1. Christopher Austin: The Genetic Realisation of Dispositional Properties

In contemporary metaphysics, dispositional properties have become ubiquitous in philosophers’ ontologies - they have been utilised to offer an analysis of everything from colour to de re modality. In the context of the philosophy of science, although they are now well-entrenched in contemporary analyses of the causal structure of the “fundamental” entities of physics – particles and fields -, they have yet to be put to much work in any such analysis of the “fundamental” entities of developmental biology – genes. This paper aims to make that application, by arguing that genetic causation is best conceptualised as dispositional causation. There hasn’t been a shortage of recent philosophical discussion concerning causality in the genome. Interestingly, most discussions have focused on what the causal role of genes is not: genetic causation is not deterministic (with respect to phenotypic traits), cannot be characterised by a semantically rich information theory, and is in no way “privileged” with respect to the processes of development. It is not the aim of this paper to offer any novel arguments for (or against) any of the those aspects of genetic causation. Instead, its goal is to elucidate the character of the causal role of the genome by showing that the widely accepted and mostly uncontested facts about the character of genetic causation fit rather neatly into an already well-explored theoretical framework of particular, causal properties – namely, dispositional properties.

The paper has two parts. The first part presents an abbreviated overview of the nature of dispositional properties, focusing on four specific features of those properties’ causal role: their individuation via stimulus/manifestation states, their functionally defined co-variance connective and its role in inference licensing via truthmaking subjunctive conditionals, their particular “causal strength”, and their quasi-representational goal-directedness. Comparing those four features to the type of causal structure that the genome possesses is the task of the second part of the paper, where it is argued that those features are present in the characteristic causal process of genes – the production/determination of polypeptide chains. The conclusion of this paper is that there is one particular type of property whose corresponding causal structure neatly matches-up with the empirical data concerning the genome and its relationship to the proteome - dispositional properties. That being the case, we ought to expect that conceptualising genomic causation as dispositional causation will allow the theoretical soil of the philosophers’ ontology to enrich the future fruit of our best empirical models.

2. Olle Blomberg: Team agency, framing and Frege cases

Proponents of team reasoning argue that it matters how agents “frame” or conceive of their decision problems. Team reasoning is a mode of practical reasoning that agents engage in when they frame a coordination problem as being a problem for them all as a team. It is practical reasoning in response to the question “What should we do?” rather than the more familiar “What should I
do?”. The answer is an ‘action profile’ that includes the actions of all team members, which maximises the chances of achieving the team's goal.

According to Pacherie (2011, 187), several agents' intentions to their part of an action profile form a “shared intention” if these are the outcome of team reasoning, where a ‘shared intention’ is a socio-psychological causal antecedent that makes a joint action intentionally joint:

Two persons P1 and P2 share an intention to A, if:

(a) each has a self-conception as a member of the team consisting of P1 and P2 (collective self-framing);
(b) each reasons that A is the best choice of action for all members of the team (team-reasoning); and
(c) each therefore intends to do his part of A (participatory intention).

Here, A is the team’s goal. The goal is that that the team enacts an action profile consisting of P1 and P2’s contributions. The agents’ notion of A cannot here be that of an intentional joint action, since this would introduce a vicious analytical circularity into the account (Pacherie 2013, 1822). Some weaker notion of team action must be in play. Gold and Sugden (2007, 128) avoid the circularity by taking A to be a “pattern of behavior that can be intended either individually or collectively, depending on the reasoning which led to it.”

I argue this account account faces a dilemma. It either fails to provide sufficient conditions for shared intention or fails to accommodate clear cases of intentional joint action that ought to be accommodated. If Pacherie allows that P1 and P2 frame the goal of the team in different ways (e.g. “that we catch the prey that rustle the leaves” versus “that we catch the prey that casts the shadow”), then the account fails to rule out “Frege cases” where P1 and P2 falsely believe that there is no single goal that each reasons is the best choice for all. Such cases are characterised by a form of mutual exploitation rather than by team agency. On the other hand, if Pacherie takes the framing of the goal to be fixed, then she fails to accommodate clear cases of intentional joint action where P1 and P2 represent the single goal of the team using different sensory modalities. Suppose P1 is blind and P2 is deaf. Arguably, that P1 and P2 represent the team goal differently shouldn’t undermine their catching the prey from being intentionally joint.

I argue that Pacherie can avoid both horns of this dilemma by introducing the following extra condition:

(d) each believes that A is a single goal that each member of the team reasons is the best choice for all (belief about single team goal).

Pacherie’s account then rules out cases of mutual exploitation but allows that participants may engage in intentional joint action while framing the goal of the joint action differently.

References
3. Adrian Boutel: Are Selected Kinds Causal?
In “Can Any Sciences Be Special?” (2010), David Papineau aims a novel kind of causal-exclusion argument at non-reducible kinds in the special sciences. Unlike Kim’s, Papineau’s argument does not depend on an objection to overdetermination—Papineau is friendly to Bennett’s (2003) answer—but on a problem with selected kinds.

The presence of multiply realisable kinds in special-science laws raises a problem of coincidence. If a high-level kind is genuinely non-reducible—that is, there is no relevant physical-level property which all members share—then it would seem massively coincidental for the members to share a common effect.

Like Block (1997), Papineau takes selection to offer an answer. It is not a coincidence that physically heterogenous causes have a common effect, if they were selected for that effect. But, Papineau argues, high-level kinds that are the product of selection cannot be causally efficacious. Their realisers are sufficient to produce the relevant effect, and having that causal power is also why the realiser was selected. Merely grouping causes of X together does not a new cause of X make. So high-level kinds are either reducible (and so not proprietary) or epiphenomenal.

I contend that this argument neglects an important aspect of counterfactual sensitivity to the presence of the high-level kind. To be sure, Papineau is right that such counterfactual sensitivity is not enough by itself. A rickety car fails to start because its ignition wire has come loose. Had the car not been rickety, other things being equal, it would have started. But still one would not say it was the ricketiness—a property shared with cars with wobbly wheels—that caused the non-starting. Reconnecting the ignition would have solved the problem, regardless of whether the car was otherwise rickety.

It is different, though, where a “non-redundancy” condition is satisfied: where wiggling the low-level cause such that the high-level phenomena is still realised preserves the effect. In such cases, the high-level phenomena is what makes the causal difference. To cite the low-level cause is still to cite a cause, but one containing irrelevant implementational detail: it is not required for the effect, in terms of Yablo’s (1992) analysis of proportionality. On this basis we can say it is because I have a pain—understood as a token of a selected kind—that I move my arm; my C-fibre activations are involved, to be sure, but any other pain mechanism would have led to the same result.

Note that this is not yet to appeal to a law relating pains to avoidance. The relevant counterfactuals can be supported at the realiser level. Indeed, such generalisations about the low-level counterfactuals arguably justify the introduction of high-level laws.

It is important to resolve this issue, because in other respects Papineau seems to be right—selection is the most likely source of high-level generalities in a physicalist world, and (together with reducible kinds and historical facts/boundary conditions) offers a plausible basis for the special sciences we have. (Indeed, it is more important that Papineau allows, since he is unduly optimistic about species-relative kinds such as human pain—it is not at all clear
that individual humans’ pain systems have any strictly physical uniformity in common, even an aggregative one.)

References:

4. Angelo Cei: The Epistemic Structural Realist Program. Some interference
The paper assesses the prospects of Epistemic Structural Realism (ESR) to constitute a sound realist response to antirealist preoccupations raised by deep historical changes in science. This aim is achieved by contrasting various forms of ESR with a case of theoretical change in the history of physics. In particular, I will devote my attention to the explanation of the Zeeman effect offered in Lorentz Theory of Electrons (see 1952) and how it looks from the perspective of Relativistic Electrodynamics. ESR nowadays features a variety of alternative views (See for instance Votsis, 2005 and Worrall,1989). I begin by highlighting the common features between such views and then I investigate the consequences of a conceptual problem that the case study makes evident. I argue that while this case seems favourable for an ESR approach it puts a considerable stress on the positions available. I finally show that the case at hand suggests an alternative version of Structural Realism that addresses the concerns in point.

