BOOK OF ABSTRACTS

The Inaugural Conference of the East European Network for Philosophy of Science

EENPS 2016

New Bulgarian University
Sofia, 24 – 26 June 2016

CONTENTS

EENPS 2016: PROGRAM

Venue: New Bulgarian University (Building 1); 21 Montevideo Str., 1618 Sofia

Friday, June 24		
8.30-9.30	***Registration*** (room 407)	
9.30-10.00	***Opening*** (room 409)	
10.00-11.00	***Invited talk*** (room 409) **Stephan Hartmann**: Assessing Scientific Theories Chair: Lilia Gurova	
11.00-11.30	***Coffee break*** (room 407)	
	A: General Philosophy of Science (room 409) Chair: Borut Trpin	**C: Philosophy of Cognitive and Behavioral Sciences** (room 405) Chair: Nina Atanasova
11.30-12.00	Martin Zach: Science and Scientism	Anita Leirfall: On the discovery of the concept of space
12.00-12.30	Bianca Savu: Modalities and Philosophy of Science: What if Essences and Ground Met Structures?	Athanassios Raftopoulos: Do Pre-cueing Effects Entail the Cognitive Penetrability of Early Vision?
12.30-14.30	***Lunch break***	
	A: General Philosophy of Science (room 409) Chair: Martin Zach	**B: Philosophy of Natural Sciences** (room 405) Chair: Özlem Yilmaz
14.30-15.00	Matthew Baxendale: What's Wrong with the Principle of Sufficient Explanation? Marathon-Running and Dancing Bees	Marius Backmann: In Defence of Randomised Clinical Trials
15.00-15.30	Anton Donchev: Advantages of the Probabilistic Interpretation of the Inference to the Best Explanation	Kamuran Osmanoglu: The Biological Reality of Race does not Underwrite the Social Reality of Race: A Response to Spencer
15.30-16.00	***Coffee break*** (room 407)	
	B: Philosophy of Natural Sciences (room 409) Chair: Luca Sciortino	**E: Historical and Social Studies in Philosophy of Science** (room 405) Chair: Andrew McFarland
16.00-16.30	Vassilis Livanios: Dispositional Essentialism and the Principle of Least Action: Friends or Foes?	Matjaž Vesel: Galileo Galilei on the Bright Side of the Moon: »Necessary Demonstrations« and the Second Earth
16.30-17.00	Claudio Calosi: Quantum Mechanical Monism	Vassil Vidinsky: The Paradigm of Contingency - A Historical Typology
17.30-19.00	***Reception*** (Restaurant "Artes")	

Saturday, June 25		
10.00-11.00	***Invited talk*** (room 409) **Stathis Psillos**: From the Evidence of History to the History of Evidence: Re-thinking the Pessimistic Induction Chair: Daniel Kostić	
11.00-11.30	***Coffee break*** (room 407)	
	B: Philosophy of Natural Sciences (room 409) Chair: Marius Backmann	**E: Historical and Social Studies in Philosophy of Science** (room 405) Chair: Matjaž Vesel
11.30-12.00	Özlem Yilmaz: Causation and Explanation in Phenotype Research	Alexandra Yakovleva: Transformations of Social Role of Science in the Knowledge Society
12.00-12.30	Stefan Petkov: Explanatory Unification and Natural Selection Explanations	Andrew McFarland: What is a Future of Value?
12.30-14.30	***Lunch break***	

	B: Philosophy of Natural Sciences (room 409) Chair: Claudio Calosi	**D: Philosophy of Social Sciences** (room 405) Chair: Alexandra Yakovleva
14.30-15.00	Vasiliki Nassiopoulou: A Process Account of Internalizing Laws as a Way to Recover Governance	Natalia Drozd: The Philosophy of Consumerism: Marketing Through the Prism of Philosophy
15.00-15.30	Luca Sciortino: Two Styles in Mathematical Thinking	Yuriy Dyachenko: Philosophical Foundations of Human Resource Development Methodology
15.30-16.00	***Coffee break*** (room 407)	
16.00-17.30	***Symposium***: Explanation and Understanding in Science (room 409) (Organizers: Lilia Gurova, Daniel Kostić, Richard David-Rus, Andreea Esanu, Lukáš Bielik)	
17.30-19.00	***EENPS members' meeting*** (room 409)	

Sunday, June 26		
10.00-11.00	***Invited talk*** (room 409) **Roman Frigg**: The Turn of the Valve: How Models Represent Chair: Richard David-Rus	
11.00-11.30	***Coffee break*** (room 407)	
	A: General Philosophy of Science (room 409) Chair: Anton Donchev	**C: Philosophy of Cognitive and Behavioral Sciences** (room 405) Chair: Anita Leirfall
11.30-12.00	Duško Prelević: Hempel's Dilemma and Research Programmes	Mario Günther: Learning Conditional Information from Stalnaker Conditionals
12.00-12.30	Demetris Portides: How Idealization and Abstraction Could Be Distinguished	Mila Marinova: Numbers within our Grasp
12.30-14.30	***Lunch break***	
	A: General Philosophy of Science (room 409) Chair: Duško Prelević	**C: Philosophy of Cognitive and Behavioral Sciences** (room 405) Chair: Mila Marinova
14.30-15.00	Íñigo Ongay de Felipe: The Principle of Parsimony and How August Weismann Used It	Nina Atanasova: Convergent Perspectivism: A Model of Integration in Neuroscience
15.00-15.30	João Pinheiro: A Systems-Theoretical Approach to Evolutionary Arguments for and against the Reliability of Cognitive Mechanisms Producers of Beliefs	Alejandro Rosas Lopez: A Mechanistic Model of the Authority and Inescapability of Moral Norms
15.30-16.00	***Closing*** (room 409)	

INVITED SESSIONS

Assessing Scientific Theories

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Scientific theories are usually assessed in the light of their empirical consequences. But how shall one proceed if a theory, such as String Theory, has no empirical consequences (yet)? Are such theories scientific at all? In this context, several physicists made what has been called a no-alternatives argument. It goes as follows:

P1: A theory T satisfies a number of desirable conditions.

P2: Despite a lot of effort, the scientific community did not succeed in finding an alternative to T that also satisfies these conditions.

(Hence,) C: It is now more probably that T is empirically adequate.

It is the goal of this talk to analyze this argument structure in the framework of Bayesian confirmation theory and to ask under which conditions no-alternatives arguments work.

The talk is based on the paper R. Dawid, S. Hartmann, and J. Sprenger: The No Alternatives Argument, The British Journal for the Philosophy of Science. 66(1): 213-234 (2015). URL: http://philsci-archive.pitt.edu/9588/

From the Evidence of History to the History of Evidence: Re-thinking the Pessimistic Induction

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The past record of scientific theories has been used to undermine the credentials of scientific realism as a philosophical view about science, that is as a view that takes science to be (successfully) in the truth-business. In this talk I will re-assess the debate about the historical evidence against scientific realism. I will argue that we should make a distinction between the evidence of history and the history of evidence and that in trying to assess the epistemic credentials of scientific theories (past or present) we should primarily be looking into the history of evidence that supports them and only secondarily at the evidence of history of science. In trying to motivate this claim, I will take issue with some key recent arguments about the conclusions that have to be drawn for current science from looking at the historical record. I will also discuss Einstein's account of the anomalous perihelion of Mercury and the evidence that supported it.

The Turn of the Valve: How Models Represent

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Scientific models are representations. Building on Goodman and Elgin's theory of pictorial representation we analyse what this claim involves by providing a general definition of what makes something a scientific model, and formulating a novel account of how they represent. We call the result the DEKI account of representation, which offers a complex kind of representation involving an interplay of, denotation, exemplification, keying up of properties, and imputation. Throughout we focus on material models, and we illustrate our claims with the Phillips-Newlyn machine. In the conclusion we suggest that, mutatis mutandis, the DEKI account can be carried over to other kinds of models, notably fictional and mathematical models.

CONTRIBUTED ABSTRACTS

A. GENERAL PHILOSOPHY OF SCIENCE

Science and Scientism

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The topic of scientism is highly controversial, yet one of great importance. It is but the opposite of a narrowly defined academic dispute. Rather, it brings together scientists from different disciplines (e.g. Peter Atkins, Jerry Coyne, Steven Pinker) on the one hand, and philosophers and other scholars from the humanities (e.g. John Dupre, Tom Sorell, Susan Haack, Massimo Pigliucci) on the other hand. Moreover, scientism has also been discussed over the pages of newspapers and other media (e.g. New York Times, Irish Times, The New Atlantis, New Republic), thus it seems to have broader appeal and social significance. Scientism is originally proposed as a pejorative term towards those who would claim that science and its methods provide the only way of gaining knowledge. That is one way to define scientism, although there are others since there is not one unique definition of scientism. However, there seems to be a shared core view in the lines of: „Scientism is a matter of putting too high a value on natural science in comparison with other branches of learning or culture (Sorell 1994)." I identify two main sources of opposition to scientism, namely from religious people and theologians, and from various scholars in the humanities.

My aim is *to analyze the problem of scientism*, i.e. what it is about, *and to argue, contra e.g. Pigliucci, Haack and Sorell, that scientism properly construed is to be accepted*. As for the analysis, there are 3 related lines of inquiry: (i) what is science? (ii) What science presupposes? (iii) What are the limits of science? I will discuss of all these.

What is science? One way to answer that question is to say that science is an inquiry using the scientific method. I will briefly discuss, how it came to be that philosophers of science have shown that there is no single scientific method (be it inductivism, deductivism or such), rather, it is a loose cluster of variously related and/or unrelated methods. The dispute between proponents and adversaries of scientism is over the scope of the definition of science. While proponents tend to broaden the definition in such a way as to claim that plumbing (Sokal and Bricmont 1998; Coyne 2015) or hunting (Sagan 1996) is a sort of science, the adversaries oppose that (Pigliucci 2015). I will argue that the term "scientist" properly applies only to the researches within scientific community (i.e. has a social meaning), but science broadly construed indeed meaningfully encompasses many aspects of our everyday life.

What science presupposes? The adversaries of scientism (e.g. Pigliucci 2008) often claim that science presupposes something while citing Daniel Dennett: "there is no such thing as philosophy-free science; there is only science whose philosophical baggage is taken on board without examination". I will argue that, contra wide-spread opinion, science does not presuppose anything and I will briefly sketch how it can be so. I will also show that the positions of the so called methodological naturalism (espoused by e.g. Pigliucci, Ruse, Pennock, Gould) and metaphysical naturalism are not a priori (in a similar fashion already done by Fishman and Boudry 2013).

What are the limits of science? Proponents of scientism deny the existence of any such limits based on the history of science (Coyne 2015), while adversaries list several things which they judge to be unsolvable by science (e.g. the nature of consciousness). I will argue that any a priori limits on science are to be dismissed in the face of a bad history of such attempts, and that we have good reasons to be optimistic about the prospects of seemingly unsolvable problems that the contemporary science faces.

Given the analysis of those three lines of inquiry sketched above, I will defend the conclusion that science broadly defined is indeed the only "way of knowing", that is, the only way to acquire knowledge about the world. Furthermore, science does not a priori presuppose anything and there is no reason to hold the view that there are real limits to science. Scientism is therefore not only a reasonable position to hold but a position we should welcome. One of the consequences which might be hard to swallow for many is that art, religion and indeed some forms of philosophy do not give us genuine knowledge about the world.

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Modalities and Philosophy of Science: What if Essences and Ground met Structures?

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"Every thing must go, 'cause all there is is structure." A part of the literature in philosophy of science in the last three decades can be taken as gravitating around the statement above— scientific realists who saw in structural realism a savior from the no-miracles argument and pessimistic meta-induction, structural realists who needed more than an epistemic account on what we actually get to know via our scientific theories. Finally, ontic structuralists whose main challenge is to defend their core-statements against criticism. We shall focus on the last part, and propose a discussion on ontic structural realism (OSR), which is taken to be commitment to the idea that the world has a modal structure, which is expressed at the mathematical level of our best theories. Modality is to be understood here in a nomological sense, and one fact is that this modality is specified as being an objective one— independent of our epistemic and doxastic states at some certain time. Berenstain & Ladyman give a detailed account on OSR, in which they pay attention to modally expressible features such as laws, causation, powers or equilibrium and conclude that the type of modality that best suits OSR is natural necessity, hence the nomological view on modality. Stathis Psillos' positive critique to ontic structuralism questions the means by which the view can account for both an abstract and concrete dimension— abstract as long as we are concerned with structures seen as being what Putnam calls "the furniture of the world", yet concrete enough to be able to be ascribed to physical systems.

Our proposal is to adopt a metaphysical framework having a logical background that addresses a similar issue— that is a theory of ground filtered through the lenses of an account on the metaphysical problem of essence. We shall exhibit the similarities that modal logic and what we might call the abstract-concrete agreement for ontic structuralism present when faced with problems of legitimately accounting for essences and structures, respectively. Kit Fine's logic accounting for the concept of 'essence' presents the lambda-abstraction mechanism, which breaks down the ontological dependence between things and the properties they display. We consider that the procedure by which the cloud of properties is formed (starting from concrete objects, and the steps that allow it to play the role of an abstract entity that can be instantiated by physical, concrete objects) is similar, at a certain degree, to scientific practice when providing a metaphysical picture on a theoretical point of view. Grounds, viewed as counterparts for the logical account of essence, are customarily associated with the ultimate form of explanation and our proposal is to adopt it as a meta-framework, that is not a framework for our scientific theories, but one that includes and gives the pattern for the philosophical approaches to those theories. Relating theories of ground and realism is no novelty (see Kit Fine's article "The Question of Realism"), nor is associating grounding theories with structural realism (Dasgupta's articles on this topic, for example). Our account has a double aim then: on the one hand, for the philosophy of logic and the metaphysics of modality, our proposal is an application and integration of Fine's essentialist logic and theory of ground. On the other hand, regarding philosophy of science, it strengthens ontic structuralism by enabling it to exhibit both causal and abstract powers.

