy is to identify a feature of social reality that prevents scientific explanation and prediction. An all-time favorite is the dependence of social phenomena on human representation. I will examine two prominent versions of the dependence thesis and concludes that they both fail. Contemporary social science is capable of accounting for the causal dependence of social reality on representation, and there is no reason to believe that social entities are ontologically dependent on the collective acceptance of a constitutive rule.

Spacetime functionalism

Eleanor Knox
King's College, London

Many (perhaps all) concepts in science are functional, but the idea that we should conceive of spacetime as whatever fills some functional role has not been much explored. Nonetheless, a functional conception of spacetime seems to be required by some theories of quantum gravity in which spacetime is non-fundamental. I'll also argue that functionalism is helpful in the context of classical spacetime theories; it has the potential to dissolve some old problems. I'll advocate a particular kind of spacetime functionalism inspired by the work of Harvey Brown.

What distinguishes data from models?

Sabina Leonelli
University of Exeter

This paper discusses the relationship between data and models, and proposes a framework to distinguish their epistemic roles while also capturing the material, conceptual and social circumstances in which these research components are developed and used to produce scientific knowledge. I focus specifically on the case of data models, and ask how and why these can be distinguished from actual datasets, particularly in cases where both data and data models are conceptualized as representations of a given target system. As concrete grounding for my analysis, I reconstruct the stages through which phenotypic plant data generated within smart glasshouses are processed and interpreted as evidence for claims about root growth. This case illustrates how whether a set of objects functions as data or models does not depend on intrinsic differences in their properties, level of abstraction or the degree of human intervention involved in generating them, but rather on the role that they play in helping to identify and characterize the targets of a given investigation.

From misreading to myth: False, unsustainable, or otherwise dubious beliefs about the development of logic and foundations

Jan von Plato
University of Helsinki

Logical constants from consequence relations: Carnap's question

Dag Westerståhl
University of Stockholm

Logical constants are crucial to logical consequence, but what makes a word log- ical? Answers in terms of *invariance* and of *proof* rules have been attempted. We suggest instead to start from the consequence relations themselves, and try to 'extract' the meaning of logical constants from these. In his 1943 book *The Formalization of Logic*, Carnap worried that even for classical propositional logic *CL*, the meanings (truth tables) of the connectives are not fixed by *CL*-consequence. However, if meaning assignments are required to be *compositional*, Carnap's worries about *CL* can be allayed. More importantly, *Carnap's question* can be asked in a precise way about any consequence relation in any logic. (To what extent) does classical first-order consequence determine the meaning of $\forall$? What about other (generalized) quantifiers? What about the intuitionistic meaning of the connectives? Or the connectives in possible worlds semantics? In (in)dependence logic? Is Carnap's question, if asked about $\Box$ in modal logic, similar to the one about $\forall$ in first-order logic? I will survey some answers and open problems. A tentative conclusion is that being fixed by a standard consequence relation, sometimes in combination with a suitable form of invariance, could be taken as a criterion of logicality. In any case, I hope to show that Carnap's question is worth asking.
This is joint work with Denis Bonnay.

Affiliated symposia

On the concept of organism

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1 Aims and description

The aim of the symposium is to survey recent discussions on the concept of organism, in order to assess whether this concept may still be of use in the philosophical investigation of biological phenomena. Indeed, even if *prima facie* it is a crucial concept for biology and philosophy of biology, the very notion of organism is still controversial, and the role that such a concept can play in the investigation of the living domain is still fiercely disputed. In the 20th century, the traditional centrality of organism has been neglected for several decades (Nicholson 2014). The standard theoretical tools that were used to understand the living were those developed by molecular biology and population genetics. Those disciplines analyse living beings at a sub-organismic level and at a supra-organismic level, respectively. Thus, the concept of organism lost its theoretical centrality. In recent years things become more nuanced. There has been a new interest in the concept of organism (Laubichler 2000; Huneman, Wolfe 2010; Gambarotto, Illetterati 2014), and many of the challenges that have been moved to the received view in biology, i.e. Modern Synthesis (MS), in some way or another rely on the idea that we should pay more attention to organisms in order to understand those features of the living beings that the standard view is unable to account for (Huneman 2010; Walsh 2010). This shift towards the organismic level represents for the critics of MS that are more sensitive to the self-organizing dimension of the living, the "attempt to answer the fundamental question of biology—*What is life?*—by re-posing it on the pertinent level of description" (Bich 2012, p. 217). The renewed attention at the organismic level has been considered to be an "antidote" for the "unbridled reductionism" that characterizes biological explanations centered on molecular biology (Nicholson 2014). According to many philosophers, the peculiarity of the living would be due to the causal regime that biological organisms realize, rooted in their "organisational closure", which is the source of their autonomy, and which differentiate them from non-living entities (Mossio, Moreno 2010). Some authors even think that the lack of theoretical fertility in biology is due exactly to the lack of a "theory of the organisms", and try to develop a theoretical proposal which may be able to take into account (and account for) the peculiarity of biology with respect to physics (Longo, Montévil 2014; Longo, Montévil, Sonnenschein, Soto, 2015). In this perspective, there is a deep and intrinsic gap between physics and biology, due precisely to the differences among the characteristic objects of this two disciplines: while the objects of physics are generic (i.e. they are interchangeable) and follow a specific trajectory (a precise path, namely a geodesic, in phase space), in biology we deal with objects that are specific (i.e. they are not interchangeable) and follow a generic trajectory (i.e. a path which can be at most defined as 'possible' or 'compatible' within the ecosystem, since it is *in principle* impossible in this context to construct a phase space of the relevant observables,

i.e. organisms, and thus predict their path) (Longo, Montévil 2014). So, in order to develop a fertile theoretical biology, we should develop the mathematics and the conceptual tools needed in order to deal with organisms. Godfrey-Smith (2016) has recently claimed that the concept of organism, notwithstanding some of its well-known limitations, may nevertheless be useful in order to integrate the issue of biological individuality with that of "the beginnings of mentality". Finally, relying on recent studies on cancer and cancer research, other authors are suggesting that an organismic perspective is required to account for inter-level regulatory processes linking the conceptual and explanatory dimensions in the scientific practice (Bertolaso 2016). But other authors think instead that "organism" is not a useful conceptual tool in dealing with biology. For example, Okasha (2011) has recently argued that we should abandon such an anachronistic hierarchical rank as "organism" in favor of a rank-free ecological hierarchy. A great amount of work has also been dedicated to untangling different concepts strictly related and yet insufficiently differentiated from that of "organism", such as the concepts of "biological individual", "evolutionary individual", and "living thing" (Pradeu 2016; Bouchard, Huneman 2013). The relevance of what we usually define as organisms seems in this way to fade, since more articulated and fine-grained categories are needed to account for the complexity of the living world. Dupré and O'Malley, for instance, raise concerns with the traditional notion of organism: "if we think of the organism as being simply whichever cooperative systems of cells are most usefully recognized for exploring biological function, then the assumption of 'one genome, one organism' starts to look like a poorly grounded dogma" (Dupré, O'Malley 2007, p. 842). In this line of reasoning, even the "autonomy" dimension that many authors take to be characteristic of the living may be put into question. Indeed, the "emphasis on autonomy is problematic [...] because even paradigmatic biological individuals [...] are dependent on symbiotic associations with many other organisms (Dupré, O'Malley 2009, p. 1). It is worth noticing that philosophical research into the issue of biological individuality are traditionally connected to Metaphysics. And if we turn our attention to the metaphysical inquiries of the biological realm that have recently been carried on, it is easy to find that "organism" is a contested issue also in this context (Casetta 2015). Some authors question the ontological status of organisms (Wolfe 2010), while other think that organisms may well be legitimate inhabitants of our ontologies (Boulter 2013). For example, according to French's proposal, which tries to extend his ontological structural realism to biological issues, "there are no biological objects (as metaphysically robust entities). All there is are biological structures, inter-related in various ways and causally informed" (French 2011, p. 172). On the contrary, according to Boulter, a neo-aristotelian metaphysical approach to biology is able to provide "a theory that will at once support the claim that biological organisms must be given a place within one's ontology, and allow one to demarcate clearly between organisms, their parts and the colonies or groups they may join" (Boulter 2013, p. 3). As Wolfe points out, organism may be conceived as an ontological 'go-between' concept, "invoked as 'natural' by some thinkers to justify their metaphysics, but then presented as value-laden by others, over and against the natural world"; at the same time we have to acknowledge that this concept "continues to function in different contexts as a heuristic, an explanatory challenge, a model of order" (Wolfe 2014, p. 151). From what has been said so far, it should be clear that the relevance and usefulness of the concept of organism, despite its long history, is still in need (and it is worthy) of further investigations. What is the theoretical centrality of the organism concept linked to? What scientific question asks for an organismic perspective? What epistemological issues are entailed by the re-emergence of the organismic language in the new technologies (e.g. organ-on-a-chip models, *in silico* medicine, etc.)? These and other questions will guide the reflections and discussions of the symposium.

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3 Abstracts of the five planned talks

- **The Organism inside Cancer and Cancer Research**
  Marta Bertolaso

Contemporary cancer research has been arising philosophical questions about the organism, its dynamics and how we explain them (Huneman, Wolfe 2010; Nicholson 2014). Cancer is, in fact, commonly described as an aberrant developmental process. I will thus focus on the following questions: Should cancer be considered an (aberrant) organismic entity? What aspects of cancer entail the pathological character of the neoplastic process? Is an organismic perspective required to understand cancer? What does this mean? At the crossroad of these questions there are important conceptual and explanatory issues (Bertolaso 2016, forthcoming). From a methodological point of view, in this talk I will offer an overview (i) of the debate in the scientific field and (ii) of the main epistemological transitions in the process of cancer explanation and understanding. I will thus illustrate some issues that open an interesting reflection about the ontological and epistemological status of the organism concept in scientific practice, in dialogue with recent studies on these topics (e.g. Moreno, Mossio 2015; Walsh 2015). The emerging philosophy of organism offers an epistemological framework able to overcome traditional dichotomies based on mereological accounts of the living being.

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- **The Organism without Idealism: Hybridity, Boundaries, Go-Between**
  Charles Wolfe

The organism is neither a *discovery* like the circulation of the blood or the glycogenic function of the liver, nor a particular biological *theory* like epigenesis or preformationism. It is rather a *concept* which plays a series of roles – sometimes overt, sometimes masked – throughout the history of biology, and frequently in very 'valuative' or normative ways, often shifting between realms or registers (Wolfe 2004, 2014), with all sorts of interesting semantic shifts (Cheung 2006). Indeed, it has often been presented as a key-concept in life science and the 'theorization' of Life (e.g. in the sense that biology is a science of organisms or is nothing; Grene, Depew 2004). In addition, perhaps because it is experientially closer to the 'body' than to the 'molecule', the organism is often the object of quasi-affective theoretical investments presenting it as essential, even as the pivot of a science or a particular approach to nature (from Hegel onwards, explicitly with thinkers such as Kurt Goldstein and, with more metaphysical investment, Hans Jonas; see Wolfe 2004, 2010, and many of the papers in Gambarotto, Illetterati 2014). Conversely, it has also been the target of some influential rejections, classically in Dawkins' vision of the organism as just an

instrument of transmission for the selfish gene (Dawkins 1976), a view itself open to 'organismic' or 'holistic' challenges (Oyama 2010). Here, instead of defending one or the other of these clear-cut ontological positions (a defense of organicisms runs the risk of giving 'laundry lists' of irreducibly organismic properties, condemned to be refuted or otherwise reduced: Di Paolo 2009), I reflect on the hybridity and 'go-betweenness' of the category of organism, from the standpoint of a 'historical epistemology of the life sciences'.

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- **Understanding Multicellular Organisms**

Leonardo Bich, Jean-Francois Moreau, Thomas Pradeu

Biological systems are organised in such a way that they realise metabolic self-production and self-maintenance. The existence and activity of their components rely on the network they realise and on the continuous management of the exchange of matter and energy with their environment (Moreno & Mossio, 2015). One of the virtues of the organismic approach focused on organisation is that it can provide an understanding of how biological systems are functionally integrated into coherent wholes by means of complex architectures of control acting on the basic thermodynamic processes (Bich et al., 2016). This organismic framework is primarily concentrated on unicellular life. Multicellularity, however, presents additional challenges to our understanding of biological systems. Several interconnected issues need to be addressed at this specific level of organisation: (1) how to explain metabolic integration and inter-cellular control and regulation; (2) which are the main actors of multicellularity, besides cells; (3) how the multicellular space is functionally organised. One possible way to address these issues in the context of the realisation and maintenance of the organism is to focus on how the "living together" of cells in multicellular systems is achieved by constraining the cellular *default state* characterised by proliferation and mobility (e.g. Montévil et al., 2016). We argue that the extracellular matrix plays a crucial active role in this respect, as an evolutionary ancient and specific (non-cellular) control subsystem. We explore how it contributes to the functional specification of the multicellular space, the organisation of mobility, and the modulation of proliferation.

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- **Multispecies Biofilms: Organisms, (Evolutionary) Individuals, or What? A Case Study from Bacteria**
  Elena Casetta, Jorge Marques da Silva

Bapteste and colleagues (2012) suggested that multispecies biofilms (together with other "mosaic" entities) might be evolutionary units (paradigmatically, organisms are units of selection, while population or species are units of evolution). Recently, it has been suggested that multispecies biofilms might satisfy the criteria required for being biological individuals, i.e. units of selection. The following two-parts equivalence emerges from the debate: on the one hand the equivalence between biological (or evolutionary) individuals and units of selection.; on the other hand, the equivalence between units of selection and "unitary organisms", as Tuomi and Vuorisalo called them (1989). While the first part of the equivalence is a matter of conventional definition and is explicitly stated (Clarke 2016), the equivalence between unitary organisms and units of selection (which is never explicitly stated but that can be inferred by the requirements listed for being biological individuals, such as having reproductive bottlenecks, forming parent-offspring lineages, having repeatable life cycles, showing overall integration; Ereshefsky, Pedroso 2013) is not. The equivalence between units of selection and unitary organisms is questionable and it requires a deeper enquiry. In this contribution we will focus on multispecies bacterial biofilms making reference to a particular case study (the biofilm made of *Acinetobacter* and *Pseudomonas putida*; Hansen *et al*. 2007a; 2007b) in order to discuss non only whether multispecies biofilms are "unitary organisms", but also whether they are units of selection, units of evolution, or both. The hypothesis that we are going to test through the case study is that they might be both units of selection and units of evolution. Whether they are organisms, it depends on the conception of organism taken into account.

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- **Organisms, Natural Selection, and Mathematical Explanations.**
  Fabio Sterpetti

According to Walsh (2010), there are two competing ways of conceiving neo-Darwinism, i.e. Modern Synthesis (MS), and Developmental Darwinism (DD). The main difference between these two accounts of evolution is that MS explains the process of evolution by appeal to the activities of genes, while DD explains the process of evolution by appeal to the capacities of organisms. This debate on the role of organisms overlaps another debate in philosophy of biology, i.e. the one on the nature of natural selection. Some authors, known as *statisticalists*, claim that natural selection, as it is usually formulated in population

genetics, is statistical in character and cannot be construed in causal terms (Matthen, Ariew 2009). On the contrary, other philosophers, known as *causalists*, argue against the statistical view and reaffirm the causal interpretation of natural selection (Otsuka 2016). Those two debates overlap since the statisticalists deny that natural selection is a genuine cause of evolution exactly because they maintain that are the interactions of individual *organisms* taken together that *constitute* natural selection. Thus, it is quite natural that many supporters of DD are also statisticalists. The problem for the statisticalists is how to conceive the explanations provided by population genetics. Many statisticalists admit that population genetics provides genuine scientific explanations, but maintain that those explanations are *non-causal* explanations (Ariew, Rice, Rohwer 2015), i.e. they are instances of mathematical explanations of natural phenomena (Baker 2009). This line of reasoning may represent a threat for the supporters of DD, since it seems to lead to the acceptance of Mathematical Platonism (MP). But accepting MP would come with a cost for the statisticalists: since MP is usually taken to be incompatible with a naturalist stance, their position should be considered an anti-naturalist view, and this would be unpalatable for many of them. This paper, elaborating on Matthen (2009) and Cellucci (2017), aims at showing that supporting DD and the statistical view does not necessarily entail the acceptance of MP.

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Goat-stags, Chimeras and Other Fantastic Creatures. Empty Terms and Existential Import in Medieval Logic.

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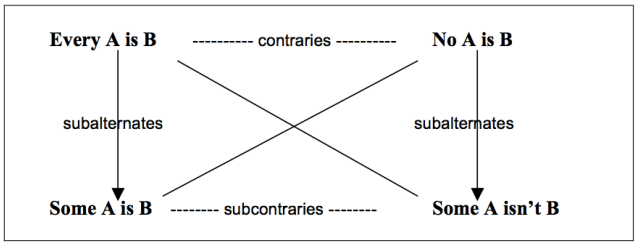
Keywords: Medieval Logic - Existential Import - Logically Impossible - Quantification - Square of Opposition

1 Aims and description

When reading medieval logical texts, it is frequent to find discussions involving an appeal to inexistent entities. These entities could be of many sorts: we may find reference to actually inexistent but possible entities, such as *Homerus* or *my future son*, as well as to only imaginable entities like *chimeras*, *goat-stags* or *golden mountains*, to entities that are naturally impossible but are employed in philosophy or sciences as useful conceptual tools, such as *instants of time* or *abstracted extensions*, and finally to entities that are not only naturally but also logically impossible, inasmuch as they are constituted by contradictory or incompossible parts, such as *dead men* and *rational stones*. In logical contexts, this appeal to inexistent entities raised a number of difficulties, mainly concerning the issues of existential assumptions in logical formulas and of the validity of usually accepted systems of inference in presence of non referring terms.

Some of the rules of inference that constituted the core of medieval logic - rules that are usually represented by means of the traditional Square of Opposition - seem to work unexpectedly in the case that some of the terms included in categorical propositions are empty. One



well-known problem is related to the proper interpretation of universal affirmative propositions like "Every A is B". Let us suppose a situation in which the term "A" has no actual referent. This means that the particular proposition "Some A is B" is false, and that its contradictory claim "No A is B" is true. But if this is the case, the universal affirmative claim "Every A is B" must be false, and therefore we have that there could be no true universal affirmative proposition whose subject is an empty term, and that the existence of the subject's referent is a necessary condition for the truth of propositions such as "Every A is B". From the point of view of contemporary logic this sounds odd, for propositions of this form are usually taken to be vacuously true in case their subject(s) is empty. A second issue concerns the interpretation of the particular negative proposition "Some A is not B". If we posit again a situation in which the term "A" fails to refer, we have that the particular affirmative proposition "Some A is B" is false, and that its contradictory "No A is B" is true. In virtue of the rules of subalternation, we should admit that the negative particular proposition "Some A is not B" is also true, which is

again strange to modern ears, for if we were to interpret particular propositions as containing an existential quantification, the existence of their subject's referents should be a necessary condition for their truth.

Apart from the the rules embodied in the Square of Opposition, there are several other laws of inferences that seem to be threatened by the presence of non referring terms, such as the rules of conversion by contraposition, the equipollence rules between possibility and necessity claims and some syllogistic forms. To explain how medieval logicians were able to overcome these difficulties, it is sometimes claimed that their logic implicitly admitted a number of existential assumptions, and particularly the assumption that all terms in categorical propositions referred to non-empty classes, and, in some cases, that all proper names have existing referents. This view is however far from obvious, and in some cases explicitly rejected by many medieval authors.

Just as contemporary logicians, medieval logicians were often worried about how to deal with non-denoting terms within their systems. They frequently admitted the presence of terms that refer to empty classes and of constants that fail to denote, and their texts show an explicit concern about how terms with no recognized denotation must be interpreted and how can propositions including them can be said to be meaningful. Moreover, they often developed sophisticated theories to distinguish between the inferences whose validity required existential assumptions and those whose validity is maintained in presence of non referring terms. The aim of this symposium is to investigate the problem of existential import in latin logical texts from the 11th century to the 14th century, and to highlight some interesting developments in the logical theories of the time that involved the reference to inexistent, abstract, imaginary or even impossible entities.

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3 Abstracts

• **"My future son is possible alive". Existential presupposition and empty names in Abelard's modal logic.** *Irene Binini*

As many other medieval logicians, Abaelard holds that all affirmative non modal propositions carry an implicit presupposition of the existence of their subjects. Many passages in Abelard's logical works show that he was well aware of the problems connected to the existential import and to the presence of empty names in categorical propositions. His distinction between internal negation and external negation - and the consequent reformulation of the traditional Square of Oppositions - seems to be motivated exactly by Abaelard's will to provide a system of logical relationships which is valid also for those propositions whose subjects fail to refer. But what about modal propositions? Does Abaelard consider the question of existential import also when he deals with modal affirmations? Do these propositions carry an implicit import just as simple propositions do? And again, if they do, is this consistent with the system of modal oppositions and equipollences Abaelard provides? In my paper, I argue that Abaelard does indeed conceive all de rebus affirmative modal propositions as having an implicit import, the satisfaction of which is a necessary condition for their truth. I also argue that Abaelard is consistent in maintaining this position both in the *Dialectica* and in the *Logica Ingredientibus*. However, once we have granted that Abaelard's modal propositions behave as simple ones with respect to their existential presupposition, we need to address a number of difficulties that threaten the validity of Abelard's modal system. I will show that Abaelard is conscious of these complications, and it is exactly because of them that he decides in the end to restrict the validity of his modal system only to those proposition whose term actually refer. Empty terms, which Abaelard struggled to take into account within his simple categorical logic, are then expelled from his modal system.

• **Peter Abelard on the existential import of modal sentences *de non esse*.** *Wojciech Wciórka*

One of the reasons that led Peter Abelard (d. 1142) to distinguish two types of negation (predicative and propositional) were his assumptions about the existential import. The so-called "separative" (*separativa*, *remotiva*) negation only "removes" the predicate from the subject, thus preserving the import introduced by the subject term. By contrast, the "extinguishing" (*exstinctiva*, *destructiva*) negation cancels the total content of the denied sentence, together with its import. In the paper, I reconstruct Abelard's views on the existential import *inside* modal affirmations *de non esse*, such as "It is possible/necessary for Socrates *not* to run". In doing so, I reveal Abelard's presuppositions about the negations inside the scope of modal operators in sentences involving modality *de re*. The problem arises once he paraphrases necessity statements ("It is necessary for S to be P") as conforming to the schema involving a "mode", i.e. "S is P {in such a way that it is not possible for S not to be P}". I argue that the "internal" negations should be interpreted as "extinctive" in all the cases. The argument in the case of possibility *de non esse* is based on assumptions about truth value: if the negation in "It is possible for Socrates not* to be a body" were construed as separative, the sentence would be rendered false, contrary to Abelard's mature view. The argument in the case of necessity depends on paraphrasing "It is necessary for Socrates not** to be a horse" as "Socrates [exists and lacks** horseness] {in such a way that it cannot be the case that Socrates lacks* lacking** horseness}. The reconstruction assumes the Abelardian distinction between the extinguishing "lacking" (*carere*) and the separative "being without" (*esse sine*).

- **Are universal terms fictitious terms?** *Caterina Tarlazzi*

This paper explores their relation, if any, between universal terms (e.g. 'man') and fictitious terms (e.g. 'chimaera') in treatises on universals and *Isagoge* commentaries from the time of Peter Abelard. If there is no *res subjecta* for universal terms, do universal terms run the risk of being fictitious? Are fictitous terms ever employed, in this context, to explain how universal terms work? Are fictitious terms employed to explain the abstraction involved in universals? Such questions will be addressed via Peter Abelard's texts and realist text, often unpublished.

- **Existential import in 13th century *sophisma* "Every man is by necessity an animal, there being no men".** *Julie Brumberg-Chaumont*

Problems raised by existential import when introduced in the medieval square of opposition has been tentatively solved by ascribing to universal propositions the same existential import (obviously) possessed by the subordinate particular and singular propositions, and by declaring negative empty propositions "vacuously true". The sophismatic literature of the thirteenth century about the *sophisma* "Every man is by necessity an animal, there being no men" shows that the idea that all particular and even all singular propositions would have an existential import is far form being obvious, and the same can be said about "vacuously true" negative propositions. As for the notion that universal propositions would have existential import, it was just condemned in Oxford in 1277. The truth of propositions such as "every man is by necessity an animal" or "every man is an animal" is independent, for many authors of the time, inside or outside sophismatic literature, from the existence of their subjects (*constatia subjecti*), as it is for their subordinate propositions; as for "no dodo is a bird", it remains a false proposition after the dodos have disappeared. Interestingly enough, we see this position defended at a very high cost, in two different directions: either by endangering the very possibility of a descent from universal to singular propositions, or by contending a paradoxical idea, namely that "Socrates is a man" is a more eternally true proposition than its corresponding universal. Both alternatives are based upon an original reflection about the semantics of proper names and their possible emptiness. The debate is connected to modal syllogistic problems widely discussed at the time.

- **Instants, chimeras, ass-men, and other imaginable things: Marsilius of Inghen on *imaginabilia*.** *Graziana Ciola*

In the second half of the XIV century, both in Logic and Natural Philosophy, we find some interesting developments involving an appeal to different sorts of imaginary entities (*imaginabilia*). I am going to focus on some passages in Marsilius of Inghen's works dealing with terms signifying impossible inexistent entities - be they naturally or logically impossible. My aim is to reconstruct Marsilius' account of *imaginabilia* and to outline its consequences on Marsilius' logical theories - in particular for some aspects of his treatment of signification, the validity of *consequentiae* and some cases of *expositiones*. I will proceed by making a comparison with Albert of Saxony and John Buridan. In the first place, I will analyse Marsilius' account of expressions such as *hominem esse asinum* - e.g. in *Consequentiae* I.2. I am going to outline the consequences of such account for Marsilius' conception of signification and its role in a particularly important argument justifying Marsilius' preferred criterion of validity. In the second place, I will examine what Marsilius has to say about terms signifying entities composed by incompossible contradictory parts, as e.g. *chimaera*. Are such entities imaginable or not? Marsilius' stance on the subject seem to have changed in time - for example between his *Quaestiones super Peri Hermeneias* and his treatises on *Ampliationes* and *Consequentiae*. Overall, Marsilius' take on chimaera shows some relevant differences with Buridan's as it is

presented, for example, in his *Sophismata*. Furthermore, I will analyse Marsilius' use of those *imaginabilia* that are convenient abstractions, conceptual tools having no real existence but which are useful to run some kinds of logical analyses. For example, Marsilius defines instants of time and abstracted extensions as *imaginabilia* of this sort - e.g. in the second book of his *Consequentiae*.

Logical Pluralism: Foundations and Applications

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1 Aims and description

Logical pluralism is the view according to which there is more than one correct logic. Given that there are various ways to define what a logic is and when a logic is correct, there are, at least in principle, many forms of logical pluralism. Yet, not for every combination, the resulting form of logical pluralism is interesting, or true. For example, a quite uninteresting version of logical pluralism would come out, if one treats logics as mere uninterpreted formal systems. More interesting versions of the view usually assume that logics are interpreted systems and that different logical systems differ in the interpretation of at least some parts of the object language (i.e., the meaning of logical constants) or of the meta-language (i.e., the notion of validity).

Logical pluralism brings with it at least the promise of an irenic recomposition of disagreements in logic. In effect, it could also be presented as the view according to which the reality of disputes in logic is illusory and should be reconceptualised as non-factual (WIlliamson, 2014; Field, 2009). This conclusion contrasts with a view of logic according to which disputes between alternative logics, or metalogics, is real and should be dealt with by means abductive methodologies typical of other parts of science (Williamson, 2013).

The recent interest in the view has been mainly generated by the work of J.C. Beall and G. Restall. According to (Beall and Restall, 2006), logical pluralism is a thesis about logical consequence, namely that there is more than one correct notion of logical consequence. More precisely, for Beall & Restall, logical pluralism is the view that there is more than one admissible precisification of a certain conception of logical consequence, whose settled core is the idea of preservation of truth in all cases. A precisification of the notion of logical consequence is a precisification of the concept of case in the schematic definition of validity (GTT); a precisification is admissible, if it satisfies the criteria of necessity, formality and normativity; the assumption is that to an admissible precisification of a notion of consequence it corresponds a logic.

The contemporary debate on this kind of logical pluralism has revived some older versions of the view such as the one defended by Carnap (1937) or by A. Varzi (2002). More recently, other forms of logical pluralism has been defended by G. Russell (2008) or S. Shapiro (2014).

The symposium will be organised in two sections: in the first we are going to discuss some general foundational issues for logical pluralism; in the second, we are going to consider some real applications of pluralism to logic.

Aim of the first part is, in particular, to discuss the relations between logical pluralism and normativity (Ferrari & Moruzzi), the question of what meta-logic should the logical pluralists use to argue for their position (Sereni & Sforza Fogliani) and the relevance of the Kripkes adoption problem for Beall & Restalls version of logical pluralism (De Florio & Morato).

Aim of the second part is, in particular, to discuss approaches to negation which are somehow either grounded on a pluralist conception of logic or are compatible with a pluralist attitude (Canavotto & Giordani), many-valued logical systems that allow to express formulas with classical values (Carrara & Ciuni), the role of paradoxes in developing anti-exceptional forms of logical pluralism (Nicolai), and, finally, the relations between logical pluralism and truth theories (Picollo & Schindler).

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3 Duration

Half a day

4 Abstracts

4.1 Foundations

- **Adopting Pluralism?** *Ciro De Florio and Vittorio Morato*

In a series of unpublished lectures (partly reconstructed in Padro 2015), Kripke defends the idea that logical principles cannot be adopted, or better that one cannot adopt a logical principle which does not already belong to her logical framework. For example, UI (universal instantiation) cannot be adopted, unless one already infers in accordance with UI. This problem is called by Kripke, the adoption problem. We believe that the adoption problem has relevant consequences for logical pluralism. According to Beall & Restalls version of logical pluralism, there are at least two correct definitions of logical consequence (i.e., there are at least two admissible precisifications of the Generalized Tarskis Thesis (GTT)) and a logical pluralist one who can endorse more than one. For example, a fan of constructive logic can endorse classical logic without abandoning her endorsement of constructive logic. Our aim is to show that the endorsement of a logical consequence and the adoption of a logical principle are two strictly connected notions and thus that, if there is an adoption problem, there is also an endorsement problem. According to the endorsement problem, relations of logical consequences cannot be adopted or, better one cannot endorse a logical consequence which does not already belong to her logical framework. In our paper, we show how the notion of endorsement (of a logical consequence) and that of adoption (of a logical principle) are connected and discuss what consequences the endorsement problem has for logical pluralism.

- **Who Watches the Watchmen? Some Metatheoretical Challenges for Logical Pluralism.** *Andrea Sereni and Maria Paola Sforza Fogliani*

We investigate some fundamental, though underexplored, metatheoretical issues regarding logical pluralism (LP), focusing on the following question:

(Q) How many logics are logical pluralists using when arguing for LP?

After showing how (Q) is prompted by the well-known the Centrality Argument [Putnam, 1978], we discuss three strategies for reply: (a) no logic, (b) one single logic, (c) more than one logic.

We argue that neither (a) what Beall and Restall [2001; cf. also 2006] opted for nor (b) are defendable. As a way out, we explore a form of modest pluralism; this requires clarifying how logics should be in order to be acceptable for a pluralist (correct/legitimate or true). Shapiro [2014] suggests that his argument for LP is non-deductive, being rather an instance of Inference to the Best Explanation (IBE); but since IBE can collide with Bayesian Confirmation Theory, LP for inductive logics must be either turned down, or further defended. In the context of an abductive metatheory for LP, we also assess the prospects of anti-exceptionalism [Priest 2014, Williamson 2015, Hjortland, 2016]. Finally, (c) amounts to:

(LP') We can argue for LP using different logics.

But then, how is one to defend (LP')? The same options open up again; assuming the third strategy is chosen, an infinite regress threatens. We thus submit that logical pluralists, however they choose to answer to (Q), face hardly surmountable challenges. This threatens the coherence of current and future versions of LP.

- **Logical Pluralism, Indeterminacy, and the Normativity of Logic.** *Filippo Ferrari and Sebastiano Moruzzi*

In this talk we clarify the nature of Beall and Restall's (2006) logical pluralism and we explore its consequences for the normativity of logic. First, we argue that Beall and Restall's logical pluralism is an indeterminacy pluralismi.e. a pluralism about the notion of logical consequence that is characterised by means of a thesis of semantic indeterminacy for the folk concept of logical consequence. Second, we provide three ways of modelling indeterminacy pluralism that seem most consonant with Beall and Restall's indeterminacy claimi.e. standard supervaluationism, non-standard supervaluationism, and subvaluationism. Third, we argue that these different models have different consequences for the normative status of controversial validity claimsi.e. those claims about the validity of arguments that are valid in one, or more, but not all the logics admissible within Beall and Restall's framework. Fourth, we argue that all of the formulations of indeterminacy pluralism discussed in this talk face a problem in accounting for the intuitive normative status of the folk concept of logical validity.

4.2 Applications

- **Classical Recapture in Many-valued Logic** *Massimiliano Carrara and Roberto Ciuni*

One debated problem in philosophical applications of many-valued logics is classical recapture: How can we secure inference of classical conclusions, under the assumption that our premises involve no abnormal phenomenon such as logical paradoxes or vagueness? Two established approaches to this problem are the so-called 'classical collapse' by Beall and 'minimal inconsistency' by Priest. This paper considers a third approach, extending many-valued systems with a normality operator. This enables us to express that a formula A has a classical value. We establish a classical recapture result and compare our approach to the methods of classical collapse and minimal inconsistency.

- **Negation as Conceptual Exclusion** *Alessandro Giordani and Ilaria Canavotto*

The aim of the present talk is threefold: (i) to develop an axiomatic conception of negation; (ii) to identify the basic principles concerning such a connective given the conception introduced; (iii) to discuss logical pluralism as generated by reflections on the concept of negation. Here negation is conceived of as a operator on propositional contents corresponding to a symmetric relation of conceptual exclusion between propositions. The resulting framework, which will be shown to be consistent both with the Aristotelian account of the notion of falsity and with the Carnapian analysis of the notion of logical falsity, will allow us to identify two basic logics of negation, whose justification hangs on the way in which the concept of truth is considered. To be sure, logical pluralism about negation will be shown to be dependent on the possibility of assigning in a principled way a truth value to all the propositions. Finally, we will compare our assessment with the semantic account of negation as a modal operator.

- **Abductivism and the standard for logical revision** *Carlo Nicolai*

According to the main tenet of anti-exceptionalism about logic, logical beliefs are revised in the same way as scientific beliefs are revised, namely following a holistic and broadly abductive methodology. Several authors have recently defended an anti-exceptionalist position

concerning logic and defended both logical monism and pluralism on abductivist grounds. A common theme is that the debate around semantic paradoxes offers a clear arena to test this methodology. Surprisingly enough – given the methodological assumptions – these claims are not corroborated by adequate abductive analyses. In this paper we provide one by resorting to recent studies in formal theories of truth. The upshot of the analysis is that there are fundamental ambiguities in interpreting the role and scope of solutions to semantic paradoxes that compromise the very applicability of abductivism to the comparison and evaluation of rival logics. Modern anti-exceptionalism, it seems, cannot count on its most reliable source of information.

- **Deflationism and Conservativity** *Lavinia Picollo and Thomas Schindler*

A wide variety of claims are often associated with deflationism about truth, including the idea that truth is metaphysically thin, not a substantial property. This has been interpreted by Shapiro, Ketland, Field, and others as asserting that truth has no explanatory power, which in turn, has been taken to mean that axiomatic truth theories should be conservative over their respective base systems. This is problematic as many intuitively appealing truth theories violate the conservativity requirement. As a consequence, the deflationist position is often deemed untenable. We argue that this line of reasoning is fundamentally misguided, as it draws upon a misconception of deflationism. We first provide a historical account of deflationism, based on which we put forward a rational reconstruction of the actual position. According to this rational reconstruction, the main theme of deflationism is that the truth predicates only purpose in natural language is to emulate higher-order quantification within first-order single-sorted languages. We argue that metaphors such as truth is not a substantial property are meant to emphasise that truth is a property that is expressed by a mere logical or quasi-logical (depending on how higher-order quantification is to be understood) expressive devise, and nothing more. As a consequence, we should not rush into concluding that truth cannot have any explanatory power, or that axiomatic truth theories must be conservative over their corresponding base systems. We maintain that, according to deflationism, truth should have as much explanatory power as higher-order logics do, and truth theories should be as conservative as higher-order logics are. In the light of the well-known results of non-conservativity of, e.g. second-order arithmetic over its first-order counterpart, we conclude that deflationists are not committed in any way to adopting conservative theories of truth.

Relations in physics and metaphysics

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Keywords: Relations, Relational Interpretation of Quantum Mechanics, Black Holes, Entanglement, Structural Realism, Wholes

1 Aims and description

The aim of the symposium "Relations in Physics and Metaphysics" is to bring together philosophers to discuss relations through the complementary lenses of physics and metaphysics.

Relations have played a role in the interpretation of physics, at least in spirit, as far back as Niels Bohr (Bohr, 1937), (Bohr, 1958), though it wasn't until Hugh Everett's (Everett, 1957) relative state formulation that relations played a significant role in the interpretation of quantum mechanics. More recently Carlo Rovelli (Rovelli, 1996) and Simon Saunders (Saunders, 1996), (Saunders, 1998) have independently developed interpretations of quantum mechanics that depend upon relations. In his presentation, "Bohr's Contextualism, Rovelli's Relationalism and Quantum Spacetime", Mauro Dorato will bridge this chronological divide and bring Rovelli's relational interpretation to bear in helping to understand and disambiguate some of the ideas of Bohr. He will then consider the question of whether treating spacetime as a non-classical entity decides against Bohr's position.

Treating spacetime as dynamical is the framework for Vincent Lam's presentation, "Global and Relational Aspects of Black Holes". In it, he focuses on the relational aspects of localization and the global nature of mass-energy. He discusses the difficulties linked to the 'quasi-local' characterization of general black holes—and of black hole horizons in particular. Lam's treatment of relations from the perspective of general relativity nicely complements the discussions that work from the context of quantum mechanics.

In this symposium, quantum mechanics again appears in David Glick's presentation "Swapping Something Real: Entanglement Swapping and Entanglement Realism". In this presentation, Glick argues that experimentally demonstrated cases of entanglement swapping do not undermine the realist position that there are entanglement relations between timelike separated regions. While he does not endorse this position, he does argue that adopting such a position for the realist allows her to avoid certain difficulties that are presented to those who deny such swapping is genuine.

Physics, especially quantum mechanics, has also played a role in justifying metaphysics that feature fundamental relations. For example, Steven French has used quantum mechanics to show that there are relations not reducible to non-relational properties of their relata (French, 1989a), (French, 1989b), Michael Esfeld has argued that we ought to consider a metaphysics built out of fundamental relations (Esfeld, 2001), (Esfeld, 2003), (Esfeld, 2004), (Esfeld, 2016), and metaphysical pictures like structural realism (Ladyman & Ross, 2007), (Ladyman, 2016a), (Briceño & Mumford, 2016), and priority monism (Schaffer, 2010), (Schaffer & Ismael, 2016) have ascended. For this symposium Federico Laudisa will present, "Is There a Metaphysical Framework for Relational Quantum Mechanics?" in which he considers what metaphysical framework might best match the original motivations of Rovelli's relational interpretation of quantum mechanics.

One metaphysical framework that has been much discussed is that of structural realism. Structural realism aims to define individuals relationally, but as Philipp Blum points out in his "Structuralism and Relational Individuation", most proponents of the view have not spelled

out how they are to get from relational individuation to the claim that individuals are "nothing over and above nodes in a structure". Blum will consider some of these possible arguments and draw parallels between them and arguments in philosophy of mathematics, concluding that the prospects for structural realists in this regard are not hopeful.

Rounding out the symposium, F.A. Muller's presentation, "Parts of the Whole Story" will consider various kinds of wholes, criteria for them, how they are logically related, and whether some can be reduced to others.

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3 Abstracts

- **Bohr's Contextualism, Rovelli's Relationalism and Quantum Spacetime.**

Mauro Dorato

In my presentation I will compare Bohr's contextual view of the relation between macroscopic instruments and quantum systems with Rovelli's interpretation of quantum mechanics. I claim that Rovelli's view helps to disambiguate some hard-to-understand philosophical positions held by Bohr, while, at the same time, vindicating some of his views. Exactly as Rovelli, Bohr is explicitly against a subjective or mind-dependent interpretation of the measurement interaction. However, at least prima facie, in order to attribute definite properties to measurement outcomes Bohr, unlike Rovelli, seems to require a sharp distinction between the quantum and the classical realm. As a matter of fact, to the extent that Bohr, exactly as Rovelli, can contextually treats quantum systems as measurement apparata (as advocated by Zinkernagel, (Zinkernagel, 2008), (Zinkernagel, 2016), (Zinkernagel & Rugh, 2016)), he can defend the legitimacy of treating spacetime as a non-classical entity. In Rovelli relationism in fact, it is not possible to describe the universe from an "external" perspective, since any attribution

of a quantum state to the universe presupposes a particular perspective *within* the universe. In this respect, the question to be explored is whether the need to consider spacetime as a non-classical entity definitely rules out Bohr's interpretation, in view of the non-existence of a sharp classical-quantum divide. However, Bohr's position could be vindicated by the problem of attributing some meaning to time the beginning of the universe a question that calls into play the measurability of one physical system by another, and therefore some kind of temporal relationism. Comparing this view of time with Rovelli's will conclude my presentation.

