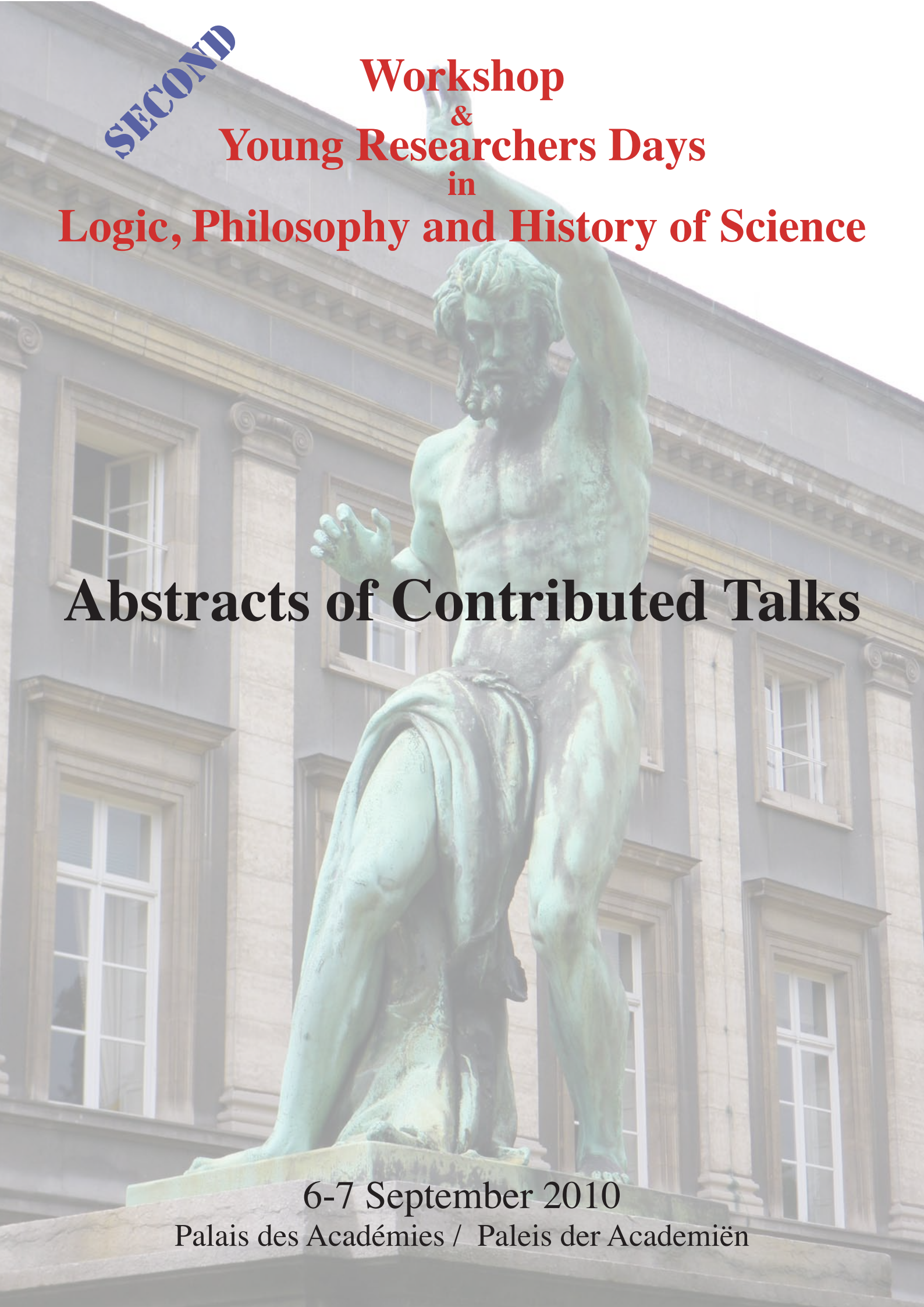

SECOND

Workshop & Young Researchers Days in Logic, Philosophy and History of Science

Abstracts of Contributed Talks

6-7 September 2010

Palais des Académies / Paleis der Academiën

Contributions

Part I

History

Olivier Carrillo

Le toucher au sein de la hiérarchie des sens: étude historique sur l'« autre » de la vision de Platon à Vésale

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Non seulement les rares histoires des cinq sens existantes débutent seulement avec la Renaissance, ce qui ampute gravement la compréhension qu'elles pourraient nous apporter, mais en outre il n'existe aucune synthèse historique qui soit spécifiquement consacrée à ce sens particulier qu'est le toucher. Une telle carence est pourtant dommageable pour les historiens des sciences.

En effet, si le rôle de la vision dans l'acquisition du savoir est bien connu des historiens des sciences, il n'en est pas de même pour le toucher. Or celui-ci n'est pas seulement une source de connaissances parmi d'autres, il est aussi l'autre de la vision par rapport à laquelle il a, de tout temps, été valorisé ou dévalorisé. L'histoire de l'acquisition du savoir requiert donc la prise en compte de l'histoire du toucher, tout comme l'histoire de la vision ne peut faire l'impasse de son autre, d'autant plus que le mécanisme de la vue a, parfois, été conçu comme une sorte de toucher.

Un savoir conçu sur le modèle de la vue amène nécessairement au discrédit d'un toucher trop incertain, trop animal. Aussi bon nombre de savants ont dénigré ce sens trop éloigné, à leurs yeux, de l'âme ou de la pensée. Platon et Descartes lui ont accordé le rang le plus bas, quant à Léonard de Vinci, il fit de l'œil «le prince des autres sens». Tout en partageant ce point de vue dans la «Protreptique», Aristote, dans le «De Anima», présente le toucher comme «la sensation la plus précise chez l'homme», ce qui fait de lui le «plus intelligent des animaux». Avec Vésale, la main savante se substituera toutefois à la lecture et à la monstration médiévales.