References

5. Marc Champagne: Timelines and the growth of historical knowledge
Historians study events that are no longer observable, so representing those past occurrences as sequential points on a line is a natural strategy. Depictions of duration may be arbitrary (e.g., there is no reason why the span of a year should measure, say, a centimeter), but ratios of duration are not (e.g., twice as long in time should mean twice as long on paper). Can this way of conveying historical knowledge lead to further discoveries? With different emphases, linguists (Simone 1995; Van Langendonck 2007), logicians (Burch 1991; Shin 1994, 2002; Hammer 1995; Allein & Barwise 1996; Pietarinen 2006, 2010, 2011, 2012), historians of mathematics (Nets 2004; Mancosu, Jorgensen & Pedersen 2005; Giaquinto 2007), semioticians (Stjernfelt 2007, 2014; Bordron 2011; Dondero & Fontanille 2012), and cognitive scientists (Glasgow, Nara y Anan & Chandrasekaran 1995; Hoffmann 2010a, 2010b, 2011; Magnani 2011; Nakatsu 2010) have all recognized that a better understanding of reasoning by icons,
specifically diagrams, is crucial to understanding problem-solving, inference-drawing, and hypothesis-making. Yet, few have studied the role that such diagrammatic representations play in a discipline like history. Although timelines have long been part of the historian’s toolbox, one reason “for the gap in our historical and theoretical understanding of timelines is the relatively low status that we generally grant to chronology as a kind of study. Though we use chronologies all the time, and could not do without them, we typically see them as only distillations of complex historical narratives and ideas” (Rosenberg & Grafton 2010). I surmise that a better appreciation of the cognitive function of timelines can reshape how (or whether) we view history as a science. History, we are often told (notably by Windelband 1919), can catalog discrete facts, but it cannot induce law-like generalizations from them. The addition of prose is tolerated as a necessary evil, but museums are tasked with housing the original indices that prevent scholarship from lapsing into purely fictional discourse. I want to argue, though, that the use of timelines in textbooks and specialized monographs challenges this view of historiographic practice, or at least shows its limitations. Like all images, lines let viewers scan them at various places. Importantly, this back and forth perusal is not merely visual, but also supports inferences. This inferential potential becomes more pronounced when parallel timelines split events into two or more groups so as to make explicit both thematic unity (on a line) and disunity (on a different line). The diagrammatic organization at hand forces interpreters to reassemble the data, thereby revealing connections (particularly interdisciplinary ones) that might have otherwise gone unseen. If we do not know in advance what conclusions parallel timelines will permit us to draw, then these sign-vehicles do more than recapitulate what is already known. I believe this surprising feature merits further scrutiny. Thus, by means of step-by-step case studies of timelines pertaining to art, politics, and technology, I will show how historical claims can sometimes be prompted by (and answerable to) a neglected iconic dimension.

6. Jean-Marie Chevalier: Diagrams as a tool for scientific reasoning
A great deal of philosophy of science discusses the various kinds of scientific reasoning. In particular, scientific discovery would be provided by a sort of ampliative reasoning per se, often called abduction or retroduction. According to such a conception, there are at least three basic reasonings, deduction, induction and abduction, which all play a part in “the” scientific method. Contrary to this view, I put forth the idea that there are no three kinds of scientific reasoning, but that deduction, deduction and induction are three modes of exposition. They are argumentative forms and ways of presenting knowledge. In particular, they do not represent, manifest or consist in a transition from premises to conclusion, but only present which knowledge can be founded on which. Such an Aristotelian interpretation, where syllogism is only a teaching technique, contrasts with the usual view that science indeed proceeds through various kinds of inference. The so-called inferences are ways of ordering knowledge already at hand. That is why the same knowledge, according to the way it is ordered, can be called deductive, inductive or abductive. Thus, the problem of the “logic of scientific discovery” is as relevant for abduction as for the other reasonings. Then occurs the problem of the manner we really reason in science. The hypothesis defended here is that we observe varieties of diagrams, or topological
models, that relate the relevant features extracted by the “ground” the ones to
the others. As for the inferences, one leading piste is that they are spontaneously
drawn like mental associations, and selected only in a second step according to
their rationality.

7. Michael Cuffaro: On the limits of classical computational systems
Bell’s and related (in)equalities are often portrayed as ‘no-go theorems’. They
are taken to demonstrate, that is, that no hidden variables description of
quantum systems can be consistent with quantum measurement statistics while
at the same time being locally causal. Without further qualification, however,
thinking of Bell’s and related (in)equalities in this way can be misleading, for
Bell’s and related inequalities are not no-go theorems /per se/. Rather, they
specify /constraints/: they tell us what a locally causal description (LCD) that
accounts for a joint probability distribution must be like; i.e., what general
properties it must satisfy. In order to make a meaningful distinction between
what is and isn’t ruled out by these (in)equalities, however, then we must (or so I
will argue) consider the context in which they are being discussed. In particular,
we must consider an additional set of constraints associated with a given
context, according to which a given LCD will be judged to be plausible or
implausible in that context.

Normally these constraints are left unstated, and this is mostly harmless
most of the time; i.e., the necessary qualifications are, in typical discussions of
the foundations of quantum mechanics, implicitly understood by all. Thus in the
context of traditional foundational discussions of quantum mechanics, a grossly
conspiratorial LCD, which reproduces quantum measurement statistics by
employing a small amount of hidden subluminal classical communication, will be
judged to be wildly implausible despite the fact that it is able to account for these
statistics---we will judge it, that is, to be a loophole not worth closing.

But the situation can change once we leave the traditional foundational
context. In particular when we are concerned, not with alternative theories of
the natural world, but with what /we/ are capable of doing with the aim of
classically reproducing the statistical predictions of quantum mechanics---with
the classical physical systems, in other words, that are possible for us to /build/
with the aim of reproducing these statistics---then such ‘conspiratorial’ LCDs
should not be judged as implausible, so long as the amount of hidden subluminal
communication required to reproduce the quantum mechanical predictions is
‘easy’ (in a complexity-theoretic sense) to realise computationally.

This has, I will argue, interesting implications. Thus consider the so-called
‘all-or-nothing’ Greenberger-Horne-Zeilinger (GHZ) equality. In the traditional
foundational literature, the GHZ equality is often held up as a more powerful
refutation of local causality than Bell’s, for while the GHZ equality can be shown
to be violated using a single quantum experiment, the violation of statistical
inequalities like Bell’s requires repeated quantum experiments to demonstrate.
Nevertheless, there is a broader sense in which Bell’s inequality is (or so I will
argue) more fundamental than GHZ, for when we move from the traditional to
the /computational/ context, the ‘all-or-nothing’ GHZ theorem loses its force. In
the computational context it is only statistical inequalities like Bell’s which can
legitimately be turned into no-go theorems. Statistical---and not all-or-nothing---
inequalities allow us to see, therefore, that there are quantum statistics that /even we/ can't plausibly build classical systems to reproduce.

8. Christian Erbacher: The History of Editing Wittgenstein as an example for SSH studies

The studies of historical, cultural, philosophical and sociological aspects of the social sciences and humanities (SSH studies) is an emerging field of research. In a recent review, Christian Dayé (2014) identified 3 themes that are likely to play a central role in consolidating SSH studies: (1) positivism in SSH, (2) the impact of Cold War on SSH and (3) the adequacy of categories of Science and Technology Studies (STS) to describe techniques and practices in SSH. The present paper summarizes research results in order to show that and how a History of Editing Ludwig Wittgenstein’s Nachlass may shed light on all three of these themes and may thus provide an illuminating example for SSH studies.

(1) Wittgenstein published only one philosophical book during his lifetime, the Tractatus Logico-philosophicus. In a first phase of reception, the philosopher’s youth work has been read as providing a foundation for the positivistic movement. However, Wittgenstein left behind almost 20,000 pages of philosophical writings and appointed three of his former students to publish from them what they thought fit. During the subsequent decades, the three literary executors edited the volumes from Wittgenstein’s Nachlass that made Wittgenstein’s later philosophy available to all interested readers. Their posthumous editions showed that Wittgenstein stood in critical discussion with proponents of positivism and undermined their assumptions most fundamentally. Thus, contrasting the reception of Wittgenstein’s early work in the first half of the 20th century, the posthumously edited later philosophy encouraged the refusal of positivism in the second half of the 20th century.

