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Giacomo Lini, Giorgio Sbardolini, Matttia Sorgon

Autori. Giacomo Lini: giacomo.lini8188@gmail.com
Giorgio Sbardolini: giorgio.sbardolini@gmail.com
Mattia Sorgon: mattia.sorgon@gmail.com

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1 Overview

The three-yearly conference of Italian Society for Logic and Philosophy of Science (SILFS) has taken place in Bergamo, the 15th, 16th and 17th December 2010. The charming venue has been the former convent of Sant'Agostino, nowadays University of Bergamo.
The conference program has been structured distinguishing plenary and parallel sessions: the first ones were 40 minutes long and designed for international guests: S. Abramsky from the Wolfson College of Oxford, A. Hagar from Indiana University, P. Janich from Philipps Universitaet of Marburg and S. Okasha from University of Bristol. The complete program of the conference can be downloaded here: `http://dinamico2.unibg.it/silfs/SILFS2010/SILFS2010_PROGR.pdf`
We followed all the plenary sessions, and the first part of our report concerns them. There is just the exception of professor S. Psillos' talk, which was indeed programmed, but didn't take place due to greek unrest of the last December.
The second part of our report concers the parallel sessions, that were 20 minutes long and designed for concurrent talks split between subject areas. We will keep this division into areas in our report and we chose four talks per area. However, it must be said that we had not watch the parallel sessions completely, since their being simoultaneous. This means that our review of the conference will be perspective, both because of subjective standpoint and because of the mentioned limitation. Still we hope we can offer a quite complete picture of the three-days conference, as we hope this overview may be considered as a general review of it.
Just a few words must be still said to introduce to our review. The structure of each review will consists in a presentation of the talk's author(s) (which we faithfully copy hereinafter) followed by a brief remark by us.
Since many compliments could be surely done to the SILFS' organization and to the University of Bergamo, an overall effect seems hard to be drawn, because of some arising perplexities. Although these are just our shared impressions about the conference, we think they must be remembered.
One could note outward that the conference have had three official languages: English, French and Italian. Apparently, this is something that has not been justified since French has been useless and Italian has often prevented the comprehension by foreigner attendance. Many defections have taken place, something that could be said about the conformity with the program (in spite of the high number of speeches). Of course these defections have been caused by different reasons, but they become an indication of lack of professionalism as the number grows.
Coming to an estimation of the inner issues, first we are seriously puzzled about the selection criterion (if any) which the SILFS has chosen the invited speakers with. One can notice that excellent talks have been put together to shallow speech without substantial scientific commitment. One has to keep in mind that who is just worthless, much lowers the general level. The topics have been various, although many were historical reconstruction and the "historiographic" approach in the study of Philosophy of Science seems therefore prevailing. Those talks whose subject was Logic and its applications, have been the highest-quality ones, by the fact that they were close to contemporary researches. More specifically, we would like to quote those concerning Quantum Computation and Logic of Quantum Mechanics. Talks of Philosophy of Biology and Cognitive Science were first-rate works, too, having showed high proficiency, though even specialistic topics were often technically handled, to the detriment of laymen (as us).

2 Plenary Talks

2.1 Peter Janich, *A methodical theory of Emergence.*

Author's presentation. The concept of emergence playing a central role in evolutionary biology was recently reconsidered and further developed in the field of mind body theories, and was pushed by challenges arising from progress in brain research. The characteristics of different types of emergence (weak or strong, synchronic or diachronic, logical or casual, ontological or methodological) is reconstructed under linguisitc and methodological aspects.
Especially the turn of perspective from objects of research to the subjects acting in research allow to determine a further type of relation between the emergent and the original phenomenon. As only described phenomena can be theoretically treated in an emergent relation to each other the means and ends rationally at the side of the scientist provides a way to methodically construct the emerging phenomena as technical purposes of models relative to technically available means.
This is not the way famous historical theories went (like Charles Darwin modeling selection according to a human breeder, and Wilhelm Wundt's "principle of heterogony of ends" in his peoples psychology ("*Völkerpsychologie*")) but opens a distinction between categorically equal and categorically different types of emergence. The method to bridge categorical gaps between the description of phenomena is limited to the descriptive access and leaves open the difference between description and ascription, between natural and moral.

Review. Professor P. Janich has opened the works of SILFS talking about two different epistemological theories and suggesting a possible solution. Naturalistic epistemology, named also evolutionary epistemology, interprets the progress of science by means of a perspective based upon the key concept of emergence. One might define "emergence" as a complex property belonging to a system to which it is irreducible. Epistemology may explain the develop of science as emergent capability of problem solving via the natural selection criterion among descriptions of scientific facts. From a philosophical point of view, such an epistemologist has to draw any aspect of knowledge back to natural science (see Giere (1985) and the point of view of P. and P. Churchland).
Second perspective is positive epistemology. This may be regarded as Wiener Kreis' epistemology and Popper's. They firmly distinguished among mathematical logics and natural science and this distinction conferes them the possibility to define science once and for all. Thus the pivotal concept of positive epistemology is the theory of method, i.e. the purpose to outline the set of mechanic-like operations which necessarily yield scientific experience.
Both these views show values and imperfections: firstly, naturalism asserts that a good description of science depends on natural laws and for this reason it might commit to deny natural laws whenever the description is wrong. Anyway this doesn't seem a definitive confutation. On the other side, positivism faces to fall in a reductionist eliminativism considering only the object of science. Although Janich referred to Quine (1951), even this criticism seems weak since based on a qualm. However Janich suggested to get over looking at folk psychology and interpreting the difference among epistemologies as a dualism of description which is not a dualism of substance as well as a dualism among rational goals of scientist and natural phenomenon. The effort of the speech is to enrich emergent theory by strengthen it with methodological issues in order to concile the difference and (in a kantian way) to overcome

the perspective dualism.

2.2 Samir Okasha, *Why does Darwin matter for philosophy?*

Author's presentation. A number of scientists and and philosophers have argued that Darwin's theory of evolution has a special relevance for philosophy that other scientific theories do not have. I offer a cautios defence of this viewpoint , by identifying a number of traditional philosophical issues, drawn from diverse areas including metaphysics, epistemology, ethics and decision theory, which can be illuminated by adopting a Darwinian perspective. I offer some speculations about Darwinism has proven so fruitful for philosophical enquiry.

Review. Professor Samir Okasha maintains that Darwinism is a fundamental instrument in philosophy; among other authors Wittgenstein denied this position, saying that one needs to distinguish between Darwinism, considered as a biological theory, and philosophical issues. Therefore Professor Okasha pursues many different philosophical topics, and finely showing how evolutionist perspective is not just interesting, but also useful.

a *Metaphysics*: Van Inwagen (1990) states that material objects are just Physical Simples or Living Organism, thus assuming a notion of organism which is totally unjustified, against which Okasha presents many counterexamples; moreover, following Ghiselin (1994) and Hull (1978) it is possible to reconcile the reality of the species with the lack of essential properties and to reduce of the notion of "species" to space-temporally individuals: so that organism relates to species as part to wholes. This argument offers a criticism to the theory of natural kinds (Putnam (1979), Kripke (1980)), showing how any genetics justification of modal identity is not scientifically confirmed.

b *Philosophy of Mind and Language*: semantical indicators aren't able to explain what a misrepresentation is. So Darwinism suggests a to naturalize intentionality; under the light of evolutionary biology, the theory of mental states focuses on the following definition: saying "p" implies representing "p" if and only if this representation causes in the organism a behavior which is fitness-enhancing when "p" is actually present.

c *Epistemology*: why our cognitive systems are as they are? Darwinism shows that assuming the existence of normative answers to that question is a genetics fallacy. On the other hand, Darwinism introduces new epistemological perspectives: evolutionary biology avoids improper questions, clarifies the relationship between belief and behavior and examines the issue of desires and volitions. Thus one may ask: do we desire what enhance our fitness? Not necessarily, in fact we are led to conceive ourselves as poor-information-processors since our genetics code doesn't provide a program enough strong to make us aware of all the consequences of our actions; this explains why we give importance to intermediate goods.

d *Decision Theory*: Evolution realizes Nash's Equilibrium and conditions our capacity of making rational choices by setting it on the benefit optimization process. For example, considering two species in the same danger situation, the surviving one is able to adapt herself as better as possible to the environmental conditions, i.e. the species that has the best rational payoff will survive.

e *Political Philosophy and Ethics*: In Rawls (1971) two fundamental points are stressed. The knowledge and availability of resources and secondly an absolute ignorance of the order (in other words: what am I supposed to obtain in particular?). This view should apply as well as to organism seen as social body. However, our genes have the same interest in maximizing biological output and they don't conflict. Strengthening Rawls' theory with the concept of randomization Okasha maintains that the Darwinian point of view explains social conflicts situations between group's and individual's interest, being consistent, as a matter of fact, with Rawls' theory.

Considering the fact that Darwinist explanations does not depend from more general scientific laws and that they can be considered adaptive explanations, Professor Okasha shows that Darwinism is able and fruitful when used in many philosophical issues: so, in conclusion, Wittgenstein was wrong, and Darwin was totally right.

2.3 Samson Abramsky, *Bell's theorem and the logic of locality.*

Author's presentation. Locality is an important theme in computation, and a central one in distributed and parallel computation, where the spacial structure of systems becomes significant.
At the level of fundamental physics, a basic assumption has been that causal influences propagate in a local fashion. Relativity constrains this propagation to happen no faster than the speed of light.
One of the most remarkable developments in modern physics is Bell's theorem, which says that quantum mechanics predict correlations between spatially distributed particles which cannot be achieved by any local 'hidden variable' theory. Subsequent experiments have confirmed that nature does exhibit non-local behaviour as predicted by quantum mechanics. This has profound implications, both for the possibilities for quantum information and computation, and for our very conception of reality.
The presentations of Bell's theorem and related results which appear in the literature use a setting of probabilistic models which can appear technically complex, even though the basic ideas are simple. Moreover, they interweave logical steps and physical intuitions in a fashion which can be confusing to the uninitiated.
We shall use a simple relational framework to develop the key notions and results of hidden variables and non-locality. We show that to a remarkable extent, the main structure of the theory, through the major No-Go theorems and beyond, survives intact under the replacement of probability distributions. In particular, probabilistic notions of independence are replaced by purely logical ones. The basic arguments are simple enough that they should be accessible to undergraduates! There are also connections to computational issues, and to ideas of dependence and independence in logic.

Review. The talk deepens into the problems related to determinism in Quantum Mechanics. Nevertheless Abramsky's speech has been really clear and complete. Locality principle is defined in Einstein, Podolsky, Rosen (1935) as follows: two or more physical quantities can be regarded as simultaneous elements of reality only when they can be simultaneously measured or predicted. It is of course deterministic and implicitly assumed in Classical Physics. Keeping this principle in mind one could measure the spin of two distinguished particles A and B in this way: $A\uparrow, A\rightarrow, B\uparrow, B\rightarrow$; since the spin can be up- (U) or down-oriented (D),

Hardy's paradox occurs.

	(U,U)	(U,D)	(D,U)	(D,D)
(A↑ , B↑)	1			
(A↑ , B→)	0	1		
(A→ , B↑)	0	1		
(A→ , B→)		1		0

where 1=possible; 0=not possible.

Can this system be realized by any local hidden variable λ ? If yes, λ may be defined such that:

$$\lambda(A\uparrow, B\uparrow, UU, \lambda) \wedge \lambda(A\rightarrow, B\uparrow, UD, \lambda) \Rightarrow (U = D)$$

which leads to a contradiction.
By a relational model, Abramsky maintains contextuality as the possibility to detect different measures jointly, due to entanglement. These acts of measurement should be causally independent from each other. The conclusion is that non-contextuality implies locality, hence non-locality implies contextuality but the converse doesn't hold.

2.4 Amit Hagar, *Counting steps: a new, finitist, and objective interpretation of probability in physics.*

Author's presentation. Objective probability in physics is understood today via three main interpretations, namely, Humean regularities, deterministic (statistical mechanics) chances, and stochastic (quantum mechanical) chances. The first two interpretations suffer from a twofold problem of the justifications of the measure. Solving this problem, here we suggest a new, fourth, interpretation of objective probability that is based on the notion of physical computational complexity. This new interpretation may also have interesting connections with stochastic (quantum mechanical) view.

Review. Hagar's interpretative purpose about probability is based upon those new insights recently opened in physics by contemporary researches in computation theory. It is possible to simulate the probabilistic behavior of a physical system considering the dynamical evolution of a related system, evaluating its energetic balance. In this case the guidelines of interpretation would be the following:

a to probability 0 of an event's verifying (impossibility) is associated the total absence of physical processes, thus by the conservation of energy principle, absence of energetic exchange.

b to probability 1 of an event's verifying (certainty) is associated the expenditure of all available energetic resources, thus the maximum energetic exchange.

Due to this two points descends that any event with probability included between 0 and 1 can be interpreted on the appropriate physical system as a certain number of effective phenomena (not all possible phenomena), such that the probability would be the ratio between consumed energy and available energy of the system. Therefore probabilistic ignorance could

be read as the dynamical evolution of a previously defined physical system. Defining functions from pure probability computation to appropriate physical systems is the most suitable method to solve issues inherent to the three main probability theory paradigms.