- **Global and Relational Aspects of Black Holes.** *Vincent Lam*

In the context of general relativity, this contribution aims to highlight the global and relational features of black holes, which are the fundamental gravitational entities of the theory, in the light of the dynamical nature of spacetime. We first consider the relational aspects of the very notions of localization and of mass-energy within the general relativistic framework. A radical consequence of spacetime itself being considered as dynamical is that spacetime localization amounts to localization with respect to a dynamical entity that has no privileged dynamical status in general: localization is to be understood in terms of the correlations among the dynamical entities of the theory rather than with respect to some fixed, non-dynamical background. In contrast, in this framework, mass and energy are only unambiguously defined with respect—in relation—to background symmetries, which are absent in a dynamical context. These latter symmetries can be asymptotic (e.g. for isolated systems), in which case mass and energy so defined have a global nature. So-called 'quasi-localization' of mass and energy to extended but finite spacetime regions proves to be difficult and further highlights their dependence on background (gauge) structures such as some particular embedding. In this perspective, we discuss the global and relational aspects of the difficulties linked to the 'quasi-local' characterization of general black holes—and of black hole horizons in particular.

- **Swapping Something Real: Entanglement Swapping and Entanglement Realism.** *David Glick*

Experiments demonstrating entanglement swapping have been alleged to challenge realism about entanglement. Seevinck (Seevinck, 2006) claims that entanglement "cannot be considered ontologically robust" while Healey (Healey, 2012) claims that entanglement swapping "undermines the idea that ascribing an entangled state to quantum systems is a way of representing some new, non-classical, physical relation between them." My aim in this paper is to show that realism is not threatened by the possibility of entanglement swapping, but rather, it should be informed by the phenomenon. I argue—expanding the argument of Timpson and Brown (Timpson & Brown, 2010)—that ordinary entanglement swapping cases present no new challenges for the realist. With respect to the delayed-choice variant discussed by Healey, I claim that there are two options available to the realist: (a) deny these are cases of genuine swapping (following Egg (Egg, 2013)) or (b) allow for the existence of entanglement relations between timelike separated regions. This latter option, while radical, is not incoherent and has been suggested in quite different contexts. While I stop short of claiming that the realist must take this option, doing so allows one to avoid certain costs associated with Egg's account. I conclude by noting several important implications of entanglement swapping for how one thinks of entanglement relations more generally.

- **Is There a Metaphysical Framework for Relational Quantum Mechanics?**
Federico Laudisa
Relational quantum mechanics is an interpretation of quantum theory which discards the notions of absolute state of a system, absolute value of its physical quantities, or absolute event ((Rovelli, 1996), (Laudisa, 2001), (Laudisa & Rovelli, 2013)). The theory describes the way systems affect one another in the course of physical interactions and the notions of state and physical quantities refer always to the interaction, or the relation, between two systems. Nevertheless, the theory is assumed to be complete and the physical content of quantum theory is understood as expressing the net of relations connecting all different physical systems. As a matter of fact, the original relational interpretation of quantum mechanics (Rovelli, 1996) was put forward in a quite operational spirit, namely to adapt the standard interpretation of quantum theory to the quantum gravity research program in a way that was thought to minimize the divergence from the Copenhagen formulation. Given the development of a rich analysis of relations in physics by the metaphysical point of view in the last years, especially in the area of the *structural realism* program (Ladyman, 2016b), the aim of my talk is to investigate what might be the metaphysical framework that best matches the original motivations of the relational interpretation of quantum mechanics itself.

- **Structuralism and Relational Individuation** *Philipp Blum*
Recent and not-so-recent brands of structuralism have been motivated by the need to relationally individuate physical entities. The precise form of the argument from relational individuation to being 'nothing over and above' nodes in a structure has not been spelt out. I discuss critically several forms of such an argument, contrast them with parallel cases in the philosophy of mathematics and conclude that the prospects for a distinctly physical form of physicalism do not look good.

- **Parts of the Whole Story.** *F.A. Muller*
We discern a number of kinds of wholes, and attempt to propose criteria for these kinds of wholes. For physical, or natural, wholes, we argue that the key is physical interaction. For conventional, or social, wholes, the key is (surprise surprise) a convention. For pragmatic wholes, the key is a concept from ecological psychology: accordances. We inquire also how they are logically related, and whether some kinds can be reduced to others. For some kinds, Universalism lurks, for others it doesn't.

Proof-theoretic semantics and the justification of logical laws

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1 Aims and description

Traditionally semantics has been denotational: according to the model theoretic view, meanings are denotations of linguistic entities. Given a proof system, a consequence is logically valid if it preserves truth from its premises to its conclusion, with respect to all interpretations. Proof-theoretic semantics proceeds the other way round, assigning proofs an autonomous semantic role rather than explaining this role in terms of truth transmission. As a consequence, the emphasis is shifted from truth conditions to assertability conditions. As (Kahle and Schroeder-Heister, 2006) put it: "In proof-theoretic semantics, proofs are not merely treated as syntactic objects as in Hilbert's formalist philosophy of mathematics, but as entities in terms of which meaning and logical consequence can be explained" (p. 503).

The programme of proof-theoretic semantics can be traced back to (Gentzen, 1935). Gentzen famously proposed that the introduction rules of natural deduction could be seen as providing the definitions of the logical connectives, with the elimination rules simply a consequence of those definitions. Gentzen's idea was first explored philosophically by (Prawitz, 1974) and (Dummett, 1991): they both considered the possibility of justifying the logical laws on proof-theoretic basis. More precisely, Dummett and Prawitz proposed that the logical laws can be justified through an analysis of the meaning of the connectives expressed by inferential rules. But which laws determine the meanings of the connectives? Which inference rules are meaning-conferring? It is intuitively clear that one must be able to discriminate among supposed proofs; otherwise every proof-system, including trivial ones in which every formula is a theorem (such as the famous system in (Prior, 1960) for the connective tonk), will be equally acceptable. Recent work in proof-theoretic semantics has sought suitable constraints on the basis of inferential considerations.

(Dummett, 1991) explored Gentzen's idea in depth and called *harmony* the requirement that there should exists an equilibrium between the conditions under which an operator can be introduced and the conditions stating which consequences can be inferred from the use of the operator. Prawitz proposed a formalization of harmony by appealing to a local form of normalization in natural deduction proofs through the so called inversion principle. Dummett also considered an alternative possibility, that meaning is fixed by the elimination rules. Another famous investigation of the justification of the logical laws is (Martin-Löf, 1996) which assign a meaning-theoretic role to the fundamental notion of judgment. One common denominator of all of the above-mentioned proposals is their revisionism with respect to classical logic: Dummett, Prawitz, and Martin-Löf (among others) think that the decision which logic is the justified one goes in favor of intuitionist logic. Recently this conclusion has been challenged from both a logical and a methodological perspective.

The main aim of the symposium is to reevaluate the current debates on proof-theoretic justification of logical laws and to put them in the broader context of inferentialism. To do this, the symposium explores the following questions: which is the correct formalization of harmony (e.g. verificationist/pragmatist, deductive equilibrium, extensional/intensional, etc.)? Besides harmony, what are the additional conditions a sound justification procedure should take into account (e.g. stability, complexity, compositionality, etc.)? Does the proof-theoretic

justification give rise to a monist or pluralist position? Which logics are justified and which logics are not (e.g. classical, relevant, modal, etc.)?

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3 Abstracts

- **In defense of classical logic.** *Norbert Gratzl*

Classical logic is a success story. Having said this there is no lack of criticism. One major critical point is that in some formalizations of classical logic, foremost Gentzen's LK (and its kin), do have multiple conclusions. This talk consists essentially of two major building

blocks: the first one is to discuss both some aspects of reasoning as formalized in classical logic and some aspects of proof-theoretic semantics instantiated by Gentzen's LK-systems. The second major building block contains (a) the propositional part and (b) a quantificational part of a single conclusion sequent calculus based on a hypersequents-extension of Gentzen's LK, coined GKS.

- **Mereological pluralism.** *Paolo Maffezioli*

Like any formal theory, mereology consists of logical axioms and proper axioms. Over the years, philosophical reasons have motivated interest in departing from the traditional framework and nding alternative axioms for mereology. In particular, proper axioms such as the principle of strong supplementation or the principle of unrestricted composition have been weaken or rejected altogether; and even seemingly innocent axioms like the anti-symmetry of the parthood relation are not unanimously regarded as unproblematic. Interestingly, logical axioms have been challenged too. For instance, free logic and plural quantification have traditionally been considered as a valid alternative to classical logic and, more recently, a mereology based on a paraconsistent logic has been proposed. Thus, in mereology there seems to be a pluralism about logical axioms and a pluralism about proper axioms: one can change the proper axioms while keeping the underlying logic; or one can change the logic and maintain the proper axioms. The two forms of pluralism disagree on which group of axioms should be revised but agree on the fact that once one has been changed the other should remain the same. There is, however, a more revisionist option which I shall discuss in this work and consists in changing both the logical and proper axioms. A notable example of such a revisionism is the constructive theory of real numbers in which the choice of intuitionistic logic yields a rejection not only of classically valid proper axioms, such as the trichotomy of strict linear orders, but also of the very notion of equality (to be replaced by apartness). While both the existing forms of mereological pluralism implicitly assume that the logical and the non-logical part of a formal theory are largely independent from each other, I will try to defend the thesis that in fact they are arguably more connected than they look.

Part of this work is a joint work with Achille Varzi.

- **Proof-theoretic semantics for conditionals and non-normal modalities.** *Sara Negri*

In this talk we shall give an overview of a method that bridges between generalizations of possible worlds semantics and the creation of analytic proof systems for conditionals and non-normal modalities.

- **Harmony, Stability and Relevant Logic.** *Hermógenes Oliveira*

The notion of harmony, i.e. the equilibrium between the inference rules governing the use of a logical constant, is a cornerstone of proof-theoretic semantics. This notion is often formulated as a requirement that the elimination rules should not extrapolate the meaning conferred by the introduction rules. Furthermore, in order to obtain a more strict equilibrium and avoid what is sometimes called "weak disharmony", a complementary requirement is often imposed: the elimination rules should fully exploit the meaning conferred by the introduction rules. Borrowing terminology from Dummett, this complemented notion is sometimes called "stability".

Dummett and Tennant have suggested to interpret stability extensionally by means of a maximality requirement: the stable rules are the strongest ones w.r.t derivability. This would rule out connectives like *knot*, the dual of *tonk*, but also substructural connectives like quantum disjunction and relevant implication. While we may consider appropriate the exclusion of quantum disjunction on the grounds that it is inspired by extra-logical considerations from

quantum mechanics, relevant implication, on the other hand, seems to arise from purely logical considerations, as made explicit in the paradoxes of material implication.

In Dummett's original discussion, the notion of stability does not describe a criterion for pairs of introduction/elimination rules for a single constant. Instead, stability describes a global criterion to evaluate inferential contexts. Accordingly, Dummett developed his notion of stability by appealing to his justification procedures. We indicate how we can control structural features, like contraction and weakening, by making slight modifications to the justification procedures. Using this device, we try to develop a proof-theoretic approach to relevant logic (which restricts weakening) by means of a modified version of Dummett's pragmatist justification procedure.

(Joint work with Eugenio Orlandelli and Mattia Petrolo)

- **On propositional variables: the atomic and the parametric view.** *Paolo Pistone*

What consequences are we entitled to draw from a proof that $p \Rightarrow p$ (where p is a propositional variable)? The standard answer, coming from model-theory, is that every interpretation $[\![p]\!]$ of p will obey the truth-table of implication. However, when considering proof-theoretic interpretations, quite different answers might be found in (quite different) literatures.

In Prawitz's and Dummett's proof-theoretical interpretation, a proof of $p \Rightarrow p$ warrants that a canonical argument for the interpreted statement can be found for a special class of interpretations only, i.e. those associating with propositional variables so-called *atomic bases*, i.e. sets of atomic inferential rules. Indeed, in this approach, if every interpretation were admitted, a vicious circle would result in the definition. This apparent limitation is grounded in the view (we call it the *atomic view*) that proof conditions must be explained in a hierarchical way, with simple (atomic) propositions grounding complex ones.

Such limitations do not appear in the proof-theoretic interpretations of polymorphism (i.e. of second order quantification): by defining the interpretation in a relational frame one can express the fact that a variable p figures as a parameter in the proof, and hence that it can be replaced by any interpretation, yielding a canonical argument in a uniform way. This *parametric view* does not demand for a hierarchical explanation of logical consequence, but takes propositional variables as free parameters in the proofs.

I will argue that the latter view, far from concerning second order logic only, provides a perspicuous picture of proofs in propositional logic. Indeed, parametricity expresses a *naturality condition* (in the sense of category theory) for proofs which, on the one side, provides significant information concerning the identity of proofs (not obtainable from usual $\beta\eta$-equivalences) and, on the other side, allows to characterize correct proofs by purely semantical means, yielding several completeness results for intuitionistic propositional logic, a problematic issue in the atomic view.

- **General Elimination Harmony.** *Marcus Rossberg*

The proposal for a proof-theoretic semantics for logical constants goes back to Gerhard Gentzen. Gentzen proposed that a logical constant is defined by its introduction-rule; its elimination-rule, in turn, is supposed to "follow" (in some sense) from the introduction rule. Michael Dummett calls pairs of rules that indeed exhibit this balance "harmonious". Jan von Plato, Stephen Read, Roy Dyckhoff, Nissim Francez, and others, have recently proposed that so-called "generalized elimination-rules" provide a mechanical procedure to construct elimination-rules of the appropriate strength for any given introduction-rule. This paper investigates potential problems regarding this procedure.

- **Harmony, Stability and Identity: Some lessons from Martin-Löf's type theory.** *Luca Tranchini*

The starting point of our talk is the observation that in the context of Martin-Löf type theory (MLTT) the notions of harmony and stability correspond to the adoption of the two kinds of rules of definitional equality that characterize (respectively) the intensional and extensional versions of MLTT. We argue that the perspective of MLTT helps clarifying two important aspects of harmony and stability.

First, although harmony and stability cannot be taken as criteria of meaningfulness (and thus it cannot be expected that they are satisfied by any meaningful expression whatsoever), they are criteria which are satisfied not just by logical constants, but by a richer class of expressions, namely those which lend themselves to be inductively characterized.

Second, the notions of harmony and stability cannot be treated on a par, on pain of trivializing the notion of equivalence between proofs. To avoid such a trivialization, the notion of stability must be reinterpreted as being subsidiary with respect to harmony.

We conclude remarking that such an asymmetric treatment of harmony and stability is encoded by the intensional version of MLTT, which is at the core of the current attempt at providing a novel foundations of mathematics on the basis of homothopy teory. We thereby argue that the inferentialist stance may help in giving a meaning-theoretical justification of homotopy type theory.

(Joint work with Alberto Naibo)

CONTRIBUTED TALKS

Proof diagrams: another parallel syntax for proof-theory

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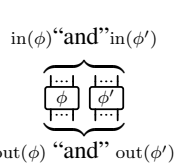
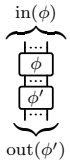
Keywords: String diagrams rewriting, Linear logic, Proof semantics.

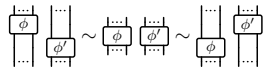
In the last century, several sciences enriched their syntax in order to model interactions. *String diagrams* are suitable for that purpose (Baez, 2009; Selinger, 2009). In this syntax, we have two compositions: the *parallel* one and the *sequential* one, which may interact by the *interchange rule*. If we consider this rule as an equality, string diagrams are a syntax for *strict monoidal categories*. This 2-dimensional representation of terms is able to capture the notion of concurrence in a more intuitive way with respect to traditional "in line" formulas Burroni (1993).



(a) Parallel composition



(b) Sequential composition (if $\mathrm{in}(\phi') = \mathrm{out}(\phi)$)



(c) Interchange rule

Proof nets are a way to represent proofs for *linear logic* (Girard, 1996). Firstly introduced for the *multiplicative fragment*, they give a graphical representation of derivations capturing a part of the proof semantics: equivalent derivations are represented by the same proof net. Even if proof nets provide a graphical cut-elimination procedure and a linear correctness criterion, some of these features get lost when we extend them in order to represent units and exponentials. In Acclavio (2016) is given an alternative 2-dimensional syntax for multiplicative linear logic derivations. The syntax of string diagrams authorizes the definition of a framework where the sequentializability of a term, i.e. deciding whether the term corresponds to a correct derivation, can be verified in linear time by only checking diagram inputs and outputs.

Aim of the present work is to enrich the syntax of proof diagrams by some terminating rewritings in order to recover a correspondence between equivalent diagrams and equivalent proofs. We conclude defining a denotational semantics for multiplicative linear logic with units by means of equivalence classes of proof diagrams.

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Logic of gauge

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Keywords: gauge theory, Weyl, logic

The logic of gauge theory is considered by tracing its development from general relativity GR (Einstein, 1916) to Yang-Mills theory YM (Yang & Mills, 1954) through Weyl's two gauge theories W18 (Weyl, 1918), W29 (Weyl, 1929a,b,c). A handful of elements—which for want of better terms can be called *geometrical justice* GJ, *matter wave* MW, *second clock effect* SC, *twice too many energy levels* EL—are enough to produce Weyl's second theory; and from there, all that's needed to reach the Yang-Mills formalism is a *non-Abelian structure group* NA (say $\mathbb{SU}(N)$). In short, I try to answer the question(s) "How did we get gauge theory? What's the logic of the historical process that produced such a theory?" It makes sense to begin with GR, and I have chosen not to go beyond YM, concentrating most of my attention on the transition from W18 to W29.

GJ is enough to produce W18 (gravity & electricity) from GR (just gravity). The quantum revolution added a third ingredient, MW (or just "matter"), which Weyl had to include. Einstein's objection, SC, led to a new gauge relation (1929), between electricity & matter, replacing the old one (1918) between electricity & gravity. The Aharonov-Bohm effect (involving electricity & matter) is the 'material' version of SC (involving electricity & gravity).

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What is the place of genetic drift in the Price equation?

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Keywords: Genetic Drift, Price Equation, Formal Darwinism Project.

The Price equation plays a prominent role in biology as it provides a formal model with which to capture a wide range of phenomena. Given its abstract and complete character, we expect the Price equation to easily account for drift as well; however, due to the different and not always coherent meanings often assigned to this concept, there is no universal agreement on where drift should be placed in the equation.

As a matter of fact, some authors interpret drift simply as an inevitable statistical error that makes real phenomena deviate from their theoretical path, while others consider drift as an autonomous process or even an evolutionary force comparable to natural selection. Consequently, the side of the Price equation in which drift should be located has not yet been properly clarified: each notion of drift has its place in a different locus in the Price equation.

To illustrate this point, we analyse Alan Grafen's Formal Darwinism Project, which refers to different interpretations of drift and, as a consequence, puts drift alternatively on both sides of the Price equation. We consider that this fact, far from showing an internal contradiction in Grafen's project, is an inevitable outcome of a loose definition of drift.

In this talk, we introduce the Price equation and explain its role in contemporary biology. We then present the different concepts of drift through an extensive review of proposals to be found in the specialised literature. We analyse some examples on how drift is considered in relation to the Price equation and, in particular, we focus on the formal treatment of drift in two articles by Alan Grafen, as an example of how the definition of drift (although implicit to a certain degree in these cases) impacts its formalisation and the very scope of the Price equation in evolutionary biology.

An improved essentiality requirement for deployment realism

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Keywords: Deployment realism; essentiality; Psillos; Lyons; prospective identification.

Psillos' "deployment" realism is committed only to the theory components which are *essential* in deriving novel predictions. H is essential when

(1) A novel prediction NP follows from H, together with the rest of the theory RT and supplementary assumptions A, but not from RT+A alone;
(2) there is no available alternative hypothesis H* which is (a) compatible with RT and A, (b) non-*ad hoc*, and (c) potentially explanatory, such that (H*+RT+A)→NP.

Lyons (2006) argued that this definition doesn't work because:

(i) it is too vague to be applicable to any historical case: e.g., in (2) it is unclear in which sense and when H* should not be available, what "potentially explanatory" means, etc.;
(ii) too many hypotheses (including false ones) would qualify as *essential*, because typical real life competitors are not compatible with RT and A, and they are *ad hoc* in Psillos' sense.

Hence, he suggested that deployment realists abandon the essentiality requirement altogether, crediting all the components which were actually employed in deriving novel predictions. But this would be a *hara-kiri* move, since history is full with hypotheses which were actually employed in deriving novel predictions and subsequently found to be false. In fact, I argue, the essentiality condition is necessary, since it stems from Occam's principle that we should assume only what is strictly necessary to explain a phenomenon. Therefore, instead of discarding conditions (1) and (2) we can replace (2) with a better working condition:

(2') H cannot be weakened to H' such that H→H' and (H'+RT+A)→NP.

If NP is risky and H fulfills (1) and (2'), most probably H is true, and any alternative H* is false. In fact, the rate of false hypotheses entailing novel risky consequences is so small that hitting one of them (without using those consequences) would be a miraculous coincidence. Instead, *all* true (and fecund) hypotheses have true (novel) consequences. Although true hypotheses are fewer than false ones, we don't find them by chance, but through reliable methods. Alai (2014) § 7 shows how (2') rules out false components.

However, checking whether H fulfills (2') is not a merely logical task: at any given time some *logically* possible weakenings of H may be considered physically impossible given certain background presuppositions. E.g., the belief that waves must propagate in a material medium prevents weakening the idea of aether to that of field.

Therefore what is essential or inessential (Vickers, 2016) cannot be distinguished prospectively, as hoped by Votsis (2011) and Peters (2014). This is why we cannot foretell the future development of theories. Yet, the (partial) truth of H can be acknowledged independently of its being preserved today; instead *if* and *when* H is subsequently refuted, it also appears that H was inessential; this is seen retrospectively, but independently of its refutation. Hence, *pace* Stanford, the selective realist defense against Laudan's meta-modus tollens is not circular.

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The concept of disease: Some limits of the non classical-views

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Keywords: disease, concept representation, prototypes, family resemblances

The problem of defining the concept of DISEASE is much discussed in philosophy of medicine. Most philosophers, while disagreeing on the exact definition, are still convinced that it would be possible to find one. However, none of them is entirely exempt from some problems. Faced with this issue, it has been proposed to regard the concept of DISEASE as a non-classical one, and new theories based on family resemblances, prototypes, or exemplars have been proposed (Sadegh-Zadeh 2008, 2011, Lilienfeld and Marino 1995, 1999). In this paper, we won't take a side in favour of either a classical or a non-classical approach to the concept of DISEASE, but critically evaluate the most relevant attempts to characterize it in non-classical terms, showing some of their limits and misunderstandings.

First, the fact the concept of DISEASE shows prototypical effects cannot be ignored. However, recognizing that a concept shows prototypical effects is different to affirming that it has a prototypical structure, thus endorsing the prototype (or the exemplar) view: recognizing the presence of prototypical effects neither constitutes a straightforward argument for the prototype (or the exemplar) view, nor implies the confutation of the classical theory.

Second, in the relevant literature there are important confusions between the notions of prototype, exemplar, and family resemblance. We argue that they can be partially explained, but still remain problematic.

Third, focusing on the notion of family resemblances, this view may be particularly awkward, as it would be difficult to circumscribe those properties that are really relevant for the disease category, and thus prevent excessive medicalization.

Fourth, many scholars adopting a non-classical view do not attempt to explicate the similarity relationship between an individual disease and the prototype, or the exemplars. This proves to be a great weakness, as what makes a certain condition an instance of the disease category is not explicated. Of course, other philosophers endorse the problem of explicating the similarity relationship; a common strategy is to refer to fuzzy logic. However, this yields to some unavoidable difficulties, which are especially related to the exact specification of the similarity relationship and the inability of fuzzy logic to handle compositionality.

Fifth, scholars adopting a non-classical view are typically looking at the common-sense concept of DISEASE, while those who are defending the classical view at the scientific concept. Of course, we are neither saying that scientific concepts are unproblematic to handle with the classical view nor that they can be defined through necessary and sufficient conditions; here we are merely stressing the difference between common-sense and scientific concepts, whose importance has been underestimate. More importantly, a non-classical characterization of the common-sense concept of DISEASE is not incompatible with a classical characterization of the scientific concept of DISEASE (Amoretti, Frixione, Lieto, in press).

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The DSM-5 definition of mental disorder: some points on the harm requirement

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Keywords: mental disorder, DSM-5, harm, psychiatry.

The DSM-5 general definition of mental disorder seems to identify a mental disorder with a harmful dysfunction (APA 2013, 20). Two requirements are stated, but they have a different import. First, a mental disorder *reflects* a dysfunction: the dysfunction is taken to be a necessary requirement. Second, a mental disorder is *usually associated* with significant distress or disability: the occurrence of distress or disability, which may be dubbed as the harm requirement, is taken to be merely usual, and not then necessary. In the present paper we shall only focus on the harm requirement.

To begin, we shall try to clarify what it means to say that the harm requirement is not necessary for mental disorder, and evaluate what reasons can be advanced to maintain that. As a preliminary point, we shall trace a distinction between regarding mental disorder as a token or as a type. If mental disorder is regarded as a token, denying the necessity of the harm requirement would amount to say that certain occurrences of a mental disorder—that is, some particular tokens of a type of mental disorder—might be not harmful. We shall argue that a similar claim is hardly questionable. If mental disorder is regarded as a type, denying the necessity of the harm requirement would amount to say that at least certain types of mental disorders—that is, some mental disorder categories—do not need to meet the harm requirement at all in order to be recognized as pathological conditions. This second option is more controversial. In the present context, we do not want to take a side either in favor or against it, but to evaluate whether or not it is compatible with the DSM-5 nosology; we shall argue that it is. First, there is an explicit claim that the concepts of mental disorder and those of distress and disability should be kept separated (APA 2013, 21). Second, a generic harm criterion is added amongst the diagnostic criteria of many (or even most) mental disorders in order to deal with the problem of disorder thresholds (APA 2013, 21); but this implies that at least some types of mental disorders do not need to meet the harm requirement.

Then, we shall try to unpack the harm requirement trying to clarify by whom, how, and with respect to whom distress and disability should be actually judged and evaluated. Our aim is to show that the harm requirement, as it is stated in DSM-5, can be interpreted in many different and contrasting ways, making its current wording ambiguous and problematic. First, who should evaluate what counts as distress and disability, and their right amount? At least three alternatives are in place: the patient, his or her family, and the psychiatrist. Second, what kinds of standards should be used to evaluate distress and disability? Two options seem to be viable: clinically "objective" standards or context sensitive standards. Third, with respect to whom distress and disability should be evaluated? Again, at least three different choices are feasible: the patient, his or her family, and society as a whole. We shall exemplify all the above possibilities citing various mental disorders and the corresponding diagnostic criteria as they are listed in DSM-5.

We shall conclude arguing that the definition of mental disorder should not consider the harm requirement as necessary. That being said, as long as the harm requirement is still present amongst the diagnostic criteria of many mental disorders, we believe that it must be better explicated and nuanced.

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The Epistemic Conditions for Collaborative Interdisciplinarity

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Keywords: Social Epistemology; Interdisciplinary Science; Scientific Knowledge Transfer; Group Cognition; Interactional Expertise.

Nowadays, the word interdisciplinarity is used in order to define and arrange specific trades of academic and scientific resources – such as concepts, models, and theories, but also people and funds – without a solid backbone theory that describes what interdisciplinarity actually means and what it takes to perform good interdisciplinary research. In particular, as reported by various authors (Barry, 2008; MacKinnon&Hine&Barnarda, 2013; Maki, 2016) assembling an interdisciplinary team is still perceived as a shot in the dark: it looks more like a wishful idea, than an planned activity. For this reason, in this article I aim at proposing a prescriptive account for conducting good collaborative interdisciplinarity in scientific and academic environment. My thesis will be focused on the discussion of the epistemic conditions that this type of collaboration requires and the article will be divided in three section.

The first part of the article will be dedicated to the description of the current development of the studies regarding interdisciplinarity. In particular I will discuss some instances of interdisciplinary research that depend on the development of good forms of collaboration. I will mainly refer to a powerful case study, conducted by MacLeod&Nersessian (2016) on a integrative biology research institute. In the second part I will properly develop my argument: I will claim that researchers who participate to an interdisciplinary project have epistemic expectations that depend on their *mono-disciplinary* background and, thus, that do not prepare them to the actual interdisciplinary work. Bringing awareness about the limited perspectives and resources of the individual researchers will be presented as the main epistemic condition to achieve good collaborative interdisciplinarity and to overcome disciplinary-related ignorance. Thus, in the third part I will present the details of the prescriptive account that derives from the previous considerations: I will examine the knowledge-building and ignorance-reducing strategies that researchers adopt in the organization of a successful interdisciplinary cognitive environment. In particular I will explain how, in order to generate interdisciplinary knowledge and reduce forms of disciplinary-based ignorance, researchers need to develop *interactional expertise* (Collins&Evans, 2002), reframing their disciplinary backgrounds and openly discussing their epistemic goals in the light of individual and common aims.

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Direct realism and the problem of perceiving into the past

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Keywords: direct realism, perceptual disjunctivism, time lag argument, visual perception, hallucination

Direct realism is the thesis that "in veridical cases we *directly* experience external material objects, without the mediation of either sense-data or adverbial contents" (Bonjour, 2013). In this paper I reconsider a traditional argument against direct realism, the time lag argument originally developed by Russell (1912). According to the time lag argument, since the light from every object takes time to affect us in order for us to perceive the object, we always visually perceive objects *as they were in the past* rather than as they are in the present time. But whatever is in a *direct* perception relation with the subject perceiving it at *t* should be temporally located at *t* rather than in the past of *t*. We should conclude that whatever it is that we are in a direct perception relation to while we perceive, if any, it is not external objects, and direct realism is false.

The time lag argument has never appeared particularly serious to direct realists. Their standard answer has been that we do directly perceive mind-independent material objects in the visual modality, provided we accept that we always perceive them *in the past* (e.g. Snowdon, 1992). What I want to show, however, is that – if they concede that we can be in a direct visual relation to external objects that are temporally located in the past - direct realists are forced to accept the untenable claim that there are some events counting as both perceptions and hallucinations.

In fact, if the direct realist concedes that every direct visual perception of *x* is a visual experience of *x* as it was in the past, she must also concede that it is possible for a direct visual perception of *x* occurring at *t* that *x* has ceased to exist at *t*. However, the standard definition of hallucination entails that a visual experience of *x* at *t* counts as a hallucination if *x* at *t* is not there where it visually appears to be at *t*. It follows that a particular visual experience of *x* at *t* which (i) counts as a direct perception, and (ii) is such that *x* has ceased to exist at *t*, counts as both a direct perception and a hallucination.

The direct realist may try to organise a defence from this new argument stemming from the time lag argument by changing the definition of hallucination. However, I show that no apparently reasonable attempt can be successful. In particular, it is not possible for the direct realist to claim that the difference among veridical and delusive visual awarenesses of *x* is that while the former are appropriately *caused* by *x*, the latter are not. *Having a mind-independent object as the appropriate cause* cannot be the factor for distinguishing between perceptions and hallucinations a direct realist makes appeal to. For according to the direct realist, external mind-independent objects *constitute* perception rather than causing it (Snowdon, 1990; Martin, 1997; Johnston, 2004).

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Induction and necessary connections in scientific practice

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Keywords: Induction, Necessitarianism, Scientific Practice, Randomised Clinical Trials.

Any satisfying solution of the problem of induction should offer a reconstruction of inductive reasoning that is applicable in scientific practice. Some necessitarians have claimed that they could justify induction by introducing necessary connections. I argue that the proposed model does not accurately represent scientific reasoning. As an example, I will investigate the reasoning employed in randomised clinical trials (RCTs).

The basic model of the necessitarian solutions to the problem of induction is that induction can be justified by reducing it to the following two-stage argument. First we infer from the fact that all Fs have so far been Gs by a supposedly unproblematic inference to the best explanation (IBE) that there is a necessary connection between F and G. We then *deductively* infer from this necessary connection between F and G that all Fs are Gs (Cf. e.g. Armstrong (1983), 104 or Ellis (1998)).

Nancy Cartwright and Eileen Munro e.g. reconstruct reasoning in medical research broadly along the line of this two-stage argument. First, from the body of evidence available to us we infer that a treatment has a 'stable capacity' to produce the desired outcome. Stable capacities are modal dispositional properties. From the knowledge of this stable capacity we deductively infer that the treatment probabilistically causes the outcome outside the test environment. Cartwright and Munro claim that RCTs alone are insufficient to establish the existence of stable capacities, but serve to test under which conditions stable capacities are exercised (Cartwright and Munro (2010), 262).

Against this, I argue that such an inference pattern is nowhere to be found in scientific research. Against Cartwright's and Munro's analysis, RCTs are meant to directly establish whether a treatment is efficacious. We infer from the fact that the desired outcome is more prevalent in the test group than in the control group that the treatment is causally relevant for the outcome in the tested sample. This inference is ampliative. But the further inference that the treatment will be causally relevant in the population is also ampliative: we infer the causal efficacy of a certain treatment in the population from its causal efficacy in the sample. This inference is ampliative because we do not have the necessary information to make it deductive: we have no knowledge of the exact causal makeup of every individual in the population or whether there are as yet undiscovered interfering factors. There is no inference to modal capacities from which we deduce the efficacy of the treatment in the population. The necessitarian analysis of inductive practice is also inapplicable in cases where there are no single modal properties that could be inferred to, as is often the case in meta-studies.

I conclude that the necessitarian attempt to justify scientific inductive inferences does not adequately represent scientific practice.

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Revisiting the liberal critiques of scientism in the social studies during the first half of the 20th century

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Keywords: Philosophy of the Social Sciences; Critique of Scientism; Nature and Society; Science and Politics; Representation and Creation.

During the first half of the twentieth century, there were many attempts on behalf of liberal thinkers to criticize the reigning scientism in the social studies. Among these, Karl Popper and Von Hayek did the most significant works (4)(5)(6)(7). However, it is often believed that these critiques were mostly motivated by the fact that during those years Marxism was dominant in the social studies, and this dominance had led to the considerable valorization of governmental planning in the political and economical life, something that was, according to the liberal thinkers, threatening to individual freedoms. My goal in this paper is to show that beyond the political inspirations behind these critiques, we can find in them a serious attempt in order to distinguish nature and society, so as to prevent the natural sciences from imposing their methods on the studies of our political and economical lives. Resuscitating these debates is all the more important since ever since the fall of Marxism as an intellectually viable doctrine, most of the modern attempts in order to impose natural sciences' methods on the social sciences are undertaken by thinkers with very accentuated liberal penchant, foremost among whom Milton Friedman and the Chicago school of economics (1)(2); and left-wing, non liberal, thinkers have, especially in the past four decades, criticized not only the reigning scientism in the contemporary social studies, but all kinds of scientific activities, including those of the mathematicians and physicists. Their criticisms have therefore failed to really influence the way that social sciences are conducted. Thomas Piketty (3), though, has realized one of the few attempts at criticizing this scientism without taking to task the scientific character of natural and mathematical sciences. Still, Piketty's critiques, even if passionately formulated, are not rigorous from an epistemological perspective. Therefore, revisiting the liberal critique of scientism in the social studies seems to be necessary for any attempt at founding Piketty's arguments from a satisfactory epistemological perspective. This would necessitate modifying and rethinking some of the liberal arguments, especially given that the dominant form of scientism in the social studies is no longer, as it was in the first half of the past century, historicism, but naturalism, that is trying to emulate the methodology of natural sciences. We would therefore try to show how by modifying some of these critiques, we can formulate a penetrating criticism against the reigning scientism in the social studies, by concentrating especially on the economical sciences.

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Realism, continuity, and the historiography of science

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Keywords: Scientific Realism, Continuity, Anti-realism, History of Science.

Being broadly understood as the claim that science provides us with a true or approximately true history of the world, scientific realism has a strong bearing on historiography of science. In general, a scientific realist commits herself to two theses: (1) science scores success after success into an ever-growing progress and (2) this progress is roughly continuous throughout history of science. Furthermore, the most important argument in support of realism, the so-called no-miracles argument, relies crucially on these theses, particularly on the continuity thesis (2). For realism to be the best explanation of science success, this success must not only be a series of more and more empirically adequate theories, but the result of a continuous progress. If success was achieved by radically different and incompatible theories, realist commitment in any successful theory could not possibly be justified.

Needless to say, both the previous theses have been challenged by antirealists of several sorts. Building on the work of Kuhn and Feyerabend in the 1960s, many writers have held that the history of science is marred with discontinuities and revolutionary breaks, thus it cannot be depicted as the triumphant march toward truth that the realist likes so much. For example, Larry Laudan has argued that a great number of past successful theories were subsequently abandoned and since one can pessimistically induce that this is likely to happen with present theories, no convincing relation between success and realism exists (Laudan, 1981).

In the attempt to answer these challenges, upholders of realism have recently developed a more acute sensitivity toward history of science. The so-called selective realism, for example, meets Laudan's pessimistic meta-induction by claiming that only the theoretical posits effectively deployed to achieve scientific success are transmitted across theory change. As Stathis Psillos has clearly stated, this position incorporates a historiographical program: the realist needs to show that (only) deployed theoretical posits survived revolutionary breaks and that they are still part of our best science (Psillos, 1999).

In this paper, I discuss the selective realism program and its implications for historiography of science. In brief, I argue that the search-for-survivors rationale that underlies the program flirts too dangerously with whiggism and its therefore very problematic from a historiographical point of view. However, I also argue that this is not a fatal flaw of selective realism, but simply a consequence of the way in which this position has been developed so far. By analyzing the concept of deployment, I suggest that realism should move the focus from theoretical posits alone to the integration between posits and practices used to put the to work. This shift allows us to see the selection of posits as a much more nuanced process involving strategies to improve the integration between posits and practices as well as the reliability of practices themselves. It also entails a fundamental redefinition of the historiographical program of selective realism: rather than looking for the posits that survived theory changes, the realist should try to map out the conceptual and historical itineraries of theories in terms of the strategies used to deploy theoretical posits.

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The problem of speciation in the cultural evolution of signaling systems

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Keywords: Species, Cultural Evolution, Blending Inheritance, (Meta-) Population, Signaling Games

In the following paper, I shall investigate whether a proper theory of cultural evolution (CE) – embedded in the framework of a generalized Darwinism - possesses the ability to synthesize the social sciences, and if CE is a good candidate for closing explanatory gaps between micro- and macrolevel phenomena in the social realm – at least in principle.

In order to achieve this, it should be possible to classify macrolevel patterns, types or clusters in CE. This is not an easy task, for the serious doubt is raised whether it is possible to identify something like "species" (classes defined by their phylogenetic history and intrinsic reproductive barriers and not merely defined by similarity) in CE. However, since this is a crucial requirement for any evolutionary classification, a macrolevel cluster of a similar sort is necessary to realize the expectation that this paper aims to realize. I will suggest to apply the "causal interactionist population concept" (CIPC), recently formulated by Millstein (2009, 2015) in the philosophy of biology. According to some critical authors, CE is in need of a valid population concept anyway (Reydon & Scholz 2015). Since CIPC is a non-formal hypothesis, I will also present rudiments of a possible formalization of CIPC using graph-theory. Finally, a possible candidate for the micro foundation of this model is presented within the framework of the evolution of language and meaning: the signaling game of coordination, which has been examined in the context of evolutionary game theory (Lewis 1969, Skyrms 2004, Huttegger 2008). It will be shown that this game theoretical micromodel can seamlessly be transferred into a macrolevel population cluster given by the CIPC.

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The dam project. Who are the experts? Philosophical lessons from the Vajont disaster

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Keywords: Experts, local knowledge, public opinion, fact/value dichotomy.

There is a problem of demarcation in public debates involving technical matters: expert vs. non-expert knowledge. There is a view that citizens have the right to participate in these debates, normally in the 'political' stages of their development. However, it is much more controversial whether participation should be allowed in the more 'technical' stages of problem solving. How effective is non-expert knowledge vis a vis expert knowledge? What contribution can it offer? Why should it be listened to? Should it be labeled as 'knowledge' at all?

In this paper we would like first to assess the difference between two types of knowledge that can be classified as expert and non-expert, namely 'scientific' knowledge and 'local' knowledge, and clarify in what sense they both qualify as types of 'knowledge'. Secondly, we will discuss whether these two types of knowledge are disjunctive or complementary. Thirdly, we will argue that if we believe that they can be complementary, a theoretical framework of conditions and practical requirements should be articulated to allow technical information and informal experience suitably to combine.

To illustrate the need for this interactive framework we analyse a case-study that displays many of the contentious features mentioned above.

In 1963 a huge landslide covered the Vajont valley (north-east of Italy), where one of the tallest arch dams in the world had been put in place (completed in1959). More than 2000 people died. The locals had repeatedly warned the scientists that the sides of the valley were too fragile to hold significant impact, and publicly raised concern. The ensuing media debate surrounding issues of safety in the valley soon became manipulated for political purposes, and the important message got wasted.

With the help of this case study we analyse how two types of knowledge (official science and local experience) may confront each other and why the fail to interact. We then draw some lessons concerning how the use of expert knowledge becomes effective and valuable in the context of non-expert knowledge

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An approach to Bi-logic by modalities

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Keywords: Bi-logic, modality, quantum logical models, foundations of psychoanalysis, First and Second Topic.