En dressant l'historique du statut du toucher au sein de la hiérarchie des sens, nous allons tenter de combler ce manque de la littérature.

Guy Claessens

Proclus, Kepler and Descartes : the imagination as self-knowledge

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Throughout its long-term history, the case of imagination has shed remarkable light on the connection between body and knowledge. As a hinge-joint, imagination communicates between the senses and the intellect. Although this mediating movement has been nearly always considered to have its starting-point in the sense world, the geometrical, productive imagination presented by the neoplatonist Proclus starts from innate concepts of the mind which are projected on imagination as on a mirror. Imagination thus becomes an intermediary receptacle that enables the soul to see its inner self. Nevertheless, from a (neo)platonist perspective this self-knowledge can at best be secondary and auxiliary, due to imagination's close connection with the senses. In this paper, I want to discuss the way in which Proclus' geometrical imagination is originally transformed into an epistemology which gives a central place to the body and sense knowledge. This crucial shift occurs when Proclus' text is received by the German astronomer Johannes Kepler. In the *Harmonices Mundi* imagination turns into an intermediary faculty that provides knowledge of our own essence *by means of* the body. This viewpoint results from a rehabilitation of the senses as trustworthy *organs*, to put it in Kepler's word: 'the eye has been made as it is because the body is as it is, and not the other way around.' In the same period, the notion of productive imagination appears in the writings of René Descartes. The difference with the Proclean stance is again similar: Descartes' safeguarding of the quantitative information submitted by the senses enables imagination to play a constructive role in acquiring true knowledge of the world. Where Proclus' imagination as a mirror allows the soul to see itself *despite* its bodily locus, both the Keplerian and Cartesian imagination permits the soul to recognize its own essential creativity *in the sensible world*.

Julie De Ganck

Jean-Hilaire Keiffer, "la femme" et la société. Biographie sociale et intellectuelle d'un scientifique à la croisée des chemins

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Notre exposé se propose de retracer le parcours intellectuel et social de Jean-Hilaire Keiffer (1864-1941). Formé à la zoologie et à la gynécologie à Liège, il fit carrière à Bruxelles comme médecin praticien et professeur d'université.

Ce personnage est surtout connu dans le domaine de l'histoire sociale en raison de ses actions hygiénistes. Il s'engagea à l'extrême fin du dix-neuvième siècle aux côtés d'un célèbre avocat féministe et pacifiste bruxellois, Louis Frank, dans la création d'une assurance maternelle obligatoire pour les femmes salariées. On le retrouva en tant que président de la Société d'Eugénique durant l'entre-deux-guerres, où il travailla à l'instauration de consultations prénatales gratuites. En qualité de professeur d'université, il dispensa également des cours aux infirmières, notamment sur la prévention et le soin des maladies vénériennes. Cet intérêt "social" pour le corps des femmes nous amène à questionner la façon dont Jean-Hilaire Keiffer envisageait la féminité dans ses écrits scientifiques et médicaux. Il travaillait en effet sur la physiologie des organes génitaux féminins, mais s'intéressait également à la constitution "psychophysique" de "la femme".

Notre communication se propose donc d'enquêter sur le parcours intellectuel de cette personnalité à travers ses écrits scientifiques dans les domaines de la biologie, de la gynécologie et de l'anthropologie, en les analysant à la lumière de ses engagements politiques et sociaux. Faire cette biographie de Jean-Hilaire Keiffer, c'est relier l'histoire de la pensée scientifique à son histoire sociale pour éclairer leurs angles morts respectifs, tout en adoptant une perspective de genre.

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Paloma de La Vallée Poussin
Ship in movement : History of research in stowing, rolling and pitching
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In the 17th century, Richelieu took the french navy in hands, for the benefit of the french nation, to create a national navy. An improvement of efficiency of ships and navigation was actively sought after, leading to theoretical research in naval matters, promoted by the State. As an essential quality of the ship, the stability was subject to research by various authors. A first turning point was the publication of the « Traité du Navire » by Pierre Bouguer, which establishes a theoretical expression of static stability. However, it soon appeared that the static stability computed on basis of the ship's plans was insufficient to estimate its sailing qualities. Indeed, the static stability is directly influenced by stowing, due to the displacement of the center of gravity. Moreover, the ship movements in rolling and pitching get stronger with the increase of stability ; and that is, in addition to the uneasiness brought upon the crew, greatly hazardous to the rigging, and hence the safety at sea. As a consequence, once the static stability understood, the Royal Academy of Sciences of Paris proposed a number of prizes for the establishment of a theory of stowing and a better understanding of rolling and pitching.

This study shows the important stages in the comprehension of stowing, rolling and pitching. A first attempt at a theory can be attributed to Hoste, in his « Theorie de la Construction des vaisseaux » of 1689. In the 18th century, Daniel Bernoulli and Leonard Euler did a lot for the establishment of the rolling and pitching theory in calm water. Around the same period, Jean-Albert Euler, and the Abbé Bossut both proposed a theory of stowing. Finally, in the late 18th, the theory of rolling and pitching was extended to encompass the movements of ships in waves.

Denis Diagre & Ivan Hoste

A Case Study of the Changing Scientific Community after The Origin*: François Crépin's Conversion to Darwinism*

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A long footnote in the first edition of Crépin's *Manuel de la Flore de Belgique* (1860) shows that the young botanist was at first very reluctant to follow the path paved by Charles Darwin in *The Origin of Species*, published in November 1859, only a few months before the Manuel. A decade later, the self-educated botanist, who had been struggling with the species concept for years, had changed his mind. Hundreds of letters from dozens of European botanists and a wealth of articles in several botanical and horticultural magazines, many of them on the taxonomy of the complex genus *Rosa*, gave the authors a good opportunity to depict the fascinating intellectual journey of this emerging botanical notability.

How did François Crépin, raised in a conservative milieu, actually subscribe to the new paradigm? How did he cope with social pressure, intellectual conservatism, lack of formal education and a need for a better professional situation? Who and what did he struggle with before becoming a *Darwinist*? Did it actually affect his job as a taxonomist?