3 Parrallel Sessions

3.1 Logic and Apllications

3.1.1 R. Giuntini (with M.L. Dalla Chiara), *The standard disk $\sqrt{'}$ quasi-MV algebra is not finitely axiomatizable.*

Author's presentation. $\sqrt{'}$ quasi-MV algebras (for short,$\sqrt{'}$ qMV algebras) were introduced in [3] as an axiomatization of the equational theory of the algebra of density operators of the Hilbert space $\mathbb{C}^2$, endowed with operations corresponding to the quantum computational connectives of *quantum Łukasiewicz disjunction* and *square root of negation*: see e.g. [5] for a general motivational introduction and for a thorough explanation of the connection between these structures and quantum computation. In [2] it was proved that the (standard) model of $\sqrt{'}$ qMV algebra, which is based on on the set of all density operators of $\mathbb{C}^2$, is isomorphic to the following $\sqrt{'}$ qMV algebra (called *standard disk algebra*):

$$D = \langle D, \underline{\oplus}, ', \underline{0}, \underline{1}, k\rangle,$$

where,

- $D = \{\langle a, b\rangle : a, b \in \mathbb{R} \text{ and } (1-2a)^2 + (1-2b)^2 \leq 1\}$

for any $\langle a, b\rangle \underline{\oplus} \langle c, d\rangle$ in D:

- $\langle a, b\rangle \underline{\oplus} \langle c, d\rangle = \langle a \oplus b, 1/2\rangle$ and $a \oplus b = Min(a + b, 1)$;
- $\sqrt{'}\langle a, b\rangle = \langle b, 1 - a\rangle$
- $\langle a, b\rangle' = \langle 1 - a, 1 - b\rangle$
- $\underline{0} = \langle 0, 1/2\rangle$
- $\underline{1} = \langle 1, 1/2\rangle$
- $k = \langle 1/2, 1/2\rangle$

In this talk we will present the solution of a long-standing open problem concerning $\sqrt{'}$ qMV algebras: namely, we show that the variety generated by the standard disk algebra D is not finitely based, and we provide an infinite equational basis for the same variety.

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Review. Quantum Logic (QL) may be well formalized as a non-commutative and non-associative many-valued logic (MVL). Adopting standard bra-ket notation for Quantum Mechanics, one can represents any quantum state by $|\phi\rangle$. A suitable interpretation for the logical constants "$\bot$" and "$\top$" is given through mapping them, respectively, onto the kets $|0\rangle = \left(0, \frac{1}{2}\right)$ and $|1\rangle = \left(1, \frac{1}{2}\right)$.
By these conditions one can characterize any possible state of a quantum system, where the quantum probability (FORMULA) holds.
Standard quantum model can not represent any reversible process. In those situations the problem is due to the fact that whenever one measures a physical quantity, one produces an interference which destroys any knowledge we had before (the standard case is that one of Schrödinger's equation $|\psi\rangle$). Thus, the standard quantum model holds good for QL but can't be considered a useful tool for any alleged theory of measure.
R. Giuntini shows how possibly to compensate for it. Irreversible processes of a quantum system can be seen as quantum operators. Thus an algebra is a model for a quantum system first by replacement of algebraic operations by quantum operators.
The algebraic model we obtain by subsequent weakening of a multi-value algebra (MV algebra) – i.e. by adding gradually many conditions – is a square-root negation quasi-MV algebra ($\sqrt{r}$ qMV algebra). One can define A as follows:

$$A := \left\langle D, \sqrt{r}, \oplus, P_0, P_1, \simeq \right\rangle$$

where:

- D is the domain. $D = \bigcup D\left(\left\langle \otimes^n, \mathbb{C}^2 \right\rangle\right)$
- $\sqrt{r}$ is the square root of negation. $\sqrt{r}\langle a, b\rangle = \langle b, 1 - a\rangle$
- $\oplus$ is the bounded sum of Łukasiewicz. $\langle a, b\rangle \underline{\oplus} \langle c, d\rangle = \langle a \oplus b, 1/2\rangle$ and $a \oplus b = Min(a + b, 1)$
- P_0 and P_1 are the false/true properties that can be represented as stated above.
- $\simeq$ is the semi-identity.

The second step is then to define a quotient with respect to "$\cong$". One does this in order to fix a variety of algebras whose members respect the conditions above and thus can be considered a class of models for the quantum system. The equation

$$p \cong s$$

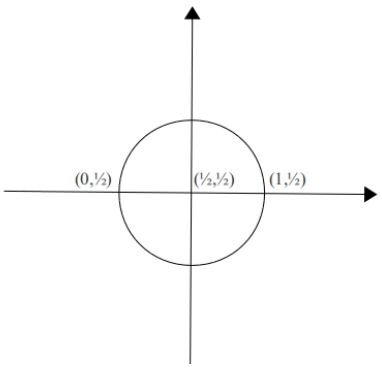
means that if p are quantum probabilities under $\left|\mathbb{C}^2\right|$, then

$$p(p) = p(s)$$

and conversely,

$$p\left(\sqrt{r}p\right) = p\left(\sqrt{r}s\right)$$

Thus one had obtained a variety of algebras which are models for a quantum system. Giuntini chose, for the sake of simplicity, to shift down the dimensions of D to the cartesian product DxD when showing his results. Following his presentation we get the disk-model



as the more suitable way to represent the relevant experimental outcomes. The disk-model belongs to the same algebraic variety of the Poincaré-Bloch's square-model, and this could be a strong theoretical validation of R. Giuntini and M. L. Dalla Chiara presentation.

3.1.2 D. Porello, *Resource allocation in substructural logics.*

Author's presentation. In the last decade, resource allocations problems have been widely investigated also from a computational point of view both in the AI and the logic community. In this paper, I will show how substructural logics provide a principled modelling of resources allocation problems, which can be classified according to the types of goods agents trade. I will present a classification of resource allocation problems with the corresponding logical language to encode preferences and we will see how dropping contraction, weakening or exchange affects the type of goods and agents valuations.

Review. D. Porello provides a logical model for treating the problem of resource allocation. The speech is referring to the joint-work made with U. Endriss on the Computational Foundations of Social Choice (see Porello and/or Endriss home pages on ILLC website for more informations and related papers).
We can model the universe considering a set of bidder and a set of (finite) goods G. A resource allocation problem consists in find an allocation function α which associates goods to bidders under some conditions. If S is subset of G, namely a bundle of goods, and w is the price associated to the bundle of goods, then the couple (S, w) is an atomic bid for S. It is possible to provide several languages (Nisan 2006) to define utility functions

$$u, u : \wp(G) \longrightarrow \mathbb{R},$$

which assigns a value to each combination of goods (in terms of prices) thus modelling the preferences of bidders.
Which evaluation of goods maximizes a bidder's revenue? This question seems to get close to the pivotal issue of the topic. A solution may be reached through the interpretation of allocation problems as poof-search problems. In other words: the problem of maximizing the bidder's revenue given a bundle of goods by finding the properly allocation can be seen as the problem of finding a proof for to entail an evaluation from bidder's revenue and associated goods, both seen as formulas. Roughly speaking, if $S = p, q, r$ are the given goods and the bidder's maximized utility (the revenue) is the value u of a bid-function assigning goods to utility, then

$$p, q, r, bid_{p,q,r} \rightarrow u$$

Moreover, goods are subsets of G. Considering the elements of G, which are goods effectively, we are free to suppose that the set of goods a,b is different from the set of goods (a, a, b), and so on. Thus we are led to model bundle of goods as multi-sets. Thus modelling goods with multi-sets in classical logic means precisely that the quantity of each kind of good at disposal is troublefree. In fact there are at least three structural rules for classical entailment, that could be even problematic when we introduce more distinctions by means of framework hypothesis. The first one is Weakening:

$W : (a, b) \rightarrow (a \wedge b)$
$(a, a, b) \rightarrow (a \wedge b)$

Weakening condition assert that if a bidder demand a particular good then he or she is willing to obtain it no matter how much of that good is effectively at disposal. The two more classical rules I previously mentioned are Contraction and Exchange:

$C : (a, a) \rightarrow (a \wedge a)$
$(a) \rightarrow (a \wedge a)$
$E : (a, b) \rightarrow (a \wedge b)$
$(b, a) \rightarrow (a \wedge b)$

Either can be clearly problematic when classical entailment is useless (e.g. when order matters). The first condition one is inclined to remove is probably C: Contraction asserts that any allocation proof for a set of goods (possibly with repetitions) implies an equivalent proof for a set of goods without repetitions. Thus if we consider (not trivially) the quantity of goods at disposal removing C, the logic associated to the new system is no longer classic. Porello provides more expressive systems removing W and investigating formal properties of the related logics. For the sake of simplicity, droppingW, C and$E$and thus making no hypothesis on the allocation problem unless that we handle with lists of goods, we get the Lambek Calculus as associated logic. This seems to be the stronger system, which is included in any other of the described ones.
GS

3.1.3 C. Garola, *Recovering quantum logic within an extended classical framework.*

Author's presentation. Classical Logic (CL) adopts a notion of truth as correspondence that must be distinguished from the logical and empirical criteria that specify how knowledge of truth can be obtained. Whenever a physical theory is expressed by means of a classical language one can introduce a (binary) compatibility relation on the set of all physical properties which provides a criterion for ascertaining whether the truth values of a pair of physical propositions can be empirically determined conjointly.
The features of the compatibility relation are then determined by the specific axioms of the theory, which must be distinguished by the logical axioms (note, however, that in the specific case if classical mechanics the compatibility relation is trivial, because all physical properties are supposed to be, in principle, compatible). The language of quantum mechanics (QM) adopts instead a notion of truth as verification [1] that collapses truth and knowledge of truth. The compatibility relation on the set of all physical properties introduced by QM has then a logical interpretation, and the formal properties of the connectives of the language follow from the specific axioms ruling the measurement process in QM (hence the quantum con-

nectives cannot bear the same interpretation of corresponding connectives of CL). Notwithstanding this, it is possible to recover the structure of a standard quantum logic (QL) into an extended classical framework by introducing a notion of truth as correspondence together with a derived notion of C-truth (certainly true-certainly false), so that classical connectives (interpreted in terms of truth) coexist with quantum connectives (interpreted in terms of C-truth)[2]. This alternative view can be constructed in a purely formal way, but it has been recently provided with a physical meaning by reinterpreting quantum probabilities as conditional (in a nonconventional sense) rather than absolute[3,4,5].

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Review. The talk takes into account different physical theories (classical and quantum mechanics) in order to investigate some kind of correlations occurring between empirical theories and their logical framework, and to show how this framework can lead us to different physical interpretations. Given a theory T it is always possible to obtain a T-dependent but concrete logic: if the association between classical logic, classical definition of truth and classical mechanics holds (see Birkhoff, von Neumann [1935]), this talk's aim is to prove that also quantum mechanics admits, with some devices, a description in terms of classical truth. To get such a description, the main steps are (given a set of propositions L):

1. Forget the notion of truth as verification (quantum mechanics notion of truth), and adopting the notion of C-truth (truth by certainty)

2. Define a physical pre-order relation $\prec$ on L

3. Select a set ϕ_L of propositions testable according to T

4. Define a complementation $'$ on the structure $(\phi_L, \prec)$

The most interesting notion is the one of C-truth, as it is defined for quantum mechanics: for every a into ϕ_L and for every S(state) into T;

- $a(x)$ is C-true in $S \leftrightarrow S$ takes place into T_{Ea}(for Ea intended as a specific property of the element a).

- $a(x)$ is C-false in $S \leftrightarrow S$ takes place into T'_{Ea} (where T' is the complementation of T)

Thanks to the points 1-4 one is able to prove that the algebraic structure is isomorphic to the algebra of classical logic. Hence the last one recovers quantum logic.
Due to that result Garola introduces a ESR (extended semantic realism) model, which is the physical counterpart of this new logical procedure. The objectivity of physical properties holds in ESR models, differently from standard interpretation of quantum mechanics. Therefore C-truth enrichment of quantum logic lead us to a new interpretation of both the logical framework of the theory, and of the theory itself.
GL

3.1.4 D. Provijn, *Relevance, anomaly and heuristics in the generation of abductive explanations.*

Author's presentation. In this paper I will compare the application of goal-directed proofs with the use of the tableau-like system KE for generating abductive explanations (hypothesis generation) and for lemma generation at the propositional level. The generation of abductive explanations based on goal-directed proofs, elaborated as an alternative for the algorithms from [1], was developed in [4], the goal-directed proofs were first described in [2]. Hypothesis and lemma generation by means of the tableau like system KE fulfilling are defined in [3]. Abductive explanations are defined as the product of backward reasoning process fulfilling all or some of the following five condition.
Given a theory T and an explanandum, epsilon find an A such that:

1. $T \vee A \vDash \epsilon$
2. $T \nvDash \epsilon$
3. $A \nvDash \epsilon$
4. A is minimal
5. $T \nvdash \neg A$

In the paper, T is consistent and is restricted to finite set of premises. Two types of abductive explanations will be distinguished: those that fulfill conditions 1 up to 4 will be called potential abductive explanations (pae) and those also fulfill condition 5 will be called consistent potential abductive explanations (cpae). The comparison of the approaches from [4] to [3] allows to highlight and treat three subjects related to the generation of abductive explanations: (i) a notion of relevance in the selection of premises from T for generating abductive explanations, (ii) a treatment of abductive anomaly, i.e. the situation in which $T \nvDash \epsilon$ and $T \vDash \neg\epsilon$, and (iii) a study of heuristics in view of lemma presented in [3]. The goal-directed proof procedure for propositional classical logic, elaborated in [2], looks upon proof search and proof construction as a goal-directed enterprise and therefore introduces search steps in the proofs themselves. In order to introduce search steps in the proofs, goal directed proofs for T dim epsilon contain prospective formulas of the form $[\Delta]A$ expressing that A can be derived from a theory T whenever the members of $[\Delta]$ are. In fact, the inference rules of the goal-directed procedure are such that "If [Δ] A is derivable form theory T then $T \vee \Delta \vDash A$". Besides a set of inference rules, the procedure also contains a positive part relation, which allows for a selection of "relevant" (useful) premises in view of the main goal epsilon, a set of making definitions to eliminate useless search paths and a set of heuristic instructions that result in a goal-directed decision method for $T \vDash \epsilon$. Whenever the construction of a proof for $T \vDash \epsilon$

shows that $T \nvDash \epsilon$, the proofs will contain information on how T can be extended so ϵ could be derived after all. Hence abductive explanations seem to be a natural spin-off of goal-directed proofs as shown in [4].
First I will shoe that the positive part relation, selecting those premises that can be "used" and as such are in way in the search process for ϵ, allows for a more sensible selection of abductive explanations than the one generated by means of the tableau-like system **KE**. A possible drawback of the goal-directed proof procedure is the need for an extra test if one wants to obtain cpae because the procedure generates pae. Once A is generated as a pae, an extra goal-directed proof is needed to check whether A is classically compatible with T, i.e. a proof for$T \vDash \neg A$. On the other hand, and this will be my second point, the goal-directed procedure also allows to produce pae in case of abductive anomaly. Finally, I will show that lemma generation as obtained by means of the "dynamic abduction algorithm" [3, p.557] mainly depends on the development of case sensitive heuristics. As goal-directed proofs implement search steps, allowing for an easy construction of heuristic , they give a perspective on how specific proofs can be reworked in order to obtain more elegant and shorter proofs and consequently on how the heuristic instructions need to be modified to obtain this result. Hence, they allow to construct the specific heuristics that are needed to give the "dynamic abduction algorithm" its full splendor.