What's Wrong with the Principle of Sufficient Explanation? Marathon-Running and Dancing Bees

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The principle of sufficient explanation (PSE) states that in order to explain 'complex' or 'macro' phenomena', it is at least sufficient to make reference to phenomena at a more 'basic' or 'micro' level. It is a principle that I have developed in order to capture the primary epistemological commitment of the layer cake model of the world (LCM), which is itself a specific articulation of the levels of organisation concept. A level of organisation concept, such as the LCM, provides an ontological and epistemological framework within which scientific inquiry might take place. A resurgence in interest in the levels of organisation concept has developed in response to scientific inquiries into increasingly complex systems ((Brooks, 2016; Eronen 2015; Potochnik and McGill 2012). Not only is the LCM a prominent account of levels of organisation, the commitments of the LCM remain prevalent in philosophical accounts of the relationship between phenomena in the world and how best to investigate them. However, the LCM is ill-equipped to accommodate contemporary scientific inquiries into complex systems, for example in climate science, systems biology, cancer research, and modern epigenetics. Thus, showing the PSE to be false constitutes a significant step towards developing a philosophical framework that engages with, and may enhance the understanding of, the increasingly complex phenomena into which science inquires.

In this paper I argue that the PSE is false. I do so by firstly articulating the sense of 'sufficiency' and 'explanation' involved in the PSE itself. I present a distinction between *local* and *global* sufficiency. Local sufficiency presents the principle as holding in *some specific* contexts, whereas global sufficiency applies the principle in all contexts. In order to explicate the sense of explanation involved in the PSE I introduction a novel, multi-level analysis of what it means to provide an explanation. There are, I contend, three levels of analysis at which explanations can be articulated. A pre-theoretic sense of explanation: communicating, informing, or eliciting a flicker of understanding in others about the explanandum (level one: explanation). The articulation and fulfilment of a set of conditions that comprise a philosophical account of explanation (level two: full explanation). Finally, the comparison of different fully articulated explanations on the basis of epistemic or super-empirical virtues (level three: good explanation). The PSE merely offers an attempt to specify a level one explanation. In other words, the PSE is not itself an account of explanation (level two), nor a standard for good explanation (level three). Thus showing the PSE to be false at level one, demonstrates its inability to aid in the explanation of complex phenomena, even with a minimal sense of the concept of explanation in place.

I then provide an argument against the PSE, the main premise of which contends that there are complex or macro phenomena for which it is insufficient (in a global sense) to make reference to a phenomenon at a more basic or micro level, in order to provide an explanation (at level one of my analysis). I substantiate this key premise with an example from a progressive research programme – the explanation of the diversification of the behaviour of foraging honey bees relative to the amount of nectar currently stored in the hive. Foraging honey bees affect the recruitment of either more forager bees or food-storage bees via their intricate 'waggle' and 'trembling' dances. How, then, do these foraging bees *know* when to initiate recruitment of bees into different roles in order to achieve the optimal balance of foraging for nectar and the processing capacity of the hive? Thomas Seeley, (1995) hypothesised that the bees adjusted their various performances based on how *difficult* it was to unload their

recently foraged nectar; that is, how *long* it took them to find a food-storage bee to unload their nectar. This theory, the unloading difficulty hypothesis, postulates that the forager bees do not, in fact, have access to the levels of nectar in the hive and do not adjust their behaviour on this basis. Rather, the diversification of their behaviour was the result of the placement of the bees in a highly organised structure of dynamic feedback systems. Through a series of experiments Seeley (Seeley 1989; Seeley and Tovey, 1994) established the theory as highly corroborated. I argue that Seeley's work on honey bees provides an excellent example to support the key premise in my argument against the PSE. Specifically it provides an example of the insufficiency of explaining the complex behaviour of the hive in terms of the constituent bees – the structural organisation and functional roles, and dynamic properties (such as 'waiting time') of the hive are essential to the explanation.

Due to the global sufficiency requirement of the PSE, this one well-specified example is enough to render the principle false. Establishing this conclusion not only means that the road towards a pluralistic account of levels of organisation is clear of debris, it carefully illustrates the sorts of complex systems (and their features) that such an account will ultimately have to accommodate.

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Advantages of the Probabilistic Interpretation of the Inference to the Best Explanation

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Inference to the best explanation (IBE) is widely used both in science and in everyday life (Lipton 1991; Psillos, 1999). Its logical structure is usually described in the following way: given evidence e and a set of competing hypotheses $h_1, \ldots, h_n$ explaining e, we are entitled to infer that h_i is closer to the truth than any of the

other hypotheses, if it best explains e, and if its explanation is good enough (for the "good enough" requirement see Lipton, 1993, 2001).

The general agreement is that IBE is ampliative, i.e. it broadens conceptual knowledge beyond what is set in the premises, and that it is non-monotonic, i.e. certain conclusions which cannot be inferred from the full set of premises may be inferred from a subset of it (Douven, 2011). What has not been unanimously agreed upon, however, is whether IBE warrants *degrees of belief* or *full belief*. In other words, the question remains open whether one should believe that h_i is true, "accepting" it as such on the basis of it being the best explanation of e, or infer that h_i is perhaps just closer to the truth than the other hypotheses, and so one should believe in its truth only partially.

The above question is crucial to the debate over the compatibility of IBE with Bayesian confirmation. If indeed IBE does not warrant degrees of belief, it would be very hard to show how it can be compatible with a probabilistic theory of confirmation such as the Bayesian one. Unsurprisingly, the full belief view, which requires the "acceptance" of the best explanatory hypothesis, has been mainly endorsed in defence of positions skeptical of a Bayesian - IBE compatibility (Salmon, 2001; Psillos, 2007).

My aim is to show that, as far as scientific practice is concerned, IBE belongs to the epistemological domain of degrees of belief and not to that of full belief. I argue that IBE would be incompatible with real scientific practice if it was the other way around. I employ Cox's theorem to defend the claim that, if IBE indeed deals with degrees of belief, then IBE is explicable in probabilistic terms. To further support my point, I present several examples from physics and astronomy in which scientists used IBE, and I show that their inferences are best interpreted probabilistically. Based on the proposed arguments and examples, I conclude that the explication of IBE in probabilistic terms better accounts for its uses in real scientific practice, when compared to the full belief interpretation.

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Hempel's Dilemma and Research Programmes

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It is argued in this paper that understanding physicalism as a research programme is the best way of dealing with the so-called "Hempel's Dilemma". In what follows, I will sketch briefly the dilemma, compare my own solution to other solutions that might be found in the literature, and try to indicate the directions in which the idea that physicalism is a research programme might be pursued.

According to Carl Hempel (1980: 195), when saying that physics is capable of explaining everything in the universe, physicalists should decide whether they appeal to the physics of the day or to a future physical theory. If they take the first horn of the dilemma, then probably physicalism turns out false, since we can reasonably expect that contemporary physics will undergo further changes. On the other hand, if physicalists take the second horn, and claim that an ideal, completed physical theory will explain everything in the universe, then all bets are off, for we do not know how the future physical theory will look like. The upshot of Hempel's Dilemma is that physicalism is either false, or that it will be false, or that it is a trivial and contentless doctrine.

One can react to Hempel's Dilemma in three different ways: One can defend currentism, defend futurism, or try to avoid the dilemma by claiming that physicalism is not a thesis that might be trivial or empty, but something else.

Currentism is most prominently defended by Andrew Melnyk (1997: 624), who is even ready to admit that current physics is likely false, but nonetheless better than the alternatives. The main problem with this solution consists in the fact that current physics is not just likely false, but it is *strictly speaking* false, since current physics is inconsistent (see, for example, Greene 2004, 333–335, for more details), and that accepting inconsistent theories as a final explanation of the universe does not seem a rational solution (Wilson 2006: 64–66).

As for futurism, some futurists hold that not any future physical theory would be acceptable to physicalists, but only those that do not postulate non-physical entities at the fundamental level of reality. The main critique of this view is that it postulates *in advance* that it is impossible for non-physical entities (such as phenomenal consciousness) to be included in the fundamental level, which is not in accordance with common scientific practice, since scientists do not postulate in advance the nature of the fundamental level, but rather discovering it empirically (see Dowell 2006: 44–45).

Futurists, like Janice Dowell (2006), think that physicalists should be satisfied even if it turns out that the future completed physical theory discovers that non-physical entities belong to the fundamental level, because, after all, physicalism is a view that physics can explain everything in the universe in a non-mysterious way. However, Dowell's solution to Hempel's Dilemma diverges from the standard classifications in the history of philosophy. For example, it might be argued that Descartes himself, who endorsed interactionist dualism, did not appeal to mysterious explanations in explaining the interaction between body and mind. He rather proposed (or at least Cartesians believed so) a solution of the mind-body problem *within his own physical theory*, without violating the principle that physics is causally closed. As it was stressed by Woolhouse (1985) and Papineau (2001: 14–15), this was probably achieved by understanding conservation laws in terms of the "quantity of motion" (mass times speed), which was considered to be a *nondirectional* (scalar) quantity. By this maneuver, the causal closure of physics was reconciled with mental causation, since there is a sense in which mind can alter the direction of body's motion leaving the total amount of the quantity of motion intact. If so, then Descartes was a philosopher

who defended (or could have defended) interactionist dualism without appealing to mysterious explanations, which would be enough to classify him, according to Dowell's proposed criteria, as a physicalist, which is rather implausible.

Given that philosophers like Descartes tried to reconcile their ontology with their preferred physical theories, the "attitudinal approach", according to which physicalism is not a thesis that might be trivial or empty, but rather an attitude like "I hereby swear to go in my ontology everywhere and only where physics leads me." (Ney 2008: 11; see also van Fraassen 2002), does not match well with the standard classifications in the history of philosophy either.

I think that the problems with currentism, futurism, and the attitudinal approach can be avoided once we understand physicalism as a research programme in Imre Lakatos' sense (Lakatos 1978). Although any hard-core contains certain propositions, the research programmes *themselves* are not a set of theses that might be true, false, trivial, or empty. Research programmes, among other things, rely upon a *decision* of their proponents to protect the hard-core of the programme (negative heuristic) as much as possible by redirecting potential counterevidence to auxiliary hypotheses, and so on. The "physical" in the hard-core of physicalism is best to understand in the manner in which the proponents of the so-called *via negativa* approach understand this term (as "non-mental", and the like; see Montero 1999), while positive aspects of the physical are expressed in "positive heuristics", in which competing models and theories are constructed.

Given that research programmes connect past, present and future philosophical or scientific activities, physicalists need not decide between current and future physical theories, as it has been required by Hempel's Dilemma.

Further issues to be addressed could be: How understanding physicalism as a research programme affects the debate on the nature of philosophical, scientistic and empirical stances (see, for example, Ladyman 2011; van Fraassen 2011), and the ontological commitments on which they are based.

Literature:

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How Idealization and Abstraction Could be Distinguished

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Broadly speaking, there are two schools of thought on how to distinguish between idealization and abstraction. Depending on the language favored by each author, the first one regards idealizations as particular forms of abstraction or abstractions as particular forms of idealization. The second one sees two distinct thought processes, or cognitive acts, operating in model-building: one strictly associated with abstraction and the other strictly associated with idealization. In this paper I argue that attempts that fall within the second school of thought fail to provide an adequate distinction. I further argue that both idealization and abstraction are particular forms of a cognitive process of *selective attention*.

Most attempts to search for a distinction between abstraction and idealization in scientific modeling have relied on a particular construal of abstraction. Abstraction is usually understood to be synonymous to “omission” of features of the target system. In this sense it is understood as a thought process that leads to the subtraction or removal of all irrelevant (or insignificant, or unwanted) features of the target system for the particular purpose the model is meant for. Such an interpretation of abstraction has led several philosophers to the conclusion that it is a distinct thought process from that of idealization, since idealization seems to be the cognitive act by which the actual characteristics of a particular feature of the target are changed or rearranged. So, one could claim that the cognitive act of removing a feature is qualitatively different from the act of changing its characteristics. This would be a reasonable conclusion to draw from the preceding premises. Furthermore, by drawing this conclusion we are able to reach some seemingly important qualifications about the characteristics of scientific models.

However, as I argue in this paper, there is a flaw in such arguments. The flaw relates to how abstraction is construed. In the first part of the paper I draw attention to the several problems such an account of abstraction and idealization faces. Let me in this summary explicate one such problem. If abstraction consists in the cognitive act of subtracting elements of the target system, it would mean that scientists know exactly what has been subtracted from the model description and thus know exactly what must be added back into the model in order to turn it into a more realistic description of its target. This idea, most of the time, conflicts with actual modeling in science, where a significant amount of effort is put into *discovering* what should be added back into the model. In other words, the practice of science testifies that scientists, more often than not, operate without such knowledge. So one is justified in questioning whether scientists actually know what they are subtracting in the first case. Since it is hard to visualize how a modeler can abstract, in the sense of subtracting, without knowing what they are

subtracting in the first case, one is justified in questioning whether a process of abstraction (in this sense) is at work in model-building.

In the second part of the paper I suggest a different interpretation of abstraction. I construe abstraction as *selective attention*, i.e. the thought process that allows a scientist to focus attention only on those features of the target that are considered relevant or useful to the task at hand. Construing abstraction in this manner does not mean that one subtracts from the totality of features the target system has until they are left with what is useful for the model's purpose. Rather, it means that one attends only to the necessary features for constructing a particular model. Abstraction is the thought process that allows one to focus on the features of interest, without removing anything from the totality of features. I provide arguments why this sense of abstraction is more suitable for shedding light on model-building.

Furthermore, if abstraction is the result of the cognitive act of selective attention, then it does not differ from idealization by being a different thought process. In fact, idealization can also be construed as being the result of selective attention. The obvious difference between abstraction and idealization is therefore to be found elsewhere. In an effort to explicate the difference between the two, I suggest that selective attention operates at two levels. The first is the level of extracting and isolating the features a modeler wishes to focus on, and the second is the level of focusing on particular qualities or quantities of these features. The first is what we commonly refer to as abstraction, the second is idealization. I urge that this is the least problematic way to distinguish between the two.

Finally, I attend to the question whether distinguishing between abstraction and idealization is adequate for explicating the kinds of simplifications that are encountered in scientific models. My answer is no. We need a third kind of simplification to make sense of modeling practices in modern science. I call this kind *decomposition*, and I argue that decomposition can be construed as a third level of selective attention, that of attending to particular ways by which clusters of component features operate together to produce a particular behavior of the target. Armed with these three levels of selective attention one is able to explicate significant aspects of model-building in modern science.

