INTERNATIONAL SOCIETY FOR THE HISTORY, PHILOSOPHY, AND SOCIAL STUDIES OF BIOLOGY

2003 Meeting

JULY 16-20
KLI and University of Vienna
Vienna, Austria

FULL PROGRAM

International Society for the History, Philosophy, and Social Studies of Biology

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THE PROGRAM AT A GLANCE

Wednesday, July 16

9-3 PM	Teaching Workshop (Rooms LR3, LR4)
3 PM	Registration Begins (Aula)
3 PM	ISHPSSB Council Meeting 1 (LR8)
6 PM	Welcome Reception (Garden)

Thursday, July 17

8-5 PM	Registration (Aula)
9-10:30 AM	Presidential Plenary (LH)
11-12:30 PM	Parallel Sessions
2-3:30 PM	Parallel Sessions
4-5:30 PM	Parallel Sessions
8 PM	Town Hall Reception

Friday, July 18

8-5 PM	Registration (Aula)
9-10:30 AM	Parallel Sessions
11-12:30 PM	Parallel Sessions
12:00 PM	Backstage at the Journals (Aula)
2-3:30 PM	Parallel Sessions
4-5:30 PM	Parallel Sessions
5:30 PM	ISHPSSB Business Meeting (LH)
Evening	Various tours

Saturday, July 19

9-10:30 AM	Parallel Sessions
11-12:30 PM	Parallel Sessions
12:00 PM	ISHPSSB Council Meeting 2 (TBA)
2-3:30 PM	Parallel Sessions
4-5:30 PM	Evening Plenary: Stephen J. Gould's *The Structure of Evolutionary Theory* (LH)
8 PM	Banquet

Sunday, July 20

9-10:30 AM	Parallel Sessions
11-12:30 PM	Parallel Sessions
2:30 PM	KLI tour: bus to Altenberg leaves
3:00 PM	Konrad Lorenz Research Station (KLF) tour: bus to Grünau leaves

Tuesday, July 22

9:30 AM	Mendelianum tour: bus to Brno leaves

PROGRAM SCHEDULE

Wednesday, July 16

9-3 PM Teaching Workshop (Rooms LR3, LR4)
3 PM Registration begins (Aula)
3 PM ISHPSSB Council Meeting 1 (LR8)
6 PM Welcome Reception (Garden)

Thursday, July 17

8-5 PM Registration (Aula)
9-10:30 AM Presidential Plenary (LH)

11-12:30 PM Parallel Sessions
- I Interpreting Evolutionary Theory (LR1)
- II Biologists in Policy-Making (LR4)
- III Biological Information (LR7)
- IV Reconfiguring Knowledge (LR2)
- V Biology and Gender (LR8)
- VI Zoos in Central Europe (LR6)
- VII Making Sense of Interlevel Causation (LR9)
- VIII Fifty Years of the Molecular Biology of Behavior (LR10)
- IX History and Philosophy of Neuroscience (LR3)
- X Genetics in History (LR5)

2-3:30 PM Parallel Sessions
- I Experiments in Experimentalism I (LR4)
- II History of 20th Century Biology (LR8)
- III A Cultural History of Heredity I (LR1)
- IV Picturing Eggs, Embryos, and Cells I: Eggs and Embryos *in Situ* (LR2)
- V Complexity: What is it Good For? I (LR5)
- VI Konrad Lorenz and Company (LR6)
- VII Genetics and Policy-making (LR10)
- VIII The Extended Phenotype Revisited I (LR9)
- IX Biological Modeling I (LR7)
- X Ecology Transformed (LR3)

4-5:30 PM Parallel Sessions
- I Experiments in Experimentalism II (LR8)
- II Contingency and Explanation (LR6)
- III A Cultural History of Heredity II (LR1)
- IV Picturing Eggs, Embryos, and Cells II: Cells and Tissues in Motion (LR2)
- V Complexity: What is it Good For? II (LR7)
- VI Women in Early Genetics (LR3)
- VII The Extended Phenotype Revisited II (LR9)
- VIII History and Philosophy of Anthropology (LR5)
- IX Biological Modeling II (LR10)
- X Biology and 'Naturalness' in Organic Agriculture (LR4)

8 PM Town Hall Reception

Friday, July 18

8-5 PM Registration (Aula)

9-10:30 AM Parallel Sessions

- I Radiobiology in the Atomic Age (LR1)
- II Evolution and Development I (LR7)
- III Philosophy of Cognitive Science I (LR8)
- IV Modes of Research in Biology (LR2)
- V Animal Communication (LR6)
- VI Metaphor and Communication I (LR10)
- VII Genes, Genomes, and Genetic Elements I (LR9)
- VIII Functions and Teleological Explanation I (LR3)
- IX Biology and Ethics/Epistemology I (LR5)

12:00 PM Backstage at the Journals (Aula)

11-12:30 PM Parallel Sessions

- I Karl Pearson, Ronald Fisher, and the Statistical Roots of Biology (LR7)
- II Evolution and Development II (LR1)
- III Philosophy of Cognitive Science II (LR8)
- IV Endangered Species, Threatened Paradigms (LR4)
- V Recent Work on Pluralism and the Levels of Selection (LR6)
- VI Metaphor and Communication II (LR10)
- VII Genes, Genomes, and Genetic Elements II (LR9)
- VIII Functions and Teleological Explanation II (LR3)
- IX Biology and Ethics/Epistemology II (LR5)
- X History, Philosophy, and Social Studies of 18^{th}-19^{th} C. Biology I (LR2)

2-3:30 PM Parallel Sessions

- I Biology and Anthropology I (LR8)
- II The Mutual Shaping of Science and Science Education I (LR5)
- III Visual Zoology on Wall Charts (LR6)
- IV Biology, Biotechnology and Policy (LR4)
- V Evolution and Politics I (LR9)
- VI Lorenz' Concept of Instinct (LR7)
- VII Life, Metaphysics, and Biosemiotics I (LR10)
- VIII Developmental Regulation I (LR1)
- IX Fitness, Drift, Evolutionary Theory (LR2)
- X History, Philosophy, and Social Studies of 18^{th}-19^{th} C. Biology II (LR3)

4-5:30 PM Parallel Sessions

- I Biology and Anthropology II (LR8)
- II The Mutual Shaping of Science and Science Education II (LR5)
- III Exhibiting Humans and Animals (LR7)
- IV Digital History of Biology (LR6)
- V Evolution and Politics II (LR9)
- VI Philosophy and Ecology (LR3)
- VII Life, Metaphysics, and Biosemiotics II (LR10)
- VIII Developmental Regulation II (LR1)
- IX Science in its Social Context (LR4)
- X Metascience from a Biological Point of View (LR2)

5:30 PM ISHPSSB Business Meeting (LH)

Evening Various tours

Saturday, July 19

9-10:30 AM Parallel Sessions

- I Issues in Sociocultural Evolution I (LR1)
- II Biology of Human Behavior I (LR2)
- III Challenging the Essentialist Story about the History of Taxonomy I (LR6)
- IV Indeterminism and Evolution (LR7)
- V European Roots of Evolutionary Psychology (LR8)
- VI Biology and Education I (LR4)
- VII Trajectories of Drugs (LR3)
- VIII Topographies of Knowledge Production in Aquatic Ecology I (LR5)
- IX William Bateson and the Suppression of Epigenetic Biology (LR9)
- X Biosemiotics (LR10)

12:00 PM ISHPSSB Council Meeting 2 (TBA)

11-12:30 PM Parallel Sessions

- I Issues in Sociocultural Evolution II (LR1)
- II Biology of Human Behavior II (LR2)
- III Challenging the Essentialist Story about the History of Taxonomy II (LR6)
- IV Perspectives on Population I (LR9)
- V Microbial Challenges (LR3)
- VI Biology and Education II (LR8)
- VII Bioinformatics and the Transformation of Biomedical Research (LR7)
- VIII Topographies of Knowledge Production in Aquatic Ecology II (LR5)
- IX Biology and Metaphysics I (LR10)

2-3:30 PM Parallel Sessions

- I The Romantic Conception of Life (LR6)
- II Ecology and Environmental Values (LR8)
- III Inheritance and Evolution (LR7)
- IV Perspectives on Population II (LR9)
- V Naples as an Evolutionary Niche (LR5)
- VI Beyond Lorenz (LR1)
- VII Sex, Gender, and Immunology (LR2)
- VIII Emergence and Mechanisms in Biochemical Networks (LR3)
- IX Biology and Metaphysics II (LR10)

4-5:30 PM Evening Plenary: Stephen J. Gould's *The Structure of Evolutionary Theory* (LH)

8 PM Banquet

Sunday, July 20

9-10:30 AM Parallel Sessions

- I Darwin (LR1)
- II Biohistory – Neo-Darwinism's Last Frontier (LR2)
- III Alternative Evolutionary Theories I (LR5)
- IV Autonomy: A Key Concept I (LR6)
- V 'Race' in Theories of Human Origins and Diversity (LR7)
- VI Science and Policy: International Perspectives I (LR3)
- VII Dimensions of Genomics (LR9)
- VIII Biology and Meaning I (LR8)
- IX Biological Hierarchies I (LR10)

11-12:30 PM Parallel Sessions

- I Boundaries in Biomedicine (LR1)
- II Scientific Change from a Biological Point of View (LR2)
- III Alternative Evolutionary Theories II (LR5)
- IV Autonomy: A Key Concept II (LR6)
- V Is There a Kantian Tradition in Biology? (LR9)
- VI Science and Policy: International Perspectives II (LR3)
- VII Between Gender, Politics, and Biology (LR7)
- VIII Biology and Meaning II (LR8)
- IX Biological Hierarchies II (LR10)

2:30 PM KLI tour: bus to Altenberg leaves

3:00 PM Konrad Lorenz Research Station (KLF) tour: bus to Grünau leaves

Tuesday, July 22

9:30 AM Mendelianum tour: bus to Brno leaves

ABSTRACTS

Wednesday, 16 July

9-3 PM, LR 3, LR4

Teaching Workshop

'Biology and Society' Programs:
Preparing Students for Biology in Social Context

In a variety of local and global issues, including environmental quality, agriculture, health care, biotechnology, and international relations, the biological sciences are interwoven with social, economic, and political concerns. Traditional undergraduate biology programs fail to critically address the social contexts of science, and programs in the history, philosophy, or social studies of biology may not foster adequate understandings of biology subject matter. Are 'biology and society' programs promising ways to prepare future policy-makers, educators, biologists, and citizens to understand and constructively influence the complex issues of biology in social context?

In this interactive workshop, participants will examine a variety of goals, topics, instructional approaches, and challenges for biology and society programs. Participants will hear from colleagues who have taught biology and society courses, and have a chance to design their own biology and society course syllabus. Presenters and topics will include:

- **Rivers Singleton**, U of Delaware, USA—Biology and society: Some idiosyncratic definitions

- **Jim Collins**, Arizona State U, USA—Challenges of supporting biology and society programs at departmental, institutional and national levels

- **Jane Maienschein**, Arizona State U, USA—The Biology and Society Program at Arizona State University (see: http://lsvl.la.asu.edu/biosoc/)

- **Henny van der Windt**, U of Groningen, The Netherlands—Efforts to bridge the gap between biology and social-ethical perspectives at the University of Groningen.

- **Manfred Laubichler**, Arizona State U, USA —The role of history of biology in a biology and society curriculum; designing a biology and society course

Organized by **Steve Fifield**, Chair of the ISHPSSB Education Committee.

Thursday, 17 July

9-10:30 AM, LH

Presidential Plenary

History, Philosophy, and Social Studies of Biology: Where Are We and Were Are We Going?

• Opening Remarks, **Lindley Darden**, ISHPSSB 2001-2003 President, U of Maryland, USA

• Welcome from local hosts, **Werner Callebaut**, Scientific Manager, Konrad Lorenz Institute for Evolution and Cognition Research, and **Astrid Juette**, Executive Manager, Konrad Lorenz Institute for Evolution and Cognition Research

• Presentation of the Marjorie Grene Prize: Winner: **Kevin Elliott**; Presenter: **Pamela Henson**, Grene Prize Committee

• The Double Face of Janus: Bringing Together History of Medicine and History of Biology, **Bernardino Fantini**, Editor of *History and Philosophy of the Life Sciences*, Geneva Medical School, Switzerland

• History of Biology Today, **Jane Maienschein**, Co-editor of *Journal of the History of Biology*, Arizona State U, USA

In 1968, Everett MENDELSOHN started the *Journal of the History of Biology* because he felt the time had come for biology to take its place alongside the physical sciences within the history of science. Yet the history of biology only gradually assumed a significant role at History of Science Society meetings in the USA, in general journals, or in faculty positions. Some historians of biology have felt more comfortable at ISH, PSA, or Biology meetings than at HSS. Yet now those in other areas complain that the history of biology has 'taken over', just as physical scientists complain that biologists are taking over NSF funds. What is the history of biology such that it is perceived as having grown from barely visible to threateningly dominant in just a few decades? Why has it not developed stronger and more robust connections with the history of medicine, psychology, and other related fields? Why the apparent impulse to speciate separate fields? Where is the history of biology going?

• Philosophy of Biology Tomorrow, **Peter Godfrey-Smith**, Associate Editor of *Biology and Philosophy*, Stanford U, USA

I will briefly tie together some promising themes in recent philosophy of biology—themes which I hope will be explored further in the future. This will be done by linking some specific projects of philosophical analysis with some general ideas about scientific language and modeling.

• Social Studies of Biology Today, **Michael Lynch**, Editor, *Social Studies of Science*, Cornell U, USA

Currently, there is no bounded sub-field of social studies of biology. There *is*, however, a growing number of social and cultural studies that focus on particular developments in biology, biotechnology, and medicine. The studies are difficult to compartmentalize by topic area, partly because they tend to reject distinctions between disciplines, as well as between pure and applied research, and science and technology. This rejection of 'boundaries' does not imply an undifferentiated view of

'science'; quite the opposite, it implies that 'technoscience' is far too differentiated to be meaningfully compartmentalized in terms of biology, bioengineering, medicine, etc. An all-too-brief review will be given of current research on biology and biotechnology.

• History of the Life Sciences at the Max Planck Institute for the History of Science,
Hans-Jörg Rheinberger, Director, Max Planck Institute for the History of Science, Germany

Most of the projects of Department III of the MPI for the History of Science (Director: Hans-Jörg RHEINBERGER) are situated in the realm of the biological and medical sciences between the 18th and the 20th century. The overarching interest in pursuing these projects is, however, not confined to disciplinary boundaries and not dominated by disciplinary questions. We are interested in the historical and epistemological conditions of scientific innovation and their relation to the history of the material culture and the practical dynamics of science. The projects can be grouped around three organizing centers: (1) history and epistemology of experimentation; (2) history of epistemic objects, spaces of knowledge, and the changes in the mental equipment of scientists; (3) concept formation and the uses of theory.

• EvoDevo at the Konrad Lorenz Institute, **Gerd B. Müller**, U of Vienna, Chairman, Konrad Lorenz Institute for Evolution and Cognition Research, Austria

The KLI promotes the formulation and discussion of new theoretical concepts, primarily in the areas of evolutionary developmental biology and evolutionary cognitive science. Research in these areas is supported by fellowships for graduate students, post-docs, and visiting scientists. In addition, the KLI organizes lecture series at the University of Vienna and hosts roundtable discussions and workshops at the Lorenz mansion. In my introduction I will focus on the EvoDevo agenda that is central to our research activities.

• Where Do We Go Now? Themes in the ISH03 Program, **Rob Skipper**, ISHPSSB 2003 Program Chair, U of Cincinnati, USA

The 2003 ISHPSSB program is, no doubt, one of the largest in ISHPSSB history. I will reveal what I take to be some of the themes of the program, highlighting special sessions. Is where the 2003 ISHPSSB program is, where history, philosophy, and social studies of biology is going?

Thursday, 11-12:30 PM

SESSION I, LR1

Interpreting Evolutionary Theory

Organizer and Chair, **Thomas Kane**

• Adaptation and Natural Selection in Caves, **Thomas Kane**, U of Cincinnati, USA

Natural selection and genetic drift represent two competing hypotheses for the explanation of genetic change in populations. The evolution of cave dwelling organisms provides an interesting empirical case for distinguishing between these two hypotheses. Some features of these organisms (e.g., hypertrophy of extra-optic sensory structures) appear to be 'obviously adaptive'. Conversely, population sizes of cave dwelling organisms are often quite small, suggesting that genetic drift may play a significant role in their evolution. Further, regressive morphological features (e.g., reduction of eyes; loss of pigmentation) that are widespread among cave dwelling forms are 'less obviously adaptive' and have often been explained in terms of genetic drift. Using multiple approaches including fitness and heritability measures, as well as population genetic and phylogenetic analysis, we find a significant role for selection in the evolution of both the hypertrophied and regressed features of a cave dwelling amphipod crustacean. The study, in part, addresses Robert BRANDON's "components of complete adaptation explanations."

• Patterns of Drift, **Robert C. Richardson**, U of Cincinnati, USA

Natural selection and drift can explain the dynamics of populations, how gene frequencies, or genotype frequencies, or gene frequencies change with time. Given an initial distribution of genes, or genotypes, or phenotypes, with realistic parameter values for the population, we are able to project a probability distribution of the relevant frequencies over time. In the absence of selection, models for drift project specific patterns of change. These can be readily illustrated using the classic work on blood types by CAVALLI-SFORZA, in which the theoretically predicted patterns are exhibited in groups of human populations. These are, in the first instance, properties of ensembles of populations. In looking at specific populations, the problem needs to be understood in terms of how likely an observed change would be under drift, and this is a fundamentally probabilistic question. In both cases, the explanations are demonstrably autonomous, in the sense described by HACKING in *The Emergence of Probability*.

• Experimental Models and Interpreting Evolutionary Theory, **Rob Skipper**, U of Cincinnati, USA

Recently there has been a flurry of philosophical work on the interpretation of evolutionary theory. There are, so it would seem, two alternatives: a force interpretation and a statistical interpretation. Conclusions about the conceptual distinction between random genetic drift and natural selection have been articulated via implications of these interpretations. What the apparent alternative interpretations of evolutionary lack is any direct reference to the role of experimental models in making the distinction between drift and selection. In this paper, I argue for an interpretation of evolutionary theory that places much importance on the role of experimental models in interpreting evolutionary theory with respect to drift and selection. The paper relies on the work presented by KANE and RICHARDSON in the same session.

Thursday, 11-12:30 PM, LR4
SESSION II

Biologists in Policy-Making

Organizer: **Heather Douglas**, Chair: **Heather Quinley**, Cinergy, USA

• How Can Biologists Contribute to Public Policy? **Carl Cranor**, U of California, Riverside, USA

This presentation, based upon both research and working with a variety of biologists, will explore several different ways in which biologists (broadly construing this term) could contribute to policy making. Most of the examples discussed will go beyond some conventional ideas about doing the science in some 'objective', value-free way. Biologists could frame research in accordance with a particular normative agenda, as some have, to ensure as far as the research can determine, that human health and the environment are protected from adverse effects. More broadly, they could set a research agenda to try to anticipate potential problems before they became serious threats to human health or the environment. There are also methodological approaches that would ensure more even-handed presentations of scientific evidence for policy purposes. Finally, there is a need also for biologists as 'honest biological brokers' in policy debates.

• Ecological Indicators: Science and Policy Intertwined in the Classification and Evaluation of Nature, **Esther Turnhout**, **Matthijs Hisschemöller** and **Herman Eijsackers**, Vrije Universiteit, The Netherlands

Ecological indicators are instruments to assess the ecological quality of nature. They use a selection of characteristics that are considered relevant and are assumed to represent the ecological quality of nature. Both science and policy are involved in the development and use of ecological indicators. Ecological indicators are used in nature conservation policy because it is assumed that they objectify and operationalize ecological quality. Because of the normative and value-laden aspects associated with nature and the personal preferences and subjective choices involved in the assessment of ecological quality, ecological indicators are often controversial. This paper will conceptualize ecological indicators as classification systems and boundary objects. It will furthermore use the concept of boundary work to describe processes going on between scientific and policy actors involved in the development and use of ecological indicators. Empirical case studies on the role of ecological indicators in Dutch nature conservation policy that were undertaken as part of my PhD research, will be used to identify key factors that influence the development, use and/or non-use and efficacy of ecological indicators. This paper will show that flexibility and ambiguity, as opposed to rigidity, are important characteristics of effective ecological indicators. However, these characteristics are outcomes of social processes and cannot be used as general rules for the design of effective ecological indicators. This paper will show that context related factors such as views of nature and organizational context/culture, are important in understanding what will be taken to be flexible or rigid.

• Electromagnetic Field Effects and the Governance of Risk: The Precautionary Principle and the Interaction of Regulatory and Biological Models in the Context of Policy-Making, **Justus Lentsch**, Institute for Science and Technology Studies, Bielefeld U, Germany

How are biological models used in the context of policy-making? What is the relation between scientific risk analysis, scientific advice and policy making? Where does science end and policy-making begin? Scientific advice nowadays plays a crucial role for policy-making. But there is no clear demarcation between risk assessment as the realm of science and risk management as that of politics any more (cf. DOUGLAS 1998). In the course of this development, the idea of scientific models as an objective resource for policy analysis is increasingly challenged. Instead one can observe an interaction between scientific and regulatory models and, accordingly, the influence of different kinds of implicit value judgments in the development of 'risk-relevant science'. This has

been shown, e.g., by DOUGLAS (1998) in the case of dioxin science and policy making, and by MORGAN and DEN BUTTER (2000) in the case of economic modeling.

To bridge the gap between scientific risk analysis and policy making it is widely suggested to adopt the 'precautionary principle' as a regulatory principle. It is a principle of how to act and how to direct further research in the face of scientific uncertainty (cf. EU Commission 2000). In this paper I will examine the case of electromagnetic field (EMF) effects by mobile phones on humans in Europe and especially in the UK. Relying on the results of the WHO report *Establishing a Dialogue on Risk from Electromagnetic Fields* (2002) I will mainly discuss the reports of two British advisory committees, that of the Independent Expert Group on Mobile Phones (IEGMP), the *Stewart Report* (2000), and of the Advisory Group on Non-Ionising Radiation (AGNIR) on *ELF Electromagnetic Fields and the Risk of Cancer* (2001). I will take a closer look at how the precautionary principle is supposed to deal with scientific uncertainties regarding the potential EMF health effects on humans. Drawing on recent accounts of models as autonomous "mediators" between theory and reality (cf. MORRISON and MORGAN 1999). I will explore the interaction of regulatory and biological models with respect to EMF health effects in the context of policy making and policy recommendations in the EMF case. Moreover, I will make explicit the different epistemic and non-epistemic value judgments implicitly involved. Finally, I will argue that in this context models become a kind of autonomous hybrid epistemic objects which are used to bridge between the normative and the positive domains of policy related science.

• Biology and Disease Causation, **Douglas L. Weed**, National Cancer Institute, USA

The role of biology in public health practice is examined in this paper. Effective primary disease prevention in public health relies upon valid and reliable claims of causation. Examples include smoking as a cause of lung cancer, human papilloma virus as a cause of cervical cancer, and diethylstilbestrol as a cause of vaginal cancer. Causality is inferred from available scientific evidence, typically published studies from biology and epidemiology. In the past fifty years, biological evidence—especially as it relates to the concept of biological plausibility—has played an increasingly important role in the practice of causal inference. In this paper, definitions and rules of inference associated with biologic plausibility are described. A research program for improving the practice of causal inference is proposed with special attention paid to the role of objectivity and values.

Thursday, 11-12:30 PM, LR7
SESSION III

Biological Information

Contributed Papers, Chair: **Ulrich Krohs**

• Information Theory and Immunology, **Andrea Grignolio**, U of Rome, Italy

My research project aims to analyze how, after 1948 and until the 1960s, information theory (Cybernetics and Mathematical Theory of Communication), and in general the information metaphor, were used within the theoretical debate on the nature of the antibody formation mechanisms of acquired immunity, and whether or not information theory carried out an heuristic role in the transition from instructive to selective models of antibodies formation. Another problem is to attempt to reconstruct, within the immunology and molecular biology history, the reasons which permitted the success of the un-semantic information instead of many other theories available which integrated the syntactic and meaning dimensions.

• Semiotic Models, the Design of Molecular Structures, and the Function of DNA,
Ulrich Krohs, U of Hamburg, Germany

Semiotic models in molecular genetics fit nicely to the underlying mechanism. Presupposing not only metaphorical content but adequacy, they either have to be fully reducible to physicalistic descriptions, or they generate a local ontology. If reduction, as I agree, fails as a matter of principle, the use of semiotic ontology is in need of further justification with respect to its adequacy to the underlying mechanism. I will present a proposal for such a justification. I localize the irreducible content of semiotic talk in the functional aspect: The function of DNA is to store information, the function of RNA-polymerase is to transcribe it into RNA, the function of ribosomes is to translate it into protein. Therefore, I have to investigate the role, which the notion of biological function plays in linking the physicalistic and the semiotic sphere. But what kind of biological function is it to store a sign? Compare it to the (a minor) function of the spleen to store blood. Storing information and storing blood seem to be different kinds of function, so it looks as if a new notion of function was required. The problem is not that serious. I propose that we get things right if we regard not storage, but information itself as the functional term. Being information is the primary function. (Storage we can get additionally for nothing.) To account for this, the etiological notion of biological function will do the job. (Instrumentalist notions would not suffice!) The etiological notion is linked to the notion of design. I will give an explication of design that overcomes the problems with this very notion. The notions of design and function bridge physicalistic and semiotic descriptions in the following way: DNA is designed by evolutionary processes to have a certain primary structure. 'Being naturally designed to…' implies by definition 'having the biological function to…', i.e., DNA has the biological function to have a certain structure. 'To have a certain structure' means in terms of semiotics: 'to be (or: to store) information'. The result of this translation is: The evolutionary acquired function of DNA is to be (to store) information. In this way, semiotic models are related via function ascriptions to molecular structures. (I will consider molecular processes as well.) The adequacy of semiotic models is strictly guaranteed by the molecular mechanisms as viewed in evolutionary perspective. This justifies the use of semiotic models to describe genetic mechanisms.

• Information Metaphors and the Meaning of 'Gene',
Richard C. Francis, Independent Scholar, USA

The gene-centric view of both development and evolution has been greatly abetted by the transformation of the biochemical DNA into a semantic or meaning-bearing teleological entity. In large part this transformation can be traced to the infiltration of folk information metaphors into biology, as a result of which the material (physicochemical) properties of genes have been increasingly deemphasized. The result is a fundamental ambiguity as to what the term 'gene' refers to. I will argue that we should reserve the term 'gene' for the relevant material bits of DNA. The immaterial bits of information that currently also go by that name require a different label in order to avoid confusion. I propose the term 'genie'. As the term implies, genies are problematically antinaturalistic, indeed, 'spooky' entities. That they currently pass for legitimate theoretical constructs is a tribute to the power of information metaphors in this information age. I will discuss two distinct types of genies: (1) the Aristotelian genies that figure prominently in gene-centric explanations of development; and (2) the Platonic genies that figure prominently in gene-centric explanations of evolution.

Thursday, 11-12:30 PM, LR2
SESSION IV

Reconfiguring Knowledge: Intersections Between Physiology and Philosophy in the Last Quarter of the 19th Century

Organizer and Chair: **Francesca Bordogna**

• Physiological Reconfigurations of Mind and Knowledge: Hermann von Helmholtz, Ernst Mach on the Unity of Knowledge, **Edward Jurkowitz**, U of Illinois, Chicago, USA

Focusing on Ernst MACH, and making comparisons to HELMHOLTZ, this paper examines the use that these leading scientists-epistemologists made of their physiological studies. Analyzing their positions regarding the central questions of objectivity and theoretical reduction, I show that they turned their physiological studies to different ends. While HELMHOLTZ interpreted aspects of the physiology of vision (corresponding points, etc) in order to create a central common store of agreed upon knowledge and conceptual unity, MACH presented a critical counterpoint. MACH turned the imperfection of knowledge grounded in human sensory apparatus to justify and make inevitable a diversity of theoretical viewpoints. While both challenged a range of traditional philosophical notions, they differed in their positions on reduction and proper scientific methodology. I suggest that HELMHOLTZ's and MACH's different positions, including those on the unity of knowledge, derived in part from their locations in the unifying German Reich and the disintegrating Austro-Hungarian Empire, and to their diverging uses of the cultural resources of German liberalism.

• Psychology Between Philosophy and Physiology: Demarcation and Discipline Formation in Late 19th Century France, **Daniela Barberis**, U of Chicago, USA

The last quarter of the 19th century in France witnessed the emergence of a new psychology, which aimed to be scientific and called itself 'physiological psychology'. It aggressively sought to break the dominance of philosophy over its subject matter and, in order to distance itself from philosophical psychology, drew near to the life sciences and especially to physiology. The foremost spokesman of this new psychology in France, Théodule RIBOT, was also the founder of its first professional philosophy journal, the *Revue philosophique.* As this simple fact indicates, the demarcation of psychology from philosophy was a complex process, a process in which physiology, given its high scientific status at that time, played an important role—as both ally and purveyor of arguments to undermine philosophical conceptions. My paper will examine the process of boundary formation leading to the establishment of psychology as an independent academic discipline in France.

• The Disputed Boundaries: William James, Philosophy, and the Naturalistic 'Science of Man', **Francesca Bordogna**, Northwestern U, USA

The second half of the 19th century witnessed the dissolution of the traditional all-encompassing philosophical inquiry into human nature and the emergence of increasingly specialized disciplines of the human subject. Physiology and the life sciences contributed to that process, by engendering a multi-sided, aggressively expanding naturalistic 'science of man'. Naturalists challenged the social and political primacy of the philosophical study of human nature, and questioned the educational value of the philosophical training of the mind. This paper locates William JAMES at the intersection of disciplinary and pedagogical debates that saw the supporters of the naturalistic and evolutionary science of man pitted against the defenders of the traditional philosophical study of the human subject. Focusing on the early years of JAMES's career, when JAMES was an instructor of physiology at Harvard College, I reconstruct JAMES's interventions in the disputed boundary territory separating 'introspective philosophy' from brain physiology. The paper describes JAMES's endeavors to project an image of himself as a 'man of the two disciplines', and his efforts foster a collaborative form of inquiry that would engage both philosophers and naturalists. I contend that

JAMES's early negotiations of disciplinary and pedagogical boundaries brought him to a novel understanding of philosophy as a cooperative enterprise in which general laws could be derived inductively from the results obtained in a wide range of sciences, including, especially, physics, physiology, and biology.

Thursday, 11-12:30 PM, LR 8
SESSION V

Biology and Gender

Organizer and Chair: **Smilla Ebeling**

• The Gendered Brain, **Sigrid Schmitz**, U of Freiburg, Germany

There is an outstanding focus in recent research on gender differences in brain structure and function with implications for human behavior and cognition. These implications are mostly drawn against a biological-neuroendocrinological background, emphasizing the 'nature-theory' of sex differences. Alternatively, recent concepts of the plasticity of brain development and neural network differentiation depending on external input and individual experiences reveal a contrasting view on the nature-nurture-debate. The latter approach points out gender constructions in the brain formed by a specific psychosocial and cultural background. Considering gender studies in modern brain research with respect to methodical evaluation and theoretical reflection, I will point out the embodiment concept of individual and cultural experience in the brain.

• Gendered Genes? **Bärbel Mauss**, Humboldt U, Berlin, Germany

Starting from the tremendous importance of biomedicine for our conception of human life, I would like to point out in my talk that there is a cultural contextuality within scientific recognition. I will do so by re-reading scientific texts out of a current field of research—the field of 'genomic imprinting'—with regard to their descriptions of fundamental categories in the modern age, especially the category 'gender'. The example of the phenomenon 'genomic imprinting' shall illustrate how cultural norms are inscribed both in the research practice and in the biomedical material. One of the remarkable features of genomic imprinting is that the effect of a gene depends on its respective parental origin, i.e., the same gene causes different phenotypical marks depending on its parental origin. Some genes can be read preferentially, i.e., they wear a sex-specific mark in the form of biochemical markers. Those signalize a certain DNA-segment that is not legible from one certain strand. In genomic imprinting, as I would like to set out, the quotation of the norm of the sexes shifts to the molecular level. In the field of genomic imprinting I will demonstrate that the biomedical complex has a normalizing effect, in particular with regard to the two sexes and heterosexuality.

• Biological Theories about Sex and Reproduction as Mirror of Ideas of Human Sex and Gender Issues, **Smilla Ebeling**, U of Oldenburg, Switzerland

In my paper I will point out interconnections of the understanding of sex and gender systems in society and biology. Biological descriptions of sex and reproduction of animals and plants mirror the ideas of human sex and reproduction, and, implemented in biological theories, these ideas of human sex and reproduction serve as explanations for human sex, gender, and sexualities. I will give historical and recent examples of critical analyses of biological descriptions of animals and plants that show naturalizations and fortifications of human sex and gender ideas. One important question of my talk concerns the application of concepts that are connotated by human values like marriage, parthenogenesis, homosexuality, and rape, for example. Should we be cautious not to apply these concepts in biological descriptions of animals and plants or do concepts like sexuality and transvestites, for instance, help to point out that biological theories carry sex and gender inscriptions?

Thursday, 11-12:30 PM, LR6
SESSION VI

Zoos in Central Europe

Organizer and Chair: **Mitchell Ash**

• A Venue of Social Pleasures: The Establishment of the Zoological Garden of Frankfurt am Main, **Ayako Sakurai**, Cambridge U, UK

The purpose of this paper is to point out two cultural moments instrumental in the success of the zoos as an important social venue in the German-speaking countries in the latter half of the 19th century. Taking the establishment of the Zoological Garden of Frankfurt am Main (founded in 1858) as an example, I will suggest that the interaction between the practice of *spazieren*—leisure walk—and contemporary print culture helped the new institution to open up a new niche in the urban cultural market. Firstly, I will demonstrate that the experience of seeing animals in the zoo derived much of its attractions from preceding acquisition of knowledge about nature from the flourishing contemporary print culture—the spectators' pleasure was sustained by the sense of reality generated from their previous encounters in illustrated journals and popular books. Secondly, I will elucidate how the practice of *spazieren* enabled the integration of the appreciation of animals into the polite culture of the middle-class *Bürgertum*. The Zoological Garden in Frankfurt was primarily designed as a pleasure garden where middle-class people would visit to take a walk with one's family or acquaintance. A stroll in such a place meant much more than physical exercise—it encompassed the regular socialising among the promenaders enfolded in the form of polite conversation. An important component of such conversation was the appreciation of the wonder and beauty of nature, which the zoological garden and its collection of animals visualised and represented in an epitomised and tangible way. The common stock of knowledge of natural history, created through the printed press, rendered these animals into amusing conversation pieces that enhanced the social pleasures of the strollers.