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Review. Provijn analyzes abductive reasoning, showing the links between it and demonstration theory, and how abduction can be considered as a strong tool in order to assure efficiency to decisional processes. Consider a goal-directed proof such: $\Gamma \vDash G$(where Γ is a set of formulas called premises). If one wants to find a cheap strategy to get results in abductive reasoning his first aim should be the highlighting of the relevant premises: in a goal-directed proof this operation is done by the so called "positive part relation". Consider, for example,
$a = A \wedge B$; $a_1 = A; a_2 = B$ ($A, B, C \ldots$ are all formulas)
$b = A \vee B$; $b_1 = A; b_2 = B$
and the relation of "positive part" pp defined by these clauses:
$pp(A, A)$
$pp(A, a) \equiv pp(A, a_1) \wedge pp(A, a_2)$
$pp(A, b) \equiv pp(A, b_1) \vee pp(A, b_2)$
$pp(A, B) \wedge pp(B, C) \rightarrow pp(A, C)$.
One notes that relation pp reduces the number of premises highlighting the relevant ones.
The next step would be the introduction of structural rules for our procedure: Goal rule: $[G]G$ introduction of the goal
Prem rule: if $A \in \Gamma$ then introduce A introduction of the premises

introduction or elimination of the logical operators formula analyzing or condition analyzing rules
Trans rule: $(\Delta \cup B \vDash A) \wedge (\Delta' \vDash B) \vDash (\Delta \cup \Delta' \vDash A)$
EM rule: $(\Delta \cup B \vDash A) \wedge (\Delta' \vDash \neg B) \vDash (\Delta \cup \Delta' \vDash A)$
EM0 rule: $(\Delta \cup *A \vDash A) \vDash (\Delta \vDash A)$ (where $*A$ is the complement of A).
Introducing marking definitions leads to the possibility to eliminate lines in our procedure. Given such a calculus one is able to define explanatory lines:
given Γ and G the line F ($f \in \mathbb{N}$) is an explanatory line $\leftrightarrow A_1, \ldots, A_n \vdash G$ is the second element of line f with $\neg pp(A_i, G)$ and line f is unmarked. A potential abductive explanation is a set of premises $A_1, \ldots, A_n$ appearing in a explanatory line.
GL

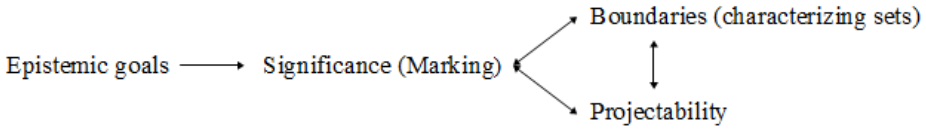
3.2 Philosophy of Biology and Cognitive Sciences

3.2.1 M. Macleod, *Inspired by Mill: an epistemic conception of natural kinds for the Life Science.*

Author's presentation. My talk concerns the relevance of the concept of "natural kind" to our understanding of scientific practice, particularly within the life sciences. As such it seeks to affect a change in our philosophical thinking away from treating natural kinds as an issue or problem for ontology towards understanding them in terms of their fundamental epistemic roles in scientific practice.
Most modern discussion of "natural kinds" these days does in fact occur with respect to the life sciences, where the concept seems central to claims of these fields but at the same time deeply problematic. Hacking however has recently questioned the value of the various natural kinds concepts, perceiving them as arbitrarily chosen ontological impositions that add nothing to our basic understanding of scientific practice, invariably cutting out categories in science that don't meet these predetermined expectations. I want to defend the usefulness of the natural kind concept in the understanding of science, but not by pursuing the standard research project of trying to discover their homeostatic causal mechanisms, but rather by arguing that natural kinds can be identified distinctly from artificial or non-natural kinds by epistemic criteria, which express belief in their "naturalness" or mind-independence in terms of productive features which in turn form basis of their use. For this I rely on aspects of John Stuart Mill's own consideration of natural kinds over 150 years ago which are remarkably sensitive to the epistemic dimension of such concepts. This gives us an approach much better suited to understanding the nature of group concept formation and use in the context of ongoing processes of research, where knowledge about casual bases may be either unknown or simply unimportant for the particular field, but there are shared practices of use and reliance on such concepts nonetheless. I will in turn present a number of insights about research practices that emerge from treating natural kind concepts in these terms in the contexts particularly of homology and functionally defined kinds in evolutionary biology.

Review. MacLeod outlines a new interpretation of natural kinds based on the close connection with life sciences' practice. He mainly argues against (Hacking [2007]) focusing on natural kind's convenience and attempts to provide an epistemologically-driven theory for scientific practice.
Many philosophers debated this topic, both through a metaphysical approach, such as the modern microstructuralism essentialism (Churchland, Ellis [2001]), the semantic essential-

ism (Krikpe [1980]) and (Putnam [1973]), the natural law-view (Fodor [1974]) or the kind-pluralism (Dupré [2002]), and through an epistemic-accomodation approach, such as the HPC view (Boyd [1988, 1991, 1999a, 1999b]), (Griffiths [2004, 2007]), (Millikan [1999]). Their results have been highly criticized: the first ones because of the counterexamples which essentialism leads to while the latter ones because of the remaining essentialism (Haeggqvist [2009]) and the lack of perspicuity in respecting methodologies (La Porte [2004] and Reydon [2009]). However, MacLeod remarks how this second perspective, which characterizes natural kinds as nuclear property clusters on which an external causal mechanism acts, seems to be a perspective very close to life science' practice.
In this viewpoint he first defines natural kinds as projectable concepts (Van Brakel [1992]): robust kinds, properties of a subset that can be projected to the class but not beyond the content of their own defined properties; then he relates to Mill's contribution (J.S. Mill [1843]) to set an epistemic criterion for natural kinds distinguishing them from epistemic artificial kinds. An epistemic natural kinds is so given by two beliefs both referred to a community of researchers: projectability and significance.
Projectability means that what is discovered of a subset holds more generally up to extension across kind members; significance, on the other hand, means that properties which characterize the kind mark at any time further unknown properties that are relevant to the epistemic purposes of the community. Avoiding any relationship with an undefined essence, they are featured as functional logically independent concepts that mark for researchers a broader set of shared properties, defining them as multi-located artefacts working as Wagner and Schenk's functional units and evolutionary stable configurations (Wagner, Schenk [2000, 2001]). Hence, in conclusion, MacLeod shows as researchers make judgements about projectability and significance of particular groupings giving the epistemic goals governing their formation and maintenance:



In conclusion, I think that MacLeod's talk illustrates an interesting proposal that leaves open a possible way to discuss the natural kinds' ontological status going beyond their simple definition in universals terms.
MS

3.2.2 S. Zipoli Caiani, *Epistemological considerations concerning the modularity of mind.*

Author's presentation. Traditional cognitive linguistics assumes language as an a-modal symbolic process grounded in the functional role of a restricted set of circuits in the brain. According to this view, language usage and understanding should be assumed as independent from action perception and execution, at the same time only dedicated modular sections of the brain should be identified as having an exclusive functional role in language processing. Notwithstanding its popularity, a radical modular theory has to face many unresolved epistemological problems such as the absence of crucial evidence that a-modal symbols exist and are

processed by strictly dedicated regions of the nervous system, and the fact that several findings from neuroscience establish that categorical knowledge is grounded in the multimodal functioning of sensory-motor regions instead of functionally dedicated areas. Contrasting the idea that language understanding is grounded in a symbolic dimension and that the categorization of abstract concepts is made possible only by an abstract and disembodied kind of comprehension, several experimental findings converge today into assuming that linguistic symbols become meaningful only when mapped on non-linguistic experiences such as those concerning environmental perception and action execution. Studies involving behavioural tasks, as well as studies involving transcranial magnetic stimulation and patients with lesions affecting inferior frontal regions of the brain have shown contributions of motor circuits to the comprehension of linguistic communication from phonemes to semantic categories and grammar. These data show that language comprehension is also grounded on the function of the frontocentral action systems, indicating that linguistic comprehension does not involve only the activation of abstract and a-modal mental representation grounded in dedicates areas of the brain; instead it also involves the critical activation of sensory-motor cognitive routines that configure a multimodal dimension for language usage and understanding. Along this time of thought, a sensorimotor approach to language states that conceptual and linguistic structures are shaped by the attributes of our perceptive skills and body dynamics, hence, it assumes that our motor activity cannot be considered as separated from our linguistic abilities. As any other scientific hypothesis, a sensorimotor approach to language must face the burden of the empirical proof. If language cognition is related with the features of our body, then experimental alteration of bodily parameters should produce measurable changes on the overt linguistic behavior and vice versa. The aim of this talk is to review a meaningful ensemble of empirical findings, from behavioral to brain studies, concerning the relationship between human bodily features and language understanding. After an introduction of several empirical outcomes from the fields of cognitive linguistics and neuroscience, my conclusion focuses on the necessity to give up a strict modular conception of our linguistic and communicative skills, therefore I propose to extend an enactive approach derived from the study of perception and action to cognitive linguistics, conceiving communication as a highly evolved and sophisticated way to interact with the world through bodily actions.

Review. Zipoli Caiani's talk concerns the relationship between language and body based on the Embodied Theory of Language (ETL). This theory claims that our language is closely related with our body and the surrounding environment and is characterized by brain, body and environment's interaction. ETL's radical form (Lakoff [1989]) upholds that:

- our concepts are grounded in the sensorimotor system;
- semantic understanding is concept retrieving;
- meaning is constituted by sensorimotor information.

In this viewpoint Zipoli Caiani tries to compare ETL to the problem of abstract concepts, which, missing any concrete reference with physical world, could undermine the sensorimotor cognition supported by the ETL. Pointing out the limits of Lakoff-Johnson's solution (Lakoff, Johnson [1980, 1999]), consisting in the a priori imagine schemas and the lack of intentionality and goal relatedness mention, Zipoli Caiani refers to the Direct-Matching Hypothesis (Rizzolatti, Sinigaglia [2008]), which shows how the observation of an action performed by others evokes the same motor activation that occurs during the planning and the

execution of an action. Then, considering the goal relatedness that characterizes this phenomenon (Umiltà *et al.* [2008]), he supposes that the meaning of words and sentences used for practical purpose is influenced by the following relationship:

Action word-sentences → evoke → Motor-goal frame → activates → Motor system

and, in order to counterchecks his hypothesis by experimental evidence, he reports some experimental results concerning both behavioral and neurobiological levels.
He shows how sentences understanding influences the execution of oriented actions (Glenberg, Kaschak [2002]), how preparing an action influences language understanding (Lindeman *et al.* [2006]) and how understanding of both action-words (Hauk *et al.* [2004]) and action-sentences (Buccino *et al.* [2005]) activate a somatotopic reaction. Highlighting moreover that the question about which system, if motor one or imaginative one, fires before regards a subliminal evidence (Boulenger *et al.* [2008]) of the threshold of 200 ms (Pulvermuller *et al.* [2005]) and the importance of motor processing for the action-related language understanding, basing on MND (Bak *et al.* [2001]) and Parkinson (Boulenger *et al.* [2008]) disease patients experiments, Zipoli Caiani concludes claiming that sensorimotor goal relatedness associated with words and sentences is preserved also in abstract contexts, i.e. the role of abstract verbs on abstract concepts (Boulenger *et al.* [2009]), but does not extend itself to all the semantic domain.
In conclusion, Zipoli Caiani succeed in arguing his thesis by clearly stressing its reasons and providing many specialist references reinforcing it.
MS

3.2.3 F. Ervas (with T. Zalla), *The role of "Naïve Sociology" in ironic* vs *literal utterances understanding.*

Author's presentation. In this study, we address the problem of irony comprehension, in order to understand whether social stereotypes play a role as contextual information in online comprehension of ironic and literal utterances.
According to Hirschfeld [1988, 1994], information is processed into social categories in order to both reduce the quantity of information and extend our knowledge of social world by capturing similarities among their members.
These taxonomies become stereotypes which provide a basis for predicting the behaviour of others and interpreting their utterances (Hamilton [1981], Spears *et al.* [1997]). Social stereotypes are part of the folk theory known as "Naïve Sociology", the spontaneous human mechanism for understanding of social groups and social relations, active from an early stage of children development (Hirschfeld [2001]). In other words, "Naïve Sociology" is a natural way to make sense of our own intuitions about the social world around us (Sperber-Hirschfeld [2004]).
The present study investigates how irony is socially perceived and whether stereotypes facilitate understanding of irony in group of adults ($N = 30$) by using a series of verbally presented stories containing either an ironic or literal utterance.
Confirming previous studies on the role of social stereotypes in irony comprehension (Katz-Pexman [1997], Pexman-Olineck [2002]), the results show that, when a character in the story has a job stereotypically considered as sarcastic, comprehension of ironic utterances improved in terms of both accuracy and latency. The results also show that, in such cases, the performance is diminished for the comprehension of literal utterances. Social stereotypes seem

to constrain anyway the on-line comprehension by creating an expectation of irony which is biased reader's understanding of literal utterances. Moreover, underpinning the hypothesis of a social function of irony (Dews-Winner [1995], Dews *et al.* [1995]), the group exhibits an overall "stereotypical" image of irony: irony is generally perceived as more mocking, but also more polite and positive.

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Review. F. Ervas' presentation is based on the most relevant results of the researches the speaker (with T. Zalla) has pursued at Institut Jean Nicod. The topic shows a deep relationship among cognitive abilities, sociology and understanding an utterance. The more this topic is interesting since it is nowadays discussed, the more a researcher can't provide a strong conclusion, and this holds for the present speech.
It is known that people suffering Autism disorder has a lot of difficulties concerning the interpretation of other minds or attributing mental states. These tests has been carried out with a group of 17-year-old people with Asperger syndrome, i.e. an high-functioning form of autism. People with this disease show difficulties in comprehension of irony, among other social and behavioural difficulties.
Not claiming to define "irony", one can notice that it is often the opposite of an utterance content, i.e. it is the opposite of what is said. It is hard to understand irony for aspergers, because they tend to interpret the meaning literally.
There are at least three points that are to be comprehended in order to understand irony:

1. the tone of voice;
2. the literal meaning;
3. that the speaker knew it wasn't true.