The Principle of Parsimony and How August Weismann Used It

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This contributed paper shall explore in close detail the notion of simplicity as an epistemic virtue in scientific reasoning by considering one use of the principle of parsimony taken from the History and Philosophy of Evolutionary Biology.

First of all, the author addresses a variety of philosophical accounts of parsimony as an epistemological virtue to be used when deciding among competing scientific theories. In so doing, the doctrines of Occam, Newton, John Stuart Mill and Ernst Mach will be considered in some lenght with a view to analyzing the extent to which these arguments fail inevitably to offer a coherent unique framework for determining the reasons (whether epistemic, aesthetic, pragmatic or otherwise) why scientists should allegedly abide by simplicity in evaluating contrasting

theories or hypothesis. Also, the point will be made that parsimony does not represent a clear attribute of scientific theories and statements as a variety of conceptions of what parsimony really means can be identified in the History and Philosophy of empirical sciences.

Secondly, the paper will scrutinize a particular example of the principle of parsimony at work. The author shall consider the structure of August Weismann´s arguments for the all-sufficiency of the principle of Natural Selection (NS). There has been a long lasting agreement among historians of Biology and Philosophers of Science alike that the work of Weismann signals that theories involving the heredity of acquired traits are flawed as that type of inheritance is not possible in Evolution.

While much debate has recently arisen in the domain of Evolutionary developmental biology challenging the Weismann barrier principle, both the proponents of the "Extended Synthesis" and the neodarwinian orthodoxy seem to coincide in that Weismann´s argument concludes that NS precludes any other principle regulating the transmission of traits in Evolution. This is a mistake as my paper will prove.

To show what is wrong to that consensus I will start off by considering Weismann´s mode of argumentation in his *Essays upon Heredity* (1889) as well as in his *Germ- Plasm. A Theory of Heredity* (1893). The analysis shall contend that the arguments on NS contained in these works are manifold and should be carefully distinguished. While on the one hand, Weismann contends that the heredity of acquired traits lacks empirical support and so is not a well-established principle on an evidential basis, another argument is independently (and somewhat contradictorily) advanced indicating that the Lamarckian principles of heredity are just conceptually unclear and therefore fail to bear any empirical content at all. I will argue that whereas the later argument, if true, would count as a strong philosophical critique of the relevance of the hypothesis of the heredity of acquired traits, the former, even if adequate, does not exclude the Lamarckian model of inheritance so long as new evidence might always arise to support it. Finally, I will delve into the pivotal role of parsimony within both arguments against Lamarckian inheritance to show that there´s a crucial role to be assigned to this epistemological concept in Weissman´s (second) argument.

A Systems-theoretical Approach to Evolutionary Arguments For and Against the Reliability of Cognitive Mechanisms Producers of Beliefs

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While "taking Darwin seriously", *pace* Ruse (1986), we, the naturalists, are bound to analyse features of cognition, reason and epistemology through the lenses of the evolutionary biologist. As an example, Lorenz (1941) has famously described reason's aprioristic features as being phylogenetically *a posteriori*. Indeed, the rationale behind his essay is simple and useful as an heuristic for naturalistic epistemologies in general: we should recognize that a main feature of our World as defined by any given Evolutionary Theory will be that cognitive mechanisms are the product of fairly contingent evolutionary processes.

Here enters Darwin's "horrid doubt" (1881) after more than a century passed no less pressing a worry and motivation for philosophizing. Borrowing from Darwin's own words, *How are we to trust the convictions of human minds?* This undoubted given of Evolutionary Theory – its contingency – fuels sceptical intuitions

regarding the reliability of our cognition (on process reliabilism see, for instance, Goldman 1986), but also the logic of reason and Logic itself (Cooper 2003), and the possibility of knowledge in general (Thomson 1995). The sceptical mind needs only to echo the ateological tenets of contemporary evolutionary theories to indirectly raise the issue of the reliability of cognitive mechanisms, for how could these mechanisms be said to be truth-tracking if they are ever changing and the process does not – orthodox non-theistic evolutionists would argue – have an aim (Plantinga 1993), nor does it render as solely adaptive those beliefs that are (at least apparently) true? Obviously, as it is so frequently defended, our cognition is ever changing and *mis*beliefs can be equally prone to enhance *fitness* (McKay & Dennett 2009), so they too will be the targets of selective pressures.

One generic and considerably abstract formulation of the Evolutionary Debunking Argument (EDA) targeting the reliability of our cognitive mechanisms can be presented as follows (modified from Kahane 2011),

1. We believe that *p*, and there is an evolutionary explanation to our believing that *p*.
2. Evolution is not a truth-tracking process with respect to the processes that produce our belief that *p*.

From which it follows that, in accordance with a certain perspective on *epistemic justification*,

∴ We are not justified in believing that p.

And if this argument is sound, then we should be sceptic of the truth-value of any of our beliefs. In fact, a usual critique of this EDA goes so far as criticizing the argument for being self-defeating, given that even our belief in the truth-value of evolutionary theory is necessary for the first premise of the argument to stand. But let us assume for the sake of the argument that the claim of self-rebuttal on one hand, and the claim of circularity attributed to the contrary argument (an evolutionary argument which would favour the reliability of our cognitive mechanisms) on the other hand are unfair readings of the arguments, or that there is a way out of self-refutation and/or circularity (Vollmer 1983, Sterpetti 2015).

In this talk we will be focusing on a specific EDA presented by de Cruz *et al.* (2011), and on the truth-value of (2). After this first step of scrutiny, an aetiological account of belief formation within a systems-theoretical approach to cognition (von Bertalanffy 1968, Maturana & Varela 1973, Riedl 1984, Wuketits 1990, Hooker Hooker 1995, 2009 & 2011) will be suggested that allows for a rephrasing of this problematic and that should be integrated in any Naturalized or Evolutionary Epistemology worthy of its name.

Our third step and concluding *Working Hypothesis* will be that cognitive mechanisms producers of (at least approximately) true beliefs serve as better guides to our actions/interactions with the environment in virtue of their *causal fit* (Northcott 2013) with the World (see also Giere 2006), whereas misbeliefs enjoy lesser or partial fit, if any fit at all (they commit the bearer to false ontologies), and are, as such and most frequently, worst guides for our actions/interactions with the World, not so often promoting the fitness of Biosystems (Stephens 2001). And this, we will argue, is a consequence of the corollary of the *Good Regulator Theorem* (Conant & Ashby 1970) in accordance to which a *good* (successful and efficient) regulator for survival (such as our cognitive system) is one that maximizes its success when it is a(n isomorphic) *model* of the regulated system (by minimizing the entropy of the system) (Clark 2013, Friston 2015).

The causal-modelling nature of our cognitive systems will then allow us to speak of truth as a property-function of causally fit cognitive systems, it will allow for a novel understanding of the perceived causal *robustness* (Wimsatt 2007) of our World, and also for the study of how our cognitive traits vary in response to changes in

the environment (Hogeweg 2002), while simultaneously recovering an approximate notion of teleology to which Mayr (1965) referred to as *teleonomy*, thus vindicating the epistemic realist's intuitions that evolution favours true beliefs.

B. PHILOSOPHY OF NATURAL SCIENCES

In Defence of Randomised Clinical Trials

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In evidence-based medicine (EBM), the Randomised Clinical Trial (RCT) is often deemed the 'gold standard' of medical research. Above their application in medical research, RCTs are also used in other fields such as Economics or practical policy making. Randomised Clinical Trials provide a standardised and easily applicable template that is meant to test claims about the efficacy of drugs and other substances, of economic measures, and of policies, in a controlled environment that is designed to eliminate statistically disruptive factors.

Recently, RCTs have faced significant criticism. Two major types of criticism can be differentiated. In my talk, I will first give a brief exposition of these challenges and will then go on to argue against them. Lastly, I will argue that much of the criticism levelled against RCTs stems from a too strong reading of their status as a 'gold standard.'

The first type of criticism is the statistical worry that we cannot be certain that our sample is not atypical with regard to possible confounding factors. Due to the complexity of human organisms, economies, or societies, randomising does not guarantee that the sample is not skewed. John Worrall e.g. (Worrall 2002) argues that it is impossible to randomise all factors, confounding or otherwise. Statistically, even if an outcome is unlikely, it might still be possible. So however unlikely, test groups might just as well be skewed. But according to Worrall, even the more modest claim that randomisation makes it at least unlikely that the groups are skewed is problematic. The reason is that there is an infinite number of possible confounders. And if there are infinitely many possible confounders, then the possibility that our groups are skewed with regard to one of them might be high.

I will argue that in the case of medical research, we know enough about the relevant causal mechanisms in the body and about the mode of action of the tested substances from other sources than RCTs that we are justified to ignore a number of factors we have good reason to expect to not be disruptive. Worrall's line or argument ignores that we are not completely blind when it comes to eliminating confounding factors. If the Human body were a causal black box, then the argument would have force. However, we do e.g. know about the mode of action of new drugs before we test them in humans. Hence we are justified in believing that e.g. hair colour will not be a confounding factor, so we do not need to account for it. So the claim that the number of possible confounding factors is infinite is questionable. In fields other than medical research as e.g. in Economics, where the causal structure is much more opaque, Worrall's argument has more force.

The second type of criticism concerns the type of inferences involved in RCTs. Nancy Cartwright e.g. (Cartwright 2007, 2010) argues that RCTs are used as what she calls „clinchers“. She holds that, since RCTs are held to be the „gold standard“, they should rigorously establish that within the sample, a certain treatment causes a certain outcome. From this we deductively infer with the aid of a set of auxiliary hypotheses that the same treatment will also cause this outcome in the population. Cartwright holds that due to our imperfect sampling methods and the insecure nature of our knowledge of the similarity between the sample and the population, this last inference fails.

I will argue that RCTs are not a deductive method. While RCTs might be deductive as long as claims about the sample are concerned, the inference from the nature of the sample to the nature of the population is an archetypically ampliative one and should not be reconstructed deductively. We do not know whether the distribution of confounding factors in sample and population is the same. And when we, as is often the case in scientific practice, consciously select a sample that is more homogenous than the population, obviously our inference from the fact that a treatment caused a certain outcome in the sample to the treatment causing the same outcome in the population is ampliative. But that it is ampliative cannot be an argument against RCTs: we know them to be fallible. Neither does Cartwright's argument establish that any other method has a higher external validity than RCTs.

In the talk, I will argue that the notion of a 'gold standard' is problematic. Cartwright seems to hold RCTs to a standard against which they must necessarily fail. It is no surprise that the deductive inference breaks down. But it is a misrepresentation of scientific practice to reconstruct RCTs as a deductive method.

Also, placing RCTs at the top of a hierarchy does not entail that it should be the only method used to test a treatment. In practice, RCTs are almost never used in isolation. For instance, mechanistic evidence as gathered through in vitro experiments might rank lower in the hierarchies of EBM, but that does not entail that this kind of evidence should be ignored or is ignored in practice.

I will also argue that while RCTs are sometimes problematic in practice, e.g. where randomisation or blinding is impossible, and have to be backed up or even substituted by e.g. case-control studies, this does not entail that a well-designed and well-executed RCT is not a better method than a well-designed and well-executed case control study. Accordingly, modern hierarchies like e.g. GRADE (Cf. e.g. Grade Working Group 2004) take into account that sometimes, good case control studies can be better than bad RCTs. Yet still, even in GRADE RCTs are deemed the overall superior method due to the grater immunity to bias, be it explicit or implicit.

It is uncharitable to read the admittedly imprecisely phrased manuals for EBM that praise RCTs as a gold standard to hold that it is a deductive method, that RCTs never go wrong in practice, or that no other method is viable.

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The Biological Reality of Race does not Underwrite the Social Reality of Race: A Response to Spencer

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Quayshawn Spencer (2014) defends the biological reality of 'race'. He argues that 'race' as used in the current US racial discourse picks out a biologically real entity. Spencer thinks that folk racial classification has a biological basis—in particular, he argues that the current US meaning of 'race' is a proper name for biologically real entity. First, Spencer says that the current US census classification yields five different races. Second, he argues that recent human population genetic research also yields an interesting level of genetic clustering at the K=5 level. Thus, he contends that the current US racial discourse matches nicely with recent genetic population clustering results, i.e. K=5 level of human population structure. (Spencer calls the K=5 level of human population structure 'the Blumenbach partition' in honor of J.F. Blumenbach.) Therefore, he argues that 'race', in its US meaning, picks out a biologically real entity, i.e. US racial categories are biologically real. However, we argue that Spencer's argument does not succeed to prove that 'race' is a biologically real entity in a *broader* sense, i.e. broader than the US meaning of race. Moreover, this broader sense of 'race' is much more interesting than the US sense, and does much better justice to the social reality of universal race discourse. Apart from this, there are internal worries with Spencer's argument, in that the kind of genotypic clustering (K = 5 level of human population structure) he relies on is not the only biologically interesting way of clustering human populations.

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Dispositional Essentialism and the Principle of Least Action: Friends or Foes?

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One of the currently quite popular metaphysical accounts of the nature of the actual fundamental properties is Dispositional Essentialism (DE). According to DE, some (if not all) of the fundamental properties and relations of the actual world have dispositional essences. Dispositional essentialists typically claim that the dispositional essence (and the identity) of each fundamental property is dependent on a pattern of manifestation relations. Each of the latter expresses the disposition of the object which bears the property to give a particular characteristic manifestation in response to a characteristic stimulus. In contrast to the Humean a-modal causally inert properties, dispositional properties are inherently modal; in virtue of their dispositional essences, the fundamental dispositional properties bestow specific powers to their bearers. In the DE-context, there is no need to assume the existence of ontologically robust laws of nature to explain the behaviour of physical systems. All physical changes occurring in the world are due to manifestations of the dispositional properties and the so called laws of nature are nothing but 'expressions' of the dispositional essences of those properties. Finally, in spite of the strong

intuitions to the contrary, the actual laws hold in all possible worlds in which the actual properties exist; in other words, they are metaphysically necessary.