(2) While positivistic philosophy was not politically neutral, but rather coupled with a belief in progress through science in the western world, Wittgenstein’s philosophy can be understood as criticism of exactly this scientistic conviction and the western way of life. This insight, however, was brought to light by Wittgenstein’s literary executors only under the influence of the political and cultural revolution in the late 1960s which led them to publish Wittgenstein’s remarks on civilization in the edition „Culture and Value“. Thus, the political developments during Cold War were decisive for the publication of a book which, in turn, changed the picture of Wittgenstein’s philosophy, presenting it as intimately intertwined with a man’s cultural criticism of a declined west.

(3) The work of Wittgenstein’s literary executors was far more than a philological treatment of inherited manuscripts. The driving force for devoting decades of work to editing Wittgenstein’s papers was the literary executors’ striving to do justice to the man they knew, to his philosophy and to his wishes for publication. The literary executors’ policies were thus not merely informed, but formed by their personal acquaintance with Wittgenstein’s philosophizing. This brings into focus the first importance of acquiring modes of conduct, practices and life-forms through the reflective activity of philosophizing. This paramount importance of téchne (rather than technology) may provide a suitable starting point for contrasting SSH studies with STS. However, as the history of editing Wittgenstein’s Nachlass proceeded, the rise of new technologies shaped also its course. In turn, digital editions of Wittgenstein’s
Nachlass contributed their share to the development of skills in digital editing and digital humanities.

9. **Samuel Fletcher**: Counterfactuals within Scientific Theories

The language of our scientific theories is rife with alethically modal statements. The truth of counterfactual conditionals concerning matters that scientific theories describe, however, is not adequately given by the application of standard possible world semantics. As developed by Lewis and others, this semantics depends on entertaining possible worlds with miracles, worlds in which laws of nature, as described by science, are violated. This is clearly unacceptable if one is interested in evaluating certain counterfactuals not as sentences broadly of natural language, but more narrowly as propositions concerning only the connections between possibilities warranted by particular scientific theories.

It is clear that many scientific theories do describe with mathematical precision the possibilities they warrant, and the practice of science itself often involves introducing additional structure on these possibilities to represent relevant similarities among them. These structures include so-called uniformities, which are used to introduce the concept of a uniformly continuous variation. Any uniform space—a collection with a uniformity—turns out to be a model of Lewis’s system of spheres (equivalently, his similarity measures), in particular his modal logic VWU. If the uniformity is separating—the uniform-structure analog of the Hausdorff condition from topology—then the corresponding system of spheres (similarity measure) yields Lewis’s modal logic VCU. (For both cases in general, the so-called Limit Assumption does not hold.) The possible worlds, however, are all consistent with the scientific theory of interest, so evaluating counterfactuals using them does not require entertaining miracles.

The analysis here is in a sense the reverse of that often found in presentations of modal logic: instead of providing a system of axioms or inference rules for sentences with various modal operators, and then proceeding to find mathematical models thereof, my approach is instead to look to the practice of the mathematical sciences, identifying the kinds of structures placed on the models of a scientific theory that are used, if only unsystematically, in alethetically modal scientific reasoning, and then point out that these structures allow one to define counterfactual conditions satisfying familiar axioms.

The advantage of this approach is that it provides the means to answer (at least in part) one of the difficult questions about possible worlds semantics: whence the similarity measure? Even in discourse internal to a scientific theory, there will typically be no canonical notion of similarity amongst the models of that theory. Nevertheless, the context of investigation can often determine which features of these models are relevant for answering a given question, and a similarity measure can then be constructed to respect these relevant features.

As an example of application, I consider the possibilities described by the theory of general relativity—relativistic spacetimes—and the context of empirically adequate approximation and idealization, e.g., evaluating counterfactuals such as, “If our universe were to have the (idealized) properties \( \{ P_i \} \), then our cosmological measurements would not be too different than they are.” In such cases, the relevant notion of similarity can be determined by
approximation of classes of certain observable quantities for certain observers described within these cosmological models.

10. Benoit Gaultier: Peirce on belief and explanatory hypotheses
In this presentation I want to examine the ground and validity of Peirce’s infamous and, arguably, strongly counterintuitive claim that “belief has no place in science”.

I argue that the most robust and interesting way of understanding Peirce’s thesis that scientific hypotheses are not to be believed is to connect it with his view of abduction and of the nature of explanatory hypotheses. For Peirce, when a scientist is trying to find an explanatory hypothesis capable of accounting for certain surprising facts F, the kind of question that is occupying her mind is: “Could the explanatory hypothesis H be the right one?”. The crucial point highlighted by Peirce is that endeavouring to answer this question is to be clearly distinguished from wondering whether the explanans indicated by H is the case, probably the case, or likely to be the case. Inquiring into whether H could be the right explanation of F is not wondering whether H is true, probably true, or likely to be true. Correlatively, when one judges or believes H to be such that it could be the right explanation of F, one does not judge or believes that H is, probably is, or is likely to be the right explanation of F. These are two radically different things according to Peirce. Believing that H is such that it could be the right explanation of F does not entail believing that H is the right explanation of F. It does not even entail supposing such a thing: crucially, believing that H is such that it could be the right explanation should not be confused with supposing or guessing that H is the right explanation.

If Peirce’s view is admitted, what is exactly the reason that one may and should want to test such explanatory hypotheses?

11. Emmanuel Genot and Agneta Guld: Holmes, Deduction and Inquiry: A cognitive approach to Hintikka’s interrogative model of inquiry
In this paper, we argue that Hintikka’s reconstruction of Holmes-like deduction remains incomplete without an explicit model of the inquirer’s awareness and memory. Providing an explicit model for these parameters is tantamount to use Hintikka’s model as a (partial) ‘blueprint’ for a cognitive architecture for interrogative learning, and suggests an approach to naturalized epistemology that actually follows closely Hintikka’s suggestion for “an epistemology without knowledge and without belief” (Hintikka, 2007, ch. 1). We delineate a learning-theoretic model that extends Hintikka’s interrogative model of inquiry, discuss the epistemological consequences of bringing in cognitive considerations, and the possible applications for the model.

It is commonly agreed among philosophers and logicians that what Sherlock Holmes calls the “science of deduction and analysis” (Conan Doyle, 1986:16) does not embody deduction in any technical sense, but rather some other form of reasoning where abduction and probabilistic reasoning take the lion’s share. However, Hintikka has argued that there is more to deduction in Holmes’ method than meets the eye (Hintikka & Hintikka, 1982). If one considers Holmes’ method as a form of information-seeking through questioning in which questions can be asked once their presuppositions established, then deducing the presupposition of a question from one’s background knowledge becomes
necessary for a question to be asked, and subsequently for new information to enter one’s reasoning. Hintikka’s model thus allows to capture Holmes-like deductions using formal proof methods, such as semantic tableaux (Hintikka 1999, 2007), and to characterize the strategies for deriving presuppositions as (part of) proof strategies.

Hintikka has on occasion discussed the role of the Inquirer’s “range of attention” relative to relevant questions (Hintikka & Hintikka, 1989), but it is represented in the model as a static parameter, i.e. as a set of presuppositions of yes-no questions that an inquirer is ready to introduce in her reasoning—or in the tableau-based reconstruction, a set of possible application of the Cut rule. We delineate a more dynamical model of the inquirer’s range of attention, where the set of questions introduced in an interrogative-deductive line of reasoning depends explicitly on the inquirer’s awareness of her environment, and her ability to recall information from memory. We compare our model with some cognitive architectures such as ACT-R and CLARION, and discuss the epistemological consequences of such a model, in particular relative to the conceptual analysis of belief, knowledge, and epistemic justification. In conclusion, we discuss possible applications of our model for the design of experimental protocols for studying: (1) collaborative inquiry learning, extending in particular protocols based on informal accounts of Hintikka’s model, as implemented in (Hakkarainen & Sintonen, 2002); and: (2) opportunistic information encountering, refining the analytic model of (Erdelez, 2004).

References:

12. Lars-Göran Johansson: Non-local correlations
The locality assumption is that if a sequence of states of two physical objects are correlated then, they must exchange information or else there is a common cause for the two object’s states. Quantum mechanics seems to be entail violation of this assumption. Moreover, several tests have been performed and there are strong reasons to believe that quantum theory is correct in this respect; hence
we must accept that there are non-local correlations in nature. But the locality condition is extremely hard to give up, it goes against our deepest metaphysical convictions.