Bi-logic (Matte Blanco, 1975) has two opposite "modes": a "symmetric" and a "bivalent" one. The logical characterization of the last is the separation of the two opposite truth values, whereas the symmetric mode cannot perform the separation, since it identifies the part with the whole thing. Following Matte Blanco, this means that the unconscious can treat infinite sets only: namely, the symmetric mode is due to an original infinite mode of thinking.
We find that both the theoretical and clinical research (Lauro Grotto, 2014) would acquire a great advantage if a formal approach could better clarify how the collapse of the infinite into the bivalent mode takes place. To this aim, our proposal is to adopt a logical language including modalities, in order to clarify the different value of assertions in different contexts.
We consider a quantum logical model (Battilotti, 2014), where quantum states corresponds to sets termed "infinite singletons" and where one can read by "symmetry" what is usually considered as "duality" in logic. The model shows also how duality is recovered by separation. Our infinite singletons are domains of quantifiers, which we introduce by suitable equations. An abstraction from our definition of quantifier allows to eliminate the specific domain and to obtain the modality of S4 (Battilotti 2016). Then the modality can describe symmetry and duality together. The modality of S4 can be read as a way to add "an infinite view" to classical propositional logic. The definability of the modality in the quantum model depends on the fact that the spin is a two-valued observable, consistently with the Kochen-Specker theorem of quantum theory. In our view this can be read as implying that 'finite means bivalent'.
From the point of view of psychoanalytic theory the complete formalization of the theory in logical terms would provide a new perspective within the long-lasting debate on the epistemic foundations of psychoanalysis (Lauro Grotto, 2014b). In particular we are here interested in exploring a possible interpretation of the formal introduction of the modal system S4 in relation to two theoretical points: first, the shift from the First to the Second Topic description in Freudian Psychoanalysis, and second, the consideration of transitional dynamics and the role of external reality in the Object Relations approach.

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Novikov's cut elimination

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Keywords: Cut elimination, regular formula, Novikov.

Our purpose is to explain the interesting, original reduction procedure invented by Pëtr Sergeevich Novikov, by means of the notion of regularity (*regulyarnost'*) of formulas, to obtain what amounts to (a sort of) cut elimination for a Hilbert-style formulation of the first-order predicate calculus.

The notion of regularity (a form of cut-free derivability) was first introduced by Novikov in 1939, and then developed in 1943, with reference to infinitary derivations in a system for the propositional calculus admitting countable conjunctions and disjunctions. This work was well described (and compared with similar results in the West, on which it does not depend) by Jon Barwise, Grigori Mints and Sergei Tupailo. Thierry Coquand deeply explained further, and reformulated in terms of games, the reduction technique developed by Novikov.

In 1949 Novikov first defined regularity for the usual (finitary) first-order predicate logic, and stated a theorem to the effect that every formula which is provable there is regular. But only in the book *Elements of mathematical logic* (Novikov, 1959), the first original logic textbook in Russian, Novikov finally published a *proof* of the result that regularity is preserved by the rules of a (usual) formal system for predicate logic, in particular by Modus Ponens. In fact, the result is thoroughly proved there for a stronger system (viz. a form of 'restricted' arithmetic, based on full first-order logic with identity, with successor and order axioms, definitions for all primitive recursive functions, without any form of induction), but the point of the proof-theoretic reduction concerns the purely logical part of the system.

To our knowledge, *this* proof has received scarce attention in the literature, and a full exposition of it is lacking. In view of the fact that Novikov's method is very different from the usual ones, this could be useful. Moreover, a comparison of this original technique with other forms of cut elimination for *finitary* systems (mainly Gentzen's one, but also the methods employed in Herbrand's Thesis) could be interesting.

As we shall see, the proof is nontrivial and requires a long series of lemmas, whose proofs are sometimes a bit involved; its (perhaps) most important peculiarity is that no induction *stricto sensu* on the complexity of (what corresponds to) the cut formula is needed: the formula is modified but (in a sense to be seen) preserved.

After a short description and explanation of Novikov's procedure, a brief comparison with other classical cut-elimination procedures for predicate logic will be given.

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Explaining eusociality: multilevel selection, kin selection and the structure of evolutionary theory

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Keywords: Philosophy of evolutionary biology, kin selection, group selection, eusociality, semantic conception of scientific theories

The origin of social behaviors in animals constitutes a major conceptual challenge for evolutionary biology (E. O. Wilson 1975). One prominent example of such behaviors is eusociality, i.e. an evolutionarily advanced form of colonial organization in which "adult colonial members belong to two or more overlapping generations, care cooperatively for the young, and are divided into reproductive and nonreproductive castes" (E. O. Wilson and Hölldobler 2005). As such eusociality represents an instance of the more general phenomenon of biological altruism. In the context of the theory of evolution by natural selection, such a phenomenon has been addressed through two main explanatory strategies: group or multilevel selection and kin selection.

The first strategy assumes that altruistic behaviors can (also) be explained as the consequence of a selection mechanism acting on some features that individuals bear as a group, such as foraging or dispersion strategies (Wynne-Edwards 1986, Sober and D. S. Wilson 1998). The second posits that natural selection acts on individuals that are genetically related to one another, and share therefore a certain proportion of their respective genotypes (Hamilton 1964, Dawkins 1976). Even though kin selection has been widely accepted among evolutionary biologists since the 1960s, group selection is being currently reconsidered as a possible theoretical foundation for sociobiology and, more generally, evolutionary theory (D. S. Wilson and E. O. Wilson 2007, Nowak et al. 2010).

Our paper will discuss the conceptual differences between these two explanatory strategies by linking the problem of explaining eusociality to the question of the conceptual and formal structure of evolutionary theory. More precisely, based on the semantic view of scientific theory (Thompson 1989), we will define two possible formal structures for evolutionary theory. We will therefore aim to identify two meta-models that could lay the foundations for such structures, so that they include, respectively, the group or multilevel selection and the kin selection explanatory strategies, i.e. R. A. Fisher's Fundamental Theorem of Natural Selection (Fisher 1930) and W. D. Hamilton's rule for kin selection (Hamilton 1964). To establish the conceptual ties between those two models and their respective explanatory strategies, we will focus on a particular case, i.e. the study of the evolution of social insect colonies, particularly ant colonies. Such an example will allow us to draw more general conclusions about the opposition between kin and group selection models, as well as about the logical and formal structure of evolutionary theory.

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Understanding superstition as extended cognition and individual niche construction

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Keywords: Cognitive science; Extended cognition; Cognitive niche construction; Material culture; Superstition.

"Academic definitions of superstition as distinct from genuine religion are hard to come by; the point of the label 'superstition' is dismissive" (Drees, 2010, p. 70). This is not to say that cognitive science has totally overlooked superstition, but it attracted far less interest than religion, notwithstanding the fact that in our life we are more likely to engage in superstitious behavior than religious one. In the second edition of his monograph about superstition, Vyse (2014) elaborates on Marmor's definition of superstition as "beliefs or practices groundless in themselves and inconsistent with the degree of enlightenment reached by the community to which one belongs" (Marmor, 1956, p. 119) and defines superstition as "the subset of paranormal beliefs that are pragmatic: used to bring about good luck or avoid bad" (Vyse, 2014, p. 24). Recent theories such as the extended mind and cognitive niche construction, though, can shed new light on superstition and its apparently unreasonable success (Clark and Chalmers, 1998; Clark, 2005; Bertolotti and Magnani, 2016). This lets us observe how most superstitions are not mere "beliefs" (such as religious beliefs could be) hosted in a naked mind, but rather involve a strong coupling between the mind and some external props allowing its extensions away from the skull: from bodily gestures, to artifacts and other agents (human and animal). The mind's capability to extend into the environment supports the related theory of cognitive niche construction, suggesting that human agents achieved better and better performances by creating external structures (cognitive niches) able to provide better and persistent scaffoldings for their cognitive performances. When it is not possible to detect and exploit the presence of a cognitive niche in the environment, superstitious practices can be identified as the possibility to deploy an *emergency*-cognitive niche projected by the superstitious agent into the world by means of a superstitious prop (item, ritual, gesture). Such a cognitive niche is poorer and less reliable but preferable to utter blank (and the consequent inaction), and most important it is still coupled with the external world (be it the body or its ecology in forms of artifacts and other agents), thus maintaining the fundamental characteristic of cognitive niches, that is cognitive distribution.

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Social cognition and hypothetical contracts

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Keywords: Cooperation, Social Cognition, Social Contract, Social Norms, Theory of Mind

Social contract theory traditionally faces three objections: that hypothetical contracts are not binding; that they model actors according to an unrealistically abstract conception of self and agency; that the theory is circular. I contend that current work in social cognition provides the tools to cope with the second and the third worry, and to deal with the first once it is framed as a stability problem. I draw on Tomasello's work on the psychological infrastructure of cooperation and on Goldman's simulation theory to figure out the mechanisms at work, and argue that they provide agents with a framework for handling hypothetical contracts, grasping their normative content, supporting the motivation to comply. The background idea is that social contract theory can be given a plausible mechanistic reading.

I frame the issue in the context of Rawls' understanding of the original position as a device of representation designed to convert a question of justification into a deliberative problem (Rawls 1971). Drawing on Tomasello's work I take that (a) cooperative activities require understanding the equivalence between self and other, hence a capacity for social cognition that supports perspective taking and role reversal, and that (b) the pro-social motives stemming from the early inclinations to help and share develop through social interaction into a disposition to reciprocity (Tomasello 2009, 2014). I further maintain that perspective taking and role reversal are supported by simulative mindreading and rest on a single mechanism of imaginative self-projection that works both in intrapersonal action planning and in interpersonal cooperation (Goldman 2013; Bruckner, Carroll 2007).

I argue that deliberating in the original position results from recruiting a suitably abstract conception of agency under the self-other equivalence that goes along with early joint actions. This can be expected to occur as a representational theory of mind is acquired and social cognition consequently rearranges, enabling agents to generalize the conditions under which joint actions are performed and to cooperate in anonymous settings (Tomasello, Rakoczy 2003, Rakoczy 2015; de Villier 2007). A related shift in the disposition to reciprocate is likely to take place at this stage, supporting the development of a general system of normative expectations about fairness.

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Molecular Biology vs. Bioinformatics: the method and the demarcation

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Keywords: bioinformatics, molecular biology, demarcation, scientific practice

In 2007 the famous American magazine *Wired* presented, on its special issue's cover, a provocative title: "The End of Science". Chris Anderson, editor in chief of *Wired* at that time, explained that sentence by arguing, more precisely, about the "end of theory" in science. According to Anderson the way scientific disciplines proceeded and advanced in the past, guided by and funded on theoretical elaboration, should be now considered obsolete in the light of a new approach based on data-gathering. Scientific method as such cannot be longer tenable and thus it should be abandoned in favour of a new picture. "The quest for knowledge used to begin with grand theories. Now it begins with massive amounts of data. Welcome to the Petabyte Age" (*Wired* cover, 2007). By *Petabyte Age* Anderson mainly refers to new research avenue provided by so called Big Data Science. The label of Big Data does not mean just a big volume of data. Despite the lack of a precise definition, it is certainly possible to select certain features of Big Data Science as they were 'hallmarks' for such an approach. Following Kitchin (2013, 2014) Big Data Science consists certainly in the quantity of data (*e.g.* petabytes), in the speed at which these data are obtained, in the variety in which they are ordered and displayed, in the global/holistic aim (in contrast to more traditional statistics), in standardised procedures both regarding resolution and identification of data, in the relational format according to which data can be easily expanded or increased in magnitude. Of course the impact of this "new way of doing" science produced new modes of considering scientific evidence but also tensions with more traditional ones. The very meaning of experiment is at stake here. As a matter of fact, many scientists, not philosophers, started to debate on the nature of their work (indeed a philosophical activity) precisely because of this clash (and not always with the adequate theoretical equipment).

The scope of this work is to provide an epistemic analysis of this methodological and technical innovation, within the life sciences, in the light of so called traditional topics of philosophy of science: the nature of scientific method and its relevance for the establishment of a criterion (or a set of criteria) of demarcation.

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Paraconsistent Weak Kleene logic: an abstract algebraic approach

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Keywords: paraconsistent logic, abstract algebraic logic, algebraic semantics, involutive bisemilattices.

Paraconsistent Weak Kleene logic, PWK, was essentially introduced by Halldén and, in a completely independent way, by Prior, and is the logic based on the weak Kleene tables, that is, it is the logic $\models_{\mathbf{PWK}}$ defined semantically by the matrix $\mathbf{PWK} = \langle \mathbf{WK}, \{\frac{1}{2}, 1\}\rangle$, where $\mathbf{WK} = \langle \{0, 1, \frac{1}{2}\}, \wedge, \vee, \neg, 0, 1\rangle$ is the algebra given by the tables:

$\wedge$	0	$\frac{1}{2}$	1
0	0	$\frac{1}{2}$	0
$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$
1	0	$\frac{1}{2}$	1

$\vee$	0	$\frac{1}{2}$	1
0	0	$\frac{1}{2}$	1
$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$
1	1	$\frac{1}{2}$	1

$\neg$	
1	0
$\frac{1}{2}$	$\frac{1}{2}$
0	1

The aim of this contribution is to study PWK under the light of Abstract Algebraic Logic. We first introduce some algebraic structures called *involutive bisemilattices*, which form a variety, $\mathcal{IBSL}$, and prove that $\mathcal{IBSL}$ is generated as a variety by the sole algebra $\mathbf{WK}$. We, then, study PWK by recourse to the toolbox of Abstract Algebraic Logic. It is not inappropriate to wonder whether the variety $\mathcal{IBSL}$ is the actual algebraic counterpart of the logic PWK. Such a guess stands to reason, for PWK is the logic defined by the matrix $\mathbf{PWK}$ with $\mathbf{WK}$ as an underlying algebra, and $\mathcal{IBSL}$ is the variety generated by $\mathbf{WK}$. We show though that $\mathcal{IBSL}$ is not the equivalent algebraic semantics of any algebraisable logic, and furthermore, PWK is not algebraisable, since it is not even protoalgebraic. We also show that PWK is not selfextensional either.

Furthermore, we characterise the Leibniz congruence of the models of PWK, what allows us to prove that the class $\mathsf{Alg}^*(\mathrm{PWK})$ of the algebraic reducts of the reduced models of PWK is a subclass of $\mathcal{IBSL}$. Finally, we fully characterise the deductive PWK-filters on members of $\mathcal{IBSL}$ and the reduced matrix models of PWK.

Undefinability of Standard Sequent Calculi for Paraconsistent Logics

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Keywords: praconsistent logics; sequent calculus; 3-valued logics.

In the present paper we focus on a family of paraconsistent logics including both the Logic of Paradox (LP) and Paraconsistent Weak Kleene Logic, (PWK) (7), which has been recently studied under different perspectives (5; 4). Different types of sequent calculi have been introduced for LP (1; 2). On the other hand, to the authors' best knowledge, the only attempt to provide a sequent calculus for PWK is (6). All the existing sequent calculi for these paraconsistent three-valued logics present non-standard features, for instance non standard axioms (3), logical rules introducing more than one connective (3; 2), or logical rules that can be applied only in presence of certain linguistic conditions (this is the case in (6)). In our approach a *standard Gentzen calculus* for a logic L is a calculus with the following properties:

1. Axioms shall be only of the form $\alpha \Rightarrow \alpha$, for any propositional variable α.
2. The premises of logical rules must contain only *subformulas* of the conclusion and each logical rule must introduce exactly one connective at time.
3. Logical rules must have no linguistic restrictions.
4. Sequents shall be interpreted in the object language, that is: $\Gamma \Rightarrow \Delta$ means that the formula $\bigvee_{i=1}^{n} \delta_i$, with $\delta_i \in \Delta$ follows from the formula $\bigwedge_{j=1}^{m} \gamma_j$, with $\gamma_j \in \Gamma$.
5. Only standard structural rules, i.e. contraction, weakening and cut are (possibly) allowed.

The main result of this work consists of proving the impossibility of providing *standard* sequent calculi for a family of logics including both LP and PWK. PWK has been extensively studied with the tools of Abstract Algebraic Logic in (4). We wonder whether the above mentioned negative result might have an algebraic counterpart.

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The emergence of philosophy of science in France: Cournot 1861-1875

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Keywords: philosophy of science, probabilism, historiography, epistemology

I would like to inquire into the emergence of a mature philosophy of science in France in the 1860s and early 1870s. Secondary literature insists on the existence of a crucial turning point in the flowering of a modern history and philosophy of science, which is commonly placed at the turn of the twentieth century or the end of the nineteenth century [Brenner 2003, pp. 1, 2, 4-5, and 7-8; Chimisso 2008, pp. 1-2, and 5-6; Braunstein 2012, p. 33; Brenner (ed.) 2015, pp. 5-6]. These historical reconstructions shift forward a cultural process that, in reality, took place some time earlier [Bordoni 2017, pp. VIII-X].

The mathematician and economist Antoine Augustin Cournot played an important role in this cultural process. Starting from 1851, he began to critically inquire into the foundations of the actual scientific practice. In 1861, an original combination of philosophy of science and philosophy of history allowed him to describe a convincing history of scientific method. Recent developments in physical and natural sciences had highlighted the differences among "contents, principles, and methods" of the various sciences, and the pivotal role of life sciences [Cournot 1861, pp. III-V and 118-22]. In 1872, the concept of *chance* was at the core of Cournot's philosophy of history. Chance had its laws, and those laws were no less reliable than the laws of physics and astronomy. Both science and history dealt with chance [Cournot 1872, pp. 1-5]. In 1875, he pointed out that statistics and probability allowed scientists to replace certainty with probability. Cournot's *probabilism* pursued a new alliance between determinism and contingency, between the stability of laws and the contingency of facts [Cournot 1875, pp. 103 and 106-7].

Cournot first reflections on foundations and methods of scientific practice were put forward in an adverse intellectual environment. His theses reappeared in the 1880s and 1890s, in a different cultural context [Naville 1883, pp. 28, 32-35, 41-47, and 50-55; Duhem 1892, pp. 143-8 ; Duhem 1893, pp. 65-6 and 68-71], and in the early twentieth century we find other traces of interest in his philosophy of science [Parodi 1905, pp. 451-4, 459, 473-5 and 483-4; Mentré 1908, pp. IV-V]. His sensitivity to history, and his scientific practice as a mathematician and economist allowed him to go far beyond Comte and positivism: we find in Cournot a sophisticated philosophy of science that re-emerged after the crisis of neo-positivism in the second half of the twentieth century.

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Logical hylomorphism in the 13th century

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Keywords: Logical form, Syllogism, History of Logic.

Two standard ideas about the history of syllogistic are questioned in this paper: the notion that syllogistic is (obviously) concerned with the form of arguments, and the presupposition that an inquiry about the nature of syllogism essentially means a study of the sole *Prior analytics*, regardless of the *Topics* and the *Sophisti Elenchi*, where another sense of "syllogismos" was used by Aristotle. A peculiar variety of "logical hylomorphism", a termed coined by J. McFarlane (2000), is built in medieval Aristotelian logic, as already hinted by C. Dutilh-Novaes (2012). The notion of a logical form, after it has systematically introduced in Late Antiquity by Alexander of Aphrodisias and rediscovered in the 12th century in the Latin world, belongs to a general approach to logic where "logical matter" not only plays as much an important role as "logical form", as already shown by K. Flannery (1995) for Alexander of Aphrodiases and by H. Hugonnard-Roche (2004) for Syriac logic, but is also, in a specific sense, *part of logical form*. The rediscovery of the *Prior analytics* in Latin context, at the turn of the 13th century, was preceded by a half-century of intense exegesis on the *Sophistici elenchi*. The philosophy of logic read in the authoritative commentary by Robert Kilwardby (*ca* 1240) displays a deep concern for the form and the matter of syllogisms, especially defective syllogisms (*i.e.* sophistic arguments), the definition of syllogism, and the relationships between "pure deductions" (the formulae expressed with "dummy letters"), concrete deductions and proofs. In a period when Aristotle's natural philosophy is also rediscovered and intensively commented upon in newly born universities, a robust concept of "hylomorphism" is built in logic as much as elsewhere: syllogistic form is also seen as the "essential form" of the argument and includes some sort of matter. An extreme version of this concept is condemned in Oxford in 1277 under Kilwardby's influence, together with other theses in natural philosophy belonging to "radical" Aristotelianism. The condemned proposition says that "materially defective syllogisms are not syllogisms".

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Predicting under structural uncertainty: Why not all Hawkmoths are ugly

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Keywords: Structural Model Error; Uncertainty; Hawkmoth Effect; Modelling Practice; Bayesian Model Averaging.

In this paper, we discuss a claim recently made by Frigg et al. (2014) that model uncertainty seriously debilitates our ability to derive decision-relevant probabilities from nonlinear models. In analogy to the well-known and much discussed Butterfly Effect, Frigg et al. introduce the Hawkmoth Effect, which arises from a sensitive dependence on structural model error (SME). Just as the Butterfly Effect describes the limitations imposed by initial condition uncertainty on the predictive power of nonlinear models, the Hawkmoth Effect describes the supposedly disastrous consequences that small errors in the structure of a nonlinear model can have for predictions derived from that model.

We show that Frigg et al. exaggerate the epistemic consequences of SME. Firstly, the illustrative example that they discuss sets up a false analogy for normal modelling practices. In reality, practices involved in model choice and evaluation help modellers to detect instances in which their predictions are likely to deviate wildly from the truth.

Secondly, we draw attention to the fact that Frigg et al.'s argument for the generality of the consequences of the Hawkmoth Effect, contrary to what the authors think, has no clear relevance for the use of nonlinear models in practice. It has already been pointed out elsewhere that Frigg et al. may be overgeneralizing their case, and that the scope of their argument remains unclear (Goodwin and Winsberg, 2016). Instead of criticizing their generalization and the abstractness of their mathematical discussion, we focus on the claim that there are no feasible strategies against the Hawkmoth Effect in current scientific practice. We argue that, even if there is a strong correlation between nonlinearity and structural instability, there are efficient strategies against the impact of SME, which are actually applied in current scientific practice. In fact, there are a number of approaches that can guard against the potential consequences of SME. We discuss one such approach, Bayesian Model Averaging (BMA), which is a modelling method that accounts for model uncertainty by applying a Bayesian perspective to the choice of a model. While Frigg et al. anticipate this objection, we explain why their reasons for doing so are unfounded by highlighting relevant insights from the statistical literature.

We conclude that Frigg et al. have overstated the epistemic consequences of SME. Normal modelling practices can help to identify the Hawkmoth Effect, and well-established statistical methods allow scientists to take steps against the impact of model uncertainty.

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Small horns, big horns

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Keywords: Zeno Metrical Paradox; Divisibility; Measure Theory; Gunk; Extension

We will address an ancient yet devilish paradox, namely the *Infinite Divisibility Paradox*: **1)** Suppose x is infinitely divisible. **2)** Then, x is either an infinite sum of extended entities (BIG HORN); **3)** Or x is an infinite sum of unextended entities (SMALL HORN). **4)** In the first case x has infinite extension, in the second case x has no extension at all.
We first argue that we can understand *infinite divisibility* in two different, and logically inequivalent ways, namely *having an infinite number of parts* —ID_1— and *being gunky, i.e. being such that each part has a proper part* —ID_2.[1] We then argue that ID_1 does not entail ID_2 insofar as something can have an infinite number of *atomic* parts, and that ID_2 does not entail ID_1 insofar as one might deny *anti-symmetry* of parthood.

The modern standard solution —the Measure Theory Solution— adopts ID_1 as the correct understanding of infinite divisibility, and ends up being both limited in scope and unsatisfactory— or so we contend. We suggest that we should understand infinite divisibility along the lines of ID_2. Then, we put forward two *intuitively plausible principles relating mereological structure and extension*. They are the following:
Extension$_1$: If x is a proper part of y, then x is less extended than y.
Extension$_2$: If x is more extended than y, and x and y are mereologically related, then x is a proper extension of y.
On the one hand, we argue that, given **Extension**$_1$ the Small Horn is simply dispensed with. In a nutshell the argument proceeds by considering a point-sized object x. Given the gunky assumption x has proper parts, that by **Extension**$_1$ should be less extended than x which is impossible. On the other hand we argue that, given **Extension**$_2$, the Big Horn is easily solved. In a nutshell the the argument is the following. Consider any object x and all of its gunky parts. An infinite-extended object is clearly more extended than x, so that it has to be a proper extension of x. But no proper extension of x is a mereological fusion of parts of x by definition of mereological fusion. Finally, we claim that our new solution has several theoretical advantages: (i) it solves both the horns, —in particular it solves also the Big Horn that the measure-theoretic solution cannot solve; (ii) it apples to finite, countable and uncountable cases (whereas Lebesgue-measure is definable only for countable-sets); (iii) it holds for material objects as well (whereas it is problematic to extend standard solution to them); (iv) it answers worries in Sherry (1988) to the point that sophisticated mathematics should not be needed to solve the paradox (we agree with Sherry that the standard solution only *defuses* the paradox).

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[1] Orthodox mereological definitions are assumed throughout

Methodological Novelties and Causal Inference in Epidemiology

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Keywords: Epidemiology, Disease Causation, Causal Inference, Exposome.

In this paper, I contribute to discussions on causal inference in the biomedical sciences by focusing on current research in epidemiology. I claim that novel approaches to causal inference recently proposed in the field should be considered significant novelties from a methodological perspective, although they do not imply new theoretical views of disease causation.

Recently, discussions on causal inference have been heating up in epidemiology (1). Some of these discussions have centred on formal modelling and the Potential Outcomes Approach (POA), whose focus on single studies and interventions has been criticised by Broadbent and colleagues (2). The issue has also been significantly discussed in molecular epidemiology. Here, researchers have developed the concept of the 'exposome', i.e. the totality of environmental exposures experienced by individuals, which includes exposure to both external, macroscopic elements and internal, molecular components. Following the Meet-In-The-Middle approach (MITM), when diseases and external components of exposure are associated, associations are studied by looking for intermediate biomarkers of internal components of exposure, lying in the middle of the causal relation. Whilst the MITM has been studied in the literature (3), its relation with other discussions on causal inference has not been investigated.

In the paper, I argue that the MITM is a methodological novelty of relevance to the causal inference debate. First, I show how it has both difference-making and production elements: researchers both aim at understanding if external exposures make a difference to the development of disease and the mechanisms through which exposures may make such a difference. I argue that this is an instance of evidential pluralism. Thus, I suggest that the MITM is a novelty for epidemiology, as it provides a more precise methodology for the use of mechanistic and molecular data. While mechanistic data has traditionally been used in epidemiological research, the MITM gives it a specific role with intermediate biomarkers. Plus, individual and mechanistic data has recently been subject of criticism in the field: the MITM solves many of these tensions, as intermediate biomarkers data is used to establish causal claims at the population level.

Finally, I reflect on the MITM's novelty at the conceptual level and consider Broadbent and colleagues' call for conceptual pluralism. I argue that, while the MITM's evidential pluralism differentiates it from the POA, its novelty is restricted to the methodological level. Evidential pluralism does not necessarily require pluralism on views of causation, since, at the theoretical level, the MITM seems to be based on traditional "variational" views of causation.

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Probability, truthlikeness, and two paradoxes of rational belief

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Keywords: rational belief, probability, truthlikeness, Lottery paradox, Preface paradox, Carneades

Providing a theory of rational belief or acceptance in the face of uncertainty remains a crucial issue for current research in (formal) epistemology and philosophy of science. A popular option is basing such a theory on the so-called Lockean thesis: a rational agent should (fully) believe or accept a proposition *h* just in case his degree of belief in *h* (expressed as the epistemic probability of *h*) is greater than some suitably chosen threshold. Unfortunately, this idea clashes against the Lottery and Preface paradoxes which, taken together, show that high probability is neither sufficient nor necessary for rational belief (e.g., Foley 1992).

In this paper, I propose a different approach to this problem. I assume that rational belief aims at approaching truth about the domain of inquiry, and that a rational agent should tentatively believe the strongest proposition *h* which is estimated as sufficiently close to the truth given the available evidence (Oddie 2014; Niiniluoto 1987). I call this idea Carneades' thesis on belief, after the Hellenistic philosopher who apparently first defended it within a coherently fallibilistic epistemology (Niiniluoto 1987). I provide two formal explications of Carneades' thesis. The former amounts to saying that one should accept the strongest proposition *h* which maximizes (a suitably defined notion of) expected truthlikeness. According to the second explication, one should believe *h* just in case the evidence makes sufficiently probable, not that *h* is true (as for the Lockean thesis), but that *h* is truthlike (to a suitably chosen degree).

As I argue, both readings of Carneades' thesis illuminate both the Preface and the Lottery paradoxes, which are given a unified solution within my account (cf. also Cevolani 2016, Cevolani and Schurz 2016). This suggests how to recover an account of full belief within a probabilistic framework while eschewing the problems raised by the Lockean thesis.

A consequence of my proposal is that it can be rational to believe propositions with low probability. This is in agreement with some other solutions to the Lottery paradox (e.g., Lin and Kelly 2012), but seems to fly in the face of our intuitive notion of belief. I defend my solution against this and related objections, by highlighting some interesting connections between my two explications of Carneades' thesis, on the one hand, and the Lockean thesis and the distinction between belief and acceptance, on the other hand. I then conclude by comparing my account with Leitgeb's stability theory of belief as based on the Humean thesis, which also provides a different way out of the Lottery paradox.

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Transition operators assigned to physical systems

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Keywords: Physical system, transition relation, transition operators, states, complete lattice, transition frame.

In 1900, D. Hilbert formulated his famous 23 problems. In the problem number 6, he asked: "Can physics be axiomatized?" It means that he asked if physics can be formalized and/or axiomatized for to reach a logically perfect system forming a basis of precise physical reasoning. This challenge was followed by G. Birkhoff and J. von Neuman in 1930s producing the so-called logic of quantum mechanics. We are going to addopt a method and examples of D. J. Foulis, however, we are not restricted to the logic of quantum mechanics. We are focused on a general situation with a physical system endowed with states which it can reach. Our goal is to assign to every such a system the so-called transition operators completely determining its transition relation. Conditions under which this assignment works perfectly will be formulated.

We start with a formalization of a given physical system. Every physical system is described by certain quantities and states through them it goes. From the logical point of view, we can formulate propositions saying what a quantity in a given state is. Through the paper we assume that these propositions can acquire only two values, namely either TRUE of FALSE. It is in accordance with reasoning both in classical physics and in quantum mechanics.

Denote by S the set of states of a given physical system $\mathcal{P}$. It is given by the nature of $\mathcal{P}$ from what state $s \in S$ the system $\mathcal{P}$ can go to a state $t \in S$. Hence, there exists a binary relation R on S such that $(s,t) \in R$. This process is called a *transition* of $\mathcal{P}$.

Besides of the previous, the observer of $\mathcal{P}$ can formulate propositions revealing our knowledge about the system. The truth-values of these propositions depend on states. For example, the proposition p can be true if the system $\mathcal{P}$ is in the state s_1 but false if $\mathcal{P}$ is in the state s_2. Hence, for each state $s \in S$ we can evaluate the truth-value of p, it is denoted by $p(s)$. As mentioned above, $p(s) \in \{0,1\}$ where 0 indicates the truth-value FALSE and 1 indicates TRUE. The set of all truth-values for all propositions will be called the *table*. Denote by B the set of propositions about the physical system $\mathcal{P}$ formulated by the observer.

We summarize our description as follows:

- every physical system $\mathcal{P}$ will be identified with the couple (B,S), where B is the set of propositions about $\mathcal{P}$ and S is the set of states on $\mathcal{P}$;
- the set S is equipped with a binary relation R such that $\mathcal{P}$ can move from s_1 to s_2 provided $(s_1,s_2) \in R$;
- the set B is ordered by values of propositions.

Our task is as follows. We introduce an operator T from B into 2^S which is constructed by means of the relation R. The question is if this operator, called *transition operator*, bears all the information about system $\mathcal{P}$ equipped with the relation R. In other words, if the relation R can be recovered by applying the operator T. In our paper, we will get conditions under which the transition operator has this property.

Gauge symmetries and symmetries with direct empirical status

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Keywords: ontology of symmetries in physics; direct empirical status; gauge symmetries.

Direct empirical status (DES) of theoretical symmetries is a subject of a growing number of articles, including (Greaves and Wallace, 2014). It has been discussed in relationship to such theoretical symmetries as Poincaré transformations, phase shifts, potential transformations and diffeomorphisms. A theoretical symmetry has DES if it is able to represent an empirical symmetry. The latter consists of two states of the world linked by a physical transformation that preserves some observable features. A theoretical symmetry, on the other hand, consists of two theoretical descriptions of the world ('theoretical states') linked by a theoretical transformation that preserves some theoretical features.

A theoretical symmetry with DES is usually interpreted as describing a passage to a physically different state of the world ('active interpretation'). This is thought to differentiate it from a gauge symmetry, i.e. a theoretical symmetry featuring observationally equivalent theoretical states, given that the latter is usually interpreted as a redescription of the same state of the world ('passive interpretation'). I will show that the situation is far more complicated, in particular in that theoretical symmetries with DES include gauge symmetries and because both gauge symmetries and the other theoretical symmetries can be interpreted either way.

(1) Besides a possibility of being passively interpreted, gauge symmetries can have a DES and so an active interpretation if we allow empirical symmetries where all the observable features are left invariant by the transformation. A shortcoming is that we cannot verify whether a gauge symmetry has this DES by observational means. (2) Gauge symmetries can also have an 'observable' DES if they are put in correspondence with empirical symmetries featuring observationally distinguishable states. However this requires accepting that the observable difference is not represented. This can be justified if we wish to represent the invariant observable features alone. On the other hand, this practical interest can also be satisfied without a gauge symmetry, by using a single theoretical state instead. (3) We may alternatively reserve the 'observable' DES for theoretical symmetries whose theoretical states are observationally inequivalent. This move constitutes an implicit basis of the abovementioned opposition between theoretical symmetries with DES and gauge symmetries. It then leads to such ideas, often assumed in the literature, as a particular ontological significance of symmetries with DES and its independence from the interpretation of gauge symmetries. However the latter idea is erroneous and the former needs refinement. For suppose that the initial state *i* and the final state *f* of a symmetry with an 'observable' DES each gets transformed by a gauge symmetry into *i'* and *f'* respectively. Any gauge transformation yields an observationally equivalent state, so *i'*, *f'* and a transformation linking them will provide as good a representation of an empirical symmetry as the original theoretical symmetry was. Now if the gauge transformations (and the gauge symmetries they constitute) are interpreted actively, then the original and the resulting theoretical symmetries with the 'observable' DES will provide two distinct stories about the unobservable underpinnings of the empirical symmetry. While if the gauge transformations are interpreted passively, the two theoretical symmetries will rather be redescriptions of the same theoretical model of the empirical symmetry. (4) Contrary to the suggestions in the literature, such formal properties of a transformation as being of a specific kind (e.g. a translation), being applied to a specific area, or being global or local do not determine when taken separately whether a theoretical symmetry constituted by the transformation has an 'observable' DES or is a gauge symmetry. For example, global translations give rise to gauge symmetries when applied to all the things in the universe, and to theoretical symmetries with an 'observable' DES when applied to a unique thing. Meanwhile, a spatial reflection cannot produce a gauge symmetry when applied to the whole universe, while diffeomorphisms cannot have an 'observable' DES whatever the restricted area they apply to. (5) While being a gauge symmetry is compatible with an active interpretation (see above), being able to represent an empirical symmetry featuring observationally distinguishable states is compatible with a passive interpretation. For example even the most paradigmatic global restricted boost capable of representing the Galileo's ship empirical symmetry can also represent a difference in conventions about velocity between the ship crew and the people on the shore.

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G. C. Rota on mathematical identity: crossing roads with Husserl and Frege

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Keywords: identity, axiomatic system, presentation, number, set.

In this paper I address G. C. Rota's account of the identity of mathematical objects and attempt to relate it with the issue of the ontological status of them. After taking under consideration Rota's distinction among mathematical facts and mathematical proofs, I attempt to highlight the phenomenological background of Rota's claim that mathematical objects *retain their identity* through different axiomatizations. Rota takes axiomatization to be a form of presentation, precisely a form of "pretending". Behind Rota's assertions, one can pinpoint Husserl's phenomenological assumption that identities are preserved through many appearances of a thing, an event, a mathematical object etc. I compare this view with Frege's point that there are more than one *ways of presentation* of the same object (e.g. the same planet as 'the morning star' as well as 'the evening star') on the base of his distinction of sense and reference. After addressing Rota's interpretation of mathematical objects in terms of a kind of *ideality*, I detect certain similarities among Rota's views and the views of Frege concerning the constitution of arithmetical identity on the grounds of 1-1 correspondence. On the Fregean account, one who gets familiar with 1-1 correlations among the instances of certain concepts becomes able to grasp the concept of natural number and acquire epistemic access to the *abstract* objects which are the instances of it. In Rota's view, the intervention of a subject is necessary for the *constitution* of numbers as *ideal* objects on the basis of certain *invariants* like 1-1 correspondence. I point out an epistemic as well as an ontological aspect of this issue and present an interpretation in phenomenological lines. Subjects detect invariants in mathematical experience which get transformed into *ideal* objects. Yet, as R. Tieszen has noted, transcendental constitution exceeds any human subjectivity. Then I deal with the problem of "mixed mathematical identities" (stated by P. Benacerraf (1965)) on the basis of Rota's use of the phenomenological notion "Fundierung". Set theory is usually taken to be a reduction basis of mathematical theories. In case of arithmetic, two well-known set theoretical versions of natural numbers are Zermelo's and Von Neumann's. I argue that questions of whether, for example, 3 is identified with either Zermelo's version or Von Neumann's version should rather be addressed on a non-reductive account, by the aid of Husserl's 'Fundierung'. Rota takes '*Fundierung' to* rule mathematical stratification and suggests it as a means to exclude any reductionist stance.

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The price ain't right: high costs for determinacy in Hale's semantics and arithmetic structuralism

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Keywords: philosophy of mathematics, structuralism, referential determinacy, second-order logic, model-theory.

In ordinarily asserting '2 is a prime number', we intuitively take ourselves to refer to a determinate number 2 which is prime. Hence, our quotidian mathematical practices point to a demand for determinacy for mathematical reference. However, Benacerraf's famous 'multiple-reductions' problem suggests that we are talking about *nothing in particular*, since '2' can refer to either {{}, {{}}}, or {{{}}} amongst others. Mathematical structuralism has, in return, been touted as a solution to Benacerraf's problem by purporting to recover absolute referential determinacy to mathematical reference. In this paper, I focus on arithmetic structuralism – structuralism about the natural numbers – and ask whether structuralism does achieve this goal, and at what cost. To do this, I examine the nature of second order Peano arithmetic's (**PA^2**) categoricity, specifically the nature of the second-order models underpinning categoricity. I contend that the standard semantics for second-order logic provides a very mysterious sort of determinacy for **PA^2**, and argue that Hale's (2015) recently proposed semantics for second-order logic does not solve this problem. His semantics, I argue, is even more mysterious than the standard semantics: defining the members of the second-order domains of models, under both semantics, turns out to be hypertasks. Finally, I turn to a final option which gives up categoricity and absolute determinacy for a more intersubjective 'determinacy' via internal categoricity. I conclude that the arithmetic structuralist must either bite the bullet on the issue of mystery surrounding categoricity proofs, or accept that absolute determinacy of arithmetic reference is not possible and settle for intersubjectively determinate reference instead. *Prima facie*, determinacy in arithmetic structuralism does not come cheap.

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Everettian Actualism

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Keywords: Everettian quantum mechanics, Relative facts interpretation, Actualism, Alvin Plantinga

One of the requirements of a metaphysical interpretation of Everettian quantum mechanics [EQM] is a theory of modality. This is not only an important element of the development of a metaphysical picture of the world, but also necessary for a full explanation of probability in EQM. Working from the perspective of a single-world interpretation of EQM called the relative facts interpretation [RFI], ((Conroy, 2012), (Conroy, 2016)) this paper argues for a modal theory similar to the actualism that Alvin Plantinga proposed in the 1970s and 1980s ((Plantinga, 1974), (Plantinga, 1976), (Plantinga, 1987)) rather than quantum modal realism, as suggested by Alastair Wilson ((Wilson, 2013), (Wilson, 2015)).

The main parallels that I develop in this paper are between Plantinga's and Everett's conceptions of (1) a "possible world" as a possible maximal states of affairs; (2) there being one "actual world"; (3) the status of states of affairs as facts rather than propositions; (4) there being no states of affairs that do not exist, though some do not obtain; (5) the fact that not all states of affairs are actual, some are merely possible, but those possible states of affairs exist, they just do not obtain.

To develop (1) I argue that we can take each term in the non-separably entangled superposition of a quantum system to be a description (either factual or counterfactual) of the way the world is. I show how each term is maximal in the sense implied by Plantinga, and so each term describes a possible world in his sense. To show this requires some work as there are at least two different ways that one might understand "maximal" in the context of Everettian quantum mechanics. Showing (2)-(5) require significantly less work and the parallels are recoverable from textual exegesis, and largely come from the development of the implications of the RFI.

The paper concludes by briefly considering how Everettian Actualism fares against traditional objections to Plantinga's view. Some of these objections are easily overcome, while others point to places where more work needs to be done. Overall the project argues that with a single-world interpretation of EQM in which generally all facts are relations, the most appropriate modal theory is actualism rather than some version of modal realism.

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Resumptive Pronouns in a Type Logical Grammar

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Keywords: Substructural logic, Contraction and Permutation rules, Resumptive Pronoun, Relative Clause, Portuguese.

The lack of the Contraction rule in a substructural logic challenges the empirical adequacy of a calculus to deal with linguistic phenomenon that require reusing semantic resources as, for example, bound pronouns. In this paper we compare two extensions of the substructural Lambek **L** calculus that encode some restricted form of the Contraction rule: **LA** and **LLC** calculus, respectively. The **LA** Calculus (Jaeger, 1998) extends **L** by adding the triple residual $\{\hookleftarrow, \hookrightarrow, \sim\}$; the logical rules for these connectives produce a local non-linear substructure inside the resource-conscious sequent because the Contraction and Permutation rules are explicitly admitted. The **LLC** calculus (Jaeger, 2005) extends **L** by adding the anaphoric connective $|$. In **LLC** an anaphoric expression is assigned the type $A|B$, which works as an expression of the type A in the presence of an antecedent expression B. In this system, a (long-distance version of) Contraction rule is encoded into the right and left rule for the connective $|$.