Moreover, this reconstruction of the life of an individual scientist highlights some crucial issues about how the 19th century scientific community worked and evolved: the role of scientific—sometimes unsuspected—networks; the spread of new ideas and concepts in the naturalists'milieu; the growing dichotomy between amateur and professional science; the noteworthy role played by European refugees in the history of the sciences in Belgium... These are some of the reasons why we think the career and ideas of François Crépin deserve further study.

Lyvia Diser

Between Science and State. The laboratory as policy instrument for the Belgian Goverment (1870-1960)

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In de hedendaagse maatschappij verschijnt het laboratorium als een polymorfe en alomtegenwoordige instelling, aanwezig op een veelheid van maatschappelijke terreinen. Mijn onderzoek behandelt het moderne laboratorium in zijn hoedanigheid van beleidsvormende instrument voor de moderne welvaartstaat en natiestaat België. Het overheidslaboratorium balanceerde op het snijvlak van twee sociale werelden: enerzijds de wereld van de overheid en anderzijds het universum van wetenschappelijke laboratoriumpraktijk. Mijn onderzoek rond Belgische staatslaboratoria werpt een blik op de culturele uitwisselingen die in deze border zone, dit grensgebied van gemengde culturele praktijken, tot stand kwamen.

Zowel de overheid als de laboratoria hadden belangen bij een alliantie. De moderne staat België beschouwde laboratoriumpraktijk als een waardevolle bijdrage voor het soort beleid dat hij voor ogen had, en als een aanwinst voor het zelfprojectie als de moderne staat op burgers en buurlanden. Anderzijds waren laboratoriumambtenaren van mening dat hun staatsidentiteit waarde toevoegde aan de maatschappelijke taak die ze voor zichzelf weggelegd zagen. Laboratoria presenteerden zich als het beleidsinstrument bij uitstek van de moderne staat en ze wierpen zich op als experts in maatschappelijke kwesties. De alliantie tussen overheid en laboratoriumpraktijk was er dan ook een van wederzijds vertrouwen. Het vertrouwen dat de overheid aan haar laboratoria schonk, was cruciaal voor de monopoliepositie als gezagdrager en expert die de overheidslaboratoria zich konden aanmeten tegenover de maatschappelijke groepen tot wie de dienstverlening van de overheidslaboratoria zich richtte, of op wie de processen van staatsvorming betrekking hadden. Soms rees er vanuit deze groepen verzet tegen de epistemologische uitbreidingsdrang van de overheidslaboratoria. Maar vaak hadden ze ook belangen bij een acculturatie met laboratoriumpraktijk.

De culturele uitwisselingen tussen deze drie stakeholders en de conceptie en perceptie van de ander en zichzelf zijn het onderwerp van de paper die ik op de Young Researchers Days wil presenteren. In deze paper zal ik een globaal beeld schetsen van mijn onderzoek en een aantal resultaten en vaststellingen bespreken.

Astrid Elbers

Funding radio astronomy in postwar Netherlands: Dutch radio astronomers and the Organisation for Pure Scientific Research

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In the first postwar decade, the new field of radio astronomy grew steadily in several countries, such as Great Britain, the US, Australia and the Netherlands. The instrumentation of which it made use (radio telescopes), required a lot of funding. Although the Netherlands were devastated by World War II, radio astronomers succeeded in getting a huge amount of money from the Organisation for Pure Scientific Research (ZWO, now NWO) from 1948 onwards.

In other countries, radio astronomy grew out of wartime radar technology and the ties with the military - especially in the US - were very strong. The first radio astronomers were almost exclusively engineers or physicists trained in radar technology. Consequently, patronage and funding by the military were self-evident. In the Netherlands on the other hand, radio astronomers were formally trained as astronomers and the research explicitly served astronomical goals. A link with the military was absent. The question is why - in an era of recovery - radio astronomy became such a priority.

I will argue that besides the traditional arguments of national prestige and possible spin-offs (telecommunication...), three other factors were crucial. First, there was the postwar positive attitude towards 'pure' science. Second, the attitude of the early ZWO in deciding which initiatives should be supported was rather passive. They left the initiative for a great deal to the scientists themselves. Third, the number of people involved - radio astronomers and staff of ZWO - was very small, which left a lot of room for personal contacts and friendships.

Sofie Onghena

An appropriate didactical instrument? Science on Belgian educational radio and television (ca. 1946 - 1968)

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In Belgium educational radio was operational since 1931; educational television was established in 1962, following the example of France and Great Britain. Both educational radio and television made programmes on science, particularly on physics, biology and technology. My research question on educational science broadcasting in Belgium is twofold: not only the representation of science in educational broadcasting meant for secondary schools will be studied, but also the reception and evaluation of this didactical instrument by science teachers will be analysed. This lecture will be based on BRT-year reports, brochures, correspondence of the Ministry of Education and pedagogical periodicals for the period 1946-1968: in these years educational broadcasting took a growing part in secondary school (science) curricula.

Science programmes on educational television and radio were only complementary to science education and did not aim at replacing the teacher. Educational television for instance wanted to illustrate and enliven science instruction, showing experiments that were not possible in the classroom because of lack of time, insufficient laboratory equipment or due to technical or safety reasons. The main purpose of educational broadcasting was to link education with practical applications in academic laboratories, in daily and industrial life, where theory was put into practice. Reports and radio plays from abroad were adapted, for example the BBC-series 'Science today' by Charles Parr or a German radio play about the Belgian scientist Leo Baekeland.

The Belgian Ministry of Education was strongly involved with educational radio and television and gave instructions to science teachers; principals were advised to reckon with the broadcasting scheme in school timetables. However, the pedagogical aim of educational broadcasting in science lessons was under discussion: some school inspectors were opposed to the inappropriate fictionalisation of science in radio plays.