In order to explain non-local correlations I propose that we reject the tacit assumption that the two spin half particles really are two individual objects, each well localised at a particular place. Quantum theory indicates that two particles constituting a spin pair are not really two distinct objects, but rather one such object, suitable called ‘spin half system’. Quite independently of non-local correlations we have very strong reasons to believe that spin particles, and in fact all ‘particles’ usually so called in quantum theory, in many circumstances are not well localised at certain places. They propagate much more like waves, considerably spread out in space. Only when interacting by emitting or absorbing photons are they confined to well-defined small regions of space. This means that two spin –half particles in a singlet state (where non-local correlations obtain) should not be thought of as ‘particles’ situated at different places without being in contact. Their respective wave functions overlap to a considerable extent, even if the bulks of the two wave functions are distant apart. So it is a mistake to think that two particles somehow exchange information at some distance without sending any signal. It is more correct to conceive of them being ‘inside each other’, like two clouds being merged, and there is a mutual adjustment between their spin states.

One may also hold that the two spin half particles constituting a singlet state are not really two different objects. One must have some principle for individuation and identity for saying that they are two different objects, and that is wanting. From a logical and semantical point of view it is the singlet state made up of two particles that is the least well defined entity one can talk about, and this conclusion is not dependent on the existence of non-local correlation; this conclusion may be arrived at just by considering the use of the word ‘particle’ in quantum theory. (The reason why we nevertheless can count quantum particles is that conserved quantities, such as charge, or energy, is quantized. But quantisation of charge or energy, doesn't entail individuation of these minimum portions of quantized quantities.)

The conclusion is that our bewilderment when confronted with non-local correlations is due to two tacit assumptions: that quantum particles are well localised objects, and that they are individuals. Both these assumptions are wrong. So non-local correlations in quantum theory do not conflict with our fundamental assumption that if the states of two distinct objects are correlated, then these object’s states are either determined by a common cause or the objects are in contact.

The non-local correlations observed in singlet states is better conceived of as an quantum analogy to the trivial fact that the state of the two sides of a coin are strictly correlated in coin flipping: if the head comes up, the tail comes down, and vice versa. Head and tail are not two different objects but rather two parts of one single object, the coin, as long as it is not destroyed. In my talk I will elaborate the details of this argument.

13. Saana Jukola: Meta-analysis and the ideals of objectivity
Meta-analysis is a method of synthesizing information from two or more studies by using statistical techniques. In evidence-based medicine and policy, meta-
analyses are often placed on the top of the evidence hierarchies, which represent the assumed strength of different types of evidence. Meta-analyses are thought to provide more precise information on the effects of treatments than individual studies (Cochrane Collaboration 1.2.2.) and to amalgamate evidence in a less biased way than other means of synthesizing studies. This is because the formal rules of meta-analysis are supposed to ensure the objectivity of the process.

In his article (2011), Jacob Stegenga has argued that meta-analyses fail to be objective because conducting them involves making judgments. For instance, when choosing what primary data to analyse, a researcher needs to consider at least the following questions: What methodological quality criteria should the included studies meet? How to solve the problems caused by publication bias? (Stegenga 2011: 500–504.) In this paper, I show that Stegenga’s reasoning is based on the so-called procedural ideal of objectivity, according to which judgments necessarily threaten objectivity. I shall argue that the ideal of procedural objectivity as the guiding rule in medical research should be abandoned. This is because the ideal, on the one hand, is practically unattainable, and, on the other hand, does not help to evaluate all of the practices that are relevant in producing reliable medical knowledge. For instance, Stegenga himself discusses publication bias and the lack of evidential diversity, i.e., basing treatment guidelines on evidence from randomized controlled trials only. The ideal of procedural objectivity does not fully capture why these issues are problematic, and thus does not give us tools for counteracting them.

The use of the concept “objective” is eminently complicated, as recent philosophical (e.g., Douglas 2004) and historical (e.g., Daston & Galison 2007) analyses demonstrate. In this paper, the focus is on the practical consequences of different understandings of what kinds of practices ensure objectivity, particularly in the context of medical research. By introducing a case in research on the possible suicide risk related to the use of selective serotonin reuptake inhibitors, I demonstrate the weaknesses of the procedural ideal of objectivity. In addition, I show why the so-called social view on objectivity succeeds better in accommodating 1) the way in which scientific research necessarily involves judgments, 2) the possible risks involved in research, and 3) the influence that the institutional context has on research activities. Adopting the social view helps us to see why the evidence produced by meta-analyses may be more reliable than the results of some other means of amalgamating evidence without having to adhering to the unattainable ideal of procedural objectivity.

References:

14. Munir Cem Kayaligil: There Is No Interlevel Mechanistic Explanation
The idea that mechanistic explanation (in neuroscience) spans multiple levels is one of the essential claims in Carl Craver’s writings — for instance, in his book

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Explaining the Brain (2007, chps. 5 and 6). Put simply, this claim amounts to an ontological separation of a mechanism from its components. It rests on the correct-sounding judgment that the behavior of a whole and the behaviors of its parts are different, where the former owes its existence to the latter. On this understanding, mechanistic explanation demonstrates this interlevel relationship — that is, it shows how the lower-level phenomena produces the higher-level explanandum phenomenon. In my talk I want to address the central problem with this line of thought.

A closer look at Craver’s elaboration of mechanistic explanation reveals an ambiguity with regards to what the whole is and what the mechanism is. We see that Craver suggests (in different places) both that the explanandum phenomenon (such as the depolarization of the neuron membrane) is a property of a whole (the membrane) and of a mechanism. Yet it is clear that the whole cannot be identical to the mechanism that produces the phenomenon in question (the membrane itself cannot be the explanation for the depolarization behavior). Moreover, the particular organization of components is not only taken (by Craver) to define a mechanism but also regarded as a property of the whole. These cast doubts on Craver’s understanding of the three important concepts of the mechanistic philosophy of science: namely, explanandum phenomenon, mechanism and organization.

I argue that Craver’s mistake is a direct consequence of conflating two different senses of “mechanism”. The concept can refer both to a certain causal process bringing a particular phenomenon of concern into existence and to a spatially extended, machine-like entity with known, specific functions (the entity can also be an ordinary machine). This mistake is in fact shared by many other philosophers associated with the mechanistic philosophy of life sciences (also labelled as “the new mechanistic philosophy”), as criticized by Nicholson in “The Concept of Mechanism in Biology” (2012). It leads these philosophers to wrong ontological commitments and this is markedly exemplified in Craver’s works.

Moreover, I join Patrice Soom (“Mechanisms, Determination and the Metaphysics of Neuroscience” [2012]) in rejecting the ontological distinctness of the explanandum phenomenon and the mechanism. I therefore ultimately aim for linking Nicholson’s and Soom's evaluations of the mechanistic philosophy. By an original illustrative example, I show how the two critiques complement each other.

Characterizations of mechanisms can be divided into two categories: according to broad characterizations, mechanisms are simply causal sequences (cf. Glennan 1996). Defenders of narrow characterizations hold that mechanisms are specific kinds of causal sequences (Andersen 2012, Garson 2013). Presupposing a narrow concept of a mechanism, I will discuss the question of how to distinguish between mechanisms and non-mechanisms by analyzing two suggestions: regularity and what I will call reverse regularity. I will develop a taxonomy of different kinds of mechanisms based on the two notions of regularity.

16. Insa Lawler: Does statistical null hypothesis significance testing provide knowledge?
Null hypothesis significance testing (NHST) is common in psychology, linguistics,
and the social sciences. By employing NHST one tries to confirm a research hypothesis (RH) indirectly by evaluating a so-called null hypothesis (NH), which usually states that there is no effect between two measured phenomena. The so-called alternative hypothesis (AH) is usually non-directional and just states that there is some effect. Importantly, AH is not RH, but only non-NH.

NHST measures the likeliness of the collected data given NH. This likeliness is stated in form of a p-value. If that is smaller or equal to a previously chosen value (often: 0.05), NH is rejected. This, in turn, gives ground for accepting AH.

Although NHST has been severely criticized for decades, it is still widely used. By carving out the epistemological implications of NHST’s flaws, I emphasize that this is problematic. I argue that NHST neither provides (i) knowledge of non-NH, nor (ii) knowledge of RH.