DE faces several challenges (for a brief overview of some of them, see Bird (2007)); the one I'll focus here concerns the compatibility of one of the major theoretical principles of physical science, the Principle of Least Action (PLA), with DE. In rough terms, PLA states that among the different possible 'routes' (that is, sequences of states) a physical system might follow in its state space the one actually followed is that which makes the quantity of action stationary.

Joel Katzav (2004) raised the issue by arguing that there exist points of incompatibility between PLA and DE. Brian Ellis (2005) responded on behalf of dispositional essentialists, but Katzav (2005) argued for the inadequacy of Ellis's attempt to reconcile the two doctrines. The debate has remained almost unnoticed until recently; yet, in their (2015), Smart and Thébault argued that the issue in question has been unfairly neglected and proceeded to discuss the main arguments of the debate. They *inter alia* examined three topics in the context of which they identified Katzav's main arguments against the compatibility of DE with PLA. These arguments aim to show that PLA a) presupposes a kind of metaphysical contingency which is at odds with the basic tenets of DE, and b) offers explanations of a different type and direction from those given by DE.

My first aim is to argue that dispositional essentialists have the resources to provide adequate responses to Katzav's objections. According to the first worry, PLA and DE are incompatible because the application of the former *presupposes*, in contrast to the tenets of DE, that the sequence of states of the object must be metaphysically contingent. I argue, against a plausible objection, that DE-ists could respond to this point by claiming that the application of PLA presupposes only a kind of logical/metaphysical possibility compatible with DE.

According to the second point of incompatibility, DE and PLA provide *different types* of explanation of the actual evolution of a physical system. The former provides an 'historical' explanation of the explanandum, whereas the latter a history-independent one. I argue that under an interpretation of PLA as a *meta-law* (which explains not the actual sequence of states but rather the form of the laws of the temporal evolution of a physical system), the two explanations are both history-independent and, hence, compatible.

The third point is that the direction of explanation of physical change DE provides is at odds with scientific explanation which proceeds from PLA to laws. I argue that, even conceding that (at least in the case under consideration) scientific practice is a legitimate source of metaphysical insight, PLA has no explanatory priority over the property-essence-based principle of DE (according to which, *all* laws of nature 'flow' from the dispositional essences of the fundamental properties). For, PLA explains the specific for each physical system laws of temporal evolution only by presupposing system's own properties.

My second aim here is to show that the really hard problem for dispositional essentialists is to provide a DE-friendly *metaphysical* account of *global* physical principles, such as the PLA. To this end, I examine and criticise Ellis's and Bird's metaphysical proposals.

My overall conclusion is that, though the three above mentioned objections pose no insurmountable difficulties to the defenders of DE, the question of the compatibility of PLA with DE has not been answered yet. In my view, this loose end is important from a metaphysical point of view, *especially if we take the latter to be a science-sensitive one*. For in that case, if a metaphysical account of the nature of fundamental properties (such as

DE) cannot prove its compatibility with a significant physical principle (like PLA), we have a good reason to be suspicious about the former.

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Quantum Mechanical Monism

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Schaffer (2010, 2015, Forthcoming) claims that Quantum Mechanics (QM) favors *Priority Monism*, the view that the universe is the only fundamental object. The argument rests on an interpretation of quantum entanglement as an emergent property. This paper presents a thorough critique of the argument and sketches some alternative takes on quantum mechanical fundamentality.

1 QM and Monism

A property is *emergent* if (i) is instantiated by a composite object and (ii) does not supervene upon properties and relations of the component parts. If emergent properties are possible there is some pressure to say that a composite object is more fundamental than its parts in that duplicating the parts will fail to fix all facts about the whole. Entanglement is naturally read as representing an emergent property. Consider quantum states of particles 1,2, 1* and 2*:

(1) $|1\rangle = |2\rangle = \frac{1}{2}(|\downarrow_x\rangle\langle\downarrow_x| + |\uparrow_x\rangle\langle\uparrow_x|) = |1^*\rangle = |2^*\rangle$

QM predicts they can compose systems 12 and 12* in:

(2) $|12\rangle = \frac{1}{\sqrt{2}}(|\downarrow_x\rangle_1 |\downarrow_x\rangle_2 + |\uparrow_x\rangle_1 |\uparrow_x\rangle_2)$

(3) $|12^*\rangle = \frac{1}{\sqrt{2}}(|\downarrow_x\rangle_{1^*} |\uparrow_x\rangle_{2^*} + |\uparrow_x\rangle_{1^*} |\downarrow_x\rangle_{2^*})$

respectively. (1)-(3) show the states of the component parts do not fix that of the composite system. On the other hand the converse always holds. Hence, the pressure to say the composite system is more fundamental. This is the general background for the QM-argument for Monism:

(4) ENTANGLED WHOLES ARE FUNDAMENTAL (EWF): If the state of a composite system is entangled the system is fundamental;

(5) THE ENTANGLED UNIVERSE PRINCIPLE (EU): There is an entangled quantum state of the universe.

Monism follows from (4) and (5).

2 **Against EWF**

I will argue that EWF rests upon an illicit generalization from the 2-particles case to the general *n*-particles case. Say that $|1....n\rangle$ is *fully separable* iff:

(6) $|1... n\rangle = |1\rangle \otimes ... \otimes |n\rangle$

whereas it is *maximally entangled* iff there are no cuts such that result in a product state:

(7) $|1...j\rangle \otimes |i...n\rangle$

Non-factorizability and maximal entanglement coincide for 2-particles but come apart more in general. Consider:

(8) $|1234\rangle = (|\uparrow_1\rangle\,|\downarrow_2\rangle - |\downarrow_1\rangle\,|\uparrow_2\rangle \otimes |\uparrow_3\rangle\,|\downarrow_4\rangle - |\downarrow_3\rangle\,|\uparrow_4\rangle) \neq |1\rangle \otimes ... \otimes |4\rangle$

(8) is *non-factorizable, yet is not maximally entangled*. Fixing the states of systems 1,2,3,4 will not fix the state of 1234, but fixing the state of the systems 12 and 34 will. In this case EWF fails.

3 No Collapse Interpretations of QM

EU entails an endorsement of no-collapse interpretations of QM. Yet many of them are inhospitable to Monism to various degrees.

Modal interpretations distinguish between the dynamical quantum state representing properties a system might have and the value state representing the properties the system does have. This immediately calls into question the quantum argument in that entanglement represent at best a possible emergent property. The problem is than how to fix the value state of a given system. Two strategies have been explored. According to the first one the values state is fixed by the dynamical state and a set of privileged observable defined on Hilbert spaces of component parts. This threatens Monism in that properties of the parts are fundamental. Another strategy has it that the value state is fixed by the quantum state alone. Yet consider a two particle system 12. Then, both property decomposition and property composition, i.e.:

(9) $[O_1] = x \leftrightarrow [O_1 \otimes I_2] = x$

Fails. It follows that fixing the properties of the whole fails to fix relevant facts about the component parts, thus undermining support to Monism.

Another case in point is the recent *nomological interpretation of Bohmian mechanics*. According to this interpretation we start with a set of particles and we specify their later development via a universal wave-function which is crucially interpreted as a humean law, i.e. a law that simply *describes* local matters of facts. Then we can interpret entangled relations as nomological relations that supervene on those local matters of fact that the universal wave-function summarizes. This undermines a crucial element in the QM-argument for monism, namely its interpretation of entanglement as an emergent property.

I will then move on to *Many Worlds* interpretation(s). There are two ways Monism can be understood in this context. The first interpretation has it that *the multiverse*, i.e. the totality of all world-branches, *is the only fundamental object*. There are two main problems with this: (i) the multiverse does not qualify as a mereologically universal element, in that decomposition of the universal state vector in its terms hardly qualifies as mereological decomposition; (ii) once a suitable basis is selected, the state of the multiverse could be obtained by superimposing the states of the branches. This seems to push towards a second interpretation which maintains that, *relative to each branch* the universe, i.e. the mereological fusion of all the material things in that branch, is the only fundamental object. Two distinct problems arise: (i) what this second interpretation offers is just pluralism in disguise; (ii) within each branch the quantum state is factorizable and thus lends no support to Monism.

Finally I will address *Wave-Function Realism*. This entails something much more radical than Priority Monism, namely *Existence Monism*, the view that there exists only one object.

In the light of the foregoing I conclude that the case for quantum mechanical monism is not as strong as it seems at first sight.

4 Conclusion

Finally I sketch alternative proposals to interpret the quantum evidence in terms of dependence relations: (i) Entanglement suggests that irreflexive entanglement relations are more fundamental than their *relata*; (ii) There are two dependence relations between quantum wholes and parts: on the one hand parts depend on the whole for their properties, and on the other wholes depend on their parts for their mereological structure; (iii) Entanglement relations induce *dependence symmetric relations between the entangled parts*.

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Causation and Explanation in Phenotype Research

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In this paper causation and explanation in phenotype research are examined with its similarities and differences to Aristotle's Theory of Four Causes. Complexity, context dependency and purpose are some of the other concepts which will also be examined.

Phenotype occurs through the interaction between organism and its environment and this interaction is a very complex net that every phenotypic trait has many interacting causes. These are related to each other in a way that they are constantly affecting and causing each other. This interrelated and complex way of their existence altogether causes the phenotype. In other saying: every part in this complexity has its specific roles/features according to its surrounding / its position within this net, so if it was in a different place, its roles/features/activities would have been very different. The research of the complex pathways of interaction net between genotype, phenotype and environment requires causal investigation which is a bit similar to the investigation of Aristotle's material, formal and efficient causes altogether. Although there is no teleological cause in the phenotype occurrence, there are still some similarities between Aristotle's Theory of Four Causes and current phenotype research. The author of this paper is aware that extracting final cause from Aristotle's theory is almost like taking out what it is. But still she argues that thinking on some similarities, even they are superficial, between current phenotype research and material-formal-efficient causes has some importance for philosophical discussions on causation and explanation in phenotype research. There is no final cause in biological phenomena but there may be final cause like cause in biology research: scientists' purpose. But the likeness degree between final cause and scientists' purpose is much more less than the likeness between formal-material-efficient causes and phenotype-genotype-materials-environment parameters.

Scientists investigate causes in phenotype occurrence as ecological, physiological, evolutionary, developmental, molecular, epigenetic and genetic factors. When they conclude an explanation about a phenotypic trait, this explanation is about one of these factors so they are giving an explanation of the phenomenon's one of the parts. If they want to give a complete explanation of the subject phenotypic trait they should research on all of these factors so they will probably need to collaborate with other scientists. The completeness of this explanation is of course limited by our current scientific knowledge, unless we are asserting new laws.

Phenotype explanation and research is context dependent. Because scientists for example:

research on a certain organism's certain response to a certain change in a certain environmental parameter. So scientists have purpose because they decide these certainties. This purpose is also context dependent: it depends on the state of scientific knowledge and the state of society. When a scientist decides these certainties so designs an experiment, it means that she/he picked a possible cause from the complexity of the phenomena. Of course she/he does not pick it randomly, as previously told this decision depends on scientific knowledge and society which are also complex systems in which complexity is even much more than the great complexity of phenotype occurrence. And at the end of the experiment if her/his hypothesis comes up as not false, she/he concludes that possible cause is actually the cause of the subject phenotypic trait. Here there are two very important points: this is a cause of the phenotypic trait in question in a specific context (in her/his experiment context) and she/he nominated it as a cause in the first place. So she/he is / should be aware that there are many other causes and also some of the other causes may be more important than the subject cause.

Both “Aristotle’s four causes” and “genotype-phenotype-environment” or “genetic, epigenetic, physiological, developmental, molecular, environmental, evolutionary factors” are related to each other very strictly that they are altogether causing the organism. We can look for –we usually look for- certain causes in certain research plans, depending on the situation and/so the purpose of the investigation, but we are/should always aware that this is a practical research way and these certain causes may affect differently in other contexts or there might be some other causes which are more important than the subject cause. If our claim is explaining an organism’s phenotype as a whole, we should search for all three causes: formal-material-efficient / ecological, physiological, developmental, molecular, epigenetic, genetic and evolutionary factors in the context of their interrelated state.

Explanatory Unification and Natural Selection Explanations

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The recent debate between the dynamical and the statistical interpretations of natural selection has centered on the question of whether all explanations that employ the concepts of natural selection and drift are reducible to causal explanations. The proponents of the statistical interpretation answer negatively, but insist on the fact that selection/drift arguments are explanatory. However, they remain unclear on where the explanatory power comes from. The proponents of the dynamical interpretation answer positively and try to reduce selection/drift arguments to some of the most prominent accounts of causal explanation.

To give a more detailed introduction of the debate, it will be necessary to at least crudely define natural selection explanations. A minimal definition could be that natural selection explanatory arguments aim to illuminate the predominance of particular phenotypes over other morphs in a population. This makes the concept of fitness a central notion, but it also assigns it a double role. Intuitively, fitness differences between phenotypes represent the fact that different heritable traits confer different ecological advantages and disadvantages on the organisms that possess them. These advantages might be typified as advantages that enhance the persistence of the organism and advantages that enhance the reproductive success of the organism. (Fitness usually refers to reproductive success, but there are cases in which persistence is more appropriate, such as when we are dealing with super-organisms, colonial organisms, etc.).

This leads to the straightforward analytical conclusion that, given a population of a limited size in subsequent generations, the fittest phenotypes will outnumber the less fit phenotypes.

Biologists investigate population dynamics by analysing fitness in two ways. First, in order to make morphological differences between organisms liable to a quantitative analysis, evolutionary biology pragmatically represents the survivability of organisms by breaking it down from a holistic propensity into singular ‘fitness components‘. One notion of fitness implies the analysis of actual ‘components‘. This analysis depends on an empirically inductive inference about the causal relevance of a particular trait or traits to the overall survival and reproduction of the organism. The life cycle of organisms is studied and comparative conclusions about the benefit of some trait variations are made.