We confront these systems with the linguistic problem of the (subject and object) resumptive pronouns (RPs) in Brazilian Portuguese (BP). In Portuguese all of the syntactic positions can be relativized by using the resumptive, non-canonical strategy: a relative clause is introduced by the invariable *que*-form and a resumptive pronoun stands in its (possibly non-peripheral) base position within the relative clause (see 1). In addition, in BP there is no morphological distinction between a subject (nominative) and an object (accusative) resumptive pronoun (see 2-3):

1. O menino **que** eu falei com **ele** ontem
   "The boy such that I talked with him yesterday"

2. O menino$_1$ que **ele**$_1$ está comendo o milho
   The boy that he.NOM is eating the corncob

3. O milho$_1$ que o macaco está comendo **ele**$_1$
   The corncob that the monkey is eating it.ACC

First, we discuss a first challenge posed by RPs for these two type-logical approaches to anaphoric pronouns, as they are necessarily free within the relative clause, although they are necessarily bound in the entire nominal phrase. Despite the fact that the type $(cn\backslash cn)/(s|n)$ with the semantic $\lambda A.\lambda B.\lambda C.((AB) \wedge (BC))$ seems, at first sight, adequate to this purpose, the rules for the pronominal connective $|$ (or $\hookrightarrow$) have to ensure that RPs are not bound by a local antecedent occurring within the relative clause. In addition, in Portuguese, unlike in English and Hebrew, resumptive pronouns, license parasitic gaps such as traces in canonical relative clauses (Asuheh, 2012). Consequently, the system has to ensure that a nominal gap, as well as the resumptive pronoun, is bound by the head of the relative clause.

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Selective Realism in History

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Key words: General philosophy of science / Selective realism / Philosophical history of science

According to selectivism, (a) theories are not monolithic proposals but intellectual constructs made of posits of various degrees of success with respect to truth, (b) empirically successful theories flourish because the world is as some theoretical explanations and narratives they posit say it is, and (c) recognizing this, scientists try to grade theory components accordingly, if with uneven accomplishment. Current selectivism (the *divide et impera* approach) arises most proximately from responses to Laudan's pessimistic inductions from the history of science, but the approach is much older, or so I argue in this paper. I trace selectivism to epistemological and methodological schemes on view since Antiquity—in e.g. Ptolemaic and Copernican astronomy; Galileo's piece-meal approach to the study of nature, also his efforts to embrace realism about both the Bible and the Heliocentric Theory; Newton's proposed reform of natural philosophy, and (at the apex of classical physics) Lorentz's reading of Maxwell's theory. These and numerous other cases, I suggest, show regular recognition by past scientists that successful theories contain both "wheat" and "chaff" that need to be detached from each other, attesting to a selectivist core at work during most of the history of natural philosophy and science. At each stage, this core together with local background knowledge guided gradation of intellectual content and preferred retention as science advanced. Until about the late Renaissance, the resulting rational gradations emphasized deductive reasoning and meta-empirical certitudes; retention of intellectual content was poor except at levels guarded against revision by metaphysical or religious convictions. Ptolemaic astronomy (which, contrary to popular opinion, embodied a partial realist stance) exemplifies this stage well. When at the dawn of modernity natural philosophers began to challenge the content and character of traditional knowledge, the gradation strategy reoriented accordingly. I focus on some emblematic episodes: (a) Galileo (Dialogue, Discourses, also his Letters to Duchess Christina); (b) Newton (Principia, Opticks); and (c) ampliative strategies in the century of Fresnel, Wheewell, Maxwell, and Lorentz. Cases such as these, I suggest, show how and why the tenets of today's *divide et impera* selectivism arose. What counted as acceptable natural philosophy altered along the way, as did the selectivist emphasis, which increasingly shifted towards partial piece-meal descriptions and theories that provided (and were meant to provide) incomplete understanding of their intended domains. It became satisfactory to pursue knowledge through less than apodictic or even deductive proof, a trend fortified by methods focused on recognizing inductive markers of truthful theories. In the early 19th century the markers of choice were parsimony and fruitfulness, predictive power gaining favor only later in the century. Recognition of these inductive indicators has led to unprecedented quality and quantity retentions of theory-parts at inductive levels. A complementary question arises, however: If selectivist schemes have been long in the background, why does selectivism seem new? The last section considers this issue and calls attention to the enduring impact of some views from the mid-twentieth century.

Determinism or indeterminism: what is minimal in tense logics?

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Keywords: tense logics, minimality, time, implication relations, symmetrical property.

In this paper we deal with the birth of tense logic by a double purpose:
(i) We show details from Hamblin-Prior and Lemmon-Prior correspondences and some other unpublished document from VLPS (2011);
(ii) We bring the concept of minimality of tense logic into question.
Our historical analysis reveals formulas and technicalities, in particular we compare K_t and $HAMB$, respectively proposed by E.J. Lemmon and C.L. Hamblin. We focus on the topic by following A.N. Prior's approach to the notion of time, e.g. (Prior, 1967). Prior credits Lemmon for the introduction of K_t. Actually, Lemmon presented to Prior some considerations on K_t in some letters from 1964 till 1966. The correspondence stopped only some months before Lemmon passed away, on July 1966. In particular, (Lemmon, 1966) discloses some observations that are relevant from the semantical viewpoint. Linearity exposes an important feature of $HAMB$: time does not branch, neither in the past nor in the future. Hamblin's first note to Prior about a structure of implication relations is in (Hamblin, 1958). Hamblin focuses on a network of relations between 30 distinct tense logical formulas, all of them derivable from the postulates of his $HAMB$ system.
Lastly: is the symmetrical property problematic for 'minimal' interpretations in tense logics?

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Information causality, the Tsirelson bound, and the 'being-thus' of things

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Keywords: Information causality; Tsirelson Bound; Einstein Separability; Mutually Independent Existence; PR-box

Demopoulos (On Theories, in preparation) has convincingly argued that Einstein's principle of mutually independent existence (MIE) is distinct from his principle of 'local action', but that Local action nevertheless supports MIE in the sense that its complete suspension would make the concept of a (quasi-) closed system, and physical thought with it, impossible. Demopoulos further argues that when MIE is thought of as a special methodological principle, it does not preclude an action at a distance theory like Newtonian Mechanics. For despite Newtonian Mechanics' violation of local action, Corollaries V and VI to the laws of motion allow one to treat spatially distant systems as quasi-closed, and therefore as satisfying MIE. On Demopoulos's view, quantum mechanics also, as a result of the no-signalling principle, satisfies a generalisation of MIE that is appropriate for an irreducibly probabilistic theory. 'Physical thought', in Einstein's sense, is therefore possible in quantum mechanics.

While no-signaling can be used to ground the 'locality' of quantum mechanics in this generalised sense, it does not distinguish quantum mechanics from other non-classical theories. In particular, the Tsirelson bound represents the maximum value of the Clauser-Horne-Shimony-Holt (CHSH) correlation for quantum systems; Popescu & Rohrlich have shown that there are 'super-quantum' theories, whose CHSH correlations may exceed this bound, which satisfy the no-signaling principle.

One can, however, derive the Tsirelson bound from a generalisation of no-signaling called 'information causality' (Pawłowski et. al., *Information Causality as a Physical Principle*, 2009), which asserts that correlations established prior to the choice of a set of data be informationally neutral. Such correlations, that is, should not be exploitable by someone with no access to the data for the purposes of predicting the values of arbitrary elements of it (Bub, *Why the Tsirelson Bound?* 2012, p. 181). On the face of it, this does not sound very different from the no-signaling principle. However 'exploitable' in the context of information causality is to be thought of more generally; it includes scenarios in which a distant party communicates classically with a party located nearby to the data set.

Classical and quantum mechanics both satisfy the principle of information causality. And by assuming information causality, one can derive the Tsirelson bound. But how should one *motivate* information causality? Typically, what is appealed to is the intuition that a world in which information causality is not satisfied would be 'too simple' (Pawłowski et. al. 2009, p. 1101), or 'too good to be true' (Bub, *Bananaworld*, 2016, p. 187; Bub 2012, p. 180).

In this paper I argue that one should rather motivate information causality, and our inquiries in general into the characterisation of the divide between quantum and super-quantum theories, in terms of the methodological principle of mutually independent existence (MIE). Correlations established prior to the choice of a data set, that *themselves* contain information about this data set—in the sense that they are capable of contributing something *over and above* what is contributed via the transmission of a number of classical bits—can, despite satisfying the no-signaling principle, be thought of as violating the principle of local action in a more general (and more subtle) sense than no-signaling. They thus run afoul of a more general version of the methodological principle of the mutually independent existence of spatially distant things.

Model-oriented and data-oriented simulations for the study of brain mechanisms

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Keywords: philosophy of artificial intelligence, philosophy of neuroscience, philosophy of cognitive science

Can computer and robotic simulations be reliably used to produce new knowledge about physical, neuroscientific, and cognitive systems, and under what epistemological and methodological conditions? This question has been addressed in the last two decades by several philosophers of science (Humphreys, 2004; Saam, 2016; Winsberg, 2001). It will be argued here that simulations can produce two broad kinds of knowledge in contemporary cognitive science and neuroscience. This distinction has been only occasionally made in the relevant literature (see, e.g., Guala, 2002; Krohs, 2008). Yet, a deeper consideration of it may offer interesting insights to reflect on the multifaceted role played by simulations in scientific research, and to assess the methodological soundness of contemporary simulation studies.

In some cases, simulations are used to obtain data on the behaviour of a system which are difficult or impossible to obtain by other means for theoretical or practical reasons. For example, simulations are deployed to visualize and study the fine-grained conformational changes of ion channels at a level of detail which is well beyond the resolution of available microscopes (Dror et al., 2012). Studies pursuing this kind of goal will be called data-oriented studies. In other cases, simulations are used to test theoretical models of the target system, notable examples being the simulation studies carried out in Artificial Intelligence and contemporary biorobotics (Cordeschi, 2002). Studies of this kind will be called model-oriented.

Data-oriented and model-oriented studies differ from one another in the type of knowledge they produce about the target system. They also differ in the epistemic requirements they must meet in order to produce that kind of knowledge. I will argue that both data-oriented and model-oriented studies must involve accurate simulations of theoretical models of the target system. Similarity ends here. To generate data about the behaviour of the target system, the theoretical model implemented in the machine must represent the mechanism actually governing the target system. This requirement is not to be met by model-oriented studies: the goal of model-oriented studies is exactly to test if the implemented mechanism represents the mechanism governing the target system or not. Model-oriented studies aim at discovering the truth of a condition which is one of the requirements of data-oriented studies. This implies that the same system cannot serve, at the same time, as a data-oriented and a model-oriented simulation.

This analysis will be based on examples drawn from the contemporary scientific literature on large-scale simulations of brain mechanism, with the additional purpose of discussing the potential interplay between data-oriented and model-oriented simulations.

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Basic structures for an ontology of neutral monism

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Keywords: neutral monism, Mach, Carnap, graph theory, mathematical structures.

My contribution starts by headlining an informal concept of structure that has been used in the philosophical tradition of neutral monism. My aim is to propose a formal concept of structure which may cover the first informal one.

I focus on Mach's neutral monistic philosophy and highlight his epistemological and ontological theses. I propose a mathematical substitute for Mach's informal concept of structure by taking into account the route hinted by R. Dipert (1997) in the paper "The mathematical structure of the world: The world as graph".

Between Mach's contributions in philosophy of science and Dipert's proposal, one finds Carnap's *Aufbau* project of world construction. I draw a comparison between the construction project of Mach and that of Carnap by exploiting a graph-theoretical approach to ontology. Finally, I briefly headline the fruitfulness of using graph theory to clarify traditional philosophical problems.

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Pluralism about logical consequence: the internal/external case

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Keywords: logical pluralism, consequence relations, internal/external, sequent calculus.

Logical pluralism is the view that there is more than one correct logic. In terms of logical consequence, it is the view that there are several equally correct accounts of logical consequence.

In this paper I discuss a thus far neglected route into logical pluralism. This is grounded in a familiar phenomenon in substructural logics: the mismatch between the internal and external consequence relations of certain logics (presented by means of a sequent calculus). Following Tarski, CRs are usually taken to be reflexive, transitive and monotonic; moreover, they hold between a set and a formula.

Given a sequent calculus, there are two ways to associate a CR to it (Avron, 1988; Troelstra, 1992). One is based on reading sequents as consequence claims. Thus a formula A is a consequence of a collection of formulae X iff the sequent $X \succ A$ is derivable in the system. This is the *internal* CR of the calculus. Since the structural rules and properties of sequent calculi can diverge from one logic to another, we can get deviations from the Tarskian concept of logical consequence. Hence the internal CR of a sequent calculus may be, e.g., not monotonic if there are no Weakening rules; or it may hold between multisets rather than a set and a single formula, etc.

The other way of associating a CR with a sequent calculus has it that A is a consequence of X if and only if the sequent $(\succ A)$ is derivable in the calculus together with the extra axioms $(\succ B_i)$, for each $B_i \in X$ and with a primitive rule of Cut (parentheses added for readability). The choice between these two CRs is of little consequence for say, classical or intuitionist logic, where the two relations are (extensionally) equivalent. But it is quite important for other logics, such as linear logic (Girard, 1987) or the non-transitive logic ST (Ripley, 2012), where they come apart.

In this paper I will argue that, when it occurs, the mismatch between the internal and external consequence relations of sequent calculi is indicative of a non-trivial form of logical pluralism.

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The problem of the hierarchy of biological individuals

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Keywords: hierarchy, individuality, natural selection, levels

"*It is a commonplace observation that nature loves hierarchies"*. (Simon 1977). This talk starts from this sentence and tries to establish whether it is true or not that hierarchy is a characteristic of individual living beings.

One of the main problems related to the definition of individuality in biology is how to account for the hierarchical structure of nature, which has been prominently emphasized in evolutionary theory (Dobzhansky 1937; Eldredge 1985; Mayr 1963; Liebermann, Vrba 1995). "*By […] hierarchy, I mean a system that is composed of interrelated subsystems, each of the latter being, in turn, hierarchic in structure until we reach some lowest level of elementary subsystem*" (Herbert 1962).

The aim of this talk is to analyse what are the consequences of this notion of hierarchy for the definition of biological individuality and to give a definition of individuals as hierarchic systems. Exploring the notion of hierarchy and levels of individuals in biology has epistemological and ontological consequences.

First of all, from an epistemological point of view we have to understand whether complexity is the necessary outcome of natural selection. Another problem is to understand why selection at lower levels does not disrupt integration at higher levels (Maynard Smith and Szathmary 1995, Calcott Sterelny 2011). A paradigmatic case in which integration between lower and higher levels is lost is cancer, in which selection acting on cells destroys tissue and organism organisation (Nowell 1976, Okasha 2006). This pathological case seems to show that biological individuals have a hierarchical organization in which the identity of parts depends both on their interactions and on higher-level effects. Therefore, relations among levels are crucial: we need a 'relational ontology of levels' (Bertolaso 2013), which will allow understanding how each level is the result of relations among underlying parts.

Then, from an ontological point of view, the problem we face today in defining the most fundamental level in biological hierarchy is the same of the old metaphysical problem of defining parts and wholes. This is an ontological concern: asking what makes of an aggregate a real individual. (Huneman 2014).

The aim of this talk is to show – through some examples – that considering individuals as hierarchies is more useful for the practice of science than discussing on levels of selection.

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Revising inferences

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Keywords: Truth, Paradoxes, Revision Theory, Proof Theory.

This talk is concerned with the combination of ideas from the 'Revision Theory of Truth' by (Gupta, Belnap , 1993) and a structural criterion of paradoxicality in terms of failure of Cut-elimination as in (Ripley , 2012; Tennant , 1982). This amounts to the construction of a revision process on proof trees, in which proof trees are revised if they are paradoxical (in the sense of our structural criterion in terms of Cut). The constructions is schematic in the sense that one may choose to restrict different rules, leading to different well-behaved sets of proofs.

We work in the language of FOL, Peano Arithmetic (PA) and a unary truth-predicate T with the typical recursive definitions for wffs etc. Our proof system is a suitable sequent calculus system leaving all structural rules explicit and with all axioms of PA as rules as well as rules for a transparent truth-predicate. It is well known that our resulting theory proves a diagonalisation lemma for a Liar sentence: $\vdash \lambda \leftrightarrow \neg T(\ulcorner \lambda \urcorner)$ and thus the empty sequent. It is observed in (Ripley , 2012; Tennant , 1982) that this derivation (and any other paradoxical derivation) requires the use of Cut. However, we know that for our base theory (i.e. our theory without T) Cut-elimination holds.(The subformula property fails, but this need not concern us here). We can thus give an extensionally adequate classification of paradoxical proof trees by saying that a proof tree t is paradoxical iff t includes an ineliminable application of Cut.

The informal idea behind the construction is the following: Start with all simple proofs, i.e. all identity sequents of our language. Then, for the next stage of our construction, we define a function G that applies all rules of our naive system once to these identity sequents giving us more complex proofs of height 1. After each application of G, we apply a revision function $R^?$ that removes all proof trees that share applications of the rule ? with some paradoxical proof tree in the current stage. The fusion of R and G is then abbreviated as $J^?$ (for jump); finite iterations of J ($J^{?,n}$) are defined in the straightforward inductive way. Since these iterations are neither increasing nor monotone, we make use of the notion of a stable tree which is defined similarly to the notion of a stable truth. We define $\mathcal{S}^{?,n}$, the set of all stable trees at some stage n, as $\mathcal{S}^{?,n} := \{t \mid \forall n \forall m\ n < m :\ t \in J^{?,n} \rightarrow t \in J^{?,m}\}$. The limit stage at ω is then defined as $J^{?,\omega} := \bigcup_{n \in \omega} \mathcal{S}^{?,n}$. Depending on the chosen revision rule $R^?$, this limit stage will be a different set of proof trees. However, they all have in common that all rules but the restricted ones remain naive and the restrictions only apply only to applications of rules that are necessary in paradoxical derivations. Thus these constructions provide a way to restrict rules only when it is necessary, leaving all unpathological applications untouched. The main idea for future work is to give (partial) axiomatisations of limit stages via nonmonotonic logics such as ones using default rules.

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Vacuous perceptions: Bridging possible and actual hallucinations

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Keywords: hallucination, phenomenal character, veridical perception, common-kind theory, disjunctivism

Philosophers mainly study perception-like hallucinations as counterexamples to the conception of veridical perception as direct awareness of the external world. Such a view is challenged by the very possibility of perceptual 'mistakes', of which hallucinations constitute the most serious case for their being instances of perception-like experiences of ordinary objects in the absence of such objects. Competing accounts of hallucinations include versions of the traditional 'common kind' view, which understands hallucinations and veridical perceptions in terms of mental states of the same kind, sharing the same phenomenal character, and indistinguishable from one another, as well as disjunctivist positions (Martin 2004) arguing that hallucination and perception, while indistinguishable by introspection, are different in kind.

'Philosophical' hallucinations and 'actual' hallucinations studied in clinical contexts, however, display different characteristics. The goal of this paper is to offer a philosophical account of actual hallucinations, which can bridge the traditional debates in philosophy of perception and epistemology with the recent developments in philosophy of psychology and psychiatry (Macpherson & Platchias 2013). Qualitative and quantitative data on hallucinations suggest that they differ significantly from the standard conception of them employed by philosophers, especially with respect to their alleged indistinguishability from veridical perceptual experiences. Outside philosophy, subjects (at least sometimes) successfully distinguish a veridical perception from an instance of hallucination by relying on phenomenal features of the occurring experience, while possibly describing their hallucinatory states as perception-like experiences (Farkas 2013). Other data (Ratcliffe, forthcoming) suggest that hallucinations may be experienced as possessing different degrees of 'perception-likeness'.

Overall, these findings challenge the conception of 'indistinguishability' employed by philosophers, and its role in characterizing hallucinations. Moreover, they suggest that 'perception-likeness' in hallucinatory experiences might come in degrees. We therefore move from a view of perception, which extends beyond the traditional Aristotelian five senses and considers 'accuracy' of experience as a matter of degrees (MacPherson 2010), to entail that the phenomenal character of hallucinations may or may not be identical to the phenomenal character of veridical perceptions, without requiring that hallucinations and veridical perception be necessarily different in kind. Thus, by employing a scalar notion of both perceptual accuracy and the perception-likeness of hallucinations, we develop an account, sympathetic to common-kind approaches, which can more easily accommodate the cases brought to the debate by clinical psychology.

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The emerging of Biology's foundations from the two major accounts of its history

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Keywords: History of Biology, Mayr, Gould, Two dichotmies, Incommensurability

Both major accounts on the history of Biology (Mayr 1972; Gould 2002) present a deep theoretical divergence inside this science. Since its beginnings theoretical biology was divided into two mutually incompatible models, i.e. the Darwinian one and the Mendelian one.(Howard 2009). This phenomenon is manifested by radical changes in meaning of the basic notions; e.g., the notion of "species" (either phenotype or genotype), "teleology", "genetic variation". By looking for the profound causes of this divergence one sees that Darwin's theory of evolution is based on both the actual infinity in time (which unboundedly and without discontinuities ranges from $-\infty$ to $+\infty$) and an organization of the theory as representing biological beings all originated according to one principle (the natural selection). Instead Mendel's genetics is based on both the potential infinity in time (one generation after another) and an organization of the theory as aimed at solving a problem, i.e. to reduce the complexity of a population to simple particulate models of genetic units. Gould has stressed that this divergence persists in contemporary Biology as the contraposition between functionalism and structuralism. In sum, these two models of a biological theory are based on different choices taken on the two dichotomies concerning the two notions of infinity (in time) and organization; since these dichotomies have been recognized also in Logic, Mathematics and Physics,(Drago 2012) Biology shares the same foundations of all scientific theories. In order to discover Biology's foundations Mayr launched a programme for a 'Science of Science' (1972, title of chapter 18). The original Leibniz's programme of 'Science of Science' has been interpreted by through the two above dichotomies (Drago 1994), which may be traced back to Leibniz's two labyrinths of the human reason.

By defining incommensurable two theories differing in their basic choices on the above two dichotomies, the theoretical incommensurability of the two models of biological theory gives reason of both the above mentioned radical variations in meaning of the basic notions and the harsh debate among the two groups of their supporters. Yet, incommensurability implies neither lack of communication nor untraslatability.(Feyerabend 1977) Indeed, Mayr (1970) has reconciled Darwin's theory with Mendel's theory by elaborating the meanings of the common fundamental notions; e.g. the concept of a gene (concerning the choice on the infinity), which in the new Mayr's theory becomes the genome, and the concept of species (concerning the choice on the organization).

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On the novelty of computer simulations for the philosophy of science

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Keywords: computer simulations; simulation models; mathematical models

In studies on computer simulation, it is common to find the assumption that simulation models —as the models implemented on the computer— are somehow similar to mathematical models—as used in the sciences. Such an identity consists in taking that, by implementing a mathematical model on the computer, we obtain a computer simulation. An example of this is Humphreys' (1990) working definition. Another example is Hartmann (1996), who defined computer simulations as a dynamic model running on a physical computer.

These considerations provide Frigg and Reiss (2009) good reasons for dismissing any philosophical interest attached to computer simulations. According to these authors, computer simulations might raise some interests in mathematics and even in psychology, but not in philosophy. As result, there is nothing philosophically novel about computer simulations that cannot be understood in terms of a more familiar philosophy —whether it is from the philosophy of models or the philosophy of experimentation. In order to keep a firm grip on their novelty, Humphreys (2009) urged to analyse simulations in and by themselves, bringing into the discussion new terms and problems.

With these ideas in mind, I propose to address two core issues. First, to develop an architecture for simulation models that amplifies and illuminates the work of Humphreys (2004). That is, to address the methodology of computer simulations by analysing the components, relations, and methods that comprise a simulation model. The purpose is to show in what respects simulation models are a special class of model alien to mathematical models —and thus worth of philosophical inquiry in its own right.

Thus understood, this architecture makes visible a major characteristic of simulation models, namely, that they *recast* a multiplicity of other models (i.e., theoretical, phenomenological, etc.). In this vein, simulation models have a much richer structure than standard mathematical models. Recasting, as I understand it here, paves the way to the second issue I address in this presentation, that is, the novelty of computer simulations for the philosophy of science.

As mentioned, Humphreys (2009) understood that the novelty of computer simulations is made explicit by showing in which respects the philosophy of models falls short to account for computer simulations. I believe that we can also make precise the novelty of computer simulations by showing in which respects they represent new challenges for established topics in the philosophy of models. To this end, I show how standard theories of representation, justification of results, and the general methodology of scientific modelling can be challenged by computer simulations.

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Confirming theories without considering rival theories

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Keywords: confirmation, eliminative induction, atomism, Bayesianism

Many philosophers think that the confirmation of scientific theories has to be contrastive: Showing that a theory is confirmed by some piece of evidence predicted by the theory necessarily involves the consideration of some or all of the concrete rival theories of the theory. Such a position seems eminently plausible. The confirmation of a theory by some piece of evidence depends on what the rivals predict about the piece of evidence. Roughly speaking, the smaller the number of rivals that predict the evidence correctly, the more the given theory is confirmed by the evidence. However, one cannot know the predictions of the rivals without examining them. So, it seems obvious that one cannot determine to what extent a theory is confirmed by some piece of evidence without considering the concrete rival theories.

This motivates the following definition. Let T be a theory and E some evidence. A piece of reasoning provides direct confirmation for T from E iff it provides confirmation for T from E (i.e., it shows that T is confirmed by E), and it succeeds in doing so without referring to any concrete rival theories of T. For example, the positive part of the hypothetico-deductive method states that a theory is confirmed by true observation statements entailed by the theory. No rival theories are considered, hence instances of the hypothetico-deductive method (if cogent as inferences) provide direct confirmation.

An important kind of non-direct, i.e., contrastive, confirmation is eliminative induction. Eliminative induction consists of two steps: First, a space of possible theories is set up and concrete theories of the space are constructed, second, theories of the space are eliminated through empirical evidence, either one by one or in larger chunks, until one theory remains which then must be true. Thus, the central idea of eliminative induction is that confirmation proceeds indirectly, through a process of elimination of rival theories. This process is pushed forward solely by false predictions of the rival theories, while correct predictions by any theories don't matter. Hence, only the rival theories, but not the last standing theory, need to be considered. In principle, the end of the elimination process, at which one theory remains and is accepted as true, can be reached without this theory ever having been examined and tested.

In my paper I want to show that direct confirmation is possible, i.e., that it is possible to reach a reasoned judgment that a theory is confirmed by some piece of evidence and do so without considering any concrete rival theories. I provide two arguments.

The first argument is an argument from scientific practice. It concerns theories that enjoy very strong empirical support. When scientists discuss and assess the empirical support of a very well-confirmed theory, they usually merely mention the evidence supporting the theory, point out its good-making features, but don't (or barely) discuss any alternative theories. In other words, their reasoning constitutes direct confirmation. A good example is Perrin's assessment of the atomic hypothesis in his book *Atoms* (1916). Perrin marshals the relevant evidence, determines its relationship with atomism, and notes its good-making features, most importantly its diversity. The evidence comes from 13 different phenomena, for example Brownian motion, radioactive decay, and the blueness of the sky. Perrin does not engage in anything resembling eliminative induction: He does not set up and explore a space of alternative theories, develop rival theories to atomism and eliminate them with the help of the evidence. The whole book is solely concerned with working out the 13 different applications of atomism, assembling the evidence, highlighting its good-making features and showing that atomism accords with it. Perrin obviously thinks that this suffices to show that atomism is true.

The second argument for the viability of direct confirmation employs a Bayesian framework. Here the problem is to determine an objective value for $Pr(E|\neg T)$ (where $\neg T$ is the disjunction of the concrete rival theories). I show how this value can be determined in an important class of cases, namely when T correctly predicts the precise outcome E of an accurate measurement of some physical quantity. The details are intricate, but the basic idea is that by reasoning probabilistically about the concrete rival theories and their relationship with E, we can reach an estimate of the probability mass of the concrete rival theories eliminated by E without actually articulating them. So, we can know the extent to which the evidence disconfirms the rivals without actually considering them. This leads to a value for $Pr(E/\neg T)$, which can then be plugged into Bayes' theorem, resulting in plausible values for the posterior of T. The argument adheres to the spirit of contrastive confirmation – the confirmation of a theory by some piece of evidence depends on how the concrete rival theories are related to the evidence –, but defies the letter of contrastive confirmation, because the rivals remain unknown.

A dappled scientific image of time

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Keywords: time, physics, metaphysics, scientific realism.

Since ancient times two different scientific images of time contend in the arena of philosophy: The first one belongs to Plato, who, in the *Timaeus* (37c ff.) states that time is the moving image of eternity. This kind of approach arrives till *Scholium generale* of Newton's *Principia*, where he distinguishes between relative and sensible time on one side, and absolute and theoretical time on the other. The second point of view is endorsed by Aristotle (*Phys.* 219b 1), who maintains that time is the number of movement. In this perspective, which influences strongly Leibniz, time is an entity derived from other physical phenomena.

The relativistic revolution strongly favors an Aristotelian approach, defended above all by Reichenbach (1928) and Grünbaum (1973), which formulate the so called "causal theory of time". But at the beginning of the seventies of the last century a new wave of Platonism arises, promoted by distinguished scholars such as Earman (1970) and Sklar (1974). In recent times, the hole argument (Earman & Norton, 1987) and, more in general, the increasing physical importance of general relativity, have brought new grist to Aristotelians' mill.

This paper moves in the Aristotelian framework, according to which time supervenes on matter and motion.

We believe that the metaphysics of time should start from, and strongly remain anchored in, natural sciences. Although time is *directly* measurable through periodic motions, its ontological structure depends strongly on the theory in which it is embedded. So, a metaphysics of time developed in a dialogue with empirical sciences is possible only if one endorses a form of scientific realism, i.e. the doctrine according to which at least some of the theoretical parts of the most confirmed scientific theories have a partial correspondence in the world.

In our work, we will try to show that, by adopting a moderate form of scientific realism and an Aristotelian perspective, the resulting scientific image of time will naturally have a "dappled" nature, in the sense that physical theories, seriously taken, do not give us a uniform concept of time, but a time with different "tonality", each one related to the different local physical conditions. In a word, a time which is plural, i.e. which is "more than one".

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Identity and indiscernibility in higher-order logic and metaphysics

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Keywords: identity, indiscernibility, higher-order logic.

The question of the identity of two objects is usually settled with the Leibnizian Identity of Indiscernible (PII):

(1) $\forall x \forall y(\forall P(Px \leftrightarrow Py) \rightarrow x = y)$

where the properties in question are commonly restricted to qualitative properties in order to rule out trivial cases.

Recent discussion has focussed on cashing out indiscernibility with first-order, rather than second-order, resources *via* model-theoretic notions. If two objects are mapped by certain automorphisms, then these objects are either absolutely or relatively indiscernible (the converse does not, in general, hold: it is well-known that there are elementarily equivalent structures which are non-isomorphic). Automorphisms have also been used in higher-order metaphysics, the philosophy of mathematics, and the philosophy of physics (for the identity of certain subatomic particles).

But the question of the identity of higher-order entities is not well understood, at least model-theoretically. I aim to fill this gap. *First*, one could easily formulate a higher-order analog of PII. But the problem is just pushed further up in the type-theoretic hierarchy, as no substantial criterion for the indiscernibility of higher and higher-order entities is provided. In particular, PII and its higher-order versions are provably true with relatively mild assumptions about comprehension principles. *Second*, one can investigate the question of the identity of higher-order entities directly *via* model-theoretic notions. But it is well-known that standard results in first-order model theory fails for higher-order languages.

Instead, *first*, I formulate suitable notions of indiscernibility for higher-order entities and discuss preliminary issues about the adequacy of formal languages.

Second, I formulate the following two conjectures (rough):

(2) If two objects of a given type are mapped by certain (suitably defined) automorphisms, then these objects are either absolutely or relatively indiscernible.

(3) If two equinumerous structures are nth-order equivalent, then in general they are not isomorphic.

In other words, (3) suggests that if two objects of a given type are either absolutely or relatively indiscernible by formulas of the relative order, then in general there is no non-trivial automorphism mapping them to each other. On a preliminary investigation, (3) might be independent from ZF, and might follow from some assumptions about certain cardinals.

Third, I discuss the relevance of (2) and (3) for higher-order metaphysics (especially w.r.t. supervenience and the higher-order necessitism/contingentism debate), the philosophy of mathematics (especially w.r.t. structuralism), and the philosophy of physics (especially w.r.t to symmetries and the identity of subatomic particles).

Logics of hyperintensional practical reasons

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Keywords: deontic logic, justification logic, practical reasons.

We introduce a hyperintensional logic for normative reasons based on justification logic. That reasons are hyperintensional means that logically equivalent propositions may be different reasons for the same action (duty, ought, etc.), and therefore cannot be substituted for each other.

The language of Justification Logic is an explicit modal language. In the explicit language formulas of the form $\Box A$ are replaced with formulas of the form $t{:}A$, where $t{:}$ is called a *term*, the deontic reading of which is *t is a reason to do A* or *t is a reason why A is obligatory.* $t{:}(A \to B)$ expresses that t is a reason to do B, on the condition that A holds.

In addition to terms denoting specific reasons the explicit modal language can also contain operations on terms. For instance, say that *s is a reason to do A* and *t is a reason to do B given that A*. By applying s to t we obtain a complex reason to do B, denoted by the complex term $t{\cdot}s$. This is expressed by the principle $t{:}(A \to B) \to (s{:}A \to (t{\cdot}s){:}B)$.

Terms may be either variables, $x, y, z \ldots$ or constants $c_1, c_2 \ldots$

Definition 1 (Normative Reasons Logic RL)**.**

A0. Axioms of classical propositional logic.

A1. $t{:}(A \to B) \to (s{:}A \to (t \cdot s){:}B)$

R1. Modus Ponens

R2. Axiom Necessitation: $\dfrac{\vdash A}{\vdash c{:}A}$ where A is any of A0 or A1 and c is some constant.

This is a very weak logic, weaker even than the basic system of alethic justifications, **J**, which also has the principles involving $+$. What about the explicit version of the characteristic principle of standard deontic logic, D? Its explicit version is the principle:

$$t{:}A \to \neg t{:}\neg A$$

which says that *if t is a reason why A is obligatory then it is not the case that t is a reason why $\neg A$ is obligatory.* Such a principle is appropriate for *pro-toto* reasons, and so we can consider the system RLD as the system of *pro-toto* normative reasons:

$$\mathsf{RLD} = \mathsf{RL} + t{:}A \to \neg t{:}\neg A$$

Of course were we to consider *pro tanto* reasons, such principle would likely be dropped. Finally we might consider that our moral reasons should be in some sense verifiable, hence we might wish to add the principles $t{:}A \to !t{:}t{:}A$ and $s{:}t{:}A \to t{:}A$ to RL to obtain the system RL+ of verifiable and justified deontic reasons.

$$\mathsf{RL}^{+} = \mathsf{RL} + t{:}A \to !t{:}t{:}A \ + \ s{:}t{:}A \to t{:}A$$

Is our logic truly hyperintensional? It is; suppose that A and B are both obligatory, and hence both true in all (accessible) deontically possible worlds and hence in that sense deontically equivalent: from this one cannot conclude that if t is a reason why A is obligatory that t is also why B is obligatory.

Lockean thesis and non-probabilism

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Keywords: Lockean thesis, conjunctive closure, stability theory of belief, probabilism, fuzzy logic.

In philosophy of science and epistemology there are qualitative as well as quantitative theories of belief. A minimal (finitely axiomatizable) qualitative belief system consists of a sentential operator Bel_α for qualitative belief of an agent α and a strongest consistent proposition believed by α (B_α):

$$B_\alpha \nvdash \bot \text{ and } Bel_\alpha(A) \text{ iff } B_\alpha \vdash A \tag{B}$$

Note that conjunctive closure of Bel_α follows from (B). A minimal quantitative belief system consists of the Kolmogorov axioms for absolute and conditional degrees of belief Pr_α:

$$\text{Non-Negativity: } Pr_\alpha(A) \geq 0 \tag{P1}$$

$$\text{Normalization: } Pr_\alpha(\top) = 1 \tag{P2}$$

$$\text{(Finite) Additivity: If } A \not\vdash C \text{ (i.e. } \vdash \neg(A \,\&\, C)\text{), then } Pr_\alpha(A \vee C) \;=\; Pr_\alpha(A) + Pr_\alpha(C) \tag{P3}$$

Partial Definition of Conditional Probability:

$$\text{If } Pr_\alpha(C) > 0\text{, then } Pr_\alpha(A|C) = \frac{Pr_\alpha(A \,\&\, C)}{Pr_\alpha(C)} \tag{P4}$$

For bridging qualitative and quantitative belief, a common threshold-rule, the so-called *Lockean thesis*, is proposed:

$$Bel_\alpha(A) \text{ iff } Pr_\alpha(A) \geq r > \frac{1}{2} \tag{L}$$

However, as the *lottery paradox* as well as the *preface paradox* show, (B), (P1–P4) as well as (L) taken together are inconsistent for reasonable choices of B_α, Pr_α, and a threshold r. There are several ways to depart from here. One is to modify some doxastic principles as, e.g., (B). One is to modify the minimal probability calculus (P1–P4). And one is to modify the full Lockean thesis (L). The first option is taken up, e.g., by Henry Kyburg who restricts conjunctive closure for Bel_α. The third option is the most conservative modification regarding epistemological orthodoxy and is taken up, e.g., by Hannes Leitgeb's *stability theory of belief*. The second option, however, has, to our best knowledge, nobody taken up in order to uphold (L), namely to modify the probability calculus in order to uphold (L). In this paper we are going to explore which kind of probability logic (a fuzzy logic) follows for Pr_α from upholding (B) and (L).

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A top-down account of explanation in Quantum Information Theory

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Keywords: explanation; information, nonlocality

In this paper I examine some ways in which Axiomatic Reconstruction of Quantum Theory (QT) in terms of Information-Theoretic principles (ARQIT) (e.g. Popescu and Rohrlich, 1994) can contribute to explaining and understanding quantum phenomena, as well as describe their explanatory limitations.

I first of all offer the outline of an account of scientific explanation within the context of ARQIT and secondly I investigate the ontological consequences of this account to evaluate the role of ARQIT in the foundations of QT. ARQIT provides novel and genuine explanations of some aspects of non-locality, but it fails to address what is seen by most as the problem of non-locality, i.e. the problem of how quantum correlations occur. Although the proposed analysis is centred on one specific theory as a case study, a good part of the analysis here proposed is grounded on generic features of ARQITs and their explanations. For the most part, the conclusions I will reach can therefore be extended, mutatis mutandis, to ARQIT in general.

As a concrete illustration of how these theories can contribute to the explanation and understanding of the quantum world, I analyse the accounts ARQITs provide of non-locality, defined minimally as quantum entanglement, yielding non-local quantum correlations in the sense of Bell's theorem. In § 1 I illustrate my case study, taken from partial reconstructions of QT (Popescu and Rohrlich 1994, Brassard et al. 2006), whose aim is to find an answer to the question 'why is our world only this much non-local, and not more than that?'.

In § 2 I make explicit some central features that should be accounted for by a model of explanation in ARQIT. I argue that ARQIT addresses one of the central questions in the foundations of QT: how does the quantum world differ from the classical one? Elaborating on the comparative kind of understanding that ARQIT provide, I conclude that ARQIT's explanation of (some aspects of) non-locality corresponds to a very concrete sense of explanation, i.e. that you explain a feature or a behaviour P of s by showing how P depends on the essence of s. Building on this, I present the proposed account of explanation in ARQIT, which takes inspiration from Mark Steiner's (1978) account of explanatory proofs. In his account Steiner replaces the notion of essence with the notion of characterising property, defined as a "property unique to a given entity or structure within a family or domain of such entities or structures" (Steiner 1978, p.143). According to Steiner's account, an explanatory proof:

"makes reference to a characterizing property of an entity or structure mentioned in the theorem, such that from the proof it is evident that the result depends on the property. It must be evident, that is, that if I substitute in the proof a different object of the same domain, the theorem collapses; more, I should be able to see as I vary the object how the theorem changes in response." (p.144)

With the notion of characterising property, defined over a given family or domain, this account traces the comparative kind of understanding provided by ARQIT explanations. The definition of characterising property therefore adequately describes the (conjunction of the) principles of ARQITs, whose function is to isolate QT against a family of theories: the principles of Popescu-Rorlich boxes aim at discovering a set of principles (characterising properties) isolating QT against the family of all (existing or imaginary) non-local theories that don't allow superluminal signalling.

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What is real in levels of reality

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Keywords: Fundamentality, Dependence, Ontic Structural Realism.

I take a level of reality to be the set of truthmakers that could be said to be described and represented by the propositions of a specific scientific domain. I consider these truthmakers as real patterns in the sense of (Dennett 1991). What kind of ontological commitment does a real pattern require? Are atoms real? There is a real structural feature of reality to which the science community has given the name "atom". The description in terms of atoms has a certain domain of applicability, which more or less coincides with the extension of the level of reality under consideration. I shall assume that in the set of possible descriptions of features of the world one could find ordered levels from coarse-grained to fine-grained. The main assumption is that a molecule depends for its existence on the atomic structure, but not vice versa. Considering the truthmakers of the atomic and molecular descriptions and the priority ordering there are three open possibilities: (1) The atom is real and the molecule not, (2) both the atom and the molecule are real, (3) neither the atom nor the molecule are real. I take here a middle level in the "graining" scale and focus on the fundamental physics in the last section. Option (1) presupposes a certain form of supervenience (the molecule is nothing over and above the atomic structure). One of its main features is that it leads to a regress which should be halted by positing a bottom level (the molecule is nothing over and above the atomic structure, which is nothing over and above the subatomic structure and so forth). The cost of not doing so is that (1) would collapse into (3), since no level could be said to be the "real" one. The main issue arising from option (2) is lack of parsimony, since applying the same strategy to the whole body of science would commit us to a huge amount of entities equally fundamental, or an ordered system with relations of ontological priority. I assume parsimony to be a guiding principle in ontological research, so I take (2) to be the least palatable option. Concerning option (3), the situation might not be as bad as it seems for the scientific realist. It all depends on what we mean with "real". If we let go the idea that the atom and the molecules are entities of some kind, we could still maintain that the relative descriptions get right some real features of the world. Depending on what we aim to represent, a description in terms of atoms or molecules would be suitable, but that neither of those imply a relation to an entity-atom or an entity-molecule. As for the priority relation, we could benefit from the recent debate about grounding and metaphysical dependence but try an antirealist stance towards it. Let's now consider the relation between (1) and (3), and assume the minimal description of a hypothetical fundamental structure in terms of minimal relations and minimal relata. (Esfeld et al. 2017) show that such a description could suit Quantum Mechanics. In their version of (1) the relations are taken to be of a spatial kind, while the relata are taken to be unextended matter points. They also argue that there is only a conceptual difference between the two, so one could go one step further and assume that this line of thinking commits one to a monistic fundamental network, since relata and relations do not pick up different substances. Combining what I said about fundamentality and (1) with (3), my aim is to take the best of both options and defend the claim that the fundamental description in terms of a relational network is not "more real" than molecules, it just is the finest-grained. Consequently, in the grounding talk, it is taken to be the most fundamental. However, since that is a fictional story, there is not a "more fundamental" reality. There is but one reality and a plethora of suitable descriptions for it. What science does is to help us find the most suitable ones and the finest-grained ones, as the finest-grained may be a minimalist relational network.