Pieter Raymaekers

Between capital and labour: The social role of engineers in Belgium during the second half of the nineteenth century

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This paper examines the (mis)match between social commitment and technical ingenuity of the Belgian engineer in the nineteenth century. The social issues, with particular attention to the impact of the Belgian workers' revolt of 1886 on the discourse of the engineers is regarded as a starting and turning point. We analyse the actions and reactions of both engineering societies and individual engineers in the field (case-study on the mines of Mariemont and Bascoup). Did the positivistic, technological discourse of progress and purely scientific and mathematical research, which corresponded with the self-image of the engineer, change as a result of this dramatic, social upheaval and did it evolve towards a more general social economic context? In other words, was the engineer willing to align his strong belief in progress with common social progress? The engineer, now and in the past, is often considered as a technically trained problem solver, connected to concepts such as positivism and belief in progress or even scientism, expertocracy and technocracy. For most of the nineteenth century, his primary preoccupations were indeed purely technological and scientific. Nonetheless, his social impact could hardly be underestimated. As a technical actor, he made an enormous contribution to the large-scale process of transformation, which characterized the age of industrial revolution. The social role of the engineer becomes even more relevant when we consider his special, professional position and status. He embodied the direct connection between capital and labour, two powerful opposite groups which were likely to collide at that time. Other aspects such as the specific role played by engineers in the development of social services and social legislation or the influence of engineering education will be included in the analysis. For example, the dominance of theoretical and mathematical subjects in the curriculum as opposed to the limited presence of courses such as political economy, philosophy, etc. reveals an interesting and relevant approach.

Truus van Bosstraeten

We are family! Teamwork in Belgian university Laboratories, 1870-1914

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Big research groups working on highly specialized subjects are nowadays identified with the large-scale science that came into being since the 1970's. Teamwork is one of the symbols par excellence of this type of science, often referred to as Big Science. Collaboration, informal or institutionalized, has however always been part of science [1]. I want to investigate how since the advent of the laboratory at Belgian universities in 1870, different ways of working together and collaboration in- and outside the laboratory took shape. Who worked in the laboratory? What where current practices? Which images and metaphors were used to refer to the laboratory or the group?

Since 1870, the university had become an institution where education and scientific research were combined. Towards the end of the nineteenth century the universities of Brussels, Ghent, Leuven and Liège built large scientific institutes. Despite the fact that different scientific disciplines were developing, the Belgian university remained a familiar institution with a relatively small academic staff up until the First World War.

Before considerable numbers of paid personnel entered the lab (this happened after WWI), labs were mostly populated by a single professor, students and a few aides. Not only did this groups work together, they also had connections outside the laboratory: at the university, national and international level. I will investigate broad developments but also highlight several specific cases. As for the period 1870-1885, I will mostly focus on Belgian laboratory 'pioneers' and for the timeframe 1885-1914 on the new institutes of physiology founded in Brussels, Ghent, Liège and Leuven.

The focus on 'teamwork'—the term is used in a broad sense—will enable the study of laboratory culture both inside and outside the walls of the lab and therefore it will contribute to the study of the laboratory as an embedded institution.

Reference:

[1] See for instance MELI, D.B. 'Authorship and Teamwork around the Cimento Academy: Mathematics, Anatomy, Experimental Philosophy', In: Early Science and Medicine, Vol. 6, No. 2 (2001), pp. 65-95

Part II

Philosophy

Mathieu Cornélis

Taking Relativity Seriously. Minkowski Spacetime, Genidentity and Persistence Through Time

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The aim of this talk is to bring together two theories involved within two really different research fields. One is the four-dimensional theory of persistence through time in a metaphysical point of view, and the other is the theory of relativity in the field of physics.

It is commonly argued that a four-dimensional object 0 persists through time by having temporal parts O(t1), O(t2), ..., O(tx) and a relation of genidentity between each part. For instance, the brown vest that I'm wearing is a four-dimensional object which is composed by different temporal parts (part-5 years ago-"new and clean", ..., part-yesterday-"clean but unstitched", part-today-"unstitched and faded") which stand in a relation of genidentity ("part-5 years ago" and "part-yesterday" are not identical parts of my brown vest but are genidentical parts of it, i.e. stand over the same world-line of my vest).

Moreover, it is also argued that, if we take relativity seriously, (1) a four-dimensional object O has to be framed in a specific relativistic language (indeed, it's clear that some pre-relativistic notions, like "moment of time", "being wholly present à t", and the like, has to be banned from our language) , and (2) to ascribe properties to temporally extended object O, one now has to keep track of a frame to which its temporal extension refers but also to keep track of the lorentz's contractions related.

But some queries arise. Indeed, how could we take the relation between temporal parts as genidentical if this very relation depends upon a particular referential frame? If the relation between different parts of a same things depend fundamentally upon a particular frame, is it possible to conceive an object (like my brown vest) as invariant through time – as it is often argued in order to account of the identity of this object? And, finally, is the concept of invariance in the field of physics ontologically relevant in the field of metaphysics?

We will conclude by redefining a relation of genidentity between each temporal part of a 4D object which takes relativity seriously.

Jan De Winter

Solving the Problem of the Distorted Research Agenda in the Health Sciences

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A well-known problem in the health sciences is the distorted research agenda: medical research is tailored to the health problems of the rich, rather than to the health problems of the poor, and for given health problems, the most promising lines of research are often neglected because they are not profitable enough. In the paper, I analyze the problem of the distorted research agenda in the health sciences in more detail and identify some of its causes.

Next, I discuss two research policies that are supposed to solve this problem: the research policy proposed by Thomas Pogge, and the research policy proposed by James Robert Brown. Pogge suggests that we should establish, as a supplement to the current market system, a Health Impact Fund that is used to reward owners of patents on medicines that improve global health. Brown, on the other hand, thinks we should eliminate patents in medicine and that all funding of medical research should be made public. I show that both research policies solve only part of the problem, as they do not eliminate all kinds of distortion. Yet, I do not reject Brown's recommendations. A research policy that is based on the elimination of patents in medicine, and that effectively solves the entire problem, is described and defended in this paper.