• The Nature of the City: Zoos as Spaces of Urban Culture,
Christina Wessely, U of Vienna, Austria

Zoos are traditionally tightly connected with urban culture, while at the same time they always symbolize the 'other', the exclusive right in the middle of the modern metropolis. The entire concept of the zoo is about negotiations about nature and culture and their struggle for meanings within its terrain. The paper tries to identify the diverse relations and references that exist between the 19th century zoo and the city. It shows how signs of the natural are inscripted in urban texts, and vice versa, meanings of the urban are embedded in the virtual wildness of the zoo. Taking the Berlin Zoo, founded in 1844, as an example, three main lines of reference between the rapidly growing metropolis and its 'Zoologischer Garten' are to be shown: The zoo produces specific choreographies of motion through establishing a wide range of explicit and implicit instructions which coordinate people´s behaviour and motion in its terrain. The so constructed semantics of the visual are very similar to techniques of urban perception the visitors know well from their urban environments. At the same time, the dominant meanings of the zoo hidden in those instructions are confronted with alternative ways of acting that show that the visitors bring their own meanings to the zoo, taken from their urban cultural background. In a second step it is to be shown how material formations which give the zoo its specific design, e.g., architectural forms which are *en vogue* in the city and determine techniques of perception there, are rebuilt and cited in the zoo. Beside those architectural connections, other moments which characterize the rapidly growing metropolis and make its visual appearance as well as its 'place image' can be found in the zoo. Themes of the city's own self-description, some of its main cultural tropes are quoted in the zoo. Thus, signs and meanings of the 'outside' urban space are imported and mixed with connotations of the natural. Yet the zoo is not a copy or an imitation of the city, but it is one of its central strategies to inscribe utopia on the terrain of the familiar and vice versa. The zoo is a version of a town, it is based on a particular urbanism and it accumulates trends of the city that are everywhere visible but nowhere

so acutely elaborated. But it's not just the city influencing the zoo and stretching out into the territory of the wild: Images of the 'modern' Berlin are closely linked with narrations of the natural which means that 'nature' does no longer—if it ever was so—occur only in strictly concluded areas like zoos or gardens but suddenly seems to pop up anywhere in the city, also at places where it is not supposed to do so. What finally is to be shown is that the zoo is, for the very reason that it has those diverse relations with the city, not the other. What it actually does, not least because of its claim to reflect social processes through its inhabitants, the animals, is that it tells us many things about urban space, about its architectural, social, and political concepts and about fears, hopes, and desires of its visitors concerning all the specific demands which different versions of the urban place upon them.

• Between Education and Entertainment: The Public Schönbrunn Menagerie in the 19th and Early 20th Century, **Oliver Hochadel**, U of Vienna, Austria

Without an audience a zoo does not make much sense. Yet while interest in the history of the zoo has increased steadily in the past decades the spectators have received relatively little attention. A very interesting intersection of science and the public is still to be researched. What did the spectators actually see when they looked into the cages? Respectively: who was trying to define what the spectators should see? What were the expectations of spectators and 'zoo management' and how did they change over time? The case of the Schönbrunn Menagerie offers interesting answers to these questions. In the course of the 19th century the number of visitors increased steadily. Unlike most other zoos there were hardly any social and no financial restrictions in Vienna. The imperial institution was non-profit oriented; until 1922 no entrance fee was charged. Nevertheless an increasing need to cater for the public's taste for entertainment was felt. This sometimes collided with the intentions of popularizers of science and 'educators of the people' trying to teach the spectators about the animal world and to draw moral lessons from it. The source material also allows to distinguish different kinds of audiences. Children, school classes, university students, artists, but also 'educated' and 'uneducated' spectators were addressed and dealt with in different ways. Dealing with the public was also linked with the attempt to discipline its behavior—by no means always successfully. "Do not feed the animals" signs were usually ignored.

Thursday, 11-12:30 PM, LR9
SESSION VII

Making Sense of Interlevel Causation

Organizer and Chair: **Carl F. Craver**

• Explaining (Away) Interlevel Causes I: A Field Guide to Levels,
Carl Craver, Washington U of St. Louis, USA

In many areas of science, it is acceptable or fashionable to posit the existence of interlevel causes. Yet the notion of interlevel causation cannot be clearly articulated or justified without careful attention to the notion of a level. On some understandings of levels (e.g., levels size or theory), there is arguably no reason for skepticism about interlevel causal claims. On other notions of level (notably those involving constituency relations) interlevel causal claims look considerably more suspect. I offer a field guide to different senses of 'level' and their implications for thinking about interlevel causation. I suggest further that the central sense of levels in contemporary biology is one that makes it quite difficult to speak intelligibly of interlevel causation—if, that is, one desires a univocal view of causation.

• Explaining (Away) Interlevel Causes II: Mechanistically Mediated Effects,
Bill Bechtel, U of California, San Diego, USA

Several cases of putative top-down causation will be presented from the following domains of the life sciences—genetics, neuroscience, and evolutionary biology. In each case we will show that either there is a mechanism mediating the interlevel causal relationship or the interlevel causal relationship is illegitimate. Mechanistically mediated interlevel causes can in general be understood as a combination of an etiological causal claim and a constitutive claim.

• Top-Down Causation and Mechanistic Explanation, **Gregory Mikkelson**, McGill U, Canada

Two different lines of evidence indicate that top-down causation (the phenomenon of wholes affecting their parts) is far more prevalent in science than has been previously recognized. First, prominent examples of top-down causation exist, such as natural selection in evolutionary biology, and diversity-stability relations in ecology. Second, an intuitive explication of the 'mechanism' concept entails that any lower-level mechanism for a same-level causal relationship must involve both top-down and bottom-up causation. Therefore, the widespread practice of explaining same-level causal relationships in terms of lower-level mechanisms multiplies the number of top-down causal relationships known to science.

• Realization, Mechanism, and Cause, **Thomas W. Polger**, U of Cincinnati, USA

According to a commonly held doctrine (sometimes called 'realization physicalism'), properties, entities and events at so-called higher levels are realized by properties, entities and events at lower levels. According to the doctrine of mechanism recently defended by MACHAMER, DARDEN, CRAVER, and their collaborators, there are at least sometimes causal interactions among entities and activities at different levels, i.e., those that are realizers and those that are realized. This paper begins to answer the question: What must the realization relation be in order for mechanism to be correct? Rather than defend a particular realization relation, I outline the constraints that any such relation must satisfy in order to support mechanism.

Thursday, 11-12:30 PM, LR10
SESSION VIII

Fifty Years of the Molecular Biology of Behavior

Organizer and Chair: **C.U.M. Smith**

• DNA and the Origins of a Molecular Neuroscience, **C.U.M. Smith**, Aston U, UK

It is noteworthy that after the publication of the WATSON-CRICK paper fifty years ago a number of prominent members of the scientific community responsible for that breakthrough transferred their attention to what seemed to them the next (if not the last) great unsolved problem in biology: the brain. Prominent amongst these pioneers were Francis CRICK himself, Max DELBRÜCK, Gunther STENT, Gerald EDELMAN. Seymour BENZER, Sydney BRENNER and, of course, many of the contributors to the Neuroscience Study Programs edited by F. O. SCHMITT. It is interesting that many of these pioneers were originally physicists and had been influenced by the great schools of quantum physics of the early 20th century. This paper reviews this migration and asks how far it altered the mindset of neurophysiologists and affected the dominating metaphors of neuroscience. How far does the implicit reductionism of the molecular approach do what the pioneers hoped: elucidate the working of the brain?

• Neurodevelopmental Complexities in Worms and Flies: Strategies for Representation and Explanation, **Kenneth F. Schaffner**, George Washington U, USA

Until quite recently it was the scientific consensus that the completion of the Human Genome Project and other model organism genomic sequence projects would yield breakthroughs in the understanding how genes influence both normal and pathological behavior. But over the past few years there has been a growing realization, amid many replication failures, that gene influences on behaviors are vastly more complex than initially expected in the early research programs of BENZER on the fly and BRENNER on the worm. One hope is that microarrays may come to the rescue and provide a new tool to dissect complex gene-behaviour relations. In this talk I review several recent microarray studies from the laboratories of CHALFIE and of GREENSPAN and White involving *C. elegans* and *Drosophila*, perhaps the best organisms for initially approaching gene-behavior relations in the context of organisms' neural systems. I cite both technical problems with neural microarray studies, such as low gene expression ratios and neural cell heterogeneity, as well as more general conceptual problems with existing approaches to representation and explanation in the microarray area. A set of strategies for likely future studies in behaviour genetics, tied to microarrays and other methodologies, is also presented.

• Neurobiology and a Renaissance in Psychoanalysis? **A. E. Manier**, U of Notre Dame, USA

Throughout his career, Nobel Laureate Eric KANDEL has published solid experimental results in the *Proceedings of the National Academy of Sciences* and bold theoretical speculation in the *American Journal of Psychiatry*. In 1998 in the latter journal, KANDEL heralded the possibility of a neurobiological 'renaissance' in psychoanalytic thought. I critically compare the details of this striking claim with recent philosophical discussions of the basic concepts of psychoanalysis.

Thursday, 11-12:30 PM, LR3
SESSION IX

History and Philosophy of Neuroscience

Contributed Papers, Chair, **Jeff Schank**, U of California, Davis, USA

• The Commitments of Computational Neuroscience, **David M. Kaplan**, Duke University, USA

A particularly important distinction for, but not generally heeded by, (practitioners of) computational neuroscience is that between *computational* and *computable*. Roughly, the former applies to what does the computing and the latter to whatever gets computed. An interesting and important question as computational neuroscience emerges as an important subfield within the neurosciences is whether there is any theoretical commitment to the stronger claim that the brain (or nervous system) is computational and not just effectively computable. It is well known that the positions and trajectories of planets comprising our solar system are effectively computable. However, the fact that the behavior of the solar system is governed by some computable function is not generally taken as evidence that the solar system is itself computational—a stronger claim on the nature of a system, to be sure. Similarly, it does not follow from the fact that the behavior of a neural system can be captured by a computational model or some computable function that it is computational. This paper argues that there are in fact mixed computational commitments within computational neuroscience—some work supports only a commitment to the nervous system being computable or capable of being modeled by some computable function, while other work betrays commitment to a much stronger claim that the nervous system is itself a computational system. This paper (1) indicates the relevance of the computational/computable distinction for computational neuroscience, (2) shows how the former side of the distinction makes some rather strong claims about the nature of the system under investigation while the latter side makes substantially weaker claims about the studied system, and (3) suggests reasons for caution on the part of those claiming that nervous systems are themselves computational phenomena.

• Rat Pups and Robots, **Jeff Schank**, U of California, Davis, USA

A new crosscutting research program is described, which combines different modeling approaches (statistical, cellular automata, system-dynamic, and robotic) to explain sensorimotor development in infant rats. We argue that no single type of model is adequate to explain the dynamics of sensorimotor development. Different types of models can address deficiencies in each other. For example, probabilistic cellular automata models are very good at probabilistically describing the dynamics of behavior, but neither explicitly incorporates physical parameters nor guides us to a single interpretation of resulting behavioral probabilities. System dynamic models allow us to explicitly incorporate physical parameters and run fast simulations, but these models run into tractability issues when there are groups of interacting individuals. This is where robotic models can prove invaluable. But, system dynamic and robotic models also run into problems when we attempt to fits rules for their behavior to actual data. Even with genetic algorithms, this can be a difficult problem. Using genetic algorithms to fit probabilistic cellular automata to data is often an easier problem and the resulting cellular automata models provide a bridge to finding good system dynamic and robotic models. The idea of a taxis (orienting response) has had a long history in the study of behavior and is often viewed as a simple 'reflexive' explanation of behavior. For example, wall following and moving to corners by Norway rats in an enclosure is often explained as a thigmotactic response to walls and corners. We found that behaviors often labeled as taxes (e.g., thigmotaxis, geotaxis) require explanations involving the whole organism situated in a structured environment. These explanations change with development. For example, when a rat pup, 7 to 10 days of age is placed in a temperature controlled rectangular arena, it will typically end up with its head oriented towards a corner. Is this a thigmotactic response to corners? Or, is it due to a preference or motivation to seek safety? Our simulation and robotic studies suggest that pups are neither orienting to corners nor exhibiting a preference or motivation to seek corners for safety. Instead, pups get 'stuck' in corners because of their sensorimotor reactions to walls and corners. Thus, at these ages, thigmotactic behavior is best explained by sensorimotor rules and the structure of the environment (the information if affords). Moreover, when we look at the dynamics of group behavior, weak responses to stimuli—e.g., pup movement on the surface of an arena with a 1 degree incline—by an individual are greatly amplified in the context of a group. These amplified group responses can be explained with the help of computer simulation and robotic interactions. Future work will focus on behavior given the same label at different developmental stages (e.g., thigmotactic behaviors) and how these behaviors require different situated explanations as new sensory systems come online (i.e., visual and auditory systems) as well as learning, memory, and motivation.

This research is supported by a grant from NSF to the author.

• Dynamics in Laterality Research, **Ruth A. Byrne**, KLI, Austria

BROCA found that in humans the ability to speak is located in the left frontal lobe of the brain. To explain this he claimed that asymmetry is not inborn but is imposed by education and civilization upon the human mind. In the second half of the 19th century the left side of the brain was considered to be the leading one while the right side was viewed as 'animalistic'. For a long time thereafter brain lateralization was thought to be unique to humans, associated with language and intellect.

Further studies on brain asymmetries of psychiatric patients as well as the beginning of WWI brought this branch of research to a dead end. In the interbellum research on mainly structural and behavioral asymmetries of invertebrates peaked briefly. This was probably distant enough from the topic of human cerebral asymmetries not to scratch on human uniqueness. The attitude towards lateral asymmetries changed dramatically when science became focused on observations of split-brain patients in the late 1960s. The research field had not only moved from Europe to the USA but was also confronted with a completely different way of thinking—by then brain asymmetry was viewed as 'dual lateralized functioning'. In the next decades the amount of publications on lateral asymmetries increased dramatically and was now also extended to nonhuman vertebrates. Work on rodents and birds in particular provided valuable insights into functional and behavioral

brain lateralization. Interestingly, research on nonhuman primates increased later, starting around 1985. Although evidence for lateralization of other vertebrates already existed, scientists explained cerebral asymmetries of nonhuman primates in terms of their close relatedness to humans. This was influenced by the idea that primate lateralization constituted a 'prelinguistic stage' of the human trait, driven by chimpanzee sign language studies. At the turn of the 21st century the idea that lateral asymmetries are a homologous trait in all vertebrates and thus not restricted to humans was finally accepted. Now, after another break of 70 years, new evidence is being found for behavioral asymmetry in an invertebrate, the octopus. How long will it take this time until science acknowledges that lateralization is not unique to vertebrates, but might be a common principle that evolves when neuronal systems must cope with complex sensory input?

• On the Uses of Strychnine: J.G. Dusser de Barenne and Cerebral Localization, 1916-1940,
Tara Abraham, Max Planck Institute for the History of Science, Germany

In 1938, the Nobel Prize winning neurophysiologist Edgar D. ADRIAN observed that up until the present, electrical stimulation of the cerebral cortex had been useful for revealing the 'receiving and executive' apparatus of the cortex but had showed little of the processes that intervene. He noted that a group working with the Dutch physiologist J.G. DUSSER DE BARENNE (1885-1940), at Yale's Laboratory of Neurophysiology, had been making headway in the analysis of these electrical records, through the local application of strychnine to the surface of cerebral cortex. First developed by DUSSER DE BARENNE in 1916, the strychnine method, in contrast to electrical stimulation, was seen to reveal clues about the organization of activity within the cortex. This paper will first focus on the work of DUSSER DE BARENNE and his colleagues, concentrating on his strychnine method and how it differed from other more traditional approaches to the problem of cerebral localization. Secondly, the paper will examine the conceptual dimensions of DUSSER DE BARENNE's work. Influenced by the work of neurophysiologist Charles S. SHERRINGTON, DUSSER DE BARENNE favoured the concept of 'functional organization' over 'localization' when speaking of the functional activity of the cortex. Finally, this paper will situate DUSSER DE BARENNE's method and his ideas in the context of both clinical and laboratory studies of cerebral localization in Interwar America.

Thursday, 11-12:30 PM, LR5
SESSION X
Genetics in History
Contributed Papers, Chair: **Ana Barahona**

• Mendel Finds a Home: Disciplinary Momentum in the American Reception of Mendel, 1900-1910, **Barbara Kimmelman**, Philadelphia U, USA

When MENDEL came to America, his earliest and most enthusiastic welcome was from academic agricultural breeders, employed at state agricultural experiment stations and agricultural colleges. To discover why, I borrow two concepts imported from the history of technology, technology transfer and technological momentum, and develop the notion of *disciplinary momentum*—the tendency of practitioners trained within a particular discipline to actively and creatively continue theoretical work and apply techniques in the direction for which their disciplinary training prepared them. Comparing and contrasting zoological evolutionists, cytologists, botanists, and breeders, I show that the technological momentum of the first three groups carried them powerfully in directions for which MENDEL's work was tangential at best. But agricultural breeders were already familiar with MENDEL's intellectual concerns as well as his specific experimental techniques. Their disciplinary momentum swept MENDEL up and carried him with them, providing MENDEL with an ample home in America and giving early American Mendelism its agricultural character, both intellectually and institutionally.

• The Role Played by Theodosius Dobzhansky in the Emergence and Institutionalization of Genetics in Mexico, **Ana Barahona** and **Francisco Ayala**, UNAM, Mexico

The first time DOBZHANSKY went to Mexico was in 1935. He participated in a trip across the states of Colorado, Arizona, New Mexico, and the countries Mexico and Guatemala, supported by the Rockefeller Foundation. He returned in 1936 and 1938 with the financial support of the Carnegie Institution. The purpose of the trips was to collect several samples of *Drosophila azteca.* During this time, the study of giant chromosomes in the salivary glands of fly larvae allowed cytogenetic and phylogenetic studies on the diversification and speciation mechanisms of these organisms. This work was done with Demetrio SOKOLOFF (1891-1973), who worked at the Escuela Nacional de Ciencias Biológicas (ENCB) at the time. DOBZHANSKY went back to Mexico in 1974, where he started the project "Population Genetics of Mexican *Drosophila*" initiated with the financial support of the National Science Foundation and the CONACyT. Professors Louis LEVINE and Jeffrey POWELL, both former students of DOBZHANSKY, and professors Rodolfo FÉLIX, Olga OLVERA, Judith GUZMÁN, Victor SALCEDA, and Ma. Esther DE LA ROSA were all incorporated in the project. Several visits to the states of Michoacán, Hidalgo, Mexico, and Morelos were organized in order to collect samples. From the analysis of gonadal chromosomes and morphological observation of specimens collected in July 1974 in Cuernavaca, the description of *Drosophila cuauhtemoci* was published in 1976. When DOBZHANSKY died in 1975, professor Wyatt ANDERSON became part of the project research team. Close to twenty articles derived from this project have been published in international journals, and several papers have been presented in specialized meetings.

• From Autarky to Eugenics: The Rex Rabbit as a Mirror of the Healthy Nation: The Emergence of Pathologic Objects and the Beginnings of Comparative Medical Genetics in Germany Around 1930, **Alexander von Schwerin**

German genetics in the 1930s witnessed somewhat of a boom in comparative genetics. Geneticists intended to construct animal (mainly mammalian) experimental systems especially as model systems for human hereditary diseases. German geneticists, and comparative medical geneticists in particular, were deeply concerned with eugenics as the prominent role of mutations in genetics from the late '20s on was inseparable of the alarmed care for the long-term health of the national gene pool. Thus, whereas the commitment to eugenics was not an outcome of Nazi ideology, it is not clear how comparative medical genetics in Germany was invented and how pathologic objects in genetics emerged. From this perspective, several beginnings of comparative medical genetics have been found to be possible.

Here I will concentrate on the research of the geneticist Hans NACHTSHEIM that led from the breeding of rabbits for national agricultural autarky to animal modelling for the purposes of human genetics and eugenic practice. This transformation started in 1929 and was only irreversible after NACHTSHEIM had joined the Kaiser Wilhelm Institute for Anthropology, Human Heredity and Eugenics. In this transformation the representational and scientific practice and the discourses of heredity and health worked together. Former historical perspectives on the breaks and continuities in NACHTSHEIM's work are being questioned by the inclusion of scientific practice.

Thursday, 2-3:30 PM

SESSION I, LR4

Experiments in Experimentalism in the Early 20th Century I

Organizer and Chair: **Sander Gliboff**

• Paul Kammerer and the Vivarium, **Sander Gliboff**, Indiana U, USA

The turn-of-the-century call to experimentalism in the study of development, heredity, and evolution, encouraged the growth of new kinds of institutions, from the seaside laboratories of Naples and Woods Hole to the Fly Room at Columbia. One of the most unconventional new laboratories was in Vienna, at the Institute for Experimental Biology, also known as the Vivarium, founded in 1902, by Hans PRZIBRAM. It was to be devoted strictly to whole-organism experimentation, with state-of-the-art equipment, but not to any particular subject area—physiology and genetics were expressly ruled out—and not to any particular model organism. Paul KAMMERER performed all of his ill-fated experiments at the Vivarium. He became the most visible and vocal proponent of the inheritance of acquired characteristics in the 1910s and '20s. However, his colleagues outside the Vivarium were always critical of his results, and he never seemed able to respond with improved experimental designs. As criticism mounted, a series of scandals steadily undermined his credibility, until he committed suicide in 1926, amid accusations of fraud, and sealed his posthumous reputation. KAMMERER's own indiscretions might have triggered the scandals, but they do not explain his entire rise and fall. Why did his results attract so much international attention from the start? Why was the attention so critical? And why was he unable to satisfy his critics with new experiments? Recent historical studies of other laboratories suggest ways in which to analyze PRZIBRAM's founding vision and the internal culture of the Vivarium, with the aim of tracing the laboratory's influence on KAMMERER's program and explaining some of its peculiarities and its reception outside the Vivarium. It will be argued that KAMMERER lived up to the Vivarium vision and piqued international interest by showcasing the art of the whole-organism experiment and the power of the laboratory's equipment. However, he was not served well by avoiding specialization on an appropriate model organism or by failing to develop expertise in genetics. The Vivarium vision was not fully shared by other laboratories, where KAMMERER's work was considered substandard.

• The Inheritance of Acquired Sexual Diversity: The Steinach-Kammerer Collaboration,
Cheryl Logan, U of North Carolina, Greensboro, USA

The biology of sexuality was transformed after 1900 by a new type of cause—the hypothetical inner secretions that were later termed 'hormones'. PRZIBRAM's Vivarium became an international center for endocrine research when in 1912, Eugen STEINACH was hired to direct the laboratory's Division of Physiology. There STEINACH perfected same- and cross-sex transplants of male and female gonads to show that the development of sexuality in rats and guinea pigs was directed by chemicals that were part of the process of normal development. STEINACH's Vivarium colleague Paul KAMMERER was also interested in sexuality, and in 1920, the two collaborated on a project that combined STEINACH's physiology with KAMMERER's social interests. Their work was entitled 'Klima und Mannbarkeit' ('Climate and Masculinity'). It presented a series of experiments on the impact of high heat on the development of sexuality in albino rats. The research fused STEINACH's reproductive physiology with KAMMERER's emphasis on the inheritance of acquired characteristics, showing that high heat produced hypersexuality, which was then inherited by the descendants of the first generation. STEINACH and KAMMERER went beyond rats, however, as they applied their experiments to sexual diversity in human culture. They compared findings on their 'heat rats' with the anthropological literature on the sexuality of indigenous peoples in the tropics and the arctic to challenge anthropologists who stressed racial explanations of sexuality based in inner, pre-deterministic (*keimplasmatischen*) conditions. I will argue that the work served each man well.

It helped accommodate STEINACH's internalist perspective to the Vivarium's externalist culture, and it provided KAMMERER with a much needed physiological mechanism with which to argue against the prevailing degenerationist approach to human sexual diversity and instead to promote his program of constructive social improvement.

Thursday, 2-3:30 PM, LR8
SESSION II

History of 20th Century Biology

Contributed Papers. Chair: **Nathaniel Comfort**

• Jim Watson's Eugenics, **Nathaniel Comfort**, The George Washington University and the Johns Hopkins Medical Institutions, USA

This year marks the 50th anniversary of James WATSON and Francis CRICK's double-helix model of DNA. WATSON, with his penchant for provocative and often sexist public statements, has long been a favorite whipping-boy for socially conscious historians of biology. Yet with rare exceptions he has been treated more as a caricature than as a serious historical actor. Once we move beyond what is offensive in WATSON, is there anything genuinely troubling? Yes, there is—but it is in most of us. Analysis of WATSON's public writings reveals strong parallels, common themes, and trends when compared with earlier eugenicists such as Francis GALTON and Charles DAVENPORT. A concrete look at the rhetoric of modern human genetics enables us to move beyond hand-wringing warnings about 'new' or 'back-door' eugenics. It makes the very definition of eugenics more complex and more dangerous. WATSON makes a persuasive case that a world of eugenic personal choice is inevitable. Molecular medical eugenics may be upon us, but if so, it will be based on rampant individualism rather than state control. Running through the history of eugenics, from GALTON to WATSON, is a theme of overweening confidence in contemporary science: the belief that now, at long last, we understand enough about human heredity to take it into our own hands.

• Wright's Shifting Balances, **Jonathan Hodge**, U of Leeds, UK

WRIGHT's shifting balance theory of evolutionary change did have origins in his analyses of animal breeding strategies. But, it also had other origins in traditions of thought concerned, quite generally, with tendencies to homogeneity in nature versus tendencies to heterogeneity, and with equilibria versus progressive change. Before he called it a theory of shifting balance, he identified it as a theory of moving equilibrium. This concept of moving equilibrium had already had an influential and instructive history in such authors as A. J. LOTKA and Herbert SPENCER. When WRIGHT's theory is understood as he understood it—as a theory of moving equilibrium in the forces making for homogeneity and heterogeneity—one can see that his original rationale for small, local subpopulations was that they ensured a role for inbreeding, rather than ensuring a role for drift. What is more, by giving inbreeding a decisive role, WRIGHT could satisfy what he took to be the demands for making any process of change intelligible, by understanding both its 'conservative' and its innovative ('radical') tendencies.

• Emile Zuckerkandl: His Early Life and the Molecular Clock Hypothesis,
Tomoko Steen, Library of Congress, USA

In 1964, Emile ZUCKERKANDL discovered a new pattern of evolution at the molecular level, and named it 'molecular clock'. His discovery is, in short, the mutants accumulate in the same rate when a same macromolecule is compared among various species. Thus, using this hypothesis, one can identify the divergence time by simply comparing the differences in the number of mutations accumulated in ach species. Since the discovery of DNA structure in 1953, eyes of biological scientists have shifted down to the molecular level. The molecular clock hypothesis was presented in this perfect setting. ZUCKERKANDL was a postdoctoral fellow under Linus PAULING back then, but

this discovery eventually made him a distinguished scientist in California. However, his early life was not so easy. He was born in Vienna (Austria) in a wealthy Jewish family; however, the raise of Nazi Germany brought great difficulties to their lives. My talk will be on his early life and discovery of the molecular clock.

Thursday, 2-3:30 PM, LR1
SESSION III

A Cultural History of Heredity I

Organizers: **Staffan Müller-Wille** and **Hans-Jörg Rheinberger**

• Elements of Hybridism, 1759-1875,
Staffan Müller-Wille, Max Planck Institute for the History of Science, Germany

In my paper I will try to analyse, how the complex and inherently contradictory notion of 'constant varieties' was fleshed out in the experimental tradition that followed upon LINNAEUS's famous hybridisation experiment of 1759 and led up to chapter XV of DARWIN's *Variation under Domestication* (1875). The researchers involved in this tradition had very different agendas: for KÖLREUTER species 'transmutation' was at stake, HERBERT's interests were taxonomic, GÄRTNER tried to find proofs for plant sexuality, WICHURA focussed on the evolutionary significance of hybridisation, and NAUDIN, finally, gave priority to questions of heredity. Yet it is possible to identify a common pattern in this experimental tradition consisting in what might be called the 'atomisation' of species: varieties were combined and produced through these experiments in a way that shifted the level of analysis from species to differential characters as the 'elements' of the species. This shift was reflected in the proliferation of categories, nomenclatures and symbol systems that were used to describe the varieties of varietal behaviour—constant vs. variable, atavistic vs. progressive, etc.—within species. Heredity emerged as a central concept in this conceptual and symbolic space to account for patterns of variation that recurred independently of environmental conditions and yet were intraspecific.

• Heredity and Adaptation in Kant,
Peter McLaughlin, Max Planck Institute for the History of Science, Germany

The consideration of heredity in the latter 18th century seems to have focused primarily on the transmission of defects, especially hereditary disease. Immanuel KANT, too, uses the concept 'Vererbung' in this context: he analyses the discussion of the transmission of defects as carried on in the three 'higher' university faculties (law, medicine, and theology) with their notions of 'Erbschuld', 'Erbkrankheit', and 'Erbsünde' (hereditary debt, hereditary disease, and original sin). But he also deals with the concept of 'Vererbung' in biology, where it was used in an area with decidedly positive connotations: adaptation. My presentation will deal with the relation of heredity to adaptation in the biological writings of KANT. Individual adaptations—as opposed to the appropriateness of the species form for its place in nature—did not constitute a central theme in 18th-century biological thought. And even for LAMARCK they are of only secondary interest. KANT's position may be an important point of departure for the study of adaptive heredity and for the quite different relation of 19th-century thought to the problem of adaptation.

• Contagion and Heredity as Causes of Diseases in Early 19th-Century France,
Laure Cartron, U of Paris I - Sorbonne, France

To study the concepts of heredity and the confusion of theories explaining this 'obscure question' requires nothing less than pinpointing why such discourse was opportune at the time and finding links between its discursive quality and the problems posed by the social environment. In this communication, I shall try to make clear in which social or political context these medical perspectives—heredity and contagion—had been favoured. With a little oversimplification one may say

that up until around 1780, there existed two fundamentally contradictory views of the causes of disease: contagion and heredity. The notion of contagion was very ancient. As early as the 16th century, Girolamo FRACASTORO (1478-1543), a Verona physician and astrologer, had developed the idea of 'contagium vivum'. Later on, Jean-Baptiste BIANCHI in 1741 and Marcus Antoniuz PLENCIZ in 1762 contended that diseases like plague, leprosy, phthisis, syphilis, and smallpox had to do with "diversi generis insecta" entering the organism and growing in number.

At the end of the 18th century, it was important to the French State to limit obstacles to trade from quarantines. This same kind of reasoning—to ease the minds of troops destined for the Egyptian campaign—prompted BONAPARTE to ask the Institut to solemnly declare that plague was a nontransmittable disease. Political reasoning had to gain from scientific discourse. The accent put on anti-contagionism and the refusal to consider contagion as a cause of illness led to the search for another etiological explanation, namely heredity.

Antoine PORTAL (1742-1832) provides a typical example of the consequent 'rush job' to use empirical data and some philosophical and metaphysical presuppositions to elaborate a theory on the recurrence of illnesses by heredity. During his career he produced forty widely diffused treatises on various diseases. Contagion being replaced by the notion of 'family diseases', he advocated the heredity of diseases in most of his works. PORTAL's system revolved around two simple principles. First, he remained true to the traditional Galenic conception that all disease was the effect of humoural imbalance. In the case of hereditary diseases, the lymph was involved. When the parents' lymph was poisoned, they transmitted a preexisting germ, a 'scrofulous virus' to the fetus. Second, PORTAL wanted to establish a theory of hereditary disease that allowed him to explain the facts observed in his experience as an anatomist. He considered his experience a unique and revealing proof: having practised so many autopsies without catching any chronic disease he concluded that chronic diseases were a matter of heredity rather than of contagion.

• French and British Conceptions of Hereditary Transmission in the 19th Century: The Conflicts Between Galton and Ribot, and Galton and de Candolle, **Carlos Lopez Beltran**, UNAM, Mexico

I will begin by reviewing some of the main ways in which the issue of hereditary transmission of physical and mental characteristics was understood by several important authors in Britain (HUNTER, PRICHARD, LAWRENCE, SPENCER) and France (PETIT, PIORRY, VIREY, FLOURENS, LUCAS). I will argue that different conceptual delimitations and projects produced divergent conceptions of heredity that evolved relatively independently from one another. Whereas the French *savants* conceived of heredity as a powerful overarching natural vital, force that constituted one of the main threads of the history of life, the British thinkers preferred a more restrained approach in which the theorising was typically delayed in the face of conflicting evidence. After the 1860s, as heredity was being brought to the front as a central biological problem, the divergence became progressively unavoidable, as metaphorical language could not be so easily used as a cushion to avoid confrontation. Francis GALTON reacted very sharply to the writings of the French psychologist Théodule RIBOT and the Swiss botanist Alphonse DE CANDOLLE. By analysing his reactions and the polemics that ensued I will try to flesh out the roots of the divergences and the attempts to bridge them.

Thursday, 2-3:30 PM, LR2
SESSION IV

Picturing Eggs, Embryos, and Cells I: Eggs and Embryos in Situ

Organizer: **Sabine Brauckmann**, Chair: **Denis Thieffry**

• Introduction

• The Unpublished Drawings of Karl Ernst von Baer, **Erki Tammiksaar**, Baer Museum, Tartu

In 1822, when BAER was promoted to a full professorship at the University of Königsberg, he established a Museum of Zoology and intensified his comparative embryological studies that finally led to the discovery of the mammalian and human egg. The findings were published in his seminal work on the *Entwickelungsgeschichte der Thiere* (1828), which illustrated the *Bildung* of embryonic eggs in three schematic tables. BAER did include mere drawings for two reasons. First, as he wrote in the introduction, he preferred the pictorial lucidness to the accuracy of microscopic observation. The other reason was that he himself lacked the professional training of an engraver. In our paper we will present new findings of unpublished drawings by BAER and will discuss his difficulties to find an experienced illustrator who could realiter reproduce the embryonic image BAER had observed in and with the microscope. In addition, we will argue for the importance of the epigenetic typus that envisaged BAER's ideas of development, and for the visual awareness in microscopic experimenting of the 19th century.

• What Made Haeckel's Drawings of Embryos Controversial? **Nick Hopwood**, Cambridge U, UK

The pictures of embryos in Ernst HAECKEL's semi-popular books are the most notorious images in the history of biology. Claims that 'the German DARWIN' forged drawings to exaggerate the similarity of early vertebrate embryos were first made in 1868, hotly debated during the 1870s and again around 1908, and revived in the late 1990s. Since then accusations of fraud have been much bandied about. This talk argues that to grasp the general significance of HAECKEL's pictures we need to place them in the history of communicating scientific images, specifically those of embryonic development. It then takes a close look at the pictures' initial publication and early reception. This makes it possible to assess their novelty and to identify the processes through which they were made controversial.

• The Political Embryo: Representing the Human Embryo at 6-7 Weeks,
Scott F. Gilbert, Swarthmore College, USA

The human embryo has become a potent political symbol. Depending on how it is represented, it can represent an independent life form, a human being, a parasite, and a not-so-human animal. Several changes occurring during embryonic weeks 6 and 7 cause the embryonic form to take a recognizably human shape. The yes come forward in the head, and the elbows and fingers become recognizable. The depiction of the embryo is affects and is affected by political debates and also by the conventions of modern science textbooks. Drawing has become much more cartoon-like, and gene expression boundaries are often more important than the anatomical structure. Our initial observations suggest an abstraction both of structure (from gross anatomical entity to gene expression profiles) and of artwork (from detailed stippling showing physical contours and to line drawings showing little or no physical details). The matter of context is also important, and I hope to be able to discuss those illustrations depicting the embryo within its uterine context versus those who (again, an abstraction) separate the fetus from its maternal milieu.