As for other forms of autism, aspergers may even understand irony, but they do not provide the right reasons to explain it. We can conclude that there is no pragmatic comprehension. Moving now on the topic of "Naïve Sociology", we can take for granted that understanding irony is involved in the main frame of social and pragmatic understanding of utterances. People show commonly high proficiency while relating themselves with a social group and if, as it seems blatantly, irony (e.g. understanding stereotypes) is a part of that relation, then there is a part of the knowledge involved in "everyday sociology" that is not shared by aspergers. Thus we may conclude that the use of sociological knowledge by aspergers has just a communicative task. The reasons for that are discussed, such as in general the reasons for autism. GS

3.2.4 M. Tacca, *Does structure matter for content?*

Author's presentation. The overall goal of argue against Heck [2007] that the non-conceptual content of vision does not depend on how visual representation combine but, instead, might depend on how spatial informations are represented in the visual system.
The thesis of non-conceptualism says that representational content is not determined by our conceptual abilities; namely, a subject does not need to exercise the same conceptual abilities at play, for example, in thoughts in order to perceive objects in the world. Most of the work in the debate over non-conceptual content has focused on a series of arguments in favor of the thesis that the content of perception is non-conceptual. Among those, the argument of the fineness of grain has been often cited as the most relevant. With fineness of grain, one indicates that the content of perceptual experiences captures far more details that we have concepts for. However, Heck noticed that the "fine-grained" argument establishes at best a weak notion of non-conceptual content - the state view - that is neutral on which kind of content perceptual experiences should have. Thus, if one wants to support the non-conceptualist thesis, one has to satisfy a stronger version - the content view - according to which, perceptual states and cognitive states have different kinds of content. What do these different kinds of content amount to? Heck proposes that the content of cognitive representation is structured in a way that satisfy a strong version of Evans' Generality Constraint (GC henceforth).
According to GC, structurally related thoughts involve the recombination of the same representations, and this recombination requires the subject's conceptual abilities to grasp the content of the constituents. On the opposite, the content of perceptual representation is unstructured; namely, visual representation do not satisfy GC. This is because, according to Heck, visual representations have a spatial structure that does not allow for systematic recombinations of primitive visual representation. Since, the content of a representation is partly determined on how representations combine, then, given the difference in structure between perception and cognition, the content of visual perceptual representations is non-conceptual.
I will argue that Heck's argument does not establish the content view. First, by considering evidence from vision science, I will argue that spatially structured visual representations satisfy GC. This conclusion seems at first to count for the conceptualist thesis. In fact, within a strong reading of GC, the systematic recombination of primitive representations requires a

subject to possess conceptual abilities. In the case of visual representations, those conceptual abilities might be explained in terms of the subject's ability to identify objects in the world. But it seems that there are still some reasons for being a non-conceptualist. For example, if non-conceptualism is true, then we can rely on a bottom-up theory of human rationality, in which conceptual capacities are at least partially explained in terms of more primitive psychological capacities shared with many non-human animals. Do we have, then, an alternative account for the non-conceptualist thesis? I propose to consider a weaker version of GC that simply states that there is a certain kind of pattern in our cognitive capacities. In this form, GC leaves open whether every kind of systematic recombination requires subjects' conceptual abilities. Then, I will consider empirical evidence for visual object representations, and argue that we might account for the non-conceptual content of visual representations by considering how spatial information and relations are represented within the visual system.

Review. Referring to the contemporary debate on conceptual contents, the talk starts from the Generality Constraint Principle stated in Evans [1982] as follows: «Thus, if a subject can be credited with the thought that a is F, then he must have the conceptual resources for entertaining the thought that a is G for every property of being G of which he has a conception» (Evans [1982], p. 104).
For instance, if a subject think "a is F" and "b is G", he can think both "a is G" and "b is F". I can't deepen here the discussion on this principle, anyway the talk face to Heck's argument that perceptual experience doesn't satisfy GC. In this view-point conceptual and non-conceptual contents are entirely different and thus beliefs and perceptions show the same difference (Clapp, Varieties of the Generality Constraint, forthcoming, available here: `http://www.filosoficas.unam.mx/~lclapp/documents/Varieties.pdf`). Moreover, if GC is a systematic principle (Johnson [2004]), namely it is equivalent to a systematic constraint principle, Heck [2007] notice that even if cognition could be systematic for GC, perception is not determined as well. Tacca argues against this conclusion since vision is systematic. Vision shares a common property with the structure of cognition, i.e. it satisfies GC.
The author's presentation lack in bibliography and references, thus I tried to furnish them within my report, although surely inadequate. There is not much to say about the talk, which was pretty interesting but quite unclear.
One might summarize that arguments have not been completely developed and that that weakened the speech.
GS

3.3 Philosophy of Physical and Mathematical Sciences

3.3.1 C. Calosi, *Persistence and Change in Minkowski Spacetime.*

Author's presentation. I offer a new relativistic argument against a particular metaphysics of persistence, namely Threedimensionalism. The bulk of the paper is the presentation of a new puzzle of change Threedimensionalism is unable to solve. The argument depends crucially on the geometric structure of Minkowski spacetime.

Review. Calosi considers, well arguing, two different metaphysical structures, threedimensionalism and fourdimensionalism, highlighting the benefits offered by the second system. Starting with the assumption that an object is considered persistent if and only if it does exists at two different times, no matter how it does (Lewis[1986]), it is possible to introduce the

definition of object both for threedimensionalism and fourdimensionalism, comparing them with the notion of change previously introduced.
For threedimensionalism an object is persistent if the space region where it is located is temporally unextended. Instead for fourdimensionalism the object is persistent if his space-time coordinates permute.
Let us consider an object x and one of his proper part y: how do we qualify x when y is moving into it? If we consider threedimensionalist point of view we are unable to note what happened, because of the definition of object: namely it seems that if a specific position of y into x is a property F one can say that both $F(x)$ and $\neg F(x)$ are true, which is inconsistent, unless we renounce at persistence of x.
But if one considers Minkowski spacetime and his structure adopting a fourdimensionalist point of view, where the leases of an object can share parts (the specific term is "overlap"), x is temporally extended and the property F could be owned in t (time instant) but not in t', so that:

- $F(x)$ is true in t
- $\neg F(x)$ is true in t'.

Therefore the notion of change obtains a rigorous accommodation into mereology, compatible with that one of persistence.
GL

3.3.2 A. Melas, *A common cause model for quantum co-relations.*

Author's presentation. After the so called Einstein-Podolsky-Rosen (EPR) thought experiment in 1935, many physicists and philosophers considered Quantum Mechanics as an incomplete theory that had to be completed by means of hidden variables in order to store a realistic image of the world. The most often discussed version of the EPR experiment is due to David Bohm (EPR/Bohm experiment), in which the main aim is to find a causal mechanism that produces the correlations between the spin of the two particles.
Two decades ago the debate about causal inference in quantum realm became particularly lively, and a large part of the literature tried to provide a causal explanation for EPR/Bohm correlations. Most of these works are based on a common cause solution and they adopt some version of Reichenbach's conjunctive fork model. Unfortunately, these attempts do not avoid bell's theorem. Starting from algebraic results of Hofer-Szabó, Szabó and Rédei on Reichenbach's common cause principle and its applicability, and starting from Suárez and San Pedro's work on the Principle of Common Cause and indeterminism, a causal, local and no-conspirative solution for EPR/Bohm correlations is offered. This solution endorses the Cartwright's idea that the best way to explain quantum correlations causally and locally is to adopt, for non deterministic contexts, a non-Raichenbachian common cause model, namely the general fork model.

Review. As many talks at the conference, Melas takes as subject Quantum Mechanics. During the debate about the completeness of the theory, many possible models have been presented, in attempt to justify quantum correlations, like entanglement. The fundamental concepts in this field of physics and philosophy are locality and determinism: with locality one intends a particular property of a function ψ describing a physical system and the behaviour of an observable which is factored while determinism means that the predictability

has value 1 of the analyzed observable, without loosing any kind of information. Assuming David Bohm's notion of hidden variables [1951, 1957], one is able to define strong locality as the union of locality, like Bell does, and theory's completeness. Bell's inequalities content increased in being the focus of last years studies, instead of Quantum Mechanics predictions, and this changing perspective has been emphasized by Melas. Due to that, it seems rightful to assume that it is possible to give models assuming both Bell's inequalities and completeness (in addition to locality). To introduce those models it is necessary to spend a few words on reichebachian concept of screening-off condition of common causes: a correlation between two or more events A and B such that,

- $P(A \wedge B) > P(A) \cdot P(B)$ (where $P(X) =$ probability of X)

suggests the idea that exists a common cause C for either of them such that:

- $P\left(\frac{A}{C}\right) > P\left(\frac{A}{-C}\right), P\left(\frac{B}{C}\right) > P\left(\frac{B}{-C}\right)$
- $P\left(\frac{(A \text{ and B})}{C}\right) = P\left(\frac{A}{C}\right) \cdot P\left(\frac{B}{C}\right)$

C is considered the screening-off conditions for such a correlation between A and B. The screening-off condition C can be separated (if one finds different common causes $C_1, C_2, \ldots C_n$) or considered common (if C is enough to explain the correlation).
For many years it has been a big issue to decide which kind of common-causes were better indicated to explain quantum correlations, like entanglement.
Melas purpose is to assume an analogous of the Cartwright's model [1987], where independently from the kind of common cause assumed, we find an explanation which looks out to be the best possible model to justify both causally and locally EPR/Bohm correlations.
GL

3.3.3 F. Boccuni, *Sheep without SOL: the case of second-order logic.*

Author's presentation. The fact that set-theoretic semantic is the standard interpretation for second-order logic (SOL) has been disputed by George Boolos's plural quantification. Since plural logic (PL) and SOL are inter-definable, they seem to provide equal alternatives. This seems to rely merely on ontological (or at least broadly philosophical) preferences.
In the present article, I am going to address a non-ontological argument for a distinction between PL and SOL. This argument will be grounded on the different mathematical applicability that, respectively, PL and SOL show to have. I will present a second-order predicative Fregean set-theory augmented with PL and I will show that its mathematical strength is significantly different from that of the same second-order predicative set-theory augmented with SOL. My conclusion will be that, in spite of the inter-definability of PL and SOL, some substantial differences between them may be found in their applicability to mathematical discourse, in particular in the different mathematical strengths of (some) second-order set-theories.

Review. Starting from George Boolos's works (Boolos [1984, 1985]) and the inter-definability between second-order logic (SOL) and plural logic (PL), Boccuni argues for a distinction between the two systems based on their different mathematical applicability. Without any appeal to ontology, the two frameworks are offered by (in the language of) second-order logic of fregean set-theory, one augmented with PL (P-PV) and one with SOL (S-PV).

PV, P-PV and S-PV's common base, referring to a sub-system of (Heck [1996]), is characterized by this axioms:

Axiom (PRC) $\exists F \forall x(Fx \leftrightarrow \phi x)$ (where ϕx does not contain F free nor bound second-order variables);

Axiom (V) $\{x : Fx\} = \{x : Gx\} \iff \forall x(Fx \leftrightarrow Gx)$.

P-PV, obtained from the extension of PV by PL, shows: an infinite list of plural individual variables xx, yy, zz, ... that vary plurally over the individuals of the first-order domain; the constant η; the existential quantifier $\exists$ for plural variables; while the atomic formulae of $L_{(P-PV)}$ are the same of L_{PV} plus $x\eta yy$. Its axioms are:

Axiom (PLC) $\exists xx \forall y(y\eta xx \leftrightarrow \phi y)$ (where ϕy does not contain xx free);

Axiom (PRC*) $\exists F \forall x(Fx \leftrightarrow \phi x)$ (where ϕx contains neither F free, nor free plural variables, nor bound second-order variables. It may contain free second-order variables and bound plural variables);

Axiom (V*) $\{x : Fx\} = \{x : Gx\} \iff \forall x(Fx \leftrightarrow Gx)$.

S-PV instead adds PV with a further round of second-order logic variables, varying over classes in general, while its axioms are:

Axiom (Impred-CA) $\exists X \forall y(Xy \leftrightarrow \phi y)$ (where ϕy does not contain X free);

Axiom (V+) $\{x : Fx\} = \{x : Gx\} \iff \forall x(Fx \leftrightarrow Gx)$;

Axiom (PRC+) $\exists F \forall x(Fx \leftrightarrow \phi x)$ (where the formulae on the right-hand side of PRC, PV's axiom, cannot be extended to those containing S-PV's general class variables X, Y, Z, ... neither free nor bound).