However, in order for fitness components to become meaningful elements in an explanation of evolutionary dynamics, biologists must trace the frequencies of trait types within a population over many generations. This introduces the second mathematical measure of fitness. Mathematical fitness involves a further idealisation and

an abductive inference. The fitness of a trait in those cases is defined as a relative statistic normalised to the fittest organism in the population. Here further complications arise, as the causal relevance of a trait to the overall persistence and reproduction of the organism might be blocked by stochastic environmental events, it might not be represented in the next generation due to random gamete sampling, etc. Therefore, at a population level we can observe deviations from the expected fitness frequencies. This phenomenon is statistically describable under the label of drift.

Both notions of fitness enter actual natural selection explanations. At the core of the debate, therefore, is the question of whether the explanations that use this double-‘faced‘ notion of fitness are ultimately explanations of causes or not. The proponents of the dynamical interpretation defend the view that empirically derived comparative fitness between individuals can be consistently extended to mathematical fitness, therefore rendering natural selection explanations ultimately causal. Another dynamical interpretation is that natural selection and drift can be defined as population-level causal processes which will again render mathematical fitness as a variable that is bootstrapped to the most relevant causes. The proponents of the statisticalist interpretation argue that the mathematical measure of fitness generates purely formal effects, therefore ultimately rendering natural selection arguments as a form of statistical inference.

I will try to reconcile both interpretations. However, instead of constructing an argument in favour of how selection and drift refer to statistical representations of population dynamics or how they are directly reducible to a particular account of causal explanations, I will focus on explanatory power. I propose conveying evolutionary explanations within the unificationist model of scientific explanation. Therefore, arguing that explanatory power in natural selection arguments is usually a result of successful unification of the theoretically relevant information about individual- and population-level facts, or that both interpretations are partially correct, since both illuminate some of the elements of actual evolutionary explanations. In fact, this position is close to Sober’s original criticism on the propensity theory of fitness, where he defines fitness as a ‘holistic’ quantity that is employed in natural selection explanations both as a causal propensity and as a statistical variable. However, the contrast between my analysis and Sober’s position is that he accepts Millstein’s interpretation of selection as a population-level causal process, while I remain critical of embracing causality as the main governing criterion of good evolutionary explanations. Instead I propose that the notions of unification and explanatory pattern could be employed to cover natural selection arguments. Thus, it is possible to resolve the tension between the statistical and dynamical interpretations if evolutionary explanations are analysed as cases in which the more general and systematic explanatory models of population genetics are extended to particular explanatory projects by the coherent addition of premises, some of which clearly convey causal information.

To support this interpretation of natural selection explanations, I present a short case study on research on sympatric speciation as an example of how population- and individual-level facts are unified to explain the morphological mosaic of bill shape in island scrub jays (*Aphelocoma insularis*).

A Process Account of Internalizing Laws as a Way to Recover Governance

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One objection to SR is that it fails to give an account of causality due to the elimination of individual objects which are considered vital in causation. In order to include causality in ontology, French claimed that the structure is modal, where causal phenomena are explained in terms of necessity, possibility, and probability. He regarded that modality is found on laws and symmetries and not on dispositional properties while other structuralists such as Esfeld and Chakravartty proceeded with incorporating dispositions in their structural approach.

However, problems can be located either if one starts with dispositions or with laws. In his Central Dilemma, Mumford summarizes his argument as follows: if laws have a governing role then either a) they are external to their instances or b) they are internal to them. If option a) holds and laws stand independently of instances, then it is not clear how laws can exert governance. On the other hand, if option b) holds, then laws are internal to properties because they are reduced or supervenient to them, but then they do not play a governing role; if laws govern powerless properties, attributing properties with powers renders laws unnecessary. Since neither option a) nor option b) are appropriate, lawlessness is the other alternative.

The essay aims to explore Mumford's argument and suggest a framework that recovers the governmental status of laws. In particular, the reality of laws will be assumed, so that laws' existence does not depend on dispositions, but in order to internalize the laws and recover their governance, dispositions will still be employed. Neither instances nor laws are taken as fundamental, so that what is fundamental is the structure of becoming which treats laws inseparably from the instances. In that sense, laws are internalized. So the suggested approach is a middle way between the view which espouses the fundamentality of laws and the view that espouses the fundamentality of dispositions. Espousing features of dispositionalism does not necessarily entail that one prioritizes dispositions over laws, so that one can retain the reality of laws and at the same time, give a dispositional account of obtaining the laws. The idea is that if dispositions were not fundamental then laws would not necessarily base their existence on dispositions. The non-fundamentality of dispositions will entail that they lose some of their power to define things and in that sense, the recovery of laws' governance will be justifiable.

The analysis will be based on the following assumptions: a) dispositions are determinable and not determinate, e.g. determinable dispositions can make sense if they are associated with quantum operators which can yield different eigenvalues, b) the approach will be mereological, that is, matter will be defined as a function of a region and the respective properties - dispositions and manifestations - will be contextual associated with some region, c) there is a series of nested dispositions, that is, a determinable disposition gives a determinate manifestation and in return, the range of possibilities in dispositions is restrained by manifested quantities of previous dispositions. (As an example of nested dispositions, one can refer to the Hamiltonian operator H; for a system described by a wave function ψ_n evolving from an initial state to subsequent ones, new dispositions (ψ_{n+1}, H) appear along with the manifestation of previous dispositions (ψ_n, H).) The range of values of the determinable disposition will be taken to stand in a whole part relation with the determinate manifestation, where the mereological relation being bidirectional (from whole to parts and vice versa) is consistent with the nested character of dispositions. The existence of a topological overlapping between regions that characterize both dispositions and manifestations, is

an indication of the inseparable treatment of dispositions and manifestations as well as of manifestations with each other.

So it will not be a bottom-up approach from determinate dispositional properties to laws. Since not only do dispositions impose restrictions (as commonly argued in the sense that they function as the causes of manifestations and therefore, what is manifested is restricted by the relevant potencies), but dispositions themselves are restrained by the manifestations in a scheme of nested dispositions, dispositions are not regarded as fundamental.

In such a framework laws supervene upon dispositions (supervenience means that if the determinable quantities were different due to different physical candidates for matter and the relevant dispositional properties, the supervenient law would be different) and it will be shown in the paper in what respects laws can govern. According to Bird, one can espouse that governance is not confined at the level of potencies but both dispositions and laws govern, if one notice that laws are external to stimulus and manifestations. The main worry in such resolution is that one should avoid any overdetermination by showing that laws and dispositions do not govern the same thing. In the suggested framework, it will be claimed something relevant, that is, laws govern manifestations although as will be shown, this is a part of the entire picture. The outcome of the essay is that laws do not become impoverished by the potency of dispositions to determine, for dispositions are disentangled from total governance; both dispositions and laws are endowed with partial governance and none plays a central governing role. As a result, the recovery of a notion of governance justifies the retention of laws in the framework.

Two Styles in Mathematical Thinking

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In the 1980s Ian Hacking put forward what he called 'the styles project': a set of ideas and suggestions, discussed in various papers which should be understood as a programme of research about the diversity of scientific rationalities. I shall use Hacking's notion of style to shed light on the basic structure of mathematical thinking. I shall describe two styles of thinking in mathematics: the postulational style (or the style of demonstration) and the algorithmic style (which consists in following step-by-step procedures). Then I shall suggest that a few features might be considered as the characterizing features of styles of thinking.

Hacking made precise claims about the features of styles: for instance, he claimed that a style introduces new types of objects, evidence, laws, possibilities and new true-or-false sentences, i.e. sentences whose truth-value hinges on the style of thinking itself (for this reason they are called *style-dependent sentences*). For Hacking, whether or not certain sentences (style-dependent sentences) possess a truth-value depends on whether we have ways to reason about it. Styles of thinking fix the sense of *certain* sentences by making them candidates for truth or falsehood. Moreover, he suggested that styles have sharp beginnings: their birth represents a sort of radical change in the way of thinking that happens in the span of few dozens of years.

Hacking's views about styles evolved over the years: in recent papers, he put emphasis on the idea that a style is a way of thinking *and* a way of *doing*, i.e. a way of intervening in the world in order to know; furthermore, he added that a style is rooted in human innate capacities and that it is a way of *finding out* . Ultimately, over the

years Hacking has brought up precise features of scientific styles of thinking:

1) A style of thinking is a way of thinking and *doing*. In particular, a way of *intervening* in the world and 'finding out "that" so and so, but also, finding "how to". Finding out what's true, and finding out how to change things' .

2) A style of thinking relies on a new kind of evidence for 'finding out in the sciences'

3) A style of thinking introduces new candidates for truth-or-falsehood, new types of explanations and/or new criteria, laws, objects.

4) A style of thinking is self-authenticating .

5) A style of thinking represents a sharp break in the history of Western thought.

Despite the fact that the claims (*1)…5)*) could provide a characterization of what a style of thinking is, it is not clear whether or not all the styles he mentioned actually possess *all* the properties Hacking added over the years. For example, from Hacking's works it does not emerge why the postulational style of thinking introduces new evidence, why it is a way of doing and whether its birth is to be considered a discontinuous event in the history of thought. Still less clear is whether the algorithmic style in mathematics deserves the label 'style of thinking'. Another problem with Hacking's claims is that he does not say whether and why being self-authenticating is a feature of *all* the styles he mentioned. Is self-authentication a feature that should characterize each style? There is no clear answer to these questions.

What makes things worse is the fact that it is unclear whether a style can be identified by necessary and sufficient conditions. In this paper I shall show that that there are at least two ways of thinking that satisfy the features 1)...5) and that we may call the algorithmic and the postulational styles of thinking. Hacking has only mentioned the former and briefly described the latter (which has been introduced by Crombie). It is a safe guess to say that the spectrum of our ways of finding out about the world is quite broad: there might be other ways of finding out that possess all the properties 1)…5) in addition to ways of thinking that do not satisfy them.

If it is shown that all the styles mentioned by Hacking satisfy the properties 1) …5), as he seems to maintain, these very properties can be considered as the characterizing properties of styles of thinking: a style of thinking in the sense of Hacking is a way of thinking that satisfies the properties *1)…5)*. This paper is a first step in this direction: it shows that two ways of mathematical thinking, the algorithmic and the postulational, satisfy the properties *1)…5)* and that they can be considered as styles of thinking, in the sense of Hacking. It is a task for future research to show that all the other styles mentioned by Hacking possess the features *1)…5)*.

It is an important corollary of this conclusion that the notion of style of thinking, as conceived by Hacking, is a useful tool for characterizing mathematical thinking. Hacking has always insisted that, beside the postulation style, there is another fundamental way of reasoning in mathematics, the algorithmic style: as I am going to explain, this is the way of thinking that we use when we make calculations or when we apply algebraic formulas. However, Hacking has never characterized it and shown in detail why this way of reasoning can be considered a style of thinking. Moreover, he has left only few suggestions as to why the postulational way of reasoning can also be considered a style of thinking. This paper develops Hacking's suggestions in order to show that mathematical thinking can be characterized in terms of two styles of thinking.

C. PHILOSOPHY OF COGNITIVE AND BEHAVIORAL SCIENCES

On the Discovery of the Concept of Space

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In this paper I shall present the first steps of an analysis of how we discover the concept of space. I will ground my analysis on the reflections of space that we find in some of Immanuel Kant's writings, first and foremost in his *Critique of Pure Reason* (hereafter *Critique*) (1781/1787), and in the *Concerning the Ultimate Ground of the Differentiation of Directions in Space* (hereafter *Directions*) from 1768. In these works, Kant gives thorough analyses both of space as such, of our concept of space, and of the relation between them. The common denominator is that he considers space as such as *primitive*, that is, as *a priori* and *unconditional*.

As primitive, space serves as a *ground* [*Grund*] from which certain *consequences* follow. The topic of ground is treated in his work *Attempt to Introduce the Concept of Negative Magnitudes into Philosophy* (hereafter *Negative Magnitudes*) from 1763. We shall later see what this ground/consequence relation implies for Kant.

The two main points of my talk are i) to account for how we discover the concept of space, given that it is not empirical, but *a priori*, and serves as an unconditional *ground*, and ii) to present Kant's arguments concerning our feeling of an inner difference of directions.

The latter may inform us about a possible link between Kant and the neuroscientific findings concerning space and spatial orientation and how these are represented in the brain (sometimes referred to as the (neuroscientific) discovery of our "inner GPS").

Do Pre-cueing Effects Entail the Cognitive Penetrability of Early Vision?

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In previous work (Author), I argued that a stage of visual processing, namely, early vision, is not directly affected by cognition in the sense that its processes do not receive any cognitive feedback in a way that would justify the view that early vision draws on cognition as an informational resource, which is the essence of the claim that perception is cognitively penetrated (CP); early vision is cognitively impenetrable (CI). The motive underlying this thesis was the empirical findings suggesting that cognitively-driven attention directly affects perception only at the time scale of late vision that succeeds early vision. Recently it has been argued that the various pre-cueing attentional effects occur in the time scale of early vision and since the attention involved is clearly cognitively-driven, pre-cueing entails that early vision is CP. In the three parts of this paper, I defend against this criticism the thesis that early vision is CI.

In the first part, I argue that the philosophical discussion concerning the CP of perception was motivated by the view that owing to the cognitive effects on perception, perception cannot play the epistemological role

assigned to it by empiricism because it does not provide a neutral ground on which to decide which of our cognitive schemes is true or false. Indeed, it is intuitive to think that perceptual experience provides defeasible evidence for beliefs, and that it does so directly without any intermediate mental states. Thus, perceiving p provides prima facie justification, i.e., rational support, for the proposition p. The most important consequence of the CP of perception, therefore, concerns the epistemological role of perception as providing the evidence that grounds perceptual beliefs; if perception is CP, perception's role in grounding perceptual beliefs is undermined. CP may be epistemically damaging because it may create insensitivity to the distal stimulus, in a way that the percept may reflect more the conceptual framework of the perceiver than the environmental input to perception.

This sets the following condition for CP, the epistemic criterion for CP: If perception (or a stage of it) is cognitively influenced in a way that renders it unfit to play the role of a neutral epistemological basis by vitiating its justificatory role in grounding perceptual beliefs, perception (or a stage of it) is CP. If perception (or a stage of it) is cognitively influenced in a way that does not affect its epistemic role, it is CI.