Thursday, 2-3:30 PM, LR5
SESSION V

Complexity: What is it Good For? I

Organizer and Chair: **Lien Van Speybroeck**

• Complexity Science to Understand Living Organisms: Kant and Beyond,
Lien Van Speybroeck and **Gertrudis Van de Vijver**, Ghent U, Belgium

Today, witnessed by the enormous amount of publications in areas as diverse as philosophy, biology, cognitive psychology, sociology, and management studies, it seems to have become a general truism that (living) entities are in fact *complex dynamical systems* that develop and evolve in relation to complex environments. Although this approach is depicted as a revolutionary alternative to reductionist, centrist, and instructionist views, it is not always made explicit where exactly its true revolutionary aspects lie: Does it make living organisms more real (ontological level)?, Does it make us understand better the intrinsic nature of living organisms?, and Does it offer more illucidating ways to model them (epistemological level)?; Does it guide us to develop new tools and research programmes, or more modestly, to ask different questions about living organisms, and are these questions within reach of being answered (pragmatical level)? One thing is sure: next to philosophy of biology, the complexity terminology is invading biology itself, which seems to make talking about *self-organization* and *systems biology* as easy as talking about DNA and proteins. Other concepts, such as *organisational cohesion*, *boundary*, *closure*, *process* and *structural hierarchy*, *emergence*, *downward causation*, *constraint*, etc., are equally high-scoring and heavily debated in philosophy of biology. However, what is the exact impact of this complexity discourse? And in what sense are the above concepts and their underlying ideas of use to the biologist or to the philosopher? How do they shed light on what orthodox views blackbox? Or is the level of abstraction too high, running the risk of capturing nothing but what is intuitively already known? This paper investigates how the debate developed with regard to KANT's view on living organisms.

• What is Life? Is Complexity the Answer? **Michel Morange**, École Normale Supérieure, France

In the 1960s, the question 'What is Life?' disappeared from the discourse of biologists. It was considered to have been answered: the riddle of Life was finding its solution in the existence of a genetic information and of a genetic program. Today, the question 'What is Life?' is no longer taboo, as it was during the last two decades: it is raised again by biologists, and its answer is now looked for in the burgeoning theories and models of complexity. My aim will be to explain this evolution by the transformations which occurred in biology during the last decades, as well as by events external to biology, such as the development of astrobiology. I will compare the answers referring to complexity to the other answers provided simultaneously by the same or other biologists.

• Irreducible Complexity in Biology, **John Collier**, U of Natal, South Africa

Many of the terms used in complexity theory are equivocal or vague, leading to much confusion about the need for special methods to deal with biological complexity. Many complex systems can be approached through approximate linear methods and/or by numerical modelling. One issue of interest is whether there are biological systems or properties that require alternative approaches, and thus differ in kind from systems that can be so approximated, or whether they just differ in degree of complexity to the extent that traditional methods from physics are just inconvenient to apply. After introducing a suitable taxonomy of complexity, I will argue that there is a class of systems that I call *complexly organized*, which cannot be fully analyzed using numerical methods, even approximately. These systems appear in physics, where they are fairly well understood. I will describe the conditions for such systems, then I will argue that at organisms and at least some of their properties are of this sort. Finally I will argue that such properties are coextensive with the set of system properties that result from spontaneous self-organization. Elsewhere, I have argued that

this is the set of strongly emergent properties. Hence, *modulo* grammatical niceties, organized complexity corresponds to spontaneously organized, corresponds to strongly emergent.

Thursday, 2-3:30 PM, LR6
SESSION VI

Konrad Lorenz and Company

Organizer and Chair: **Richard W. Burkhardt, Jr.**

• Konrad Lorenz as a Student of Karl Bühler, **Veronika Hofer**, U of Vienna, Austria

This paper focuses on a hitherto unknown aspect of LORENZ's intellectual biography. It investigates the history and the systematic consequences of the fact that LORENZ received all of his academic education in psychology under Karl BÜHLER, at the time (1928) when he was about to begin a second career in animal psychology. Under the influence of BÜHLER and his assistant Egon BRUNSWIK, the 25-year old LORENZ was first 'imprinted' with psychological theories and method. This provided him with his orientation within the vast and multiparadigmatic field of psychology in his day. I will demonstrate the ways in which the biologically-oriented and communication-based psychology of BÜHLER and BRUNSWIK provided the particular conceptual framework that served as LORENZ ' starting point in comparative psychology.

I will provide an outline of the core themes of the BÜHLER-BRUNSWIK school of Viennese psychology in order to analyze the similarities in the psychological concepts of BÜHLER, BRUNSWIK, and LORENZ in more detail. The history of their personal relationship is almost as compelling as the insights in the transformations of the respective theoretical and methodological conceptions. Utilizing new archival materials, we can identify a previously unknown part of LORENZ' complex personality.

• "Howling with the Wolf Pack": Konrad Lorenz and National Socialism Revisited,
Benedikt Föger and **Klaus Taschwer**, U of Vienna, Austria

The 'brown past' of Konrad LORENZ has been discussed for exactly 50 years now. After Daniel LEHRMAN's first famous critique in 1953, and especially after Theodora W. KALIKOW's papers in the late 1970s, LORENZ' entanglement with National Socialism was thematized in quite a few papers and books both by biologists and historians of biology. Still, many questions remain heavily disputed: What were the reasons for LORENZ to become a member of the NSDAP? What motivated him to publish papers in which he was "howling with the wolf pack," as he put it in 1950? Did LORENZ's biopolitical hypotheses and propositions simply support the mainstream of Nazi ideology or did they also differ in some relevant points? What did LORENZ know about the crimes and cruelties of the regime? What happened in Posen (Poznan), where LORENZ first worked as a psychologist for the army and later on as a psychiatrist? In our paper we try to answer these questions on the basis of documents that were known already but also on the basis of recently found material.

• Konrad Lorenz and Niko Tinbergen: Complementarities and Conflicts Between the Co-founders of Ethology, **Richard W. Burkhardt, Jr.**, U of Illinois, Urbana-Champaign, USA

LORENZ and TINBERGEN are properly regarded as the co-founders of ethology. The present paper explores the long-term relations between LORENZ and TINBERGEN, paying particular attention to the factors that established and maintained their friendship and collaboration and also to the various differences in practice, personality, politics, and understandings of animal behavior that distinguished the two of them and led to some frictions between them.

While LORENZ took the lead when it came to laying the field's initial conceptual foundations, TINBERGEN's experimental and analytical talents were an invaluable complement to LORENZ's theory-building. And while LORENZ championed the insights that could be obtained by studying

the behavior patterns of birds that he reared by hand, TINBERGEN always preferred to be a field naturalist studying creatures in the wild. Where LORENZ was inclined to emphasize the comparative study of instinctive behavior patterns along the traditional lines of the comparative morphologist, TINBERGEN went on to develop an understanding of the function of behavior patterns in specific ecological contexts. The firm friendship LORENZ and TINBERGEN established by1937 was severely strained by the WWII. TINBERGEN's decision to put aside deeply-felt wartime grievances for the sake of ethology's postwar recovery was of real importance for the field's subsequent development.

Thursday, 2-3:30 PM, LR10
SESSION VII

Genetics and Policy-making

Organizer: **Heather Douglas**, Chair: **John S. Emrich**

• From Recombinant DNA to Stem Cells: Genetics, Cultural Authority, and Public Policy,
John S. Emrich, George Washington U, USA

Scientists have derived a great deal of their cultural authority from the promise that their new discoveries and techniques will provide a better life, whether it be through cheaper electricity, greater domestic protection, or through a longer and healthier life. It is through the creation and sustaining of cultural authority that public policy is created to promote science and assuage public fears. In the 1970s, the creation of public policy began to change as the public began to question the authority of all of the branches of science, from Rachel CARSON's *Silent Spring* to protests over nuclear power plants to the controversy concerning recombinant DNA. Unlike nuclear physics, which boosts the support and strictly regulated protection of the USA government, genetics does not have the prestige of the federal government in creating public policy. Since the late 1970s, public policy concerning genetics has largely reflected the cultural authority of geneticists in the USA It is therefore important to understand how geneticists have created and lost their cultural authority and how their relationship with the public has impacted the creation of public policy in the past and today.

When recombinant DNA was first created in 1971 it held the promise to cure that disease as well as many other genetic maladies. The initial excitement over the new technology soon gave way to concerns from other geneticists that the technology put biologists in a 'pre-Hiroshima' situation In order to guarantee the safe use of the technology and avoid strict government regulations, the geneticists created their own policy that was quickly approved by the federal government lack of public involvement. Though they were able to create their own public policy, the cultural authority of genetics was damaged. Throughout the late 1970s and throughout the 1980s, the rise of bioethics, the greater involvement of the pubic in policy decisions, and the unfulfilled medical promises of recombinant DNA caused the authority of genetics to ebb.

The launch of the Human Genome Project (HGP) in 1990 gave a lift to the cultural authority of genetics as the project promised therapies and cures for genetic diseases. The HGP was not devoid of a public voice as the Ethical, Legal, and Social Issues branch was included. Though it is still to early to tell if the HGP will succeed in it promises, genetics once again finds itself in the midst of a potentially restrictive public policy. Recently stem cell and cloning, both therapeutic and reproductive, have posed a new threat on genetics' cultural authority. This threat is given a public voice by President BUSH and his belief that the "fundamental value and sanctity of human life" outweighs his "desire to promote vital medical research." It is within the context of genetic cultural authority and how it has changed that we might be able to understand how and why two new promising techniques might be severely restricted by public policy in the USA

• Human Molecular Genetics: Two Different Approaches,
Adelaida Ambrogi, Universitat de les Illes Balears, Spain

In the first part of this paper I will focus on KITCHER's (1996) book. I will stress the importance of this book, not only because of the scientific field he has chosen as the object of his analysis, but also because of the kind of issues he identifies as key issues for that analysis. Yet my aim is not to review the book, but rather to use it as a starting point for exploring some more general issues concerning the philosophy of biology. KITCHER's book is not an epistemological book. It is about the ethical and social issues raised by the advances in human molecular genetics. Yet, my general aim will be to show to what extent the belief in the supposed sharp distinction between epistemological and ethical issues frame the philosophical analysis of science. Firstly, I will show that in this book the author's epistemological assumptions condition the ethical-social content of the book. Moreover, I will show that this fact has significant consequences. As an example, I will focus on the key proposal of the book: utopian eugenics. From my overall analysis of the book, I will infer two conclusions: a specific one, about utopian eugenics itself, and another more general conclusion. I will try to show that a significant consequence of assuming that sharp distinction as starting point of the analysis, is the distortion of both, the task of identification of key issues, and the choosing of the proper approach to study the nature of such issues. My next step will be to provide a view of the place and nature of philosophical analysis regarding the key scientific field of human molecular genetics. This view will be taken from major professional journals on philosophy of biology. I will not present, of course, a systematic search, but rather a significant sampling of it. The aim will be to look for the weight KITCHER's assumptions have in the philosophical community. I will end the paper by offering a proposal of my own. In my proposal I start by considering a particular field of research in human molecular genetics, i.e., the genetics of breast cancer. I will try to show to what extent and under what conditions an alternative starting point to the one assumed by KITCHER will result in a different agenda of key issues in human molecular genetics; an agenda, moreover, that will require different kind of answers to the questions raised by these issues.

Thursday, 2-3:30 PM, LR9
SESSION VIII

The Extended Phenotype Revisited I

Organizers: **Kim Sterelny** and **Matteo Mameli**, Chair: **Matteo Mameli**

• The Implications of Niche Construction for Evolution,
Kevin N. Laland, U of St. Andrews, Scotland

The seemingly innocent observation that the activities of organisms bring about changes in environments is so obvious that it seems an unlikely focus for a new line of thinking about evolution. Yet niche construction, as this process of organism-driven environmental modification is known, has hidden complexities. By transforming biotic and abiotic sources of natural selection in external environments, niche construction generates feedback in evolution, on a scale hitherto underestimated, and in a manner that transforms the evolutionary dynamic. While *The Extended Phenotype* can be regarded as a logical and pragmatic means of tackling niche construction within the framework of standard evolutionary theory, this framework leaves the generality and significance of niche construction under-appreciated. Standard evolutionary theory affords niche construction the sole status of product of evolution, but short-circuits its fundamental role in the evolutionary process. A complete understanding of the impact of niche construction in evolution requires an Extended Evolutionary Theory (ODLING-SMEE *et al*., 20f03), which recognizes that evolution depends on two general selective processes, not one; natural selection and niche construction. We support this argument with empirical data, theoretical population genetics and conceptual models, and describe new research methods that could be employed to put it to the test.

• The Implications of Niche Construction for Ecology, **John Odling-Smee**, U of Oxford, UK

By expressing extended phenotypes, and by modifying natural selection pressures, organisms generate feedback in evolution. This process of 'niche construction' changes the evolutionary dynamic. It also brings forward the synthesis of the disciplines of ecology and evolution, for instance, by making it possible to incorporate abiotic ecosystem components more fully in evolutionary theory. This paper will discuss this potential synthesis. It will also demonstrate how niche construction supports the ecological process of 'ecosystem engineering', and how by doing so, it contributes to the regulation of energy fluxes and nutrient flows through ecosystems.

• Extended Genotypes, Extended Phenotypes and the Nature of Inheritance,
Matteo Mameli, London School of Economics, UK

Parents cause their offspring's existence. Moreover, parents cause their offspring to acquire many features similar to those the parents have. These two kinds of causal influence that parents exert on their offspring are logically distinct. Inheritance refers to the second kind of causal influence. On our planet (but not in other possible worlds), parents can cause their offspring to acquire features similar to the parental ones in two ways: (1) by means of genetic transmission, i.e., by providing the offspring with copies of the parental genes; and (2) by means of some kinds of (non-reproductive) parental behaviours, i.e., by performing behaviours that (in some way or another) affect the phenotypes of the offspring and make these phenotypes similar to those of the parents. Processes of the first kind can be called 'the *extended-genotype* route to inheritance'. Processes of the second kind can be called 'the *extended-phenotype* route to inheritance'. This paper explores some of the features of the extended phenotype route to inheritance and the ways this route compares to and interacts with the extended-genotype route to inheritance.

Thursday, 2-3:30 PM, LR7
SESSION IX

Biological Modeling I

Contributed Papers, Chair: **Michael Weisberg**

• Tradeoffs in Biological Model Building, **Michael Weisberg**, U of Pennsylvania, USA

In this paper I consider a set of tradeoffs which confront biological model builders. Any discussion of the tradeoffs confronting the model builder owes a large debt to Richard LEVINS' pioneering discussions of these issues. But rather than focus on LEVINS' work, I will discuss a series of two-way relationships between generality and other properties of models. Although my primary goal in this paper is to defend the existence of these tradeoffs and other relationships, I will also discuss how they bare on discussions of model-based scientific explanation in population biology.

• Understanding the Usefulness of Biological Models,
Sabina Leonelli, Vrije Universiteit Amsterdam, The Netherlands

In her recent *Making Sense of Life* (2002), Evelyn Fox KELLER argues that models used in the life sciences posses properties that distinguish them from the ones used in other sciences. Biologists strongly prefer models that are immediately useful in an actual experimental setting. Also, successful models in experimental biology tend to be 'whole organisms' rather than abstract schematisations exemplifying a few isolated properties of a physical entity or process. Especially since the demise of the 'master molecule' view in genetics, models are used to acquire an understanding of the dynamic interconnections among individual components which characterise life processes. KELLER's pragmatic and holistic concerns are reflected in her claim that the explanatory utility of the model is fundamental to its selection, its role in experiments and its interpretation.

This paper develops KELLER's defence of the uniqueness of biological models by focusing on her definition of explanatory utility. I argue with her that this notion should be explicitly related to the notion of scientific understanding: hence, I maintain, a discussion of the philosophical significance and meaning of this latter notion should not be limited to KELLER's suggestive reflections. In order to clarify her account, I propose a distinction between two types of scientific understanding, that is between the understanding provided by a model in the context of discovery and the use that biologists make of such understanding in explanation and justification. The former, or creative understanding, refers to the individual critical engagement in scientific research that leads to unexpected theoretical insights. The latter is defined as communicative understanding, i.e., the social sharing of the insight acquired at the individual level. Given this distinction, I suggest that the usefulness and the explanatory power of a model in the life sciences should be evaluated in light of the type of understanding provided by the model itself. I shall support my claim by reference to the history of cell biology—particularly to the way in which different models of the cell membrane (since its 'discovery' in the 17th century) have enabled and shaped our current understanding of cell physiology and intercellular connectivity.

• On Middle-Range Theories, **Pablo Lorenzano**, Universidad Nacional de Quilmes, Argentina

SCHAFFNER points out that most of the debate in philosophy of biology about the structure of theories has taken place in isolation from major changes in the general philosophy of science and has tended to be concentrated in the area of evolutionary biology, neglecting other areas, in particular those which he argues are more typical in the biomedical sciences and possesses 'theories of the middle range'. These theories—which lacks of universality and are 'interlevel' and more 'reticulated' and less 'linear' than those of physics—are better characterized as (overlapping) collections of models. In accordance to that, SUPPE holds that the unmodified Semantic Conception, which has been utilized by BEATTY, LLOYD, and THOMPSON to analyze evolutionary theory, can accommodate and provide philosophical understanding of these interlevel middle range theories. On the other hand, SCHAFFNER considers that a modified version of the Semantic Conception could give a better account of the characteristic features of these theories. In this communication, I will try to show that another semantic or model-theoretic approach, namely, the Structuralist View of Theories, can do this job without need for modification or loss.

• The Evolution of Germ and Soma: An Evaluation of the Models,
Grant Yamashita, U of California, Davis, USA

The transition from unicellularity to multicellularity was a major transition in the history of life. The differentiation of cells into two distinct types, germ and soma, followed this transition. Germ cells, on a strict definition, give rise to individuals in the next generation while somatic cells do not. Over the last two decades, models by BUSS, MICHOD, MAYNARD-SMITH; SZATHMÁRY, BELL and KOUFOPANOU; and others have been proposed that attempt to understand the evolution of this distinction. In this paper I evaluate the models and discuss the importance of a consistent and robust conception of germ cells to these models. I outline the prospect for a coherent, robust account of the evolution of this key character of multicellularity.

Thursday, 2-3:30 PM, LR3
SESSION X

Ecology Transformed

Organizer and Chair: **Ed Hackett**

• Human Dimensions of Ecology, **Jim Collins**, Arizona State U, USA

Integrating humans into basic ecological research questions is a pressing issue for scientists and policy makers that would benefit from historical and sociological analysis. Humans are an integral

part of Earth's ecosystems, yet ecologists conducting basic research often place humans external to the biological system. As a result the science of ecology largely lacks a theoretical and conceptual framework for incorporating humans. Where exactly do humans fit into modern ecological theory? What do we know from the past that can inform such a discussion? How have assumptions and conclusions regarding the need to integrate humans changed over time? From a historical and philosophical view, how did ecology get to this point? What political, social, and institutional factors shaped ecology's development? Did these factors differ in Europe and America? 'Nature' is where most ecologists see their research focused, but what is 'natural' and what is 'not nature?' And what does that mean— specifically, do ecologists really want to say that no humans in an ecosystem is 'natural' and humans in an ecosystem is 'not natural?' The latter view pervades much of the research reported in ecology's leading journals, but does that mean humans are an organism out of bounds for the best theoretical and conceptual research in ecology? And if not, how do we include humans—and how have scientists done this in different ways over time? An increasing number of scholars are arguing that incorporating humans into basic ecology requires research that integrates the social and natural sciences, and some ecologists and social scientists are struggling with how to do this. Studies in the history, philosophy, and sociology of ecology will advance our understanding of the complex theoretical and practical challenges associated with integrating humans into basic ecological research.

• Where Rivers Fail to Fulfill Their 'Natural Functions', **Susie Fisher**, The Open U of Israel

Today we naturally assume that it is our right to expect a river running through our city to be pleasant looking and odorless. The inhabitants of London during the 19th century did not enjoy such a privilege. In fact, towards mid century they became involved in a heated polemic regarding the state of river Thames. FARADAY wrote in a letter to the *Times*: "The smell was very bad, and common to the whole of the water; it was the same as that which now comes up from the gully-holes in the streets; the whole river was for the time a real sewer… I have thought it a duty to record these facts, that they may be brought to the attention of those who exercise power or have responsibility in relation to the condition of our river; …. If there be sufficient authority to remove a putrescent pond from the neighbourhood of a few simple dwellings, surely the river which flows for so many miles through London ought not to be allowed to become a fermenting sewer." So the river was foul looking and ill smelling, but should the people of London be truly concerned about it? What was the real nature of the problem, they wondered, was it merely an aesthetic environmental problem, or did the river present a health hazard? Did the river's look mean that the water was indeed unsafe for drinking? These were not the only questions that had to answered, there were others. How can one determine the water's safety? And accordingly, how should the river be treated? And whose responsibility was it to fix the river? Four interested parties had a say in the debate which developed during the 1850s: the general public, chemists, microscopists and epidemiologists. Each party found something different in the water whose relevance to the debate had to be proven. With so many interested parties involved one could perhaps have hoped that the issue would be resolved on the basis of scientific evidence. In this case chemists represented a maturing science whose status was improving daily and their warrant for action was sought after. But could scientists actually provide such a warrant? It is quite clear from FARADAY's letter and the questions raised by the various interested parties that the problem did not pertain to science only, obviously social and political factors would have to be considered. In my paper I will focus on the part that William ODLING (1829-1921) played in this water debate. ODLING was a well-known chemist, a certified physician, and an enthusiastic public health officer. As we shall see, this dictated diverse and often conflicting decision making in matters concerning environmental issues.

• Ecology Transformed? NCEAS and the Changing Pattern of Ecological Research, **Ed Hackett, John Parker, David Conz, and Jonathon Bashford,** Arizona State University, USA

"Because you know something is happening here but you don't know what it is, do you, Mr. Jones?" Bob Dylan, Ballad of a Thin Man, 1966

Since 1990 observers of science have detected a transformation in the social arrangements of scientific research. For some this is the advent of mode 2 knowledge production, the coming of 'post-normal science' or 'postacademic science', or the negotiation of a new social contract for science. Others sense a new form of economic activity, 'academic capitalism', a new institutional entwining into a triple helix or a new organizational form and career pattern. And still others are skeptical that anything new at all is happening.□

As summarized by WEINGART, this new mode of knowledge production has many of the following characteristics:□

- Knowledge is produced in a variety of new contexts that may be outside the university, ephemeral, or virtual (such as research networks and collaboratories).
- Knowledge is produced with an eye to its potential users and usefulness, not solely to increase the store of fundamental understanding of nature.
- Research is transdisciplinary in its conduct and its transmission, and is often embodied in the researchers rather than conveyed through the traditional pathways of publication.
- The quality of research is evaluated with more than the usual criteria of peer review in mind, and the evaluation is conducted by a heterogeneous collection of reviewers. More specifically, economic, political, and social considerations accompany scientific and intellectual criteria in guiding review.□
- Knowledge production must justify itself in social and political terms; knowledge is no longer an end in itself, or a means to an end that may only later be discovered. Researchers are expected to cite public benefits to justify their claim on resources.□

The aim of this paper is to use an extended case study to explore facets of the new production of scientific knowledge. The case study concerns the National Center for Ecological Analysis and Synthesis (NCEAS), in Santa Barbara, California. NCEAS originated in 1996 with a grant from the National Science Foundation (NSF) and additional support from the State of California; it was recently awarded a further six years of support. NCEAS was established with the expectation that ecology would change in several fundamental ways:□

The process of ecological research would become more collaborative, would involve more disciplines, and would engage issues of policy, resource management and practice. The scale of ecological analysis would increase from disjointed 30-meter plots studied experimentally to integrative analyses that drew inferences about larger areas and broader processes (in space and time) by pooling data across plots. The method of ecology would change to embrace more synthetic thinking and analysis, more frequent use of archival data, more quantitative tools of analysis and modeling. The research technology of ecology would change to include shared data resources (stored at NCEAS), new quantitative tools, and new methods of using the data (including knowledge networks to promote remote, asynchronous, computer-mediated collaboration). The knowledge and theory of ecology would change, reflected both in the characteristics of specific publications and in the overarching knowledge about ecology that would build upon publications.□

We have conducted fieldwork at NCEAS at various times over the past three years, observing working groups; interviewing NCEAS administrators, postdocs, and working group members; administering a brief questionnaire; examining documents, publications, and citation data. Using this material we discuss how and with what effects NCEAS is changing the conduct, content, and context of ecological research.

• Computational Models in Biology, **Steven L. Peck**, Brigham Young U, USA

For the last 50 years analytical models have dominated theoretical ecology. Largely because there are standard methods used to test and understand the dynamics of these models—largely based on dynamical systems theory. However, due to the recent increases in computational power, simulation models have begun to play an important role in understanding complex ecological systems. Analytical models are generally limited in the number of variables that they can handle. Models that incorporate both spatial and genetic components are severely limited using standard methods of modeling. Computational models can overcome this problem but at the cost of higher complexity, which can sometimes reach the levels of complexity found in biological systems. Because

there is no standard theory on how to analyze computational models, they are often held in suspicion as being nonsense by theorists who use standard modeling methods. How do we develop a theory of using, testing, and verifying computational models? Are there standard methods that can be developed to make these models as robust and useful and analytic models? This paper will explore these questions with suggestions on how to ground computational models in a stronger philosophical framework.

Thursday, 4-5:30 PM

SESSION I, LR8

Experiments in Experimentalism in the Early 20th Century II

Organizer: **Sander Gliboff**, Chair: **Lloyd Ackert**

• Experimentation without Limits: Herbert Spencer Jennings and Pragmatism at the Zoological Laboratory of Johns Hopkins, **Judy Johns Schloegel**, Indiana U, USA

While American pragmatism is well understood as a philosophy rooted in evolutionary thought and the principles of scientific, and specifically, experimental practice, less is known, conversely, about the role played by pragmatism in shaping biological practice in the early decades of the 20th century. This paper examines pragmatist thought as the primary resource employed by Herbert Spencer JENNINGS for constructing and executing his vision for experimental research at the Zoological Laboratory of The Johns Hopkins University in the years immediately before, during, and after WWI. Arising from his early investigations of behavior in lower organisms, JENNINGS became involved in the debates over vitalism and mechanism initiated by Hans DRIESCH. In response to the philosophical impasse that characterized that controversy, JENNINGS not only advocated but also practiced throughout his career a method of 'radically experimental analysis' for the study of living phenomena, which was inspired by William JAMES's radical empiricism. This paper considers JENNINGS' radical experimentalism and his embrace of pragmatist ideals more broadly as an attempt to build experimental biology on a pluralistic philosophical and political foundation. It compares the ideals that informed JENNINGS' studies of inheritance in protozoa and his receptivity to the inheritance of acquired characteristics to the production and managerial priorities that informed a competing vision of experimental biology that developed contemporaneously in T. H. MORGAN's Columbia laboratory.

• Experimental Ecology in Soil Science: The Reception of Sergei Vinogradski's 'Direct Method', 1920s-'30s, **Lloyd Ackert**, Johns Hopkins U, USA

In the 1920s-'30s, an international group of soil scientists found their experimental programs for investigating soil microbes being challenged from an unexpected source. Beginning in 1924, the Russian Sergei VINOGRADSKII, one of the founders of modern microbiology, promoted a novel 'ecological' method for soil microbiology. Translating his 19th-century thermodynamic vision of a 'cycle of life' into a 20th-century ecological laboratory method, he investigated microorganisms in the competitive complexity of their natural environment. Based in a small laboratory in rural France, VINOGRADSKII disseminated his method to agricultural experiment stations and microbiological institutes across Europe and the USA He cajoled like-minded soil scientists to adopt his 'direct method' as a replacement for Kochian pure culture methods. Examining the varied reactions to VINOGRADSKII's method in five settings—at the Delft School of Microbiology in the Netherlands; the Agricultural Experiment Stations at Rutgers University, New Jersey and Rothamsted, England; the Department of Agricultural Microbiology, Leningrad; and the State Research Institute of Rural Husbandry, in Austria-controlled Poland—reveals a widespread interest in practical methods for studying soil ecology. The researchers at these locations, however, introduced VINOGRADSKII's

method into their own investigations according to the demands of their own experimental programs, and interpretive styles. At a time when both soil microbiology and ecology were becoming disciplines, VINOGRADSKII—based in part on his authority as one of the few remaining classical investigators in microbiology—encouraged a new investigatory direction.

• Panel Discussion

Thursday, 4-5:30 PM, LR6
SESSION II

Contingency and Explanation

Organizer and Chair: **Stuart Glennan**

• Robustness and Idealizations in Ecological Explanations,
Jay Odenbaugh, U of California, San Diego, USA

It is a truism that models in theoretical ecology are highly idealized. That is, they are composed in part of assumptions that are false of the systems of interest. These idealizations are inescapable given the complexity of populations, communities, and ecosystems. Likewise, it is a common assumption of philosophers of science that a purported scientific explanation is a genuine explanation only if it is true, or at least, approximately true. If this is correct, then most of the theoretical explanations that ecologists offer are not genuine explanations. In this paper, I argue that robustness analysis can help us avoid this less than desirable conclusion using examples from the work of Henry HORN on modeling forest succession as a Markov process. In one sense, false models can explain biological phenomena. Or, put in a slightly different fashion, robustness can show us when idealized assumptions do not matter to the explanatory success of a theoretical model.

• Generative Entrenchment, **William C. Wimsatt**, U of Chicago, USA

To be generatively entrenched (GE-d) is to play an important generative role in the ontogeny of an adaptive system. This is a degree property. Modifying a character with greater GE has more downstream effects, which, if not adaptively coordinated, are increasingly likely to be maladaptive. Thus there should be greater evolutionary conservatism for the trait. So traits tend to be modified and elaborated more at later stages of development, which increases the GE of traits developmentally 'upstream' of them. Though it has important exceptions, this process is itself exceedingly robust. It leads to the evolutionary fixation of contingencies which become generators for other regularities, which are in turn modulated by other contingencies, in iterative repetition almost indefinitely. The evolution from adaptation to exaptation to adaptation through a series of layered kluges exemplifies this process, producing in the developmental design of organisms and their radiating phylogenies a fractal pattern of contingencies on all scales of detail and scope. I know of no other systematic process which can and will reliably do so. This fractal pattern of contingency is a commonly noted feature of history. to be expected whenever these generative and re-productive relations are found—in biology, technology, cumulative learning processes, or the history of cultures. To a significant degree then, generative entrenchment is why history matters, and has the character that it does. A robust process for producing contingencies which matter, and doing so on all scales should frustrate attempts to give law based characterizations of most biological processes, and raise questions about the usefulness of trying to do so. Since mechanisms can be characterized at different levels of organization, with mechanistic explanations fitted to exploit stabler arbitrary contingencies, and tuned to ignore details that are transient or with effects 'smaller' than a given threshold, and then articulated across scales, mechanistic explanations are not similarly infirmed. We can thus understand why mechanistic or 'engineering' explanations should be so favored in these areas. We also see why natural history should have an ineliminable foothold, and why such fascinations with detail should be endemic throughout the biological and human sciences. Indeed were there any laws in the Darwinian Sciences, these patterns of generative entrenchment

would be one of them. Then we would have the paradox of a law-like explanation for why no laws are to be found in these domains.

• Mechanisms and Contingency, **Stuart Glennan**, Butler U, USA

Many philosophers of biology have come to believe that biological explanations involve appeals to mechanisms rather than laws. While there are good reasons for this shift, I shall argue that on certain conceptions of mechanisms, mechanistic and nomological explanations are closer to each other than may be thought. In particular, the operations of mechanisms are understood to be robust and repeatable; and accordingly, their behavior can be described by generalizations that, while not nomologically necessary, are at least strongly invariant. Because of this common feature, it is unclear whether one can appeal to mechanisms in providing explanations of singular events (e.g., speciation or extinction events) that are causal consequences of chance or contingent occurrences. In this paper I shall try to elaborate a conception of mechanistic explanation according to which such singular events are mechanically explicable.

Thursday, 4-5:30 PM, LR1
SESSION III

A Cultural History of Heredity II

Organizer and Chair: **Staffan Müller-Wille**

• Genealogical Tools and the Notion of Heredity: The Development and Use of Family Trees in the Case of Hereditary Chorea, **Yoshio Nukaga**, Max Plank Institute for the History of Science, Germany

This paper analyses the development and use of genealogical tools known as 'family trees' that allow medical practitioners to see hereditary family diseases. Family trees, consisting of family names and a tree diagram on personal traits, were increasingly introduced by neurologists during the late 19th century. However, social scientists have devoted only scant attention to the historical and social processes through which medical practitioners came to use family trees as scientific device for nosographic classification of hereditary diseases such as hereditary chorea (one of the initial terms used to describe Huntington's disease). The purpose of this paper is to trace the complex processes by which family histories, namely the descriptive nosography of family members, and family trees became distinct during the late 19th century. This paper argues that family trees of hereditary chorea turned out to be an important clinical tool, under the influence of genetic theories and with the establishment of teaching hospitals—although, in the case of hereditary chorea, family trees were used to support different interpretations of the notion of heredity. The use of family trees was made possible through several conditions including the centralisation of medical registries and care (namely, of patients and medical practitioners in hospitals) and the circulation of medical records and texts.

• Producing Vaccines, Producing Variation: Agronomy, Race and Empire in the Science of Microorganisms, **Andrew Mendelsohn**, Imperial College, UK

Before 1880 there was no consensus on the biological nature of micro-organisms. Students of bacteria saw either fixed species (following COHN and KOCH) or a flux of form and function (following NÄGELI, BILLROTH and others). Yet after 1880, as I have argued in a recent paper, an international consensus was rapidly achieved, in which bacteria were seen to vary within species, like other organisms. How could a broad and notably French-German consensus on a supremely controversial topic—bacterial species—have been so rapidly forged? This paper argues that an unusually powerful model was provided by vaccines, as invented by PASTEUR in 1880 and then developed, mass-produced, distributed, and used. Not biological method and theory, but vaccine safety and efficacy formed and stabilized phenomena of inheritance, species, and variation.

• Color, Artifice, and Seeing the Physical Basis of Heredity,
Nancy Anderson, Max Planck Institute for the History of Science, Germany

In the 1880s the cell nucleus, chromatin in particular, emerged as the likely locus of the physical basis of heredity. Chromatin, a term coined by Walther FLEMMING in 1879, called attention to the substance's affinity to stains, the basic aniline dyes, in particular. Chromatin could then be contrasted with the term 'achromatic' (absence of colour), which was used to describe unstained nuclear matter. As a significant and new tool for the cytologist and embryologist of the second half of the 19th century, aniline dyes provided both morphological data and elementary evidence of the chemical make-up of that material which was thought to link generation to generation. That is, methyl green was an excellent tool for tracking phosphorus through mitosis. The 19th century was an age of colour and perception theories and a moment when the industrial/commercial world was transformed by the invention of synthetic dyes. Work in the laboratory on the material essence of heredity was 'visual' work, and colour was the discrete domain of sight even (especially) through a microscope. Narratives on the continuity of life were pieced together through the arrangement of dyed and dead cells: image and meaning became attached to hue.