Focusing on P-PV and S-PV's axioms Boccuni shows that S-PV system has two sorts of second-order variables with a common domain where general class variables are not allowed on the right side of PRC+ . Hence, in S-VP, general classes do not correspond to sets. On the other hand, since P-PV plural variables and second-order variables have two disjointed domains, one may freely interpret axiom V*.
In conclusion she presents three translation theorem among P-PV and S-PV proving that S-PV interpret a fragment of P-PV while the converse does not hold and concludes claiming that P-PV is mathematically stronger than S-PV.
In my opinion, this technical talk, although the requiring expertises about second-order logic, was clear in every theoretical shift.
MS

3.3.4 C. Mazzola, *Becoming and the Algebra of Time information.*

Author's presentation. It is a trend in contemporary philosophy of science to deny the existence of objective becoming: the idea of a "moving now", of a sequence of instants progressively coming into existence and then passing away, is often maintained to be ruled out by contemporary space-time theories. Since the rise of special relativity theory, time is represented as one of the four dimensions of a differentiable manifold, along which space-time points or events are partially (local linearly) ordered by the relation "before than" (symmetrically "after than"). According to the mainstream position, this picture would support a static conception of time (block-universe), according to which all events are given once and for all,

though they distribute on different space-time locations and are consequently ordered respect to the before-after relation. The hypothesis that time might possess an internal dynamics, the idea of a moving now, is then dismissed as having no objective significance or being theoretically superfluous (e.g. Weyl [1949]; Capek [1961]; Grünbaum [1973]; Horwich [1987]; Price [1996], just to cite few). I challenge this very charge of irrelevance, for there exists an essential feature of time -its algebraic structure- on the basis of which time can be endowed with an intrinsic dynamics. Arnold [1998] associated any real-valued differential equation on a n-dimensional differentiable manifold (state space) M with a phase flow or a continuous dynamical system, i.e. a family of diffeomorphisms from M to M, indexed by the set R of time intervals. Giunti and Mazzola [2010] generalize this model to an arbitrary deterministic system by simply taking M to be a non-empty set (also called the state space) and by replacing the time set R with an arbitrary monoid $L = (T; +)$ (i.e. a non-empty set together with an associative binary operation and an identity element), which they called the time model. This way, they obtain the weakest mathematical structure (called a dynamical system on L) capable of describing the temporal evolution of a deterministic system; such a structure consists of the state space M together with a family, indexed by T, of functions from M to M, which satisfy an identity and a composition condition.
It is a well-established result of the algebraic theory of monoids that any monoid can be associated, via a left monoid action, to a unique family of transformations on itself (Clifford and Preston [1961]). On this basis, each monoid L can be equivalently described as a special kind of dynamical system on L called its time system, whose dynamics is uniquely determined by the algebraic properties of L; in particular, the time system of any monoid con be understood as a representation of the orbit of its identity element.
Being locally diffeomorphic to (a subset of) the real line, physical time is naturally endowed with the algebraic structure of a monoid; as such, it is intrinsically associated with a time system on itself, which describes the orbit of the unique instant whose coordinate is zero - namely, the unique instant at which local physical clocks are initially set. For each frame of reference, that instant naturally represents the present time, i.e. the local "now". According, though in a relativistic ontology there can be no place for a unique present moment, it is still physically meaningful for each frame of reference to speak of a local moving present, whose dynamics is objectively rooted in the algebraic properties of local time.

Review. The problem of Temporal becoming in physical theories is characterized by Mazzola through three key-points, referring to Price's concepts (Price [2010]):

- the static component
- the dynamic component
- the directional component

These points synthesize three pre-theoretically intuitions: the first one concerning that at each time there exists a unique objectively distinguished present moment, the second one that there is a continuous shift of the present, while the last one that the present moment moves from past to future. In order to treat the time's development in a metaphysically neutral sense with a suitable algebraic structure and to define the time's oriented direction, he identifies in a specific dynamical system (Giunti, Mazzola [2010]) the least mathematical structure needed to model the evolution of a deterministic system. This kind of system is featured as an ordered pair $DS_L = \left(M, (g^t)_{(t \in T)}\right)$ if and only if:

- M is a not-empty set;

- $L = (T, +)$ is a monoid with identity 0;
- $(g^t)_{(t \in T)}$ is a family of functions on M, indexed by T;
- for any $x \in M$ and any $t, v \in T$, $g^0(x) = x$ and $g^{(t+v)}(x) = g^t(g^v(x))$;

where M is the phase space of the quasi-dynamical system, T is the time set, L is the time model and for any $t \in T$ the function $g^t : M \to M$ is a state transition of duration t.
At this stage, through a reduction on a single monoid, Mazzola shows a time system (namely an ordered pair of a monoid) that describes the internal dynamics of a time model $L = (T, +)$ and can be seen as the dynamical component of becoming in physical theories. In this theoretical background, showing also that the time system and monoids have the same mathematical structure (Giunti, Mazzola [2010]), Mazzola gets along defining:

1. the orbit of a point (for any $x \in M$ the set $orb(x) := \{y \in M : \exists t \in T(y = g^t(x)\})$ illustrating how the dynamics of a time system concerns the dynamics of the identity element;
2. the t-Future of a point ($F^t(x) := \{y \in M : y = g^t(x)\}$);
3. the t-Past of a point ($P^t(x) := \{y \in M : x = g^t(y)\}$);
4. a model for the future ($F(x) := U_{(t \in T)} - \{0\}^{(F^t(x))}$), the past ($P(x) := U_{(t \in T)} - \{0\}^{(F^t(x))}$) and the present ($\Gamma(x) := \{y \in M : y = g^0(X)\}$) of a point, which explicit the existence of an uniquely distinguished present moment at each time, answering to the second key-point;
5. the Garden of Eden ($g^t(y) \neq x$), the primitive states composed by points with no past history from which a dynamics starts.

Mazzola concludes asserting that the problem of time becoming in physical theories is closely related with the particular algebraic features of the time model adopted by each specific theory.
MS

3.4 Epistemology and the History of Sciences

3.4.1 S. Bordoni, *From analytic mechanics to energetics: Duhem's early steps towards complexity.*

Author's presentation. Although Pierre Duhem's history and philosophy of science are well known to historians and philosophers, his physical theories are definitely less known. Since 1886 to 1896 he undertook a demanding project of unification. He tried to unify physics and chemistry according to a double strategy: the first principles corresponded to the two laws of thermodynamics, and the mathematical structured consisted of a generalization of analytic mechanics. In 1896, at the end of a decade of theoretical researches, he published an extensive essay, *Théorie thermodynamique de la viscosité, du frottement et des faux équilibres chimiques*: starting form general equations for thermodynamics, he put forward a unified description of irreversible processes in mechanics, electromagnetism and chemistry.
That unification allowed him to cope with the complexity of the physical world: phenomena outside the scope of ordinary mechanics, thermodynamics, or thermo-chemistry could find a suitable description in the new theoretical framework. He outlined a generalize physics of

"qualities", which renewed the Aristotelian tradition of natural philosophy. Classic mechanics, electromagnetism, thermodynamic and chemistry were different instances of a physics of transformations, wherein the concept of "transformations" was nothing else but the translation of Aristoteles' wide concept of "motion". Conversely, mechanical motion became the easiest instance of a wider class of "motion" or transformations.
Both historians and philosophers of science have made use of scholarly words in order to describe Duhem's scientific heritage: some words are energetics, thermodynamic view of nature, and anti-mechanism. Are the suitable words, or merely "consolations for specialists"? With regard to the first label "energetics", we can notice that Duhem gave it the meaning of generalized Thermodynamics rather than the meaning of a world-view or a general meta-theoretical commitment. We find a remarkable conceptual distance between Duhem and some upholder of "energetics" like G. Helm and W. Ostwald. If Duhem developed a sophisticated mathematical theory of thermodynamics, the latter insisted on the principle of conservation of energy as the sole foundation of physics. In particular, Ostwald developed a physical world-view wherein, in Ostwlad's words «the concept of matter, which has become indefinite and contradictory, has to be replaced by the concept of energy». In no way the name of Duhem can be associated to that kind of "energetism".
With regard to the second label "thermodynamic world-view", we can notice that in reality Duhem tried to found all physics on the two principles of Thermodynamics, but at the same time, he translated thermodynamics into the language of analytic Mechanics. We could say that we find in Duhem both a mechanical foundation of thermodynamics and a thermodynamic foundation of mechanics. With regard to the third label "anti-mechanism", he refused to make use of specific mechanical models of heat but, at the same time, made recourse to mechanical analogies in order to describe other physical phenomena. An instance of these analogies can be found in Duhem's 1896 analysis of chemical "false equilibrium", wherein the comparison with the motion with friction along an inclined plane is put forward. The analysis of Duhem's physics allows us to better understand the specific kind of his anti-mechanism. Hid did not trust in specific mechanical models like atomic or kinetic models at a microscopic level, but relied on mechanics as a conceptual and mathematical structure, which had to be enlarged in order to include the description of more complex events.
In Duhem's 1886-1896 theoretical physics, in particular the structural analogy between energetics (in the sense of generalized physics) and analytic mechanics we can find the roots of his subsequent epistemology as well as the roots of the twentieth century theory of complexity.

Review. Bordoni's historical speech constisted in pointing out Duhem's philosophical framework from his scientific works.
Placed into the debate between Mechanics and Thermodynamic in the last decades of the XIX century, Duhem's contribution to physical sciences reflects many of his philosphical insights. Physicists were divided into two groups: those who endorse mechanics, explaining the reality in terms of motion, position and momentum, and those who endorse energetics, giving importance to the notion of energy as explanatory tool. With his holistic point of view, Duhem collocates himself between these two positions.
His approach to energetics shows his philosophical background: when Bordoni says that Duhem tried to ground all physics on the two principles of thermodynamic, it is important to notice that both are translated into the language of analytic mechanics; this remarks that it is impossible to describe his thought into one specific stream, but is necessary to watch all Duhem's works in the field of physics by the light of his epistemology.
GL

3.4.2 M. Toscano, *Poincaré and determinism.*

Author's presentation. One of the topics of the Epistemology of Complexity is the criticism of classical determinism. In my Ph.D. thesis I analyzed the contribution gave by Jules Henry Poincaré (1854-1912) to the discovery of chaotic dynamics and the influence of such a discovery on his epistemological thought. The aim of my paper is to point out the new meaning given by Poincaré at the determinism by the light of his scientific and of the French Philosophical context.
Since 1881 Poincaré introduced a new qualitative approach in the study of differential equations. He immediately understood the possibility to employ this new geometrical method to several fields, like, for example, celestial mechanics; Poincaré recognized the possibility to approach the three body problem through a new perspective, not focused on the analytical resolution but on the demonstration of the global stability of the system. In the next five years he developed the mathematical tools necessary to solve such a problem.
In the meanwhile he was interested in many other fields and in 1885 he published on the Swedish journal Acta Mathematica his paper *Sur l'equilibre d'une masse fluide animée d'un mouvement de rotation*, where he introduced for the first time the term "*bifurcation*": Poincaré proved that a particular equilibrium shape of a rotation fluid mass could be a part of several equilibrium shapes sequences. So, considering such a particular equilibrium, its past and its future were not determinable in an univocal way.
Four years later, in 1889, Poincaré won a mathematical competition, organized by the Acta Mathematica, with an article concerning the three body problem. In such a work he gave a proof of the stability of a three body system (in the so called "restricted case"). Only later he found a mistake that invalidated his proof: this was the discovery of Chaotic Dynamics. In the following years, Poincaré dedicated a part of his scientific works to such a subject and in his monumental work *Les Méthodes nouvelles de la Mécanique Céleste* he introduced the homoclinic points. Both in his works on the three body problem and in the one of the stability of the fluid mass, Poincaré clashed with the limits of classical determinism.
These scientific discoveries can be considered at the basis of the following analysis of determinism formulated by Poincaré in his epistemological works like *Le valeur de la science* (1905), *Le hazard* (1907) and *Dernières Pensées* (1913). Also the French philosophical context has to be considered to understand Poincaré's definition of determinism. The French scientist was forced to reply to the attacks moved by Le Roy, Bergson and Boutroux against scientific knowledge. Poincaré on one hand was conscious of the necessity to reform the principles of classical science; this was the case of the determinism. Poincaré saved determinism, but he gave it a new interpretation by the light of the intrinsic limits it revealed.

Review. Toscano's talk is a brief, and merely historical overview on Jules Henri Poincaré and his works on determinism. As one of the most important authors in the field of Dynamic of Chaos, it might be interesting to see how his thought gets in touch with the notion of determinism, as it was commonly conceived in XIX century. Poincaré's point of view on determinism is in some ways connected with the most important epistemological concept he introduced; i.e. conventionalism: scientific theories are just conventional, insofar they're all based on assumptions which are free constructions. It is impossible to prove them in a definitive way. The theory of chaos shows that even determinism has to be considered such an assumption. In Poincaré's view, a determinism vindication has to be done arguing that it is

the most convenient theoretical item to make predictions with a low margin of error. As one can see in *La Science et l'Hypotèse*, the same argument could be given in the field of Geometry, where euclidean geometry is chosen, despite riemmanian and lobacevskijan, thanks to a pure pragmatical crietria.
GL

3.4.3 G. Gherardi, *Alan Turing and the foundations of computable analysis.*

Author's presentation. The relevance of Alan Turing's work for the rigorous treatment of the notion of "effective calculability" is well-known. Nevertheless, the profitable employment of Turing machines for the characterization of computable functions of an integer variable has obscured his important achievements in computability theory for real numbers and real functions. In particular, this is the case with his famous paper *On computable numbers with an application to the* Entscheidungsproblem (Turing [1936]), as the title itself suggests: since all natural numbers (and rational numbers) are trivially computable, it is manifest that Turing's interest pertained to real numbers in a peculiar way. Currently, there is no accepted extension of the "Church-Turing thesis" to the domain of real numbers: different approaches to computable analysis, such as the "Type 2 Theory of Effectivity - TTE" (Weihrauch [2000]) and the "real-RAM machine" model (Blum *et al.* [1998]), constitute non-equivalent paradigms. Nevertheless, these approaches develop ideas and methods introduced by Turing. In particular, Turing [1936] and Turing [1937] provide a foundation for TTE, whereas the techniques developed in Turing 1948 are more related to the real-RAM model.
In Turing 1936, Turing introduced the first definitions of "computable real number" and "computable real function" and proved some important results that still characterize these concepts in contemporary computable analysis. In Turing [1937] Turing discovered that the well known decimal representation of real numbers is not satisfactory for computability theory and replaced it by a new representation which is an early example of an "admissible representation", according to currently established terminology. Admissible representations are important as they provide a natural theoretical environment for the investigation of the computable properties and functions on many topological spaces, such as the Euclidean space. Finally Turing's theory of algorithms contained in Turing [1948] introduced a notion of computational complexity substantially accepted by the real-RAM machine model, and presented a method of dealing with approximation errors (based on the notions of a condition number and of an ill-conditioned system) which is still used in numerical analysis.
Although the importance of Turing's work in computability theory for real numbers has explicitly been recognized in computable analysis literature (Blum [2004], Cucker [2002], Miller [2004], Weihrauch [2000]), no systematic investigation on this important aspect of Turing's scientific research has been developed yet, to my knowledge. The aim of my talk is then to give a first survey of the main results obtained by Turing in this field.