In the second section, I argue that the epistemic role of perception centers on, but is not exhausted in, the percept, because it is the percept that ultimately grounds the belief whose content matches the content of the percept. The percept is formed in late vision because it presupposes that the object and its features have been identified and this takes place in late vision. It is, thus, late vision that delivers the most important item in the justification process. The details of the processes by which late vision forms the percept have been discussed elsewhere and I will not go to them here. It suffices to say that the epistemic role of late vision is affected by cognition and, thus, late vision is CP.

The epistemic role of early vision is determined by the fact that early vision retrieves from the distal visual scene information that feeds to late vision and is used for the formation of the percept along with the to semantic information made available in late vision by cognition. This information (the proximal image or stimulus) provides the "evidential" basis on which the various hypotheses concerning the identity of objects in the visual scene that are formed in late vision will be tested. In other words, the role of early vision is to retrieve from the environment the information that will be used by late vision in order for the distal objects in the visual scene to be identified. As I have argued (Author), the contents of early vision consist in the proto-objects corresponding to the distal objects and their properties.

The task at hand is to determine whether the indirect cognitive effects on early vision affect its epistemic role, i.e., whether they intervene in the information retrieval process occurring in early vision in a way that diminishes the sensitivity of perception to the environmental data. If the extrinsic cognitive effects on early vision fail to affect its epistemic role, *per* the epistemic condition, the indirect effects do not entail the CP of early vision.

In the last part, I argue that the pre-cueing effects on early vision do not affect its epistemic role. In spatial pre-cueing, first, the anticipatory effects do not determine the percept since by pre-cueing enhances the responses of all neurons tuned to the attended location independent of the neurons' preferred stimuli and keeps the differential responses of the neurons' unaltered. In the case of object/feature pre-cueing, second, although the anticipatory effects enhance the activity of the neurons responding preferentially to the pre-cued object or feature increasing the likehood that they will be selected eventually for further processing, early vision still retrieves in parallel information concerning all the objects and features present in the visual scene so that these objects be individuated independently of whether they are targets or non-targets. The pre-cueing effects on early vision, do not affect what information is retrieved from the visual scene. This is so because the cognitive effects do not affect the perceptual processing itself and do not influence the retrieval of information from a visual scene. It follows

that the concepts that figure in the cognitive contents that guide the attention involved in pre-cueing do not affect the contents of the states of early vision and, thus, do not shape the information retrieved from the visual scene. This means that cognition does not affect the selection of the 'evidence' or the information against which hypotheses concerning object identity will be tested in late vision. It follows that pre-cueing and the various cognitive effects underlying it do not affect the epistemic role of early vision.

Learning Conditional Information from Stalnaker Conditionals

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Douven (2012) dismisses Stalnaker conditionals as a tool to model the learning of conditional information. We show that the learning of conditional information may be modeled by (Jeffrey) imaging on Stalnaker conditionals. The basic idea is to take the learned meaning of the Stalnaker conditional(s) as constraint on the similarity order between worlds into account. We show that this new account models the three examples, which Douven takes as benchmark, in accordance with the prescribed intuitions. Additionally, we generalise the account to cover uncertain conditional information, apply the generalisation to model the Judy Benjamin Problem, and compare it to an account proposed by Hartmann and Rad (2014). We conclude that Douven's dismissal is unjustified, and that the proposed method is a viable alternative to Hartmann and Rad's learning method.

Numbers within our Grasp

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Embodied theories of cognition maintain that all our knowledge is grounded in our modal systems for perception and action (Barsalou, 2008). This means that we acquire knowledge via activated relevant sensory-motor representations (Jaennerod, 2006). However, numbers have been traditionally considered abstract knowledge for which the embodied perspective could not account. Since the discovery of a consistent number-to-space mapping (Dehaene et al. 1993) there have been many reports of sensory and motor biases elicited by numerical concepts (e.g. Andres, 2008). The relation between numbers and space is best reflected by the so-called SNARC effect (Dehaene et al. 1993), according to which small numbers (e.g. 1 or 2) are associated with left lateral responses, and larger numbers (e.g. 8 or 9) are associated with right lateral responses. Moreover, this pattern has been observed with various tasks and different effect systems: hands, feet (Wood et al. 2008), or eye movements (Myachykov et al. 2015). These observations led many researches to the hypothesis of a spatially oriented *Mental Number Line*, which is a representation of the numerical meanings in our minds (Dehaene, 2011), with small numerical magnitudes on the left, and larger numerical magnitudes to the right. The same numerical meaning for "small" and "large" has been shown to modulate different motor actions, such as reaching and grasping. For instance, Andres et al. (2004), Badets et al. (2007), Ranzini et al. (2011), and many others, report

that numerical magnitude interacts with motor actions like executing a precision or power grip, estimating an object's graspability or an object's potential functionality (i.e. affordance).

The present paper focuses of the interaction between motor and numerical cognition with respect to object's affordances. The latter notion refers to the functional characteristics of objects with action capabilities (Tucker and Ellis 1998). We provide empirical evidence from different studies showing motor and sensory biases induced by the SNARC effect. These observations suggests that numerical cognition is also grounded in our perceptual and action systems. We also report results from a series of experiments conducted in our university laboratory in which we investigate the interdependence of both SNARC effect and object affordances. We presented participants with eight numbers (four small and four large) and a picture of a frying pan with handle pointing to the left or to the right. Participants had to perform a parity judgement task with bimanual response. We varied the presentation of the stimuli in the experiments: numbers were presented before, after, or simultaneously with the picture of the frying pan. Our data suggests that the interplay between number magnitude and object affordances is not of an automatic nature and needs time to develop. This also points to the possibility that numerical magnitude and motor action could share common representational platform and rely on common mechanism. Generally speaking, our results are in support of the embodied nature of the numerical representations.

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Convergent Perspectivism: A Model of Integration in Neuroscience

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This paper articulates convergent perspectivism as a model of integration in neuroscience. The thesis is that convergence of results produced on the basis of multiple experimental perspectives is necessary for validation of knowledge claims in neuroscience and it is also sufficient for integration of knowledge thus validated.

The model is based on the notion of convergent validity which requires convergence of results from multiple experimental setups in order to establish the validity of studied effects (Campbell and Fiske 1959). If the reproducibility of converging results from different independent experiments is established, the theoretical constructs postulated in these experiments get validated. This procedure, therefore, favors maintaining a multiplicity of different experimental setups for the study of presumably identical phenomena. I argue that the convergence of results from different experimental setups should be regarded as a necessary condition for validation of those experimental results. Reproducibility of results from identical experimental setups is in turn necessary for establishing the reliability of the different experimental designs individually. However, the reproducibility of experimental results in neurobiology and cognitive neuroscience has been challenged on a number of occasions (e.g. Sullivan 2009 and 2010). Lack of reproducibility of experimental results means lack of integration of knowledge even if knowledge claims are valid within the context in which they were produced originally (Sullivan 2009). Further, if neurobiology and cognitive neuroscience could not be integrated individually, nor would be neuroscience which relies on the methods of both. Thus, an important question needs to be addressed. How is reproducibility of experimental results in neurobiology and cognitive neuroscience to be ensured in order to enable knowledge integration in neuroscience?

The question is normative and I will consider two strategies for addressing it. However, the answer will also have to be compatible with the practice of neuroscience. This is why I will look into methodological discussions as well as successful practices in both neurobiology and cognitive neuroscience in order to identify successful practices of validation and integration of knowledge claims. This will enable me to articulate a prescriptive model of integration in neuroscience.

One strategy for addressing the question of reproducibility of experimental results is for experimenters to strive to achieve unification of their experimental practices through standardization of protocols and procedures. This strategy will ensure the reproducibility of experimental effects and results across laboratories and therefore will be sufficient for integration of knowledge claims thus produced. However, there are risks associated with this strategy. It may deprive science from variety that leads to discoveries (Wahlsten 2001). It also leaves science vulnerable to the so called "standardization fallacy" where standardization may lead to the reproducibility of artifacts and errors (Würbel 2000). In other words, the strategy for integration through standardization puts in jeopardy the validity of knowledge claims. Variation in multiple experimental designs, protocols, and instruments is conducive of validation of experimental results and standardization would eliminate it.

The second possible – and I argue better – strategy is to start by ensuring the validity of experimental tools and designs. Atanasova (2015) shows that in the process of validation of animal models – which are the main experimental tool in neurobiology – neurobiologists construct multiple experimental arrangements to study the

phenomena they explore from multiple partially overlapping perspectives. This practice is readily identifiable in the use of test batteries. Test batteries are semi-standardized sets of tests, which often overlap with respect to the functions they measure. Vorhees (1996) argues that the employment of different tests for measuring the effects studied in neurobiological animal models provide converging data on a given functional domain targeted for study. This strategy ensures that the measured effects are not artifacts of the experimental setup and the results are thus valid.

The analysis of the practice of validation of animal models motivates my articulation of the principle of convergent perspectivism as a principle for maximizing experimental knowledge in neurobiology. According to convergent perspectivism, it is necessary to employ multiple experimental designs and tests for the study of targeted neuro-cognitive phenomena in order to validate the knowledge claims articulated on the basis of animal model experiments. When results from multiple experimental perspectives converge, the knowledge claims thus produced are validated. I consider it trivially true that converging results from multiple experiments can then be integrated. In other words, convergence is a sufficient condition for integration of experimental results and the knowledge claims produced on their basis.

Further, I show that a very similar strategy for validation of experimental results is employed in cognitive neuroscience as well. I analyze the use of multiple imaging techniques such as fMRI, MEG, and EEG in the study of conscious and non-conscious information processing (e.g. Dehaene et al. 2011) and show that the motivation is analogous to that of neurobiologists using test batteries for the production of converging experimental results. Thus, convergent perspectivism applies as a model of validation of theoretical constructs in cognitive neuroscience as well. I argue that here too it should be regarded as a necessary condition for validation and a sufficient condition for integration.

Finally, convergent perspectivism can be extended as a strategy for integrating converging results from neurobiology and cognitive neuroscience. Therefore, it is a good candidate for a normative model of integration of neuroscience in general.

Convergent perspectivism successfully meets the recent challenges for the integration of neuroscience. Moreover, it puts a common denominator under two traditionally competing models of the unity of neuroscience, Craver's mosaic unity (2007) and Bickle and colleagues' convergent 3 analysis (Silva et al. 2014). Both of these models identify the need for employing multiple experimental approaches in the study of the phenomena targeted in neuroscience. For example, Craver defends the need to use multiple techniques for detecting the components of mechanisms the description of which is the ultimate goal of experimentation in neuroscience in his account. Bickle and colleagues explicitly analyze the role of converging results from three types of experiments in the identification of causal connections and the articulation of causal explanations in cognitive neurobiology. Convergent perspectivism, thus, is a powerful model of integration in neuroscience.

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A Mechanistic Model of the Authority and Inescapability of Moral Norms

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Naturalizing normativity in the moral domain seems to be a tough nut to crack. Recent literature in evolutionary ethics has converged on the claim that evolutionary explanations debunk rather than vindicate moral and evaluative claims (Joyce 2006; Street 2006). Their function is not to track truth, but to promote fitness. I agree that evolutionary explanations represent a plausible working hypothesis for the origin of values and moral norms. However, though antirealism is the natural outcome for values in general, moral norms cannot be handled in the same way. Moral norms have a special authority and inescapability that values in general do not share. These properties can be vindicated without abandoning the antirealist framework. I propose a vindicatory view by explaining moral judgment as the output of the interaction between at least two proximate mechanisms: namely a disposition to confer value on others and the ability to share the contents of our minds.

When we say that a given behavior is morally wrong, „morally wrong" conveys the thought that the behavior is prohibited by a norm with a special authority. This authority is overriding (trumps any other egocentric norm) and inescapable (applies to everyone). The evolutionary debunking literature argues that these properties refer to nothing in the world. Our belief in moral authority is thus false, but useful to build stable and cooperative societies,

for which our naturally inherited altruistic dispositions were insufficient (Joyce 2006; Kitcher 2012). Once we understand the evolutionary explanation, the peculiar force of moral norms vanishes into thin air. This, at least, is the claim.

This argument contains a mistake. Surely, if values in general depend on what promotes fitness, moral values cannot question this fact. But they don't. The authority and inescapability in question is an internal affair, something that arises within agent- relative and egocentric values. Moreover, they do not arise for any species whatsoever, but only for species with a special psychological makeup. And they arise as the output of a mental mechanism designed to facilitate cooperation, and to push it to levels that are only matched by multicellularity and by the eusocial insects. Because of the fundamental antirealistic stance, whatever supports moral authority and inescapability cannot be mind-independent. Moral wrongness, i.e., the authority and inescapability of moral norms, is supported and produced by a special mental mechanism.

Joyce (2006) suggested that this mechanism is moral judgment itself. It emerged as a complete capacity, not e.g. out of our pre-existing altruistic dispositions, but in addition to and in competition with them (Joyce 111-115; see also 16, 44, 49-50; also Kitcher 2012, 69). Moral judgment is posited as a mechanism without "innards." But this view of the mechanism is harmful for the project of naturalizing moral normativity. One consequence is that we have no insight into whatever explains our experience of moral normativity. A further consequence is that the content of the norms to which moral clout is attached does not follow from an understanding of the mechanism itself. It is stipulated that they have to do with *cooperation*, and with *public* prohibitions against harming and cheating, but the mechanism, because it lacks "innards", cannot illuminate how these features of moral judgment arise (Joyce 2006, 111; 115). What we need is a mechanism that explains the force of moral normativity, its public character and gives us some insight into the content of moral norms.

Moral judgment is better viewed as the output of a mechanism. If we understand the innards, we shall understand the output. An early attempt to disclose the inner workings of the mechanism was offered by Darwin himself. He wanted to explain the force behind "ought-judgments" (Darwin 1871/1981, 70), distinguishing between the overpowering but transitory force of selfish instincts and the calm but persistent demand that social instincts place on us. In this way he explained the power of moral consciousness, that which makes us feel regret of remorse if we violate moral norms (Darwin 1871/1981, 91). This explanation, however, seems ultimately insufficient, because it is not clear why the demand of social instincts should be felt persistently through time in any social species.