First, NH is usually believed to be false from the very beginning. But NHST is not apt for evaluating this belief, because it is sensitive to sample size. Using large samples, every NH can be rejected. Using small ones, some obviously wrong NHs cannot. Thus, NHST gives no good confirmation for non-NH. Even worse, NHST only gives the probability of the collected data given NH. From this, however, one cannot conclude that NH is likely to be false.

Even if NHST did provide (i), i.e. knowledge of non-NH, my second claim applies: first, if every NH can be rejected, NHST does not provide useful information for confirming RH. Secondly, (i) does not give good enough support for RH. RH can be false even if AH is true, because many RHs are compatible with AH; AH only states that there is some effect, but not which one. Statistically underpowered experiments yield even higher false discovery rates.

I further show that NHST does not fulfill any knowledge requirements of the dominant knowledge accounts in epistemology. NHST does not even render a true belief knowledge.

All this supports giving up NHST as research instrument and focusing on alternative statistical means – a step that has been repeatedly demanded.

17. Aki Lehtinen: Allocating confirmation with robustness
A result is said to be derivationally robust if it can be derived from several sets of assumptions. Derivational robustness concerns derivational relationships between assumptions and results, and as such it does not consist in collecting new evidence. Orzack and Sober (1993) have shown that the robustness of a result is irrelevant for evaluating its confirmation status. However, their argument only considers direct evidence for the robust result, but real models often have a large number of results which may confirm the robust result indirectly. Evidence is indirect with respect to a given result if the result does not imply the evidence but yet the evidence confirms it (see esp. Laudan and Leplin 1991).

I will show that derivational robustness of a result may increase the degree to which existing pieces of evidence indirectly confirm it. Given that models incorporate auxiliaries that are known to be false, models are typically modified and refined so as to see whether the auxiliaries are driving the results. As a result they spawn families of models with partly overlapping sets of assumptions. Individual members of a family of models typically share a set of assumptions that is sometimes called the common core.
A puzzle that still engages philosophers is whether our representation of space is entirely the result of learning from experience, or whether it is an innate capacity that is ready-made in our brain (or mind) from birth. A third option is to consider space as a product of both inner and outer factors that together make up our capacity for representing space, and for orienting ourselves in space.

In a study published in Science in 2010, it is argued that our capacity for spatial orientation is inner in the sense that there seems to be a ready-made (cognitive) structure in our minds enabling our spatial representation. Experiments on rats have shown that this inner structure consists of three distinctive types of cells with three distinctive functions: i) place cells that fire when a rat moves through a particular location within the environment, ii) head direction cells that fire when the rat’s head points in a particular direction, and iii) grid cells that fire in repeated discrete locations as the animal moves around in the environment.

18. Anita Leirfall: On the Discovery of the Concept of Space

How does the brain represent space?

A puzzle that still engages philosophers is whether our representation of space is entirely the result of learning from experience, or whether it is an innate capacity that is ready-made in our brain (or mind) from birth. A third option is to consider space as a product of both inner and outer factors that together make up our capacity for representing space, and for orienting ourselves in space.

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forming the vertices of a polygonal grid that covers the environment (global structure). Quite recently, so-called boundary vector cells were discovered. These are cells that respond to the presence of an environmental boundary at a particular distance and direction from the rat.

Interestingly, in these studies, the researchers refer to Immanuel Kant’s view on space. Both the recent Nobel prize winner John O’Keefe, who discovered the place cells, and his co-winners Edvard and May-Britt Moser, who discovered the grid cells, all refer to Kant’s arguments for how space is represented when interpreting their findings. This makes a case for looking further into Kant’s arguments on space.

Kant on inner sense – the discovery and organisation of concepts
In his work Concerning the Ultimate Ground of the Differentiation of Directions in Space (1768), Kant argues that we have a distinct feeling of an inner difference between directions like, for instance, left and right, in the subject. I will argue that this feeling of an inner difference is caused by a negative magnitude which is causally efficacious. Kant presents his arguments concerning negative magnitudes in his work Attempt to Introduce the Concept of Negative Magnitudes into Philosophy from 1763. In this work he argues that a negative magnitude is an effort, or a power, of the mind. Further, we become conscious of this power of the mind through an inner feeling. A negative magnitude is a special form of mental activity that is neither the spontaneity of a discursive thought nor a receptivity of the senses.

In this paper, I shall have a closer look at how we acquire knowledge. One way of acquiring knowledge is to generalise from particular instances. Ways of generalizing are by abstraction, classification, conceptualisation, like, for instance, Kant argues when it comes to our judgmental knowledge. Yet there is another way of acquiring knowledge, namely through an inner sense, or inner feeling, to which we have immediate access and which counts as a significant indicator for our knowledge about spatial properties like, for instance, spatial directions. This kind of knowledge does not concern our concepts or empirical laws. Rather, it concerns the discovery and organisation of our concepts.

Further, I will look into Kant’s arguments concerning our inner sense, or inner feeling, of spatial directions, in order to shed light on the question how we discover our fundamental concepts, like the concept of space and spatial directions.

References:
Immanuel Kant (1763): Attempt to Introduce the Concept of Negative Magnitudes into Philosophy
Immanuel Kant (1768): Concerning the Ultimate Ground of the Differentiation of Directions in Space

**19. George Masterton, Frank Zenker and Peter Gärdenfors:** Conceptual spaces, structural realisms, and continuity in theory change
Scientific theories postulate entities and relations. Scientific realism is the
position that the entities and relations so postulated are real in the sense that they both exist and are knowable. As new theories replace others in historical succession, however, new entities and relations may be introduced, while some old ones may disappear. The dynamics of scientific theories hence provides an explanatory challenge to realist positions in philosophy of science: How can the postulates of our best scientific theories be real if those same theories are constantly being replaced by better theories with different ontologies? Structural realism (Worrall, 1989; Ladyman, 2014) responds to this challenge with the claim that, even if the posited theoretical entities differ between predecessor and successor theories, there is nonetheless continuity in the structure of the theories.

A viable structural realist position in philosophy of science should be clear on the questions concerning (1) what theoretical ‘structure’ is, and (2) how continuity of such structure can be judged. Unfortunately, the answers to these questions in the literature remain abstract and are rarely rich enough to describe scientific practice adequately (Stanford, 2003). In reply to these questions, this paper presents arguments for the following two theses: (1) Conceptual spaces (Gärdenfors, 2000) provide a rich framework for identifying the structures of scientific theories. (2) By studying the types of changes in the underlying conceptual space that occur when one theory is replaced by another, the continuity in structure becomes more apparent than is typical among structural realists.

To make both theses more concrete, we will predominantly work with three physical theories presented in their phase-space formulations; namely Classical Mechanics (CM), Special Relativity Theory (SRT), and Quantum Mechanics (QM). While a theory’s phase-space does not exhaust its conceptual space, the phase-space does constitute its most central part. Thus, by treating the transitions from CM to QM and from CM to SRT in the theories’ respective phase-space formulations, the similarities between the conceptual spaces become evident and allow an initial assessment of the degree of similarity between the theoretical structures.

This talk reviews key tenets of structural realism, particularly the idea that there is structural continuity through diachronic theory-change. Against this background, conceptual spaces are introduced as a model of scientific conceptual knowledge, along with five change operations that model diachronic conceptual changes. Since the change operations vary in terms of degrees of severity, they yield a comparative measure of radical change. We then provide phase-space formulations of CM, SRT and QM. These descriptions allow us to compare these three major physical theories in terms of the change operations on conceptual space required to transition between them.

20. Corrado Matta: Qualitative methods and confirmation: insights from qualitative computation

This paper discusses the concept of evidential support in the case of qualitative methods in social research. A large amount of literature has criticized the use of the term evidence in qualitative research (Lincoln 2005; Morse, Swanson, and Kuzel 2001). These criticisms rest on the assumption that evidential support can be defined only if the evidential base consists of quantitative data, that is, data operationalized at
interval or ratio scales. In contrast, I argue that, under the assumption of a revised version of Glymour’s bootstrapping approach to evidential support (1991), there is no conceptual disagreement between the concept of evidential support (or confirmation) and qualitative methodology. My argument has the following structure: first of all, I attempt a reconstruction of the problem of confirmation for qualitative data, drawing on Lincoln (2002), Lincoln (2005), and Freeman et al. (2007). According to these critiques, qualitative data is affected by a problem of arbitrariness. This means that the content of a data unit depends on the meaning that both the observed individual and the interpreter attribute to it. Therefore, the question whether, for instance, an utterance is an instance of a claim turns out to be arbitrary.