A possible objection against my proposal is that patent protection has several positive effects, and that without patents, we may not be able to cause these effects. I rebut this objection by showing how the positive effects of patent protection in medicine can be achieved without patents. Four positive effects are discussed: (1) the fact that medical research is sufficiently funded, (2) the efficient execution of medical research projects, (3) the translation of medical research results into health outcomes, and (4) the public availability of medical research results.

Mihnea Dobre

Knowledge of the physical body. Descartes and Cartesians on the quest for certainty in natural philosophy

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In his *Principia philosophiae*, Descartes distinguished between two types of certainty – absolute and moral – claiming for the core part of his physics to possess "more than moral certainty." If absolute certainty is reserved for his metaphysics and most of his physics is described as morally certain, what he deemed as 'more than morally certain' is still a matter of debate in respect to his natural philosophy. In my paper, I shall refer to Descartes's argument for the existence of physical bodies and to its reception in the philosophical writings of some of his followers. I shall focus in particular on the views expressed by three Cartesians, namely Jacques du Roure, Géraud de Cordemoy, and Jacques Rohault. Not only that these authors display different views on this matter, but they contributed to the development of a new approach to natural philosophy from a Cartesian point of view. All of these authors represent interesting case studies in the role of the quest for certainty in physics, contributing to the current studies on the so-called "Scientific Revolution."

Isabelle Drouet

Causal decision theory and theories of causality

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Since the 1970s, some authors maintain that decision theory cannot avoid causal considerations and, accordingly, they support causal decision theory. On the other side, philosophy of causality has significantly developed in the last 50 years, and at least three contemporary conceptions of causality (the probabilistic, counterfactual, and interventionist theories) are likely to be referred to in the context of causal decision theory. The question, therefore, arises whether the conception of causality one adheres to makes a significant difference to causal decision theory. In a recent paper entitled "Causal reasoning and backtracking" (2010), James Joyce suggests a negative answer to this question. Here, focusing on the counterfactual and interventionist conceptions of causality, I would like to discuss his arguments and, as a consequence, qualify the answer he suggests.

Raoul Gervais

Functional versus mechanistic explanations in the cognitive sciences: explaining depression and neuroticism

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In this article I examine so-called functional or decomposition explanation; a type of explanation that is at the core of the cognitive sciences. I argue that these explanations take their form because of the epistemic interests of the researchers. Where these interests are restricted to prediction or sheer intellectual curiosity, showing how a particular cognitive state operates by decomposing it into different elements may be all that is required. However, if the interests are preventative or therapeutic, such explanations must be paired with an account of what particular neural feature (or set of features) is actually responsible for realizing a given state. That is, in these cases the functional account is 'deepened' or 'augmented' with information about the brain: a type of explanation that is known in the literature under the heading 'mechanistic explanation'. To illustrate how this works, I will consider a concrete example taken from scientific practice: the explanation of neuroticism. It would appear that there are two types of explanation possible here. The first type defines neuroticism in terms of psychological states. On this account, neuroticism is an effect induced by other psychological states, in particular, by depression. It is a complex state, comprised of several different elements (such as increased anxiety, insecurity etc.). The second type of explanation cites the actual neural mechanisms involved in the realization of depression, which, as is corroborated by more than half a century of research, is a shortage of serotonin, or 5-hydrowytryptamine, in the brain. As both types of explanation serve different epistemic interests, explanatory pluralism seems a fitting position for a philosopher to take.

Laszlo Kosolosky

Analyzing Consensus Conferences: A social-epistemological study of the benefits and efficiency of climate and medical consensus conferences

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This paper studies the way in which assessment reports are produced by consensus conferences. The focus is on climate consensus conferences (as organised by the Intergovernmental Panel on Climate Change) and medical consensus conferences (as organised by the National Institute of Health and the Canadian Association of Gastroenterology).

In part 1, I analyse the review process behind the assessment reports by answering two interwoven questions for each type of conference; i.e. 'What are the benefits of consensus conferences?' and 'Do we need consensus conferences to attain these benefits?' The first question is handled by subjecting Thagard's (1999) answer for medical consensus conferences to critical scrutiny and by looking whether his answer can be extended to climate consensus conferences. As for the second question, I denounce the claim that meta-analysis is a better alternative.

In part 2, a social-epistemological exercise is undertaken to look whether the review process of consensus conferences can be altered so as to enhance its efficiency. Thagard (1999) has stated that it is because of the way the review-process is built up, that consensus conferences can contribute to the reliability, fecundity and practical benefit of beliefs. By changing this review-process we could let consensus conferences make a bigger contribution, thus increasing its efficiency, which will result in enhancing their position as the best alternative. First, I propose that there are differences in the way climate and medical researchers interact with each other, differences that turn out to have an effect on the efficiency of the corresponding conferences. Secondly, I propose that extending the group of consensus participants with stakeholders enhances the reliability of consensus conferences. Applying both proposals to scientific practice gives a firmer ground to the discipline of social epistemology and philosophy of science in general.

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Geerdt Magiels

Science as an ecosystem: the case of the discovery of photosynthesis

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An old story tells how six blind men were asked to determine what the object in front of them looked like. Each was touching a different part of an elephant. One describes a pillar, another a rope. The third feels a tree branch, the fourth a fan. Another says he touches a wall, the last says he holds a flexible pipe. A wise man explains that all of them are right. The reason every one of them is telling it differently is because each one touched a different part of the elephant. The elephant has all the features mentioned, but to understand what the elephant is, all the different perspectives have to be integrated - irreconcilable as they may seem.

In the philosophy of science something similar seems at hand. Several approaches to explain or clarify science co-exist. Many perspectives are defended, sometimes furiously attacking each other. Understandably so, as none of them seems to be able to catch the essence of what science really is, the science as practiced by millions of real people around the clock all over the world: a messy, ambitious and complex human undertaking, in which individuals and groups, theory and practice, society and culture each play a crucial role.