Review. Gherardi illustrates Turing's important achievements in computability theory for real numbers and real functions focusing on his two famous papers: Turing [1936] and Turing [1937].
Starting from the introduction of Turing machines for computations on infinite bit sequences based on the Theory of Effectivity (TTE) developed by the computational non-halting problem, Gherardi provides a definition of a representation (for a set X, $p_x :\subseteq \{0,1\}^N \to X$, where the pair (X, p_x) is called represented space) and illustrates two admissible representations for

real numbers (Turing [1936]):

- the standard representation: $\delta_R : p \in \{0,1\}^N$ is a standard name of $x \in \mathbb{R}$ if p encodes an enumeration of all open intervals (x_0, x_1) with $(x_0, x_1) \in \mathbb{Q}$, such that $X \in (x_0, x_1)$;
- the Cauchy representation: $\delta_R^C : p \in \{0,1\}^N$ is a Cauchy name of $x \in \mathbb{R}$ if p encodes a sequence of rational numbers $x_0, x_1, x_2, \ldots$ such that $|(x_n - x_m)| \leq 2^{-n}$ for $n < m$ and $\lim_{n\to\infty} x_n = x$.

concluding that a representation of p_R of $\mathbb{R}$ is admissible when $p_R \equiv_t \delta_R \equiv_t \delta_R^C$ while for every base k, $p_{(k,b)} \equiv_t \delta_R^C$. Defining moreover the realizers, Gherardi approaches to Turing rejection of the binary decimal expansion representation of real numbers (Turing [1937]), following the discovery of Cauchy representation's insuitability. Hence Turing's new representation β_R associates any sequence $gamma = i1\ldots10c_1c_2c_3$ (with $1\ldots1$ n-times), for $i, c_j \in \{0, \}$, with the real number $\alpha = (2i-1)\cdot n + \sum_{r=1}^{\infty}(2c_r - 1)\cdot\left(\frac{2}{3}\right)^r$; while it determines the same class of computable numbers of Cauchy's one (Turing [1937]), it also provides possible corrections in order to identify more precisely the particular real number.
On the other hand, defined the real function, $f(\alpha_n) = \alpha_{(\phi(n))}$ (Turing [1936]), Gherardi shows how the Intermediate Value Theorem (Turing [1936]) is the first explicit example of computational interpretation of a classical analysis' theorem, based on Turing's definition which, providing discontinuous computable real functions, is unacceptable for TTE.
In conclusion this talk was very interesting, well-argued, explicit in each step and it showed an important aspect of Turing's work that is not always put in relevance with mastery and accuracy.
MS

3.4.4 K. Krzyzanowska, *Belief ascription and the Ramsey test.*

Author's presentation. Sentences about beliefs have been perplexing philosophers since the very beginner of analytic philosophy. Questions about their proper use as well as questions about cognitive mechanisms that underpin their production and comprehension are also widely discuss in psychology. Even after three decades of discussion, there is still no agreement among psychologists regarding many aspects of our ability to ascribe beliefs (especially false ones) to others. In my presentation I am going to focus on a hypothesis proposed by Riggs *et al.* [1998] and discuss possible consequences of it for the semantics of belief reports. Riggs *et al.* suggest that there is a close connection between understanding of other people's false beliefs and counterfactual conditionals. Both seem to involve a mechanisms that Riggs *et al.* called "modified deviation". This mechanism is strikingly similar to the "Ramsey Test" (Ramsey [1929]) according to which we decide whether to accept a conditional statement "If p, then q", by adding hypothetically the antecedent p to our stock of beliefs, making minimal changes to maintain consistency, and then deciding whether q is acceptable in the resulting hypothetical belief state.
Specifically, the connection between reasoning with counterfactual antecedents and false beliefs ascription is this: in the standard false belief task (Wimmer and Perner [1983]) a child has to "put herself in the protagonist's shoes" to answer a question about the protagonist's belief about the location of a given object. The child has to delete from her own stock of beliefs only those pieces of information that the protagonist is not aware of, namely that object has been removed. Her modified belief set plays a similar role as a belief set modified by an antecedent of a conditional: it determines the closest possible world in which the counterfactual

information is true.
Stalnaker [1968] developed Ramsey's idea into a full-fledged semantics for conditionals. According to this, one decides whether to accept a conditional statement "If p, then q", by considering the closest possible world in which p is the the case and determining whether q holds in that world.
Formally, there is a selection function that takes an antecedent of a conditional and a world where the conditional is evaluated as arguments and (the nearest) possible world as a value. In view of the aforementioned connection between belief ascription and counterfactual conditionals, it is natural to think that we can modify this theory in a way that it will yield a model for belief ascription. The crucial modification concerns the first argument of the selection function. In case of belief reports it is a set of propositions P that an ascriber has to revise his own belief set with to simulate the agent's state of mind. For every agent A whose beliefs are reported and any possible worlds x, y there is a selection function f such that:

$$f(P, x) = y$$

where x is a world in which the belief report is evaluated and y is one of A's belief worlds.
Belief report "A believes that p" is true if and only if p is true in the possible world y that is a value of $f(P, x)$. Formally:

$$v(\text{“}A \text{ believes that } p\text{”}, w) = 1 \textit{ iff } v(P, f(P, x)) = 1$$

References:

1. Ramsey. F. [1929/1990], General propositions and causality, in D.H. Mellor, ed., "Philosophical Papers", Cambridge UP, pp.145-163.

2. Riggs, K.J. et al. [1998], "Are errors in false belief tasks symptomatic of a broader difficulty with counterfactual-ity", Cognitive Development 13, 73-90.

3. Stalnaker, R.C. [1968], A theory of conditionals, in J.W. Cornman, A. Hausman and C. Echelbarger, eds, "Studies in Logical Theory", American Philosophical Quarterly: Monographs 2, Oxford: Blackwell, pp.98-112.

4. Wimmer, H. and Perner, J. [1983], "Beliefs about beliefs: Representation and constraining function of wrong beliefs in young children's understanding of deception", Cognition 13(1), 103-128.

Review. Krzyzanowska shows a formal model for belief ascription based on Riggs *et al.*'s hypothesis of the modified deviation (Riggs *et al.* [1998]) concerning the connection between false belief ascription and counterfactual conditionals.
Showing how beliefs are related to mental representation and how belief reports refer to intentional contexts (Premack, Woodruff [1978]), Krzyzanowska, basing on Wimmer and Perner's task (Wimmer, Perner [1983]), quotes "The post-office story": a false belief test that illustrates the problems of reality's representation with counterfactual situations. In this view-point, she introduces the Ramsey Test (Ramsey [1929]) and the Stalnaker's theory of conditionals (Stalnaker [1968]), and exposes her proposal: through changing the first argument of the selection function an ascriber can revise his own beliefs' set relating it to the agent's one. Hence formally:

$$v(\text{“}A \text{ believes that } p\text{”}, w) = 1 \textit{ iff } v(P, f(P, x)) = 1$$

This talk proposed an interesting attempt to treat belief ascription in a semantic formal model based upon the most important achievements of the theory of mind and counterfactual conditionals.
MS

3.5 Methodology and Philosophy of Science

3.5.1 L. Tambolo, *The Verisimilitudinarian* versus *the Epistemic Approach to Scientific Progress.*

Author's presentation. In this paper I discuss the rival approaches to scientific progress mentioned in the title. First, I show that an argument recently proposed by Alexander Bird against the verimilitudinarian approach (VS) is flawed (Section 2). Secondly, I argue that the epistemic approach (E) defended by Bird, besides having no obvious advantages over VS (Section 3), cannot explain some causes of progress that VS ought to be preferred to E. VS is the view that progress can be explained in terms of the increasing verisimilitude of scientific theories. Advocated by authors like Ilkka Niiniluoto (1999) and Theo Kuipers (2000), VS has been attacked by Bird (2007a, 2008) because of its alleged inability to account for the intuition that, in order for a sequence increasingly verisimilar beliefs to count as progressive, these beliefs must be appropriately grounded in the evidence. Bird tries to make his case by devising a hypothetical example in which, by sheer luck, the use of an irrational method for generation of beliefs (such as astrology) yields a sequence of increasingly verisimilar beliefs. According to Bird, a supporter of VS is bound to consider such a sequence as a genuine instance of progress. In fact, if the search for highly verisimilar beliefs (theories) is viewed as the main aim of science, then any such sequence must count as progressive. But our intuitions say that this cannot be the case, since a genuinely progressive sequence of beliefs must be justified - that is, it must exhibit an appropriate grounding in the evidence. Therefore, VS cannot be the correct account of progress.
This line of argument, I claim, may seem compelling only to those who ignore a substantial body of literature, in which the upholders of VS have proposed various methods to deal with the problem of the justification of the estimates concerning the relative verisimilitude of scientific theories (the so-called "epistemic" problem of verisimilitude). Therefore, contrary to what Bird claims, no supporter of VS is bound to consider as progressive a sequence of unjustified increasingly verisimilar beliefs. According to Bird, a crucial advantage of E - which views scientific progress as the accumulation of knowledge - is that knowledge is a complex "object" which, unlike verisimilitude, includes both the ingredients that are necessary for progress, i.e. truth and justification. Indeed, according to Bird (2007b, 2010), since knowledge entails truth and justification, the conclusion that progress and the accumulation of knowledge are coextensive follows quite naturally.
I argue that this conclusion does not follow. First of all, with respect to Bird's account of justification, it must be noted that it revolves around the idea that an agent X's beliefs are justified *iff* they were formed in "a legitimate way" - i.e., in such a way that, if the environment reacts normally to X's cognitive efforts, then these efforts will (typically) be successful. This idea is sound, but it does not seem to land specific support to E, since it is compatible, for instance, also with a reliabilist account of progress, and in any case it certainly fits the key intuitions underlying VS. Secondly, and more importantly, given that on Bird's account knowledge entails truth, E cannot reconstruct as progressive certain theoretical transitions, generally considered as progressive (e.g., from Aristotelian to Newtonian mechanics), that

involve high-level theories - at least, if one accepts the very plausible view that high-level theories are, strictly speaking, false.
For the above reasons, I conclude that VS is a better account for progress than E.

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3. A. Bird (2008), "Scientific Progress as the Accumulation of Knowledge: A reply to Rowbottom", Studies in History and Philosophy of Science, 39, 279-281.
4. A. Bird (2010), "The epistemology of science - a Bird's Eye view", forthcoming in Syntheses.
5. Th. Kuipers (2000), "From Instrumentalism to Constructive Realism", Dordrecht, Kluwer.
6. I. Niiniluoto (1999), "Critical Scientific Realism", Oxford, Oxford University Press.

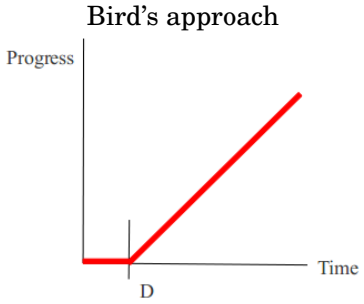
Review. Tambolo compares many different theories of scientific progress, among which he outlines three main definitions:

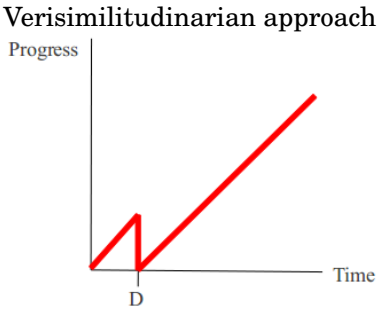
(a) Progress consists in the increasing of knowledge (Bird 2007a));

(b) Progress consists in increasing problem-solving (Kuhn 1962);

(c) Progress amounts to the accumulation of truth, i.e. increasing verisimilitude.

Although the third point could be considered from a semantic point of view, by concentrating on "truth", one may want to keep a structure for definition (c) which is parallel to (a) and (b); thus, it is possible to define progress in terms of increasing verisimilitude, something which is equal to define it in terms of accumulation of truth.
First of all, it is important to stress that (a) is different from (c), since verisimilarity is different from knowledge: more precisely, verisimilarity can be considered as an approximation of knowledge.
By Bird, (c) conflicts with our intuition but Tambolo argues that it does not. Bird asserts that verisimilar beliefs can be generated by a weak hypothesis, and then a scientist may put forward a rational hypothesis. According to Bird, scientific progress begin with the discover that former hypothesis were weak.
These different accounts of progress may be pictured as follow:





Tambolo's conclusions seem however not enough strong. He denies that Bird's examples count as instances of knowledge, because post-Gettier idea of knowledge is given by justified truthlike belief. This is undoubtedly correct, but it is scarcely convincing when applied in the present discussion, trying to falsify Bird's premises.
GS

3.5.2 A. Borghini (with M. Nathan), *Diacronic identity in biology and philosophy.*

Author's presentation. In the biological sciences, we find four independent criteria for identifying parts of an individual across organisms and species: (1) morphology; (2) function; (3) evolutionary history; and (4) development. In this essay we set to study the peculiarity of development as a criterion for identity. Along the way, we discuss also the relationship among the different criteria, and how each of them (or their interplay) influences the philosophical concept of identity across parts.
Before the establishment of the evolutionary framework, scientist appealed to morphological and functional criteria for the individuation of organisms and their parts. However, and perhaps unsurprisingly, identity attributions based solely on morphology and function can be misleading in more complicated scenarios, such as the evolution of limbs in arthropods (Carroll (2005), Gould (1989)). Once evolutionism was established, a phylogenetic notion of identity became a further criterion available to biologists for assessing the identity of parts of organisms across the species.
However, in some circumstances, morphology and functional attribution, even when supplemented with evolutionary history, are insufficient for establishing the identity of two parts. Consider the following example. For a long time, the origin of insect wings has been a matter of contention (Carroll (2005)). The problem is to identify which parts of ancestral organisms later developed into wings: what corresponds to (is identical with) the modern wing in an ancestral species. The solution might come from an analysis of development. (We say might because the evidence accumulated so far is significant, albeit not yet conclusive.) Developmental biologists have found that a few proteins that are required for building a wing - most notably Apterous and Nubbin - are also selectively expressed in the respiratory lobe of the outer branch of crustacean limbs. This suggests that wings were derived from a branch of an ancestral leg (more specifically, from the gills of an aquatic ancestor) as opposed to being independent outgrowths of the thoracic body wall in wingless insects. From a philosophical perspective, the interesting point here is that the identity of parts becomes the identity of specific molecular and developmental process.
Developmental conditions bring in a novel aspect to the business of part identification: identity of tye of process. What is remarkable is that it is the identity of certain kinds of diachronic events that is at stake. A developmental process has discrete stages, qualitatively distinct

and coming into a specific order.
It is our conviction that developmental processes force us to reconsider criteria of identity for individuals. In metaphysics, an individual is identified by means of one of three criteria: identity of properties, functions of causal histories. However, we never explicitly appeal to the type of process that brought an individual (trait or organism) into being in order to identify it. For instance, two individuals will be two pieces of gold in virtue of their constitutive molecules having atomic number 79; two objects can be said to be two tables in virtue of their similar or identical function; they will be two members of the species *Homo Sapiens* in virtue of their evolutionary histories (see Lewis (1986) and Kripke (1980)). But in focusing exclusively on morphology, function and evolutionary history, we overlooked the possibility that some individuals might also be identical in virtue of the fact that they were formed and developed through similar processes. If this is the case, we have to rethink the very notion of identity, when applied to individuals.