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Is there a *pictorial* motor world? The case of *pictorial* action properties

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Keywords: Picture Perception, Action Properties, Dorsal Visual System, Motor Neuroscience

What perceptual state are we in when we see an object in a picture? In other words, what kinds of properties does our visual system attribute to the depicted object? Call this question 'PSPP'. In order to answer PSPP, philosophers (Matthen 2005; Nanay 2011, 2015) relied on the two visual systems model, according to which our visual system is divided into two streams, a ventral stream for object recognition, whose processing is allocentric, and a dorsal stream for visually guided action, whose processing is egocentric (Milner and Goodale 2006) - the account of picture perception that follows this model is called the dorsal/ventral account of picture perception (henceforth: DVAPP) (Nanay 2015; Ferretti 2016a, 2016c, 2017). Following this model, the DVAPP denied that we can be in a dorsal perceptual state when perceiving a depicted object. This is because a depicted object is not physically graspable/manipulable and cannot be egocentrically localized, as a normal object, by the dorsal stream. This fact led philosophers to be sceptical about the possibility of dorsal attribution of action properties to depicted objects. Here I show that dorsal perception can ascribe action properties to depicted objects. This genuinely brand new claim suggests the possibility of the presence, in perception, of *pictorial action properties* and, thus, of a *pictorial motor world* and offers a possible answer to the important question about which are the properties of the world represented in perception (Siegel 2010). I first report the argument by the DVAPP about the impossibility of dorsally attributing action properties to depicted objects. Then, I introduce evidence on the dorsal visuomotor system, which is involved in detecting action properties (Ferretti 2016a, 2016b, 2016c, 2016d; Zipoli Caiani and Ferretti 2016; Ferretti and Chinellato, in press). Thus, I report evidence concerning the visuomotor activation of the dorsal stream during picture perception. Using this evidence I will suggest that we can attribute action properties not only to normal objects, but also to depicted objects. Therefore, picture perception and face-to-face perception are more similar than previously thought.

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Popper's third world as Frege's kingdom of objective thoughts

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Keywords: kingdom of objective thoughts, psychology, third world, epistemology without a knowing subject

In the proposed paper we will analyze the theory of the third world introduced by Karl Popper in "Objective Knowledge. An Evolutionary Approach" in relation to Frege's kingdom of objective thoughts. The main purpose of this paper will be presenting influences of Frege's ideography on Popper's works about the third world as well as pointing out similarities between those theories and characterizing their different critical views of the status and role of psychological theory of meaning.

Frege's ideography has provided reasons to believe in the possibility of supporting metaphysical assertions on the basis of formal logic, which was considered as an innovative perspective in the field of formal logic. This approach has turned the theory of meaning into the position of the first philosophy (position occupied previously by the theory of knowledge); earlier we had only some formal languages based on metaphysical assumptions. Frege in his paper about sense (Sinn) and reference (Bedeutung) stated that the sense of a sentence is the special thought which can be considered as an independent thought of our acts of thinking and which is universally objective. (Frege, 1948) Frege illustrated this point of view by developing his own theory of meaning. We will focus on consequences of his theory and make use of it while analyzing Popper's concepts. It will require introducing some logical aspects coming from Frege's ideography, e.g. act of judging and act of thinking or distinction between sense and reference.

The Popperian distinction is as follows: (1) the world of physical objects, (2) the world of subjective experiences, (3) the world of objective knowledge, i.g. the knowledge contained in books or papers (abstractive products of human minds). (Popper, 1979).

Popper, inspired by works of Heitinga, found that the problematic situation manifesting itself in the third world sheds much light on the second world, however never vice versa, so through that link, between the third and the second world, we are constantly developing our understanding of objective knowledge. What is more, as suggested by Popper, there is a close analogy between the development of the knowledge and biological development-evolution. Biological development is dependent on the structures from the third world, so the reason why the Popperian theory is called an anti-psychology theory is that it is leading from effects to causes, not vice versa. The third world is an autonomous world and any subjective findings can expand it. Our discovery of knowledge doesn't arise from itself, but they are stimulated by the objective development of knowledge. For instance, the discovery of natural numbers entails a distinction between even and odd numbers or recognition of primes numbers. The third world is actually a product of human mind, what doesn't means that it is not an autonomous product.

To sum up, we will examine Popperian epistemology without knowing the subject as a kingdom of objective thoughts. After that, we will distinguish these theories and use it as a standpoint to criticize the psychological theory of meaning.

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Stopping rules as experimental design

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Keywords: philosophy of statistics, likelihood principle, stopping rules, experimental design.

A "stopping rule" in a sequential experiment is a rule or procedure for deciding when the experiment should end. Accordingly, the "stopping rule principle" (SRP) in statistical inference states that, in a sequential experiment, the evidential relationship between the final data and a hypothesis under test does not depend on the stopping rule: the same data should yield the same evidence, regardless of which stopping rule was used. For example, consider two sequential experiments with binary outcomes, such as a sequence of coin flips. The first has the following stopping rule: perform 12 experiments (coin flips) and report the number of successes (heads). The second has a different stopping rule: continue to perform experiments (coin flips) until the number of successes (heads) reaches 9. Now suppose the second experiment also records 9 successes (heads). The SRP will then demand that the two experiments bear the same evidential value for any hypotheses about the bias of the coin.

In general, Bayesian statistical methods satisfy the SRP insofar as they rely on likelihood ratios, which are invariant under different stopping rules for the same data. On the other hand, classical statistical methods (whether Fisherian or Neyman-Pearsonian) do not, insofar as they rely on test statistics whose values depend on the probability distribution of possible—not just actual—data, and clearly the two sequential experiments' possible outcomes are not the same. Thus the SRP (along with the so-called likelihood principle, which entails it,) is a central point of contention between the two schools of evidence statistical inference.

I consider a variety of arguments advanced in both the statistical and philosophical literature in favor of the SRP—the argument from intentions (Savage, 1962; Edwards, 1992) and the arguments from deception, waste, and impracticality (Sprenger, 2009)—in light of viewing a stopping rule as an integral part of a sequential experiment's design. I also consider arguments to explain away the undesirable features of the SRP (Backe, 1999; Steele, 2013), such as the so-called "sampling to a foregone conclusion." Doing so reveals that many of these arguments are unsound, and only an argument from decision theory (Sprenger, 2009; Malinsky, 2015) weighs inconclusively on the SRP. But, by conceiving of stopping rules as a part of an experiment's design, one can clarify which aspects of that design are evidentially relevant for hypotheses tested by those experiments.

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On the ontology of biological species

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Keywords: Biological species, Species-as-individuals view, Species-as-kinds view

For a long time, biological species were considered universals or kinds of which the organisms that are members of the species are instances. This view, which dominated from Aristotle to Linneaus, was undermined by Evolutionism. According to this theory, biological species have a temporal beginning and a temporal end, and they evolve. Abstract entities, such as universals, are usually conceived as timeless and, thus, as entities that have no beginning and no end and cannot undergo changes. Many scholars have concluded that biological species cannot be kinds (Hull (1965a), Hull (1965b), Sober (1980)). A new ontological interpretation of species and of the relations between species and members of species was necessary. Michael Ghiselin and David Hull, among others, have claimed that species are complex individuals and that organisms belonging to a species are parts of that species (Ghiselin (1966), Ghiselin (1974), Hull (1976), Hull (1978)). Individuals have a beginning and an end, and they can change over time. Therefore, they seem to be the right ontological category into which to place biological species. The view that species are individuals suffers from problems that can hardly be overcome (Stamos (2003), Slater (2013)). However, it is not my aim here to review these problems. This talk has another aim: the main reason for adopting the species-as-individuals view is that species evolve; therefore, we need to investigate whether the evolution of species is compatible with the thesis that species are kinds. I will defend two theses:

1) we actually speak of the beginning, extinction, and evolution of entities different from biological species that are undoubtedly abstract. Therefore, the evolution of biological species cannot be a decisive argument for considering them individuals. Here, I will focus specially on languages in light of the fact that biological species and languages show relevant resemblances, which were noticed by Darwin himself (Darwin 1871: 59-61). Other examples, such as theories and cultures, would be equally suitable for the aims of this talk. A language is constituted by two components: a) a system of signs and b) a set of syntactic rules. Signs, as types, are abstract entities. But the second component is abstract, too, being constituted by a set of rules. Thus, languages are complex abstract entities that are formed by simpler abstract entities. However, languages, as species, have a beginning in time, evolve, and become extinct.

2) I will show how timeless entities, such languages and biological species, can have a temporal evolution. Timeless status and evolution do not seem to be compatible. However, my aim is to show that there is a sense in which they actually are. The basic idea is this. Words and rules are abstract entities, but the fact that they are instantiated in particular uses of a language is a temporal fact. Words start to be used, then are used for a certain amount of time, then fall in disuse. However, we do not say that we speak a new language when one of its words is not used anymore. Rather, we say that the language we speak has changed a little bit. In a parallel way, the traits that constitute a species are abstract, but the fact that they are instantiated in a population of organisms is a temporal fact. Traits start to be instantiated, then are instantiated for a certain amount of time, then disappear. However, if a single trait is no more instantiated, we do not say that a new species has arisen. Rather, we say that the species has changed a little bit.

Who is afraid of scientific imperialism?

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Keywords: Scientific Imperialism; Scientific Progress; Disciplinary Autonomy; Disunity of Science; Epistemic and Pragmatic Justification.

Over the last few decades, there has been significant philosophical discussion of scientific imperialism (henceforth, SI), the systematic application of a discipline's findings and methods to model and explain phenomena investigated by other disciplines (e.g. Cartwright, 1999, Dupré, 1995, Mäki, 2013). The involved authors provided increasingly sophisticated conceptualizations of this notion and debated at length about the justifiability of SI. There are at least two reasons why SI deserves philosophical scrutiny. First, SI contributions target a wide range of both natural and social disciplines, and have widespread implications for modelling and theorizing in these disciplines (e.g. Lazear, 2000, on the impact of economists' SI contributions). And second, SI contributions raise pressing epistemic and pragmatic concerns, which bear on issues of great societal and political relevance (e.g. Dupré, 2001, on the political implications of evolutionary psychologists' SI contributions). To date, however, widespread disagreement remains regarding both the identification and the normative evaluation of SI. In this paper, I aim to remedy this situation by articulating an informative characterization of SI and by providing a normative evaluation of the most prominent criteria proposed to ground opposition to SI.

The paper is organized as follows. In *Section 2*, I explicate the notion of SI and distinguish it from various forms of non-imperialistic cross-disciplinary interaction. In *Sections 3-6*, I identify and appraise four influential criteria proposed to ground a normative critique of SI. I consider in turn: the objection from *disciplinary autonomy* (e.g. Aizawa and Gillet, 2011, Fodor, 1974), which opposes SI contributions on the alleged ground that these contributions reduce or threaten the relative autonomy of the imperialized disciplines from the imperializing ones; the objection from *the disunity of science* (e.g. Cartwright, 1999, Dupré, 2001), which holds that the modelling and explanatory differences between the imperializing and the targeted disciplines undermine the prospects of SI interactions between such disciplines; the objection from *counterfactual scientific progress* (e.g. Clarke and Walsh, 2009), which opposes SI contributions insofar as these contributions preclude the targeted disciplines from progressing in the way they would have progressed in the absence of SI contributions; and the objection from *cumulative constraints*, which subordinates the justifiability of SI to the satisfaction of a series of ontological, axiological, institutional, and epistemological constraints (e.g. Mäki, 2013).

I shall argue that these criteria provide an informative basis for assessing some instances of SI, but do not yield cogent reasons to think that SI is inherently disputable or unjustified. If correct, this result has at least three implications of general interest for the ongoing debate about the justifiability of SI. First, the critics of SI should provide more convincing reasons for their opposition to SI and ground their calls against SI on more plausible empirical and normative presuppositions. Second, what is objectionable (if anything) about some SI contributions is not their imperialistic character, but rather the empirical and/or normative flaws in their presuppositions and the unwarranted societal and/or pragmatic implications some derive from such contributions. And third, the justifiability of SI contributions is best judged in terms of specific case studies rather than in terms of general evaluative criteria that abstract away from the modelling and explanatory practices of the examined disciplines.

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Causal exclusion and causal Bayes nets

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Keywords: causal exclusion; mental causation; causal Bayes nets; interventionism

In a nutshell, causal exclusion arguments (cf. Kim 2005) assume non-reductive physicalism and conclude from several premises that mental properties supervening on physical properties cannot cause physical or other mental properties. The notion of causation used in these arguments is, however, typically somewhat vague and not specified in detail. Because of this, the validity of these arguments may depend on the specific theory of causation endorsed.

In this talk I reconstruct two versions of exclusion arguments and evaluate their validity within the theory of causal Bayes nets (Spirtes et al. 2000). The theory evolved from the Bayes net formalism. It connects causal structures to probability distributions and provides powerful methods for causal discovery, prediction, and testing of causal hypotheses. It probably gives us the best empirical grasp on causation we have so far. Hence, it allows for an empirically informed treatment of causation and, so I hope, also for an empirically informed evaluation of the validity of causal exclusion arguments.

Another strong motivation for this endeavor is that causal exclusion arguments have recently been intensively discussed (e.g., Shapiro & Sober 2007; Woodward 2015) within an interventionist framework, and that interventionist accounts do have a natural counterpart within the theory of causal Bayes nets (CBNs). So the hope is that we can draw as of yet unconsidered conclusions for the interventionist debate surrounding causal exclusion arguments from a reconstruction on the basis of the theory of CBNs. This seems especially promising since one of the main problems interventionists have when testing causal efficacy of properties standing in supervenience relationships to other properties is that these properties cannot be simultaneously manipulated by interventions. So the interventionist account seems to have some kind of a blind spot when it comes to testing causal efficacy of such properties. The theory of CBNs, on the other hand, provides a neat and simple test for causal efficacy not requiring fixability by means of interventions.

The talk will be structured as follows: In part 1 I briefly introduce two variants of the causal exclusion argument. In part 2 I reconstruct these two variants within the theory of CBNs and evaluate their validity. This requires an answer to the question of how supervenience relationships should be represented in CBNs and a test for evaluating whether the instantiation of a property X at least sometimes contributes something to the occurrence of another property Y. I will argue that supervenience relationships can be treated similar to a CBN's causal arrows. A method for testing a property's causal efficacy is already implemented in the causal minimality condition (Spirtes et al. 2000, p. 31). I conclude part 2 by demonstrating that mental properties supervening on physical properties cannot be causally efficacious if causal as well as supervenience relations are assumed to obey the core axioms of the theory of causal nets. In part 3 I investigate the consequences of these findings for the interventionist debate on the causal exclusion argument. In part 4 I defend my suggestion to treat supervenience relationships similar to causal arrows against an objection raised by Woodward (2015).

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Applicabilty problems generalized

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Keywords: Applicability of Mathematics, Philosophy of Mathematics, Mathematics, Physics.

The effectiveness of mathematics in physics has been topic of debate in the philosophy of science in the last decennia (see for example Steiner 1998; Pincock 2012; Bangu 2012). In their attempt to clarify the applicability of mathematics to physics, philosophers usually only focus on cases of applicability of mathematics to physics and ignore *other* kinds of application *of* (or *to*) mathematics. However, since the application of mathematics to physics is just part of the more complex interrelation between physics and mathematics, it might be that such an approach is actually too narrow. Maybe, if we better understand how this kind of application (from mathematics to physics) compares to other kinds of application, we might be able to better understand the applicability of mathematics to physics as well.

A kind of applicability, which is usually not taken into account when dealing with the problem of math-to-physics application, is the application of *physics to mathematics*. This subject has been broadly neglected by the philosophical debate on the applicability of mathematics (to my best knowledge, Urquhart 2008a and Urquhart 2008b are the only relevant exceptions). Actually, in contemporary physics and mathematics there is a *fruitful circulation* of methods and representative strategies, in which not only mathematics can be effectively employed to modelize physics, but also physics can be fruitfully "applied" to mathematics to generate new strategies of mathematical analysis. This (unreasonable?) *effectiveness of physics in mathematics* is still unheeded by the philosophical community and awaits to be explored.

The presupposition that these kinds of applicability are completely different from (and therefore not relevant for) the understanding of the applicability of mathematics to physics might well be wrong. If there were analogies between these three kinds of application, then we might exploit these analogies in order to offer a *generalized* account for mathematical application, and to better understand the complex relationship between physics and mathematics.

In this talk I am going to develop this suggestion. I will present some examples of math-to-physics, math-to-math, and physics-to-math application. Then I will make some considerations about the possible analogies that can be traced among them, and I will analyse whether these analogies might be of any help in clarifying the applicability problems and the relationship between physics and mathematics.

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A four-level account of scientific representation

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Keywords: scientific models; scientific representation; semantic view.

According to a basic version of the semantic view, a theory consists of a theoretical definition, which specifies a class of mathematical systems, and a theoretical assumption, which specifies a relation between mathematical systems and paradigmatic physical systems belonging to the intended domain of the theory. The mathematical system plays the role of (i) *logical model* with respect to the theoretical sentence (it satisfies that sentence) and (ii) *representational model* with respect to the physical systems. Still, while the relation of satisfaction is well-understood, the characterization of the relation of representation between a source system and a target system, ϱ for short, is extremely controversial. In the current debate (see French 2014, ch. 5, for an introduction), ρ is typically characterized as an agent-dependent relation. In fact, the project of providing an objective, agent-independent, characterization of ϱ, either as a relation of similarity (Giere 1988) or as a kind of morphism (van Fraassen 1989, Bueno & French 2011) is subject to well-known criticisms (van Fraassen 2008, Giere 2010). By contrast, the appeal to agent-dependent conceptions of ϱ takes care of the facts that (1) ϱ is irreflexive and asymmetric, since it is the source system that is selected as a representational tool; (2) only some respects of similarity are relevant, since the source system is selected in view of some traits and not others; and (3) a model is an abstract representation of a concrete system, since the link between the source and the target system is provided by the agent.

Our main aim here is to provide an objective, agent-independent, account of ϱ, based on a decomposition of ϱ into three representational relations, ϱ_1, ϱ_2, and ϱ_M, and a corresponding distinction between: (1) a set of data M_1, which are ϱ_1-connected to a physical system S and synthesize our observations on the behaviour of S; (2) an empirical model M_2, which ϱ_2-represents the observed behaviour of S as an empirical path through a certain observational space of states; and (3) a mathematical model M_M, which is an instance of a mathematical law and constitutes an ideal ϱ_M-representation of the empirical path associated to S as an abstract path through a certain abstract space of states. The characterization of ϱ as $\varrho_M \circ \varrho_2 \circ \varrho_1$ we offer is new, due to the way ϱ_1, ϱ_2, and ϱ_M are construed. In particular, we will argue that: ϱ_1 is an appropriate inverse of a *measurement operation*, to be conceived of as a homomorphism; ϱ_2 is an appropriate inverse of a *data modelling operation*, to be conceived of as a partial similarity; ϱ_M is an appropriate kind of *abstract modelling operation*, to be conceived of as a *homotopy*, and thus as an equivalence relation. As we will show, this account (i) fits the intuition that a mathematical model represents a physical system precisely when what is observed about the physical system can be accounted for by the model, (ii) takes care of the relevant traits of the relation of representation, and (iii) is agent-independent, since the construction of ϱ is independent both of the purposes and of the perspectives of scientists.

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Hilbert and the foundations of theory of plane area

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Keywords: Hilbert; purity; plane area; De Zolt's axiom; continuity.

The importance that Hilbert bestowed to 'purity' in his early axiomatic investigations is well known, and it has been recently analyzed in two important works (Hallett, 2008; Arana & Mancosu, 2012). In this context, 'purity' is tied to the requirement of the 'purity of methods of proof', according to which theorems must be proved, if possible, using means that are suggested by their content (Cf. Majer & Hallett , 2004, pp. 315–316). A prominent example of this kind of purity inquire is the (im)possibility of finding a purely projective proof of Desargues' theorem in the plane, avoiding any kind of spatial assumptions. Now, it can be argued that purity demands were also operating more generally in Hilbert's axiomatic construction of Euclidean geometry. To be more precise, a central concern that motivated Hilbert's axiomatic investigations from very early on was the aim of providing an *independent basis for geometry*. By proving that one is not required to resort to any kind of numerical assumptions in the construction of a major part of elementary geometry, Hilbert was pursuing the central epistemological goal of showing that geometry should be considered, regarding its foundations, a self–sufficient or autonomous science. Then, a main goal of Hilbert's axiomatization was not only to show that geometry should be considered a pure mathematical theory, once it was presented as a formal axiomatic system; he also aimed at showing that in the construction of such an axiomatic system one could proceed purely geometrically, avoiding concepts borrowed from other mathematical disciplines like arithmetic or analysis.

The aim of this presentation is to analyze the relationship between these purity demands and Hilbert's reconstruction of the theory of plane area in *Foundations of Geometry*. On the one hand, I will argued that the construction of this central part of elementary geometry presented a serious and appealing challenge to Hilbert's general aim of providing a *purely synthetic* axiomatization of this geometrical theory; in other words, to his epistemological and methodological concerns of constructing elementary geometry without resorting to any kind of numerical assumption. On the other hand, I will claim that purity concerns were also behind Hilbert's search for an "elementary proof" of the *axiom of De Zolt*, i.e. a geometrical proposition whose validity is indispensable for the construction of the theory of plane area. Finally, I will conclude with a more general discussion on the role played by 'purity', as a methodological and epistemological guiding principle, in Hilbert's axiomatic construction of Euclidean geometry.

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From labelled to internal sequent calculi for Lewis' conditional logic $\mathbb{V}$

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Keywords: conditional logics; proof theory; internal calculus; external calculus.

Conditional logics extend the language of classical propositional logic with a new conditional connective $>$, suited to represent conditional sentences - such as counterfactuals - that cannot be captured by material implication. In his book *Counterfactuals* (1973), David Lewis defined a whole family of conditional logics characterized by *sphere semantics*, a generalization of possible world semantics. Sphere models are special types of neighbourhood models, with the property that for any world x, the family of neighbourhoods of x is nested.

We define a labelled sequent calculus $\mathbf{G3V}$ for Lewis' system $\mathbb{V}$ and show how this calculus can be used to simulate the rules of an internal sequent calculus for the same system. The calculus $\mathbf{G3V}$ is a **G3**-style labelled calculus based on neighbourhood semantics, similarly to the calculi for conditional doxastic logic **G3CDL** (Girlando et al, 2016a) and preferential conditional logic **G3CL** (Negri and Olivetti, 2015). Unlike **G3CL**, which is based on the conditional operator $>$, the present calculus takes as primitive the comparative plausibility operator $\preccurlyeq$, thus being (to the best of our knowledge) the first labelled system which explicitly accounts for this connective. The semantic condition for $\preccurlyeq$ is $x \Vdash A \preccurlyeq B$ iff $\forall \alpha \in I(x)(\alpha \Vdash^{\exists} B \to \alpha \Vdash^{\exists} A)$, where $\alpha \Vdash^{\forall} A$ iff $\forall y \in \alpha(y \Vdash A)$ and $\alpha \Vdash^{\exists} A$ iff $\exists y \in \alpha(y \Vdash A)$. This condition justifies the (sound) rules for the operator in the labelled calculus.

It is possible to simulate in $\mathbf{G3V}$ the rules of the *internal* sequent calculus $\mathcal{I}^{\mathsf{i}}_{\mathbb{V}}$ for $\mathbb{V}$ (Girlando et al, 2016b). The sequents of $\mathcal{I}^{\mathsf{i}}_{\mathbb{V}}$ have the form $\Gamma \Rightarrow \Delta, [\Sigma_1 \lhd A_1], ..., [\Sigma_n \lhd A_n]$, where each $[\Sigma_i \lhd A_i]$ is called a *block*, with Σ_i multisets of formulas and A_i formulas. The intended interpretation for a block $[S_1, .., S_k \lhd A]$ is $(S_1 \preccurlyeq A) \vee ... \vee (S_k \preccurlyeq A)$, i.e. a block is a syntactic structure representing a disjunction of $\preccurlyeq$- formulas.

Our result relies on the fact that each block can be interpreted in the language of the labelled system as expressing the semantic condition which corresponds to a block. Thus, to each $\mathcal{I}^{\mathsf{i}}_{\mathbb{V}}$ sequent $\Gamma \Rightarrow \Delta, [\Sigma_1 \lhd A_1], ..., [\Sigma_n \lhd A_n]$ there corresponds, modulo a translation t, a $\mathbf{G3V}$ sequent $a \in I(x), a \Vdash^{\exists} A_1, ..., a \Vdash^{\exists} A_n, \Gamma^t \Rightarrow \Delta^t, a \Vdash^{\exists} \bigvee \Sigma_1, ..., a \Vdash^{\exists} \bigvee \Sigma_n$, with a new label. The translation is used to establish completeness of $\mathbf{G3V}$, since $\mathcal{I}^{\mathsf{i}}_{\mathbb{V}}$ is cut-free complete. A full equivalence result between the two calculi, possibly extended to the other systems of Lewis' logic, is object of current research.

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How to FOOL the expressive power of RDF

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Keywords: semantic web, knowledge representation, formal ontology, ontological language, first order language.

The basic idea of the Semantic Web is that the whole complex of our knowledge is a huge directed labeled graph. Each node of this graph is an IRI (Internationalized Resource Identifier) of a resource. A resource is an entity of any kind (document, thing, event, concept, etc.), while its IRI is an appropriate string of Unicode characters that globally and univocally identifies the resource. Each arrow of the graph is labeled by an IRI that identifies a two-place relation (called a "property" in RDF and its ontological extensions RDFS and OWL). Such a relation is intended to hold for the ordered pair of resources that corresponds to the source and target node of the arrow.

RDF is the declarative language that allows us to represent a knowledge base as a directed labeled graph. In fact, according to the W3C specifications of this language, any set of RDF statements can be formally identified with an appropriate directed labeled graph. However, the expressive power of RDF is quite limited, for the following reasons. (i) RDF does not have any means to express the negation of a sentence; (ii) RDF can only express the conjunction of two or more atomic sentences, but it does not have any inbuilt capacities to express the other connectives (disjunction, implication, double implication); (iii) RDF has very limited facilities (called "blank nodes") to express quantified statements. Blank nodes only allow for a purely existential quantification of the conjunction of an arbitrary number of atomic sentences; (iv) all predicates of RDF have two places. This means that RDF can directly express only those statements that involve two-place relations. Statements that involve unary relations (John is tall) or relations with three or more places (John gives a rose to Mary) can only be indirectly expressed in RDF, by first translating such relations in convenient binary ones. As far as unary relations are concerned, RDF provides inbuilt facilities for their translation (RDF classes and the special property rdf:type). However, for n-ary relations with $n \geq 3$ there are no approved W3C standards (known as "Recommendations") for their expression in either RDF, RDFS, or OWL.

The severe expressive limits of RDF are partially overcome by its ontological extensions RDFS and OWL. However, even OWL 2 (the most recent version of OWL) does not reach the full expressive power of a first order language. An unfortunate consequence of this fact is that, at the moment, huge portions of our knowledge cannot be made available on the Semantic Web as linked data, not even in principle.

In this work, I am going to present the basic lines of FOOL (First Order Ontology Language), a new and surprisingly simple ontological extension of RDF that allows for the expression of *any* sentence of a first order language as a *data set*, that is, a collection of directed labeled graphs. The basic device of FOOL is the *parametric pattern*, an RDF graph of a special form, which provides for the expression of an arbitrary atomic sentence, that is to say, an atomic sentence whose predicate may have any number $n (n \geq 1)$ of places. The expression of molecular and quantified sentences, which may include all the standard connectives and quantifiers, is then obtained by combining parametric patterns and *named graphs*, a new feature of the latest release 1.1 of RDF.

Social cognition, social neuroscience and evolutionary social psychology: what's missing?

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Keywords: social cognition, socially engaged cognition, social neuroscience, evolutionary social psychology.

Since the 1980s the study of social cognition has been the mainstay of theoretical and experimental social psychology. Within this cognitive paradigm, social cognition is defined in terms of its social objects, such as other persons or selves, as opposed to non-social objects, such as tables, trees and tarantulas (Fiske & Taylor, 2008). This cognitive paradigm has also stimulated the development of two distinctive sub-disciplines, namely social neuroscience, concerned with the identification of the neurophysiological substrates for the processing of information and affect relating to social objects, namely other persons and social groups **(**Cacioppo, & Decety, 2011), and evolutionary social psychology, which aims to provide explanations of social cognition in terms of its reproductive advantage in ancestral environments (Simpson & Kenrick, 2009). While much important work has been developed in this tradition, I suggest in this paper that something important has been left out. This is the theoretically rich and fertile conception of *socially engaged* cognition embraced by early American social psychologists, in which social beliefs and attitudes were conceived as beliefs and attitudes oriented to the represented beliefs and attitudes of members of social groups (Dunlap, 1925; Katz & Schanck, 1938). On this conception, a Catholic's belief that abortion is wrong, for example, is socially engaged if it is held because and on condition that other Catholics are represented as holding that belief; if this representation provides her motive for holding that belief. In contrast, a Catholic's belief that abortion is wrong is individually engaged if it is held for reasons or causes independent of whether any other Catholic (or any member of any other social group) is represented as holding that belief: if, for example, it is held on the basis of reasoned argument or compelling evidence, or has been beaten into her as a child. It is suggested that this earlier conception of socially engaged cognition should be integrated with contemporary research on social cognition. However, this theoretical conception cannot simply be grafted onto the contemporary conception of social cognition as cognition directed towards other persons and social groups, for the two conceptions of social cognition are orthogonal. Although we have many socially engaged beliefs and attitudes about other persons and social groups, socially engaged beliefs and attitudes are not restricted to beliefs and attitudes directed towards other persons and social groups: we can have socially engaged beliefs and attitudes about non-social objects, such as the origin of species and the existence of N-rays. Conversely, one can have individually engaged beliefs and attitudes directed towards other persons and social groups on the basis of inductive experience or cognitive heuristics such as stereotyping. This means that the cognitive/affective and neural mechanisms that underlie cognition directed towards social objects cannot be assumed to be identical with whatever cognitive/affective and neural mechanisms underlie socially engaged cognition, and suggests the need and opportunity for an novel research program to explore the cognitive/affective mechanisms underlying socially engaged cognition, *whatever its objects*. It also seems doubtful if there can be such a thing as an evolutionary social psychology of socially engaged cognition, as opposed to an evolutionary social psychology of cognition directed towards other persons and social groups. For if any form of cognition is a product of a cognitive module that provided a reproductive advantage in an ancestral environment, then any explanation of cognition in terms of such an inherited program is an explanation of individually and not socially engaged cognition. Moreover, it is doubtful if socially engaged forms of cognition did provide humans with a reproductive advantage in ancestral environments, since there do not appear to be homologues of socially engaged cognition in the animal kingdom, even among the higher primates.

Dewey's structural realism: a viable option?

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Keywords: Structural Realism, Dewey, Reichenbach, Godfrey-Smith

The aim of this presentation is to analyze and investigate Dewey's pragmatist version of Structural Realism. By this label I mean to suggest that a comparison can be drawn between Dewey's ontology of scientific objects and contemporary positions in philosophy of science. Such interpretation is not completely original: Peter Godfrey-Smith has devoted some articles to the issue, arguing that Dewey put forth a heterodox form of structural realism grounded on his pragmatist conception of relations. Godfrey-Smith's reconstruction of Dewey's philosophy of science is authoritative, and has the merit of highlighting some reasons for the contemporary relevance of the pragmatist theory of scientific objects. However, I believe that Godfrey-Smith downplays some other aspects of originality of Dewey's position. My goal is to correct what I believe are the shortcomings of his analysis, and to present a different and – I hope – more nuanced and faithful account of Dewey's views.

The approach of my presentation is mainly historical and reconstructive: I will focus on the debate between Reichenbach and Dewey to shed some light on the fundamental tenets of the latter's realism. In his contribution to the Schilpp volume dedicated to Dewey, Reichenbach suggested that Dewey's identification of the scientific object with relations, instead of with some of existing non-relational things, entails the conclusion of the non-reality of scientific objects. Dewey replied that that was not the case. My thesis is that Dewey is right, and I will show why he was persuaded that he could easily escape Reichenbach's critical remarks. My argument is made of three steps.

First of all, I will argue that Dewey's instrumentalism is not at odds with a strong form of realism. Rather the contrary, Dewey is a *realist* for what relations are concerns. This is a direct consequence of his pragmatist conception of meaning, according to which the meaning of a concept is the sum of all the conceivable consequences that follow from acting according to it. He maintains that an object, insofar as it can be known, is a general pattern of relations. Accordingly, the unobservable objects posited by science *are* these relational structures.

Secondly, I will reconstruct Dewey's theory of relations. Dewey holds that *all* relations are real. In the case of scientific relations – that is, those relations that constitute scientific objects – these are produced when *existential* connections existing in the situation from which inquiry arises are noted, elaborated and transformed with the help of scientific language. I will argue that Dewey's theory of relations is radically different from standard accounts, being grounded on the idea of potentiality.

Finally, I will conclude that Dewey's pragmatist version of structural realism lies somehow in between the ontic and the epistemic form, thus providing an interesting new perspective on the contemporary debate. Dewey believes that relations are what can be known about entities existing independently of our theories. At the same time, however, he holds that relations and structures have a metaphysical priority over individuals. I will maintain that Dewey's structural realism is consistent on its own premises – contrary to what Godfrey-Smith seems to believe – and that it can be expanded into a general theory of scientific objectivity.

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Natural kinds: from twilight to night?

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Keywords: natural kinds, HPC, microessentialism.

Natural kinds are dear to metaphysicians and philosophers of language, who often assume a microessentialist conception on which natural kinds are united by inner essences.

Philosophers of science widely recognize micro-essentialism as inadequate for many kinds traditionally counted as natural – especially in biology (see e.g. Dupré (1981), LaPorte (2004)) but also for chemistry (e.g. Needham (2001)) . Many, however, embrace Boyd's HPC theory (Boyd 1991): the idea that natural kinds are property clusters united by underlying homeostatic mechanisms. Fewer embrace Hacking's thesis (2007): "there is no such thing as a natural kind".

I shall examine Hacking's thesis from a decade of hindsight.

First, I will consider Tahko's (2015) recent (partial) defence of natural kind essentialism in response to criticism of micro-essentialism by Needham (2011). I argue that it fails to adequately answer the complaints against micro-essentialism advanced by Needham and Hacking.

Second, I will consider recent refinements of Boyd's original HPC theory. These are often offered as responses to problems besetting the HPC theory, some concerning the indivuation and carving of mechanism tokens and types, respectively (Craver 2009, Buckner 2015), some concerning the idea that kindhood goes with hand in hand with homeostasis (Ereshefsky & Reydon 2015; Magnus 2011), and some concerning the very idea that kinds require unification by mechanisms (Slater 2015). I argue that responses to these problems – such as those offered by Magnus (2011) and Martínez (2015) – offer many valuable insights, but tend to undermine rather than underwrite the existence of a select class of kinds (the natural kinds).

In conclusion, I argue that by and large, Hacking's thesis stands.

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The theory of paraconsistent quasi-structures
A new structural approach

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Keywords: quasi-set theory, non-classical logic, structures, self-referential paradoxes, quantum physics

Abstract Paraconsistent quasi-set theory merges two different frameworks: quasi-set theory and paraconsistent logic. Quasi-set theory was developed to describe indistinguishable objects in quantum physics. The indistinguishable objects in quantum physics are objects, which have not any individuality. However, paraconsistent logic analyzes the contradictory potential realm of quantum superpositions. Here, paraconsistent quasi-set theory is to be extended to a *paraconsistent quasi-structure theory* Q_p. Quantum particles describe rather *structures* (quantum structures) than objects. Paraconsistent structures (e.g. quantum superpositions) contain undecidable contradictions, which cannot be solved within classical mathematics or classical Aristotelian logic. For this reason, the concept of identity has to be restricted and new principles have to be introduced: the Paraconsistent-Quasi-Relation Principle (PQRP) and the Paraconsistent-Foundation Axiom (PFA). This theory of paraconsistent quasi-structures (Q_p) is a non-classical logic and can deal with paraconsistent quasi-structures without to explode into triviality. It provides not only a logico-mathematical framework to deal with quantum objects, but also provides the formal structure to understand and to analyze self-referential paradoxes.

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Aristotle and Avicenna on the predication of 'existent'

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Keywords: Aristotle, Avicenna, existent, predication, univocal, equivocal, modulated univocal

In the *Categories*, Aristotle claims that there are two major kinds of predication. First is called *'synonymous'* (translated as univocal) which is used when a term predicated of several subjects and all shares the same meaning or definition. The second kind of predication is called *'homonymous'* (equivocal) which is used when a term predicated of several subjects and all has different meanings (1a1-13). Aristotle thinks that the two aforementioned types of predication do not help to predicate the term *'existent'* to the ten different modes of beings developed in the *Categories*. In other words, *'Socrates exists'* has neither the same nor a completely different meaning from *'Red exists'*. Aristotle knows that in order to constitute the science of being qua being, he should find a new mode of predication. Because in *Posterior Analytics* he claims that the subject matter of science should be said univocally to the things it investigates. But although the subject matter of Metaphysics is *'what being is'* (1028b3), the term *'existent'* cannot be predicated univocally. Aristotle develops such a solution: *'There are many senses in which a thing may be said to 'be', but they are related to one central point, one definite kind of thing, and are not homonymous. .....some things are said to be because they are substances, others because they are affections of substance, others because they are a process towards substance, or destructions or privations or qualities of substance…'* (1003a33-45). Accordingly, it seems the phrase *'.To one central point, one definite kind of thing'* refers to substances. Thus the term *'existent'* (being) has two senses: *paradigmatic* and *derived* sense. Primary substances are said to *'exist'* in the paradigmatic sense whereas accidents can be said to *'exist'* only in a derived sense since their existence is contingent on the existence of primary substances. By discovering this *pros hen predication* Aristotle is able to predicate all modes of beings with *'existent'* and thus makes them the things investigated under the science of being qua being. Avicenna, on the other hand, develops another way to solve the problem of predicating 'existent' to different modes of beings and hence to be able to constitute the science of being qua being. He calls the term *'existent'* as a modulated term (*isman musakkikan)* since its '*..meaning is the same when abstracted, yet not the same in every way but similar' (Maqulat, book 1, Ch.2).* Accordingly 'Sokrates exists' and 'Red exists' share the same meaning of 'existent' (hence univocal), but its mode of application to these instances differs in terms of priority and posteriority and degree of deservingness (hence modulated univocal). This idea derives from his famous quiddity-existence distinction. Therefore the predication of 'existent' is not about the quiddities of beings, but it is a non-constitutive concomitant (lazım gayr muqawwim), that is, a kind of inseparable accident of every quiddity. The aim of this study is to investigate how Aristotle and Avicenna predicate *'existent'* to the things and try to find out what are the motives behind their theories.

A dynamical systems approach to causation

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Keywords: state space, time evolution, descriptive states, projective states

Our approach aims at accounting for causal claims in terms of how the physical states of the underlying causal system evolve with time. Causal claims assert connections between two sets of physicals statestheir truth depends on whether the two sets in question are genuinely connected by time evolution such that physical states from one set evolve with time into the states of the other set. We demonstrate the virtues of our approach by showing how it is able to account for typical causes, causally relevant factors, ”the” cause, overdetermination, preemption, prevention, and omission.

Quantum particles don't have properties

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Keywords: Particles, properties, primitive stuff ontology, de Broglie–Bohm theory.

Mass and charge in classical mechanics are unproblematic. They are generally interpreted as intrinsic and local properties of particles (being intrinsic means that the mass and charge of one particle doesn't depend on the properties and behavior of the other particles; being local means that mass and charge are located in the particles, that is, where the particle is). So these classical properties are part of physical reality as they are an essential part particles.

My concern is whether this standard view is tenable for mass and charge in the de Broglie–Bohm pilot-wave theory. Brown (1996) put forward three arguments that mass and charge are properties of both particles and the wave-function. First, thought experiments with empty waves show that the wave-function carries mass and charge. Second, if mass and charge were properties only of the wave-function, the wave-function could not recognize the species a particle belongs to in order to correctly guide it—Brown dub this the *problem of recognition*. Third, since mass and charge appear in the guiding equation, they must be intrinsic properties of particles, too.