Secondly, I analyze the problem of arbitrariness in the light of Glymour’s bootstrapping approach to confirmation, in order to qualify the problem of arbitrariness in a more rigorous way.

Then, in order to look for a possible solution to the problem of arbitrariness, I consider a concrete case of qualitative research in educational science (Kempe and West 2001; West and Rostvall 2003; Rostvall and West 2008). My methodological strategy consists here in a) reconstructing the categorization of the raw qualitative data and the building of a coding manual, and b) comparing the manual coding with automated coding. This latter process involves the use of qualitative computing (Brent 1984; Richards 2002), a methodology that is gaining popularity among social researchers and that consist in the use of automated tools for the analysis of qualitative data such as, for instance, the Nvivo software (Crowley, Harre, and Tagg 2002). This software has been originally used in order to facilitate operations in qualitative research that have traditionally been time-consuming, such as the coding the empirical material. However the software provides nowadays some analytical instruments that can improve the reliability of data analysis. For instance, Nvivo includes a number of automated functions such as auto-coding and the word-tree functions. The first generates codes in terms of most frequently used terms, the seconds provides a graphic representation of how a selected term is used in sentences in a text data set.

By comparing manual and automated coding I sketch a conceptualization of evidential support that is specific for qualitative methods. This conceptualization is a modification of Glymour’s approach, which preserves the core of Glymour’s theory and adds some more conditions that specify the relationship between data and claims in qualitative research.

In particular, I argue that a theory of confirmation for qualitative data needs to capture two features of the relationship between data and claims in qualitative research:

1. Observation units are translated into units in the evidential base typically according to a presence/absence criterion. This means that an action, an utterance, or a chunk of text are considered as relevant data in relation to other possible and salient actions, utterances and texts that are not observed in the same situation.

2. A body of evidence confirms a hypothesis in relation to both a theoretical framework and one or more concurring hypotheses. Without these two latter specifications the relationship between data and claims is arbitrary.
I argue that a modification of Glymour’s approach that captures these two features can solve the arbitrariness problem while remaining true to the basic principles of the bootstrapping approach. Hence, I conclude that even in qualitative research evidential bases are used to confirm claims; and that therefore the typical skepticism against the concept of evidence that is commonly manifested in the methodological discussion about qualitative research is not justified.

References

21. Ave Mets: Normativity of scientific laws
Scientific laws or the so called laws of nature are usually regarded as descriptive, e.g. of abstract models presenting regularities operating in nature, contrasting them to legal norms that are regarded as explicitly normative, prescribing social order. In this paper I defend the position that (mathematical) laws of nature are normative similarly to legal norms: they prescribe actions or end results of actions, states of art. I discern three different (non-comprehensive and non-exclusive) ways, each having two levels, in which laws of nature are normative. Firstly, conceptual normativity means prescriptions of how the world is to be thought or talked about. On the more general level, this means analyticity and
clear in treating the world and its properties, on the more concrete level – special scientific terms and visions that are transferred to common thinking e.g. through general education. Secondly, epistemic normativity means the standard of (exact) sciences for reaching certain knowledge. On the theoretical level, this means mathematicity, on the material level – experimental activity in laboratory, through which mathematics is connected to material reality. This is particularly poignant in the present situation of science politics, where this epistemic ideal leads what is considered as legitimately called science and hence worth of support. Thirdly, practical normativity means ways of how the world outside laboratory should be treated. On the narrower level, this means designing and producing apparatus and other artefacts based on the knowledge of special sciences, on the broader level – reshaping surroundings – nature and society – according to the scientific world picture. The categorisation is supported and illustrated by 1) analyses of notions of explicit and implicit normativity, pertaining correspondingly to law and other explicitly normative systems, and to science, world picture, and other not explicitly normative systems (including law); 2) historical prescriptions in sciences, or historical forerunners of scientific disciplines that explicitly contained prescriptions; 3) contemporary accounts of normativity in science in various respects: in scientific practice, in politics, economy, world picture, etc., including norms of truth, theoretically and practically accounting for real world objects, etc.; and 4) grounds of normativity of mathematical laws of nature and thence of mathematicality: I surmise that normativity of mathematics and particularly of mathematically expressed orderliness stems from human need for certitude that he tries to achieve by rearranging nature so that she is orderly and that man would have control over her. Mathematics, mathematical laws and technology based on those enable numerically foresee the results of rearranging the world, and thus diminish randomness, and insecurity and uncertainty arising from it. Mathematics as unique, apodictic and allegedly universal, enables treating sundry phenomena of the world with simple idealised terms and functions, and thus legitimise more ignorance of material idiosyncrasies as noise and error.

22. RuneNyrup: How Perspectival Realism Is and Isn’t Realist
Perspectival realism has been defended, most prominently by Ronald Giere (2006a), as a middle position between standard forms of scientific realism and antirealism. Perspectivists claim that science only warrants claims about the world that are relativised to some observational or theoretical perspective. Although our best models and theories still give us knowledge about the world, both its observable and unobservable aspects, this is a more qualified kind of knowledge than traditional forms of scientific realism claim science produces. Critics have questioned whether perspectivism is able to maintain this middle-position. On the one hand, some argue that perspectivism amounts to a form of social constructivism (Lipton 2007) or is simply “rebranded instrumentalism” (Morrison 2011). On the other, some critics argue that perspectivism fails to distinguish itself from more sophisticated versions of traditional realism, in particular that any plausible version of perspectivism would collapse into, or least be consistent with, a form of partial or selective realism (Votsis 2012). This paper examines the prospects for perspectivists to balance this tightrope. I argue three main points.
(1) Perspectivism does aim to be different from selective realism. This is seen from how it deals with cases of multiple inconsistent models, i.e. where two or more mutually inconsistent models provide our best account of different aspects of the same phenomenon (Teller 2001, Giere 2006b). Whereas selective realists try to formulate criteria allowing them to be unqualified realists about parts of these models (Chakravartty 2010), perspectivists claim it is possible to be qualified realists about all aspects of the two models.

(2) A clear notion of perspective relative realism can be extracted from Giere’s paradigm example of perspectival knowledge: colour vision. For agents relying on the same, well-functioning perspective, P, differences in their colour judgements track perspective-independent differences between objects: If objects x and y differ in colour (for agents relying on P), then x and y differ in their perspective-independent features. But the converse does not hold. Even if x and y have the same colour (for agents relying on P), they cannot be guaranteed to share any perspective-independent features. We are only warranted in inferring that they share features which are defined with essential reference to perspectives – e.g. that they have the disposition to appear yellow to agents relying on P.

(3) Giere argues for his theoretical perspectivism via high-level analogies between his account of scientific representation and colour-vision. I argue that a more promising strategy is to conceive of theoretical perspectives as extensions of observational perspectives, similar to and continuous with instrument-aided extensions. These extensions support more sophisticated ways of interacting with the world and allow us to make claims which track different and more fine-grained sets of objective differences. This differs from instrumentalists and constructivists in that perspectival knowledge track objective features in the world, both its observable and unobservable parts. But it is unlike partial realism in that we cannot give any substantial perspective-independent descriptions of what those features are.

References:

23. Simonluca Pinna and Simone Pinna: The epistemological importance of geometrical notions

In philosophy of physics the ontological and epistemological status of spacetime is an open question. Two opposing theses are involved in solving this issue today. According to disappearance thesis (DT), spacetime is not a fundamental structure of physical reality, but it emerges at less basic levels, e.g., at the “low energies” treated by some theories in quantum gravity (QG). Thus, even
geometrical notions are derived by the dynamical equations of the fundamental QG theory (Rovelli, 2004).

Primacy thesis (PT), on the contrary, claims that spacetime and the underlying geometrical notions are still fundamental in any physical domain for epistemic reasons (Hagar and Hemmo, 2013). Indeed, in order to verify the predictions of a physical theory, one must make measurements, but all measurements are reducible to geometrical magnitudes measurements. Therefore, if a theory aims to be empirically meaningful, it cannot get rid of the notions of space and time.