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Review. A. Borghini and M. Nathan have developed interesting arguments concerning the diachronic identification of individuals in Biology and Metaphysics. The structure of the speech shows how suggestions belonging to Biology could enlighten typical metaphysical discussions, such as the long-debating problem of diachronic identity.
Three criteria are commonly accepted in Metaphysics: form, function and causal history. Before evolutionary theory, biologists handled morphology and function as (as an) identity criteria. The parallel with Metaphysics is so far clear if one accepts the double analogy between morphology and metaphysical form and between the two concepts of function. Since this analogy seems unproblematic, one can move on.
By acquiring the point of view of evolutionary biology the scientist is able to identify individuals considering two more criteria: evolutionary history and development. It is quite simple to pursue the analogy by drawing metaphysical identity criterion of causal history near to biological identity criterion of evolutionary history.
Why are morphological and functional criteria unsatisfying for identity? Borghini gives three example: trilobites, scorpions and millipedes. They could not be distinguished, since they are all arthropods because of their similar organs. In spite of that, evolutionary history provides a criterion for identity through phylogeny (i.e. to come from a common ancestor).
Coming to the last point of the discussion, developmental criterion can not be reconstructed in a strict analogy with Metaphysics. From an evolutionary point of view, it's necessary; it is not sufficient but it's relevant. However, none of these criteria could be taken as sufficient. Developmental criteria might be particularly considered as further criteria for identity which can make us able to describe more precisely similar parts. In fact there are strong evidences that the identity of parts might be explained as identity of development. Thus the criteria are

even not valid to all sorts of entity. The aim of these research is to establish an up-to-date set of criteria for identity, which is biologically conceivable. Present criteria, such as in Putnam (1975) and Kripke (1980) are unsatisfying and biologically rough if they mean that a natural kind is identified by a network of properties.
GS

3.5.3 M.C. Amoretti (with N. Vassallo), *Scientific knowledge: situatedness and intersubjectivity without standpoints.*

Author's presentation. Feminist standpoint epistemologies of the sciences have some merits which is hard to disregard. More precisely, we believe that they correctly place their emphasis on the fact that scientific knowledge is situated: our understanding of the natural and social world partially depends on our specific perspective on the world. Moreover, they rightly hold that there is no one single epistemic subject able to produce scientific knowledge independently of other epistemic subjects. Finally, the standpoint of women can be a useful resource for the sciences for at least three reasons. Firstly, if we assume the standpoint of women, we may be able to identify new scientific problems as well as new research agendas. Secondly, the standpoint of women can contribute to ensuring strong objectivity of the sciences, since women have the kind of dual vision that yields a better epistemic position on the world. On the one hand, they are "outsiders" and can understand their own situation in a way inaccessible to the dominant group (male scientist). On the other, they are also "outsiders within": they live and work within the dominant framework without having any interest in perpetrating it; they are not engaged in self-deception and thus can critically analyze that framework and unmask scientists' sexist and androcentric biases. Lastly, the standpoint of women can play a pivotal role in the context of justification. Since women have no interest in defending a distorted (sexist and androcentric) description of the world, it would be easier for them to find new ways and experiments to test scientific theories.
Although these general qualities of feminist standpoint epistemologies of the sciences, one who wishes to uphold the very notion of standpoint is also faced with an unavoidable dilemma. If standpoint epistemologists want to defend the epistemic privilege or advantage of the women's standpoint, they need to ground it in some biological or social facts (e.g., cognitive style, common experiences, work conditions), ignoring the evidence that each woman has her own particular identity, assuming the existence of a feminine "nature", and thus embracing essentialism privilege and advantage, they have to consider all standpoints at the very same level. We believe that neither option is positive for science or the philosophy of science, and thus that no feminist standpoint epistemology of the science can actually be endorsed.
The aim of this paper is to demonstrate that there is no reason to appeal to any feminist standpoint epistemology of the sciences in order to argue for the situatedness of scientific knowledge, to recognize the presence of perspectival biases, to stress the importance of pluralism, and to defend the necessity for more democratic and less sexist practice in sciences. We shall demonstrate that there is no need to suppose that some perspectives are more reliable or advantaged than others (as feminist standpoint epistemology of the sciences actually do), because it is the very presence of various, and even conflicting perspectives on the world that democratizes the natural and social sciences and may eventually yield to their strong objectivity. Moreover, we shall point out that by abandoning the notion of standpoint, it would be much easier to reconsider and reevaluate all the differences among women, and hence revise, deconstruct - and perhaps even obliterate - the very concept of "woman".

Review. M. C. Amoretti explains some problems related to a relativisation of epistemology to standpoints. A standpoint could be roughly considered as the centered context of evaluation of knowledge. This means that a standpoint is a social group standpoint which obviously generalizes a private (and thus fallible) point of view from which evaluate knowledge. M. C. Amoretti argues that providing an epistemology in terms of standpoint is somewhat insufficient.
One could have appreciated the clear order of Amoretti's argument: first of all, we have to define what a standpoint is by furnishing its relevant properties. If members of a social group tends to assume the achievements of the dominant group in their society, then a standpoint for science is considered objective within the hierarchical society. This sociological account avoids any contradiction with the fact that a standpoint carries an epistemological asymmetry, something that is quite obvious given our intuitions about a standpoint's role in sociology of science.
One can now states some pros of standpoint epistemology. The most important pros are:

(a) that standpoint epistemology stresses the situatedness and perspectivity of knowledge, and

(b) that standpoint epistemology stresses the social dimension of knowledge.

Feminist epistemologists assert the epistemic privilege of women's standpoint as it is reminded in author's presentation above. However, Amoretti's criticism to this point of view seems strong and precise since feminist epistemology leads to essentialism in evaluating tools for knowledge. That should clearly not implies any ideological commitment to politics or to the social role of a particular group; therefore, one is inclined to reject the idea of epistemic privilege. Thus all standpoints are at the same level: this means that the very notion of standpoint is meaningless, and we can give up that notion. Rejecting the standpoint epistemology implies rejecting the related privileged epistemology as well as the possibility to define a social group through a specific kind of epistemic values.
One can now states some pros of non standpoint epistemology or epistemology without standpoints. Amoretti argues that they are the very same of standpoint epistemology, namely (a) and (b). In fact we don't need the notion of standpoint to stress situatedness, perspectivity and social dimension of scientific knowledge since a pragmatic point of view is sufficient. Practice is sufficient to get the intersubjectivity of scientific knowledge. In other words the most relevant pro in adopting an epistemology without standpoints is that we are adopting a deflationary model of scientific knowledge which doesn't prevent us to express its good points and conversely doesn't fall into the bad points that Amoretti criticizes.
GS

3.5.4 E. Di Bona, *What Mary learns: phenomenal concept and abilities.*

Author's presentation. Several strategies have been produced to argue against the knowledge argument proposed by Jackson (1982, 1986). The arguments purports to show that physicalism is false or at least is incompatible with the existence of consciousness. Roughly speaking, the dissidents against this argument can be divided into two groups: those who doubt the truth of both premises, and those who contest the validity of the argument. My aim is to address one of the strategies elaborated in order to undermine the validity of the argument, and I will discuss, in particular, the one suggested by Lewis (1988) and Nemirow

(1980, 1990), the so-called "ability hypothesis".
The ability hypothesis assumes that Jackson uses an equivocal meaning of "know". In the first premise of the argument, "know'" is used to express propositional knowledge, while in the second premise it is used to express knowledge-how or ability knowledge. According to Lewis, what Mary learns, when she escapes from the black and white room and she first sees red, is how to recognize, to imagine and to remember experiences of red things. Hence Mary grasps mere abilities and not a new proposition, since she has already holds it before leaving the room; the experience give her information that is not propositional. As Crane (2003) suggests, Lewis' response presupposes two things:

i. that knowledge-how is ability knowledge, and it is not completely different from, and irreducible to, propositional knowledge; and

ii. that in spite of the fact that Mary acquires abilities, she comes to know neither a new proposition nor an information different from the possession of abilities.

After a brief introduction of the knowledge argument and the objection elaborated within the ability hypothesis, the present work is focused on a detailed discussion of the two previous claims: (i), (ii). In the first part I will set out that, according to Stanley and Williamson (2001) and Williams (2007), the distinction know-how/know-that, as discussed primarily by Ryle (1949, 1971), is not at all exhaustive. If we admit either that know-how sometimes does and sometimes does not consist of propositional knowledge or that knowledge-how is simply a species of knowledge-that, we reach the conclusion that the distinction does not seem to be effective anyway; and even though we take the following step to identify having knowledge-how with having an ability, it remains unclear why the abilities Mary gains cannot be expressed by propositional knowledge; therefore the first claim (i) is dubious, contrary to what Cath (2009) seems to demonstrate.
Then, in the second part of my work, I will analyze the second claim (ii). According to Lewis, the only information Mary learns are the abilities to recognize, remember and imagine experiences of red. Tye (1995, 2000) proposes an interesting answer to Lewis' account, based on the difference between knowing what it is like to experience red and the possession of the aforementioned abilities. Tye interweaves these two kinds of knowledge; he revises the ability hypothesis with the following solution: Mary learns what red looks like and, in addition to this ability, she learns what it is like to experience red, which is however a propositional knowledge. I will embrace Tye's perspective, but I will replace what he calls "knowing what it is like" with "phenomenal concept". My explanation of what a phenomenal concept is, the concept that Mary lacks in the room, is roughly based on Loar (1990, 1997) point of view. In conclusion, the ability hypothesis is not an exhaustive explanation of the before-after difference if it is not enriched with the notion of the phenomenal concept.

Review. The talk refers most notably to two Frank Jackson's papers: Epiphenomenal Qualia (Jackson (1982)) and What Mary did not know (Jackson (1986)). Jackson developed there his "Knowledge Argument", which has been stated to reject physicalism. In the former paper, it is stated as follow:

> Mary is a brilliant scientist who is, for whatever reason, forced to investigate the world from a black and white room via a black and white television monitor. She specialises in the neurophysiology of vision and acquires, let us suppose, all the physical information there is to obtain about what goes on when we see ripe tomatoes, or the sky, and use terms like "red", "blue", and so on. She discovers, for

> example, just which wave-length combinations from the sky stimulate the retina, and exactly how this produces via the central nervous system the contraction of the vocal chords and expulsion of air from the lungs that results in the uttering of the sentence "The sky is blue". (It can hardly be denied that it is in principle possible to obtain all this physical information from black and white television, otherwise the Open University would of necessity need to use colour television.) What will happen when Mary is released from her black and white room or is given a colour television monitor? Will she learn anything or not? It seems just obvious that she will learn something about the world and our visual experience of it. But then it is inescapable that her previous knowledge was incomplete. But she had all the physical information. Ergo there is more to have than that, and Physicalism is false. (Jackson (1986))

Henceforth the speech did not overdo a review of the debate and a deepening of the concepts involved within it, then expliciting the assumptions and their consequences. We can sum up concluding that Mary knows all the physical information concerning human color vision except for some information. Thus not all information are physical. In his 1988 paper, Lewis answered that Mary acquires a new ability after she came out from the black and white room. As Di Bona points out, this thesis dates back to Ryle: both Lewis and Ryle would assert that knowing-how is different from knowing-that. Referring to Stanley and Williamson's denying that knowing-how implies knowing the ability to do, Di Bona denies the converse too and thus, she concludes that Lewis first claim is at least dubious. The next step is to reject the second claim, something Di Bona obtains through arguing, roughly speaking, that Lewis account for to know-how is unsatisfactory. The talk ends once reached these conclusions.
GS

3.6 Epistemology of Social Sciences

3.6.1 D. Rizza, *Applied mathematics in social choice theory.*

Author's presentation: Recent attempts to provide a general account of the application of mathematics (in particular Pincock [2004], Bueno & Collivan [forthcoming]) have focused on the following presupposition: (i) mathematical structures are what is being applied; (ii) the application process relies on the existence of structure-preserving mappings bridging empirical settings and mathematical structures. In this talk I intend to show that the any framework relying upon (i) and (ii) is inadequate to study applicability in general, because it overlooks the existence of fundamentally different types of applications, some of which do not satisfy assumptions (i) and (ii) above. I intend to support this claim by considering a case study in which (i) and (ii) fail and where mathematics plays an explanatory role that cannot be characterized in terms of mappings and mathematical structures. The general conclusion to be drawn is that an adequate study of the applicability of mathematics should be particularly concerned with providing a classification of different types of applications.
These conclusions may be substantiated by looking in some detail at a case of study from social choice theory, namely the application of ultrafilters (and allied concepts) to the problem of preference aggregation, in particular to Arrow's theorem for a finite society (as studied in Kirman and Sondermann [1972], Hansson[1976]). In this context mathematics is used to deal with an empirical problem concerning the possibility of combining in normatively acceptable way the preferences of a group of individuals (e.g. voters) in order to determine a collective or

social preference. This application of mathematics does not rely on either (i) and (ii) above. It does not rely on (i) because what is applied is not a structure but a mathematical concept (ultrafilter) that proves fundamental to describe the family of decisive coalitions of a voting system.