I offer a different hypothesis. I propose to explain moral judgment as product of the interaction between at least two other mechanisms. One mechanism confers value on the wellbeing of others, a similar value as selfishness confers on the wellbeing of the self. It is the capacity to take interest in their wellbeing, to value their joy and disvalue their suffering. Through this mechanism a primitive, non-derivative desire to avoid harming others and to help them enters into our motivational makeup. But because our selfish desires are also primitive, this does not confer any special authority on other-regarding desires. The special authority must come from another psychological capacity. In my opinion, it comes from the capacity that comparative primatologists call "shared intentionality". This capacity refers to the ability to form beliefs and intentions shared with other individuals, such that the state of being shared is known to all, and that the participants can predicate these mental states of a collective subject: "we think", "we intend", etc. It is a crucial cognitive ability that underlies the human capacity to engage in collaborative action (Bratman 1992) follow and enforce norms, and create social institutions

(Tomasello & Carpenter 2007). Shared intentionality creates a public sphere, in which all subjects participate as equals. When humans acquired this capacity, the conflict between selfish and other-regarding dispositions experienced by everyone (excepting perhaps psychopaths) became also public knowledge and was solved in the only possible way compatible with publicity: with the special authority and inescapability of two rules: 1. selfish desires never justify harming others, except in self-defense and 2. helping others is also required, but the extent of it is subject to a greater degree of individual freedom.

D. PHILOSOPHY OF SOCIAL SCIENCES

The Philosophy of Consumerism: Marketing through the Prism of Philosophy

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It is not enough to have a good mind; the main thing is to use it well.
Rene Descartes

XXI century has already become the age of globalization, innovation and fierce competition practically in every sphere of life. It's becoming more and more difficult for companies to gain new markets and amaze customers with splendid and beautiful items or services. In the world where we can solve all our problems just with the help of our gadgets and do the shopping on-line just in one click, the basic needs of average person have considerably changed.

One of the most powerful instruments of marketing is doing market surveys and researches, but sometimes we have to look in the past and analyze it, or just find the reasons of different types of customer's behavior.

The aim of this research is to analyze and compare the development of philosophic thinking on the one hand, and different marketing strategies that took their place during historical periods. The next thing is to investigate which marketing strategies are used by well-known and most powerful brands and enterprises to create the whole picture of XXI century's system of thinking. It will help to find out which of philosophical currents the most appropriate one in modern society is and which ones should not exist on global marketing area. Moreover, by analyzing the past we will be able to make some predictions about possible changes in the nearest future. The emphasis will be put on types of behaviors in different customers' groups divided by age, sex, gender, social status and level of wealth.

The research is generally based on analyzing of international companies' experience in realization its original concept and purpose by conducting activity in different environment. Kinds of thinking, implemented methods and final results will be taken into consideration. By using date obtained from Internet, articles, books and methods of comparison in the course of this research it is supposed to disclose the role and importance. This paper should be a brief summary of the connections between marketing and people`s way of thinking during the whole process of history and in today's society as well.

Philosophical Foundations of Human Resource Development Methodology

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Human Resource Development is a process of learning, organizational development and professional growth of staff aimed at solving current and strategic objectives of the company by achieving higher individual and organizational effectiveness, employee development in terms of their contribution to the company. Problem of analysis of the methodological basis of the Human Resource Development requires further studying. The aim of the paper is description of philosophical foundations of Human Resource Development in enterprises.

One of the most influential philosophical doctrines is realism that asserts the reality of certain objects, depending on the type have several options ontological realism: universals or ideas - Platonic or Aristotelian in realism, scientific objects of scientific realism, etc. [1]. This fundamental, ontological meaning of realism may be supplemented by an epistemological point of view, according to which real objects are also recognizable. This epistemological realism asserts that the facts about these objects are known or can be known, that the subject of knowledge have epistemological access to the object of knowledge and there is no insurmountable barrier between the perceiving subject and the existing object [1]. At the same time simplified and isolated conditions of economic models usually cannot be reproduced experimentally, making it difficult to verify the truth of empirical economic theories and therefore economic realists are fallibilsts who taking inconclusiveness of existing knowledge and the ability to adjust and review in the light of new evidence and provements [1]. Economic realism argues that economic reality is an objective (although to some extent dependent on consciousness) existing structure, and economic theories, albeit incomplete and some false elements capable to reflect some important aspects of reality correctly [1]. Two of the most influential scientists in the field of economic realism is U. Maki, who develops new conceptual tools that reflect the peculiarities of economics based on a wide range of philosophical sources, and T. Lawson, who draws from the philosophical system of G. Bhaskar and criticizes the main direction of classical economics [1].

One of the modern economics paradigms is critical realism, whereby the world that is the subject of scientific research, consists not only of events given to us in experience or perception, but also (those which are not limited to them) of structures, driving forces and trends, which, although not directly observable, but nevertheless underlying real events, manage them or facilitate their implementation [2]. T. Lawson proved the theory of social philosophy according to which various forms of social structures (relationships, rules, institutions) form a relatively autonomous area, which is the result of interaction between humans and dependent on it, but has properties that cannot be reduced to this interaction and it has the opposite effect [3]. Within this paradigm the world consists of the objects that are structured and structured not transitive, that is, first, not confined to events (which may be present or may not be present in our experience) and secondly, exist independently of their identification [4]. The purpose of economic theory is identifying structures that govern these activities, the conditions in which it is immersed, which facilitate it and are transformed through it, these explanations entail understanding certain practices, analysis of unknown conditions for their implementation, unconscious motivation behind them, implied the skills used in these practices and their unintended consequences [4]. Despite the fact that society and the economy are the product of economic agents, they are opaque to some extent for these agents, so the task of economic theory is the description of all that has to be done (or not that aware of the processes involved in agent) to an isolated social phenomenon, a set of practices or actions made possible [4]. According to

critical realism the science is identifying and studying the structures and mechanisms that underlie the experience of events, and the task of science is to comprehend the reality at its deepest level.

Today we can observe the expansion and deepening of human impact on the world, including the society. According to B. Latour, society is not predetermined concept, its essence is determined according to a specific time and spatial context and a type of social connection between things that are not social [5]. Today's changes create new cultural paradigm of the globalized future, which has no generally accepted name yet. A distinctive feature of this paradigm metaphysics is update [as qualitative facilitate of the new creation opportunities] of virtual worlds [6] with the help of information and communication technologies and transfer of human social, intellectual and even physical activity in virtual space. The world seen in terms of the possible, necessary and actual, in the conditions of spreading the influence of virtual reality and establishing an arbitrary laws of the last mentioned issues, begins being analyzed in terms of processes and management. The development of information and communication technologies led to widespread use of social networks, distance learning and artificial intelligence that delocalizes and even globalizes human social activity. The use of virtual reality became easier with the development of the access to the tools of its formation. Their use allows you to create an environment for effective support of Human Resource Development.

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E. HISTORICAL AND SOCIAL STUDIES IN PHILOSOPHY OF SCIENCE

Galileo Galilei on the Bright Side of the Moon: »Necessary Demonstrations« and the Second Earth

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From *Sidereal Messenger* (1610) onwards Galileo more and more explicitly defended Copernicus' heliocentric cosmology, which led to a fierce polemics about the theological dimension of the heliocentrism in the years 1613–1616. Galileo responded with a series of letters – the so called Copernican letters (*Letter to Castelli*, *Letter to Dini* and the most famous of all *Letter to the Grand Duchess Christina*) – explaining his understanding of the relationship between the divine truth (the Bible) and the natural truth. In this context he ventured into a Copernican interpretation of some biblical passages, which aroused concerns among theologians and even among his fellow mathematicians. Jesuit father Grienberger, for instance, who replaced Christopher Clavius at Collegio Romano as a teacher of mathematics, pointed out that "he would have liked Galileo to first carry out his demonstrations, and then get involved in discussing the Scripture".

Galileo, in his turn, continuously claimed that his Copernican conclusions are based on "what sensory experience (*la sensata esperienza*) places before our eyes" (he also used the terms "sense" and "thousand experiences") and on what "necessary demonstrations (*le necessarie dimostrazioni*) prove to us concerning natural effects" (also: "demonstrative and necessary reasons", "thousands necessary demonstrations"). In one of his letters he claimed that he was "in the process of collecting all of Copernicus's reasons and making them clearly intelligible to many people; and I am adding to them many more considerations, always based on celestial observations, on sensory experiences, and on support of the physical effects".

What exactly does Galileo mean when he speaks about the "necessary demonstrations" or "demonstrative and necessary reasons"? William Wallace argues that the answer to that question could be found in Galileo's youthful logical writings on Aristotle's *Posterior Analytics*. According to him Galileo learned about "the method of the demonstrative regress" and used it when he was explaining his astronomical discoveries. Wallace claims that the overall logical form of several Galileo's arguments for his Copernican astronomical discoveries is that of demonstrative regress as described by Galileo himself and recognized by Zabarella in Aristotle's *Posterior Analytics* I, 13. The demonstrative regress involves two progressions: one from the effect to the cause and the other from the cause to the effect. They are separated by an intermediate stage (the so called "work of the intellect"; *negotiatio intellectus*, *consideratio mentalis*, *meditatio*) wherein one sees the causal connection between the two as necessary and adequate to explain the phenomena. Wallace argues his case on the example – among others – of Galileo's discovery of the mountains and valleys on the Moon, which make the Moon "another Earth".

I agree with several points Wallace makes, but find it very difficult to accept that the overall form of the demonstrative regress (which can be clearly applied on Galileo's reasoning) is *all* that Galileo did when he was explaining the moving spots on the surface of the Moon as a result of the mountains and the valleys. Even more

importantly: given that several alternative explanations of these phenomena were possible, one of them mentioned by Galileo himself, the crucial question is not what kind of overall reasoning he used, but whether he actually "demonstrated by necessity" the existence of valleys and mountains on the Moon.

My talk is divided into five parts. First, I will take a brief look at the discoveries made by Galileo with the telescope and his Copernican conclusions made thereupon. Second, I will roughly examine his logical notes and ideas expressed therein and explain how they can throw light on "necessary demonstrations" for his Copernican interpretation of the nature of the Moon in the *Sidereal Messenger*. Third, I will take a look at his explanations on the several phenomena observed on the Moon. I will put them into the context of other philosophers' and astronomer's ideas on the nature of the moon (Melanchton, Kepler, Sarpi, Delle Colombe, mathematicians of the Collegio Romano). Some of them shared same ideas with Galileo, some proposed alternative solutions. Finally, I will address the question of whether Galileo proved demonstratively that the Moon is the second Earth.

The Paradigm of Contingency - A Historical Typology

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The **paradigm of contingency** which I would like to talk about is really influential in 20th-21st century science (and the popular *indeterministic* talk is based exactly on such influence). It's not only related to quantum mechanics and probability theory (through the concept of *chance*) or to modal logic (through the concept of *contingency* itself) but it's closely tied to the recent developments in information theory (through the concept of *randomness*). The idea is to present in a concise form one prolonged and ongoing research on historical and scientific notions of the *contingency paradigm*. The end product has a more general stake because it is a proposal of a certain *typology*. By **paradigm of contingency** I mean five different concepts which have interesting and complex inner relations:

1. **Contingency (as an alternative)**. This is the main notion to deal with. It encapsulates all others enlisted below and serves as their conceptual *foundation*, so the list presented here is *hierarchical*. In a strict contemporary sense the term "contingency" refers to the *status of propositions* that are neither necessarily true nor necessarily false (in this case necessarily means "under every possible valuation"). It can be traced back to Aristotle who has stated that the domain of rhetoric deals explicitly with contingent and relative matters but the problem with future contingents and its logical interpretation became both prominent and a long lasting one; and rhetoric couldn't provide a satisfactory answer. We can find slightly different but essentially similar definitions in such a vast range of thinkers (St. Aquinas, Leibniz, Laplace, I. Kant, G. Hegel, A. Kolmogorov, R. Carnap, David Lewis, Saul Kripke, Timothy Williamson…) that it becomes almost trivial to say that in *a broad sense everything which could be otherwise is contingent*.
2. **Accidental (as a cause)**. This is typical philosophical (non-scientific) notion which can be traced back again to Aristotle – it presupposes contingency as such but it is a *causal concept*. That's really important because contingency itself is neutral with regard to causal/non-causal distinction. So everything accidental is by definition contingent and this is the first time when causality enters the domain of contingency. But

at the moment *chance* was settled as mathematical notion (see 4) the term "accidental" started to lose its explanational and ontological status.

3. **Coincidental (as a reference)**. This is the most widespread and ubiquitous usage when we think about contingencies in our everyday life. Of course, it is the vaguest among the terms presented here and there is a reason for this. Unlike all other concepts there's almost no *common* mathematical or abstract reasoning behind analysis of this term. The general picture is that coincidence, although definable and having rich history, is still undeveloped concept. It has disparate meanings which go astray in all directions – *τύχη* as overlapping of independent processes (Aristotle), the popular esoteric idea of *Synchronizität* (Carl Gustav Jung), the empirical and psychiatric notion of *Apophänie* (Klaus Conrad, Peter Brugger), or *patternicity* (Michael Shermer)... But from philosophical (and historical) point of view we can still characterize *coincidence in a broad sense as "overlapping or connection between independent processes or facts"*.
4. **Chancy (as a process)**. In the middle on 17th century there was an unexpected and important turn influenced by the imminent progress in mathematics. For the first time the proto-concept of chance (in a mathematical sense) was introduced by Blaise Pascal and Pierre Fermat in their famous correspondence. What is chancy is of course contingent, but not every contingency can be reduced to chance or calculation. From then on the theory of probability took shape (Bernoulli, Laplace, Kolmogorov); slowly but gradually it became more and more important and valuable not only in relation to gambling and statistics, but even to *classical physics*. We can easily state that "chance" is one of the leading conceptual terms (and cultural metaphors) in 20th century. *In a broad sense we talk about chance(s) when from identical initial conditions different outcomes can be expected*.
5. **Random (as an object or a product)**. After the middle of 20th century Andrei N. Kolmogorov, Per Martin-Löf and Gregory J. Chaitin independently from each other came upon a different problem within the contingency domain and proposed a solution which is now regarded as a crucial backbone within the theory of information (their research was related and influenced by the works of Richard von Mises). The idea was that we can think of contingencies not only in the means of the process itself (something compulsory when talking about chance) but we can deal with the outcome itself, *without knowing anything about its cause or its process*. So the concept of "product randomness" has emerged. The main achievement though was not the establishment of this new concept but rather the measurement of randomness through compression. Nowadays randomness plays a significant role in the foundations of a number of scientific theories and methodologies and it's a pity that the term was largely neglected in the philosophical literature. *In a broad sense what cannot be compressed anymore is random* (it has common application in computer technology).