In this context, the philosophy of science intends to make some conceptual clarification. First, the use of the notions of space, time, geometry and measure in the different interpretations of the physical theories must be specified. One will be able, thereby, to ascertain whether the essential proposition of the DT, i.e., that the geometrical notions are not fundamental, is coherent with the physical theories to which it refers.

Second, in the contest of a naturalistic epistemology one must consider the epistemic role of the cognitive systems underlying the representation of the concepts of quantities. In cognitive science a large amount of data, particularly those regarding the SNARC effect (Spatial-Numerical Association of Response Codes), reveals a tight correlation between numerical and spatial representations (Dehaene et al., 1993). Some researchers (e.g., Walsh, 2003) claim that this correlation may be due to the existence of connected neural structures that underlie the elaboration of spatial, temporal and numerical representations (unified theory of magnitudes (UTM)). This connection has been observed in humans as well as in non-human animals, and this fact sheds light on the adaptive importance of geometrical representation of magnitudes.

The latter issue could suggest an epistemological argument against the DT main claim: if the spatiotemporal (geometric) notions are necessary to represent any quantity (i.e., numbers and magnitudes), every form of quantitative knowledge of phenomena – scientific theories included – should presuppose these notions. We propose that such an argument may give consistency to a philosophical position that unifies empiricism with the assumption that sets of phenomenal data refers to a reality independent from representations, interacting with the cognitive subject and determining his ontogenetic and phylogenetic development.

References:

24. Carlo Proietti and Antonia Franco: Social norms and free-riding in Academia
It is often commonplace to say that many public and private institutions are inefficient, in the sense that they do not satisfy the high-quality standards they could fulfill with a different setup or structure. In many cases the malfunctioning hangs on the social norms that govern such institutions, e.g. individuals that can provide high quality are not adequately prized or are marginalized. Origgi and Gambetta (2009) investigate the special case of Italian academia where, they argue, despite the official policies promoting high standards, unwritten social rules act the opposite.

Examples taken from the Italian academia are perhaps folkloric and extreme but, one may suspect, not so isolate or unusual. Interestingly, Origgi and Gambetta suggest that inefficiency may be explained as the effect of an iterated two-player game with two types of players: Low-doers and High-doers. In such a game players may perform action H (for High) or L (for Low). High-doers get a higher payoff from the coordinate action HH, which generates a socially optimal outcome, while Low-doers get better reward from LL, which instead gives rise to a socially suboptimal one. A similar payoff distribution is supposed to motivate the interest of Low-doers to coordinate for LL - thus generating low social outcomes – as well as their coalitional power and their ability to resist the effect of official incentives for H-doing.

In the present paper we develop further this idea in a game-theoretic framework and build a multi-agent simulation model (in NETLOGO) to test this hypothesis and some of its consequences. We first present a game theoretical approach to the problem, both with a discrete payoff table (to formalize the original scenario of Origgi and Gambetta) and with a continuous payoff function per agent (to diversify the agents with a finer granularity). This allows us to model agents with different parameters of the payoff function, and to let them have a learning algorithm on their own. For the simpler discrete games we illustrate some Nash equilibria and show how they give hints about the causes of the phenomena mentioned above. For the more complex games we provide a formal description and a numerical solution. Instead, for even more complex scenarios we only rely on simulations. In our simulation framework we make use of social networks theory to generate academic societies as close to reality as possible. We further study the outcomes of our analysis (e.g. number of pre-retirements per year, average payoff per type of agent, etc.), and their dependence on different input parameters (e.g. the percentage of H-doers, the retirement rule, assignment of sanctions or prizes, etc.). We then comment on how our findings fit with the game theoretical framework, and argue why it happens when they diverge. Finally, we also comment how in our framework the phenomena presented earlier (the LL equilibria) emerge, and how the implementation of correctional measures (e.g. sanctions, a modified hiring strategy, etc.) could affect/improve faulty academic systems.

References:

In current philosophy of science, the most widely accepted account of scientific explanation is the causal account of explanation. I argue that a re-evaluation of the received causal account is needed for the following reason: the causal account cannot provide a general theory of all scientific explanations, since there are compelling examples of what appear to be non-causal explanations. The main goal of this talk is (1) to develop a more adequate account of scientific explanation – a counterfactual account – that provides a unified framework for both causal and noncausal explanations, and (2) to identify criteria for distinguishing between causal and non-causal explanations.

According to the causal account of explanation, the sciences explain iff they identify the causes of (or the causal mechanisms for) the phenomenon to be explained (see Cartwright 1983, 1989; Salmon 1984, 1998; Lewis 1986; Machamer, Darden and Craver 2000; Woodward 2003; Craver 2007; Strevens 2008). Two reasons speak in favor of the causal account: first, many paradigmatic explanations in science are causal explanations; second, the causal account successfully meets desiderata that previously proposed accounts of scientific explanation failed to satisfy (Salmon 1989: 46-51), such as the covering-law account (Hempel 1965) and the unificationist account (Friedman 1974; Kitcher 1989).

However, is it really the case that all scientific explanations causal explanations? The answer to this question seems to be negative, because scientists give non-causal answers to why-questions. Since the early 2000s, a number of compelling examples of (seemingly) non-causal explanations have re-entered the arena of philosophy of science. Examples of non-causal explanations come in a surprising diversity: for instance, they are based on non-causal laws, purely mathematical facts, symmetry principles, renormalization group methods, inter-theoretic relations, and so forth (see, for instance, Batterman 2002; Bokulich 2008; Huneman 2010; Lange 2011, 2013a,b; Pincock 2012; Saatsi and Pexton 2013; Weatherall 2011). In the recent debate, the primary goal of discussing examples of non-causal explanations has been to show that certain scientific explanations cannot be accommodated by the received causal account of scientific explanation. Hence, the main goal has been a negative one, i.e. to undermine the causal account. The current debate is largely silent on a more positive and constructive approach to non-causal explanations. The goal of this talk is to advance such a constructive approach:

1. I provide an account of what makes non-causal explanations explanatory,
2. I propose criteria for distinguishing causal and non-causal explanations.

Regarding (1), I argue that non-causal explanations work by revealing non-causal counterfactual dependencies between explanandum and explanans. Such a counterfactual account of non-causal explanations is an extension of Woodward’s (2003) causal version of the counterfactual account. Hence, the counterfactual account provides a unifying framework for causal and non-causal explanations – both are explanatory because they reveal counterfactual dependencies, or so I will argue (see Frisch 1998; Bokulich 2008; Saatsi and Pexton 2013). Causal explanations are explanatory in revealing causal counterfactual dependencies (based on causal generalizations) between explanandum and explanans. Noncausal explanations are explanatory in
revealing non-causal counterfactual dependencies (based on non-causal generalizations) between explanandum and explanans. I will argue for the adequacy of the counterfactual account by applying it to three paradigmatic kinds of non-causal explanations: (a) purely statistical explanations, (b) renormalizations group explanations, and (c) genuinely mathematical explanations.

Regarding (2), I propose to distinguish between causal and non-causal explanations on the basis of so-called Russellian criteria of causation (including criteria such as asymmetry, time-asymmetry, the distinctness and locality of causal relata, intervenability, and so on). I argue that an explanatory relation is causal iff all of the Russellian criteria apply to it; otherwise it is non-causal.

References:
26. Georg Schiemer: What is theoretical truth?

In the logical analysis of science, theories are often described as axiomatic systems formulated in a language whose vocabulary is bipartitioned into observational and theoretical terminology. A central interpretive issue in the modern literature since Carnap concerns the proper understanding of the latter. What is the meaning of such terms? How, if at all, is their reference to non-observable entities fixed? In what ways does a scientific theory contribute to the specification of the interpretation of its theoretical vocabulary? Questions of this sort are usually referred to as “the problem of theoretical terms”. The philosophical discussion of this problem usually centers around the formulation of a proper semantics for theoretical terms and, consequently, on the semantic evaluation of theoretical sentences, i.e. statements containing such terms. More specifically, different theories are currently on the market that aim to capture a central semantic property of such terms, namely the fact that their meaning is usually left incomplete or undetermined in a relevant sense.