This paper will present the emergence and acceptance of the chromosome theory of heredity, emphasising the role of biological dyes in the first phase leading up to the theory's link to the rediscovered work of Gregor MENDEL in 1902-03 and the decade following. There is no doubt that colour played a crucial role in the development of the chromosome theory of heredity, but it did not do so without some contention. FLEMMING and his colleagues working on chromatin/chromosomes in the 1880s and '90s heavily relied on, but did not entirely trust, the dyes they used. Colour could deceive and material dyes chemically or physically alter organic tissue creating misleading artifacts. A slow convert to the chromosome theory of heredity, Thomas Hunt MORGAN complained that colour itself may have misled scientists into a false theory merely by capturing attention: "Many theories of heredity have been based on the shifting changes in the chromosomes alone. Their capacity for stains has greatly facilitated their study, while the rest of the cell that does not show much differentiation in staining capacity has been ignored." Indeed, biological stains were tools to transform matter and create artificial phenomena. As Tim LENOIR has so succinctly explained, "What exists in nature is complexity. We analyse this complexity by manufacturing pure, isolated phenomena in the laboratory." It was aniline dyes used on organic cells that began this analysis and theoretical trajectory connecting chromosomes, genes, and DNA.

• Commentary, **Hans-Jörg Rheinberger**, Max Planck Institute for the History of Science, Germany

Thursday, 4-5:30 PM, LR2
SESSION IV

Picturing Eggs, Embryos, and Cells II: Cells and Tissues in Motion

Organizer and Chair: **Sabine Brauckmann**

• Theodor Boveri: Drawing and Knowing Heredity and Early Embryonic Development,
Helga Satzinger, Technical University of Berlin, Germany

Theodor BOVERI (1862-1915) was one of the leading German biologists of the early 20th century. His contribution to the chromosomal theory of heredity was based on cytology and cytoexperimental techniques combined with most skillful microscopic drawing. For BOVERI both chromosomes and cytoplasm played their part in the process of heredity and embryonic development. BOVERI had to postulate mechanisms and structures in the cell which got lost in the further development of genetics. They could not be investigated with the new approach of Mendelian genetics and chromosome mapping by the MORGAN group. It will be argued in the talk that a careful investiga-

tion of BOVERI's drawings and texts can show the interdependence of vision, drawing, experimentation, and knowing.

• Egg Membranes as Media: Visualizing Influenza Virus on the Developing Chick Embryo, 1935-'40, **Michael Bresalier**, Cambridge U, UK

Visualization in and on embryos has often been concerned with other scientific objects entirely. "From 1935 to 1955 one can summarize my life as learning about influenza in chick embryos," reminisced the Australian virologist and future Nobel immunologist F. M. BURNET. Largely as a result of BURNET's work, developing chick eggs have figured centrally in human and animal virus research. No place has this entanglement been more evident than in work on influenza. In 1935, four years after E. W. GOODPASTURE showed that chick chorio-allantoic membranes could be used for virus culture, and two years after flu virus had been isolated from ferrets, BURNET started growing the virus on developing eggs. Having calibrated lesions on the egg membrane against those made in ferrets, he learned to see the virus in a new way, and used the new system to produce images through which to explore the viral identity of flu. As BURNET made membranes operate like the microbiologist's phage plaques, he claimed the technique afforded unprecedented precision in visualizing virus lesions. By the late 1940s, his systems for cultivating influenza virus in the amniotic and allantoic cavities were in use worldwide, including in vaccine production. The history of this system reveals the kinds of materials, tools and practices involved in visualizing viruses in the interwar years. Comparing and contrasting it with other visualization systems shows how their differences mattered to debates over the nature of viruses. We also show how, in adapting an embryological object to very different demands, the chick embryo came to be visualized in new and perhaps unexpected ways.

• On the Dynamics of Contact and Motion,
Sabine Brauckmann, U of Utah School of Medicine, USA, and KLI, Austria

In my paper I will introduce how Paul WEISS visualized his experimental data on neuronal outgrowth and cellular movement with electronmicroscopy and time-lapse technique. Until the 1950s he and his collaborators improved the experimental procedure of phase-microscopy and tissue culture to demonstrate the axonal transport and the self-aggregation of cell suspensions. In 1960 he and Cecil TAYLOR produced a media event with their film session at the NAS when they announced their data on the self-sorting of cells, demonstrating that cell suspensions have the ability to reconstitute complete body organs. At the same time WEISS lectured to a broader audience about the esthetics of form, or the beauty of the cell with films showing the development of salamanders, swimming ciliates, the flight of geese, LEDUC's growth models, fighting army ants, crowds at Coney Island, rotating cell nuclei, and torpedo spirals. In short, WEISS employed the visual media to corroborate his experimental data on cellular contact guidance and peristalsis, as well as to visualize his conceptions of molecular ecology and the beauty of the organism. My objective here is to show (1) how WEISS applied the tool of electronmicroscopy and time-lapse technique to his experiments on cell locomotion, and (2) how he used these gadgets to inform professional audiences about his experimental data and to communicate science to the public.

• From Embryonic Slices to 4D Visualization and Reconstruction: Some Milestones in Ascidian Development Studies, **Denis Thieffry**, Universite de la Meditaerranee, France

I will deal with the recent evolution of our visualisation tools and spatiotemporal representations of the developing embryo, from photon microscopy to electron microscopy, and from CDD in vivo imaging to in silico reconstructions. In this respect, I will focus on one group of model organisms, Ascidians, which present a wide variety of interesting developmental features. The talk will be articulated around the use of specific technical devices and the corresponding picturial production.

Thursday, 4-5:30 PM, LR7
SESSION V

Complexity: What is it Good For? II

Organizer and Chair: **Lien Van Speybroeck**

• Foundations of the Life Sciences, **Diedel Kornet**, Leiden University, The Netherlands

In our cosmos each chronologically primary level of organization (atoms, molecules, living systems, etc.) has a fixed number of possible stable configurations, determined by our laws of nature. At a particular moment, under the then holding conditions, only a number of that lot are actualized. At present virtually all now possible atomic configurations of elementary particles are actualized. At the moleculair level, however, far fewer than all now possible stable configurations of atoms are actualized. At the level of living systems even less than all now possible of such stable configurations of molecules are now actualized. A decreased proportion of filled possible configurations at levels of organization of increasing complexity is accompanied by an increasing difficulty to discover general patterns. This is the fundamental reason why we already have the well formulated 'Atomic Theory' but not yet a comparable 'Living Systems Theory'. When only a small number of all configurations now possible have been instantiated it is contingent which of those have made it in history. It is not surprising that disciplines studying the entities of hierarchically higher levels of organization, such as living systems, are tempted to stay limited to the study of the historical path and idiosyncrasies of those systems that made it. A fruitful search for the 'Living Systems Theory' is most likely to come from interdisciplinary complexity studies and Astrobiology.

• Increase in Complexity, **Eörs Szathmáry**, Collegium Budapest, Hungary

Although complexity is hard to define, it is a fairly general belief among biologists that it has increased, at least along certain lineages, in evolution. Many simple measures used to quantify simply will not do to for the justification of this belief. Even more involved measures, such as algorithmic complexity are not adequate since existing, evolved organisms are not minimal. Other measures, such as 'physical complexity' merely restate in fancy language that natural selection produces adaptations, and that complex adaptations are more likely to appear in complex environments (of which an important part is other complex organisms, making the argument circular). The number of differentiated cell types seems to be a better hunch. The question is how it relates to genetics. Whereas for a while it seemed that the number of expressable genes correlates well with organismic complexity, it now turns out that the interaction among genes is much more important. Indeed, the number of transcription factors increases in the order of prokaryotes, protists, nematodes, Drosophila and mammals. The genome has occasionally come to the solution of *delegated complexity*, i.e., when a novel way of generating complexity is launched by genetic initial conditions: the immune and nervous systems are excellent examples. I shall analyze how organismic complexity has increased in the major evolutionary transitions and attempt a coevolutionary explanation (showing the 'autocatalytic nature' of complexity).

• Complexity and Fragility, **Michael Susko**, Independent Scholar, USA

Typically, it is emphasized that complex systems have a robust quality in response to perturbations, but we can also examine such systems from the point of view of their fragilty. A complex system, as in the evolution of primates, takes a long time to develop, going through a vulnerable period of development in order reach full complexity. The evolution of intensive more complex neural systems required sleep, high amounts of energy, and periods of trail and error learning—resulting in vulnerability. Nonetheless the presence of fragility and plasticity offered evolutionary opportunity. There is a constant price for complexity, yet we find this drive in evolution. This presentation focuses on this paradox.

• Some Explorations on Species as Complex Systems,
Thomas Reydon, Leiden U, The Netherlands

The species problem is among the most persistent problems in the philosophy of the special sciences. The main reason why this problem still is not definitively resolved is that it is commonly understood as concerning the ontology and the epistemology of a single scientific concept, whereas in fact a number of separate concepts are at stake. The term 'species' features in biological theory in a number of distinct roles that cannot be played by one and the same concept. A well-known distinction is for example between species as 'units of biodiversity' and species as 'units of evolution'. Units of biodiversity are historical entities: they are static pattern entities (commonly seen as the basic building blocks of the tree of life) that are composed of organisms from past, present and future. Units of evolution are process entities, i.e., units that participate as coherent wholes in (micro-)evolutionary processes in much the same manner as molecules participate in chemical processes. As such, they are concrete entities that are composed exclusively of synchronously living organisms held together by various cohesive forces. 'Unit of biodiversity' and 'unit of evolution' do not merely denote two different epistemic functions of one concept, but denote two patently different kinds of entities that both are called 'species'. Thus, 'species' is best seen as a homonymic term that stands proxy for a number of distinct scientific concepts. (In total, I suggest, there are four distinct concepts at stake in the species problem.) In this presentation I explore some of the consequences of the above perspective on the species problem. Given that a number of distinct concepts are at stake, not all conceptual issues regarding 'species' should be tackled using the same framework. One promising option is to attempt to apply the framework of complex systems dynamics in order to clarify particular issues that arise in relation to one ore more of the concepts that are at stake in the species problem. (It is however clear that not all meanings of 'species' are susceptible to treatment within this framework). With respect to the concept of species as dynamical process entities, for instance, issues such as the causes of their internal coherence, the underlying dynamics of their spatial distribution, etc., seem well suited to be treated within the complexity framework. I explore what insight might be gained by framing such issues in complexity terminology, and how such a framing could be realized.

Thursday, 4-5:30 PM, LR3
SESSION VI

Women in Early Genetics

Organizer: **Ida H. Stamhuis**, Chair: **Michael Dietrich**

• Muriel Wheldale Onslow and Biochemical Genetics, **Marsha Richmond**, Wayne State U, USA

Muriel WHELDALE (1880-1932) was a member of William BATESON's school of genetics at Cambridge University (1903-1910). A distinguished graduate of Newnham College, WHELDALE investigated the inheritance of flower color in Antirrhinum, a topic of particular interest to plant geneticists, including Erwin BAUR in Berlin. WHELDALE's focus on the genetic basis of pigment formation increasingly led her to do cutting-edge work in biochemistry, culminating in the publiccation of her landmark work, *The Anthocyanin Pigments Of Plants* (1916). In 1919 she married the biochemist Huia ONSLOW, with whom she collaborated until his death in 1922. The present paper will examine WHELDALE's work in genetics, and especially focus on the early linkage of Mendelian methodology with new techniques in biochemistry that eventually led to the founding of biochemical genetics.

• Beyond the 'Boss and the Boys',
Michael Dietrich and **Brandi Hevalow**, Dartmouth College, USA

In the early 20th century, Thomas Hunt MORGAN's network of *Drosophila* researchers transformed the science of genetics and made the *Drosophila* fruit fly into one of the most powerful genetic

tools of the 20th century. Historians of genetics have justifiably spilled gallons of ink describing MORGAN's group and its efforts. Despite significant numbers of women within MORGAN's group, however, the model accepted by most historians is one of the Boss and the Boys. This paper seeks to recover the history of women within *Drosophila* genetics and analyze the gendered division of labor within *Drosophila* laboratories that rendered women invisible yet indispensable.

• Women in Early Genetics in Russia, **Elena A. Aronova**,
Institute for History of Science and Technology, Russian Academy of Sciences, Russia

The paper will overview the involvement of women in genetic studies in Russia in 1920s-'30s. In the two main centers of early genetic research in Russia women played an important role. In Moscow a group of talented women trained by Nikolai KOL'TSOV at the private Public university and the Advanced Courses for Women joined the staff of his newly organized Institute of Experimental Biology, which became one of the centers where the new science of genetics developed. In 1922 the Department for Genetics of the Institute was headed by Sergei CHETVERIKOV who managed to bring together young talented students, including two prominent women geneticists, Elena A. TIMOFEEFF-RESSOVSKY (FIDLER) (1898-1973) and Elizaveta I. BALKASHINA (1899-1981). Both were the key figures of the group and their works on genetic analysis of laboratory and wild populations of *Drosophila* made an important contribution to the developments of the population genetics in Russia. Nikolai VAVILOV's group, which formed the second center of the early genetic research in Russia, located in Leningrad, also included a considerable number of women. In the new field of plant genetics VAVILOV recruited scientists, whose initial educational background was in plant physiology, botany, and agriculture—areas, which had been traditionally more feminized than other fields. Some botanists and plants physiologists turned geneticists were disposed towards physiological and developmental approach in their genetic studies. Their scientific practices of handling with plants influenced their views on heredity as the property of the whole organism or of the whole cell, which couldn't be reduced to the action of the genes located in the nucleus. Such views, for example, were expressed by Evgeniia N. SINSKAIA (1889-1965), who started her scientific career as a specialist on meadow culture and shifted to plant genetics, soon becoming a leading scientist at VAVILOV's Institute of Plant Cultivation. The 'physiological' approach shared by many female VAVILOV's coworkers in the 1920s came to some collision with their male patrons' more classical Morganian research program, which attributes to the gene a kind of omnipotence. The works by Russian women geneticists reflect the emergence of the two different scientific styles in genetics research in Russia in the1920s-'30s: the rather classical MORGAN style of genetics research and the more 'holistic' and physiological approach. The paper will discuss the work of several Russian women geneticists of different generations and different backgrounds, their impact in the early genetic research in Russia, and the role of the object in the choice of methodologies and theoretical approach in the early genetic studies.

• Successful Women in Genetics, **Ida Stamhuis**, Vrije Universiteit Amsterdam, The Netherlands

After the rediscovery of MENDEL's laws, the study of heredity gained momentum. Northern Europe was prominent in this development. The Dutchman Hugo DE VRIES, the Dane Wilhelm JOHANNSEN, and the German Erwin BAUR belonged to the first engaged in the new discipline of genetics. There were also female researchers who made considerable contributions. Tine TAMMES in the Netherlands, Elisabeth SCHIEMANN in Germany and Kristine Elisabeth BONNEVIE in Norway were successful in their career, all three became professors. How could that happen in a period that female professors were a scarce phenomenon? What did they do? What was their place in the society of geneticists of that time? Were they in contact with each other? My talk will report about my findings on these successful Northern European female geneticists

Thursday, 4-5:30 PM, LR9
SESSION VII

The Extended Phenotype Revisited II

Organizer: **Kim Sterelny** and **Matteo Mameli**, Chair: **Matteo Mameli**

• Extended Phenotypes, Extended Organisms: A Physiological Perspective on Evolution, Natural Selection and Adaptation, **J. Scott Turner**, SUNY College of Environmental Science and Forestry, USA

Phenotype, whether conventional or extended, is commonly regarded as a reflection of an underlying genotype. Natural selection, meanwhile, depends upon a physiological interplay between the phenotype and the environment presented to it. To coin an aphorism: "the organism proposes, and the environment disposes." Adaptation, finally, involves the progressively harmonious fit between phenotype and environment. DAWKINS' notion of the extended phenotype undercuts this conventional view: if the environment is part of an organism's genotype, how then can the organism adapt to itself? The paradox is resolvable, but only through a physiological, as opposed to a genetic, theory of natural selection and adaptation.

• Embodied Cognition and the Extended Phenotype,
Kim Sterelny, Australian National U, Australia

One central message of *The Extended Phenotype* is that we should not presuppose that the boundaries of adapted systems are those of the organism. A similar message has recently emerged in non-classical cognitive science; the cognitive system of the agent, it is argued, extends into the world. Agents modify their environments in ways that systematically and permanently enhance their capacities. Hence in explaining the power of human cognition, we should treat these modifications as part of the cognitive system of the agent. This paper examines some of these ideas and relates them to those of *The Extended Phenotype*.

• Round Table Discussion

Thursday, 4-5:30 PM, LR5
SESSION VIII

History and Philosophy of Anthropology

Contributed Papers, Chair: **Karen M. Hillis**

• Race as a Research Variable: Reassessing the Scientific Controversy,
Koffi Maglo, Dibner Institute, MIT and U of Cincinnati, USA

By the mid of the 20th century, biologists such as Ashley MONTAGU believed that the concept of race is biologically empty. However, recent controversy over the work of Franz BOAZ but also some genomic and epidemiological findings seem to give the concept anew momentum. But are the interpretations of these findings coherent?

• French Race Theory and the Parisian Society of Anthropology: The Debate over 'La Force noire', 1909-'12, **Joe Lunn**, U of Michigan, Dearborne, USA

During the years immediately before the outbreak of WWI, the French military advanced several proposals designed to strengthen the French army in the event of war with Germany. One of these envisioned recruiting West Africans for possible service in Europe. In his book *La force noire* (1910), General Charles MANGIN, the leading proponent, asserted that many West Africans, owing to the character of their biological and social evolution, would make excellent soldiers. Drawing

on the work of leading biological and anthropological theorists, MANGIN specifically argued that the less developed nervous system of Africans would render them less vulnerable to the psychological terror produced by explosives, that their stamina en marche was superior to that of Europeans owing to centuries of portage, and that several of the more intellectually advanced 'races' could provide sufficient noncommissioned officers to lead the remainder of the African troops. MANGIN's arguments were hotly debated in French society; indeed, over 6,200 articles on the subject of African recruitment appeared between 1910-'12. My paper examines this public discourse in the Parisian Anthropological Society (most of whose members supported the scientific assumptions underlying MANGIN's proposals). I will argue that both proponents and detractors of this scheme employed biological arguments to buttress their various 'expert opinions' and that irrespective of the points made, all discourse was based on widely held assumptions about the hereditary characteristics of Africans. After it was adopted in 1916, this policy resulted in African losses three times as great as those of French soldiers. Like the Holocaust, this episode offers graphic evidence about the human consequences of the application of biological race theory during the first half of the 20th century.

• Rudolf Virchow: Darwinism, Anthropology and Heredity in the Second Half of the 19th Century, **Dirk Backenköhler**, U of Tübingen, Germany

Historiography of science only recently corrected the myth that Rudolf VIRCHOW was long regarded as an opponent of the theories of descent and evolution. A myth that that arose in his battles with Ernst HAECKEL and was nurtured mainly by HAECKEL himself and by HAECKEL's contemporary biographers. VIRCHOW considered himself a 'friend of this theory' but not a 'follower' of DARWIN and indeed had pronounced his belief in the variability of species as early as 1858. However, to understand his views about evolution fully one needs to look at the anthropological research he conducted in the second half of the 19th century, his views about the processes of heredity, his trials to establish scientific education in the Prussian secondary school system, and of course his empiricist idea of science. These points taken together define the grounds on which VIRCHOW was able to build his own interpretation of evolution theory.

Thursday, 4-5:30 PM, LR10
SESSION IX

Biological Modeling II

Contributed Papers, Chair: **Derek Turner**

• Biological and Cultural Evolution, **Mark Bedau**, Reed College, USA

I describe a method for visualizing and measuring the process of adaptive evolution in both biological and cultural evolution, and I illustrate how this method is applied to data from real biological evolution, from computer simulations of biological evolution, and from real evolution of technology—an example of cultural evolution. This method facilitates quantitative and empirical comparisons between biological and cultural evolution.

• Biological Possibilities, **Jason Zinser**, Florida State U, USA

There appears to be a bifurcated answer to the question of whether any X is biologically possible. On the one hand, the organism must be situated historically at the end node of the Tree of Life. I will call this kind of possibility bio-historical possibility. Accordingly, any possible organism must be genetically expressible, fitted to a particular environment (at least for any novel trait to propagate throughout a population), and branching off from a preexisting organism. On the other hand, corresponding to a more traditional approach to possibilities, to be biologically possible an organism must not violate the laws of biology. I'll call this kind of possibility bionomic possibility. Under this conception, biological laws would consist of a more restricted set than physical laws.

Dan DENNETT develops both of these conceptions of biological possibility. Dennett argues that while the bionomic approach might define the rigid boundaries of biological possibility, the most useful conception of biological possibilities is the bio-historical approach. In this essay, I will address the questions of whether or not a pluralistic account of biological possibilities is consistent? Whether an account of biological possibilities can be anything other than bio-nomic? And, what properties of biology could support a biohistorical (nontraditional) account of possibilities?

• The Good, The Bad, and The Impossible, **James Maclaurin**, U of Otago, New Zealand

Theoretical morphology is a powerful tool for the investigation of biological possibility. But, like many other biospaces (ecospace, devospace, the adaptive landscape etc), it suffers from the suspicion that its foundations are more metaphorical than topological. This paper examines the epistemic basis of the discipline and compares it to other well-known biospaces.

• Biomechanical Laws in Paleontology, **Derek Turner**, Connecticut College, USA

Conventional wisdom has it that laws serve two purposes in science: to predict what *will be* observed and to explain what *has been* observed. Conventional wisdom also has it that practitioners of historical science (e.g., paleontology, geology) are more interested in reconstructing token events and processes of the past than in discovering regularities among event types. In this paper, I present three case studies in which paleontologists use ahistorical generalizations of biomechanics for the purpose of drawing conclusions about unobservable prehistoric organisms. Thus, I will argue that paleontology offers some interesting exceptions to the conventional wisdom about scientific laws, while at the same time arguing that paleontology is historical in a sense that is unexpected given the usual way of contrasting historical and experimental science.

Thursday, 4-5:30 PM, LR4
SESSION X

Discussion Session

Biology and 'Naturalness' in Organic Agriculture

Organizer and Chair: **Henk Verhoog**

• Biology and 'Naturalness' in Organic Agriculture,
Henk Verhoog and **Edith Lammerts van Bueren**, Louis Bolk Institute, The Netherlands

Research about the meaning of 'naturalness' in organic agriculture showed that three aspects of the organic concept of nature (the natural) are of great importance:
- A radical distinction between living nature and non-living nature
- The integration of culture and nature in the concept of agro-ecology
- The idea of the characteristic or essential nature of living organisms.

These aspects relate to longstanding discussions in the philosopy of biology about the nature of life, holism and reductionism, and about natural kinds. We would like to stimulate a discussion after two short papers, one about the more philosophical issues and one about the practical implications for plant breeding and the cultivation of plants. We think that the practical context of organic farming can shed new light on the philosophical issues mentioned.

Friday, 18 July

9-10:30 AM

SESSION I, LR1

Radiobiology in the Atomic Age

Organizers: **Angela N. H. Creager** and **Maria Jesús Santesmases**, Chair: **Hans-Jörg Rheinberger**

• Mice and the Reactor: The 'Genetics Project' and Atomic Politics in 1950s Britain,
Soraya de Chaderavian, Cambridge U, UK

After WWII Britain invested in atomic energy research and a national atomic bomb project. Simultaneously, the government called for research into the biological and medical effects of radiations. Work under this remit focused on the introduction of radioactive tracer elements in biological research and in medical diagnosis as well as on the clinical use of radioactive radiation in the treatment of cancer and other medical conditions. One project, however, gained particular political attention during the 1950s: this was the study into the long-term effects of low dose ionizing radiations on mice populations, known as the 'genetics project'. Research in this field, which built on extensive studies initiated as part of the Manhattan Project and continued at Oak Ridge, Tennessee, started in the UK under the supervision of Conrad WADDINGTON at Edinburgh University. The project under the leadership of Tobias CARTER later moved to the Radiobiological Research Unit at Harwell, the site of the Atomic Energy Research Establishment, where investigations were scaled up to include the irradiation of millions of mice as well as of other organisms. The paper will look more closely at these population experiments, their political meaning and their place in the postwar history of genetics.

• Alexander Hollaender's Vision of Radiobiology at Oak Ridge and Beyond,
Karen Rader, Sarah Lawrence College, USA

During the period from 1945 to 1960, the Atomic Energy Commission (AEC)'s sponsorship of biological science increased nearly sevenfold; by 1959, for example, nearly 20% of the members of the Genetics Society of America were engaged in AEC-supported research or training programs. Historiographically, however, the meaning and consequences of this dramatic shift in biology's system of funding remain unappreciated. The goal of this paper will be to illuminate these issues through a focus on the work of Alexander HOLLAENDER. HOLLAENDER was the founder and longtime director of the Oak Ridge Biology Division; there he defined and directed the AEC's intramural program of radiobiology research. The paper will examine the complicated relationship between HOLLAENDER's individualist entrepreneurial role at Oak Ridge, and the role he played as international ambassador for the AEC's biological research program. HOLLAENDER drew on his own experiences as a biophysicist in his understanding of how the system of biological patronage and genetics research in the postwar world might be integrated, and his programmatic vision had profound consequences on how postwar biological scientists viewed their own work—both within their laboratories and in society-at-large.

• The Radioisotope Distribution Program of the U.S. Atomic Energy Commission, 1946-'60,
Angela N. H. Creager, Princeton U, USA

The extensive use of radioisotopes was one of the most conspicuous features of postwar radiobiology, and represented one of the material consequences of the 'physicists' war' for postwar biomedical research. This paper will focus on the activities of USA Atomic Energy Commission (AEC) in promoting the postwar utilization of radioisotopes, and offer some assessments of the scientific and political consequences of this multifaceted program. This government initiative did not originate the use of radioisotopes in research; the first experiments in biomedical and agricultural re-

search using radioisotopes dated to the 1920s. The construction of numerous cyclotrons in the 1930s and '40s increased the availability of many radioactive isotopes (including, importantly, those of carbon), but these remained scarce commodities. By contrast, the AEC's program, which was developed by Manhattan Project scientists at the end of the war, vastly increased the availability and affordability of radioisotopes by employing the nuclear reactor at Oak Ridge as a production and distribution site. Consumption of AEC-produced radioisotopes in both laboratories and clinics grew rapidly in the postwar years, with shipments of radioisotopes with biomedical application—phosphorus-32, carbon-14, and iodine-131—leading the trend. The radioisotopes also had an important role in the AEC's public relations efforts, since radioisotopes offered one of the first and most concrete benefits of atomic installations for science and medicine, as frequently touted in agency Congressional reports and publications. The paper will conclude with some analysis of the political circumstances and consequences of the commercialization of radioisotope production in the 1950s.

• Nuclear Policy and Biomedical Experimentation in Spain: The Introduction of Radioisotopes, **Maria Jesús Santesmases**, Consejo Superior de Investigaciones Cientificas, Spain

The early introduction of radioisotopes in biological and biomedical research in Spain took place from the late 1950s onwards by two different means, closely interlinked. On one hand, a tiny group of biomedical researchers in biochemistry and experimental endocrinology spent a significant part after their training periods abroad. On the other hand, nuclear policy at the top level of the FRANCO dictatorship's government was aimed at developing nuclear physics through international agreements with the US and the international organizations it created, such as the UN International Atomic Energy Agency. The accession of Spain to the organizations and the international agreements signed by the government contributed to establishing technical training for the use of radioisotopes applied to biomedical research, through the Spanish Instituto de Estudios Nucleares. At the same time, foreign funds allowed a group of endocrinologist led by Gabriella MORREALE and Francisco ESCOBAR in Madrid to introduced radioisotopes in their research, and at the Junta de Energía Nuclear (the Spanish board of Nuclear Energy), a group led by Carlos DÁVILA did research on radiobiology. This paper will emphasize how these two groups became full-fledged research schools that were extremely influential in the later developments of this area in Spain, DÁVILA's in connection to chemistry and MORREALE-ESCOBAR to biomedicine. Access to new research materials and the introduction of Spain into the Western scientific networks through a foreign policy for international collaboration in science and technology served as mutually reinforcing processes.

Friday, 9-10:30 AM, LR7
SESSION II

Evolution and Development I

Contributed Papers, Chair: **Arantza Etxeberria**

• The Influence of Development in Evolution: The View of Pere Alberch,
Tomás Garcia-Azkonobieta and **Arantza Etxeberria**, U of the Basque Country, Spain

These days many voices demand the need of including the role of developmental processes in the general theory of evolution. The notion of a 'developmental constraint' was coined to explain the fact that not all conceivable living forms are actually observed: the idea is that there are factors that limit the amount of variability available, thus somehow biasing evolutionary change towards certain paths and not others, and that some of these factors have a developmental character. Pere ALBERCH gave many clues about ways in which developmental processes can explain limitations of the space of morphologies. His question was: why do we observe some forms in nature and not others? In his opinion an adequate account requires an internalist perspective (for as far as neo-Darwinism considers that the main source of adaptation is Natural Selection, it is an externalist

theory). The internalist approach is an alternative way of studying morphological evolution, a method rooted in an understanding of the properties of organisms rather that of the properties of the environment. The focus is in the internal rules that control the appearance of morphological variation and the evolutionary consequences of this internally determined order. In this paper we rehearse some of his main ideas about the influence of developmental constraints, including the examples taken from nonfunctional teratologies, and we draw some consequences for the elaboration of the Developmental Evolutionary Theory or EvoDevo.

• Toward a Philosophy of EvoDevo,
Werner Callebaut, KLI, Austria and Limburgs Universitair Centrum, Belgium

Reflecting on the renewed interest in the relationship between evolutionary biology and developmental biology that awoke in the 1980s, ATKINSON (1992) speculated that their reunion "may result in a new subdiscipline of biology, if there is a set of unique concepts and methods which tie the various research approaches together." In this paper I reflect on the state of the art of EvoDevo from the combined perspectives of the philosophy and social studies of biology.

It is often felt that such concepts as bauplan, canalization, developmental constraint, evolvability, and modularity may serve to unify EvoDevo, and some authors would add evolutionary innovation and the origination of form to this list as well. The data- and technique-driven character of much of the relevant research is likely to impose additional requirements on the new synthesis. When HAECKEL proposed his views on recapitulation, the union envisaged was primarily one of descriptive sciences. For HAECKEL, as for DARWIN before him, comparative embryology served as the source of information in the construction of phylogenies. Although the nexus between development and evolution may to a large extent remain descriptive, developmental biology has come a long way and today offers a wealth of models and theories—also, but not only, because it has gone molecular. Moreover, the great debates within evolutionary biology focus in part on the causal mechanisms of evolutionary change rather than on the elucidation of particular phylogenetic relationships. Opposing developmental biology to evolutionary biology in terms of causal versus functional explanation, then, would be an oversimplification.

The task of elucidating a philosophy of EvoDevo is complicated by the circumstance that it is not merely competing theories that are at stake, but much broader theoretical (as well as empirical) perspectives. Approaches to development and evolution such as gene selectionism, developmental systems theory, developmental structuralism, or dialectical accounts are typically presented as 'package deals' that include their own explanatory strategies, methodology, and 'background philosophy' in which a characteristic set of metaphors typically play central heuristic and rhetoriccal roles. My main concern in this paper will be to what extent the links that are suggested between certain theoretical views and metalevel views within the related but distinct research programs that together constitute EvoDevo are really robust or even inevitable rather than the result of, say, the idiosyncracies of researchers' scientific and philosophical education. Building on the work of HOOKER, MAHNER and BUNGE, and others who have argued convincingly that one can resituate neo-Darwinian processes within a complex systems framework without going to either empiricist or rationalist extremes, I will argue that much of the current confusion results from the contingencies of disciplinary 'boundary work' and that the cards could be profitably reshuffled.

• A View to a Failure? Part-Based and Kind-Based Science in Evolutionary Developmental Biology, **Rasmus Winther**, Indiana U, USA

In recent years we have nearly achieved the coveted 'synthesis' between evolution and development—or so the standard interpretative story goes. However, the actuality, and even possibility, of this synthesis is far from obvious given the severe differences between (1) macroevolution and mechanistic developmental biology, and (2) microevolutionary mathematical evolutionary genetics theory, including levels of selection theory. This is a tension whose roots can be seen already in DARWIN's and WEISMANN's work, as I have alluded to in previous history of biology publications. In subsequent philosophy of biology articles, I called the two contrasting perspectives coordinating

work in each of these two general areas the 'Integration' and the 'Competition' perspectives, respectively. I now extend this argument by noting that these perspectives are respective exemplars of two distinct views on the nature of science. (1) 'Part-based science' focuses on compositional part-whole relations such as mechanisms, and functional interdependencies, among system components, i.e., parts. (2) 'Kind-based science' focuses on formalistic kinds, such as the genes of mathematical evolutionary genetics theory, and their properties derived by abstraction. The two views do overlap—the concrete parts of the Integration Perspective are abstracted (e.g., kinds of modules) and the formalized genes of the Competition Perspective are sometimes considered parts (e.g., multilocus models with interaction effects, i.e., epistasis, between different loci)—but the theoretical object emphasized, part or kind, is clearly different in the two sorts of science. My general distinction furthers work in two areas. (1) The synthesis of evolution and development. Before the serious difficulties involved are resolved, no synthesis is actual, or even possible. There is no guarantee for such a resolution—intimations of a failed synthesis are real. (2) The role of parts in science. Although philosophy of science has extensively analyzed kinds and abstraction, it has paid almost no attention to either parts as theoretical objects or system partitioning as a theoretical activity. Through philosophical analysis, I hope to be able to contribute to an understanding of the importance of parts in science.

• The Problem of Innovation and Novelty, **Alan C. Love**, Indiana U and U of Pittsburgh, USA

Biologists since before the time of DARWIN have been struck with two aspects of living entities: their tremendous diversity and their exquisite adaptedness to their environmental surroundings. In the 20th century, the most successful theoretical framework for dealing with the phenomena of adaptation and diversity was the Modern Synthesis, (neo-Darwinism). It has recently been claimed by a number of proponents of evolutionary developmental biology (EvoDevo) that there is a distinct problem or suite of evolutionary phenomena not addressed by the neo-Darwinian framework: evolutionary innovations or novelties. For example, in the preface to a forthcoming volume the editors provide the following summary statement: "The present volume is motivated by the conviction that the origination of morphological structures, body plans, and forms should be regarded as a problem distinct from that of the variation and diversification of such entities (the central theme of current neo-Darwinian theory) and that the generative determinants of organismal form must be included in any productive account of the evolution of developmental systems and organismal form in our postgenomic era."

The present paper is an attempt to set out the philosophical contours of the problem of evolutionary innovation and novelty. I begin with a brief discussion of the notion of a problem agenda, which is an investigative unit for philosophy of science that begins to formalize what it means to talk about different problems in biology (and other sciences). With this in hand I articulate three dimensions of the problem of innovation and novelty: (1) the origin of functions versus forms; (2) representation, or the generality of theoretical and experimental investigations of function and form; and, (3) level of biological hierarchy in focus. These dimensions are collected under the phrase 'the problem of apomorphic character origination', which is demonstrated to be distinct from the problem of adaptation and other biological problem agendas. In the concluding section I evaluate claims by proponents of EvoDevo that they are in a unique position to tackle this distinct problem in comparison to traditional evolutionary theory. In particular, I argue that the ability to eventually produce satisfactory answers to the problem of apomorphic character origination demands an ongoing multidisciplinary synthesis, which is unlikely to hold together due to the asymmetry of data collection among EvoDevo's purported disciplinary constituents.