Looking at science as a multilayered process, from an ecological point of view, could help to re-unite the disparate approaches and paint a picture of science in all its bewildering complexity. An inspiring case to illustrate this perspective is the discovery of photosynthesis. This is the most important biochemical process on earth. Still, very few people seem to know who discovered this, when that happened and how it was done. However, the tale of Jan IngenHousz (1730-1799) and his scientific endeavours shows science in action. It opens up an undisclosed chapter of the history of science which can help to shine some light on the ingredients and processes that shape the development of science - science as the age-old undertaking of collecting trustworthy knowledge. The fate of the discovery of photosynthesis and of photosynthesis research can be better understood if one takes into account as well the individuals involved, as their social, cultural and historical context, interwoven in social interactions and interconnected by the theoretical, instrumental and practical requirements of their research. This paves the way for a fresh multidimensional approach in the philosophy of science, taking steps towards an "ecology of science".

Laurence Perbal

Human genetics, an Anglo-Saxon specificity?

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Although from Europe, mainly from German speaking parts, it is in United States and England that human genetics has really grown in the early 20th century. The reasons for that are scientific, ideological and cultural. Firstly, Mendelian genetics is well established in these countries, reinforcing a hereditarianism of innate traits. In most of Latin countries, the biological tradition is Lamarckian and then it is a heredity of acquired traits that still dominates. Moreover, human genetics in the early 20th is deeply related to the eugenic movement. Eugenics, although different in U.S.A. and in England, has been a significant financial and ideological support for its development as a scientific discipline in these both countries. Germany, under the leadership of the Nazi regime, has been a major research center too. On the contrary, in France - and Belgium -, eugenics was related to the social hygiene movement, so it has mostly stimulated the development of social reforms in public health domain. For these reasons, and some others linked to the specific philosophical and sociological contexts of that time, human genetics has been developed lately in the Latin countries and may be seen, in a way, as a specific product of Anglo-Saxon culture.

Yves Pierseaux

Manifeste pour une nouvelle révolution relativiste

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Il est philosophiquement cruel de vouloir séparer une mère de ses enfants. Il est tout aussi mathématiquement inadmissible de vouloir séparer une hyperbole de ses asymptotes. C'est pourtant ce que l'on fait en cinématique relativiste. On écrit en effet les trois types d'intervalles de Minkowski respectivement spatial R (hyperbole Ox), lumineux ou nul (asymptote) et temporel T (hyperbole Ot). Les hyperboles parfois appelées "hyperboles d'échelle de Minkowski" sont très rarement signalées dans la plupart de exposés sur la relativité restreinte (RR). En effet, on considère généralement que les asymptotes subsistent lorsque R tend vers l'infini, autrement dit lorsque les hyperboles disparaissent. Donc le fameux "cône de lumière" subsiste indépendamment de l'hyperbole d'échelle qui s'évanouit. En vérité avec un intervalle infini on retrouve non pas les bissectrices ($c = 1$) mais les axes Ox ($t = 0$) et Ot ($x = 0$) de la cinématique prérelativiste (sans cône de lumière, sans vitesse finie, c=1). Admettons dès lors, en totale conformité avec la transformation de Lorentz (TL), qu'il existe non seulement une vitesse FINIE mais un intervalle spatial FINI. Ce dernier définit un horizon de Hubble R directement relié à une constante cosmologique qui caractérise le vide spatio-temporel classique. Cette hyperbole étant une ligne d'univers, il y correspond aussi une accélération (propre) hyperbolique minimale. Cela résulte immédiatement de ce que la TL est une rotation hyperbolique et que dans tout mouvement de rotation, il y a une accélération "de Milgrom" ou une vitesse angulaire hyperbolique autrement dit une constante "de Hubble". En procédant à une petite retouche de la RR (Milne) nous la transformons en une théorie cosmologique. Nous obtenons ainsi une expansion accélérée de l'Univers qui est désormais confirmée par les observations récentes. Nos travaux précisent comment il faut réinterpréter le mouvement rigide de Born-Rindler et definir le nouveau facteur d'échelle (Bondi) qui doit prendre la place du facteur d'échelle de la métrique de Friedmann-Lemaître. Ils indiquent aussi comment il convient de definir une distance hyperbolique avec l'ellipsoïde lumineux de Poincaré (univers en expansion sans big bang).

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Explaining Emergence
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The analysis we develop here is initiated by the fact that the intrinsic ambiguity of the classical maxim of emergence: « the whole is more than the sum of the parts » has led today to an impressive conceptual dissemination which tends to make the debates about relationships between scientific fields obscure and misleading. Indeed, different interpretations of this classical maxim gave rise to distinct concepts of emergence that can hardly be reconciled in a unitary and relevant antireductionist framework.

The prime objective of our paper is then to propose a theoretical construct of a precise philosophical concept of emergence which is antithetic to a concept of reduction which also has to be clarified. Accordingly, employing Rudolf Carnap's terminology, we propose an explicatum of the traditionally ambiguous concept of emergence via three definitional requisites widely discussed in the scientific literature: supervenience, holism and downward causation. These conditions allow us to define emergence as in-principle irreducibility, i.e. the logical impossibility to explain and predict a complex macro-phenomenon from information relative to its micro-structure. The concept of emergence which satisfies simultaneously these required conditions proves to be (1) faithful to the philosophical legacy of the British emergentists, (2) philosophically adequate to construct a classificatory pattern which is able to limitate the speculative dangers of the polysemy of the classical concept and (3) coherent with the fundamental methodological presuppositions of the scientific practice.

Secondly, on the basis of our explicatum of emergence, we propose a first taxonomy of different concepts of emergence, a taxonomy in which the taxa are to be associated with different degrees of satisfaction of the definitional requisites used in our first conceptualization. By classifying the uses of the emergence concepts in this way, our taxonomy allows us to clear up the nebulous debates relating to the reductionism issue.