Moreover, the application of ultrafilter does not rely on (ii) because it acts directly on the empirical problem of preference aggregation, by capturing the interrelations among the dominating coalitions of an aggregation procedure. In the light of these facts, it is particularly relevant that ultrafilters are explanatory effective: this is because various negative results in choice theory can be unified by showing that they all arise from the presence of ultrafilters or filters of dominating coalitions (e.g. Gibbard [1969], Weymark [1984], Campbell [1990], Koshevoy [1997]) and because the notion of ultrafilter allows the isolation of the informational content of Arrow's theorem (as show in Sen [1983]).

It is noteworthy that the explanatory effectiveness of ultrafilters arises in a mapping-free context: this means that some mathematical explanations of empirical facts are generated by the introduction of mathematical concepts and so they are independent of the positing of mathematical entities, which is implicitly required by the semantic introduction of mathematical structures through mappings.

It follows that the notion of mathematical explanation varies according to the type of application being considered and that, in general, different uses of mathematics correspond to different types of application. For this reason it appears important and promising to study the applicability of mathematics not trough a general unifying model but through a classification of types of application.

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Review. This talk takes into account applications, and also applicability, of a mathematical theory on empirical setups. Against a normative purpose (see Pincock [2004], Bueno & Colyvan [forthcoming]) by which the use of mathematical mappings preserves the empirical structure of the examined problem, Rizza shows how it's necessary to study the applicability of mathematics through a classification of the kind of applications one intends to use, in order to provide an explanation of the tractability of the problem, which is not always provided by the algebraic theory. The case of Arrow's theorem is particularly recommended to show this inefficiency.
Given a set that represents a finite set society, with cardinality greater than 2, and given a decisional problem to the individuals of this set, such that at least 3 different alternatives are possible (the most common case is the one of an election with 3 candidates) it is not possible to identify a function representing individuals' choice useful to satisfy the following requirements:

- universality
- non-imposition
- independence of irrelevant alternatives
- monotonicity
- non-dictatorship

The set-theoretic interpretation of this kind of situation leads to identification of decisive coalitions sets (sets such that the members' choice can decisively affect the result of the election) with ultrafilters. The intersection of all decisive coalition sets gives a set formed by a singlet (a voter). This set is an ultrafilter too, so a decisive coalition set formed by just one element (a dictator).
GL

3.6.2 C. Grasseni, *Responsible innovation: co-research as a socio-scientific methodology of ethnographic enquiry.*

Author's presentation. Risk "assessment" and "management" have become staple food for many disciplinary discourses and policy statements about innovation. This reflects on possible strategies for a socio-scientific study of food production and provisioning in many and complex ways. A socio-scientific methodology in fact requires a language of analysis that is in many ways distinct and different both from disciplinary debates on innovation and from policy discourse of risk. I shall use the case of critical consumption and of the current resurgence in the social appropriation of food production and provisioning, to argue that such methodology should be ethnographic.
The problem of how to pursue and foster responsible innovation – for instance, in the field of GM crops or large mammal cloning – should find a solution in criteria for responsibility in relevant public debates. This turn means having a clear value framework which inspires political action (politics, at its best, being a participatory deliberation of the goals of the polis, namely of the community of citizens).

Social Studies of Science and Technology provide a critical attitude to thinking about the assumptions and the languages in which such debates – or the absence of debates – establish local practices of reciprocal relationship between science and democracy. Techno-scientific innovation in fields that are the subject matter of bioethical scrutiny and inquiry – such as how to assess and manage risk in frontier fields: animal cloning for instance – reflect facts about science in society which have been highlighted in vast bodies of literature: for instance, that the lab is porous and that the scientific objects and facts are not just of interest and concerns to experts but to society at large. In particular, media and public debate on scientific facts are not just matter of "Public Understanding of Science" but a matter of their social appropriation and co-construction (Latour, Jasanoff, Wynne). It is particular important to social scientist to understand how technoscientific questions are configured not only within disciplinary and public discourse, but also how they are appropriated in practice, since science and technology are political objects and they permeate everyday life.
A rich constellation of grassroots movements are posing the question of food as a political object of risk perception and of collective deliberation, and organising alternative food provisioning networks that value health standards, but also social and environmental sustainability in food production. From Slow Food to Community Supported Agriculture, from groups of Solidarity-based Purchase (GAS) to transition towns, a collective and participatory response to the public perception of technoscientific innovation and of risk is emerging in a concerted way. Which constructive scenarios do these movements provide for rethinking the role of the social sciences in evaluating the roles of technology in society? How do they convey expectations of responsibility in scientific research? Is responsible behavior in food production and provisioning by definition averse to technoscientific innovation?

Review:Grasseni treats a particular productive and economical behavior, with the aim to show how it could be considered as an important issue for account for social and economical progress. The study case is the AFN (alternative food network) one: it consists in common citizens' cooperation and co-production and it also involves the commerce of these goods, which is totally managed by the people, not gathered in food-societies. This framework shows citizens' remarkable appropriation of many technological, scientific, social-economic practices, specific knowledges, and specific language. The notable aspect is that it all happens in a field often considered reserved, peculiar to who has specific skills. As well shown by Grasseni, this is not only a case of skill enhancement, but it also concerns the relationship between science (or technology) and democracy, defined as deliberation of the goals of the citizens, reflecting a base economical behavior of a community.
GL

3.6.3 G. Lo Dico, *The puzzle of verbal reports in cognitive psychology.*

Author's presentation: Historically or mythologically (it depends upon the author), 1956 is a year to remember for experimental psychology. In fact, it is considered the year when most psychologists dismissed behaviourism and accepted cognitivism as a new outlook. Cognitivism can be said to be revolutionary, in a certain sense, because it radically changed the object of study of scientific psychology: psychologists were allowed to talk about mind, not only of behaviour. However, 'new' cognitivism maintained two fundamental tenets of 'old' behaviourism: the assumption that mind cannot be publicly observed and that the only empirical evidence at disposal can be behavioural. In other words, like behaviourists, cognitivists

assumed an anti-introspectionistic and anti-subjectivistic point of view. This perspective is generally defended by appealing to two related arguments: on the one hand, by maintaining a clear distinction between unconscious processes and conscious contents and, on the other, by defining verbal reporting unreliable and fallacious in principle. A paradigmatic example of such a defense can be found in the 1977 article of the psychologists Nisbett and Wilson. According to them, subjective verbal reports have to be considered unreliable not only when they are incorrect, but also when they are correct. This is because, when subjects verbally report their mental operations they do not do it on the basis of any true introspection, but on their judgments about how such processes work. Thus, because they are not based on true introspections, also verbal reports that look accurate cannot be reliable. Although Nisbett's and Wilson's article was criticized and rebutted by many authors, their position is still influential in cognitive psychology. In fact, although psychologists make a large use of verbal reports (for example in experiments in decision-making or reasoning), they tend to discard them as unreliable and thus prefer other kinds of empirical evidence.
Among the critics of Nisbett and Wilson, we can find the psychologist Ericcson and the economist Simon with their seminal 1985 book. According to them, verbal reports can be important data for experimental psychology if adequately treated. They propose an articulated methodology (named 'protocol analysis') for individuating and avoiding the common problems coming from these data and for dealing with them in an objective way. It is important to stress that this proposal is based upon an information-processing model composed of different processes typical of cognitive psychology's tradition. In Ericcson's and Simon's view, this model is a necessary framework both for interpreting verbal data obtained in an experimental setting and for connecting them to the behaviour manifested in such a setting because it allows to minimize the subjective character of verbal data and to avoid any reference to late 19th and early 20th century introspective method. However , it is matter of debate whether their proposal can actually do without introspection or subjectivity.
In this paper, I'll provide a critical evaluation of protocol analysis. I'll argue that, pace Ericcson's and Simon's warnings against introspection, their methodology must imply a certain degree of access to cognitive psychology: in fact, if it accepted protocol analysis as a method of inquiry, it should dismiss its anti-introspectionistic and anti-subjectivistic outlook and thus require a substantial revision.

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Review. Focusing on the foundation of cognitive psychology Lo Dico outlines the development in psychology which led to refuse behaviorism towards cognitivism. This talk mainly concerns two important contributions of cognitive psychology: (Nisbett, Wilson 1977) and (Ericsson, Simon 1980) concerning verbal reports in the new light of cognitivism.
First of all, Lo Dico shows how the passage from behaviorism to cognitivism has been less revolutionary what we could have commonly supposed remarking that the new prospective did not appear anti-behaviorist at all. There are two basic assumption of the former approach, that mental events are not directly observable and that the behavior is the only objective evidence available, which gave rise the case study of verbal reports. In this view-point Nisbett

and Wilson claim that subjective verbal reports must be considered always unrielable, both correct and incorrect, due to the lack of any true introspection. Nisbett and Wilson support this thesis claiming that cognitive operations are inaccessible, having previously tracked a distinction among conscious contents and unconscious processes.
This thesis has been criticized by Lo Dico. He shows its informal level arguing how Nisbett and Wilson do not discuss any study in details and seem to do not consider the different methodologies for obtaining and treating verbals data. Evincing moreover the unfalsifiable fashion of the argument the speaker finally remarks that the distinction among conscious contents and unconscious processes is unclear because supported by a confused distinction between cognitive processes and experience contents.
At this stage the talk focuses on Ericsson and Simon's proposal. Based on a defined information-processing model, it identifies a recognition memory, a long-term, a short-term memory and others components which work at both the unconscious and conscious level. In opposite to the first one, this proposal conceives certain form of verbalization called concurrent and retrospective verbalization reliable and information-rich such as observable behavior. Verbal data are so the output of the mind's information-processing and an indirect evidence of how that model works.
Although these two positions seem to diverge in two different ways, Lo Dico argues in conclusion how they reach two closely similar results either maintaining an anti-introspectionist outlook, while Nisbett and Wilson's form is stronger than Ericsson and Simon's one, which try to avoid the subjective character of verbal data. He also concludes that the last argument, admitting that the subjects have a minimal degree of access to their mental processes, cannot refuse the introspectionism and forces cognitive psychology to renounce to the two behaviorism's basic assumptions. In my opinion this talk succeed in showing his aims understandably, presenting a pivotal psychological topic with clearness and simpleness while Lo Dico was able to illustrates both the position in a well-argued way.
MS

3.6.4 E. Atukeren, *Causality-invariance: new insights into economic and social processes.*

Author's presentation. A very specific notion of causality is due to Granger (1969, 1988, Journal of Econometrics). Granger takes a pragmatic approach and defines causality in terms of predictability. Although this concept of causality leads to more questions than it answers, it has been a major tool of empirical analysis especially in economics in the last several decades. While Patrick Suppes' notion of probabilistic causality provide the basis for Granger's definition, the former is better known and regarded among the philosophers of science. Nevertheless, Granger causality has also received recent interest as well. For instance, James Woodward (2008: 234) states that: «Roughly speaking, X Granger-causes Y if X is temporally prior of Y and information about X improves our ability (relative to some baseline) to predict whether Y will occur. Interestingly, Granger-causation turns out to be a different notion of cause (and hence to be associated with a different notion of causal correctness) than the interventionist notion. X can be a Granger-cause of Y even though it is not a cause in the interventionist sense. It is thus a live question whether we should adopt this notion of cause instead of the interventionist notion.»
In this paper, we first examine the uses and abuses of the concept and the applications of Granger-causality in economics and social sciences. Then, we focus on a new development in

the definition of Granger-causality, namely, the notion of "causality-in-variance". The concept of causality-in-variance was developed by Cheung an Ng (1996, Journal of Econometrics). The idea is that the traditional approaches to probabilistic causality are geared at detecting causality in mean and hence ignore the possibility of causal relationship in the second moment (variance). It may be that a stochastic variable X does not cause another stochastic variable Y in mean but only in variance. Cheung and Ng suggest a predictability-based approach in defining causality in variance and a procedure to detect it.
The concept of causality-in-variance has wide ranging implications in understanding the causal relationships in economics and in social sciences in general. It also has the potential to address a well-known problem in probabilistic causality. The story is as follows.
Let us assume that Mary is a good stone thrower and she can hit the target (a window) with 80 per cent probability. John, on the other hand, is a poor stone thrower and he can hit a target with only 40 per cent probability. One day, as Mary aims at a window she sees that John is also aiming at the same window and she does not throw the stone. John does so and hits and breaks the window. In this case, the problem from the probabilistic causality approach is that for an event A to be cause of another event B, A should increase the probability of B. In the stone-throwing example, the opposite happens. Mary's inaction to throw the stone indeed decreases the probability of breaking the window and yet the window still gets broken. In this paper, we demonstrate that the causality-in-variance approach can offer new insights into this problem. Further illustrations of the applicability of causality-in-variance in social processes are also offered.
Reference:

1. Woodward, J. (2008) Invariance, Modularity, and All That: Cartwright on Causation, L. Bovens, C. Hoefer, and S. Hartmann (Eds.) Nancy Cartwright's Philosophy of Science, Routledge Studies in the Philosophy of Science. Routledge. UK.

Review. Atukeren shows a new interpretation of Granger's causality (Granger 1969, 1988) called "causality-in-variance" focuses on the possibility of a not interventionist causal relationship in an event characterized by the variance of many factors.
In this view-point the concept of "High moment" plays a central role: starting from Woodward's analysis (Woodward 2008), which asserts the importance of x's temporal priority and information for Granger-causing y, Atukeren defines causality-in-variance as an event where the x variable influences the y variable in a high moment. Although between the two variables could not be a interventionist causal relationship, in a high moment they might be related and the "y-effect" could be strictly influenced by the "x-cause". For instance, in the case of a golfer which plays in a windy day: if he pulls the ball a gust of wind (x) could move the top of a tree and allow the ball (y) to approach the green; although x does not cause y it may cause-in-variance y. Atukeren illustrates how this notion of causality is pivotal in econometrics and finance, where with a statistic definition of causality-in-variance it would possible to have a clear perspective about the grown of possible high moments in central events.
This talk outlined a useful notion of causality for econometrics and finance and Atukeren provided many examples where such a notion can play a fundamental role in choosing among different alternatives.
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