• The Miscued Mind: Psychopathology and the Neglected Role of Developmental Cues,
Brian Garvey, Trinity College, Ireland

Evolutionary Psychology (EP) typically claim a high degree of developmental robustness for the species-typical cognitive mechanisms underlying human psychology. That is, EP proponents claim that those mechanisms reliably develop in a wide variety of different environments. The adaptive benefits of this developmental robustness are often offered as an argument in support of this claim. I argue that this adaptive argument, at best, only supports developmental robustness relative to the different environments in the Pleistocene Era, and cannot support claims of developmental robustness in unprecedented modern environmental conditions. The active role of extra-genetic developmental cues must be taken into account, requiring us to consider what happens when they are different or absent. Much is made in the literature of EP of problems caused by the misfit between 'Stone Age minds' and the modern world; and EP also allows for psychological disorders due to genetic abnormalities. But the assumption of extensive developmental robustness obscures a third set of possibilities—failure of mechanisms to develop, or abnormal development, due to modern environmental conditions. An account of some psychological disorders in these terms would bear parallels to psychoanalytic aetiologies in terms of developmental vicissitudes.

• Words That Alter Brains: A Shift in Neurocognitive Research?
Simona Ginsburg, The Open U of Israel

The reductive program of the cognitive sciences, dedicated to uncovering bottom-up causal explanations of behaviour, has recently been infiltrated by attempts to demonstrate physical changes occurring in the brain after mental activities. Will these attempts turn the program upside-down? Approaches that search for the biological basis of behaviour, from genes to neurons, still reign high over the neurosciences. Reductive strategies continue to flourish, but at least in the field learning it has long been established that mental processes result in alterations of synaptic strengths and neuronal activities. Only recently has this glaring fact been picked up and extended. The idea that words may influence the brain, in a top-down manner, has been regarded as an unscientific claim. Yet striking new studies, which reveal similar changes in brain areas following psychoactive drug treatment on the one hand and verbal therapy on the other lend credibility to this very idea. The present paper explores how the exclusively reductive doctrine is beginning to give way to more mixed research styles in the neuro-cognitive sciences, including top-down heresy. It appears that a similar tendency is taking place in many areas of biology. Three major developments may have contributed to the shift in neuro-cognitive research: (1) the widespread use of neuro-imaging techniques (the new bandwagon), (2) the growing respectability of 'consciousness' as a legitimate entity of research in science, and (3) an increasing moral discomfort with biological determinism.

• Folk Psychology and the Theory of Mind Module, **Matthew Ratcliffe**, U of Durham, UK

The view that our folk psychological abilities are enabled by an evolved cognitive module is currently popular amongst philosophers and evolutionary psychologists. This paper argues that such claims presuppose an implausible conception of 'folk psychology'. Commonly adopted characterisations of folk psychology are inadequate in two central respects. First of all, the emphasis on rationally interconnected propositional attitudes arbitrarily excludes various other intersubjective capabilities, which are inextricably bound up with propositional attitude ascription. Second, the claim that the role of folk psychology is to predict and explain others' behaviour is misleading, amounting to an unwarranted 'scientisation' of everyday intersubjectivity. A more enriched description of folk psychology is sketched, which points to a plethora of interacting abilities, as

opposed to a discrete, domain-specific capacity or body of knowledge. Hence the claim that we have a unitary, modular 'theory of mind' is, it is argued, highly implausible. However, an evolved ability to 'simulate' is, it is suggested, likely to be a constituent of interpersonal understanding.

Friday, 9-10:30 AM, LR2
SESSION IV

Modes of Research in Biology

Organizer and Chair: **Elihu M. Gerson**

• Between Philosophy and Experiment, **Manfred D. Laubichler**, Arizona State U, USA

Theoretical biology emerged as a discourse among biologists from a variety of different experimental disciplines and some philosophers and physicians during the early decades of the 20th century. The discourse of theoretical biology was centered around the conceptual, epistemological, and methodological foundations of biology as well as the relation of biology to physics and metaphysics (the problem of the autonomy of biology), the specificity and characteristics of biological processes (such as regulation, differentiation, inheritance, and organic transformation), and the representation of biological knowledge. It is concluded that theoretical biology in the first half of the 20th century was simultaneously part of biological research and constituted a critical (or metatheoretical) analysis of biology as such. It therefore was a precursor of today's philosophy of biology as well as the (more restricted) agenda of modern theoretical biology.

• Alternative Modes of Biological Research, **Elihu M. Gerson**, Tremont Research Institute, USA

Many of the changes in evolutionary biology which took place around the turn of the 20th century have been described in terms of a conflict between two competing ways of doing research (e.g., as 'naturalists' vs. 'experimenters'). Often, the debates have been described in terms of a new and better way of doing science replacing an older one. This paper proposes an alternative way of thinking about these events. There were three, not two, modes of conducting research active around 1900: the historical-comparative, the causal-analytic, and the mechanical-elucidative. Each mode had its own typical way of framing problems, theorizing and handling data, although each made at least occasional use of others approaches as well. In each mode, some researchers were pushing forward with new theories and methods, while others were lagging behind. The organization of the debates which took place at the time were therefore much more complex than a simple division between older/less scientific and newer/more scientific. Instead there were multiple overlapping debates in which work in the different modes was often misunderstood and misrepresented.

• Theoretical Biology at the Beginning of the 21st Century,
Peter Hammerstein, Humboldt U, Germany

Compared to physics and mathematics, biology is a very young scientific discipline. Among the benefits of this late origin is the availability of a large body of knowledge in physics and mathematics that biologists can draw upon while they are trying to develop their own theories about living systems. We currently observe a rush of activities in biological theory development, mathematical and increasingly also computational, and, as it turns out, experimentalists, desperate to further explore the meaning of empirical results, increasingly solicit these activities. Indeed, many experimental biologists now maintain that the future of biology will depend on its ability to develop theories. This paper will investigate four different elements of Theoretical Biology at the beginning of the 21st century; its role in (1) data analysis and presentation (Bioinformatics, biological measurement theory); (2) mathematical model building; (3) concept formation; and (4) the integrative function of Theoretical Biology

Friday, 9-10:30 AM, LR6
SESSION V

Animal Communication

Organizers: **Georgina Hoptroff** and **Tania Munz**, Chair: **Georgina Hoptroff**

• Emotive Cries to Functional Calls: Clarence Ray Carpenter and his Technological Tools, **Georgina Hoptroff**, U of Minnesota, USA

During the early 20th century, primate calls were generally understood to be without function. The calls were merely a form of emotional expression. By the 1930s, American primatologist Clarence Ray CARPENTER had begun to understand primate calls as forms of communication serving important social function. This paper will focus of the work of CARPENTER's work with the Asiatic Primate Expedition (A.P.E.) of 1937, and the way that recording technology was used in the field to improve field techniques. I will argue that the use of recording technology was part of CARPENTER's conscious effort to overcome the limitations of the field whilst maintaining its advantages.

• The Primate Playback Experiment, **Gregory Radick**, U of Leeds, UK

In the early 1890s the American naturalist R. L. GARNER became famous for experiments on animal language using the Edison cylinder phonograph. By recording the utterances of monkeys and then playing the recordings back to the monkeys and observing their responses, GARNER claimed to establish the existence of what he called "the simian tongue," different only in degree from human languages. Celebrated within science and without for its power to vindicate the evolutionary theory of language origins, the primate playback experiment nevertheless largely disappeared from the scientific repertoire in the early 20th century. It returned to wide acclaim only in the late 1970s and early '80s, with the work of Robert SEYFARTH, Dorothy CHENEY, and Peter MARLER on the alarm calls of vervet monkeys. This paper will compare and contrast the rationales for the primate playback experiment in its 19th- and 20th-century forms. I shall argue that underlying the differences are important but little-noted changes in evolutionary theory and in disciplinary self-conception.

• Dancing Bees and Bickering Scientists: Karl von Frisch's Work on Bee Communication and the Dance Language Controversy, **Tania Munz**, Princeton U, USA

Through a series of experiments that spanned several decades, Karl VON FRISCH determined that bees communicate the location of profitable food sources to their hive mates by means of a symbolic dance language. Although VON FRISCH's work was widely recognized for its elegance and experimental rigor and earned him a Nobel Prize in 1973, his findings did not go unchallenged. In particular, beginning in the mid-1960s, two American scientists—Adrian WENNER and Dennis JOHNSON—published several articles that questioned VON FRISCH's bee language interpretation. They asserted that bees rely on olfaction rather than communication to locate food and sparked a series of heated exchanges and replication attempts. This paper will examine what was at stake in this debate with respect to animal language, scientific method, and animal mind and will address why bees talked to some scientists but only danced for others.

• Commentary, **Robert J. Richards**, U of Chicago, USA

Friday, 9-10:30 AM, LR10
SESSION VI

Metaphor and Communication I

Organizer and Chair: **Brendon Larson**

• The War on Invasive Species, **Brendon Larson**, U of California, Santa Barbara, USA

This paper examines implications of framing the control of invasive species as a metaphorical war. Invasive species are ones that thrive in a new region to which they have been introduced by humans. There are myriad examples of invasive species, and many conservation biologists are concerned with their potential effects. It is thought that they may disrupt ecosystem services necessary for humans and that a few highly adaptable species will eventually dominate much of the earth's surface, leading to globally impoverished biodiversity. These issues were summarized in a recent *Issues in Ecology* paper, which is "designed to report, in language understandable by non-scientists, the consensus of a panel of scientific experts on issues relevant to the environment." The abstract of this paper includes strong wording that suggests the issue is being framed to the public as a war on invasive species. Although popular articles often invoke this language, it is usually not so evident in scientific treatments. In this paper, I consider potential implications of using a war metaphor for invasive species, as well as lessons for understanding metaphors in science communication. I argue that the war metaphor may actually have negative implications for biodiversity, particularly given its cultural implications post-September 11. Ironically, it appears that conservation biologists partly depend on a similar metaphoric repertoire as those who oppose conservation measures.

• Variability and Biodiversity, **Elena Gagliasso**, U of Roma, Italy

This talk aims to suggest and analyse a relationship between two influential concepts in the life sciences: 'biodiversity' and 'variability of life'. The difference between biodiversity and variability of life is that the former is a technical term and the latter is a descriptive expression. The modern meaning of biodiversity came from genetic population and ecology at the same time: the differences in the genetic pool are crucial for survival in everchanging ecosystems and their valorisation is a central part in modern evolutionary theory. Instead, the variability of life is an aesthetic and contemplative statement and a taxonomic starting point to create order and clusters, arose for similarity into a multiple levels of being. Genetic biodiversity is represented by the occurrence of different genetic forms within organisms belonging to the same species. Etherozigosity, resulting by crossing of differents lineages, improves plasticity of the species and helps to face environmental changes. Ecological biodiversity concernes the relationships among individuals, species and their abiotic environment. So we find the expression of ecological biodiversity on three levels: biocenosis, ecosystems, landscapes. In both fields (genetic population and ecology) heterogeneity and the individuals' centrality are important causes of evolution and at the same time, they are historical products of evolution. Unlike variability of life, biodiversity's caracters -diversity, relationality, individuality—became influents directory in scientificity and in culture in the late 20th century. In that period biodiversity point out a new cross paradigm involving evolution, ecology and cultural studies. Moreover, biodiversity as a metaphor influences a current way of thinking, an epistemological guideline and a cognitive replacement of subjects.

• The 'Good Genes' Metaphor and Model of Sexual Selection by Marlene Zuk, **Viorel Paslaru**, U of Cincinnati, USA, and Institute of Philosophy of the Romanian Academy, Romania

Since 1982 Marlene ZUK has defended a version of the of the 'good genes' account of sexual selection according to which animals choose mates after they scrutinize them for characters whose expression is dependent on health and vigor. Her hypothesis goes against the population genetics theory that predicts zero heritability of fitness for any balanced polymorphism for a selected trait, such that no one mate is better for 'good genes' than any other. I analyze the research done by ZUK

on the red jungle fowl since then and show the heuristic and cognitive roles that the 'good genes' metaphor plays in her explanation. I also argue that the metaphor has to be seen as a way of introducing a non-formal model, as opposed to a population genetics' model, in order to explain sexual behavior.

Friday, 9-10:30 AM, LR9
SESSION VII
Genes, Genomes, and Genetic Elements I:
Conceptualizing New Developments in Molecular Genetics
Organizer: **Karola Stotz**, Chair: **Paul E. Griffiths**

• Representing Genes: Testing Competing Philosophical Analyses of the Gene Concept in Contemporary Molecular Biology, **Karola Stotz**, U of Pittsburgh, USA

Previous research by Paul GRIFFITHS and me has established that competing analyses of the concept of the gene in contemporary biology can be operationalized and tested by statistical analysis of questionnaire data obtained from working biologists. The paper will report on the current study by GRIFFITHS and STOTZ that brought together the leading researchers in the field to design a research instrument and test some of the core claims made by these researchers. The project will lead to progress in understanding how different gene concepts contribute to biological research, to revealing the deficiencies in current conceptualizations of the gene and will contribute to work on the public understanding of genetics. The purpose of the study is not to arrive at one or more correct 'definitions' of the gene, but rather to map out the variation in the gene concept and to explore its causes and its effects. The main focus of the talk will be how we constructed, in cooperation with leading researchers in the history and philosophy of biology and with the help of researchers in other fields with relevant expertise, testable versions of claims about how groups of contemporary biologists think about genes, and how to test those claims.

• When Conceptual Analysis is No Longer Protected From the Methodology of Social Science, **C. Kenneth Waters**, U of Minnesota, USA

The 'Representing Genes' project, which is aimed towards elucidating gene concepts by examining the behavior of practicing scientists via sophisticated questionnaires, raises interesting questions about the nature of conceptual analysis in philosophy of science. To what extent can philosophers' research on the gene concept be subjected to this kind of empirical test? Are the claims made in the literature on the gene concept descriptive, prescriptive, or a combination of the two? And if the latter is the case, to what extent can descriptive and prescriptive elements be separated? I will clarify and address questions raised by the prospects of having one's philosophical claims about the conceptual foundations of a science face tests carried out by those employing methods from social science.

• Pleiotropy and Environment, **Richard M. Burian**, Virginia Tech U, USA

Recent work on gene concepts has been greatly influenced by recognition of the extent to which the boundaries and specific sequence of DNA transcripts (example: rat á-tropomyosin) are altered by the cellular environment. DNA transcripts are, of course, altered in many other ways. Even coding RNAs are altered in various ways, including, inter alia, transsplicing, so that the final mature transcript may derive from different genes (as conventionally identified) and even from different strands of a given chromosome. Additionally, post-translational modification of polypeptides is very common. All these phenomena are sensitive to the cellular and, indirectly, to the external environment. Recent work in 'eco-devo' (*Evolution and Development* 5(1), January-February 2003) reinforces challenges about the extent to which genes specify the organism or organismic traits by showing that and how entirely different morphs are produced in response to external envi-

ronmental cues. Against this background, I raise some questions about which, to date, I have seen no literature. Are the various effects of so-called pleiotropic genes differentially sensitive to different environmental influences? If so (as seems initially plausible), does this add to the burden of difficulties in articulating satisfactory gene concepts?

• Commentary, **Kenneth F. Schaffner**, George Washington U, USA

Friday, 9-10:30 AM, LR3
SESSION VIII

Functions and Teleological Explanation I

Contributed Papers, Chair: **Mark Lebow**

• Functional Properties and Causal Relevance, **Mark Lebow**, Columbia U, USA

This paper examines the nature of causal role functional properties. My concern is to explain how functional properties are instantiated by different structures. Within philosophy, such properties have been receiving attention recently in relation to the notion of causal relevance. The properties of a structure are said to be causally relevant to the functions it instantiates. The paper will show how the notion of causal relevance helps to clarify when a structure instantiates a function. In the first part of the paper, I describe the standard account of causal role functions offered by Robert CUMMINS. The account permits that different kinds of structures can have the same function despite being causally distinct. In the second part of the paper, I argue against the account by showing that it depends on an illicit notion of causal relevance. If the properties of a structure must be causally relevant to the function it instantiates, then causally distinct structures cannot have the same function.

• Teleological Explanation: Shaping Contingency into Scientific Practices,
Sergio F. Martinez, UNAM, Mexico

One of the most fundamental issues in the philosophy of biology is the issue about the place and role of teleological explanations in biology. I will provide a brief summary of the discussion and show that the different ways of approaching the issue are related with 'explanatory needs' of different practices. Next I will try to give a philosophical account of this fact. The basic claim is that scientific explanations are historical patterns of understanding that get dispersed in different practices because they tend to accompany successful ways of accounting for contingency.

• Adaptation as Selection-based Teleology: Darwin's Response to Gray on 'Bringing Teleology Back' to Biology, **Scott Thomson**, Virginia Tech U, USA

The question of teleology and its role in understanding the biological world has been an interesting question ever since ARISTOTLE. It has been a vexing question ever since DARWIN. Since the mostly clearly teleological world is a purposefully designed world, and since a purposefully designed world is at odds with a Darwinian world explained by chance and natural law, it is easy enough to understand the persistently strong trend to want to abolish all talk of teleology in biology. We have seen attempts to excise teleology ranging from the reductionistic philosophy of NAGEL, to the use of the term teleonomy, to the more recent trend to talking about functions. In the midst of this, some have continued to claim that, in fact, the idea of teleology still has some place in biology. And from time to time, DARWIN himself is dragged back into the debate. Did he see his theory as being in any relevant sense teleological? The lightning rod which draws this question most strongly is the statement made by Asa GRAY praising "Darwin's great service to Natural Science in bring back to it Teleology: so that, instead of Morphology versus Teleology, we shall have Morphology wedded to Teleology." To which statement Francis DARWIN reports that DARWIN responded, "What you say about Teleology pleases me especially and I do not think anyone else has ever

noticed the point." Two things make this statement and response well worth a closer look. (1) GRAY was a supporter of Darwinian natural selection, but also of theistic purpose and design in evolution. (2) DARWIN had a long history of disagreeing with GRAY over the issue of design. One would think that it is unlikely that DARWIN could have supported the same idea of teleology as GRAY? and that he particularly would not want to be seen as supporting GRAY's idea of theistic design. So what was DARWIN up to?

Friday, 9-10:30 AM, LR5
SESSION IX
Biology and Ethics/Epistemology I
Contributed Papers, Chair: **Fritz Alhoff**

• Normative Turn of Evolutionary Ethics: The Invisible Evolutionary Ethics?
Tomislav Bracanovic, U of Zagreb, Croatia

The contemporary evolutionary ethics was mainly shaped as the meta-ethical enterprise, trying to resolve questions like: Do biological considerations support moral anti-realism or moral realism; or: Can normative claims be derived from descriptive ones? It will be argued that this prevailing 'meta-ethical tendency' of evolutionary ethics, although it is not insignificant, may turn into a burden for the entire discipline: by giving too much weight to meta-ethical implications of evolutionary theory, evolutionary ethics dangerously departs from its natural environment of empirical considerations within evolutionary biology. One of the problems is that, although 'morality' appears to be some kind of 'phenotypic trait', we still have no unique evolutionary explanation of its origins: all we have is various strands of persuasive, yet different and frequently mutually exclusive 'plausibility arguments'. In view of this, it seems illogical to reflect about the meta-theoretical status of 'moral claims' or about the facts they allegedly refer to, before the 'morality' itself is satisfactorily explained. The central argument is that the best option of evolutionary ethics is merging with, supporting, and correcting other (non-biological) normative ethical theories.

• Evolution and Moral Realism, **Kevin Brosnan**, U of Wisconsin, USA

Arguments that seek to undermine moral realism on the basis on the alleged evolutionary origins of our moral beliefs (such as those given by RUSE and WILSON) are often rejected because they commit the genetic fallacy. SOBER has argued that claims about the origins of a belief, evolutionary or otherwise, should not be used in deductive arguments (because of this fallacy), but may be used to support inductive conclusions. In this paper, I argue that, even if we accept SOBER's point, there are no strong inductive arguments against moral realism that rely on the evolutionary origins of our moral beliefs.

• The History of Evolutionary Ethics, **Fritz Alhoff**, U of California, Santa Barbara, USA

Ever since DARWIN's publication of *The Origin of the Species*, the ethical implications of evolution by natural selection have been debated. DARWIN's own views have, in my opinion, been substantially undervalued; much of his own writings on ethics foreshadowed contemporary developments in sociobiology. After considering DARWIN's theory of moral sense, I move on to the most famous advocate of evolutionary ethics, Herbert SPENCER, who allegedly sought to derive normative conclusions from the fact of evolution. While this move has been ridiculed by some, such as G. E. MOORE, modern commentators are more inclined to grant SPENCER a more charitable reading and, in fact, it remains an open question whether SPENCER actually committed the naturalistic fallacy as he is so often accused. SPENCER's contemporary and friend, T. H. HUXLEY, also offered a theory of evolutionary ethics, though he reached the exact opposite conclusion of SPENCER; HUXLEY thought that natural selection was cruel and should be opposed rather than endorsed.

After considering these theories, I discuss the critics, Henry SEDGWICK and MOORE, though these men draw heavily off of David HUME.

Friday, 11-12:30 PM

SESSION I, LR7

Karl Pearson, Ronald Fisher, and the Statistical Roots of Biology

Organizer and Chair: **Nancy S. Hall**

• The Development of Pearsonian Statistics vis-à-vis Biological Populations of Species,
Eileen Magnello, U College London, UK

The analysis and interpretation of biological population of species was pivotal to the development of the foundations of the modern theory of mathematical statistics owing largely to the work of the Darwinian zoologist, W.F.R. WELDON (1860-1906) and the polymath, Karl PEARSON (1857-1936). Whilst mid-Victorian statisticians whose interests lay in biology or medicine, such as Edwin CHADWICK, William FARR, Francis Bisset HAWKINS, Florence NIGHTINGALE and Adolphe QUETELET were instrumental in bringing about significant changes in the use of vital statistics in medicine, at the General Registrar Office, or in hospitals, their analyses were underpinned by the use of averages, proportions and percentages. This use and type of statistics began to change in the 1870s when Francis GALTON (1822-1911) began to examine statistical and biological variation and showed that standardised comparisons could be made by using the law of frequency (or the normal distribution).

At the end of the 19th century, the content and practice of statistics underwent a series of transitions that led to its emergence as highly specialised mathematical discipline. These intellectual (and later institutional) changes were, in part, brought about by a mathematical-statistical translation of the Darwinian changes in ideas about what kinds of natural process occur in the world. The Darwinian idea that had the greatest impact on the development of mathematical statistics was DARWIN's redefinition of the biological species as something which could be viewed in terms of populations: in contrast the Aristotelian essentialistic idea of 'types' which formed the basis of the morphological or typological concept of species used by a number of biologists until the end of the 19th century focused on averages rather than individual variation.

This paper will show how PEARSON and WELDON's mathematical reconceptualisation of Darwinian 'statistical' population of species in the 1890s provided the framework within which a major paradigmatic shift occurred in statistical techniques and theory. It will also be shown that WELDON's work on the shore crab in Naples and Plymouth from 1892 to 1895 not only brought them into the forefront of ideas of speciation and provided the impetus to PEARSON's earliest statistical innovations, but it also led to PEARSON shifting his professional interests from having had an established career as a mathematical physicist to developing one as a biometrician. This paper will also examine the various kinds of plant, animal and human populations that PEARSON and the biometricians grappled with from 1892 to 1932, which ultimately enabled PEARSON to construct the foundations of the new discipline of mathematics statistics in the early years of the 20th century.

• Ronald Fisher and the Development of Statistics for Biological Research,
Nancy S. Hall, U of Maryland and U of Delaware, USA

Sir Ronald A. FISHER (1890-1962) created a large body of statistical methods, designed originally for small samples, that various scientists have been using since the 1920s. In 1919 he went to work at Rothamsted Agricultural Station in Harpenden, Hertfordshire, north of greater London, where he developed new statistical methods for agricultural research programs, and thus established more

rigorous and formalized statistical methods than had existed previously. His innovations led to his work on experimental designs that enabled him to develop the analysis of variance, which included Latin squares and randomized blocks. The fundamental contribution of the analysis of variance to the discipline of modern statistics was that it allowed for the simultaneous testing of many treatments and for the study of treatment interactions. It then became possible, for example, to test three kinds of seed and five fertilizers, all in the same experiment. Among FISHER's innovations in experimental design was the requirement of randomization, for the dual purpose of elimination of bias, whether from the material or the researcher, and of a valid test of significance. FISHER's 1925 book, *Statistical Methods for Research Workers*, became a handbook that was widely used in biological research.

• Commentary, **Rob Skipper**, U of Cincinnati, USA

Friday, 11-12:30 PM, LR1
SESSION II

Evolution and Development II

Contributed Papers, Chair: **Vivette Garcia-Deister**

• Holism in Biological Development, **Jesse Love Hendrikse**, U of Calgary, Canada

Developmental systems theory stresses the context-sensitivity of developmental factors. An organism's phenotype is determined jointly by a host of factors, and the effect of each is contingent on the presence of the others. This emphasis, together with DST's deflationary attitude to the explanatory power of the gene, has led some critics to level a charge of holism against DST (KITCHER, 2001; STERELNY *et al*. 1996). I will defend DST against the charge of holism in two steps. The first argues that the context-sensitivity of a causal factor's contribution to an effect is not unique to biological cases. Explanations in physics must also contend with the fact that nature does not make it easy to decompose causal factors into individual influences. I will outline how techniques from CARTWRIGHT (1989) can be used to measure the contribution genes or other factors make to developmental outcomes. The second step reconstructs how DST can make sense of the decomposition of developmental factors into individual influences without privileging one type of factor over another. A proper evolutionary account explains how genes and other factors have evolved together such that they jointly produce certain developmental outcomes.

• Genetic Causality and the Organism, **Pieter Lemmens**, U of Nijmegen, The Netherlands

In biology, the 20th century can indeed be characterized as the 'century of the gene' (KELLER 2000). From the 'rediscovery' of MENDEL's work on peas onwards, biological explanations have increasingly become genetic, if not exclusively gene-based explanations. The gene, either as Mendelian-evolutionary—so in the neo-Darwinist research tradition—or as molecular—as in developmental genetics and more recently in evo/devo—has obtained the status of central and ultimate explanatory category. Both evolution and development came to be explained principally—in almost complete independence of each other—with reference to the genes as the fundamental theoretical entities: evolution as change in gene frequencies of populations under the influence of natural selection, gradually eventuating in the 'coding' of a 'genetic program', and development as the gene-instructed morphogenesis of phenotypes, viewed as the progressive 'decoding' of this program. Organismal evolution thus came to be conceived as the result of an overall adaptive interplay between the phenotypic powers of the genes and the adjustive forces of the environment (LEWONTIN 1984). At its most extreme, this gene centered view has led to a kind of biological metaphysics in which the genes (by fiat of natural selection) emerge as omnipotent creators and governors of life's phenomena, as an ontological *fundamentum absolutum* of biosystems tout court. Although current research in biology is still overwhelmingly dominated by gene-centered (lately: genome-centered) approaches, and although gene-based explanations of evolutionary and develop-

mental processes abound, many biologists and philosophers of biology have expressed their doubts about the eventual prospects of this insistent gene exclusivism. DNA, after all, is just one of the constituents a developing organism has at its disposal and it becomes clearer everyday that genes are not the only resources that are differentially transmitted across the generations. While some authors are still optimistic with regard to the possibility, in principle, of giving an exhaustive account of ontogenies and their phylogenies in terms of evolved (through the combined effects of selection, contingency and constraints, both positive and negative) genetic toolboxes, regulatory networks, modules or gene-based morphogenetic fields. others proclaim the end of the gene era and speculate about a future biology 'beyond the gene'. At least since the surprising data of the Human and other Genome Projects have begun to accumulate, the insight is growing that the answer to the problem of organismal form, its development and its evolution, cannot be given exclusively on the basis of stories about gene action, genetic programs and information 'in' those genes. Once thought to be the unmoved movers of the biochemical cosmos, genes are perceived nowadays ever more realistically, and therefore ever more elusive, to be just one of the many elements in this cosmos, with no special authority over the others. Genes have lost their alleged causal primacy. They are becoming decentralized and put back into their original cellular and organismal milieus: they are not self-replicating, they do not produce the phenotype, they can not be regarded as the causal origo's of development, they interact with and are conditional upon genome organization and epigenetic mechanisms, and their functioning or 'meaning' is thoroughly context-dependent. It now appears that the gene centrism of 20th century biology lived by overburdening its ultimate explanatory device with causal responsibilities, ascribing the genes, and them alone, the final, formal and efficient causalities encountered in living organisms, thereby 'alienating' (LEWONTIN) them from those organisms and depriving those organisms of every causal agency of their own. In my paper I shall first give a short outline of this 'history of alienation' by which the organism came to be 'forgotten' more and more in favour of the genes. Then I will focus upon the (mis)understanding of genetic causality implicated in this enterprise. Finally, I will argue for the return of an organism concept as indispensable, though probably not ultimately fundamental, to both evolutionary and developmental biology.

• Why Developmental Biology Outsmarted the DST, **Vivette Garcia-Deister**, UNAM, Mexico

What have come to be called 'gene-centered' explanations of development aroused a series of empirical and philosophical questions that have received detailed attention by authors like OYAMA, GRAY, GRIFFITHS, and KNIGHT, among others. These and other authors' ideas promptly built up to a critical theory of the ways and means of biological investigation that demands reference to contingency, context-dependency, and the inclusion of a broad array of causal factors in biological explanations of development. One of the main features of Developmental Systems Theory (DST) is its compromise with anti-reductionism. While the main ideas of DST have been celebrated, its all-inclusive appeal has been ctiticized for its inability to carry-out a useful research program, especially at the molecular level. In response, followers of DST such as NEUMANN-HELD (2001) and MOSS (2001) characterized each an alternative notion of 'gene' which, they claim, is useful for experimental research while it remains loyal to the purposes of DST, including the anti-reductionist consensus. This paper aims to show, through a philosophical thesis, that experimental biologists confront a world (via experimental models) that is, not only instrumentally but realistically, divided into levels of organization—thus the usefulness of reductionism as a research strategy. A concrete example will show—perhaps paradoxically—that because this configuration imposes the researcher and his goals material restrictions and constant resistance, it makes room for contingency and context-dependency. I will confront the functional notion of 'gene' that developmental biologists use with those of NEUMANN-HELD and MOSS to reveal, on the one hand, that researchers came up with similar ideas before the DSTers and in a different manner: through reductionist practice rather than anti-reductionist critique. On the other hand, I will argue that NEUMANN-HELD'S and MOSS' gene concepts may be useful to developmental biology not because of their 'openness' or loyalty to DST, but because they are liable to take part in a reductionist research strategy. The historical thesis will grant that, during the early years of a discipline, anti-

reductionism may be effective, but it will defend reductionism as the most fruitful and promising strategy in a discipline's advanced stages such as the actual situation of developmental biology.

• The Primacy of Genes in Development, **Sang Wook Yi**, Hanyang U, South Korea

It had become clear among molecular biologists since the 1970s that the developmental process of eukaryotes is much more complicated than that of prokaryotes. In particular, people realized that it is impossible to employ rather simple regulation models of bacterial genomes to explain pattern formation during the development of higher organisms. One of the consequences of this realization was that the simple notion of genetic control during phenogenesis should be changed into a more sophisticated picture of an interactive network where genetic influence is only one of many significant factors. From the complex network of developmental interactions, you may choose genetic factor as your research topic, and follow its effect along the developmental pathways. But then you may do similar research for cellular environments. So there seems to exist symmetry as regards where to locate the focus of research efforts. Some biologists and philosophers however think otherwise to various degrees. For them, the symmetry cannot exist, or does not exist, or tend not to exist. It is because genes are privileged in the ontological way, or in the epistemological way, or in the pragmatic way respectively. My paper examines each claim, and investigates whether those claims are defensible. First, I briefly consider the claim that genes are ontologically privileged because genes are the fundamental unit of selection or of inheritance. I emphasize that this issue is (to say the least) extremely controversial. We simply do not have a wide enough consensus on these ontological issues to settle whether the symmetry cannot exist or not. Second, I consider the claim that genes are epistemologically privileged because only genes, not for instance enzymes, carry information. I notice that there are a number of information concepts around, and whether genes carry information or not depends on which concept of information one may take to be biologically illuminating. Then I argue that whatever criteria one may take for information, if genes pass the criteria, the non-genetic factors do as well. Finally, I consider the claim that genes are pragmatically privileged because genes happen to be most easily controllable in the current lab situation. I admit that genetic factors became relatively expedient to manipulate thanks to the development of various molecular technologies. But I argue that controllability is a historically contingent matter, especially contingent on the future course of molecular biology research, and therefore that we should not draw a hasty methodological conclusion from our insufficient experience of direct manipulation in the developmental regulation research.

Friday, 11-12:30 PM, LR8
SESSION III

Philosophy of Cognitive Science II

Contributed Papers, Chair: **Michael Kuba**

• Mozart Effect: Can Creativity be Enhanced? **Felix T. Hong**, Wayne State U, USA

By exposing college students to MOZART's Sonata for Two Pianos in D major (K488), RAUSCHER and coworkers found that the students' performance on IQ tests for spatial-temporal tasks was enhanced. This controversial claim became known as the Mozart Effect. Perhaps the claim was not entirely unfounded. Based on a joint approach of cognitive science, artificial intelligence and biocomputing, I have recently proposed a new interpretation of Dean SIMONTON's chance-configuration theory. This interpretation makes it possible to understand why visual thinking is superior to verbal thinking and what constitutes intuition can now be articulated in less elusive terms. The interpretation also leads to a speculation regarding the Mozart effect that is experimentally testable. It was noted that MOZART, as compared to other classical symphonic composers, had a penchant of presenting multiple melodies concurrently. The exposure to such music may enhance the listeners' mental skill of parallel processing of information, thus enhancing the listeners' ability to perform spatial-temporal tasks as well as visual thinking.

• Baldwin Again and Again, **Barbara Continenza**, U of Rome, Italy

Characters individually acquired by members of a group of organisms may eventually, under the action of selection, be reinforced or replaced by similar hereditary characters. This is the essence of the so-called 'Baldwin effect'. The evolutionary theory of 'organic selection', proposed at the end of the 19th century by the American psychologist James Mark BALDWIN, stressed that mental growth is a part of biological adaptation and that organic and mental adaptation is one and the same process. 'Development' is the keyword of this theory that was for a long time ignored by psychologists and biologists judging it as a compromise between Lamarckism and Darwinism or a 'simulation' of Lamarckism by Darwinism (J. HUXLEY, E. MAYR, C. WADDINGTON, G. SIMPSON, N. TINBERGEN, J. PIAGET, and so on). 'Rediscovered' by evolutionary epistemologists (D. T. CAMPBELL, K. POPPER), the 'Baldwin effect' is now evocated more and more frequently by a great number of different authors who, starting from diffent interpretation of BALDWIN's theory, use it for different ends and in different theoretical contexts. BATESON, EDELMAN, DENNETT, DEACON are some of these 're-rediscovers'. In my paper I will try to examine some historical and theoretical aspects of the current discussion.