Hassan Shirazi

Invariance: A truthmaker for a necessary truth

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Usually it is argued that a necessary truth is true, no matter what situation holds, therefore it cannot be true in virtue of some specific facts. David Armstrong, though he agrees with the premise of the argument, believes that necessary truths are true in virtue of some distinguishable facts: internal relations, and in particular partial identity. I show that taking partial identity as the truthmaker for some necessary truths, say 'red is a color', yields inner problem in Armstrong's metaphysics. Instead, inspired by his own elegant system, I suggest that Functional Laws or Invariance is a preferable truthmaker for such necessary truths. It deserves inner consistence of his system and compatible with his theory on color as well.

Arne Vangheluwe

Miracles and metaphysics of science: going to the essence

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In aglosaxon academic communities the questions regarding "Science and religion" have known a powerful development in the past decennia, up to a point where a new discipline is taking form. One of the issues adressed there is divine action: how to articulate actions of an intelligent, planning and intervening deity to a worldview that has the natural sciences as its prime source of ontological information. In other words, how to make sense of divine intervention when faced with claims of determinism and laws of nature that seem to accept no exception? Does it make sense to speak of the "breaking" of a law of nature, and do we really need this strong-handed notion? If so, a scientific worldview and a theistic worldview seem to be mutually exclusive.

We have come a long way since David Hume's famous essay On Miracles, even when it is still at the center of numerous contributions. Several solutions have been suggested to disarm this head-on collision of natural and supernatural causality. For instance, quantum mechanics and chaos theory have clearly modified the way we think about determinism in physics. But is this enough to avoid the conflict? A proper enquiry in philosophy of science is needed here to dig into the ontological status of laws of nature. Empiricists, inductivists, necessitarians or essentialists each have given different accounts of what is a law of nature. We will look especially at the essentialist viewpoint of Brian Ellis regarding laws of nature, and we will try to draw conclusions for a modern-day conceptualisation of (what we think to be) the essence of divine action in a scientific world, i.e. what it is to be a "miracle", and we hope to show that this fascinating question in no way ends up in an irreducible choice between science and religion.

Part III

Logic

Mathieu Beirlaen

Paraconsistent Deontic Logic

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One of the problems with Standard Deontic Logic (henceforth **SDL**) is that it trivializes any premise set containing one or more *normative conflicts*. Typically, such conflicts are formalized as $OA \wedge O\neg A$, where OA stands for something like "it ought to be the case that A", "someone ought to bring about A", "I ought to do A" etc. (any of these will do for the present purposes). For a discussion on the reality of normative conflicts, and for some examples of their typical formalization, see [2].

The focus on conflicts of the type $OA \wedge O\neg A$ has led many authors to neglect other types of conflicts. As a rather awkward consequence, this narrow focus has caused the invention of systems that can allow for conflicts of the type $OA \wedge O\neg A$, but that trivialize conflicts that are in some sense "weaker" than these.

I will argue that, if one wants to allow for all types of conflicts in deontic logic, one has to make the logic's negation paraconsistent and that, moreover, if, as Priest demands in his [3], one wants to maximally isolate inconsistencies, modal interdefinability assumptions must be relaxed.

I will present the paraconsistent deontic logic **DP**, which meets the above demands, but also lacks some intuitive properties of **SDL**. This shortage, however, can be met by non-monotonically extending **DP** to the logic $\mathbf{DP}^r$, which is defined within the adaptive logic framework from [1] and which, for premise sets containing no normative conflicts, delivers the exact consequences that **SDL** would deliver.

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Lorenz Demey

Looking for the Right Notion of Epistemic Plausibility Model

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The notion of epistemic plausibility model (EPM) is widely used in contemporary epistemic-doxastic logic. However, two of the main authors in this area, Johan van Benthem and Alexandru Baltag, define these models in different ways. This paper provides an indirect methodological argument for Baltag's definition, by developing the model theory of EPMs.

We first introduce both van Benthem's and Baltag's notion of EPM, and briefly discuss some important operators which can be interpreted on such models. We then move to the model theory of van Benthem's EPMs, by defining various notions of bisimulations (parametrized by a language $\mathcal{L}$), and showing that $\mathcal{L}$-bisimilarity implies $\mathcal{L}$-equivalence. We establish a Hennesy-Milner type theorem, and prove two undefinability results. The notion of bisimulation for conditional belief, however, turns out to be unsatisfactory. We explore two possible solutions: adding a modality to the language, and putting extra constraints on the models. At first sight, neither of these solutions seems to be preferable over the other one.

We then turn to the relationship between bisimulation and dynamic updating. We will show that bisimilarity is preserved by the two main updates (public announcement and radical upgrade) according to the second solution, but not to the first solution. Since bisimilarity-preservation is a theoretical desideratum, the second solution is thus to be preferred over the first one. Furthermore, we will show that if the extra constraints of the second solution are imposed on van Benthem's notion of EPM, then it closely resembles Baltag's notion.

This can be summarized as a methodological argument for Baltag's notion of EPM. Although van Benthem's notion is more general/expressive (and thus better suited for concrete modeling purposes), Baltag's notion gives rise to a more elegant model theory (while maintaining sufficient generality). Hence a reflective equilibrium arises, favoring Baltag's notion.

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Non-standard models of arithmetic

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My work sets out to investigate the philosophical significance of non-standard models of arithmetic. In particular, I will strive to put forward the mutual relationship between Philosophy, Logic and Mathematics with respect to the study on non-standard models which has taken shape over recent years.

Not only has non-standard models of arithmetic encountered a scarce success in what concerns philosophy, but also an organic presentation on the topic has never been written.

This work will examine three distinct phases in the development of the research on the non-standard models of arithmetic conceptually as well as chronologically.