The present talk will discuss two accounts to formalize the semantics of theoretical sentences—both originating in Carnap’s work on Wissenschaftslogik—and compare them both from a conceptual and logical point of view. The first account is based on the "indirect interpretation"-view of theoretical terms and has recently been developed in detail in (Andreas 2010). Roughly speaking, this is the view that the meaning of such terms is determined by the axioms of a scientific theory. In Andreas’ work, this idea is developed into a model-theoretic explication of theoretical truth: A scientific sentence (containing both observational and theoretical terms) is theoretically true if true in all admissible extensions of its intended observational model, that is, in all extensions that also interpret the theoretical terms in accordance with the theory’s axioms.

The second approach to specify the semantics of scientific statements can also be traced back to work by Carnap, namely to his attempt to explicitly define the theoretical vocabulary of a scientific theory in a language containing a epsilon-operator (Carnap 1961). Briefly, a Hilbertian epsilon-operator is a logical term-forming operator that acts semantically like a choice function: for a given predicate A(x), the epsilon term built from it denotes one arbitrary element in the extension of A if it is non-empty. Carnap’s definition of the theoretical terms makes critical use of epsilon-terms built from the Ramsey sentence of the theory in question. Informally speaking, the definition picks out an arbitrary sequence of “theoretical entities” in the domain of the background language as the referents of the terms given that the theory in question is satisfied. The
semantics of scientific sentences can also be specified in terms of the choice 
semantics underlying the use of such terms: A sentence containing theoretical 
terms is (generally) true if its epsilon-term translation turns out true under 
every possible choice function assigned to the epsilon operator.

The main aim in this talk is to present these two formal accounts of 
theoretical truth as well as to analyze their conceptual similarity. Specifically, we 
will present a metalogical result showing that Andreas’ and the epsilon-based 
semantics for theoretical statements are in fact equivalent. A second aim in the 
talk will be to further compare the underlying logics of both accounts and their 
respective inferential strength. In this regards, the following question will be 
addressed: Do both formal reconstructions allow us to model adequately the 
informal use of deductive reasoning in scientific practice?

27. Luigi Scorzato: A simple model of scientific progress based on only two 
cognitive values

I argue that all known models of scientific theory selection and scientific 
progress rely, more or less implicitly, on some notion of simplicity (or some 
equivalent virtues), that cannot be left ambiguous. However, I also argue that the 
idea of simplicity which is commonly used for theory selection in the scientific 
praxis is not as obscure and slippery as it is widely believed to be. The key is to 
recognise precisely the subtle but deep interplay between the simplicity of the 
formulation and the precision of measurements in real scientific theories.

For example, the Standard Model of particle physics represents a spectacular 
unification of a huge variety of phenomena and it currently agrees, with 
remarkable precision, with all the experiments. Physicists agree that the 
problems with the Standard Model are the lack of an elegant unification with 
General Relativity, the lack of naturalness, and the presence of too many free 
parameters, with curious values. Since none of these is a problem of empirical 
adequacy, it is essential to understand what are the non-empirical cognitive 
values associated to them. What makes the answer difficult is that, in principle, 
we could solve all these problems by rewriting our fundamental laws as \( \Xi = 0 \), 
where each fundamental equation of the Standard Model and General Relativity 
is mapped to some digits of the variable \( \Xi \). Superficially, we would have an 
elegant, parameter-free, formulation.

I argue that understanding what’s wrong with the \( \Xi \) formulation is the key 
to understand what the scientists actually mean when they use a notion of 
simplicity for theory selection.

It was notice (Scorzato, 2013) that the \( \Xi \) formulation of a theory \( T \) has a 
well defined limitation: the experimental results cannot be reported in the form 
\( (*) : \Xi = \Xi_0 \pm \Delta \). However, any theory \( T \) should at least: (i) include some 
properties whose measurements can be expressed in the form (\( * \)) and (ii) be 
such that any other measurement of \( T \) can be expressed in terms of the above. I 
will show that this framework allows the definition of a notion of complexity of 
the assumptions of \( T \) (we call it \( C(T) \)) that approximates well the idea of 
complexity that is practically (often implicitly) used in science.

On the basis of this result, I build a simple model of progress, that is based on 
just two cognitive values: empirical adequacy and the complexity \( C(T) \) of the 
assumptions of \( T \). I compare this model with paradigmatic cases of real progress 
and with other classic views of progress (Popper, 1963; Lakatos 1970; Kuhn,
I conclude that this simple model gives a fairly good account of scientific progress, and I see no reason to claim that more independent cognitive values are necessary to understand scientific progress.

References:

28. Edit Talpsepp: Essentialism-related reasoning patterns and the 'Darwinian revolution'

It is a commonly held assumption that essentialist thinking is inconsistent with evolutionary theory and should be abandoned as the result of adopting it. Essentialist thinking, according to which biological species have something like a physical essential property, shared by all and only the members of a species, is ascribed to pre-Darwinian taxonomists and assumed to be abandoned as the result of something like the 'Darwinian revolution'. The reasoning patterns that are assumed to be implied by essentialism involve the beliefs in: 1) the immutability of species; 2) the transformational view of evolution; 3) sharp boundaries of species taxa; 4) species monism; 5) taxonomic monism. (In my usage, taxonomic monism is the assumption that the biological diversity might be classified in only one correct way; species monism is the assumption that we can classify the biological diversity on the basis of only one species concept.)

My claim is that not all the Darwinism-clashing reasoning patterns that are associated with essentialism are actually implied by essentialism, or imply each other. The philosophical independence of material essentialism and some Darwinism-clashing reasoning patterns allows us to hold these reasoning
patterns even if we don’t posit (material) essentialism to species taxa. Also, the fact that most of these reasoning patterns are philosophically independent of each other explains why they do not all have to be held or abandoned at once.

Discussing these matters, I will distinguish between particular and non-particle essentialism (assigning a particular vs non-particle material essence to a species) that lead to somewhat different reasoning patterns. Particular material essences are supposed to be the essences of certain kinds in every possible world. Non-particle material essences can be described as ‘the material property characteristic to the kind members’ - for instance, the genetic properties essential to the kind ‘cat’ (if we could talk about essences in modern biology) or a cat’s heart/soul/blood (in the context of folk biology). Non-material properties do not have to be fixed material properties; the main requirement for them is to define the kind that they are essences of. The difference between particular and non-particle material essences is that we can identify a kind on the sole basis of particular essences, whereas in the case of non-particle material essences we first have to identify a kind, at least roughly, on the basis of some other diagnostic cues, and then study its ‘essence’.

Particular material essentialism leads for instance to the assumption about the immutability of species, non-particle essentialism leads to the transformational view of evolution. Both particular and non-particle material essences lead to the assumption of sharp species boundaries and taxonomic monism (but not species monism - as all modern species concepts are based on relational properties of species, assuming that particular essentalism leads to species monism would be a category mistake). In my presentation I will characterize the implication relations between the Darwinism-clashing reasoning patterns and justify my claim that concerning the abandonment of biologica essentialism, the ‘Darwinian revolution’ is no tas abrupt as it’s usually assumed to be.

29. Iulian Toader: Idealization, Inequivalence, Indeterminacy

The standard realist move against the claim that no scientific theory can secure determinacy of reference is to embrace structuralism and argue that indeterminacy of reference does not imply indeterminacy of truth conditions. To show that determinacy of truth conditions can be secured, one typically insists on empirical or computational constraints that might eliminate the “unintended” interpretations that make a theory true. Idealization procedures in scientific practice indicate, however, that such interpretations are necessary to account for a range of natural phenomena. In my talk, I discuss the implications of this fact for semantics and modal ontology.

In particular, after providing some background on classic indeterminacy arguments, I describe the problem raised by unitary inequivalence relations in quantum theories, a problem caused by the failure of the Stone-von Neumann theorem in idealized systems, e.g., in systems with an infinite number of degrees of freedom. Then I argue that this is not a problem with determinacy of reference (and if it is, then it can be easily solved by relativizing reference), but rather a problem with determinacy of truth conditions. I point out that insisting, as many do nowadays, on physical constraints that could restore determinacy of truth conditions cripples the quantum theory by making it unable to account, e.g., for the mass of massive elementary particles. Then I argue that a recent attempt to
accommodate indeterminacy of truth conditions, by considering the “unintended” interpretations of the theory as possible worlds, commits the realist to a possibilist modal ontology. I end by defending a naturalist proposal to the effect that modal ontology should stem from scientific practice, rather than being derived from folk theories and then imposed on this practice.