So, the paradigm of contingency has at least 5 main trends but we can group them in several different ways – through causal/non-causal or formal/non-formal distinctions. *I would like to present in my paper these inner relations too (based on historical development of the notions)*. If we clarify the differences *between contingency proper, accidental cause, chance, randomness, and coincidence* it will be easier to explain why they often *seem* to overlap in their applications.

Every typology faces standard difficulties which are mostly unavoidable: It simplifies in a radical way complex historical and conceptual structures, tendencies, contents and so on. On the other hand, (and if it's developed properly) it should help to settle some debates or rather channel them in a more precise way.

Transformations of Social Role of Science in the Knowledge Society

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The core of this paper is a transformation of standards and nature of scientific activity regarding intensively changing social role of science. This is a problem of transformation of social role of science and how it changes the standards of scientific activity, scientific creativity, its nature and structure.

Unlike classic science, nowadays specifics of scientific activity have an obvious social and collective nature and growing orientation to application of scientific knowledge to improve technologies. At the same time "knowledge societies" undergo changes in social area because knowledge transforms into human habitat. Various scientific societies become more active subjects of modern science. Social role of science grows and transforms not only the environment and society, but also science itself. The vividly expressed social nature of modern scientific knowledge is confirmed not only by social organization of science, but different manifestations of sociality such as competition between various research programs, theories, schools, and continuing battle for priorities, scientific leadership, professional and social acceptance, funding, investment attractiveness etc. Science increasingly loses the principle of universality (universal accessibility) of scientific work and its result. The mechanism of production and consumption of scientific and technical knowledge undergoes transformation. The knowledge economy can exist only in society in which obtaining and application of knowledge should be defined not only by their applied nature and economic effectiveness but also by the various ways of infiltration of this knowledge in the human life. Today community of scientists is a community of socialized participants of public discourse. If socialization is understood as a versatile and collective process of construction of identity, formation of social experience in which special attention is paid to subject's activity in the process of socialization, then this approach can be applied to understanding of specificity of activity, for example, of groups and organizations.

This is the main question of the paper – how does social nature of scientific inquiry impact on goal of science itself, which is achievement of objective knowledge; how it contributes to the achievement of this goal? How does growing social role of science influence structure of scientific creativity and scientific work? How do such social realities as competition and effectiveness impact on process of scientific creativity and its results? Does the percentage of subjectivity of scientific knowledge decrease on account of increasing of a role of communicative components in the process of obtaining and assessment of knowledge by collective subject of modern scientific knowledge? What is a collective subject of scientific inquiry and how is problem of its identity by analogy with the problem of identity of scientist solved?

We need to clear how four imperatives of scientific ethos, formulated by R.Merton, remain today as the same ethical regulators of scientific creativity. Largely objectives of research are connected to review and present status of each imperative of CUDOS, and revealing of the extent of ambivalence in scientific work linked to influence of changes in the social role of science in the last years. It is related to a question of what should we consider as a result of scientific activity? In this context profit, in fact, is accepted as value orientation reducible to the concepts of "success" (achievement of results close to programmed as a goal) and "effectiveness" (achievement of results with minimal costs). However, acceptance of profit as a value orientation generates serious moral contradictions when it is interpreted as benefit in general or as moral good.

Our hypothesis is that an extent of enforcement and necessity in scientific activity should remain on a certain acceptable minimal level and should not be defined by growing social role of science and other external circumstances and factors. Merton wrote that "in societies like ours, the pressure exerted by desire of success connected with the conquest of prestige, leads to elimination of effective social restrictions in a choice of measures applied to achieving of this goal". In this context special significance is gained by social dimension of science and problem of scientific creativity, not exclusively from point of processes of "pure" knowledge, but from point of a social requirement for particular knowledge.

What is a Future of Value?

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Don Marquis' Future of Value (sometimes also Future Like Ours) argument against abortion is widely cited as one of the most prominent anti-abortion arguments in the abortion literature. Its appeal is said to derive from several positive features. If correct the argument is said to give an account of the *prima facie* wrongness of killing adult human beings like you and me. It also claims not to require a full-fledged psychological account of personhood, as Marquis claims such debates often end in stalemate. But perhaps even more interestingly, the argument does not require the adoption of any particular religious doctrine to support the assumptions it makes. Unlike its religiously inspired rivals, the Future of Value argument begins with a set of seemingly plausible assumptions that appear to require no supernatural commitments to generate its conclusion, thus making it an argument whose assumptions can be accepted by both religious and non-religious people alike. In this respect, Marquis believes his argument is not only consistent with a naturalistic outlook, but indeed supported by biological considerations. In this essay I wish to argue against this last claim. More specifically, I survey the current evidence for positing the existence of a special kind of property, the property of *having a future of value*. According to Marquis, a future of value is a special kind of dispositional property whose existence explains the truth of certain sorts of counterfactuals - if *a* had not been killed at a time t1, then *a* would have gone on to value at a later time t2 - that are required for his account to work. Although there is some theoretical evidence for positing such a property, e.g. from the ethical or metaphysical domains, there is currently little to no direct biological evidence to support the claim that this property is biological in nature. I then go on to recast the discussion in terms of current cluster kind theories, and I argue that there is simply not enough evidence for supposing the property to be robustly associated with fetuses to support the truth of counterfactuals Marquis needs for his theory to work.

SYMPOSIUM

EXPLANATION AND UNDERSTANDING IN SCIENCE

Explanation is recognized both, as an important goal of science, and as a complex activity aimed at deepening our understanding of the world. Since the publication of Hempel's and Oppenheim's classical papers on D-N and I-S models of scientific explanation (Hempel & Oppenheim 1948; Hempel 1965), the literature on explanation has grown enormously. The original aspects of the logical structure of explanatory models have been progressively supplemented by important methodological details of distinct categories of explanatory factors (e.g., causal processes of Salmon's (1984) CM model; Kitcher's (1981; 1989) unification schemas; van Fraassen's (1980) pragmatic theory; or Woodward's (2003) interventionist framework, etc.). Current philosophical conceptions of explanation focus on different levels of explanation (representative, ontic, cognitive), it's formal and non-formal aspects (argument structure, description of mechanisms or mathematical properties of models), it's modal features (interventionism, counterfactual dependencies in highly idealized models, possible world modality, and in general analysis of causation) and they reflect the diversified explanatory practices in special sciences.

In particular, there are tight conceptual connections between the notion of explanation on the one hand, and the notions of theory, models, laws, causality, probability, dispositions, counterfactuals, reasons, rules etc., on the other hand. Second, the search for a general theory of explanation has been substituted by a more pluralist picture based on the assumption that different domains of phenomena require distinct explanatory strategies (regularities vs singular event, dynamical or functional vs mechanistic). Finally, the exact relation and the role of understanding in scientific explanation is not yet fully understood. Some philosophers hold that understanding is a subjective psychological by-product of an objectively adequate explanation (Hempel 1965) and that good explanations don't require understanding (Trout 2002). Others deem those two categories as (relatively) independent: they point to the fact that, on the one hand, there are explanations without understanding of the phenomena being explained and, on the other hand, there is understanding of things (e.g., some non-causal or mathematical facts; Lipton 2009) which we do not get to as a result of any other explanatory procedure (e.g. two-dimensional semantics approach in Canberra plan).

The organizers of this symposium aim at bringing together some of those different methodological approaches to explanation and understanding. The participants will address in their papers the key questions of the relation between explanation and understanding:

i) What makes a non-causal explanation explanatory?
ii) How do dispositional explanations relate to causal explanations?
iii) How do we reach understanding via scientific models?
iv) In what sense do causal notions provide an understanding?
v) What role do social rules play in action-explanation?

The symposium papers address some of these relevant issues of explanation, understanding and their relations in natural sciences, social sciences, humanities and mathematics.

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Why Dispositional Explanations Should Not Be Construed as Causal Explanations Even if Such a Construal Is Possible

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Causal explanations and dispositional explanations allow for drawing different inferences about the explained phenomena and thus they contribute to our understanding of these phenomena in different ways.

If, for instance, we know that "B, because A", and we interpret that as a causal explanation, this allows us to infer:

(a) If A didn't occur, B would not occur as well. (If we are committed to the counterfactual account of causation), or

(b) $P(B|A) > P(B|\text{non-}A)$ (if we are committed to the probabilistic account of causation),

and so on.

If, however, we interpret "B because A" as a dispositional explanation (e.g. as "B is a manifestation of A"), knowing it will allow us to draw inferences which are different from (a) and (b). We are entitled e.g. to expect other known manifestations of A if we vary the background conditions. We, however, are not entitled to such expectations if we construe the link between A and B as causal insofar as causal claims are ceteris paribus claims, they imply nothing if we change the background conditions.

Therefore, if we value the special way dispositional explanations contribute to our understanding of explained phenomena, we should not try to construe them as causal explanations, even if such a construal seems possible, as the causal construal of dispositions would obliterate the inferential benefits of dispositional explanations. This

argument has important methodological implications for the discussions on personality trait explanations in psychology where traits are largely recognized as dispositions.

Explanation and Understanding in Highly Idealized Topological Models

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Topological explanations are derived from network or graph models. Such models are network or graph idealizations of real systems, which normally have two elements: vertices or nodes and edges or links/connections. A graph is a set of vertices connected through edges (Newman 2010). Vertices in different fields of investigation may represent different things. In neuroscience vertices might represent neurons or brain regions and the edges, the synapses or functional connections between the regions.

For example, stability or computational economy of the brain or ecological communities are explained by an appeal to the fact that a network model that represents them has fewer hubs (highly connected vertices) and at the same time high clustering as a local topological property with fewer short path length connections between hubs.

The features of network topology allow us to understand the system dynamics as a function of its structure. This explanation is specifically non-causal because the explanans cites only mathematical values of the structure. This renders the topological explanations as mathematical in both Batterman (2010) and Lange (2013) sense. In Batterman sense they don't cite any causes but explain by abstracting away from such details. In Lange's sense they are mathematical because their modality is based on some sort of non-causal dependency (posited as mathematical necessity) rather than on dependencies based on causal relations.

But the question arises whether simply understanding topology suffices for explanation, or something more is required. Craver (2016) and Bechtel and Levy (2013) maintain that topological explanation has to show how topology is actually embedded into the causal structure, or what in a system gives rise to particular topology.

I argue that such details are not required and that just understanding topology or in general some mathematical features of models suffices for explanation.

Some 'Aspects' of Scientific Understanding in a Modeling Context

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Almost neglected in the classical literature on explanation, the topic of scientific understanding got momentum in recent research. Current positions on scientific understanding might be put, roughly, into two opposing camps: one emphasizes its close connection to scientific explanation and explanatory knowledge; the other advocates a decoupling of understanding from explanation and a non-reductive conception. The first view dwells on the possibility of reducing understanding to explanatory knowledge and its related epistemology (Khalifa 2012,

Strevens 2013). The other usually involves a search for non-reductive elements that are implicitly present in explanatory knowledge, but nonetheless are distinct from explanatory knowledge. Possible candidates for such non-reductive elements could be skills and abilities (de Regt 2009), a comprehension ability that goes beyond the semantic one (Newman 2014), the skill to model emergent properties of complex systems (Wilkenfeld&Hellmann 2014), or the skill to devise implicit models with artificial neural networks.

In this contribution we will focus on issues related to the scientific understanding that could be achieved in a modeling context. Models (and the related model-based understanding) are often invoked by non-reductionists (Wilkenfeld&Hellman 2014, Lipton 2009) in their confrontation with the "redundant view" advocates (Khalifa). On the other hand, the "redundant view" does not so often discuss models. In fact, as our argumentation will suggest, an appeal to models might not be as conclusive as to provide a straightforward support for any of these positions. By studying and making the case of computational models, we will analyze the chances that models provide both scientific explanations and scientific understanding. From there, we will extract some consequences and suggest ways to reconsider the topic by taking seriously the idea of non-explanatory understanding (Lipton 2009).

From Reason-Explanations to Rule-Explanations

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In "Aspects of Scientific Explanation" (1965), Hempel presents a unifying theoretical framework for explanations in both, natural and social sciences. He suggests that the difference between explanations of natural phenomena (*qua* particular occurrences) and explanations of human actions consists in different theoretical components of explanatory arguments: In case of natural phenomena, we invoke the relevant law(s) of nature, whilst in explaining human acts we refer to agent's reasons and some rationality principle(s).

In my paper, I elaborate on Hempel's account of rational explanation. I propose a complementary account according to which some actions are adequately explained by making their social or institutional framework explicit.

First, I introduce the components of this explanatory account: an agent A, social role S, a set $\{P_i\}$ of rules which underlie the practices of agents having the social role S; a hypothetical rule $p \in \{P_i\}$ which is to be related to a given action; the initial conditions C to which rule p applies; and, a social fact or an institutional action which is to be explained (i.e., *explanandum*). Then I explicitly specify some minimal constraints imposed on these elements and formulate the idealized assumptions which I am committed to.

Second, I show how these different components glue into a coherent explanatory account. On this view, to explain certain action E as a social or institutional action consists in specifying i) the actual social role S of an agent A, ii) the relevant rule(s) p belonging to some institutional framework $\{P_i\}$ of rules governing action E, iii) the initial conditions C to which rule(s) p applies, and iv) an assumption that agent A acted on p. Putting these components together amounts to a reconstructed argument (of deductive or inductive form) the conclusion of which is, simply, the explanandum-action (sentence).

Moreover, Hempel argued that rational principles in reason-explanations are empirical (*qua* being dispositional) rather than normative. I reconsider this question and show how it is possible to sustain both – empirical and normative – modes of rule-explanations. In particular, I argue that we can distinguish explanations of *implicitly* rule-governed actions as *dispositional*, and hence, *empirical*, and those of *explicitly* rule-governed actions as *normative*.

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