I show that these three steps can be challenged: that empty waves can be causally efficacious doesn't mean that the wave-function has mass and charge, the wave-function has no problem of recognition, and the guiding equation can be symmetrized so that there is no way to associate mass and charge with specific particles. My criticism leads to an ontology deprived of intrinsic properties; indeed, the only entities that exist are particles moving in space and time. I call such an ontology *primitive stuff ontology*. This is an elaboration of a *primitive ontology*, which is indifferent to the status of properties. The wave-function is then regarded as nomological, as a representation of the motion of particles. And as a nomological entity it cannot have physical properties like mass and charge.

In conclusion, the standard interpretation of mass and charge as intrinsic and local properties is no longer valid for the de Broglie–Bohm quantum theory. While Brown et al. argue that classical properties in addition have to be non-local properties of the wave-function, I show that that there are no intrinsic properties at all. Instead, mass and charge are best regarded as constants of nature in a primitive stuff ontology.

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Undecidability of the spectral gap. Philosophical issues and some ways around them

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Keywords: undecidability, physics, mathematics, problem-solving

New formal results from Cubitt–Perez-Garcia–Wolf (2015a and 2015b) on the spectral gap problem add a new chapter to the issue of undecidability in physics. These results address the question: is the Hamiltonian of a quantum many-body system gapped or gapless? where the notion of 'gap' is a way of expressing quantum phase, or phase transition.

The straight theoretical answer, according to Cubitt–Perez-Garcia–Wolf, is that it is undecidable. They prove that:

1. the spectral gap is algorithmically undecidable;
2. the spectral gap is axiomatically independent.

The core of their proof is a reduction to the halting problem of a Turing Machine, thus employing a representation theorem. A remarkable feature of these limitative results is the fact that they reveal *new* physics phenomena. Moreover, they offer an advancement on Richardson's (1968) and Da Costa–Doria's (1991) treatments of undecidability in physics: they do not simply give a general existential proof of undecidability in physics, but consider undecidability in the context of an interesting and particular physical system.

My paper sets out to examine some aspects of the proof of these theorems, their effect, if any, on a reductionist viewpoint, and discusses some implications like the fruitfulness of limitative theorems, the role of mathematical methods in physics and, in general, the way problems are approached in physics.

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The symmetry underneath the ontological argument

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Keywords: ontological argument, modal logic, symmetry

The ontological argument is the main subject of one of the longest and most complex philosophical vicissitudes. From its birth thanks to Anselm of Aosta first formulation to its most recent formalization by Kurt Gödel, it has seen a succession of detractors and supporters, as well as flaws first identified and later worked around or corrected through successive reformulations.

Without claiming to have solved the issue or stating definitively whether or not it proves the existence of God, here I would like to show a "metalogical" principle that all versions implicitly embrace, that I consider essential to obtain a working ontological argument. I believe that this is the principle implicit in the metaphysical outset of those who admit both the existence of God and its *apriori* provability.

The ontological argument is inherently modal, even in versions where its modality is less obvious, it feeds on concepts such as *necessary, possible, contingent*. Specifically, the underlying principle of all the ontological arguments I have examined (Anselm, Scotus, Descartes, Leibniz, Gödel) emerges from the need to use two different concepts of possibility.

In a nutshell: the required predicate of existence for the being “God” is that of an actual existence, as in the actuality/potentiality Aristotelian dichotomy, and consequently the concept of possibility originates from the *principium plenitudis* (*in aeternis idem esse et posse*); on the other side, to ensure the necessity and coherence of the being “God”, purely logical modalities are used, where “possible” assumes the meaning of noncontradictory.

While every ontological argument has its own peculiarities, their general backbone is as follows: a point is reached where it is stated that God is a necessary being, or that its essence necessarily implies existence, then that it is also possible; lastly, the possibility of a necessary being implies its actuality. Written into symbols, the scheme of this last step is:

$$\Diamond\Box G \rightarrow G$$

The similarity with the modal axiom B is evident, however, the distinction is that the being in the antecedent has a different ontological status from the one in the consequent: the first is a non-contradictory being, logically possible; while the second is an actual being, thus causally possible.

Kripkean semantics helped me interpreting this scheme as a symmetry. Symmetry between the actual world and a merely logically possible world accessible through special rules. This metalogical principle of symmetry is the aforementioned principle implied and required by the ontological argument.

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Towards a process ontological account of causal transitions in hierarchic complex systems

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Keywords: causality, systems biology, nonlinear relationship, process ontology, part-whole.

The main division between biochemistry and systems biology lies in what it is taken as a base unit to understand the system on hand: If the base units consisted of cells, tissues, organisms then it is within the scope of systems biology while the base units of biochemistry are chemical structures of the living systems. In this paper, I claim that to grasp the causal mechanisms that systems biology seek for, taking the base units as static individuals cannot provide a sufficient explanation. That is to say, the ontology that gives priority to properties would not constitute as a basis for accurate understanding of the complex systems. The very reason for that is, as I will discuss in the first section, property-based ontologies undermine the (nonlinear) relations and thus, gives an account of causality as the manifestation of a disposition. Note that, by property-based ontology I refer specifically to dispositionalism which proposes that the properties are real constituents of a system and the 'dispositions are intrinsic to the things that have them' (Ellis, 2007). Even though it is argued that relationships can be thought as dispositional properties (ibid 82) thereby a system can be thought as nested properties, I will defend the contrary in the second section: to grasp a living system, one must seek for the ontology based on processes. In this way, the ontological priority will be given to becoming over being (or say, static individual) then the feature of dynamism of the complex systems is supported (Stein, 2004). A complex system, in fact, that is dynamic and has many elements interacting nonlinearly in causal feedback loops that generating unstable states (Mainzer&Chua, 2012). Moreover, I argue that the relations are establish the structure of a system, thus parts of a system cannot be isolated from that system. The parts exist through that system- it is the system that makes them as what they are. However, the explanatory architecture of the property-based ontologies presupposes that, namely, extracted 'being's as bottoms and tops which establish relations according to their dispositions. In this sense, considering the part-whole relations I will claim that the terms of bottom-up and top-down (interlevel) causation are deceptive since they implicitly adopt the extracted parts within the wholes. But, if so, how can one demonstrate the interlevel causal relations in these hierarchic complex systems? In the recent literature (Craver&Betchel 2007; Gillett 2013) the notion of interlevel causation considered as problematic as well. These arguments, mostly, developed in order to be a maneuver that preserves causality against the objections that originated from the basic Humean assumption of 'causes and effects cannot be the same'. As a solution, it is suggested that to take part-whole relations as 'constitutive' relation rather than a causal relation and to leave causality within the borders of intralevel events. I found that approach is problematic since the causal relations taken in the sense of traditional-linear account of causation. This issue will be discussed in the last section with an emphasis on that why the traditional causal accounts cannot provide adequate demonstrations for the complex systems which hold nonlinear relations. I will defend that we are in need of a new explanatory architecture and, as a candidate; it is possible to build such an architecture based on process ontology that embraces the characteristics like nonlinear causality, autopoiesis, and emergence. As a conclusion, I assert that this kind of an explanatory architecture will suit best for biological systems.

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Two-sided sequent calculi for four-valued logics

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Keywords: sequent caluli, four-valued logics, first degree entailment.

On the basis of correspondence analysis for many-valued logics, we present a general method to generate cut-free sequent calculi for paraconsistent truth-functional four-valued logics that are close to *first-degree entailment* (*FDE*). A four-valued logic L_4 evaluates arguments consisting of formulas from a propositional language $\mathcal{L}$ built from a set $\mathcal{P} = \{p, p', \ldots\}$ of atomic formulas, using negation ($\neg$) and finitely many additional truth-functional operators of finite arity. In L_4, a valuation is a function v from the set $\mathcal{P}$ of atomic formulas to the set $\{\emptyset, \{0\}, \{1\}, \{0,1\}\}$ of truth-values 'none', 'false', 'true', and 'both'. We use the following shorthands: $\boldsymbol{n}$ abbreviates $\emptyset$, $\mathbf{0}$ abbreviates $\{0\}$, $\mathbf{1}$ abbreviates $\{1\}$, and $\boldsymbol{b}$ abbreviates $\{0,1\}$. A valuation v on $\mathcal{P}$ is extended recursively to a valuation on $\mathcal{L}$ by the truth-conditions for $\neg$ and the truth-conditions for the finitely many additional operators of finite arity. The truth-conditions for $\neg$, which is a paraconsistent four-valued negation, are as follows:

$$\begin{array}{lll} 0 \in v(\neg A) & \text{iff} & 1 \in v(A) \\ 1 \in v(\neg A) & \text{iff} & 0 \in v(A). \end{array}$$

An argument from a set Π of premises to a set Σ of conclusions is L_4*-valid* (notation: $\Pi \models_{L_4} \Sigma$) if and only if for every valuation v it holds that if $1 \in v(A)$ for all A in Π, then $1 \in v(B)$ for some B in Σ.

First, we show that for every truth-functional n-ary operator $\star$ every truth-table entry $f_\star(x_1, \ldots, x_n) = y$ can be characterized in terms of two sequent rules. For instance, the truth-table entry $f_\star(\boldsymbol{b}, \mathbf{1}) = \mathbf{0}$ for a binary operator $\star$ is characterized by the sequent rules $L^{\star+}_{\boldsymbol{b1 0}}$ and $R^{\star-}_{\boldsymbol{b1 0}}$:

$$\frac{\Gamma/\Delta, A \qquad \Gamma/\Delta, \neg A \qquad \Gamma/\Delta, B \qquad \Gamma, \neg B/\Delta}{\Gamma, \star(A,B)/\Delta}\, L^{\star+}_{\boldsymbol{b1 0}}$$

$$\frac{\Gamma/\Delta, A \qquad \Gamma/\Delta, \neg A \qquad \Gamma/\Delta, B \qquad \Gamma, \neg B/\Delta}{\Gamma/\Delta, \neg \star (A,B)}\, R^{\star-}_{\boldsymbol{b1 0}}$$

Consequently, every truth-functional n-ary operator can be characterized in terms of 2×4^n sequent rules. We use these characterizing sequent rules to generate cut-free sequent calculi and prove their completeness with respect to their particular semantics. Lastly, we show that the 2×4^n sequent rules that characterize an n-ary operator can be systematically reduced to at most four sequent rules.

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Logical concepts and structural properties

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Keywords: Structural properties, logical concepts, invariance, definability

Our starting point in this talk is the observation of a striking similarity between how philosophers of logic define logical concepts and how philosophers of mathematics define structural properties. To illustrate this observation, consider invariance based accounts. There is the so-called Tarski-Sher thesis, which states that a concept is logical if and only if its extension is invariant under arbitrary permutations of the domain of objects (Sher (1991); Tarski (1986)). Compare this to what we may call the Carnap thesis, which states that a property is structural if and only if its extension is invariant under taking isomorphic copies of objects (Carnap (2008)). In both cases, invariance of extensions under a class of bijective functions is what matters. Moreover, both logical concepts and structural properties have been tied to definability. In the case of logical concepts, McGee (1996) and Bonnay (2008) have obtained results that connect logicality under the Tarski-Sher thesis to definability in purely logical languages. Similarly, authors on mathematical structuralism, such as Shapiro (2008) have claimed that we can view structural properties as properties we can define from the basic properties and relations on structures. These similarities are not by accident. We claim that there is a close and systematic connection between logical concepts and structural properties. In this talk, we will explore this connection in detail and use it to shed new light on the connection between logicism and structuralism in the philosophy of mathematics. Based on a closer discussion of different invariance and definability criteria for both types of concepts, we first argue that logical concepts can be viewed as a kind of limit case of structural properties. The other way around, the claim is that structural properties are simply logical properties in the formal languages of mathematical structures. We discuss this claim in detail and show in what sense it vindicates a particularly structuralist account of logicism.

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Cognitive and societal justification of Neyman's "inductive behavior" conception of the objective of science

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Keywords: frequentism, statistics, policy, decision, belief control

Jerzy Neyman, a co-founder of frequentist paradigm in statistics, dismissed any type of philosophical school which maintained that scientific inference forms a basis for establishing what we should believe: "(…) the conviction of the possibility of a universal normative regulator of beliefs is common to the writers on inductive reasoning and may serve as a definition of this particular school of thought" (Neyman, 1957, 15). Neyman stated that "The beliefs of particular scientists are a very personal matter and it is useless to attempt to norm them by any dogmatic formula" (Neyman, 1957, 16). That's why he insisted that "(…) to accept a hypothesis H means only to decide to take action A rather than action B" (Neyman, 1950, 259).

Neyman justified frequentist statistics and his interpretation of it by referring to metamathematical considerations about the meaning and applicability of certain statistical concepts (Neyman, 1937, 340-345). But apart from mathematics, when we shift to a cognitive and societal perspective, the question remains: is it really pointless to use science as a belief regulator and is the principal role of science really to guide actions rather than beliefs? The aim of my paper is to support Neyman's views by providing non-metamathematical arguments for positive answers to both of these questions.

In reference to psychological (Nęcka et al., 2006, 563) and epistemological (Alston, 1988) findings along with contemporary scientific policy (ICSU, 2004), it can be argued that the postulate that scientific outcomes should be guiding beliefs seems to be unrealistic and unnecessary. Socio-economic utility appears to be the crucial goal of applied as well as basic research, which means that scientific inferences are expected to guide actions which should be practically advantageous to the society. Beliefs are hardly explicable (Skyrms 2000, 130), difficult to control, and regulating them is not necessary to control actions. Actions in turn are fully empirically tangible and far more tractable than beliefs. Scientific inferences can fruitfully serve as standards for making decisions and performing actions relative to specified evidence and risk preferences. Therefore Neyman's "inductive behavior" philosophy is supported not only by his arguments that could be classified as belonging to metamathematics, but it is also fairly well-grounded from the societal and cognitive perspectives.

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Relativity, nonlocality and the consequences

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Keywords: Nonlocality, Relativity, Quantum Mechanics, Retrocausality

Bell's theorem manifests a tension between quantum nonlocality and relativity by asserting that any realistic account of EPR-type correlations must admit nonlocal influences between distant events (Bell, 2004, Ch.2, 16). Reconciling nonlocality and relativity is one of the greatest challenges facing modern physics and may require a radical revision of our current understanding of space, time and causation (cf. Maudlin (2011)).

In this paper, I want to make precise where exactly the tension lies and explore the possible solutions that are currently on the table. Those are:

1. Modifying the structure of relativistic spacetime by introducing a preferred foliation. This can be done without violating formal Lorentz invariance (Dürr et. al., 2014).

2. Pursuing a relativistic generalization of stochastic collapse models, while giving up on a *causal* account of nonlocal correlations (Tumulka, 2006).

3. Admitting retrocausal influences (Costa de Beauregard, 1977; Reznik & Aharanov, 1995; Cramer, 1980; Price, 1996; Lazarovici, 2015). This could be done in a fully relativistic way by exploiting the (past and future) light-cone structure of relativistic spacetime.

I will discuss the vices and virtues of these approaches as well as their philosophical implications. I will conclude that a precise formulation of relativistic quantum mechanics will in any case have profound consequences for our understanding of space, time and causation.

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Dispositionality and symmetry structures

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Keywords: structures; symmetry: dispositional; categorical

A number of metaphysicians and philosophers of science have raised the issue of the modality of the fundamental structures of the world. Although the debate so far has been largely focused on the (alleged) inherent causal character of fundamental structures, one aspect of it has naturally taken its place as part of the dispositional/categorical debate. In this talk, I focus on the latter in the case of the fundamental symmetry structures. My main aim is to provide reasons which undermine the plausibility of a dispositionalist account of symmetry structures. To this end, I begin by arguing that symmetry structures should be construed as (holistic) features in order for the debate under consideration to make sense. Yet, construing symmetry structures as features presupposes that all symmetry structures could be interpreted ontologically/realistically; an arguable claim, especially in the case of gauge (local internal) symmetries. Granted the required ontological interpretation of symmetry structures, I then proceed to explore the various ontological options available to the dispositional structuralist in order to unearth difficulties related to their possible adoption. In particular, I briefly examine ontological interpretations of symmetry structures as dispositional properties of a) objects, b) fundamental properties, and c) laws. I also discuss a possible interpretation of symmetry structures as manifestations of the (putative) dispositional essence of the actual world. In all cases I argue that there are various problems a dispositional structuralist should address; some of them are pertinent only to the particular version of ontic structuralism one endorses (eliminative, moderate), while others affect all structuralistic stripes. Finally, based on the conviction that the definitional characteristic of dispositional features is that they provide by themselves a metaphysical explanation of various modal (causal) truths about the actual world, I draw on recent work about the relationship between laws and symmetries in order to show that contemporary physics suggests that symmetry structures cannot plausibly be construed as having this explanatory role. In particular. I argue that laws cannot be 'derived' by symmetries alone and that fact clearly undermines dispositional structuralists' claim that symmetries can by themselves explain the fundamental modal truths about the actual world that modern physics reveal. The upshot is that the question 'Are the fundamental symmetry structures dispositional?', if it does make sense, should be answered in the negative.

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Sober ontic structural realism

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Keywords: ontic structural realism, category theory, Yoneda lemma

(Radical) ontic structural realism ((R)OSR) is the claim that "there can be relations without relata": there are no 'things' and only 'structure' exists. As it stands, ROSR attracted the attention of physicists and philosophers tackling foundations of Quantum Mechanics [Eva, 2016], and of some category theorists, because of the affinity between ROSR and the claim (see [Mac Lane, 1998, §I.1]) that it is possible to define a *category* via its arrows only (*objects*, here, are relata, and *morphisms* are relations between relata).

The present work proposes the following analysis of ROSR:

- We outline some elementary concepts in category theory (CT) that clarify better the relationship between CT and OSR, also correcting a number of mathematical inaccuracies in the current literature. Such misunderstandings fetter the reader from seeing that CT relies on a less extreme (and more easily defended) claim than ROSR;
- The mathematical principle underlying such more moderate position is (almost) a triviality rooted in *Yoneda lemma*, a cornerstone of elementary category theory, and yet it is perceived as a rather elusive idea. Starting from this principle we propose what we call *sober* ontic structural realism (SOSR);
- SOSR, assumed in virtue of the Yoneda lemma
  - clarifies the precise rôle of 'points' in category theory, offering a 'weak extensionality' principle as a substitute of the set-theoretical one;
  - clarifies that categorical (and higher-categorical) structures *are absolutely not only determined by their 'points'* $1 \to \mathbb{C}$ (this is a terrible misunderstanding propagated by the current literature), and even in situations where they are, this choice is only the reflection of a more general principle.

  Examples of this behaviour, and 'soberifications' of some of the ideas proposed in parts of the literature are proposed.

As a purely philosophical position, SOSR poses the following question:

> Are there *systematic, non-structural* and yet interesting features of a mathematical object?

To some extent, the existence of such a property is undetectable until using category theory to classify 'relata', and therewith should be ignored. SOSR reinforces the belief that equivalent categories 'are one and the same object', in the same way sets having different names for the same number of elements have to be regarded as one and the same structure.

This seemingly rigid point of view preserves the radical nature of ROSR (in that we deliberately concentrate on structural properties 'building' our foundations on some form of naïve univalence, according to which 'isomorphism behaves like equality'), but avoids the awkward and somehow absurd consequences of assuming that 'there are no objects'.

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Theory-relative constitutive principles and evolutionary biology: relaxing a conceptual tool

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Keywords: constitutive principles, evolutionary theory, pluralism, scientific theories

My paper engages with recent trends in philosophy of science defending the view that there are some parts of a scientific theory - namely, the constitutive principles - that have a different epistemic status from the other empirical statements of the theory (Friedman 2001, Stump 2015). I argue that some *theory-relative* principles in evolutionary biology perform a 'constitutive' role similar to the one described by most of the contemporary literature on constitutive principles in physics, in that they are *domain-specific preconditions* for the formulation and testing of the empirical results of a theory. However, there are important differences, given that the interaction of the constitutive character of these elements with other features, such as generative entrenchment, scope, and generality, as discussed by Wimsatt (1987), makes them more suitable to a *pluralistic* and *context-sensitive* characterisation than offered by standard treatments like Friedman's (2001).

Firstly, I analyse how the constitutive role of what Lewontin (1970) identified as the core tenets underlying the principle of natural selection (variation, heritability, differential fitness) can be understood in the light of various attempts to *formalise* these principles (Thompson 2007, Griesemer 2013, Barberousse and Samadi 2015). Secondly, I examine the way they have been used to *internalise* elements that were originally in the background of the process of Darwinian evolution, by means of what have been called 'strategies of endogenization' (Okasha 2016). I argue that the constitutive function played by these principles can be conceptualised in terms of a *trade-off* between formalisation and endogenization, where these quasi-axiomatic principles have a guiding role both in model-mediated scientific theorising (Diez and Lorenzano 2015) and in the historical development of the evolutionary framework of inquiry, in that they provide fundamental elements of its conceptual machinery.

Finally, I briefly consider two further examples that allow a comparison-and-contrast with the case of natural selection: The role of the Hardy-Weinberg principle in the development of population genetics, and Carroll's principles for a genetic theory of morphological evolution. I argue that assessing these case studies in terms of my suggested framework leads to a 'relaxed' view on constitutive principles, which replaces a dichotomous distinction between what is or not constitutive *tout court* with a graded picture to be understood in terms of paradigmatic, minimal, and marginal cases.

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Interpreting quantum mechanics: a tentative modal account

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Keywords: quantum mechanics, interpretation, empirical equivalence, possible world

Take *T* to be an arbitrary scientific theory. For *T* it is useful to distinguish among three levels: [1] formal level, [2] empirical level and [3] theoretical-metaphysical level. Level [1] is the level of the *formal content* of *T*. Level [2] is the level of the *empirical content* of *T*. Level [3] is the level of the so-called *interpretation* of *T*. We may take level [3] as to be constituted by a certain set of theoretical-metaphysical assumptions answering questions along the following lines: What counts as a possible explanation of the observable outcomes we deal with when we apply *T*? What causal mechanisms would give rise to the set of observable phenomena belonging to level [2] of *T*? The theoretical-metaphysical assumptions in question are not to be conceived as factive, but rather as hypothetical and modal, or so I will argue. They are not meant to describe the world *as it is,* but rather the way the world *might be,* given the empirical evidence we have at hand. They don't provide us with knowledge of a causal mechanism, but rather of a possible causal mechanism – in the sense that they tell us how a certain given set of observable phenomena *might arise* or *might have arisen*. What these assumptions refer to, then, is what I will call a *possible world*, by which I mean a world that would give rise to the set of phenomena in question, if it happened to be actual.

From these general assumptions, I will propose a reconstruction of the «interpretation-problem» in quantum mechanics (QM) as a situation in which we have to deal with a certain *possibility space*. I take the possibility space associated with QM as to be made of a set of possible worlds (i) that are compatible with the formalism of QM and incompatible to one another, (ii) that are similarly explanatory and epistemic valuable and (iii) that are empirically equivalent (i.e. that would give rise, if actual, to the same set of observable phenomena). Now, given this picture, how are we to make QM intelligible? And how are we to find out which possible world, or which part of which possible world associated with QM corresponds to the actual world? The following questions, I believe, might and *should* help us in dealing with these problems: are there common traits among the possible worlds in question? *Must* there be any common traits among them, in order for them to be part of the possibility space of QM and in order for them to save exactly the same set of observable phenomena? Are these possible worlds related to one another in some way and, if yes, how? Are they related in a way that enables us to recognize certain symmetries across the possibility space they jointly inhabit? Are there any constraints holding on the possibility space and, if yes, which are they?

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The invention of the counterfactuality in the Stone Age of the Scientific demonstration

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Keywords: Eleatics, counterfactual deduction, prehistory of logic.

Two different deductive styles are carried out by Parmenides and Melissus, on the basis of their different solutions to the issues of physis and of what is. Although they did not establish the formal set of logical rules, for sure they somehow contributed to their development. On one hand, Parmenides' poem flows in an interesting sequence of passages that can be sorted out as contents foreword, methodological premises, *krisis*, conclusions and corollaries. On the other hand, Melissus wants to limit the logic to ontology and he organizes a long counterfactual deduction. On one hand Parmenides employs modal reasoning to render a perspective on natural phenomena, on the other hand Melissus rejects it and he organizes a complete scheme of counterfactual arguments in order to disappear any philosophy of nature and leave room just for philosophy of *being*, that is ontology.

In Parmenides' poem, the most critical moment of the Goddess' reasoning is when a transition from *is* to *ought* is produced (fr. 8.9-11) and the modality subverts categoricity. Two interpretative principles help to understand the logical structure of the Goddess' discourse: (1) the force of argument is its deductive form; (2) the principle of the excluded middle has got a modal counterpart, perfectly *equivalent* to the principle's categorical version. According to Wedin (2014), the latter principle, evidently wrong because '*x* is' is not equivalent to '*x* ought to be', is the fallacy that weakens the rejection of the second *logos*, the path of opinion. So, he appeals an alternative version of principle 2, (2'): The principle of the excluded middle has got a modal extension, perfectly *compatible* with the principle's canonical version. However, this extended version does not avoid further problems (e.g., the semantic problem: why is not allowed to think *what is not*, but, just when it is declared, it is thought and discussed?). I propose a third principle, (3): To strengthen the principle of the excluded middle, counterfactual reasoning is needed. Counterfactual demonstration is the formal structure within which the unjustified passage is mediated. Nevertheless, we are only allowed to talk of (proto-)counterfactual in Parmenides (Mansfeld, 2016).

Melissus is more radical. His *Peri physeos he peri tou ontos* is the first written text about ontology and for the very first time *being* becomes something to think about. Loenen (1951) already investigated the demonstrative core of Melissus' fragments and, more recently, Palmer (2004) trod an alike path again, expounding the ontological contents from Melissus' deductive structure. In Melissus' fragments, nature disappears and being remains as the only thinkable object so that only logic remains to give a help to think. Nevertheless, logic happens to be strongly limited by ontology. Counterfactual demonstration is way more extensive in Melissus, through that all the features of being are treated and where logic is just an instrument for ontology.

A tendency to demonstrate through deduction has been acquired since the very beginning of ancient philosophy. Despite their differences, Parmenides and Melissus deserve a place in a Stone Age of scientific demonstration and their common purpose is to establish what is by the force of argumentations. However, they were not doing logic: they were trying to reason logically and while doing it, something like a "deductive method" starts to take shape, involving constantly counterfactual reasoning. This discover will open doors for demonstrative science.

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Patterns of complex chains of hearsay: an epistemological framework

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Keywords: hearsay, testimony, epistemology, trust, reputation.

Indirect testimony or hearsay is a communication process whereby a sender conveys a given proposition *p* to a recipient that has not been produced or designed directly by the sender, but rather obtained from another person identifiable as the source or origin of *p*. The elucidated scheme represents the most simple pattern of what is referred to as a chain of testimonial transmission (Coady 1992, p. 211) which may be complicated at will providing for both the presence of an array of sources releasing *p*, as well as the presence of several intermediate nodes existing between source and recipient. Besides, much of the information obtained by third parties via different means or media such as newspapers, books, television, radio, Internet etc., may be rightly ascribed to hearsay or indirect testimonial (Coady 1992, pp. 50-53). The concept of *hearsay*, still from an epistemological angle, may also be associated, *inter alia*, to terms such as 'rumours', gossip, 'grapevine', 'scuttlebutt', 'chatter', 'urban myth', 'small talk', etc...(Bertolotti and Magnani 2014; Coady 2006, 2012; Gelfert 2013, 2014). Such terms may often overlap in regard to the phenomena they describe (Gelfert 2013, p. 6) and therefore share common areas of analysis. Nevertheless, it is all important to observe that each phenomenon characterised by the aforementioned concepts provides for the manifestation of an informal communication process (Gelfert 2013, p. 6) that, at a logical level, is in any case ascribable to a chain of indirect testimonial transmission. In line with Martini (2016), this work takes into account the diverse typologies of complex chains of indirect testimonial transmission within an anti-reductionist approach, thanks to the investigation into three epistemic properties that are very much interrelated, namely *trust*, *reputation* (Burge 1993; Faulkner 2011; Fricker 2006; Hardwig 1985, 1991; Origgi 2004, 2012) and *coherence* (Blanshard 1939; Bradley 1914; Bonjour 1985, 1999; Harman 1973; Lehrer 1986, 1994; Olsson 2014).

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Formality of logic and Frege's *Begriffsschrift*

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Keywords: Logic; Formality, Frege, Begriffsschrift

A number of scholars (e.g. van Heijenoort 1967, Ricketts 1986, Goldfarb 2001, Korte 2010) have argued that Frege's conception of logic is at odds with the contemporary one, particularly with regard to the question of what the subject-matter of logic is. According to the contemporary conception – dubbed 'schematic' – the subject-matter of logic is the logical form of sentences and arguments. According to Frege's conception – dubbed 'universalist' – the subject matter of logic is reality's most general features. The two conceptions have different stances on the question whether logic is formal. On the schematic conception logic is formal because it is devoid of content, and only attends to the form of sentences, arguments, and their logical relations. On the universalist conception, by contrast, logic is not formal, because it has a maximally general content, and is thus the most general science.

My aim in this paper is to cast doubts on this interpretation. I show that there is evidence in *Begriffsschrift* (and related publications) that suggests that Frege endorsed the idea that logic is formal. On Frege's view, while particular sciences use concepts with a particular content, the task of logic (and thus of Frege's own Begriffsschrift) is to provide the logical cement or formal scaffolding that ties up together the propositions used in particular sciences, and to provide a reliable method for testing the validity of inferences within each of the particular sciences. Frege explains the above distinction by using the formal/material opposition. In every high developed language, says Frege, "we may distinguish the formal part […] from the material part proper." (Frege 1979, 13). These two parts correspond to the two different symbols Frege introduces in *Begriffsschrift*, namely "those which have a completely fixed sense, and those which one can take to signify various things" (Frege 1972, § 1). The latter symbols – being *schematic* – could be imbued with the contents pertaining to particular sciences (for instance, could be used to express mathematical concepts), the former, on the other hand, express logical relations (conditional, negation, etc.). The two kinds of symbols can combined in order "to form a single formula language"; and in Frege's *Begriffsschrift*, "the existing symbols [of mathematics] correspond to the word-stems of [ordinary] language; while the symbols I add to them are comparable to the suffixes and [deductive] formwords {Formwörter} that logically interrelate the contents embedded in the stems" (Frege 1972, 93).

I will also argue that this interpretation has two further virtues: firstly, it makes good sense of the idea – expressed in the Preface to *Begriffsschrift* – that the aim of Frege's concept-script is that of "adding a new domain to [the particular sciences], indeed the one situated in the middle adjoining all others" (Frege 1972: 105). Secondly, it connects nicely with Frege's oft-repeated view that logic has a special kind of normativity, in its being normative "for all thinking, whatever its subject matter" (Frege 1979, 128).

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Econophysics as a theory of financial markets: an instance of reductionism in the social sciences.

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Keywords: econophysics; financial markets; reductionism; scientific explanation.

I propose an analysis of Econophysics as an instance of reductionism in the social sciences and I claim that it exhibits some problematic features, especially when considering the plausibility and the explanatory usefulness of the reduction of market agents to physical entities.

Econophysics is a theory of financial markets whose underlying assumption involves an analogy between financial systems and physical systems. It replaces concepts and tools used by the orthodox theory of markets with concepts and tools developed in physics. Whereas the orthodox theory borrows its fundamental concepts from neoclassical economics and uses tools developed by probabilistic theory which suits the neoclassical assumptions; econophysics borrows its models from statistical mechanics, a branch of theoretical physics that studies the average behaviour of systems whose state is uncertain and therefore seems appropriate to describe the dynamics of financial markets. In this perspective, then, all the features described in the orthodox financial theory are translated in the language and concepts of physics: financial markets are described in terms of disordered, out- of- equilibrium complex systems composed of a huge number of interacting parts; agents acting in the markets are accordingly described in terms of a particular kind of particles and their reciprocal interactions in terms of cooperative effects. Finally, market crashes are studied in terms of critical phenomena.

Following Nagel (Nagel, 1961), I argue that econophysics represents a clear instance of reductionism. In particular, it shows three reductionist characteristics:

1) The two domains are ordered hierarchically: economics, which is more complex, with a larger quantity of laws with a restricted scope, can be reduced to physics, which exhibits few, all-embracing laws, powerful enough to describe phenomena from the economic domain.

2) There is a special "bridge principle" between the two systems: since both are composed of enormous number of interacting parts, scaling theory is a working concept for both and may therefore function as a bridge.

3) In its unifying activity, econophysics provides a simple, elegant explanation of financial phenomena.

On the other hand, any reductionist enterprise exhibits critical theoretical flaws of different kinds: they can be of a methodological order, as argued by (Grosholz, 2007), of an epistemological (Cartwright, 1999) or even of an ontological one (Dupré, 1995). Following Dupré's work, I focus on the ontological problems raised by econophysics: I argue that the reduction of market agents from heterogeneous, complex entities to homogeneous, simple ones (particles) overlooks important problems such as the different strategies of decision- making and the different strategies for appraising and evaluating risk. These features are still highly debated in financial theory and there is no univocal description for them; nonetheless they are essential for a thorough, even though formally less elegant explanation of financial markets' dynamics and, more specifically, market crashes' dynamics.

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Mathematical generosity and ontological parsimony

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Keywords: Inference to the best explanation, Enhanced indispensability argument, Mathematical explanation, Ontological parsimony, Platonism.

Few authors have addressed the question of how Inference to the Best Explanation (henceforth IBE) may act at the level of mathematics and unobservables physical posits in scientific explanations (e.g. Pincock 2012 and Hunt 2016). Among these, Alan Baker has recently analyzed the topic from a novel perspective, thus giving a new twist to the analysis of mathematics in scientific explanations (Baker, 2016). More precisely, Baker has linked considerations of ontological parsimony and explanatory power with aspects that concern the use of a more (or less) general mathematical apparatus in a particular class of explanations in science, namely optimization explanations. He has pointed out how ontological parsimony is an indicator of explanatory power, and a stronger (i.e. more general) mathematical apparatus sometimes reduces concrete commitments. In tracing this linkage between ontological parsimony, explanatoriness and the use of mathematics, Baker not only has advanced a novel (though partial) account of how mathematical entities directly contribute to scientific explanations. With his study he has also provided a story of how mathematical and physical posits interact in a particular class of explanations, and this has a direct consequence on how IBE may work at the level of unobservables physical *and* mathematical objects in the very same scientific explanation.

In this paper I take Baker's analysis as starting point to consider a broader analysis of the connection between ontological parsimony, explanatoriness and the use of mathematics in science. After a short summary of Baker's main claim, I discuss his position and show that his analysis is hard to be used as a lever to support the platonist stance in the enhanced indispensability argument. Next, I depart from Baker's analysis and I offer some more general considerations that show how the use of a more general mathematical apparatus in science may lead to an explanation which has more explanatory power but the same ontological commitment. To illustrate my point, I briefly discuss one example of optimization explanation taken from physics in which the use of a more general mathematical framework leads to a better explanation. In this case, I claim, the stronger mathematical resources permit to disclose some aspects of the explanandum that were not know before but that are linked to a broader web of knowledge that is relative to that particular scientific phenomenon. In this process of mathematization, mathematics is explanatory because it shows that and how the 'more' connections hold, thus securing our web of knowledge about the scientific fact itself.

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Sensation in Hermann von Helmholtz's physiology between mechanics and the a priori

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Keywords: history and philosophy of science, a priori, perception, sensation, H. von Helmholtz

19-century organic physics consisted of establishing scientific physiology by introducing mechanical concepts such as matter and force and using physical methods. In this framework Hermann von Helmholtz succeeded, for instance, in measuring the conduction velocity of the nerve impulse and proved the temporal dimension of sensation, which challenged the common view of sensation as something immediate. Presuppositions, methods, and achievements of organic physics have been widely discussed in historical literature and STS (e.g. Dierig; Finkelstein; Heidelberger; Holmes; Hörz; Leiber; Kremer; Olesko; Rotschuh). Argumentative strategies and explanatory models have been reconstructed (De Kock; Lenoir; Mann; Turner). The impact of physiological discoveries upon philosophical, psychological, and aesthetical theories of perception has been analysed (Bailhache; Hatfield; Hui; Steege; Vogel).

The aim of this paper is to use 19th-century physiological literature and secondary bibliography to discuss the relationships between metaphysical conceptions, epistemological issues, and physical methods in the theories of perception developed by Helmholtz. The result is an explanation of the meaning of the a priori in Helmholtz's physiology and philosophy of perception and the assessment of its epistemological relevance. In order to investigate the processes of objectification in Helmholtz's theories of perception, descriptive methods, mechanical models, and theoretical results of his psychoacoustic research are compared. This comparison is aimed to identify the complex nature of sensation in Helmholtz's accounts and prove the extent to which, using Cassirer's phrase (1944), "perception expands the particular datum," involves the integration of sensation into a "total experience," and is "an objective factor in knowledge." Helmholtz shared the conviction that the domain of sound stays in a close connection with "pure sensation" and can be made intelligible through a "scientific foundation." This meant for Helmholtz primarily to adopt mechanical models of explanation (Lenoir, 2006), which was his strategy to account for the complexity of perception (Steege, 2013), and prove objective processes in the experience of sound.

The paper has four sections. After presenting the main methodological changes involved in organic physics, the focus is on the notion of "specific nerve energy" which was central in the making of sense physiology and the explanation of sensory qualities. The third section compares Helmholtz's references to Kant, Müller, and the a priori in his physiological works and popular lectures. I argue that whereas Helmholtz's mechanical modelling helps him identify objective structures in perception, Helmholtz's references to the (Kantian) a priori coincide with explanatory deficits from a physiological perspective. The final section discusses some epistemological implications of the problematic status of the a priori in Helmholtz's physiology. Whereas late 19th-century theories of perception aimed to achieve objectivity this kind of investigation only achieved rudimentary levels of objectivity and psycho-physiological research was not able to devise a full explanation of sensory processes. Helmholtz tried to solve this difficulty by resorting to the a priori. I show why this solution was not coherent and raise the question of whether the impossibility of a full physiological explanation of sensory processes was merely a historical matter or represents a limit of science.

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A KRIPKE-STYLE MODAL SEMANTICS FOR A REFINED VERSION OF DE FINETTI'S THEORY OF TRI-EVENTS

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Keywords: Three-valued logic partial logic conditionals counterfactuals probability logic

Partial logic has been recently used to elucidate de Finetti's notion of event with respect to Quantum Mechanics and other contexts (Flaminio, Godo, Hosni, 2014). This paper, while resorting to partial logic, takes another direction.

The theory of tri-events goes back to de Finetti (1933,1935). De Finetti's aim was to find a logical theory for conditional probability that plays the same role that sentence logic plays with absolute probability. For de Finetti, conditional probability must be viewed as the probability of a conditional event, called by him tri-event. A tri-event is a conditional sentence, which may be true, false, or 'null' (that is devoid of truth value). This allowed de Finetti to represent conditionals sentences by sentences of the form (C | A), where the symbol '|' is a three-valued truth-function.

It is easy to show that, in de Finetti's Logic, a sentence of the form (φ|φ) is not in general a tautology. This shows that de Finetti's Logic is at odds with almost all conditional logics, including Adams' *p*-entailment. This last Logic aims at satisfying the equation Pr(if A then B) = Pr(B | A), which is implicit in de Finetti's Logic. Moreover, while de Finetti did not define any logical consequence relation in terms of his truth-tables, a general result, due to McGee (1981), shows that no such a relation in terms of any standard many valued logic may fit Adams' *p*-entailment. The fact that (φ|φ) is not valid, shows also that de Finetti's Logic does not fit conditional probability in the way that Boolean logic fits absolute probability.

In my previous papers (2009 and 2011), to deal with the difficulties of the original de Finetti' theory, I introduced a modification of the truth-table algorithm. This algorithm provides a new semantics for tri-events. According to this semantics, every sentence that can be true but cannot be false is considered as valid (dually every sentence that can be false but cannot be true is considered as inconsistent). Due to the way in which the semantics is defined, this theory is called the *Theory of the Hypervaluated Tri-Events* (THT). THT is in accordance with probability theory, inasmuch two tri-events have the same probability value for every probability function if and only if they have the same truth-conditions according to THT.

The purpose of this paper is (a) to reformulate THT adopting a Kripke-style semantics, which is more general than the previous theory inasmuch it allows atomic sentences take the null truth-value, (b) to compare the new theory to its rival theories with respect to compound of conditionals and counterfactuals, and (c) to give details about a new epistemic theory of both indicative and counterfactual conditionals.

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Wittgenstein's influence on Turing's Test

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Keywords: Wittgenstein, Turing, Turing Test, social concept of intelligence, language.

There are many different interpretations of the Turing Test (see for example Moor 2006, Epstein, Robert, Beber 2009), but my aim here is to understand what Turing wanted to do by adopting the game as a possible discrimination between machines which would give themselves away and machines which could deceive a jury made of average non expert judges. This social attitude of Turing's vision about intelligence could be supported using the textual evidence of the first time in which the idea was discussed: at the end of the 1948 paper about Intelligent machinery (Turing 1948). In this work Turing embraced the position according to which what is considered an intelligent behaviour is something that is linked both to the characteristics of the individual under evaluation and to the observer's perspective (Turing 1948/2004, 431).

According to Turing the relevance of an intelligent task was related to the social and technical capabilities of the observer of attributing the intelligent behaviour to various devices or human beings, position which is very similar to Wittgenstein's approach to language, in order to explain the process of understanding a sentence within a human conversation. This is the reason why the jury that is supposed to evaluate the success at the imitation game was mandatory made of non-expert judges.

The social capabilities that the machine needed to show was rather deceptive as well as the imitations of human behaviours were. If it is possible to interpret intelligence as a social characteristics attributed to an agent, no matter whether it was a human being or a machine, Turing was convinced that in some reasonably short period of time: “the use of the words and general educated opinion will have altered so much that one will be able to speak of machines thinking without expecting to be contradicted” (Turing 1948, 449).