• I Play, Therefore What Am I? **Michael Kuba**, KLI, Austria

Working in comparative play research one faces the problem that a widely accepted definition of play behavior is still lacking. Also, many of the mammalocentristic researchers simply neglect the possibility that play behavior can be found in non mammalian species. Authors like MACLEAN (1985, 1990) stated that play behavior is one of three 'signature behaviors' separating mammals from other vertebrates. Is their skepticism only due to biological findings or is there a deeper underlying scientific belief that impairs us from looking at the very basic manifestations of play behavior? Viewing the history of play research I will try to show how their antecedents influenced contemporary play researchers. The beginning of play research can be found in ancient Greece. PLATO and ARISTOTLE believed that play was the most noble and effective form of learning. Ever since philosophers have tended to concentrate on play as a form of very elaborated verbal or non-verbal games. Pointing out the problems in the field of play research, I will show where and how scientists were influenced by each other and where a future basis of collaboration might be.

Friday, 11-12:30 PM, LR4
SESSION IV

Endangered Species, Threatened Paradigms: A Fresh Look at the Recovery of Vanishing Wildlife

Organizer: **Mark V. Barrow, Jr.**, Chair: **Matt Chew**, Arizona State U, USA

• A Passion for Peregrines: Captive Breeding of Endangered Falcons in the USA,
Helen Macdonald, Cambridge U, UK

The Peregrine Falcon (*Falco peregrinus*) had been a highly contested species in the cultures of American ornithologists, conservationists, falconers and sportsmen for many years before a DDT related population crash made it a conservation cause celebrée in the 1960s and 1970s. By this time, breeding populations east of the Rockies had disappeared entirely and the total extinction of the North American peregrine seemed increasingly likely. Following a 1965 conference at the University of Wisconsin exploring the species' decline, intense efforts to breed peregrines in captivity began. Breeding projects were led both by conservationists aiming to establish a self-perpetuating population of captive birds for eventual release back into the wild and by falconers concerned that increasing legislative protection for the peregrine would limit their access to wild birds for falconry purposes. Despite these divergent rationales, the degree of co-operation between institutional, private and falconers' projects was remarkable. Organizations such as the North American Falconers Association (NAFA) and Raptor Research Foundation (RRF) were crucial in the dissemina-

tion of information between projects and in the gradual codification of successful breeding techniques and technologies. By 1975 over 200 large falcons, mainly peregrines, were being produced each year by private 'backyard' breeders, research centers and institutional projects such as the Peregrine Fund at Cornell University.

• 'Dragons in Distress': Annals of the American Alligator,
Mark V. Barrow, Jr., Virginia Tech U, USA

Since the end of the 18th century, when the Quaker naturalist William BARTRAM published a particularly harrowing account of his encounter with the American alligator (*Alligator mississipiensi*) during his travels across the southeastern USA, this species has captivated the imagination of naturalists and the broader public alike. Yet, within a century after BARTRAM depicted this large, ancient reptile, its fortunes had greatly declined. Overhunting for the hide trade, destruction of the wetland habitat that served as its home, and systematic slaughter at the hands of local residents who feared this imposing predator (which could grow to nearly more than 16 feet in length) began to greatly thin its numbers. By the first half of the 20th century, these and other threats to the alligator had vastly amplified, while during this same period American naturalists—like A. M. REESE, E. A. MCILHENNY, and (later) Archie CARR—began to study its life history and document its declining status. Despite a lack of public support for the species, these naturalists and colleagues succeeded in having it officially protected through a series of state and local laws. When illegal trade in alligator skins continued despite this nominal protection, naturalists and wildlife conservationists pushed to have the species preserved under the terms of the first Endangered Species Act (1966)—much to the consternation of state officials who resented federal intervention into wildlife management within their borders—and the much more sweeping revised version of the act that passed seven years later. With federal protection and growing public support, the alligator population quickly rebounded to the point that it began regularly appearing in lakes, on golf courses, and even in swimming pools across the southernmost portion of its range. In parts of Florida and Louisiana, this once officially endangered species now is often considered a nuisance and a threat. This paper explores the role that naturalists played in rescuing the American alligator and the numerous controversies that swirled around its study, protection, and recovery.

Friday, 11-12:30 PM, LR6
SESSION V

Recent Work on Pluralism and the Levels of Selection

Organizer and Chair: **Rob Wilson**

• The 'Averaging Fallacy' and the Levels of Selection,
Samir Okasha, U of York, UK, National University of Mexico

This paper compares two well-known arguments in the units of selection literature, one due to SOBER and LEWONTIN (1982), the other due to SOBER and WILSON (1998). Both arguments concern the legitimacy of 'averaging' fitness values across contexts and making inferences about the level of selection on that basis. I show that the two arguments are incompatible, their apparent similarity notwithstanding. If we accept SOBER and LEWONTIN'S criterion for when averaging genic fitnesses across diploid genotypes is illegitmate, we cannot accept SOBER and WILSON's criterion for when averaging individual fitnesses across groups is illegitimate, and vice versa.

• Collections or Collectives? Perspectives, Parameterization and Pluralism in the
Levels of Selection Debate, **Ben Kerr**, U of Minnnesota, USA

The concept of group selection has had a rough, tumultuous history. George WILLIAMS' famous book all but buried the concept; however, David Sloan WILSON's analysis of intrademic selection in structured populations gave multi-level selection theory a second chance at life. Using

WILSON's basic framework, we claim that some of the debate has centered on how groups are treated: either as the milieu affecting individual fitness or as fitness-bearing entities in their own right. These two treatments correspond to a type of individualism and 'multi-levelism,' respectively. We rigorously develop these different points of view through alternative parameterizations of the population-structured selective process and then show that the views are mathematically equivalent. We argue that there are advantages to taking both points of view when approaching selection in a population-structured situation. Thus, we advocate 'gestalt-switching' pluralism.

• Pluralism, Entwinement, and the Levels of Selection, **Rob Wilson**, U of Alberta, Canada

This paper distinguishes and critiques several forms of pluralism about the levels of selection, and introduces a novel way of thinking about the biological properties and processes typically conceptualized in terms of distinct levels. In particular, 'levels' should be thought of as being *entwined* or *fused*. Since the pluralism discussed is held by divergent theorists, the argument has implications for many positions in the debate over the units of selection. And since the key points on which the paper turns apply beyond this specific issue, the paper may prove of general interest in thinking about the metaphysics of science.

Friday, 11-12:30 PM, LR10
SESSION VI

Metaphor and Communication II

Organizer and Chair: **Brendon Larson**

• Metaphors and the Dynamics of Knowledge: 'Genetic Code' and 'Information' in German Molecular Biology During the 1950s and '60s, **Christina Brandt**, Max Planck Institute for the History of Science, Germany

This paper addresses the epistemic role of metaphorical concepts like 'biological information' and 'genetic code' in the history of molecular biology by exploring the interactions between metaphorical representations and experimental practices. In the first part of the paper I will briefly discuss different theories of the use of metaphors in science. The second part deals with a historical case study: the focus will be on the work of the German biochemist Gerhard SCHRAMM. During the 1950s SCHRAMM's research on the tobacco mosaic virus made important contributions to the emerging molecular biology. In the early 1960s SCHRAMM started to investigate the biochemical origins of life. The metaphors of 'code', 'information', and especially scriptural representations of heredity played a crucial part in the course of SCHRAMM's research: Developing from popularizing illustrations (in the 1950s) to experimental entities (in the 1960s), these metaphors were eventually incorporated into the philosophical core of SCHRAMM's concept of a 'new biology' which reevaluated longstanding questions concerning the essence of life. With respect to this case, I argue that metaphors are not merely means of popularization but rather representations with an operational force of their own.

• Anthropomorphism in Cladograms, **Hanno Sandvik**, U of Tromsø

The understanding of evolution is often impaired by anthropomorphic thinking. Most laypeople's picture of evolution is both essentialist and teleological, and 'evolution' is more or less understood as a metaphor for 'anthropogenesis'. Among professional evolutionary biologists, it is acknowledged that essentialist and/or teleological thinking is an inadequate tool for representing evolution. Nevertheless, unconsciously or implicitly such anthropomorphic thinking often enters into evolutionary narratives even among professionals. O'HARA has reviewed some of the narrative devices that introduce anthropomorphisms into evolutionary trees. Most of these are avoided when using cladograms instead of diagrams, phylograms, cartoons, or the like. Therefore, the advance of cladism has greatly reduced the degree to which anthropomorphic thinking can distort presenta-

tions of evolutionary history. There are two devices left, however, which are even more subtle than the ones avoided by cladograms. These are differential resolution and sort order. Both potentially reinforce, and are reinforced by, the 'anthropogenesis' metaphor of evolution. The two narrative devices are explained, and their potential to distort the understanding of evolution is exemplified. In addition, an investigation of the frequency of both anthropomorphisms in recent text books in systematic zoology is presented. The relevance of these insights for teaching phylogenetics is discussed.

• Natural Selection in Cosmology, **Zachary Myers**, U of Maryland, USA

According to Marjorie GRENE, a reason why DARWIN's theory of natural selection has been so convincing is because it provides an explanation of evolution in terms of the mechanistic tradition of explanation, which is to say it is both logically simple and automatic. It is logically simple in so far as it unites many disparate phenomena under a single hypothesis and it is automatic since it interprets organisms as having evolved from random errors (i.e., genetic variations or mutations) on one hand and by blind and automatic control of environmental pressures on the other. Given the success of the theory, theorists in fields other than biology have continued to explore the extent to which it can be applied. In his book *The Life of the Cosmos*, Lee SMOLIN gives an extensive account of why the universe has evolved the way it has. In doing this he begins by suggesting that (the values of) the cosmological parameters that have made the universe so diversely structured and hospitable to life, are the products of what he calls 'cosmological natural selection'. This view stands as an alternative to and goes beyond the anthropic principle which states that out all the possible universes that may have been created, we are fortunately living in the one that has made the development of intelligent life possible. While the anthropic principle states what may have happened, SMOLIN offers a means of explaining how it may have happened. According to SMOLIN, given the tendencies for black holes to collect and condense huge quantities of matter, the extremely condensed state of matter in its core reflects a similar state of condensed matter that created the Big Bang. So instead of the black hole's core creating a singularity (a point of infinitely condensed matter) it creates a new big bang, which gives rise to a baby universe. Thus, the more black holes a universe has, the more offspring universes it will reproduce. SMOLIN's theory makes the analogy of physical parameters to biological genes. The space of parameters is compared to the collection of all possible sequences of DNA. The average number of offspring universes produced by a universe with a particular set of parameters is compared to the average number of offspring from creatures with a particular set of genes. This 'fitness' of creatures in both domains depends on their position on a 'fitness landscape' in an abstract space of genes or parameters. The rate of reproduction is measured for how strongly it varies with variations in possible gene or parameter combinations. As consistent as his theory is with its initial postulates it can at best only be seen as analogous to DARWIN's theory of natural selection, and so determining how well the mechanism of natural selection can be applied to the universe as a whole will ultimately depend on the specific question one is proposing to answer. This paper will examine the parallels between cosmology and biology, and then analyze some places in his theory where the natural selection analogy breaks down. However, despite these shortcomings SMOLIN ultimately succeeds in his efforts to employ the mechanism of natural selection to help explain the specific phenomenon in question: How is it that the parameters of the standard model of fundamental physics are so perfectly tuned, despite the overwhelming improbabilities, so as to make it possible for us to evolve as we have?

Friday, 11-12:30 PM, LR9
SESSION VII

Genes, Genomes, and Genetic Elements II:
Evolutionary Implications of New Developments in Molecular Genetics

Organizer and Chair: **Karola Stotz**, U of Pittsburgh, USA

• The Evolutionary Significance of Alternative Splicing, **Stephen M. Downes**, U of Utah, USA

Alternative splicing allows for the production of many gene products from a single coding sequence. I introduce the concept of alternative splicing via some examples. I then discuss some current hypotheses about the role of splicing, including the claim that spicing is a significant contributor to the complexity of the human genome. Hypotheses such as these bring into question our working concepts of the gene. If much of the expressive capability of the genome is produced by post-transcriptional controls such as splicing, then the implicit referent of our gene concept may include more than any particular DNA coding sequence. Next I briefly introducing some hypotheses about the evolution of mechanisms alternative splicing in higher organisms. I conclude that attention to post-transcriptional control mechanisms should play a central role in the developing philosophy of molecular biology.

• "The Gene is Dead, Long Live the Genome," or Isn't It? **Raphael Falk**, The Hebrew U, Israel

Developments in the sequencing of whole genomes and in simultaneously surveying micro-arrays of many thousands of translation products of specific cells have emphasized the futility of attempts to identify genes as structurally discrete entities along the genome. Up to a thousand 'transcriptons', alternatively spliced and edited RNA products of given DNA sequences, were identified. Returning to Johannsen's definition of a gene as 'something' that refers to an *invariant entity of inheritance and development*, we may view genes either as generic terms for units of inheritance whose referents are pragmatic ad hoc and context-dependent, or as (epistemologically) representing entities of cell functions. Specific functions are essential for or conducive to the survival of cells (and of whole organisms), and structures that happened to secure such functions are selected for. In other words, it is cellular functions that determine the structural referents along the DNA. To the extent that genes assume specific structures, these are the consequences of the evolutionary pressures of cellular and organismic functions. This is not to exclude that DNA sequences constraint and facilitate functions of the genome as a system.

Function, in terms of 'in order to' is a teleological formulation of reverse causation. But, as emphasized by WRIGHT, it may merely signify that something it stands for is natural (not preprogrammed, yet not random), being of general relevance rather than a one-time haphazard relation. "Functional explanations are in some sense etiological, concerning the causal background of the phenomenon under consideration." The etiological, causal background of genes as functions is natural selection. Whatever sequence of DNA available that would provide the functions (even if not 'perfect' in retrospect) would do. Genes in the age of the genome obtain again their theoretical role as intervening variables, abstractive variables that purely 'summarize' characters. The importance of DNA sequences is that of all possible phenotypes these are the most basic ones from which we can read off the genotype directly.

• Commentary, **Lenny Moss**, U of Notre Dame, USA

Friday, 11-12:30 PM, LR3
SESSION VIII

Functions and Teleological Explanation II

Contributed Papers, Chair: **Stefan Linquist**

• Function and Accidental Doubles, **Yuichi Amitani**

This presentation deals with one criticism toward the etiological theory of natural function. Ruth G. MILLIKAN referred to the possibility of accidental doubles of functional items as one of criticisms against her theory of proper function. This possibility can be a counterexample to other functional analyses based on the history of functional items. But etiological theorists generally have not taken this possibility seriously, because they have thought that this is merely a 'logical' or 'philosophical' possibility. This presentation will try to show that this is not the case and that since the criticism miss the naturalistic character of the theory, it will not be a critical counterargument against the etiological theory.

• Modules as Mechanisms, **Stefan Linquist**, Duke U, USA

One thing DOBZHANSKY might have meant by his famous dictum, "nothing in biology makes sense except in light of evolution," is that if a biological system can be interpreted functionally, it must be understood in terms of evolution by natural selection. If this is what he meant, it raises an important question: What are the limits of functional interpretation? Recently, a discipline that interprets the human mind in terms of its evolved functions has been gaining momentum. Evolutionary psychologists propose to explain the proximate structure of the mind in terms of a plethora of computational modules, each one being individuated by its (alleged) adaptive function. In this paper I challenge the idea that modules, defined functionally, are a type of proximate mechanism. Following an argument that William JAMES raised AGAINST GALL—the phrenologist and grandfather of modularity theory—I argue that functionally described 'mechanisms' cannot serve as the basis for proximate psychological explanations. I also entertain the recent suggestion that the evolutionary psychologists' program can be rescued with the notion of a 'virtual module'. By relinquishing their aim of identifying proximate mechanisms, evolutionary psychologists can potentially immunize themselves against such objections.

• Revisiting Bohr and Delbrück, **Daniel J. McKaughan**

The impact of Niels BOHR's 1932 "Light and Life" lecture on Max DELBRÜCK's lifelong search for a form of 'complementarity' in biology is well documented and much discussed (cf. FISHER and LIPSON, 1988; KAY, 1985; ROLL-HANSEN, 2000; STENT 1998), but the precise nature of that influence remains subject to misunderstanding. I argue that the standard reading, which sees DELBRÜCK's transition from physics into biology as inspired by the hope that investigation of biological phenomena might lead to a breakthrough discovery of new laws of physics, is colored much more by SCHRÖDINGER's gloss on these issues in *What is Life?* (1944) than is often acknowledged. Following ROLL-HANSEN (2000) in locating BOHR within the Kantian teleomechanical tradition, I contend that BOHR expected that purposive and functional aspects of biological phenomena could not be made intelligible on a strictly mechanistic approach. BOHR envisioned an analysis employing teleological notions as basic explanatory concepts and predicted that mutually exclusive teleological and mechanistic approaches would be jointly necessary for an exhaustive understanding of life. Although DELBRÜCK took a more applied empirical and less self-consciously philosophical approach, he shared BOHR's hope that a detailed scientific investigation would vindicate the view that at least some aspects of life are not reducible to the terms of either classical mechanistic models or to quantum physics. DELBRÜCK did not, as James WATSON, Gunther STENT, and others suggest, pursue a physical and chemical analysis of biological phenomena in anticipation that investigation would lead to advances in physics such that functional features could be understood in entirely mechanistic terms. Rather a central motivation for DEL-

BRÜCK's research was the hope of encountering essential limitations in the reductionist project that would require supplementation by a perspective employing purposive concepts.

Friday, 11-12:30 PM, LR5
SESSION IX
Biology and Ethics/Epistemology II
Contributed Papers, Chair: **Michael Bradie**

• Developmental Systems Theory and the Evolution of Morality,
Luke Russell, U of Sydney, Australia

Evolutionary explanations of moral behaviour (EEMB) have been rejected by many philosophers on the basis of the following argument: (1) Moral behaviour is clearly influenced by many non-genetic factors, from upbringing to argument and deliberation. (2) Adaptations are determined by genetic factors alone. Therefore (3) Moral behaviour cannot be an adaptation. Advocates of EEMB often attack (1), i.e., they try to show that, even though particular instances of behaviour are influenced by environmental conditions, the underlying mental modules that cause moral behaviour are inherited via genes, and are unalterable. However, Developmental Systems Theory (DST) suggests a different method of defending EEMB. Since DST advocates a much broader and looser conception of inheritance, it denies (2). In this paper I consider whether DST makes EEMB more plausible, and whether the DST version of EEMB differs significantly from non-evolutionary explanations of moral behaviour.

• Sociobiological Imperialism and the State of Epistemology and Ethics,
Michael Bradie, Bowling Green State U, USA

In 1975, E. O. WILSON published *Sociobiology* in which he sought to codify studies of the biological basis of animal behavior. By so doing he envisioned providing the tools for incorporating the human social sciences into the Modern Synthesis. In the first chapter, "The Morality of the Gene," he suggests that "ethical philosophers who wish to intuit the standards of good and evil" and yet fail to consider the evolutionary foundations of the biological systems that make moral reasoning possible are barking up the wrong tree. Appeals to natural selection, it was suggested, will "explain ethics and ethical philosophers, if not epistemology and epistemologists, at all depths." WILSON does not have much to say about epistemology in this book but, in the last chapter, the section subtitled "Ethics" begins with the following proposal: "Scientists and humanists should consider together the possibility that the time has come for ethics to be removed temporarily from the hands of the philosophers and biologized." In effect, WILSON was enjoining ethicists and epistemologists to take a hiatus until the sociobiologists could give a proper account of the nature of morality and human knowledge. Despite WILSON's injunction for epistemologists to cease and desist, they continued on their merry way, untouched by charges of irrelevance. Twenty-six years later, John ALCOCK pronounced the "triumph of sociobiology." Is this the death knell to philosophical ethics and epistemology? In this paper, I explore the problem of normativity in ethics and epistemology and assess the degree to which any proclaimed sociobiological 'triumph' can be said to be complete.

• The Normative Demand in Evolutionary Epistemology, **Paola Hernández Cháves**

This paper aims to offer one possible response to the demand for normativity in evolutionary epistemology (EE). It has been argued that as far as EE is devoted to accounting solely the facts of evolution it cannot offer any answer to the epistemological concern with giving and offering reasons, which is the defining labor of epistemology, i.e., justifying. If this is correct, they claim, EE cannot be properly called 'epistemology'. There have been different attempts to solve this controversy. Some of them have tried to minimize the role of normativity, while others have tried to

come to terms with it. I will assess those trials. Then I will try to show that the critics misunderstand the purpose of EE, and, as if this were not enough, they undermine epistemology itself.

Friday, 11-12:30 PM, LR2
SESSION X

History, Philosophy, and Social Studies of 18th-19th Century Biology I

Contributed Papers, Chair: **Jenny Beckman**

• The Behemoth of the Pampas, **Irina Podgorny**, CONICET, Argentina and **Fernando Ramirez**, CNRS, France

The Megatherium, a South-American fossil mammal found in the Pampas of present Argentina at the turn of the 18th century, was the first fossil to be known having no representatives in modern fauna. Its description has been taken as the starting point of vertebrate paleontology. The aim of this work is to analyze the role played by the Megatherium in the early 19th century debates on the anatomy of animals and the building of the anatomy of fossil fauna. In his search to explain animal anatomy, CUVIER proposed two principles 'conditions of existence' and 'subordination characters' which basically reflects Cuvier's anatomy conception that any anatomical character has a function. With a different conception, Geoffroy SAINT-HILAIRE proposed that anatomy was based not on the function but on the structure—the real subjet of the anatomy—, being the function a secondary modification. Cuvier included the Megatherium into the mammal order of Edentates and compared its skull anatomy to that of modern sloths. Based on this comparison and on Buffon's description of sloths, structuralists proposed the Megatherium as an example where anatomical parts don't play any function. The Megatherium became thus an argument to refuse Cuvier's anatomy conception. Buckland, in the frame of Natural Theology and a proclaimed follower of Cuvier, supported the functional view of anatomy by carefully describing the details of the function of Megatherium's every single anatomical part in a long chapter of his volume of the Bridgewater Treatises. The Megatherium played an important role in the discussion between functionalists and structuralists. The building of the anatomy of Megatherium was therefore subjected to this theoretical debates, but also to the problems linked with the kind evidence which supported it.

• Science or Poetry: Vernacular Plant Names and Education in Sweden c. 1900, **Jenny Beckman**, Uppsala U, Sweden

My paper deals with the standardisation of vernacular botanical nomenclature in Sweden around 1900, and its use as a tool in primary and secondary education and to encourage botanical interest in the wider community. Though lists of vernacular names of plants and animals had been collected at least since the 18th century—LINNAEUS being the most famous collector of the period—it was not until the late 19th century that attempts were made to distil these into a single list of national plant names, and to name those plants (the majority) that had no vernacular names. The focus of this study is the heated debate about Swedish plant names that raged through scientific and pedagogical journals in the early years of the 20th century.

The importance of nomenclature as a means of communication among scientists need hardly be stressed; and the late 19th century is also a period of increased standardisation in scientific biological nomenclature. However, it can be argued that naming played an even more central role among non-academic botanists, for whom the ability to name—in Latin and in the vernacular—plants was essential for the demonstration and use of their knowledge (cf. Anne SECORD's work on artisan botanists). Through vernacular names, notions of classification and natural relationships could be disseminated outside the universities; while at the same time a channel was established through which non-academic plant knowledge could be rendered useful to university botanists.

• The Dutch Debate on the Doctrine of Bacteriology, 1875-1900, **A. de Knecht-van Eekelen**

In the last decades of the 19th century the world witnessed the rise of bacteriology. However, in the Netherlands there were influential opponents to the 'bacteriological doctrine'. In leading medical journals as well as in the popular press medical doctors expressed their doubts about the role of bacteria as specific elements of disease. The work of Robert KOCH was neglected for more than ten years while on the other hand some spokesmen adhered to the theory of abiogenesis. This paper discusses the background of this debate in the Netherlands and shows its influence on the position of medical doctors in the scientific community.

Friday, 2-3:30 PM

SESSION I, LR8
Friday, 11-12:30 PM

Biology and Anthropology I

Organizers: **Eva M. Neumann-Held** and **Mathias Gutmann**, Chair: **Eva M. Neumann-Held**

• Technique as the Form of Human Action: Humans as Self-developing and Self-conceptualizing Entities, **Mathias Gutmann**, U of Marburg, Germany

The definition of humans usually refers to a comparison with animals. Compared e.g., with primates, humans are defined as zoon logon echon, animal sociale or tool-making animals. From a methodological point of view this definition of humans refers to a predicative conceptual scheme. Consequently, technique becomes a simple predicate that denotes a specific feature of *Homo sapiens* and the naturalisation of technique seems to be quite inevitable. An alternative may be gained, by characterising technique as the 'form of human action'. From this point of view, tool-making and the use of tools is a medium of human self-development and one of the main preconditions of human self-conceptualisation. As a result of this dialectical reinterpretation a new methodological starting point for the understanding of the development of human abilities and skills can be gained, by avoiding some naturalistic shortcomings of the comparison of animals and humans.

• Early Hominids as Tool-users, **Christine Hertler**, Goethe U, Germany, and **Friedemann Schrenk**, Senckenberg Institute, Germany

The origin of the genus Homo is currently associated with changes in early hominid lifestyles evoked by changing environmental conditions in East Africa. One way to cope with the challenges consisted in the rearrangement of relations to the environment including new ways of tool usage. The changing lifestyles in turn resulted in corresponding anatomical developments, ultimately leading to the unique way of life of modern *Homo sapiens*.

• What is a 'Human Being?' **Eva M. Neumann-Held**, U of Marburg, Germany

Technological progress in the so-called life sciences increasingly allows us to analyse, predict and manipulate the composition and development of life forms, including human beings. Therefore, it might seem an obvious consequence that the reflective norms, which supply arguments in law and ethics, refer to scientific knowledge to secure criteria for determining not only the beginning and end of 'a life', but criteria for determining what it means to be a human being, also. 'Human being' is understood as nothing other than a biological category. However, whereas it is undoubtedly true that *Homo sapiens sapiens* is a taxonomic category, in this talk it will be questioned whether the criteria for such taxonomic sorting really supply a comprehensive tool for determining 'what is

human' in non-biological contexts. Furthermore, I will argue that some shared understanding of 'human being' must precede any biological description.

Friday, 2-3:30 PM, LR5
SESSION II
The Mutual Shaping of Science and Science Education I: Classroom Perspectives and Practices
Organizer and Chair: **Steve Fifield**

• Eugenics On-line Archive as an Educational Research Tool, **Garland E. Allen**, Harvard U, USA

The study of eugenics has enjoyed considerable relevance in the history of various aspects of the life sciences, sociology of science, and science and society courses in recent years. Other than the usual published sources, however, undergraduate students seldom get the opportunity to examine archival documents such as personal and professional letters, photographs, eugenic interview forms, or fieldworker reports. With a grant from the NIH ELSI panel, the Cold Spring Harbor Laboratory has organized an on-line eugenic image archive with thousands of items relating to the history of eugenics in the USA (the collection is now expanding to Europe). The images are grouped into topical areas, preceded by short background essays as introductions. Images can be downloaded and printed out for ready reference. This presentation will be aimed at showing how this resource can be used to teach students archival research methods as well as offering them the opportunity to carry out specific projects in the history of eugenics.

• ISHKABIBBLE in Teaching 'Molecular and Cell Biology', **Muriel Lederman**, Virginia Tech U, USA

I am teaching a sophomore-level cell and molecular biology course for biology majors that I developed. The course requires students to see science in a new way, including the nature of the world of science and the relationship between science and the rest of the world, in addition to scientific content. Three senses of scientific 'cultures' can be identified; the institutions of science, the interaction of science and society, and the internal aspects of how science is constructed and practiced. Advocates for scientific literacy support the inclusion of the first two aspects of 'cultures' in teacher training and undergraduate education. The third aspect is largely neglected and may be the key to gender equity in science. I will discuss the course content, the goals for the required writing assignments, and a preliminary assessment.

• Teaching Climate Change Theory From the Point of View of Biological Education: An Interdisciplinarsy Approach, **Marcia Cristina Espineira Dias**, Cidade Universitária, Rio de Janeiro

Climate change science occupies a growing space in the discussion of the environmental situation of the Earth. This paper intends to investigate the broad net of knowledge involved in climate change science and how it can bear on the teaching of this science in the context of the biological sciences. The science of the Earth and its climate is strongly coupled to the basic sciences of physics, chemistry and biology. The integration of all these fields of knowledge is very complicated, both in research and in teaching. This need for integration is an old question, marking the field since its emergence at the end of the 19th century. In that time, it was already hard to convince the scientific community about the need to integrate different sciences in order to understand the Earth's climate. Now, the same situation appears when one conceives of the goal of building a good approach to teach students about the science of climate change. Moreover, it is very important to use a hi!storical approach to understand the construction of this science. In biology, climate change science is very close in its approach to the environmental sciences. Teaching about climate change in biology classes generally takes place in connection with environmental issues. These is-

sues have social, economic and political dimensions, which also influence the development of climate change research. To teach effectively about issues of climate change, biology teachers must have a background in these related fields and have access to ongoing teacher training. Agencies engaged in climate science research need to help make the results of this research available to teachers.

• Instructor's Practices In, and Attitudes Towards, Teaching Ethics in the Genetics Classroom,
Jinnie Garrett and **Joan Booth**, Hamilton College, USA

Developments in the science of genetics have many deep and far-reaching consequences in society. However, most science students receive little training in ethics and are thus poorly prepared to deal with, or advise about, the social consequences of progress in genetics. We were interested in determining both the quantity of ethics taught in undergraduate genetics courses and the instructional methods used to cover ethical topics. We were also interested in determining the attitudes of genetics instructors to the inclusion of ethical issues in their course. Are they satisfied with their treatment of ethical issues? If not, why not? In order to gain insight into these questions, copies of a survey were mailed last summer to approximately 500 national universities and liberal arts colleges located in the USA One hundred and fifty-one responses were relevant in our study. The majority of the professors who responded expressed a belief that students should be exposed to ethics in the science classroom, and most instructors do attempt to introduce the subject into their course in some form or another. However, most also responded that they do not believe they devote adequate time and/or attention to ethical issues citing 'lack of time' as the most common reason for this situation. Overall, we found support for far more exposure of students to ethical issues in the genetics class, and as part of their undergraduate education, than actually occurs. Instructors must therefore value all of the genetics content more than ethical issues they could raise.

Friday, 2-3:30 PM, LR6
SESSION III

Visual Zoology on Wall Charts

Organizer and Chair: **Helmut Zacharias**

• Leukart's Zoological Wall Charts Recovered at Pavia University,
Ernesto Capanna, Roma U La Sapienza, Italy

The cellars of Palazzo Botta, the old institute of zoology at the University of Pavia, recently unraveled a remarkable treasure. A voluminous chest with 18 drawers contained 110 large color lithographs. In 1877, Rudolf LEUCKART together with Hinrich NITSCHE initiated the edition of these zoological wall charts, which were used as visual aids all over the world. An obituary in *Nature* (1898) attested that LEUCKART (1822-1898) was a pioneer of parasitology, an authority on invertebrate animals, a dedicated teacher, one of the most eminent figures in the zoological world. At Pavia University, Pietro PAVESI (1844-1907) acquired the rather expensive set of charts when he was professor of zoology and director of the institute of zoology from 1875 to 1907. PAVESI's acquisition, the Pavia collection, is an outstanding example of an universal aid of scientific education that has been conserved almost completely. Now, these lithographic *Wandtafeln* with dimensions of approximately 1000 x 1400 mm were restored and their reproductions published in a representative pictorial atlas. We outlined the interesting biographies of the 22 expert authors. The edition is supplemented with a CD-ROM comprising not only the charts but also the original explanations, most of them in English, French and German. Here, we will show some depicted examples of invertebrate animals. The beginning of developmental biology, the teaching of 'scientific' zoology and further important items from 18th century biology will be discussed.

• Contributors from Vienna to Leukart's Zoological Wall Charts,
Helmut Zacharias, Pavia U, Italy

Rudolf LEUCKART (1822-1898) initiated the edition of 113 zoological Wandtafeln when he was professor of zoology and zootomy at the Royal Saxon University in Leipzig. An almost complete set of 110 charts was recently found at Pavia University (Redi *et al.* 2002, Capanna this ISHPSSB meeting). The artwork and the explanations to the charts originated during the period 1877-1900 by a total of 22 authors. Two of them were Karl GROBBEN (1854-1945) and Berthold HATSCHEK (1854-1941), both from Moravia, both temporary members of the Austrian-Hungarian Polar Expedition 1890-1898, and both professors of zoology at Vienna University.

GROBBEN made two contributions to the zoological wall charts showing *Lamellibranchiata*. *Pecten* (scallop) and *Mytilus* (common mussel) dominate on chart C077, whereas *Cardium* (common cockle), *Venus verrucosa* (warted Venus shell) and the soft-shell clam *Mya truncata* are shown on C089. GROBBEN spent all his academic life in Vienna where he became professor and director of the zootomical institute 1893, and director of the First zoological institute 1896. He coined the correlative terms Protostomia and Deuterostomia in a great review on the animal kingdom 1908. GROBBEN revised a zoological textbook that Carl CLAUS (1835-1900) had initiated and later earned international reputation with many editions by Alfred KÜHN. The city of Vienna awarded GROBBEN the price of Prince Eugene 1944. With his home destroyed in WWII, the old professor tried to flee to Tyrol, but he had an accident. Thus, he was taken to a hospital in the city of Salzburg where GROBBEN died on 13 April 1945. HATSCHEK heard lectures by Carl CLAUS in Vienna, Rudolf LEUCKART in Leipzig and Ernst HAECKEL in Jena. Leipzig University awarded him a PhD for his study on the development of Lepidoptera in 1877. The HATSCHEK hypothesis about the Trochophora unified the Scolecida, Articulata and Mollusca within the novel group of Zygoneura.

HATSCHEK became professor of zoology at HAECKEL's recommendation at Prague University 1886-1896. The marine animals, which permanently retain the chordate characteristics of metameric segmentation, notochord, dorsal nerve chord and gill slits, were named Cephalochordata or Acrania. HATSCHEK typified this group according to Amphioxus lanceolatus and drew the model organism in his only chart, C072, from which we learned the fundamental plan of vertebrate organization. Against the opposition of Carl CLAUS, HATSCHEK moved to Vienna to head the Second Zoological Institute as from 1896. Thus he became a close colleague to GROBBEN. The Academy of Sciences in Vienna made HATSCHEK a fellow only in 1932. However, the Nazis expelled him from the Academy and, furthermore, deprived him of his property. He died lonely in Vienna on 18 January 1941.

• Leuckart's Zoological Wall Charts—Now Cyber Charts,
Cathy Norton, Woods Hole Marine Biological Laboratory, USA

For the past few decades, in the dark recesses of the MBL/WHOI library stacks, the LEUCKART wall charts were stored in cylinders and brought out occasionally for 'show' by librarians giving tours. These charts were used in the early summer courses at the Marine Biological Laboratory and then 'preserved' in the library. In the Festschrift, published in 1892, commemorating LEUCKART's 70th birthday: it contained the names of the 9 Americans who were students or fellow scientists. Seven of the nine American named were directly associated with the MBL during its formative years notably, Charles Otis WHITMAN, the first director of the Marine Biological Laboratory (1888), who introduced courses in zoology, botany and physiology in those early years. In 1995, a former MBL physiology student donated a nearly complete set of the charts to the library. It was at this time that the library was bringing on-line a web site and decided to test on-line image collections and thought one of the best methods of conservation for some of the damaged charts would be to digitized them and post them to the web. Both WHITMAN and LEUCKART would have been stunned at the results of this 'web publication'. More than 100,000 hits were made to the site the first 3 months it went on-line, and in August of 1996 this web site was featured on the cover of *Science*. In the summer of 2002 a celebration of these chromolithographs was put on exhibit in the

Swope Conference Center at the Laboratory in larger than life- size photographs and will soon be available on our MBL web site.