1. *Non-standard models as "pathological" problem - the philosophical roots*: I claim that Dedekind (rather than Skolem) should be considered as the one who discovered non-standard models. I will consider Dedekind's letter to Keferstein and his categorical characterisation of his system. On the other hand, I will discuss Skolem's proofs of the existence of non-standard models and their relationship with Skolem's finitist mathematical credo.

2. *The hybrid phase - some repercussions on the present-day philosophy of mathematics*: I will consider the present attempts to recover from the mismatch between the scarce interest that philosophy has paid for non-standard models over the last 60 years and the abundant mathematical results from the 1960's on.

   The hybrid phase sets out from the consideration given to the mathematical achievements on non-standard models within the philosophical debate on the nature of arithmetic. Some of those questions concern, for example, the structuralism as position in philosophy of Mathematics, and possible reasons based on mathematical practice to prefer second order logic over first order one.

Laurent Dubois

Consequences of the introduction of a constant for the 'void/potential' into a standard set theory

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Our communication will consist in a short description of the Lambda-1 language, i.e. the first-order language of a standard set theory in which has been introduced a constant Lambda with an interpretation as 'void/potential' in the domain D of objects. The constant Lambda denotes a pseudo-object. The notion of pseudo-object will be defined by contrast with that of object, in the state of affairs, the notion of set. We will describe the semantics of the Lambda constant and will establish the axiom of the void; it says that Lambda belongs to every set; the modification of some axioms of ZF will follow, including a simplification of the axiom of the infinite set. After that, we will wonder if the introduction of the Lambda constant allows the construction of the empty set without the use of an anti-tautology or of a specific axiom.

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Intensional Paradoxical Set Theory
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In this paper we study one method for identifying terms in some paradoxical set theory. Such a theory is based on comprehension restricted to positive formulas in a first-order language with $\in^+$, $\in^-$, $=^+$, $=^-$ as primitive relations, where $\in^-$, $=^-$ respectively stand for the weak negation of $\in^+$, $=^+$.

Our method for examining such a paradaxical set-theory makes use of so-called *co-admissible models*. By introducing a suitable notion of forcing, we show the existence of a structure G satisfying the comprehension scheme. In that model, identification of terms is also possible, as follows: whenever two formulas φ and ψ are strongly equivalent, one may conclude that the corresponding terms $\{x \mid \varphi(x)\}$ and $\{x \mid \psi(x)\}$ are "positively equal" in G.

Hans Lycke

Unifying the Adaptive Logic's Proof Theory. Three Strategies for the Price of One

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Adaptive logics are standardly characterized by three main elements: a lower limit logic (LLL), a set of abnormalities, and an adaptive strategy — see e.g. Batens [1]. The LLL should be a reflexive, transitive, monotonic and compact logic. Moreover, the LLL should contain classical logic! This last requirement was introduced to simplify the formulation of the adaptive logics and the metalinguistic proofs. In this paper, I will elaborate on the possibility to alter this requirement to the requirement to contain a particular extension of classical logic, viz. the extension based on the infinitary language $\mathcal{L}_{\omega_1\omega}$ — see e.g. Bell [2] and Nadel [3]. In fact, I will show that the standard format of adaptive logics can easily be altered accordingly, which is mainly due to the fact that all required properties still apply to the LLL, albeit in a slightly different way.

The logical symbols of classical logic were introduced to express metalinguistic properties. As these properties range over the infinite, turning to an infinitary language is not only technically sound, but seems philosophically sound as well. Moreover, to turn to the infinite also has two major advantages. First, I will show that, in contradistinction to the standard format presented in [1], the proof theories of adaptive logics based on different adaptive strategies can now all be characterized in a unified way. Second, I will propose a new adaptive strategy, viz. the finite minimal abnormality strategy, that arises naturally from this unification and that has some very interesting properties.

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Between NF *and* NFU

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Something in Set Theory have disturbed mathematicians for more than seventy years: not only the problem of the consistency of NF, but also the (slight?) modification introduced by Jensen in 1969 to the axioms of NF, consisting in relaxing the axiom of extensionality so as to allow the existence of atoms. This modification gave birth to the consistent system NFU, roughly speaking NF with Urelemente. But we know that this slight modification of the axiom of extensionality generates quite a lot of changes. For instance, NF disproves the axiom of choice (AC) whereas NFU doesn't even prove an axiom of infinity. However, there is a trend to study models of NFU to shed light on the possible consistency of NF. Following a suggestion of Forster, it's interesting to study models of NFU produced by moving downward/upward an ordinal κ of a model of ZF with automorphism σ and by trying to reduce the "distance" between κ and $\sigma(\kappa)$. Following this way and thanks to the notion of ambiguity, we study parallelisms between results on NF and those for NFU. By introducing different notions related to ambiguity, we will be able to present some possible ways to study the problem of the (in?)consistency of NF.

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Indefinitely large and infinite lotteries

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Exploiting the parallels between large, yet finite lotteries and countably infinite lotteries, we gain insights in the foundations of probability as well as in epistemology. In standard probability theory, with the axioms as introduced by Kolmogorov [1], it is not possible to describe a countably infinite lottery. De Finetti's reaction to this problem was to replace countable additivity in the axioms by the weaker requirement of finite additivity [2]. The probability assignment for a fair, finite N-ticket lottery with exactly one winner is trivial: every ticket has a probability of winning equal to 1/N. From an epistemological viewpoint however, the case where N » 1 is problematic: a paradox can be obtained for belief statements about such a lottery. This lottery paradox was first constructed by Kyburg [3], who concluded that the conjunction principle (CP) does not hold for rational acceptability. Although we are in favor of a formal analysis of beliefs, the popular threshold-based model of belief does not seem adequate. The infinite and finite case can both be analyzed using a form of nonstandard analysis (NSA) [4]. For the infinite lottery, NSA makes it possible to assign infinitesimal probabilities to each ticket. For the finite lottery, we propose a threshold-free model for beliefs based on an alternative formulation of NSA, called relative or stratified analysis [5]. We derive a rule that tells us when CP holds and when it is violated.

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