During the two terms in 1939 which Turing spent in Cambridge, back from Princeton, he followed Wittgenstein's lectures on the foundation of mathematics (Wittgenstein 1939/1976). Turing started the 1950 paper with the well-known question about “can machine think?”. We will be surprised to see that this is exactly the same question posed by Wittgenstein in the middle of the Blue Book, though in a different scenario: if it was clear that an amoeba certainly did not speak, or write or discuss, 'is it possible for a machine to think?' (Wittgenstein 1965, 47). By interpreting the Turing Test as a consequence of Wittgenstein's approach to language, considering intelligence as a social concept, the idea seems to start building a fake mechanism of deception within the mechanical device, which resulted in the end with a behaviour that failed to be forecast by the programmer herself. The other crucial element for intelligence detection in machine’s behaviour was the unpredictability of the mechanical output, which was due to the integration of some sorts of random, or pseudo-random routines.

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Problems with statistics: How do we choose the relevant population in our studies?

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Keywords: Statistics - Population - Mortality - Public Health

Statistics has been defined several times as the science of uncertainty since its foundations lay in the mathematics of probability theory. This is certainly true, but it is also the science that concerns *data*. Since the analysis of almost every phenomenon, be it a physical, financial, social, or political, is based on some empirical data, Statistics is ubiquitous. Thus, it becomes a crucial demand to have a clear methodology to extract and to interpret these information. Approaching Statistics, one learns that to formulate a study of a given character of a particular population, the appropriate universe of analysis has to be defined. Nonetheless, often this methodological rule is not respected: many analyses suffer from an ill-defined methodology which poses serious doubts on their reliability. Often, one can encounter statistical analyses performed with an incorrect or unfair population. It is worrisome that in epidemiology, especially when applied to issues concerned with public health, a notable number of studies faces these problems. I will discuss the case study offered by Pirastu (1997). They analyzed the mortality of workers of a petrochemical plant, and concluded that the subjects exposed to the action of VC/PVC had a higher rate of mortality for liver cancer w.r.t. the population of the Region where the plant is located (Veneto). Considering the reanalysis of Gennaro (2008), more precise information are available regarding (i) the rate of mortality due to liver cancers, and (ii) concerning a broader class of diseases which affected the workers exposed to these toxic agents. This reanalysis found an elevated death risk for all causes among the whole group of exposed workers, and *specific death risks among specific subgroups*. On the contrary, the preceding analysis revealed a statistically significant *reduced* mortality from all deaths combined, and a not statistically significant reduction of mortality from all tumors among the whole cohort. It is crucial to stress that the latter studies investigated the entire cohort, and used the *general regional population as a reference*, this analysis may hidden some health risks. Re-analyzing the population of the workers, dividing it into different classes of exposed and non-exposed subjects, it is possible to see a specific set of diseases for each subclass, a result which was not mentioned before. My aim is to present in detail this methodology and try to deduce general morals concerning the selection of the relevant population in statistical studies. This case study shows that to individuate the relevant population is less obvious than what it seems: one may encounter selection biases, but also, especially in studies with consequences for the public health, there seems to be business biases (Gennaro and Tomatis coined this term) when conflict of interests are evident. To evaluate whether or not errors in the procedure of the population's selection is due to business bias is, unfortunately, far from obvious and possible work for philosophers of science.

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The return of a demarcation problem

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Keywords: philosophy of science, history of philosophy of science, demarcation problem, Karl Popper, Rudolf Carnap.

Recent papers by Sebastian Lutz (preprint) and Hans Halverson (2012) have done much to spur a reevaluation of the problem of demarcation. While Lutz focuses his attention primarily on Rudolf Carnap's demarcation criteria and Halvorson focuses on rehabilitating syntactic approaches to interpreting scientific theories, my interest is directed elsewhere: formalising and extending Sir Karl Popper's early two demarcation criteria first articulated in *Logik der Forschung* (Popper, 1934/5).

I first argue Popper's two criteria are superior to A.J. Ayer and Carnap's respective criteria. For example, since Alonzo Church's review of Ayer's *Language, Truth and Logic* (Church, 1949), there has been considerable space dedicated in the philosophical literature to the tacking problems of irrelevant conjunction and disjunction. However, I show these problems do not target Popper's criteria: Popper's criterion of empirical predictability correctly identifies which statements within a theoretical system are irrelevant.

I then address one edge case: isolated existential statements of the form 'There exists an X that is Y'. I provide grounds independent of Popper's criteria to conclude that isolated existential statements are empirically non-significant. Consequently, one diagnosis for the failure of Carnap and Ayer's criteria is that tacking problems are a symptom of an underlying problem, *viz.* their criteria improperly categorise isolated existential statements as empirically predictive.

After formalising Popper's criteria, I propose an amendment motivated by a neglected paper by John Wisdom (1963): as traditionally formulated, Popper's criteria are about statements that are nomologically predictive; however, as I show, it is more appropriate to formulate these criteria in terms of physical possibility indexed to the present state of technological and imaginative abilities of an epistemic community.

I conclude the taxonomy of sentences is different than how it is usually understood in the literature on demarcation: there are in fact three different classifications for sentences, rather than two: a statement *S* may be (1) empirically significant (*S* is predictive), (2) empirically non-significant (*S* cannot be predictive) or (3) indeterminate (*S* is possibly predictive). I then provide historical examples of statements transitioning from the third category to the first.

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The variety of evidence thesis and the reproducibility problem: Statistics meets formal epistemology

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Keywords: Bayesian epistemology, replication, reliability, foundations of statistics, evidence in medicine

In this paper I analyse the two main research strategies advocated by opposing schools in medical methodology ("evidence elitism" and "methodological pluralism") and underscore their epistemological underpinnings, with a particular focus on the role of reliability and varied evidence in the two camps.

Since the latter strategy can be made more general by appealing to the Variety of Evidence Thesis, I analyse this thesis and its diverse versions, by delving in particular on the version presented by Bovens and Hartmann (2003), where the interaction of reliability and replication has an essential role in defining the epistemic value of varied evidence vs. replication. I then present Claveau's variation of this model (2013), which models unreliability as systematic error (bias), and go on to propose a model (developed in Landes and Osimani, forthcoming), where a distinction is made between random and systematic error.

Our model delivers results that contrast with both Bovens and Hartmann (2003), and Claveau (2013): When evidence is highly biased relatively speaking (bias much larger than random error), then confirmation is greater for varied evidence. This is in conflict with Bovens and Hartmann results where the VET fails for unreliable evidence (in their sense of unreliability). Furthermore, when evidence is only weakly biased, then the model favors replication; and for low values of both kind of errors, the area where VET fails become negligible. These results are in line with recent simulation studies by Romero (2016), and Stanley et al. (2014).

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Real number algorithms and open texture

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Keywords: Real number algorithms, recursive analysis, BSS model, computing over the reals, open texture, formalization of intuitive concepts, foundation for scientific computing

Since A. Turing's seminal work, computability over integer numbers has been well investigated and developed to a rich mathematical theory. However, things are less clear in the domain of real numbers and functions, and the area is still open to further investigation. More specifically, whereas the mathematical community universally accepts that the intuitive calculable functions over the integers are exactly the Turing computable functions, there is no similar result about which real functions are the computable ones. Rather, there are different accounts specifying what it means for a real function to be computable or not. Interestingly, we have to do here with an area of mathematics that lacks a unanimous treatment within the mathematical community. As Weihrauch puts it:

> While for mathematical branches like linear algebra or recursion theory there are canonical foundations [..] [all offered models of real computation] have important concepts in common, but differ in their contents and technical framework. [..] This mirrors the fact that computable analysis still has no generally accepted foundation. (Weihrauch, 2000, 2)

The good aspect of this, from a philosophical point of view, is that we can obtain some useful insights into the process of rigorization of concepts that have informally been used in the mathematical practice. This paper is an attempt to pin down and discuss some of such insights.

I examine two main approaches: the *BSS model* (an algebraic approach developed by Blum et al. 1997) and the *recursive analysis* approach (see, e.g., Weihrauch 2000). Both models formalize what is meant by an 'algorithmic routine over the real numbers'; however, they characterize different functions as the 'computable' ones. The recursive analysis approach, for example, admits only continuous functions as computable, so relatively "simple" functions, such as the floor function $f(x) = \lfloor x \rfloor$ (integer part of x), are not computable. On the other hand, the BSS model admits noncontinuous functions as computable but not transcendental ones, such as the exponential function $f(x) = e^x$. Both models, though, claim to offer a foundation for scientific computing.

I try to show how the conflict between the two models is the result of different (but equally legitimate) ways to *regiment* our intuitive notion of a 'real number algorithm'. I argue that, at a first pre-theoretic level, this notion exhibits *open texture*, meaning that there exist mathematical processes about which it is potentially completely undetermined by the so far established use of the concept whether they fall under its extension or not. So, although both computer science and numerical analysis are about *algorithms*, some nuances of the pre-theoretic conception are understood differently within each tradition. By pinning down the different understandings and motivations in each case, I show how the different ways of sharpening the intuitive notion call for different idealizations, and thereby lead to conflicting formal models.

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Modal extension of LK-Quarc

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Keywords: Modal logic, Sequent calculus, LK, Quarc, Cut elimination

The Quantified Argument Calculus (Quarc), presented in Ben-Yami (2014) following a previous outline in Ben-Yami (2004), is a system of quantified logic arguably closer to natural language than the standard predicate calculus, but which nonetheless retains the prerequisite properties of a well-behaved logic system.

While Quarc is presented as a system of Suppes-Lemmon style natural deduction, in a recent paper, Pavlovic and Gratzl (2016), a sequent-calculus version of it was formulated as LK-Quarc, a modification of the system in Gentzen (1969). Pavlovic and Gratzl (2016) prove that the system possesses a series of properties, most notably cut elimination property and its associated corollaries.

This paper extends the system LK-Quarc to include modalities, following considerations from the final sections of Ben-Yami (2014) and adopting a framework from Negri and von Plato (2011). Cut elimination theorem, as well as some associated corollaries, are then demonstrated for a modal extension of LK-Quarc into a series of modal systems (K, T, B, 4, S4, S5). Finally, the paper discusses the intuitiveness of the extension as well as outlines work yet to be done.

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Disease as Essence Annihilation

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Keywords: Disease; essence annihilation; negative vital value; dysfunction

In the literature about the nature of disease, we can distinguish between two main theories of disease: one – labeled "the axiological theory of disease" -, according to which disease is a (kind of) negative vital value, the other – labeled "the dysfunctionalist theory of disease" -, according to which disease is a biological dysfunction. These theories are based on two different intuitions that we have about what disease is. The axiological theory of disease accounts for our intuition that attributing to a (part of an) organism the property of being diseased is negatively evaluating this (part of the) organism, viz., attributing to it a kind of negative value; saying "*x* is diseased" is a kind of negative evaluative judgment. We would intuitively say that the kind of negative value at issue is a vital one, for we may define a vital value as following: *φ*-ing is vital to *x*, iff *φ*-ing is good or bad, and *φ*-ing is necessary to the maintenance or destruction of *x*. The dysfunctionalist theory of disease accounts for the intuition that, when we judge that a (part of an) organism is diseased, we mean that this (part of the) organism incorrectly functions (or is dysfunctional). Saying that *x*'s liver is diseased is saying that *x*'s liver incorrectly functions, meaning that there is a biological process depriving *x* from its correct functioning or its constitution. It is obvious that a definition or complete theory of disease should account for, in a unified and coherent way, both the axiological and the dysfunctionalist theory of disease. The purpose of this talk is to provide such a definition or complete theory of disease - that we may label "essentialism about disease" -, according to which, roughly, disease is the annihilation of the essence of (a part of) an organism. The talk is organized into two parts. The first part presents the above axiological and dysfunctionalist theories of disease. The second part provides an original complete theory of disease labeled "essentialism about disease". In the second part of the talk, I argue for the following definition of disease: *x* is diseased, iff (1) *x* is a part of an organism or *x* is an organism, and (2) *x* has a negative vital value, and (3) *x*'s essence is being annihilated. Let us examine points (1), (2) and (3) each in its turn. For (1), I argue for a fine-grained individuation of what can be diseased, viz., the disease bearer: only an organism - e.g., a human being or a plant - or one of its parts – e.g., the human heart, bone, rationality or even personality (as parts of the human organism) - can be literally diseased. For (2), according to the axiological theory of disease, disease is a (kind of) negative vital value alongside other negative vital values like injury. This suggests that the relationship between disease and the kind negative vital value is a relationship between, respectively, a thick and a thin value; one can give the following definition of a thick value: *x* has a thick value (or value species), iff *x* has a thin value (or value genus), and *x* possesses a differentiating property (or differentia). For (3), following essentialism about disease, that this differentia has to be the annihilation of the essence of the disease bearer is justified by our everyday comparative uses of "disease". Indeed, disease comes with degrees; we have to make clear that *x* can be more or less diseased (than *y*) by saying that there is a biological process more and more destroying the essence of *x*. The process of annihilation most often (due to the usage of drugs or to the immune system) remains unfinished; thus, when one says that one is diseased *simpliciter*, one is actually, most often, merely a little or very diseased. The reason why disease is *x*'s essence annihilation is that a biological function may be defined as a dynamic essential property of the function bearer (e.g., the function of pumping blood can be said a part of the essence of the heart), where the whole essence (or the essential property) of *x* is all the necessary and sufficient conditions for *x* to be the case (e.g., the heart is an organ pumping blood), and a part of the essence (or an essential property) of *x* is a necessary but insufficient condition for *x* to be the case (e.g., the heart pumps blood). As a biological dysfunction, viz., as a biological process depriving *x* from its correct functioning or constitution, disease is, thus, the annihilation of *x*'s essence. To conclude, I show how essentialism about disease coherently unifies the axiological and dysfunctionalist theory of disease through the notion of essence annihilation.

Interoperabilty and knowledge modelling: epistemological issues in DLs and Ontological Semantics

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Keywords: Interoperability; Description Logics; Knowledge Representation.

Interoperability is a current central issue both in the industrial and scientific applications of knowledge-based systems (Bernard & Martin, 2015). The typical architecture for those systems lays on one or more formal ontologies, based on DLs-based OWL (Baader et al., 2007), and on different reasoning algorithms. In this paper, in the first place, I will analyze the necessity for interoperability between different ontologies and consequently the notions of ontology matching, mapping and reuse (Noy, 2009). In particular I will examine the domain specific definition of interoperability, dealing with other critical concepts as exchange, standard, framing and definition.

The second part will enlarge the focus to a general Knowledge Representation (KR) point of view (Van Harmelen et al., 2008), considering the most recent debate upon definition and knowledge modelling in formal ontology (Smith et al., 2016). From this perspective, it will emerge how KR is language dependent and strictly bound to several issues in Natural Language (NL) semantics, well shaped in the Ontological semantics framework (Nirenburg & Raskin, 2004). Either looking at the cognitive functions of modelling and definitions or at their logic functions, the attention to the defining techniques must be considered as central. In fact, since the first studies in upper ontologies devising (Guarino & Welty, 2000) and in NL semantics and knowledge (Filmore C., 1976), it emerged an essential framing problem for isolating the domain specific knowledge and all the borne constraints. Filmore's work on the congnitive concept of *frame*, later developed in the Framenet project, has to be considered seminal in the cognitive field as well as in ontology building.

In conclusion, I will justify the centrality of the framing process in conceptual modelling and, briefly confronting some different strategies for this task, I will sketch some suggestions suitable for interoperability and framing.

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From Affect Circumplex to Affect Pendulum? Insights from psychopathology on the relation between arousal and valence

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Keywords: psychopathology; psychiatry; affect; arousal; valence; Core Affect; Affect Circumplex; emotions

In the past thirty years, the debate on emotions and other affective states has flourished in a number of sub-fields within psychology and philosophy. Notably, there have been several attempts to construct models that would successfully describe affective experience by individuating its basic components. Pioneering work in this sense has been conducted by James Russell (2003). On his account, our affective lives are characterized by a baseline neurophysiological state called Core Affect, which consists in a certain degree of arousal (activation) combined with a certain degree of valence (pleasure-displeasure). These two allegedly independent dimensions are usually represented in what Russell dubbed the Affect Circumplex (1980). For example, an emotion such as anger would be characterized by high arousal and negative valence, whereas a calm mood would be characterized by low arousal and positive valence: these two states would thus occupy opposite corners in the Affect Circumplex.

In this talk we draw on Russell's notion of Core Affect to explain some salient features of psychopathology, with a focus on depressive disorders. The argument is divided into three parts. In §1 we argue that, despite the current lack of knowledge about underlying mechanisms, Russell's Affect Circumplex is a legitimate and well-suited tool for describing the affective states underlying a number of mental disorders. In §2 we focus on a recent study conducted by Russell and his collaborators (Kuppens *et al.*, 2013) in order to raise a challenge to their account. In brief, we argue that some important features of psychopathology concerning the relation between arousal and valence can be captured only if affective states are measured diachronically (i.e. over time) as opposed to synchronically (i.e. at a given time t). Indeed, we believe that the almost exclusive focus on synchronicity has obscured some important interactions between the two dimensions of Core Affect. In §3 we sketch a working hypothesis about a currently unexplored interaction between arousal and valence, that we dub the "Core Pendulum Hypothesis". Specifically, we suggest that – in ordinary as well as pathological cases – arousal could have a modulating effect on valence, acting like the rod in a pendulum: the longer the rod, the greater the oscillation. When arousal increases, valence tends to oscillate more: this may happen both in non-pathological situations (e.g. when one is over-caffeinated) and in pathological ones – e.g. bipolar depression. On the contrary, when arousal decreases it becomes easier to get "stuck" in a particular valence: this happens whenever we have trouble snapping out of a mood, and more severely in cases such as unipolar depression. Such hypothesis may elegantly account for the weak but consistent V-shaped relation between arousal and valence found by Kuppens *et al.* (2013). We conclude the talk by devising some possible ways in which the Core Pendulum Hypothesis could be tested.

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Extended computational systems

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Keywords: computationalism, philosophy of cognitive science, extended cognition.

Computationalism is the view that "intelligent behavior is causally explained by computations performed by the agent's cognitive system (or brain)" (Piccinini, 2009, p. 515). In this broad sense, computationalism is not committed with any position about the nature of mental states and processes. However, the received view of computationalism is not neutral in this sense. According to Classic Computationalism a cognitive system computes on strings of internal symbols which encode representations. Wilson (1994) maintains that the majority of computationalists accepts the following *computational argument for individualism*:

P1 We individuate mental states and processes as computational states and processes.

P2 Computational states and processes instantiated by a cognitive subject supervene on the intrinsic, physical properties of that subject.

C Mental states and processes supervene on the intrinsic, physical properties of a cognitive subject.

Wilson states that this argument should be rejected because P2 is false. Indeed, a "computational system could transcend the boundary of the individual and include parts that individuals environment" (Wilson, 1994, p. 352). This position is admissible only insofar one refuses the so-called *formality condition* (Fodor, 1980), namely the idea that the formal properties of mental states are to be intended as intrinsic properties of mental symbols–such as their shape, size, etc. If we accept this conception of formality, indeed, we should also agree with the arguments from computationalism to individualism. However, there is another notion of formality, currently used in logic, according to which formal properties are the properties of formal systems. Here, the precise formal property of a symbol is given by its interpretation in a formal system. With this concept on hand, we are no more forced to accept the computational argument for individualism. I show that Wilson's idea is perfectly consistent with Wells' *ecological functionalism* (Wells, 2005). Then, I explain why we should take some specific kinds of cognitive phenomena, that I call algorithmic activities, as true examples of extended computational systems. Lastly, I discuss why such cognitive systems may constitute genuine cases of extended cognition, although in a different sense from what originally proposed by Clark & Chalmers (1998).

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Transparency, belief formation and consciousness: a deadlock for the extended mind?

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Keywords: extended mind, belief formation, consciousness.

The debate on extended mind has flourished in the last twenty years, but there is an important question that remains unanswered in the literature: is the past-endorsement criterion valid? The past-endorsement criterion is among the criteria that Clark and Chalmers (1998) elaborated in order to respond to the "cognitive bloat" objection. Its purpose is to restrict the contents that are plausibly ascribable to an extended mind and those – like the Encyclopedia Britannica in one's bookshelf or Google pages – that are not.

To the criteria describing the features of portability, availability and reliability is added a criterion regarding an alleged previous conscious relation between the agent and the content in question. This forth criterion has the aim of restricting the sense of "dispositional" to the states that are "non occurrent", but they have been occurrent at least once.

So, Clark and Chalmers (1998), referring to the case of Otto, argue that "the information in the notebook has been consciously endorsed at some point in the past, and indeed is there as a consequence of this endorsement". In a footnote, though, they promptly admit: "The status of the fourth feature as a criterion for belief is arguable (perhaps one can acquire beliefs through subliminal perception, or through memory tampering?), but the first three features certainly play a crucial role".

Soon enough, Rupert (2004, 2009) identified in the forth criterion a hidden reintroduction of a form of internal privilege, which is also not compatible with the account of belief formation given by cognitive psychology. If an extended content, in order to be considered a mental one, has to have been endorsed consciously in the past, it means that consciousness – that according to Clark and Chalmers is not and will probably never be extended (1998, Clark 2009) – plays a fundamental role in determining what count as mental. Even if this objection is discussed in the literature (Clark 2008, 2010), we believe that the problem remains. So, the past-endorsement criterion puts the extended mind supporters in front of two options:

1) if they maintain the past-endorsement criterion, they have to find a way to answer to the criticism about the reintroduction of the internal privilege, and to explain their account of belief formation that seemingly requires a conscious endorsement (contra the idea of beliefs acquired by e.g. subliminal perception);

2) if they reject the past-endorsement criterion, they have to find another way to respond to the cognitive bloat objection.

In this paper we want to discuss the past-endorsement criterion and these surrounding problems and to propose a different solution to the objection of the cognitive bloat, based on a modified version of the principle of parity that introduces a further condition to identify the alleged extended mental contents. The condition has to do with a potential direct access to those states to the personal mind of the subject. In other words, it has to do with the concept of transparency.

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On polymorphism and quantifiers in Linear Logic

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Quantification over propositions carries with it the issue about the existence of propositions referring to all propositions, including itself. The investigations on higher-order type theories and on the related notion of parametric polymorphism constitute the technical counterpart of this old foundational problem.

It is within such investigations that Linear Logic, through the coherent space semantics, was born in 1987 ([Gir87]). Linear Logic provided considerable advances in several areas of proof-theory: its most well-known features are (1) the so-called *pons asinorum*, i.e. the decomposition $A \Rightarrow B = !A \multimap B$ of implication into a non linear connective (the exponential !) and a linear connective (the multiplicative $\multimap$) and (2) the introduction of proof-nets, a graphical syntax for proofs in which rule permutations become geometrical invariants.

Maybe surprisingly, the connections of Linear Logic with polymorphism haven't received a comparable attention in the literature. The theory of quantifiers for proof-nets has been developed by J.-Y. Girard in two papers ([Gir88, Gir91], both published in the SILFS proceedings). This theory, while extending point (2) to quantifiers, does not extend point (1). However, a *pons asinorum* for quantifiers would be expectable, since polymorphic quantifiers are usually presented as products over the type of all propositions.

In this paper we investigate polymorphism in the case of proof-nets for second order multiplicative linear logic and we present a new definition of polymorphic quantifiers for proof-nets, satisfying both features (1) and (2) and yielding a correctness criterion equivalent to Girard's criterion with jumps.

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Conservation laws and Sean Carroll's critique of mind-body dualism

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Keywords: conservation laws, mind-body problem, substance dualism, property dualism, mental causation

In recent years many views, including physicalism, have had problems with mental causation. Property dualism with distinctively mental causal powers shares something like the traditional mind-body problem usually associated with interactionist substance dualism. It is timely then to reconsider the logic of the old mind-body problem regarding both conservation laws and Carroll's modern physical analog of Princess Elisabeth's objection.

Conservation laws are often said to undermine such causally efficacious dualism(s). While conservation laws often appear as a black box in the philosophy of mind, conservation laws have a well understood theoretical basis and some features that affects the viability of this criticism of such dualisms, for better and for worse.

First, conservation laws are primordially *local*, holding not for the universe as a whole, but for every place separately. Second, consequently local conservation laws form an infinite conjunction, so the failure of this conjunction is an infinite disjunction that permits local conservation laws to hold nearly everywhere (such as in refrigerators and stars), even if they were to fail in minds. Thus there is a gentle failure property, not Bunge's universal catastrophe. Third, conservation laws have a theoretical basis in symmetries due to Noether's theorem: energy (momentum) conservation is a consequence of the uniformity of natural laws over time (space, respectively). Fourth, Noether's theorem has a converse: conservation laws imply symmetries, so by contraposition, violation of symmetries implies violation of conservation laws. Thus one should positively *expect* conservation laws to fail, given dualism. Thus to expect conservation laws to hold *in the brain without an empirical neuroscientific basis* is simply to expect the absence of distinctively mental influences on the brain, *i.e*., to beg the question against such dualism. Fifth, though energy-momentum in Einstein's General Relativity has peculiar features and suffers from interpretive disagreement about local conservation laws, the theory doesn't ultimately help dualism. This discussion has assumed the principle of least action in classical field theory and ignored quantum physics, thus meeting the conservation objection in its strongest form. What remains of the objection is that thus far we haven't seen distinctively mental causation in the brain, despite increasingly detailed investigation.

In the interest of refuting life after death, Sean Carroll (Carroll, 2011) has claimed that nonphysical mental causation would "overthrow everything we think we have learned about modern physics". He frames the issue clearly: how should one modify the Dirac-Maxwell-Einstein equations for electrons coupled to electromagnetism and gravity in order to accommodate distinctively mental causation? Without disclosing a mathematical argument, Carroll ridicules the task of answering such questions as an "absurdity". Here preliminary steps are taken to fill in Carroll's argument and to ascertain how such a how-possibly explanation is constrained by mathematical physics. A novel difficulty arises involving the generalized Bianchi identities in General Relativity.

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Theoretical roots of the computational and representational paradigms in cognitive neuroscience

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Keywords: cognitive neuroscience; computationalism; structural representation

It is commonplace in cognitive neuroscience to employ computational and representational vocabulary to characterize various neural processes, not only, the construal of neurons as computing and representing seems more obvious when engaged in their higher cognitive functions. However, a sound theoretical account of neural computation and representation turned out to be problematic. Most of the controversies, we argue, arise because both notions have been borrowed from disciplinary domains foreign to neuroscience and psychology. In the case of computation, for example, the common ground of many criticisms is the ill posed metaphor between computers and minds (Cordeschi and Frixione, 2007). As a consequence, most of the defensive efforts rely in extending the concepts from their original domain to that of the mind, the risk is that the extended concept become liberal enough to apply to everything. In the case of computation, an impressive effort has been spent in the last few years in distilling a notion of computation workable for brains and computers, leaving apart rolling stones, soap bubbles, piles of sand, and the many pancomputationalists' amenities (Miłkowski, 2013; Fresco, 2014; Piccinini, 2015). Less progress has been achieved for the concept of representation, which, even if related with computation, borrows its best articulation, as *structural representation* (Ramsey, 2007), from the measurement theory domain (Suppes and Zinnes, 1963; Swoyer, 1991). We argue that this legacy, while fruitful in the first place, turned later into a source of troubles, allowing, for example, Morgan (2014) to show that structural representations are ubiquitous in all sorts of natural and man-made systems, including car's oil light. We believe that structural representation is an appropriate and important notion for neurocognitive processes (Plebe and De La Cruz, 2016), requiring a foundational effort parallel to that done with computation, moving away from the original measurement theory domain, and reconciling with the current knowledge of the structure of neural circuits. An example of structural formalization related to neural population coding (Kriegeskorte, 2009) will be provided.

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Towards robust causal claims in pharmacological risk assessment

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Keywords: scientific inference, pharmacology, evidence, relevance, similarity, analogy.

In pharmacology and epidemiology, reasoning by analogy is at the basis of inductive inferences from study to target population. Because of the context sensitivity of many causal associations in the biological realm, these associations may hold only in specific populations, and evidence about causal effects related to one population may not license similar conclusions about another population, unless the two populations have been established as *analogous*, and relevant co-factors taken into consideration (see, e.g., Chan & Altman (2005), Doll & Peto (1980), Worrall (2007)). Knowledge about an agent's mechanisms and about its impact on the biological environment might be sparse and come from quite heterogeneous sources, however. Yet, already if only little information about the agent's class of molecules is available, for example, this can justifiedly be exploited for causal assessment *via analogy*. A recent paper by Landes, Osimani, and Poellinger (Landes et al. (2017)) explores the possibility of amalgamating heterogeneous evidence in a Bayesian reconstruction of scientific inference for the integrated probabilistic assessment of a drug's causal (side-)effects. Building on Bovens & Hartmann (2003), this framework opens the possibility of tracing the dynamics of analogical reasoning across distinct epistemological categories: theoretical hypothesis (Hyp), testable indicators (Ind), and evidence reports (Rep). Within this layered reconstruction of scientific inference, I formally explore the interplay between heterogeneous evidence and the different components of a causal hypothesis in pharmacological risk assessment. For this purpose, I will explicate causal hypotheses as a four-place relation between cause, effect, target population, and causal structure in order to utilize formal explications of similarity and analogy for the evaluation of the relevance of given evidence for the investigated hypothesis. Relating the causal hypothesis and different sources of evidential support in an *epistemologically* interpreted Bayesian network allows for "zooming in" onto the hypothesis and formally explicate its components in a *causally* interpreted Bayesian network. In this talk I will discuss how embedding such a causal structure in the layered evidence-amalgamating network facilitates (i) locating (and distinguishing) causal co-factors and ceteris-paribus population characteristics and (ii) explicating how cumulating relevant evidence may shape hypotheses about risk in pharmacology towards more robust causal claims.

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Anticipatory systems. A first introduction

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Keywords: anticipation, impredicative systems, modeling relation, Robert Rosen.

Anticipation occurs when the future is used in action. From this perspective, anticipation consists of two necessary but distinct components: a forward-looking attitude and the use of the former's result for action. A weather forecast in itself is not anticipatory in our sense. Watching a weather forecast and, as a consequence taking an umbrella before going to work is instead an anticipatory behavior. Anticipation's two components are consistent with Rosen's definition: "An anticipatory system is a system containing a predictive model of itself and/or its environment, which allows the system to change state at an instant in accord with the model's predictions pertaining to a later instant" (Rosen, 2012, pp. 8, 313, originally published in 1985).

The anticipatory processes and models that allow the future to become part of actions in the present may be either internal or external. Watching a weather forecast and behaving accordingly is to use an external model; actions taken on the basis of subjectively constructed psychological expectations are internal. Internal anticipation is particularly important for understanding the anticipatory behavior of people, groups, organizations and institutions. A major issue concerning internal anticipation is its origin: where does internal anticipation come from? We will see that this apparently minor issue serves as a dividing line that distinguishes radically different theoretical perspectives (Poli, 2017).

It is our thesis that behavior is primarily anticipatory, while reactive behavior is only a secondary – albeit important – component of behavior. A system behaving in an anticipatory way – an anticipatory system – makes decisions in the present according to 'anticipations' about something that may eventually happen in the future. One of the key starting points for this research is the contention that the currently dominant theories of time and causation are unable to incorporate anticipation and, therefore, are in need of serious revision.

When first confronted with the proposition that anticipation is a subject worthy of research, most people seem to think that anticipation is a feature that human beings possess because we are such highly complex and wonderfully sophisticated cognitive agents. That is not what the theory of anticipation claims. Indeed, the major surprise embedded in the theory of anticipation is that anticipation is a widespread phenomenon present in and characterizing *most* types of real systems. Life in all its varieties is anticipatory; the brain works in an anticipatory way; the mind is obviously anticipatory; society and some of its structures are anticipatory; even non-living or non-biological systems can be anticipatory (Poli, 2018). All this often comes as a surprise.

If all that is true, and providing that the necessary supporting evidence is offered, it follows that a proper understanding of anticipation necessitates the adoption of new scientific (as well as philosophical) frameworks.

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Idealization and abstraction in scientific modeling

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Keywords: Idealization, Abstraction, Scientific Models

It is commonplace that Science doesn't work by describing the target systems of the world in their full complexity, but instead some general principles that are believed to be non-accidental generalizations are used as antecedents that guide the construction of models of physical systems. Such models are thought to be among the primary instruments of scientific representation of target physical systems and it seems commonplace among philosophers and scientists that model-building is steered by acts of simplification.

Attempts to explicate model-simplifying assumptions have led to exploring the nature of such assumptions and their epistemic significance, as well as the different ways by which they are employed and their uses in model-building. Needless to say that much insight has been gained from this endeavor. In this paper I focus on one aspect—that seems to be the starting point of some—of these efforts. Namely, the frequent admission that model-simplifying assumptions involve two rather distinct characteristics: the omission of features of the phenomena from the scientific representation of respective target systems (often referred to as *abstraction*) and the simplification of features that are retained in the representation (often referred to as *idealization*). It seems to be one thing to omit a characteristic from a description and it seems to be another thing to retain a characteristic in the description but change some of its attributes. This apparent difference dictates a question: how can the two kinds of assumptions be defined so that the given definitions lead to a clear-cut distinction of the two?

Not everyone addresses this question or shows any indication that they consider it of any importance; in fact, some regard these two kinds of assumptions as two particular facets of their own generic notion of idealization that, by and large, are both present in scientific uses of simplifying techniques. Others, however, pay particular attention to the question and attempt to spell-out particular ways by which to explicate the distinction. Such examples include Jones (2005) and Godfrey-Smith (2009). In this work, I will defend the thesis that although it is possible to distinguish idealization from abstraction as two facets of the same cognitive act, the attempts by the latter group of authors to distinguish the two fail to meet the desired goal.

I explain some conceptions of the distinction between abstraction and idealization. I offer some arguments why these conceptions fail to meet their purpose. Finally, I suggest a different way by which to distinguish the two: that both idealization and abstraction could be understood as particular forms of the same cognitive act (or thought process)—that of *selective attention*. By attending selectively to particular aspects of the target system modelers abstract away from unwanted noise, and by attending selectively to particular features of those aspects, or in particular ways, modelers idealize. The same cognitive act employed in different ways is responsible for the two ostensibly distinct categories of model-simplifying assumptions.

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Scientific realism and the philosophy of causation

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Keywords: covering-law explanation; scientific realism; causal powers; dispositions.

In the last few decades, the "covering-law" model of explanation has become a major target of criticism in the philosophy of science. Much of this criticism has been directed at the problems of confirming covering-law hypotheses. Other critics have pointed to the limited explanatory power of such hypotheses and to the impossibility of consistently extending them beyond their context of discovery. Still others have emphasized the trade-off between the generality of law-like correlations and the ability to provide fine-grained causal accounts of individual phenomena.

This paper will argue that such criticism is best interpreted in terms of the alternative between *scientific realism* and *anti-realism*. While it is not always explicitly presented through this dichotomy, most criticism of "covering-law" explanation implicitly targets its lack of realism. Aside from the specificities of individual critiques, large law-like correlations are generally seen as problematic in that they systematically fail to capture the causal processes that are believed to have actually generated the phenomena being studied, and hence fail to causally explain them in any relevant sense. Proponents of the mechanism-based model of explanation, for instance, tend to link explanatory power to the capacity to "open up the black box" of law-like correlations and detail the causal chain of events that lead up to the *explanandum* (see e.g. Elster 2007).

This paper will argue that this view puts important constraints on the metaphysics of causation underpinning scientific realism. To show this, the paper will draw on a useful distinction between "causation as a relation of dependence" and "causation as production" (Hall 2004). The former view essentially describes causation in terms of counterfactual dependence, i.e. as a relation that holds in all possible settings in which the causing event may occur. As even proponents of this view acknowledge, however, causation as dependence amounts to a sophisticated regularity view, since dependence in all possible settings can be straightforwardly reformulated in terms of regular co-occurrences of a cause and its effect in all possible settings (Mackie 1974, Beebee 2006). As such, causation as dependence is utterly incompatible with scientific realism.

Productive causation, on the other hand, understands causation as a real-word process capable of genuinely generating outcomes. While this view is not qualified any further as an alternative to causation-as-dependence, some have altogether equated it with the "power-based" view of causation once prominent in metaphysical debate (e.g. Strawson 1987, Groff 2016). On this view, causation is seen as the materialization of dispositional properties by virtue of which the entities that populate the world are capable of producing effects. While this position has deep roots in the history of philosophy, it did not resurface until relatively recently in metaphysics and in the philosophy of science.

The argument above seeks to contribute to the epistemological debate by showing the relevance of the metaphysics of causal powers to scientific explanation. Indeed, the main claim of this paper is that only a power-based account of causation can support a realist approach to explanation alternative to the covering-law model of explanation.

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Bacon's New Atlantis, the Newtonian scientific method, and the Cartesian analysis

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Keywords: scientific method, method of analysis, inductive method, hypothetico-deductive method.

Bacon's New Atlantis stands out for its detailed account of the way science should be exercised in order to deliver knowledge of the world and technological applications. I examine the structure of the scientific academy of New Atlantis, which reflects the structure of the scientific method as envisaged by Bacon. I will underline the parallels to Descartes' and Newton's method of analysis, which was construed as being a universal scientific method. I argue that there is continuity across the thoughts of these thinkers on the nature of the scientific method. This is important if one considers the standard view that Bacon and Newton promote the inductive empirical method of science in which knowledge is acquired through inductions form the phenomena without any hypotheses intervening in the process, whereas Descartes endorses the hypothetico-deductive view in which knowledge is acquired through drawing conclusions from general principles delivered by the exercise of pure reason. This view, which dominated until the 90's has fainted but still retains some grip.

In the first part, I examine the structure of Bacon's scientific academy and its repercussions for the proper scientific method as envisaged by Bacon. Among the most important ingredients of the scientific method are

(i) the gradual ascension from experiences, including both observations and experiments, to hypotheses and theories;

(ii) the firm distinction between three stages of scientific inquiry, namely, data collection, physics (that aims to discover the mid-level regularities governing the phenomena), and metaphysics (that aims to discover the higher-level causes of the phenomena), and

(iii) the conducting of crucial experiments.

In the second part, I discuss the parallels between Bacon's and Newton's views concerning both the gradual ascension from data to the mid-level regularities that govern the phenomena and from there to the causes of the phenomena, and the requirement that crucial experiments are required to establish a hypothesis, to bring to the fore the close affinities between the two methods.

Finally, I underly the close connection between Cartesian themes in scientific method and the Baconian scientific method as it emerges from New Atlantis. Specifically, I discuss Bacon's and Descartes' views that analysis is the method of discovery and proof, and also their views that all scientific inquiry consists in discovering the ways some simple natures are combined to produce the composite natures or phenomena. The right proportion or combination of simple natures is to be discovered by means of proposing hypotheses about possible causes, testing them, and performing the appropriate crucial experiments. This is the process Descartes describes in the sixth part of the *Discourse* and this is also the process clearly proposed as the proper scientific method by bacon.

A new proposal how to handle counterexamples to Markov causation à la Cartwright, or: Fixing the chemical factory

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Keywords: causal Markov condition; screening off; counterexamples; non-causal dependence; Cartwright

The causal Bayes net framework (Spirtes et al., 2000) is nowadays probably the most promising philosophical approach to causation from an empirical point of view. It is a powerful tool for formulating and testing causal hypotheses, for learning causal structure on the basis of statistical and/or experimental data, and can even be used to predict the outcomes of interventions when only purely observational data is available. Another advantage over competing philosophical theories of causation consists in the framework's closeness to successful empirical theories of the sciences (Schurz & Gebharter, 2016).

However, there seem to be problems with the causal Bayes net framework as a general theory of causation. One of the most prominent of these problems is constituted by a counterexample to the core assumption of the theory put forward by Cartwright (1999). Cartwright has constructed a scenario in which a chemical factory produces a certain substance with a certain probability. When this chemical is produced, however, the chemical factory always produces a pollutant as a byproduct. Cartwright further assumes that the chemical factory is the only common cause of the product and the byproduct and that there are no other causal connections around. The scenario then violates the core assumption underlying the whole causal Bayes net approach, the causal Markov condition, by implying a dependence between the chemical and the pollutant (conditional on their common cause, i.e., the chemical factory) that is excluded by the causal Markov condition. Similar scenarios involving indeterministic processes such that conditionalizing on a common cause does not screen off its effects can be found in the micro as well as in the macro domain.

In this paper we explore a new way to handle counterexamples such as Cartwright's (1999). One crucial precondition for successful causal modeling is that the systems of interest do not feature variables standing in other than causal relations. We argue that scenarios such as Cartwright's chemical factory violate the causal Markov condition because they do not meet this precondition. These scenarios involve a specific kind of non-causal dependence. We then develop a method for representing this kind of non-causal dependence based on a recent proposal how to handle the problematic scenarios put forward by Schurz (in press). Though Schurz' approach is technically elegant and more general, our representation seems to have some advantages: It preserves the original causal Markov condition and the basic idea underlying it: Once all causal paths connecting two variables are blocked, the two variables do not causally depend on each other anymore. In addition, our approach allows for drawing a metaphysically more complete picture of what is going on in the problematic scenarios; the non-causal dependencies involved in these scenarios are represented by non-causal paths. Another advantage will be that it seems to more clearly allow for richer explanations of certain observations.

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A new numerical treatment of Bertrand's paradox

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Keywords: Bertrand; paradox; probability; infinitesimals; grossone.