Friday, 2-3:30 PM, LR4
SESSION IV

Biology, Biotechnology and Policy

Organizer and Chair: **David Castle**

• Adoption and Governance of Biotechnology in Democracies,
Keith Culver, U of New Brunswick, Canada

Applications of the results of the science of genomics raise a range of complex policy issues. Commercial application of genomics in products such as transgene farmed salmon raise a range of ethical, scientific, and policy issues related to the welfare of the fish, its ecological impact in aquaculture sites, and public interest in balancing these costs and risk against the desire for this new kind of food protein. To address these issues we need new ways of educating ourselves, and perhaps new ways of coming together to handle these issues in local communities where local expertise and needs play a crucial determining role in choice of normative regimes and aquaculture practices. The new information communication technologies (ICTs) provide new tools for information, consultation, modelling and comparison of policy options. This paper evaluates emerging models for use of new ICTs to connect and bridge the differences between research, policy, and local community cultures. These models aim to help research, policy, and aquaculture communities in their evaluation of competing accounts of the risks and benefits of aquaculture and the kind of normative regime required to promote maximum benefit from aquaculture.

• Bioremediation and the Ecology of Fakes, **David Castle**, U of Guelph, Canada

Restoration ecology is a nascent field in which biotechnology is finding an important role. Policy makers expect ecologists to characterized the original state of sites intended for restoration, but establishing the original is less an act of recall and more an act of creating a bounded entity and integrating it into a coherent natural history. Biotechnology provides an engineering solution which satisfies questions of technical feasibility. In a context of biotechnological determinism, feasibility becomes an exclusionary reason for accepting an 'Eden Principle' that bridges the fact-value gap and justifies the restorationist program. This principle appears to do a great deal of work in restoration ecology. To what extent does it provide a coherent picture of ecology, and does it provide adequate normative grounds for environmental policy?

• The Object in Risk Assessment, **Bill Hannah**, U of Guelph, Canada

There is a complicated setting for the introduction of transgenic fish: they are a new, controversial technology being introduced into an unstable, unstudied natural environment. Risk assessment bears the burden of establishing and managing the evidence needed for appropriate policy formation. The relationship that risk assessment has with both the models that serve as its evidence and the policies that are produced is therefore very interesting. I intend to look at these relationships with an eye on the interplay between value considerations (normally attributed to the policy and management side) and evidential/fact considerations (commonly held to be found on the assessment, modelling side). It can be shown the nature/culture dichotomy seems to blur in the creation of biotechnology, the fact/value dichotomy collapses in its assessment. There is a very complicated interplay between the values and the facts. This dichotomy is far too simple to reflect what is going on in the policy environment. I hope to show one way in which values and facts are relating in the risk assessment of transgenic fish. Ecological modelling, contentious evidence to begin with, serves as evidence in ecological risk assessment. I hope to show that policy bias is affecting this evidence (the models), the assessment (where the models fit in risk schemes), and the policies.

Models, risk schemes, and policies necessarily refer to one particular entity. I will argue that in the case of ecological risk assessment of transgenic fish and arguably in other cases the entity in question is ambiguous as one moves through the process. This ambiguity allows bias to push the evidence to favour moratoriums/bans, or to favour acceptance.

Friday, 2-3:30 PM, LR9
SESSION V

Evolution and Politics I

Organizer: **Naomi Beck**, Chair: **Paul Weindling**, Oxford Brookes U, UK

• The Politics of French Evolution Theories, 1790-1810, **Pietro Corsi**, U of Paris, France

LAMARCK's 1800 proposal of a sketchy evolutionary thesis capable of explaining in genetic terms the tree of life, and the more systematic doctrine he expounded in his 1802 *Recherches sur l'organisation des corps vivants*, were by no means the only pronouncements on the subject to appear in France under the 'Directoire' regime. From 1795 through 1802, several naturalists expressed similar views. Yet, rare mention is made of authors and works that attracted considerable attention at the time. In view of later developments, only LAMARCK's work has been credited with the dubious title of 'precursor', with reference to the explosion of evolutionary ideas at the European level as from the 1830s. It will be argued that complex political reasons were at the basis of the fate of early evolutionary doctrines in Revolutionary and Imperial France, as well as of their resurgence during the Restoration.

• The Battle Over Evolution in Germany: Ernst Haeckel's Struggles with the Religious Right and Political Left, **Robert J. Richards**, U of Chicago, USA

Ernst HAECKEL argued, from a scientific vantage, the dangers of mixing religion in affairs of science and the state. His opposition to religion and leftist politics embroiled him in struggles with several religious groups (e.g., the Thomistbund and the Keplerbund) and with politically oriented scientists like Rudolf VIRCHOW. These battles culminated with HAECKEL's debate with Eric WASMANN, S.J., and the awarding of BISMARCK with a doctorate in morphology. I will trace out the roots of HAECKEL's attitudes and indicate how they gave shape to his theory.

• Scientific Socialism: A Case Study in the Diffusion of Spencerism in Late 19th Century Italy, **Naomi Beck**, U of Paris I, France, U of Chicago, USA

What motive is there to reopen the 'Herbert SPENCER file'? Undeniably, the success of his *System of Synthetic Philosophy* was only ephemeral and hardly outlived its author. Nonetheless, it would be a mistake to overlook the profound impact of his evolutionary theory and the important part it played in the general public debate over political, economic and ethical issues of the time. In order to grasp the dynamics in the appropriation of Spencerian ideas it is essential to understand the historical context in which they were diffused. My paper will offer preliminary remarks concerning the role played by the milieu in this process of interpretation. They will be followed by a presentation of an interesting case study: Enrico FERRI's 'Scientific Socialism'. I will examine FERRI's attempt to prove that Marxian socialism is the logical outcome of evolutionism, and point out to the discrepancy between his position and SPENCER's political views.

• Commentary, **Paul Weindling**, Oxford Brookes U, UK

Friday, 2-3:30 PM, LR7
SESSION VI

Lorenz' Concept of Instinct

Organizer and Chair: **Richard W. Burkhardt, Jr.**

• The Instinct Concept of the Early Konrad Lorenz, **Ingo Brigandt**, U of Pittsburgh, USA

Peculiar to LORENZ's view of instinctive behavior is his strong innate-learned dichotomy. He claimed that there are neither ontogenetic nor phylogenetic transitions between instinctive and experience-based behavior components. The present study discusses how LORENZ came to held this controversial position by examining the history of LORENZ's early theoretical development in the crucial period from 1927 to 1937, taking possible influences into account. This early history of LORENZ's ideas helps to highlight the methodological and conceptual steps that LORENZ took and which brought about his specific position on instinctive behavior.

• Instinct in the '50s, **Paul E. Griffiths**, U of Pittsburgh, USA

In the early 1950s the instinct concept developed by LORENZ in the years leading up to WWII seemed, from the perspective of students of animal behavior in the UK, to be both a genuine break with the past and a central theoretical construct of the new ethology (an exception to this view was J. B. S. HALDANE, who argued that LORENZ's ideas were not really so dissimilar from those of the leading early 20th century instinct theorist William MCDOUGALL). The enthusiasm for LORENZ's concept was, however, shortlived. By the late '50s the 'English speaking ethologists' had rejected LORENZ's approach in favor of one which stressed the interaction of heredity and environment in the ontogeny of all behaviors. This paper examines three events that may be relevant to this change: the visit of Daniel LEHRMAN to the UK in 1954; the work of William H. THORPE, and particularly his 1956 *Learning and Instinct in Animals*; and the theoretical papers of Robert A. HINDE, particularly his rejection of the hierarchical instinct theories of LORENZ, TINBERGEN, and the distinctive instinct theory of Adriaan KOORTLAND. The paper concludes with a brief examination of LORENZ's response to these developments in the UK.

• Lorenz on Innate Behavior and Phylogenetic Information,
Derek Browne, U of Canterbury, New Zealand

In *Evolution and Modification of Behavior*, LORENZ adopts the view that behaviour itself is never, strictly speaking, innate. Instead, information expressed in (instinctive) behaviour is innate: phylogenetic information. This is information about the world—e.g., female sticklebacks have information with some such content as this, that objects exhibiting red below are potential mates. I describe the advantages of this concept of innateness over recent conceptions, e.g., that innateness is canalization. But I conclude nonetheless that the concept of phylogenetic information is of limited value in causal explanations of (instinctive) behaviour.

Friday, 2-3:30 PM, LR10
SESSION VII

Life, Metaphysics, and Biosemiotics I: Biology and Metaphysics

Organizers: **Naomi Dar** and **Charbel Niño El-Hani**, Chair: **Naomi Dar**

• What's Going On: Mapping the Central Issues in the Emergence Debate,
Charbel Niño El-Hani, Federal U of Bahia, Brazil

The debate about emergence has recently re-emerged. A great number of emergence theories has been proposed in recent years, as the concept of emergence has been increasingly used in such dif-

ferent research fields as Artificial Life, cognitive science, evolutionary biology, theories of self-organization, philosophy of mind, dynamical systems theory, connectionism, synergetics etc. The role played by this concept in so many fields has been directly responsible for revitalizing emergentism as a philosophical trend. This seems quite natural, as many of these fields are gathered under a general description as the 'science of complexity', concerned with 'the complex emerging properties of life and mind' (EMMECHE 1997). Now, to talk about property emergence is no longer perceived as something that stands in opposition to scientific thought. The very term 'emergence' has become popular in the context of computer models of non-linear dynamical systems, complex systems research, Artificial Life, etc. However, in these contexts the term is often used in vague and imprecise ways without a rigorous debate concerning how it might be explained or defined. But we should try to keep an exact meaning of the term 'emergence' clear. In as much as it has carried for a long time a burdensome load of confusion about many of its aspects.

• Replication and Metabolism: Means to an End? Searching for Another Definition of Life,
Naomi Dar, The Hebrew U of Jerusalem, Israel

The two main characterizations in definitions of life metabolism and self-replication (which amounts to evolution through natural selection) fail to explain the metaphysical characteristics of life. The definition of life as a metabolic system stresses the spontaneous emergence of life. Accordingly, once the right conditions exists, a living system inevitably forms, thus underscoring the chemical properties of living systems. in contrast, the self-replicating theory upholds the uniqueness of life as a biological/evolutionary systems, thus placing an emphasis on natural selection as a driving force in the evolution of life. The thermodynamic definition of life regard life as a far from equilibrium system. However, by focusing on the thermodynamic characterization of life, such definitions of life, have a tendency to confuse the outcome for the cause of the emergence of life. Both of the major theories reflects the perception of the properties of life-as-we-know-it, such as carbon-based life, or the specific physical origin earthy life conditions. As a result, the definition of life in the origin of life theories is too narrow. This paper proposes another definition of life. The key element in this definition is the interaction of the system with the environment, and the imperative contrast adjustment to changing conditions. It is hypothesized that the most essential property pf life, spanning all stages up to the highest cognitive abilities, is the ability of autonomous units to sense the environment in which they live, and respond to it. This definition of life has a bearing on the current understanding of the interacting of living entities, such as bacteria, with their environment, and the nature of the genetic changes (Darwinian and Lamarckian alike)living systems experience in their struggle to survive. Viewed from this perspective, the characterization of life-as-we-know-it is no less than a description of the means by which the goal of survival, under the conditions prevailing on Earth, is attained.

• Emergence, Nonlinearity, and Living Systems: A Metaphysical Lecture from Biology?
Slobodan Perovic

It is widely believed that among philosophers that a higher-level property, if it is a physical property, must be instantiated by a complex structure consisting of more basic physical properties. Dynamic properties of a higher that most basic level are thus merely recombination of atomic properties. Consequently, no dynamics describing changes in the world, such as development, and/or interactions between physical, chemical, biological, or other systems, can possibly contradict this claim. Traditionally, ontologically emergent properties are understood to be novel 'internal' properties of complex entities that cannot be reduced to lower-level properties. Taxonomies of emergence driven by reductionist motives regard such properties as mythical (e.g., vital forces), acknowledging a possibility of only epistemic emergence in the world of physical properties. I propose in response that such a taxonomy may be incomplete. Biological systems as they are explained in terms of non-linear dynamics, I suggest, may fit requirements of non-epistemic emergence exhibiting properties of rationally holistic terms. In a system explained in terms of nonlinear dynamics, none of the external properties influencing the system is singled out as the

cause of its abrupt changes. Instead, a relation among the constituents of the system seems to be responsible for such a turn of event. I will illustrate applications of nonlinear dynamics to the cases of metabolic control and biological pattern-formation. I will outline relevant conceptual and empirical questions that should be addressed in order to answer whether the accounts concerning biological and possibly other types of natural systems, which appeal to nonlinear dynamics, may be suggesting that behavior of these systems goes beyond epistemic emergence.

Friday, 2-3:30 PM, LR1
SESSION VIII

Developmental Regulation I

Organizer and Chair: **Carl Bumba**

• 'Triggering' is the Wrong Metaphor, **Jason Scott Robert**, Dalhousie U, Canada

Some scientists explicitly adopt the terminology of the 'triggering' of genomic and genetic potential in their explanation of development and the aetiology of disease: "The relationship between genes and the environment can be compared to a loaded gun and its trigger. A loaded gun by itself causes no harm; it is only when the trigger is pulled that the potential for harm is released. Genetic susceptibility creates an analogous situation, where the loaded gun is one or a combination of susceptibility genes (alleles) and the trigger is an environmental exposure." But this sort of metaphor is untrue to the nature of development. Though 'potentiality' is a notion that has been problematic ever since ARISTOTLE, I argue (along with many others) that whatever developmental potential there is resides not in genes or other developmental resources but rather in their synergistic interaction. Understanding how genes are regulated is a crucial part of explaining organismal development, but regulatory networks are not at all like genetically loaded guns with environmental triggers. As a number of developmental biologists and commentators have made clear, "the nature of interactions is the primary issue in development." After problematizing the 'trigger' metaphor, I elucidate my view of interactive generative systems in development, of which genes are crucial components (but not as repositories of developmental potential).

• 'Accommodation' in Evolutionary Developmental Biology, **Julio Tuma**, U of Chicago, USA

A fundamental problem of Development (which organisms must solve at each step and throughout their ontogeny) is: the preservation of function while increasing variation to insure for future adaptability or increased functionality. One can view this dilemma as the accommodation of external and internal selective forces while maintaining a functioning unit, or alternatively, as the maintenance of a signal's effect (a function) when there exists lots of environmental interference. Accommodation (as I will propose in this paper) is the ratcheting up of variation (both in structure and process) while preserving functional continuity (making things still work) via a slew of auxiliary mechanisms that try to cope with the environmental modulation. Compartmentalization and modularity form an important and refined subclass of processes of accommodation, but there exist others as well (e.g., second-site revertants, codon third base redundancy, alternative splicing of primary transcripts, alternative promoter selection, trans-splicing, various repair mechanisms, etc.). Quasi-independence of traits is an important result of many of these processes of accommodation (particularly, if selection is to work upon them). However, the evolution of development is usually a messy affair, utilizing kludges as proto-modules on their way to becoming major innovations. Signals in development are often open-ended, decomposable and fungible. They have to be in order to accommodate the variety of environmental options presented. Despite all this, organisms are able to reliably pass on structures and developmental processes while maintaining functional wholes. The maintenance of variation is due to more than just a lack, or a relaxation, of selective pressure; it is a fundamental mechanism for coping with what would otherwise be an informational overload—the need to program for every possible outcome and to maintain this rigid program set in the face of mutation pressures and external changes.

• The Evolution of Higher Order Developmental Control, **Carl Bumba**, U of Vienna, Austria

The symphony of cellular movement, growth, and gene expression that reliably emerges from genetically-encoded and environmental factors is generally thought to be conducted by a small group of molecular determinants. These highly conserved molecules determine the most basic body plan features, while genetic differences in their regulation presumably account for most of the morphological variation seen among taxa. Post hoc analysis of developmental processes and gene disruption experiments indicate that these interdependent developmental regulators are organized in a hierarchical fashion. In this talk, I will provide evidence that networks of molecular interactions that control development are not necessarily emergent properties of entire developmental systems or our own abstractions of those systems, but are real assemblages of specific genes under natural selection. I propose that control networks that govern the sequences of genetic regulation underlying developmental processes must be partially decoupled from the developmental cascade itself. Specifically, evolutionary changes in developmentally invariant chromatin characteristics at developmental control genes may have widespead effects in the genetic regulation of phenotypic traits. Such chromatin pre-patterns may constitute a higher order form of genetic regulation in complex morphology.

Friday, 2-3:30 PM, LR2
SESSION IX

Fitness, Drift, Evolutionary Theory

Contributed Papers, Chair: **Jessica Pfeifer**

• Tautology, Probability and Drift, **Frederic Bouchard**, Duke U, USA

Evolutionary Theory provides causal explanations of the biological world. Such explanations are usually instantiations of law-like statements. After showing how and why evolutionary explanations should avoid being turned into mere tautological statements about the survival of the fittest, I show how the best currently available descriptions of the principle of natural selection come at the too high cost of making evolutionary theory merely schematic (e.g., propensity view of fitness) or incapable of identifying real evolutionary processes (e.g., central tendencies account). I then provide an argument to justify the rehabilitation of a design-problem view of fitness. I conclude by showing how such an account leads to a better understanding of the notion of drift.

• The Real Problem with the Propensity Account of Fitness,
Jessica Pfeifer, U of Maryland, Baltimore County, USA

The propensity account of fitness has recently faced criticism from its previous defenders due to GILLESPIE's findings that variance can affect selection and FINSEN's and BEATTY's similar finding with respect to skew. Some believe that these results merely require a revision of the mathematical formulation of fitness. SOBER, however, argues that these revisions actually have profound implications for the metaphysics of fitness attributions. Given that the revised definition requires reference to population size, SOBER argues that "an organism's fitness is not a propensity that it has," since populations size is extrinsic to the organism.

In the first part of this paper, I argue that SOBER's concerns are misplaced. SOBER mistakes the role that the reference to population size plays in GILLESPIE's revised measure of fitness. The mathematical facts underlying fitness attributions are the entire probability distributions affecting offspring contribution. However, when one attempts to summarize some aspect of this distribution, e.g., expected outcome, then other features of the distribution, such as skew and variance are left out. GILLESPIE's revision is simply an attempt to correct for this omission. However, the amount of correction needed for within generation variance diminishes as the population gets larger. As the population size increases, the frequencies will more likely reflect the probability distribution. Consequently, the overall frequency will have a higher probability of reflecting the expected outcome.

The reference to population size is required only because of the need to correct for the omission of information about the probability distribution. Fitness is a function of the entire probability distribution, and this distribution does not require reference to the population size. Consequently, GILLESPIE's findings do not in and of themselves entail that fitness cannot be a property of an organism.

There is, however, a fundamental problem with the propensity view that relates to its inability to deal adequately with the notorious *reference class* problem. This is the focus of the second part of the paper. Though this problem is faced by all accounts of probability, it is particularly intractable in the case of propensity accounts of fitness. The problem is that it seems difficult, if not impossible, to define the reference class with enough specificity to yield an objective probability distribution, while at the same time including only those factors considered relevant for fitness. In order to get an objective probability distribution for offspring contribution, all causally relevant features must be specified. This is why BRANDON requires that the probabilities be defined relative to a homogeneous environment. On BRANDON's account, for the purposes of selection, we are only interested in those factors that make a difference to the relative fitness of the organisms in the population, where fitness is defined as the expected offspring contribution (corrected for variance and skew). However, this would require that we include factors, such as being struck by lightning, that are often considered irrelevant to selection (though not irrelevant to evolution), since lightning strikes affect the expected number of offspring. But, as MILLS and BEATTY argue, we only want to include those factors of the environment that discriminate successful and unsuccessful reproducers due to some physical difference between them. Lightning strikes are relevant to selection only if some organisms in the population have some feature that allows them to be better able to survive or avoid them. Therefore, when defining probabilities relevant for fitness, we want to ignore some of the features of the environment that are causally relevant for reproductive success, but not relevant for natural selection. The question is whether ignoring causally relevant features makes sense on a propensity account of probability. While this seems to provide support for an epistemic interpretation of the probabilities associated with fitness, I conclude the paper by suggesting ways that one might resolve the above problem without resorting to such an epistemic view.

• What Fixes Fitness? **Grant Ramsey**, Duke U, USA

Since Herbert SPENCER's introduction of the phrase 'survival of the fittest' in 1864, what fitness is and how it can be measured has been debated. Conceptualizations of fitness fall into two classes: probabilistic and non-probabilistic. Although the non-probabilistic conceptions are appealing in their directness and simplicity—one can learn the exact fitness of an individual just by counting its descendents—they are plagued with problems. This paper argues that non-probabilistic fitness is neither explanatory nor predictive and cannot fulfill its required role at the center of the theory of natural selection. Instead, the theory requires that fitness be probabilistic. But if fitness is probabilistic, what fixes these probabilities? Fitness is fixed by an organism's genes and environment in such a way that each organism's fitness does not change over time. Thus, an organism cannot raise or lower its fitness. And if this is true, then phenomena that apparently require fitness exchange—altruism, for example—must be reconceptualized.

Friday, 2-3:30 PM, LR3
SESSION X

History, Philosophy, and Sociology of 18th-19th Century Biology II

Contributed Papers, Chair: **Anna Maerker**

• The Taxonomist's Regress, **Anna Maerker**, Cornell U, USA

In 1791 grandduke FERDINAND OF TUSCANY was offered a living chimpanzee for acquisition for the recently founded public Museum of Physics and Natural History in Florence. The purportedly

hermaphrodite animal was sent to the museum to evaluate the potential monstrosity. Its classification posed a problem for the local experts: they came to the conclusion that the claim to hermaphroditism had been motivated by the observation of a clitoris in the animal, a feature on whose presence in chimpanzees there was no consensus among 18th-century naturalists. As the experts pointed out, there were different ways to classify the animal: either it was an exception to the assumed lack of a clitoris in chimpanzees, and thus indeed a monstrosity, or it shared this feature with all normal females of the species. The animal's monstruous status depended on the examiners' expectations.

In the first part of this paper, I argue that this classificatory problem is structurally similar to the 'experimenter's regress', the problem of deciding on the successful replication of physical experiments analyzed by Harry COLLINS. Here I draw on the Edinburgh school's reading of WITTGENSTEIN, according to which both problems of replication and classification are variations of the phenomenon of 'finitism' exposed by WITTGENSTEIN as one of the core features of knowledge production. In the second part I take up WITTGENSTEIN's subsequent anti-individualist epistemology for investigating the historical specificity of the classification problem in the Florentine case. I use archival sources from the museum and early modern zoological publications to examine how actors solved the 'taxonomist's regress', and to analyse how this solution reflects on the institutional and conceptual transformation of natural history around 1800 concerning the epistemological status of monstrosities, different models of gender and their impact on classification, and new articulations of expertise in the service of the state.

• Our Protist Ancestors, **Andrew Reynolds**, U College of Cape Breto, Canada

In the mid-19th century Ernst HAECKEL was among the first to follow up DARWIN's theory of descent with a speculative proposal about the evolution of the metazoa from the single-celled protists. In doing so he invoked the principle of the division of labour in a way that clearly reflected contemporary European attitudes of social progress and a hierarchical ranking of civilizations. Current evolutionary theory about the origin of the metazoa retains an important role for the principle of the division of labour. In this paper I discuss whether 'external' values similar to those affecting HAECKEL's original proposal can be seen to influence modern evolutionary theory. Some of the modern biologists to be discussed will include John Tyler BONNER, Lynn MARGULIS, and Leo BUSS.

• Biology and Sociology in the 19th Century: Differentiation and Complexity,
Silvia Caianiello, ISPF, CNR, Italy

The aim of the paper is to analyse Herbert SPENCER's recovery of the notion of development worked out by Karl Ernst VON BAER in the domain of embryology. In this context development was understood as a process moving from homogeneity to heterogeneity, in which the most general features of the type appeared at first, and the more specialized ones in succession, showing a gradual increase in individuality. SPENCER, by amplifying VON BAER's principle from the context of embryological development to the context of evolution, generalizes a notion of complexity as differentiation of functions, which, by melting with MILNE-EDWARDS' idea of a physiological division of labour in organism, will be successfully implemented in the raising sociology, notably in DURKHEIM, where it will turn into an essential instrument for the description of complex societies.

• Knowledge and Innovation in an Agricultural Context: Commercial Plant Breeding in Germany in the Second Half of the 19th Century, **Thomas Weiland**, Munich Centre for the History of Science, Technology, and Medicine, Germany

In Germany commercial plant breeding took off in the second half of the 19th century—a period that witnessed a tremendous increase in agricultural productivity. Although the German agricultural research system was already well developed at that time, the birthplace of commercial plant breeding was not academia but the farms of a new class of agricultural entrepreneurs located in the

Prussian province of Saxony and the adjoining regions. These mostly wealthy and academically trained farmers were highly successful in producing new varieties that were sold not only all over Germany but also to many other European countries. The paper discusses the work of plant breeders such as Wilhelm RIMPAU (1842-1903) and Ferdinand VON LOCHOW (1849-1924) who were crucial for the development of commercial plant breeding in Germany. The analytical focus is on the interrelation of scientific knowledge and agricultural innovation. It is argued that the knowledge of heredity produced by natural scientists was of little importance for the evolution of commercial plant breeding in 19th-century Germany. Rather, plant breeders developed their breeding methods through trial-and-error procedures and systematic experimenting. It is not that plant breeders were unaware of or even uninterested in scientific concepts about hereditary phenomena —quite the contrary. But the available scientific knowledge was not suitable for the problems plant breeders had to cope with in their day-to-day work and thus only of marginal help.

Friday, 4-5:30 PM

SESSION I, **LR8**

Biology and Anthropology II

Organizer and Chair: **Mathias Gutmann**

• From Biology to Anthropotechnics, **Jerome Goffette**, U of Claude Bernard, Lyon, France

From Antiquity to modern times, the paradigm of medicine retained a strong and constant direction: the fight against illness. This fight includes an anthropological basis against suffering, a social representation of the physician, and a conceptual strain between the normal and the pathological. Above all, there is an ethical imperative: the imperative of care, i.e., the obligation to help other people in distress. However, as the result of modern discoveries, biology offers new ways of proceding that in part do not square with the imperative of care. Contraception and abortion, procreation techniques, aesthetic surgery, methods of amelioration of sport capabilities, and control of emotions and desire, for example, are at stake. Those techniques are both trivial and paradoxical: doctors use them, but they stand outside of the imperative of care. To resolve this apparent paradox, a hypothesis is suggested: those techniques form a new profession, aiming at the biological modification of an individual to improve her/him (physically, psychologically, etc.). The conceptual strain underlying this new pattern is the ordinary and the modified. In particular, its purpose is not an ethical imperative, but a desire of a improved individual being. This new profession may be called *anthropotechnics*. Compared to medicine, anthropotechnics is very specific. Its fundamental concepts are radically different, sometimes opposites. The sequence of consultation starts with the expression of a desire (and not with an illness). Likewise, professionnal deontology must be different. The imperative of care involves an obligation to help. But anthropotechnics is unnecessary and the deontological problem is rather why and how regulate anthropotechnical practices. It corroborates our hypothesis: a new profession, anthropotechnics, really seems in progress.

• The Human vs. Animal Distinction as Part of Man's Construction of 'Nature': New Perspectives, **Mathias Brochhausen**, Johannes Gutenberg U, Germany

The human-animal distinction can be interpreted on the one hand from a sociobiological point of view. On the other hand it has to be seen as a part of man's construction of nature. Man's construction and definition of what nature is and what should be regarded as natural has undergone huge changes, as the history of ideas shows. Both aspects of the human-animal distinction can be traced back to traditional dualistic thinking. In getting this two ends together, new insights concerning eminent issues in the philosophy of biology can be achieved. They will concern the discussion of human phylogeny and the influence of social values in that debate on one hand, on the other hand

new answers to questions concerning animal rights and the moral status of animals can be suggested.

• Let's Face Our Expressions, **Michael Bölker**, U of Marburg, Germany

Quite recently, the question what distinguishes humans from non-humans has received a molecular turn. Large-scale comparison of gene expression patterns between humans and apes was performed for different organs. These studies revealed that a strong selection for differences in expression levels has occurred predominantly for brain-specific genes. Furthermore, it was demonstrated that a gene required for the acquisition of normal spoken language has also been subject to positive selection during the evolution of modern humans. These findings will be discussed in the light of different genetic and gene concepts.

Friday, 4-5:30 PM, LR5
SESSION II

The Mutual Shaping of Science and Science Education II: Historical and Philosophical Studies

Organizer and Chair: **Charbel El-Hani**

• The Gene Concept as an Example of Content in Science and Science Education,
Veronica Flodin, Stockholm, U, Sweden

The overall aim of this paper is to study the relation between scientific research and lecturing in biology at the university level. Teaching at universities is at the intersection between the construction of new knowledge and the presentation of knowledge constructs. How to create courses and choose content are pressing decisions to make, and something one could call 'educational dilemmas'. When different learning situations were investigated, especially in the natural sciences, the focus has been on student (mis)conceptions and different pedagogical methods. The content presented in the teaching and learning situation has been treated as given. I am interested in the way in which the content, construed by the researcher and structured in the teaching material, contributes to learning and educational dilemmas. As an example of content I have chosen the gene concept. The 'gene' is used in almost every biological subject. It is also a concept that has been investigated by philosophers of science and by researchers in science education. It has changed both denotation and connotation over time, and is clearly very context-dependent. Questions I want to raise in this paper are: What significations/implications are connected with the gene concept? What structures of knowledge and traditions of knowledge are connected with it? How is it used in different research areas, different educational settings? What happens when the researcher as a lecturer transfers knowledge from her/his research practice to an educational setting? What happens when you transfer a vague concept like 'gene' that works well in the research context to the educational context?

• 'A Skilled and Trained Eye Brain Behind It': Teaching in the Cambridge School of Zoology,
Helen Blackman, Cambridge U, UK

This paper examines the teaching of embryology in Cambridge from the mid-1870s to the mid-1890s by focusing on teaching tools such as textbooks, microscopes and slides, diagrams, and museum specimens. It was during this period that the Cambridge teaching staff introduced practical teaching and examinations into the syllabi for the natural sciences. I explore the development of practical teaching alongside the exam system's emphasis on the categorisation of specimens. I argue that whilst historians of biology have traditionally contrasted practical teaching with an older methodology which encouraged the development of taxonomic skills, the Cambridge system shows that these two were used to encourage similar skills. I trace the links between the in-depth knowledge needed to justify the use of competing classificatory systems, with the knowledge ob-

tained in the carefully controlled environment of the teaching laboratory. The Cambridge school of Zoology saw itself as a world-renowned leader in both teaching and research. Former Cambridge students took up teaching positions in Birmingham, Manchester, London, Glasgow and Montreal. They took with them a style of teaching which fostered a distinctive approach to nature, believing that they were showing their pupils how to train themselves. This paper considers the implications for such an approach in the teaching of embryology to both medical and natural science students.

• Commentary, **Steve Fifield**, U of Delaware, USA, and **Charbel El-Hani**, Federal University of Bahia, Brazil

Friday, 4-5:30 PM, LR7
SESSION III

Exhibiting Humans and Animals

Organizer: **Tatjana Buklijas**, Chair: **Lynn Nyhart**

• Darwin in the Cage: Changing Perceptions of Primates in the Late 19th-Century Viennese Menagerie at Schönbrunn, **Oliver Hochadel**, U of Vienna, Austria

In the last third of the 19th century Darwinism was a widely discussed topic, in particular the idea that man descended from ape. At the same time the first primates—chimpanzees, gorillas, and orangutans—arrived in European zoos. This coincidence drew even more crowds to the monkey cages, a place that had always been a main attraction in the zoo. This paper will reflect on the zoo as a very special place for public science that has so far been neglected. Taking the Viennese Menagerie at Schönbrunn as its main example, the paper will show that the zoo was an ambiguous medium for the spread and discussion of Darwinist ideas. On the one hand popularizers such as Alfred BREHM even held spontaneous lectures in front of the monkey cages in his zoo in Berlin. On the other hand journalists and caricaturists were quick to squeeze endless fun out of the allegedly intimate relationship between man and ape. Ernst HAECKEL and other Darwinists kept reiterating that man does not descend from some a present-day primate but from some common ancestor. Yet this distinction did not fit popular Zoo-Darwinism.

• Public Anatomy in Vienna, **Tatjana Buklijas**, Cambridge U, UK

The 19th century Vienna School of Medicine was widely appreciated in the contemporary medical circles as the best place to study human anatomy, both for the quality of instruction and for the abundant provision with the main teaching material—of corpses for dissection. This paper aims to go beyond the University and to examine anatomy/anatomies presented to and accessed by the general public in a wide spectrum of forms and places. These range from exhibitions at the Panoptikon und Anatomisches Museum established in 1871 in the Prater amusement park by the former circus trainer Hermann PRAEUSCHER, to popular lectures in human and comparative anatomy given by the university professor Carl Bernhard BRUEHL at the Institute of Zootomy between 1863 and 1890 and later on in his own home. By looking at the kinds of anatomy presented, as well as at the participants in these events, I intend to examine the relationship between academic and public anatomy, as well as to reveal the purposes to that public anatomy served, in particular with respect to such socially prominent issues as Darwinism or women's rights. I shall close by showing how public anatomy changed as the politics was transformed around 1900.

• Bodies that Matter: On Popular Displays of Anthropological Knowledge in Vienna Between 1900-'40, **Klaus Taschwer**, U of Vienna, Austria

Like many other European cities around 1900, fin-de-siècle Vienna was obsessed with exotic human bodies. For example, in the Prater, the city's popular amusement park, native peoples from all over the world—e.g., Inuit, Ashanti, etc.—were shown to the curious Viennese citizens in

regular exhibitions. At the same time physical anthropology was established as a scientific discipline, partly at the university and partly at the Vienna Natural History Museum. As I shall argue in my presentation, this process of institutionalization was accompanied by many efforts to bring this new knowledge back to the public. An overview is given of where, how, and why physical anthropology was displayed and popularized, i.e., in lectures and courses at the Viennese university extension lectures and courses at the adult education centres at *Volkshochschulen,* respectively, and later on also in exhibitions at the institute of anthropology and the Natural History Museum. One of my main arguments is that these exhibitions not only served to 'educate' the people but also helped to strengthen the scientific position of anthropology by showing off its biopolitical importance. Finally, I will discuss whether these popular displays of anthropological knowledge and eugenics before and after 1938 also served as a legitimation for the racial hygiene of National Socialism.