If one were to draw at random a chord in a circle, what is the probability for it to be shorter than the side of the inscribed equilateral triangle? This question, originally posed in (Bertrand, 1889), gives rise to a puzzle, known as Bertrand's paradox, because distinct drawing procedures exist, each of which determines a distinct value for the sought probability (Bertrand described three). This puzzle has generated an extensive literature, aiming to tame the paradox by means of classical (i.e. measure-theoretic) probability theory. Recently (see (Gyenis and Redei , 2015)), Bertrand's paradox was shown to be an unproblematic phenomenon within classical probability. This result, interesting as it is, does not show that Bertrand's question does not have a unique answer, but only that classical probability cannot determine it. A different set of conceptual resources may be needed to this end. It is clear from the character of the problem that the resources required must overcome two difficulties: (i) they must afford discriminations of size between infinite collections that look indistinguishable from a classical set-theoretic perspective; (ii) they must provide *numerical* discriminations of size, amenable to arithmetical computations, since numerical probability values have to be obtained out of them. *Desiderata* (i) and (ii) can both be satisfied by adopting the computational methodology introduced in (Sergeyev, 2003), whose employment in the context of probability models is suggested in Sergeyev (2009). Sergeyev's approach makes it possible to provide numerical estimates of the sizes of collections of chords and affords the means to tackle Bertrand's paradox in an elementary, combinatorial manner. I show how the paradox can be solved by appealing to a partition of the unit circle into as many equal arcs as there are natural numbers. The number of equal arcs is specified by means of Sergeyev's infinite unit, called *grossone* and measuring the length of $\mathbb{N}$, conceived as a sequence. This initial numerical specification makes it possible to introduce a uniform, discrete distribution on an infinitely large collection of chords. A counting argument then suffices to obtain a numerical estimate of the probability of selecting a chord shorter than the side of the inscribed equilateral triangle. What I obtain is an approximate probability value, which can be made more precise by taking finer partitions of the circle, but that already fixes the finite part of the relevant probability, namely $2/3$. I conclude by showing that, when the same numerical approach is applied to the three distinct drawing methods discussed in the literature, each of them yields the same finite estimate of the sought probability. These methods yield distinct probability values under a classical treatment, which cannot be sensitive to the character of Bertrand's infinite geometrical setup.

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The multi-field interpretation of the wave function in Bohm's theory

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Keywords: Bohm's theory, wave function, multi-field

Bohm's theory (Bohm, 1952; Bohm & Hiley, 1993) is a quantum theory with a clear physical interpretation: an N-particle Bohmian system is described by a configuration of N particles in 3D space and a (generally) entangled N-particle wave function, which is defined in 3N-D configuration space. However, there is yet no common agreement about the metaphysical interpretation of the wave function within the theory. Two main options have been proposed in the literature:

1. The wave function is a real physical field in configuration space (Albert, 1996; Albert, 2013);
2. The wave function is a nomological entity, similar to the Hamiltonian in classical mechanics (Goldstein & Zanghì, 2013). The nomological interpretation has been further developed in the metaphysical frameworks of the Dispositionalism (Belot, 2012) (Esfeld & al., 2014) and Humeanism (Esfeld, 2014).

In this paper, I propose a third option, namely to regard the wave function in Bohm's theory as a new type of physical field in 3D space. Dating back to (Forrest, 1988), and following the terminology introduced by (Belot, 2012), I shall call such a new field a "multi-field". The multi-field is a straightforward generalization of an ordinary field in classical mechanics: instead of assigning a precise value at each point of 3D space (as a classical field does), it assigns a precise value to N-tuples of points of 3D space. The configuration space is therefore best understood as a mathematical space rather than a physical space, its number of dimensions representing the degrees of freedom of the wave function rather than the number of spatial dimensions over which the quantum field is defined. Moreover, the multi-field interpretation (dis)solves the problem of communication, since both the wave function and the particles live in the same 3D physical space.

The multi-field interpretation, in short, permits to think of the wave function in Bohm's theory as a real physical entity (contrary to the nomological interpretation) while retaining the ontology of the theory in the ordinary 3D physical space (contrary to Albert's view).

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Scientific self-correction: the Bayesian way

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Keywords: scientific self-correction, replication crisis, Bayesianism, statistical inference, behavioral science.

It is a popular view that by following the scientific method carefully and rigorously, scientists will eventually detect the mistakes which they have made, correct them and get closer to the truth. This belief is known as the "self-corrective thesis" (SCT).

Current practice in the behavioral sciences casts doubt on SCT. Replications and original experiments often differ in terms of the statistical significance of outcomes and observed effect sizes (e.g., Makel et al., 2012; OSC, 2015). Given this divergence, do we have reason to believe that aggregated evidence from a series of replications will really converge on the truth?

Answering this question requires a statistical framework for data analysis and the aggregation of experiments. In this contribution, we evaluate whether SCT is correct if data are analyzed and aggregated in (i) the null hypothesis significance testing (NHST) framework; (ii) a Bayesian framework. In both frameworks, we investigate the following thesis with the help of computer simulations:

SCT*: Given a series of identical replications of an experiment, the meta-analytical aggregation of their effect sizes will eventually converge on the true effect size.

Under ideal circumstances, scientists (1) have sufficient resources to discern the true effect (i.e., power is large enough); (2) report results independent of the direction of the effect; (3) report results independent of their statistical significance ($p < .05$). While we have reasons to believe that SCT* is true in that utopian case, previous work (Romero, 2016) indicates that SCT* does not hold in the NHST framework when conditions (1)-(3) are relaxed. Even failure of a single condition can lead to heavily biased effect size aggregates.

Our study conducts a similar set of simulations for the Bayesian framework where condition (3) is replaced by an appropriate Bayesian equivalent. That is, results are only reported if the Bayes factor for either hypothesis exceeds a certain threshold. Unlike NHST, the Bayesian framework has the conceptual resources to quantify evidence in favor of the null hypothesis. Hence, effect sizes are less likely to be overstated. Our simulation study shows scenarios under which this conjecture holds (e.g. when researchers explore small effects). In such scenarios, the Bayesian framework yields less misleading effect size estimates and credible/confidence intervals than the NHST framework.

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The extension of second-order logical consequence and a proof-theoretic redundancy theory of truth

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Keywords: higher-order logic, inferentialism, proof-theoretic semantics, truth, redundancy theory

I develop a fully schematic, inferentialist, proof-theoretic account of higher-order logic. The consequence relation characterized is genuinely stronger than the consequence relation of the usual axiomatic or proof-theoretic systems of second-order logic, but still evades the problems of the full model-theoretic account.

Moreover, the account allows for the definition of an indefinitely extensible series of partial truth-predicates. In other words, for any language whatsoever (provided it is formulated somewhere in the hierarchy of higher-order predicate logics), a truth-predicate is definable with purely logical and proof-theoretic means. This suggests that enough "truth" should be available to the inferentialist to do much of the theoretical work that opponents occasionally claim cannot satisfactorily be carried out by inferentialism: inferentialists can provide a 'T' for a JTB-style account of knowledge; if an inferentialist wanted to express that truth is the norm of belief, a strong enough notion of truth is available for her to do so; and so on.

Further, deflationists claim that the truth-predicate does not express a substantive property, but that instead its only role is as a logical device to express otherwise inexpressible generalities. But with the indefinitely extensible series of partial truth-predicates available for free to the inferentialist, deflationism appears as a half-way house: truth-predicates are purely logical, and indeed eliminable since they are logically definable. No ineliminable truth-predicate is required for the expressive purposes the deflationist has in mind. It seems that deflationism in fact leads to a redundancy theory of truth.

Explaining unification in physics internally

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Keywords: Internal Unification, Inductive Metaphysics, Quantum Field Theory.

History of physics not only informs us about instances of unification, but seems to reveal a pattern, a general tendency towards unity. But how do we explain this practice of unification? First, one could argue that physics simply aims at unification for its own sake drawing on proposals that determine unification as the goal of science. Second, one may argue that physics aims at explanation, with explanation *being* (or being supported by) unification. Especially this response can rely on traditional philosophical takes on unification. Third, one may argue that physics pursues a '*unificationist methodology*' based on metaphysical (e.g. unity of nature) or metatheoretic (e.g. 'economy of thought') principles. Here, the reasoning is that physicists employ some *external* principle in terms of an additional metaphysical (or metatheoretical) premise. All three responses explain the unificatory practice by acknowledging either unification itself, or related principles *guiding* scientific research. But what about metaphysically agnostic positions arguing that science aims at empirical adequacy, for example? Are such positions capable of explaining the overall tendency towards more and more unified theories?

My claim is that they can provide an explanation. In paradigmatic attempts at unification physics can be shown to proceed via *modi operandi* that are all rooted in genuine physics methodology, e.g. inductive generalization, eliminating artifacts from a theory (e.g. frame dependence), and expanding the realm of an established theory.

To support my claim I consider historical case studies, and investigate a particular take on quantum gravity (QG) that is able to shed light on the issue of unification by *deducing* Einstein's principle of equivalence within the framework of quantum field theory (QFT) (**?**). Here, unification reveals as a *reduction* of general relativity to QFT. I will argue that it is part of the genuine methodological practice of physics to uncover and expand the explanatory resources of a given theoretical framework as far as possible, and thereby promote *internal* theoretical progress. Unification results as a *by-product* of exploiting methods of genuine physical research.

This internal perspective is interesting for new research programs in metaphysics called *Inductive Metaphysics* (IM). According to IM, metaphysical inquiries should not only meet scientific facts, but *employ* them as (empirical) premises, and proceed in accordance with scientific methodology. Furthermore, IM takes seriously that we should not read off metaphysical commitments from a particular theory alone, but also consider available alternative theories.

However, since the ontological commitments of the respective theories may differ substantially (*cf.* approaches to QG), it seems that we cannot hope for a unique determination of any positive metaphysical inference without ultimately refering to a priori reasoning by help of external principles like simplicity or unification (*cf.* **?**). Otherwise, we might only be able to carve out a 'common ground' as a merely *negative* constraint on metaphysics.

But viewing unification as an internal result of science (not a metaphysical presupposition) opens up the possibility to further constrain inferences in metaphysics *positively*. Although IM is not opposed to a priori reasoning in general, the internal view on unification seems to better meet its general approach than the canonical take on unification.

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Neo-Logicism with(out) Grounding?

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Keywords: Grounding; Neo-Logicism; Abstraction Principles; Epistemology of Mathematics.

According to Neo-logicists (cf Hale&Wright [2001]) a form of logicism can be achieved by taking Hume's Principle ("The number of *F*'s is the number of *G*'s iff the *F*'s and the *G*'s are equinumerous") as an implicit definition of the concept of cardinal number – from which, in a suitable second-order logical system, Peano's Axioms for arithmetic can be derived via what is known as Frege's Theorem.

We first distinguish a number of claims that characterize neo-logicism:

(**EPI) EPISTEMOLOGICAL CLAIM**: HP can provide the concept of finite cardinal to a subject lacking it but empowered with the resources of a full second-order logical language;

(SEM) SEMANTIC CLAIM: HP suffices to stipulate truth-conditions for identity statements between singular numerical terms ("the number of the concept *F*" – "#F"), thus fully characterizing the meaning of such terms;

(**ONT) ONTOLOGICAL CLAIM**: (in an suitable logical system) HP, once instantiated by suitable logical concepts, suffices to prove the existence of countably many objects (finite cardinals), whose identity conditions are given in terms of equinumerosity of concepts;

(EXP) EXPLANATORY CLAIM: HP, as an implicit definitions of the concept of finite cardinal, constitutes an explanation of such concept.

According to Frege, "the aim of proof is […] also to afford us insight into the dependance of truths upon one another" (1884, § 2). One way to understand this is to require that the structure of proofs from logical laws and definitions to arithmetical theorems reflect a metaphysical order of fundamental and derivative truths. As a consequence, HP should not just stipulate that $\#F = \#G$ iff the *F*'s and the *G*'s are equinumerous; it must also explain how it is that the former identity holds *in virtue of* such equinumerosity:

(DEP) DEPENDANCE CLAIM: the order of explanation provided by (instances of) HP should mirror the relations of dependance (fundamentality/derivativeness) among truths.

The exact relationship between (EXP) and (DEP) has been neglected, and little has been done to highlight the relevance of grounding to the philosophy of mathematics. Our aim is to explore how different notions of grounding can be adopted in a neo-logicist framework in order to substantiate (EXP) and (DEP).

To do this, we first rehearse the relevant notions of grounding and dependance.

We then consider the few contributions investigating these issues: Rosen [2010], Schwartzkopff [2011], Donaldson [2016]. We identify a number of limits for these views, among which: (A) they suggest a neo-Aristotelian view of natural numbers as derivative entities which clashes with the neo-logicist platonist picture; (B) Schwartzkopff [2011] defends an *Explanatory HP* (EHP): necessarily, if $\#F = \#G$, the former is identical to the latter because the *F*'s are equinumerous with the *G*'s; but EHP cannot be what underlies the relation between (DEP) and (EXP): HP, as a stipulation, demands no previously available resource for reference to cardinals; but no one lacking the concept of cardinal could understand the antecedent of EHP – analogously to what was rebutted by Wright [1990] against Field [1984].

We then emphasize a major difficulty in applying grounding to HP and abstractive definitions: while the simmetry of logical equivalence is essential to the definitional role of HP, asimmetry is essential in characterizing grounding as a notion of (non-causal) explanation.

Finally, we develop further skepticism against the use of several notions of grounding to substantiate (DEP). We conclude by considering the most promising notion in this lot, i.e. *conceptual grounding* as canvassed by Schnieder [2006], and by exploring whether it could be developed in order to sustain the neo-logicists' needs. In the end, we aim at throwing light on how grounding can be made to serve foundational projects in the philosophy of mathematics.

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Quantitative and qualitative genetics. Issues in contemporary models

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Keywords: Quantitative Genetics, Biometrics, Mendelism, Behavioral Genetics, Multilevel Analysis

During the 19th century, F. Galton and G. Mendel developed quite different views of inheritance and biological variability. These views came to be characterized as two opposing theoretical traditions: a *quantitative* tradition, represented by the biometric approach, and a *qualitative* tradition, advanced by Mendelism. Biometricians conceptualized phenotypic variation in a quantitative way, by focusing on phenotypic traits as related to several hereditary factors, varying continuously over population (e.g., height and IQ). By contrast, Mendelians conceived phenotypic variation in a qualitative way, by focusing on discrete phenotypes (e.g., pea color), causally related to single underlying generative factors.

With the constitution of the Modern Synthesis, the quantitative view has triumphed over the qualitative one. In his popular paper, R. Fisher (1918) denied the validity of the qualitative approach, starting to treat the discrete phenotypic variation as a limiting case—mainly concerning experimental conditions. He proposed a quantitative-additive model, according to which: a) in natural populations, phenotypic traits vary continuously and are influenced by several alleles; b) the inheritance of every single allele is explained by Mendel's laws; c) the effect of every involved allele is *small* and *accumulates* with other genetic effects and with environmental influences.

Nowadays, Fisher's model is still pivotal in genetics research, insofar as it provides a unified explanatory framework based on statistical analyses. For instance, it is at the core of the study of complex traits via genome-wide association studies. However, it should be noticed that, in its original formulation, the model was not designed to involve any ontological commitment. That is, it was only for the sake of simplification that Fisher came to assume quantitative traits as related to the small and additive effect of a countless number of alleles. In spite of this, additivity today represents an important ontological assumption in quantitative genetics: it is not only related to statistical inquiries, but rather it is understood as a biological principle.

We shall take behavioral genetics as a research field in which the quantitative model is adopted to provide a quantitative explanation of the genotype/phenotype relationship (e.g., Plomin et al., 2009). Here, additivity is assumed as a multilevel features of biological systems: alleles' additive effects influence the phenotype passing through proteins and biochemical processes.

We assay whether the multilevel quantitative analysis is suitable to account for the relationship between genotype and human behavioral traits. We then highlight the relevance of non-additive phenomena to understand genetics and development. Indeed, it is worth noticing that the additivity assumption leads quantitative genetics to overlook the importance of notable aspects of complex systems, such as gene-gene and genes-environment interactions.

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Why does formal deductive logic start with the classical Greeks?

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Many ancient peoples studied "logic" in the broad sense of argumentation, but the study of formal deductive validity starts with the classical Greeks alone. For some reason, the only person to invent a study of deductive validity in virtue of form was Aristotle, and all other logicians, everywhere in the world, have had his example to follow.

How can it be that, though all ancient peoples argued, only the classical Greeks developed formal logic?

We contend that formal logic emerged in classical Greece as a result of two crucial factors—one geographical, the other political.

First, unlike other regions in the ancient world, classical Greece had a geography that favored small states, dominated by large urban crowds. The ease of navigating the Mediterranean Sea caused the commercial classes in the Greek cities to grow, and the small size of these states—a consequence of the many mountains and islands of Greece—meant that these same commercial crowds ended up dominating the politics of the classical age. As a result, political questions were settled, not by kings or small groups of nobles, but in mass meetings like the Athenian Assembly. And the mechanics of these meetings put special emphasis on public argumentation.

Second, these same crowds, when called to make political decisions, often behaved irrationally. Such crowds had dominated the Athenian Assembly, but when Athens lost its long war against Sparta, and then followed this loss with the execution of Socrates, a reaction among intellectuals led to the development of formal logic. Philosophers focused increasingly on the difference between rational argumentation and irrational, and this theme, first developed by Plato but later expanded by Aristotle, culminated in the world's first known system of formal deductive logic.

To be sure, the emergence of formal logic required more than a few steps. Plato had insisted that the rhetorical force of an argument was often different from its rational force—a point he had stressed in opposition to the Sophists. And Aristotle's system then depended on a further crucial insight, that the logical force of an argument (in the case of a syllogism) depends on its logical form, as distinct from its rhetorical form. But we believe the ground for these developments was prepared by political experience.

In general, we believe the argumentative politics of the Greeks affected many aspects of classical Greek culture—including mathematics (which plays a substantial role in Aristotle's logic, many of his examples being mathematical). But what was distinctive about Greek mathematics, it may be remembered, was *not* that the Greeks calculated; many ancient peoples calculated. Instead, what set Greek mathematics apart was the emphasis on proof, and proof is a subject that appeals especially to a population that has already been sensitized to argumentation. We believe the Greek relish for logical demonstration, even in mathematics, comes once more from an argumentative environment.

Throughout, we attribute a change in intellectual history to aspects of political history, and we draw our argument from our recent book *If A, Then B: How the World Discovered Logic* (Columbia University Press). Nevertheless, we continue to consider further objections to our view—and to refine our outlook.

The metaphysics of Bohmian dispositions

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Keywords: Bohmian mechanics; quantum mechanics; dispositionalism; wave function; realism.

In this contribution, I assess two different dispositionalist interpretations of the wave function within Bohmian mechanics. Belot (2012), Esfeld *et al.* (2014) and Dorato (2015) consider that the universal wave function represents a single holistic disposition instantiated by the entire universe. These authors reject an alternative interpretation that attributes to each individual particle a disposition to move in a certain way, depending on its own position and the configuration of the rest of the particles. I will refer to the former interpretation as [DIS1] and to the latter as [DIS2].

Both Dorato and Esfeld *et al.* dismiss [DIS2] because they consider that postulating dispositions instantiated by each Bohmian particle amounts to reifying the effective or conditional wave function of the particles but not the universal wave function. However, they argue that it is the universal wave function that is fundamental and should be reified. Although I agree with this caveat, I do not consider that defenders of [DIS2] reify the conditional or effective wave functions of the individual particles. This can be shown by means of a simple illustration. Let us suppose that the universe has only two Bohmian particles *a* and *b* with positions (at *t*) X_a^t and X_b^t, respectively. Let the universal wave function be $\Psi(x_a, x_b, t)$. Now, supporters of [DIS2] attribute to particle *a* (at *t*) the disposition to move in a particular way were particle *b* placed at a position $X \neq X_b^t$. However, this counterfactual information can*not* be derived from *a*'s conditional wave function at *t*, $\varphi_a^t \equiv \Psi(x_a, X_b^t, t)$, since by definition, this is obtained from the universal wave function assuming that *b*'s position is X_b^t.

Dorato (2015) also claims that quantum realism commits to interpreting the wave function as an abstract entity. After rejecting configuration space realism, Dorato reaches this conclusion by arguing that it is a consequence of both nomological realism and dispositionalism that the wave function represents something abstract. According to this author, since the universal wave function represents a disposition of the *set* of all particles, it stands for a property of a mathematical object and therefore, it stands for something causally inactive and abstract. Here, I contend Dorato's conclusion assuming that supporters of [DIS1] can interpret the universal wave function as a disposition of a concrete physical object, namely, the mereological fusion of all particles. I also submit that the dispositions reified by the advocate of [DIS2] are concrete, not abstract. This follows from the fact that these dispositions are attributed to individual particles and, therefore, it can be naturally assumed that each disposition is instantiated where the corresponding particle is located.

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Nomological mapping account: a comparison

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Keywords: Nomological mapping account; causal structure; mechanistic account.

One of the major problems in the philosophy of computing literature is that of explaining exactly what does it mean for a concrete physical system to realize a computation. Specifically, the task is to build a bridge between the mathematical theory of computation and concrete physical systems.

A possible solution to the issue is to construct a direct bridge between the states of the physical system as given by a description of the system and the steps of the computation; once this is done, the modeller can argue that the physical system's behaviour resembles the evolution of the computation and thus implements it. Those accounts, called *simple mapping accounts* (Putnam (1988); Giunti (1997)), have the advantage of being simple and they provide a constructive procedure to assess if a concrete physical systems is realizing a given computation. Nonetheless, they have been extensively criticized (Fresco(2014); Piccinini (2015)) mostly due to the fact that they are too liberal: too many physical systems end up realizing too many computations.

This issue, which falls under the name of *pancomputationalism*, can be partially solved by providing more sophisticated versions of the mapping accounts. An approach in this direction is the one followed by David Chalmers (Chalmers (1994)), who proposes a causal mapping account. Chalmers' idea is that a physical system realizes a computation, when that computation mimics the causal structure of the physical system. Since causality supports counterfactual situations, Chalmers' addition restricts the amount of possible mappings between concrete physical systems and computations, thus partially avoiding pancomputationalism.

Even though Chalmers' account is an improvement on simple mapping accounts, it is argued (Scheutz (2012)) that causal mapping accounts are still too liberal, allowing unwanted realizations; moreover, they rely on obscure and unexplained metaphysical notions such as causal structure. For those reasons, a more rigorous and scientifically grounded account of realization was proposed in Fano et al. (2016): this account, called *nomological mapping account*, is based on the idea that a system realizes a computation only if the computation mimics the behavior of the physical system as described by our best scientific theories.

The aim of this presentation is to present the nomological mapping account, comparing it with previously proposed accounts. The comparison will be made by assessing some general objection moved towards simple mapping accounts and some specific objections moved towards Chalmers' account. The comparison will reveal a close connection between the nomological mapping account and recently proposed (Piccinini (2015)) mechanistic accounts of physical computation. It will turn out that the nomological mapping account avoids most of the objections, while providing insights onto the remaining ones. Finally, the nomological mapping account clarifies some key notions of mechanistic accounts, therefore also improving the latter.

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Contingency, counterfactual history, underdetermination

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Keywords: inevitability thesis; contingency thesis; counterfactual history; underdetermination.

Over the past few years, a heated controversy has concerned the inevitability/contingency of the results of successful science. In a nutshell, advocates of the so-called *inevitability thesis* view our successful theories as unavoidable stages in the development of scientific inquiry. Proponents of the so-called *contingency thesis*, on their part, maintain that history of science might have taken paths leading to alternatives S_1, S_2, etc., to our current science, and that said alternative sciences would include theories significantly different from, but nonetheless as successful as, the ones currently embraced by scientists. (See Soler, Trizio & Pickering 2015 for the state-of-the-art of the controversy.)

In this paper, we explore two intertwined issues related to the inevitability/contingency controversy.

First, we discuss whether and how counterfactual history of science can bear on the controversy. It seems clear that the defense of *counterfactual history* of science is of crucial importance to proponents of the contingency thesis. In fact, claiming that the path followed by (some branch of) science might have been different from the one it actually followed requires one to be able to at least loosely sketch the allegedly possible alternative developments (see Kinzel 2015). However, philosophers of science have devoted little attention to the question of what limitations one must impose on speculations concerning alternative paths of development of scientific inquiry in order for such speculations to generate narratives lending credibility to the contingency thesis. We suggest that having a look at the long-standing debate within history and the social sciences concerning the merits of counterfactuals is a fruitful way to approach the problem. A central insight to be gleaned from the debate on counterfactuals in history, we argue, is the *plausibility requirement*: counterfactual histories are restrained in such a way that they exhibit the right kind of plausibility, construed as consistency of the scenario to which the counterfactual narrative leads with what historians know about the actual world. It is the historians' partial and imperfect knowledge of how the actual world works, and of how historical actors typically behave in it, that allows them to—fallibly—recognize sound counterfactuals. Analogously, we suggest, the plausibility requirement ought to govern the construction of sound counterfactual histories of science. As we point out, counterfactual narratives satisfying the plausibility requirement allow one to at least loosely sketch possible alternatives to (some branch of) our current science. However, for the sake of plausibility, such alternatives have to lie in the close vicinity of our science, thereby providing support only to qualified versions of the contingency thesis.

Secondly, as we emphasize, the inevitability/contingency controversy seems to exhibit a remarkable structural similarity to the problem of the *underdetermination* of theory by the evidence. In an underdetermination scenario, one has to choose from among competing scientific theories based on a body of evidence that does not tell decisively in favor or against any of them. Analogously, one can characterize the inevitability/contingency controversy as the problem of choosing from among a collection of competing possible histories such that (a) it includes actual history, and (b) none of its elements is incompatible, in the relevant sense, with what we know about the world (provided that the competitors satisfy the plausibility requirement). We suggest that, in light of the plausibility requirement discussed above, counterfactual narratives leading to underdetermination-like scenarios need to be short-term ones. Each successive alteration that one introduces in the historical record leads the narrative farther away from the actual past, thereby making its assessment more difficult. For the sake of plausibility, however, the narrative needs to remain sufficiently close to the actual past.

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Epistemic Advantages of Multiversism

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Keywords: Undecidability, Set-Theoretic Multiverse, Naturalism, New Axioms

In a recent paper, [3], Maddy has argued that there is presently no conclusive reason to construe set theory as being concerned with a multiverse rather than with the universe V.

The overall goal of this paper is to provide a careful response to Maddy and, thus, help make a full case for the multiverse view.

First, I will review alternative accounts of the set-theoretic multiverse ([2], [4], [5], [6] and, especially, my [1]) and I will then argue that a multiverse theory may be epistemically more attractive than a universe theory, in particular with reference to requirements that a Maddian naturalist or *arealist* is likely to feel as most prioritary, such as, for instance:

1. *Unification*. Set theory is meant to be a conceptually unified arena where the whole of maths can be carried out. However, a universe theory fails to provide, at least *prima facie*, an adequate representation of the independence phenomenon. On the contrary, a multiverse theory seems more apt to represent current set-theoretic practice.

2. *Elucidation*. A multiverse theory has better prospects to elucidate the conceptual nature of tools, such as *forcing, inner models, ultrafilter constructions*, etc. needed to produce set-theoretic models (along the lines, for instance, of Hamkins' use of a naturalist account of forcing in [2]).

3. *Demise of robust realism*. A naturalist/arealist is interested in *existence* and *truth* claims, only insofar as they serve the purpose of developing mathematics. In particular, the choice of alternative set-theoretic axioms only conforms to *internal* epistemic criteria. A multiverse theory seems to be more adequate to reflect the wealth of available choices and connect them to such criteria of evidence.

I will also briefly touch on how a multiverse theory can fulfil one further goal, that is, that of concretely studying different concepts of set, or set-theoretically relevant concepts (such as that of continuum) and axioms, by studying inter-universe relationships. This is still work in progress.

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Neighborhood semantics for deontic necessity

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Keywords: deontic logic, modal logic, normality

Normative claims and judgments are widespread. These are modal claims which, amongst other things, grant permissions and make demands of us. For instance:

1. (a) You cannot bring coffee to the library.
   (b) You should not break a promise.
   (c) You must pay your taxes.

One important and interesting normative construction is the deontic conditional. Consider, for instance:

2. If you want to get to Harlem, you should take the A train.

This talk will focus on the logic of deontic conditionals. There are three important features of deontic conditionals that we ought to account for in giving a logic for such normative constructions.

The first feature is that deontic conditionals are modal conditionals. That is, the most plausible representation of the underlying form of deontic conditionals like (2) has it that there is a modal operator scoping over the antecedent. Also, one thing to note is that in the ordinary examples discussed in (1), it is very plausible to think that they are in an important sense conditional. For instance, one reading of (1a) is: if you do not wish to get kicked out, you cannot bring coffee to the library.

The second feature is that deontic conditionals have an underlying *ceteris paribus* character. An ordinary utterance of (2) comes with the idea that it applies in normal circumstances. Additionally, deontic force persists even in the face of abnormal cases.

The third feature is that deontic conditionals are context sensitive. By this, I mean that the meaning of the deontic vocabulary involved is context sensitive. When we say that you should take the A train, we mean *should* in a particular sense. For instance, we could mean something like: if you want to get to Harlem, you should, *in light of the aim of getting there as quick as possible*, take the A train.

But there is another, deeper general desideratum that pertains to deontic logic. Norms vary in strength and elsewhere I have argued that there are weak norms and strong norms. This distinction roughly correlates to the distinction in strength between *should/ought* and *must*.

With these motivations in mind, I motivate an approach to deontic modality based on neighborhood (or Scott-Montague) semantics, and I distinguish between local and global notions of deontic necessity. The upshot is that there is no one deontic conditional, for such conditionals are associated with different forms of deontic necessity.

Understanding by algebraic closure

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Keywords: linear factorization, quantifier elimination, uncountable categoricity.

Recent work in philosophy of mathematics emphasizes a concern with understanding in the practice of mathematicians, a concern that goes beyond issues of justification and validity, to issues having to do with explanation and depth. Such issues arise, for example, when a theorem is given a new proof, which may help one see better why the theorem is true, rather than merely justifying the belief that it is true. The new proof is often considered more explanatory or deeper than the old ones. Similar issues also arise when solving problems: some solutions may be more explanatory or deeper than others.

A typical means for mathematical understanding is the operation of algebraic closure. This is an operation that algebraically extends a domain of objects. For example, the complex numbers algebraically extend the real numbers, and the former are said to be the algebraic closure of the latter. The main reason that is usually taken to justify the claim that algebraic closure serves understanding is the satisfaction of solvability conditions over such closures. The fundamental theorem of algebra (henceforth, FTA), i.e., the statement that every equation in one unknown is solvable over the complex numbers, is often thought to bring a kind of simplification that is alleged to improve understanding.

Nevertheless, one worries over the extent of such benefits of algebraic closure, since they do not cover some of the most fundamental cases in the history of mathematics, like Euler's equation. There is also a sense in which what is important about the FTA is not the extension of a domain of objects, but the extension of systems of computations, so that if the FTA improves understanding, this is not due to algebraic closure, but rather to the permanence of computational rules.

Such worries aside, though, one would be interested in finding out whether it is indeed simplification via the FTA that explicates the connection between mathematical understanding and algebraic closure. This talk describes several properties of algebraic closure (linear factorization, quantifier elimination, and uncountable categoricity) and evaluates each with respect to their potential contribution to understanding.

Epistemic injustice and medical diagnosis

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Keywords: Epistemic Injustice, Medical Diagnosis, Pain Description.

In *Epistemic injustice. Power and the ethics of knowing*, Miranda Fricker argues that women are particularly vulnerable to the phenomenon of epistemic injustice: they are not recognized as trustworthy experts more often than men. In this contribution, I will be focusing on a particular aspect of this kind of injustice: the fact that the ill women are more exposed than ill men to the experience of not being heard by doctors or health professionals. More precisely, I attempt to show that epistemic injustice in medical diagnosis might be linked to mechanisms (sometimes unconscious) that make doctors fail to recognize female patients as trustworthy and competent with respect to their illness conditions and to readily incorporate their knowledge into decision-making.

The paper is divided in two parts. In the first part, I present Fricker's notion of epistemic injustice. In my analysis, I pay particular attention to the women' common reports that they feel "silenced", not listened to, not taken seriously. In the second part, I discus some cases of cardiovascular disease as cases of testimonial and hermeneutical injustice and I suggest a way to mitigate this phenomenon. Here is a more detailed layout of my argument. As narrative analysis has shown, female and male patients may share and describe their illness experience very differently. There are, for example, gender-specific differences in the description of chest pain and – according to some research studies – women are more likely than men to be underdiagnosed and under-treated. This happens – I suggest – mostly because female patients' reports are often ignored, sometimes heard but not considered; taken as irrelevant, not sufficiently articulated, or are less understood from health professionals and seen as not corresponding to their expectations.

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Grounding as metaphysical causation in spacetime physics

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Keywords: Grounding, causation, spacetime, background independence.

In recent literature (see, e.g., Wilson, 2017), some strong suggestions have been made that metaphysical grounding and standard ("nomological") causation are in fact two species of the same genus relation. In this talk, I will explore the possible consequences of this view for the interpretation of spacetime theories. In particular, I will focus on the claim that the way spacetime structures "act" on matter is not causal in a standard sense, but instead is better described in terms of metaphysical grounding.

In the first part of the talk, I will consider the claim that general relativity is a peculiar theory because it vindicates some sort of metaphysical "action-reaction" principle. This fact is often taken as a strong clue that, in general relativity, spacetime structures are endowed with causal efficacy. However, the physical picture entailed by general relativity has proved to be extremely resilient to a plain causal interpretation of spatiotemporal properties. In this regard, I will critically review two "anti-causal" arguments put forward in Livanios (2009) and Katzav (2013), and show how both challenges can be defused by shifting the accent from nomological to metaphysical causation.

In the second part of the talk, I will highlight that, if the causal efficacy of spacetime structures has to be understood in metaphysical rather than nomological terms, then also the spacetimes of classical mechanics and special relativity are endowed with causal capacity despite the fact that they do not satisfy the metaphysical action-reaction principle.

In the third part of the talk, I will consider the objection that the proposed framework muddles up an overwhelmingly important concept in spacetime physics, namely, that of background independence. On a naive reading, such a feature is taken to be related to the fulfillment of the action-reaction principle so, if we maintain that spatiotemporal structures can only count as metaphysical causes, then background independence inherits a rather obscure physical meaning. I will reply to this objection by pointing out that the notion of background independence should not be linked to the fulfillment of the action-reaction principle, but instead should be related to the operation of counting possible worlds.

Finally, I will briefly mention that the framework discussed would be of great help in accounting for the emergence of classical spacetime structures from a fundamentally a-spatiotemporal reality, as suggested by some current theoretical attempts to quantize gravity.

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DNA: specific difference maker but not developmental determinant

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Keywords: evelopmental causation, biological specificity, determination

Crick (1958) famously proposed that the relationship between DNA sequence and gene products should be understood in terms of specificity and determination. In particular, Crick clarified that specificity should be thought as a relation of determination: the linear sequence of nucleotides in a sequence of a DNA molecules *determines* the linear sequence of nucleotides in an RNA molecule and the linear sequence of nucleotides in a sequence of an RNA molecule in turn *determines* the linear sequence of amino acids in a protein (its primary structure). Waters (2007) has argued along Crick's lines by refining Woodward's (2003) manipulative analysis of causality. Waters makes two important conceptual distinctions in order to defend his argument: that between difference-making causes that "fully account" and those that only "partially account" for a phenotypic outcome; and that between "specific" and "non-specific" difference-making causes. In this talk I shall argue that, even though DNA is indeed a specific difference maker, it is difficult to make sense of the claim that it is a developmental determinant. First, I shall argue that DNA is not the only causally specific difference maker. Waters' analysis of specificity - like Woodward's (2010) - might be flawed as they take into account only the possibility of manipulating the concentration level of molecular factors instead of their chemical structure. Furthermore, specificity is - as Weber (2006) has convincingly argued – a continuous rather than a dichotomous property. I shall propose a theoretical rationale for defending this thesis that draws inspiration from the model of developmental equivalence of environmental and mutational inputs (Zuckerkandl & Villet 1988). Secondly, I shall argue that it is difficult to make sense of the thesis that DNA sequence fully accounts for a specific phenotypic outcome, i.e., that it is a necessary and sufficient condition for the determination of the structure of mRNA. I shall propose a theoretical rationale for analysing this thesis that draws inspiration from the switch model of development (West-Eberhard 2003). For argumentative purposes, I shall only consider the simpler process of prokaryotic transcription: if it turns out that even the formation of prokaryotic mRNA is not fully accounted for by DNA then, by extrapolation, DNA is not a developmental determinant. Thirdly, I will note that, if the thesis that DNA is not a developmental determinant is endorsed, an apparent tension with the widespread and seemingly unproblematic use of the language of genetic coding in prokaryotic genomics emerges.

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Conceptual analysis in philosophy of science

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Keywords: conceptual analysis, naturalism, empirical philosophy of science

It is uncontroversial that the method of inquiry that philosophers of science use is the method of conceptual analysis. However, one can then legitimately ask what this method comes down to. One further issue concerning this method is the question how it squares with naturalism. In this paper I distinguish two broad construals of the method of conceptual analysis, one belonging to the non-naturalized philosophy of science while the other to the naturalized approach. I then argue that philosophers of science should pay more attention to the naturalized version of conceptual analysis if they aim at constructing a relevant theory of science.

I call the first construal "the philosophical conceptual analysis" and identify its three main characteristics. I call it "philosophical" in order to highlight the attitude that comes with it, i.e. one usually starts with some philosophical question that has a long tradition and one then proceeds to view science through this philosophical prism with its historical baggage. The above mentioned characteristics include the following: (i) analysis of meta-scientific concepts such as theory, model, explanation, representation, law of nature etc. can be pursued independently of the content of sciences; (ii) formalization is an adequate tool in all contexts; (iii) analyzing concepts consists in searching for necessary and sufficient conditions. I criticize all three characteristics. First of all, by ignoring the actual scientific practice one engages in a rational reconstruction of science that is filled with philosophical constructs rather than with actual insights into the workings of science. Second, formalization is indeed a powerful tool but it also has its limits. Inspired by the power of formal logic, many philosophers have used it rather bluntly to the point of absurdity (guilty of this are e.g. Laudan, 1977, Salis, 2016). Third, to understand a concept one does not have to be in a position to lay down the necessary and sufficient conditions.

I call the second construal of the conceptual analysis "the empirical conceptual analysis" to highlight the fact that it builds on two sorts of empirical data. (i) "secondary data" by which I mean using actual scientific findings as a starting point for philosophizing; e.g. cognitive psychology and historical case studies for tracking the development of scientific concepts (Nersessian, 2008). (ii) qualitative and quantitative data which form an integral part of the so called empirical philosophy of science (Wagenknecht, Nersessian & Andersen, 2015). Using these methods, one is in a position of offering a view of science that fits the actual practice; and this cannot be achieved otherwise (e.g. see findings of Dunbar, 2004). These methods thus provide us with a solid ground for constructing more general philosophical theories of science that are potentially of interest to scientists as well as to broader public.

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The Copernican creative shift and the Keplerian Revolution

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Keywords: Copernican Revolution, uniform circular motion, heliocentrism, scientific research programme, creative shift.

This paper focuses on the analysis of the Copernican Revolution presented by Imre Lakatos in *The Methodology of Scientific Research Programmes* (Lakatos, 1978). My claim is that Lakatos' picture of the Copernican Revolution is incorrect according to his own standards and that this has important consequences for the overall methodology of scientific research programmes.

Lakatos considered heliocentrism to represent the hard core of the Copernican research programme, and the uniform circular motion of the celestial spheres as an auxiliary hypothesis within the positive heuristic of the programme. I show that a much more plausible hypothesis for Lakatos would be to consider the uniform circular motion of the celestial spheres as its hard core and heliocentrism as a mere auxiliary hypothesis. Indeed, it is now well-known that the heliocentric hypothesis had already been proposed in the Antiquity within what Lakatos calls the 'Pythagorean-Platonic research programme' (Africa, 1961), whereas nobody until Kepler had ever tried to question the principle of (uniform) circular motion of the heavenly bodies (Hanson, 1961; Dreyer, 1953). Therefore, I suggest that according to Lakatos' methodology it would be better to consider the Copernican work a "creative shift" (*cf.* Lakatos, 1978) of the Platonic-Pythagorean research programme, and to use the expression of "Keplerian Revolution" (Hanson, 1961) to indicate the superseding of the Keplerian astronomical research programme over the Copernican-Pythagorean-Platonic one. I finally argue that, according to this interpretation, the concept of 'creative shift' has a far greater importance than Lakatos thought and that its correct understanding gives rise to a significantly different picture of the methodology of scientific research programmes.

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Bad Company: The Less I Know The Better (?)

Luca Zanetti

The paper is about the Bad Company problem in the philosophy of logic and mathematics, and various morals that can be drawn from it. Bad Company is, in the first place, an objection against the neo-logicist program in the philosophy of mathematics. Neo-logicists contend that a priori knowledge of the fundamental truths of arithmetic can be attained by the stipulation of Hume's Principle, namely

$$\forall F, G \text{ (the number of } F = \text{the number of } G \leftrightarrow F \text{ 1-1 } G$$

and deriving from it the axioms of Peano Arithmetic in second-order logic. Neo-logicists claim that Hume's Principle plays the role of an implicit definition of the concept of number. But there are many principles of the same form which are inconsistent or pairwise unsatisfiable. Therefore, Hume's Principle would lack the suitable form to be a good implicit definition.

Although this objection is particularly vivid against neo-logicism, it applies also to other foundationalist views that resort to implicit definitions. Philip Ebert and Stewart Shapiro mention the case of mathematical structuralism. At the same time, Prior's connective Tonk can be considered as a case of 'bad' connective. Finally, Hannes Leitgeb points out that the unrestricted truth-schema is a case of Bad Company as well.

These four cases are rarely considered altogether. In the first part of this contribution we shall argue that they are actually alike. The second part is devoted to some comparative remarks. In particular, Richard Heck showed that the consistency of neo-logicist principles is not effectively decidable. Recently, Crispin Wright has argued that the stipulation of these principles is nonetheless knowledge-conferring, at least in 'good' cases, absent any reason to think that the principle in question is not acceptable. However, this view seems to be at questionable in other cases of Bad Company.

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