• Commentary, **Lynn K. Nyhart**, U of Wisconsin, USA

Friday, 4-5:30 PM, **LR6**
SESSION IV

Digital History of Biology: The Virtual Lab and the Encyclopedia of the History of Biology

Organizer: **Manfred Laubichler**

A demonstration by **Janet Browne**; **Richard M. Burian**, Virginia Tech U, USA, **Manfred Laubichler**, Arizona State U, USA, **Ilana Löwy**, **Hans-Jörg Rheinberger**, Max Planck Institute for the History of Science, Germany, and **Henning Schmidgen**

Friday, 4-5:30 PM, LR9
SESSION V

Evolution and Politics II

Organizer: **Naomi Beck,** Chair: **Paul Weindling**, Oxford Brookes U, UK

• Medical Refugees, Evolution and Politics: Three Austrian Case Studies,
Paul Weindling, Oxford Brookes U, UK

In order to assess the impact of medical refugees from Nazism, it is necessary to move from selective approaches to taking a 'total population' approach. This allows a broader perspective taking into account distinctive Continental styles of science which never successfully transferred as well as the success stories. This provides a rationale for a comprehensive European study of medical and biological refugees, 1930-'48. In this paper I consider the general issue of biologists as refugees in the period, as well as three Austrian-born examples: Walther FINKLER, Ignaz ZOLLSCHAN and Felix GROSS. I consider particularly Lamarckian approaches to evolution, issues of *Weltanschauung*, and the contrast in careers in the receiving context.

• The Politics and Biology of Virtue, **Abigail Lustig**, Dibner Institute, MIT, USA

Focusing on the work of W. D. HAMILTON and E. O. WILSON, I discuss how and why explanations of 'altruism' were created in the mid-1960s as a central focus of evolutionary biology. These new theories of altruism, phrased in the economic language of cost-benefit analysis and rational choice theory and depending on a definition of altruism that was no altruism at all, but only a form of disguised selfishness, laid the groundwork of sociobiology, evolutionary psychology, and debates about the biological basis of human nature that continue to be disputed today. I discuss some

of the political reasons, both within the history of biology and in society at large, that have made sociobiological explanations of human nature both so troubling and so attractive.

• Human Nature, the Genetic Fallacy and the Politics of Development:
Philosophical Anthropology as Critical Theory, **Lenny Moss**, U of Notre Dame, USA

The ethical and political implications of thinking about human beings biologically, as it is expressed in the work of Steven PINKER and other evolutionary psychologists, turn on presuppositions about human development in which 'nature' or 'the biological' is equated with preformationism while epigenetic developmental plasticity is assimilated to 'nurture' or 'the blank slate'. An alternative, less preformationistic, approach for weaving biological and cultural strands together into a unified, nondualistic fabric was pioneered by mid-20th century German philosophical anthropologists. The prospects for a renewed philosophical anthropology will be critically compared and contrasted with that of evolutionary psychology both with respect to empirical warrant and to questions of their respective practical/political implications.

• Commentator: **Paul Weindling**, Oxford Brookes U, UK

Friday, 4-5:30 PM, LR3
SESSION VI

Philosophy and Ecology

Contributed Papers, Chair: **Matther Haber**

• Invasion Biology and its Critics, **Matthew Haber**, U of California, Davis, USA

Recent papers by Kristin SHRADER-FRECHETTE and Mark SAGOFF have been critical of studies in invasion biology. I evaluate the claims made by these critics with particular regard for criticisms stemming from the definition of key terms in invasion ecology. One problem the critics have raised is that there is no consensus definition of key terms in invasion ecology. Why, though, should this count against invasion ecology? I use a comparative method to show that a lack of consensus definition of key terms is, in fact, a trend found in many fields of biology. One particularly good candidate for demonstrating this is the term 'species' and its use in systematics and evolutionary biology. There are several conceptual frameworks available from these fields demonstrating how a coherent and precise biological research program can proceed in the absence of a consensus definition. Critics of invasion biology have also raised concerns over how reliable a guide invasion ecology is for public policy and legislation. The concern is that in the absence of precise definitions for legal entities such as 'non-indigenous species', public policy is in danger of being arbitrary and stipulative. Political and legal definitions, however, ought not to be confused with scientific concepts. Contrary to the claims of the critics, I show that we ought not always hold legal definitions to the same standards as scientific definitions. Again using the term 'species' as a model, I examine the relation of political and legal definitions to the scientific concepts upon which they are founded. I find that there are often good reasons for legal and scientific definitions of biological entities to have different degrees of resolution, precision, and consensus. These departures reflect the different strategies employed by scientists and politicians in specifying definitions in light of different sets of aims, values, norms, and goals. Invasion biology is currently emerging as a new field. It is bringing together researchers from many different fields of biology, and encompasses the study of many extremely complex processes. As such, we ought to expect that part of these researchers' job will be to learn to communicate with each other. Biologists from many fields, however, have experience dealing with different usage of the same terms, and have developed strategies to accommodate this. Interdisciplinary studies of extremely complicated processes such as biological invasions is very complex, but this complexity ought not be mistaken for incoherence or imprecision.

• The Politics of Biodiversity, **Uta Eser**, Fachhochschule Nürtingen, Germany

Although 'biodiversity' widely enjoys the reputation of a 'scientific' concept there is much more to it. The scientific rhetoric dominating biodiversity discourse tends to conceal economic, social, and cultural interests that also shaped the politics of biodiversity. I therefore suggest to interpret 'biodiversity' as a boundary concept that not simply refers to a quality of nature 'out there' but is a product of negotiation processes between different actors with different agendas. I will discuss the diverse interests that are satisfied by a politics of biodiversity. This approach will allow to ask for the appropriate role of biologists in policy making and the shifting boundaries between science and society.

Friday, 4-5:30 PM, LR10
SESSION VII

Life, Metaphysics, and Biosemiotics II: Biosemiotics

Organizers: **Naomi Dar** and **Charbel Niño El-Hani**, Chair: **Naomi Dar**

• What is Dialectical Biology? **Terence Sullivan**, U of Wisconsin, USA

Dialectical biology has recently received critical attention from Peter GODFREY-SMITH and Philip KITCHER. The former asserts that it is incompatible with Darwinism while the latter describes that it is in constant danger of lapsing into holistic obscurity. But what exactly is dialectical biology? In this paper I examine dialectics both as a method for coming to understand the biological world, i.e., as an epistemological concept, and also as a claim about the nature of the biological world itself, i.e., its ontological nature. Drawing upon the work of Richard LEWONTIN and Richard LEVINS I will argue that dialectical biology is constituted by three key claims: that the world must be viewed as a whole, that this whole is undergoing constant change, and that this change is the result of opposing between poles of a contradiction. Further, I illustrate the methodology and ontological aspects of dialectical biology by means of examples involving niche-construction theory and the sex ratios respectively.

• Towards a Personalist Biology, **Konstantin S. Khroutski**, Novgorod State U, Russia

Primarily I draw the attention to the so-called 'Biological Evolutionary Paradox' in relation to a person: Man is a uterine element of the one common whole cosmic evolutionary process of life on Earth (Process, in abbreviation); however we deny the search for universal evolutionary knowledge and rely on the plural (different and often incompatible) sources of knowledge in defining man's nature: biological, sociological, psychological, etc. This paradoxical situation calls for the creation of original philosophical bases that could provide the integrated comprehension of man's universal nature. This is precisely the approach of mine. Initially, I have pushed forward the original (Cosmist) philosophical fundamentals: cosmological, ontological, epistemological. First of all, in my way, I rely on the novel universal bioreductionism—of true functional essence, but not of the common morphofunctional approach: of reducing living phenomena from the biosphere to populations, organisms, cells, organelles, genes, etc. On the contrary, Cosmist 'functional' bioreductionism means that every living subject (organism) on Earth (man, primarily) has the health-design—its/his/her basic inherent (cosmist) distinct functionality—to contribute ultimately to the well-being of Process. Herein, the key principle of CosmoBiotypology has been introduced. The latter establishes, in accordance with the Cosmist philosophy and theory, that man's gratifying feelings and perceptions (of his/her vital activity), his/her appropriate social surroundings, and his/her physiological biotype has the same single basic inherent functional meaning. In this functional triunity the personal (subjective) perceptions of man have the decisive significance. Hence, Cosmist theory leads to a 'person-driven' scientific biology, which is aimed at the integration of subjective experience and objective knowledge: of biological, psychological, and sociological data about man.

• Metaphysics in Physics and Biology, **Günther Witzany**, Bürmoos/Salzburg (Austria)

At the moment foundation and justification of basic terms in physics and biology is not very satisfying. The distinction between sentences about the world and the sentences about these sentences is not very clear and what is the final point of the foundation of the language we use in everyday communication and in artificial language of scientific theories dealing with models that want to explain the world? Is it possible to find a final point of argumentation you aren't able to reflect beyond?

• Panel Discussion: Is Biology More than a Sophisticated Chemistry? What Does the Metaphysics of Biology Look Like?

Friday, 4-5:30 PM, LR1
SESSION VIII

Developmental Regulation II

Organizer and Chair: **Carl Bumba**

• Redundancy and the Connectionist Theory of Gene Control Networks,
Roger Sansom, Texas A&M U, USA

Redundancy in ontogeny consists in having multiple independent systems that can achieve the same function, such as thermoregulation. Such redundancy is adaptive because it extends the range of environments in which the organism can reproduce, to environments in which one system fails for some reason. Redundancy in gene control networks consists in the behavior of units being determined by multiple input units. Such redundancy increases the range of cellular environments in which a cell will produce the most adaptive cellular behavior from its repertoire. Selection for redundancy in gene control networks would result in the activity of units being regulated by multiple exciting and inhibitory input units. Empirical results suggest that gene control networks have such high connectivity. Redundancy may also be selected at the lineage level.

High connectivity is a typical feature of a connectionist networks. I propose that the gene control network of a cell can be understood as a connectionist network, designed by natural selection to regulate adaptive ontogeny. This is not the view that all regulation of ontogeny is due to the gene control network, but rather that gene control networks are connectionist. This theory offers a very general view of regulation of gene activity that stands in stark contrast to that of Stuart KAUFFMAN, which predicts low connectivity.

• Developmental Modules as Units of Selection? **Gerhard Schlosser**, U of Bremen, Germany

The modular organization of organisms has recently received increasing attention. Comparative studies have revealed the existence of developmental modules, i.e., integrated networks of interacting elements that develop or operate as relatively autonomous units and are, therefore, often repeatedly employed during development. Such developmental modules can be identified at many different levels ranging from molecular interactions (e.g., modular regulation of genes by different transcription factors), to networks of interacting genes or proteins (e.g., gene regulatory and signalling modules), to cell types, complex organs and even entire organisms (e.g., as modules in higher level symbiotic associations). In addition, developmental modules appear often to be conserved evolutionarily and tend to coincide with units of coherent evolutionary change. How can this coincidence be explained? One answer, proposed from the perspective of the hierarchical theory of natural selection, could be that developmental modules act as units of selection. I want to argue here that this leads us astray, because coherent evolutionary change of several elements along an evolutionary trajectory does not require that these elements belong to a common unit of selection in any particular selection process. Rather, one element can constrain the evolutionary

modifications of another element in a succession of selection processes even when both elements never exist in several variants simultaneously. Therefore, we need different conceptual tools to characterize units of coherent evolutionary change. I suggest that the latter units (termed 'units of evolution' or 'modules of evolution') are units of reciprocal constraints on evolutionary transformations, which may be conceptualized as units of elements tied together by high probabilities of fitness interdependence ('coevolution probabilities') relative to some given space of possible variants.

• From Molecules to Ecologies: Biology Becomes the Science of Sensing,
Luis Emilio Bruni, U of Copenhagen, Denmark

We can observe an incipient trend that considers biology as a science of 'sensing'. Biologists from different subdisciplines assign increasing importance to the 'informational processes' in living systems and pay more attention to the 'context' in experimental biology (e.g., from quorum sensing to info-chemicals to signal transduction in general). With the rapid development of molecular techniques, there is in all branches of biology a sort of interaction with molecular biology. There is in the literature a call for integration of molecular and ecological perspectives. But instead we find a tendency to reduce the latter to the former, i.e., decompose (reduce) the ecological complexity into its molecular 'components', with the understated goal of mapping an ecosystem in terms of molecular kinetics. One should of course consider also the epigenetic continuum in between. After 50 years of debate and usage of the 'information' concept in molecular biology problems arrive with signal transduction networks and regulation, where we can see the conceptual emergence of a process of 'natural regulation' which is analogous to 'natural selection' in the sense that in the same way we think about selection without a selector we are thinking about regulation without a regulator. In the same way natural selection is something that exists but is not (phisically) there, 'regulation', as the mechanism that orchestrates and directs (interprets) the signals represented by molecules that bind to each other in specific ways when their concentrations are statistically relevant, starts to look as something that exists, but who knows where it is. Signal transduction networks constitutes one of the first conceptual links between molecular and ecological approaches in general. Whereas DNA was the dominant and central element in the conceptual framework of biology, today its place has been undoubtedly taken by signal transduction. However, it also seems that the ultimate goal of the reductionist strategy would be the massive characterisation of signal-transduction networks and the elicitors of the cascades that determine complex genetic reactions in response to variable environmental cues. How can we relate the different emergent levels of informational processes and semiotic contexts in developmental trajectories? How to interpret systems are formed at these levels? How 'information' is conveyed through the continuum?

Friday, 4-5:30 PM, LR4
SESSION IX
Science in its Social Context
Contributed Papers, Chair: **Sebastian Linke**

• How to be a Successful Fraud, **Roberto de Andrade Martins**, State U of Campinas, Brazil

Suppose that a moderately competent researcher wanted to be regarded by contemporary and future scientists as someone who was always correct, who was better than any of his contemporaries (in his specific field), being ahead of his time, the creator of new theories and concepts, a master of experimentation and theoretical reasoning, able to present decisive arguments and proofs, unjustly attacked by enemies of the truth. Such a person could use both 'scientifically neutral' strategies (strategies that are strictly social ones, being independent of the content of scientific publications) and 'scientific' ones, by means of careful manipulation of the scientific discourse, trying to induce other scientists to believe that he is indeed a superb researcher. This paper will elucidate both types and will then focus upon the second form of strategy, presenting several

successful rules followed by smart players, such as: (1) always keep the back door open, protecting yourself against future criticism, by hiding yourself under vague concepts and claims; (2) never show your weaknesses—conceal all limitations, inconsistency, unexplained facts, etc.; (3) hide your debt to others—always present your work as original and due to your own individual effort, only acknowledging your very small debt to others if you really need their support; and several other useful precepts. The full strategy will be illustrated by an analysis of Louis PASTEUR's research on silkworm disease ('pébrine') and his controversy against Pierre Jacques Antoine BÉCHAMP during the decade of 1860.

• Sociobiology in the German Press: The Media Reception of a Controversial Theory,
Sebastian Linke

The discipline sociobiology (SB) was first proclaimed by E. O. WILSON 1975 in his book *Sociobiology: The New Synthesis* (1975) and has caused a scientific controversy since then. WILSON defines SB as the "systematic study of the biological basis of all social behavior, including humans." SB was strongly criticized for its assumption that even the most complex forms of social behaviour in man and animals is determined by genes. The scientific claims and theories of this new discipline were discussed extensively by the proponents and opponents of SB as well as by scientists of other disciplines (e.g., social science, philosophy). In the ongoing debate, no general agreement is reached about the application of SB and its relevance for human behaviour. Despite the controversial discussion in the scientific community, SB was often presented one-sidedly in the public media as a revolutionary and convincing theory. Although this was noticed by many scientists, the role of the media in the debates surrounding SB was never studied in detail. In this paper the presentation of SB in the German press between the late 1970s and '90s is examined. My talk will include a quantitative and a content analysis of the representation of SB in the scientific press (*Science* and *Nature*), and in the main German newspapers (*Frankfurter Allgemeine Zeitung*, *Der Spiegel*, *Die Zei*t, *Süddeutsche Zeitung*). By analysing the discrepancy between the scientific and the media-discourse around SB I will examine how the popular media reacted to this 'hot issue' and how this kind of popularisation has changed over the last two decades.

• The Uncertain and the Unruly, **João Arriscado Nunes**, Center for Social Studies, Portugal

Biomedicine and public health raise interesting challenges to the growing body of scholarship on uncertainty, risk and the 'risk society', and to the design of 'preventive' and 'precautionary' interventions in public policy. The specificities of these fields rest upon a diversity of 'unruly' configurations of 'health' and 'disease' generated by the play of complexity and singularity. Approaches such as Developmental Systems Theory or related ones provide a range of useful insights and tools to explore this field and to design and assess modes of intervention which try to engage with the aforementioned 'unruliness'. The latter are inextricably technical and political, situated and contingent, involving a diversity of heterogeneous actors and of distributed modes of knowledge and experience. This paper explores some of these issues through a discussion of a number of recent experiences in the management of a range of health problems including cancer, 'environmental illness' and so-called 'emerging diseases'. These experiences draw on transdisciplinary approaches to health problems and on different forms of participatory research and management of these problems.

Friday, 4-5:30 PM, LR1
SESSION X

Metascience from a Biological Point of View

Contributed Papers, Chair: **Paul Pojman**

• Mach's Biological Origin, Purpose, and Nature of Science, **Paul Pojman**, Towson U, USA

This paper is an exploration of the impact of late 19th century German biology upon the history of positivism and the philosophy of physics. Ernst MACH, known primarily as a physicist and philosophical precursor to EINSTEIN, grounded his theory of knowledge within the evolutionary theory of his era. Not only did MACH put forward one of the first biological models of theory change, but he also saw science itself as a continuation of the evolutionary processes. The purpose of science is to lift the human cognitive system out of the false local minima it has adapted to over the course of evolution, and readapt it to a deeper understanding of nature. Science is nature's way of coming into equilibrium with her herself.

• Charles Lyell's Experimental Historical Science, **Alessandro Pajewski**, U of Chicago, USA

LYELL held an 'ecological' picture of the economy of nature, divided into interdependent systems and subsystems, each one of which can be considered to be in a condition of internal (and external) dynamic disequilibrium. To give an explanation for the present state of a given system it is necessary, according to LYELL, to trace its historical trajectory and to individuate the causes responsible for such a trajectory. A historical trajectory is often affected in a consequential manner by the local conditions of the system at any time in the past. Because these conditions are variable and are also influenced in a complex way by the outcome of other historical trajectories, it is generally very difficult to produce a predictive explanation for phenomena within these systems, or even to identify the causes that affect these historical trajectories. Clearly, most of these historical processes cannot be replied in a laboratory. LYELL's methodology focuses on explaining geological and biogeographical processes on the basis of well established causes instead of ad hoc hypotheses. This methodology includes the interpretation of 'historical' records (i.e., geological strata, fossils) on the basis of causal processes and the testing of scientific hypotheses against 'historical' records. This testing is not only necessary to support theories, but also to modify and refine them. LYELL hence envisioned geology as an intrinsically historical discipline, both for its methodology and for the nature of the objects that it needed to explain.

• Larry Laudan, Richard Dawkins, and the Solution to Problems in Sociobiology,
Julio Muñoz-Rubio, UNAM, Mexico

According to LAUDAN, scientific progress is carried out by means of a competition among research traditions, the latter being more efficient with a greater heuristic capacity. But that capacity does not by force lead to the discovery of truth. Applying this model to the sociobiological conception of DAWKINS, one observes its high metaphorical content, which not necessarily expresses true phenomena. This is not an obstacle to solving many problems, regarding, for example, the units of selection. However, I place DAWKINS' model in its historical context and state that the problems he solves come from an ideologized and fetishized approach to reality. DAWKINS pretends that the behaviours based on cost-profit dynamics are universal ones, when in reality they are just behaviours proper to the capitalist period of history. Thus, DAWKINS' discourse makes sense only within the confines of this ideologized and fetishized world. Hence, its heuristic quality is very limited, compared to that of the dialectic approach by LEWONTIN or GOULD. I conclude that, in order to make LAUDAN's model even more coherent, it is necessary to endow it with an historical approach, that allows to place the type of problems and solutions that research traditions offer, in concrete contexts.

Saturday, 19 July

9-10:30 AM

SESSION I, **LR1**

Issues in Sociocultural Evolution I

Organizer: **Marion Blute** and **Sigrid Glenn**, Chair: **Sigrid Glenn**

• Units of Interaction in Cultural Selection, **Sigrid S. Glenn**, U of North Texas, USA

Cultural units of replication have been designated as memes. The function of memes in cultural evolution parallels the function of genes as units of replication in organic evolution. But replication is not the only function necessary for selection to occur; differential interaction is also necessary. As selection produces increasingly complex phenomena in both organic and cultural domains, the resulting hierarchically organized entities entail replication and interaction that occurs at many, and often different, levels. Although genes and memes continue to function as replicators in organic and cultural selection processes, emergent units at both levels come to function as interactors. This paper describes well researched behavioral processes that may have been involved in the emergence of the earliest cultural level interactors and it suggests one way in which the first cultural level interactors may have evolved, providing a foundation for the evolution of cultural complexity.

• The Gap Between Cultural Selection Theory and Sociology, **Agner Fog**

Cultural selection theory is a nomothetic research tradition (i.e., seeking regularities) while much of contemporary sociology is dominated by idiographic thought (i.e., regarding events as unique). Proponents of these two paradigms find it hard to understand and accept each other's research. Explanations of this communication gap are suggested. Some of the strengths and weaknesses of each paradigm are discussed. Several examples are given to support the suggestion that cultural selection theory is superior for explaining unintended developments, while sociology is useful for understanding the subjective meaning of cultural phenomena. Finally, the possibilities for integrating the two lines of thought are discussed.

• Is Cultural Evolution Lamarckian? **Maria E. Kronfeldner**, U of Regensburg, Germany

When the analogy between biological evolution (BE) and cultural evolution (CE) is discussed nowadays, it is quite common to call CE Lamarckian, mostly without asking what a Darwinian account as opposed to a Lamarckian would actually be. It will be shown (i) that undirectedness is the central criterion for distinguishing between a Darwinian and a Lamarckian system, (ii) that the claim that CE is Lamarckian rests on a fallacy and (iii) that in CE we can find correct analogues for the Darwinian as well as for the Lamarckian account of evolution. To show this, I will present the idea of Universal Darwinism, which is the crucial philosophical assumption behind the analogy between BE and CE. Then I will introduce the formal criteria, which have to be met for a system to be Darwinian. Next, I will explain the meaning and importance of undirectedness in BE. Finally, the question of cultural evolution as Lamarckian or Darwinian will be addressed: I will introduce three interpretations David Hull gives of the thesis that CE is Lamarckian and I will comment on them. This will lead to a fourth interpretation and the main thesis of this paper: experience as the Lamarckian and creativity as the Darwinian aspect of CE.

Saturday, 9-10:30 AM, LR2
SESSION II

Biology of Human Behavior I

Contributed Papers, Chair: **Bradley A. Kelley**

• An Evolutionary Account of Self Deception, **Bobby Robinson**, Florida State U, USA

Al MELE offers the following challenge to biologists and anthropologists: empirically demonstrate an instance in which an agent engages in self-deception, such that he simultaneously believes a proposition p, and believes its negation, ~p. I argue that social evolution provides us with at least two avenues by which to find such empirical evidence. One approach is causal, such that maintaining the belief p is sufficient for believing ~p. The other is causal, such that one belief is caused by a social influence, whereas its negation is caused by the genes. I show that a simple story can be told that satisfies MELE's conditions, and provide an evolutionary explanation for the story.

• Can Evolution Explain Insanity? **Dominic Murphy**, California Institute of Technology, USA

This paper discusses recent attempts to argue that much human psychopathology is the product of adaptation, either to an ancestral environment or to features of the current environment. Although the evolutionary arguments are sometimes worked out with care, they tend to seize on one or two features of a disorder rather than looking at the complete clinical picture. The review suggests that these hypotheses are often to be faulted on psychological rather than, or as well as, biological ones.

• Behaviorism, Ethology, and Evolutionary Biology: Forms of Behaviors and Their Relation to the Biological Sciences, 1950-'60, **Bradley A. Kelley**, Virginia Tech U, USA

Using a taxonomy I have developed of types of behaviorism extant in the early post-war period in American experimental psychology, I examine writings, published interviews, and my own personal interviews to assess the relationship between these very different behaviorisms and the biological sciences. Recent academic and scientific work on evolutionary psychology (EP) suggests that EP is a synthesis with cognitive psychology, whereas behaviorism fails to integrate well with the biological sciences, providing (further) argument that cognitivism is, in the words of Jerry FODOR, "the only game in town." I reject this argument. Criticisms of behaviorism all too often fail to take into account the wide variety of behaviorisms, creating a strawman that makes it appear that behaviorism isn't adequate to account for various phenomena, in this case, that it cannot integrate with the Modern Synthesis as it was being developed in the period in question. Focusing on the major figures of B. F. SKINNER and the too-often forgotten J. R. KANTOR, as well as a 'paradigm articulator' of behaviorism during the period, William S. VERPLANCK, I show that a taxonomy adequate to the complexities of the historical situation leads us to reassess the very relationship between psychology and evolutionary biology, hence coming to a new understanding of evolutionary biology and EP.

Saturday, 9-10:30 AM, LR6
SESSION III

Challenging the Essentialist Story about the History of Taxonomy I

Organizer and Chair: **Mary P. Winsor**

• The Essentialism Story as a Historical Phenomenon, **Mary P. Winsor**, U of Toronto, Canada

The 'essentialism story' is the claim that from at least the 17th through much of the 19th century, taxonomy was dominated by the philosophy of the Platonic *eidos*. The story draws its polemical

power from the contrast between the rational idealism of 'typological thinking' and the pragmatic realism of Darwinian 'population thinking', a dichotomy that by its vividness tempts the unwary to assume that those are really the only two ways of thinking about living kinds. Since elementary historical investigation into the taxonomic literature and practice of those centuries shows them to consist mostly of ways of thinking that cannot be squeezed into either side of the dichotomy, the question arises, why has the essentialism story stood so long unchallenged? At one level the answer is surely that the essentialism story is a product of the 20th century. After a long obscurity, Platonic idealism emerged in the 20th century into a new era of fame, or notoriety. Authors as diverse as Alfred North WHITEHEAD, Arthur LOVEJOY, Mortimer ADLER, and Karl POPPER portrayed PLATO as a major influence on Western thought. The principle author of the essentialism story, Ernst MAYR, learned in his youth that typological morphology was a form of Platonic idealism. Yet from 1600 to 1900, those with intimate knowledge of taxonomic practice, including Charles DARWIN, did not notice the essentialism supposedly rampant. If we ask how the essentialism story originated, we find that it was put together, step by step, between 1953 and 1969, based more upon inference than on historical research. It is really an instance of 'rational reconstructing' the past as it logically ought to have unfolded. After all, the phenomenon that living things obviously belong to natural kinds was prominent in PLATO and ARISTOTLE's writings, where the key terms 'genus' and 'species' are prominent. On the other hand, a proper biological understanding of species and of higher categories was only possible after 1859. Thus, it seems to follow automatically that by reading about those ancient authors we can understand how pre-Darwinian biologists must have thought. The overwhelming irony here is that while condemning a great error in the analysis of living things—the failure to appreciate life's rich variety, which is its wellspring of change—the essentialism story commits the same offense against human history neglecting the rich variety of human thought and action, from whence its power to change also comes.

• John Ray, Joseph Tournefort and Essences: The 'Species Problem' in the 17th Century,
Susan McMahon, Max Planck Institute for the History of Science, Germany

The early modern concern with classification as the self-conscious and deliberate activity of a community of natural historians is commonly located in the late 17th century. Among the earliest and best known controversies over 'proper' classification and its methods was that between John RAY FRS (1627-1705), the 'Prince of English Botanists', and Joseph Pitton DE TOURNEFORT (1656-1708), Professor of Botany at the Jardin du Roi in Paris. Phillip SLOAN (1972), in the classic account of this dispute, saw it exclusively as an intellectually-driven debate which had as its centre the ontological status, and consequently the philosophical appropriateness, of essences as criteria for character selection. Thus, for SLOAN, the ultimate outcome of the RAY-TOURNEFORT contest was seen in theoretical terms with limited methodological consequences for the future of scientific classification. However the stratagems of the contemporary natural history community to resolve the disagreement between the two foremost botanists also reveals that more was at stake than arid philosophical principles, including issues of local scientific culture and personal reputation as well as national prestige. Furthermore, an ontological commitment to essences had no practical effect on how individuals went about classifying. By taking into account both the purposes and the practices of early modern classifiers, the contest may be seen to have arisen from the imperatives of establishing a useful method.

• Iterative Aspects of Systematics, **Sara Scharf**, U of Toronto, Canada

Many histories of systematics are written in the context of garnering support for contemporary approaches to longstanding issues in the field. A particularly favoured strategy for building up credibility for a technique is to paint rivals' or predecessors' methodologies as foolishly inadequate at reflecting the reality of the natural world, as a foil to more enlightened strategies. The forced dichotomy inherent in using this rhetorical device, however, obscures systematics as it was practiced, setting up cardboard villains to destroy rather than elucidating sticky points of practice that would enable effective critiques of more insidious problems in the discipline.

One such dichotomy that seems to have taken particular hold on the imagination of writers on systematics is that of top-down versus bottom-up methods of classification. The same dichotomy also shows up in the guise of a priori versus a posteriori character choices. If systematists had but these two options, and many of them prioritized their characters a priori, leading to top-down classifications, one would expect to see myriad arrangements of the same taxa in the literature, with little agreement among them. A close examination of systematic practice in historical context, however, reveals that, though this scenario describes the arrangement of poorly-known taxa well enough, it does not take the iterative aspects of systematics into account at all. In fact, as systematists became aware of more organisms in given taxa, as the organisms' properties became known in greater detail, and as the peer review process—as informal as it was at first—opened up a dialogue about the organisms' interrelationships, the arrangement of taxa tended to converge and become more stable. In other words, increasing familiarity with a taxon enables character choices that in turn enable classifications to better reflect taxonomic relationships.

Monitoring the organisms included in a given taxon over time illustrates how the iterative nature of systematic practice causes greater stability in taxon membership. This situation is especially apparent when following a group from great obscurity to familiarity. Poorly-known taxa have historically been relegated to 'dumping grounds' until their characteristics have become sufficiently documented—their taxonomic affinities taking centre stage to their analogies—to merit treating them as distinct. That this has been the case even in the late 1700s and early 1800s—when a priori methods of describing relationships of relatedness among organisms were explicitly seen by some systematists as unashamedly viable options—lends weight to the superior importance of systematic practice in its iterative glory over the theoretical backdrop before which it plays itself out.

Commentator, **Staffan Mueller-Wille**, Max Planck Institute for History of Science, Germany

Saturday, 9-10:30 AM, LR7
SESSION IV

Indeterminism and Evolution

Organizer and Chair: **Frederic Bouchard**, Duke U, USA

• Individual Fitness as the Source of Probability in the Theory of Natural Selection,
Robert Brandon, Duke U, USA

This talk is part of a larger project exploring the probabilistic foundations of Evolutionary Theory (ET). Here I will outline three controversial claims about fitness: (1) Fitness attaches to a whole life cycle, not to a part nor to multiple cycles; (2) The concept of fitness (or ecological fitness) has as its primary role in ET the explanation of ecological performance during the life cycle—not the explanation of evolutionary success; and (3) Fitness attaches to individuals (sensu HULL) not to ensembles or populations. My primary focus will be on (3). I will show that ensemble or population level interpretations of fitness have no way of distinguishing biologically meaningful from biologically meaningless groupings of organisms and that therefore they have no means to make accurate predictions of even moderately complicated evolutionary scenarios. It follows that such interpretations of fitness are not explanatory and ought to be rejected. The ramifications of this for current controversies over the nature of the statistical character of ET will be discussed.

• Populations, Probability, and Natural Selection,
Roberta L. Millstein, California State U, Hayward, USA

Recent papers on the Montane Willow Leaf Beetle provide an excellent case study of one way in which biologists go about demonstrating evidence for selection over drift. An examination of this case study will illustrate the causal basis for claims about natural selection as well as the necessity

of taking a population-level approach. This population-level approach will in turn have consequences for understanding probabilities in the theory of natural selection.

• On the Interpretation of Probability in the Theory of Genetic Drift,
Marcel Weber, U of Hannover, Germany

The theory of genetic drift is widely viewed as describing a type of processes where chance intervenes in evolution (e.g., during population bottlenecks). Many think that the chance relevant here is of the objective kind, i.e., drift is widely seen as irreducibly stochastic. However, it is also possible to view drift as a fully deterministic, but not fully predictable process. Those who have taken such a deterministic perspective on evolution have assumed that determinism makes the theory of genetic drift a mere predictive or systematizing device without any real explanatory value. I try to steer clear of both of these extant interpretations of drift. Even on the assumption of determinism, drift theory can be given a realist interpretation. The key to realism in spite of determinism is a suitable interpretation of probability, namely an ensemble interpretation. I shall sketch such an interpretation and contrast it with the interpretation of probability in selection theory.

Saturday, 9-10:30 AM, LR8
SESSION V

European Roots of Evolutionary Psychology

Organizer and Chair: **Werner Callebaut**

Introduction, **Werner Callebaut**, KLI, Austria, and Limburgs Universitair Centrum, Belgium

• European Roots of Evolutionary Psychology, **Franz M. Wuketits**, U of Vienna, Austria

Evolutionary Psychology (EP) has a long and venerable tradition in Europe. It has been advocated and advanced by naturalists and some philosophers as well. Charles DARWIN's *The Descent of Man* (1871) and *The Expression of the Emotions in Man and Animals* (1872) can be regarded as the first clear manifestations of EP. DARWIN recognized that psychic and mental capacities and their various expressions are deeply rooted in evolution and therefore are subjects to evolutionary studies. "He who admits," he wrote, "… that the structure and habits of all animals have been gradually evolved, will look on the whole subject … in a new and interesting light." George John ROMANES, his "young disciple," continued and deepened DARWIN's arguments in his *Mental Evolution in Animals* (1883), which is a milestone in 19th-century EP. Herbert SPENCER's *Principles of Psychology* (1870) is also to be mentioned in this context. In the German-speaking countries Ernst HAECKEL, the indefatigable popularizer of Darwinism, and the physicists and philosophers Ernst MACH and Ludwig BOLTZMANN stand in the tradition of EP. A distinctively clear expression of the evolutionary approach to psychology can be found in the early writings of Konrad LORENZ, one of the founders of ethology and one of the forerunners of what is now known as 'evolutionary epistemology', In following the European roots and fingerprints of EP from the 19th century to the middle of the 20th century, I concentrate on some leading figures and their work: Ernst HAECKEL (1834-1919), Ernst MACH (1838-1916), Ludwig BOLTZMANN (1844-1906), George John ROMANES (1848-1894), Georg SIMMEL (1858-1918), and Konrad LORENZ (1903-1989). I also try to show which particular understanding of evolution (e.g., gradualism) is underlying the various evolutionary approaches to psychology. These are mainly Darwinist, but some Lamarckist elements must not be denied.

• Senorimotor Schemes and Conceptual Integration: Piagetian Themes in Fauconnier and Turner's Evolutionary-Psychological Blends, **Marc De Mey**, Ghent U, Belgium

In their recent book, *The Way We Think: Conceptual Blending and the Mind's Hidden Complexities*, Gilles FAUCONNIER and Mark TURNER propose a solution for what Steven MITHEN in his 1996 book *The Prehistory of the Mind: A Search for the Origins of Art, Science and Religion* has called the 'cultural revolution'. The idea is that a rather abrupt onset of wide scope symbolic activity between 40,000 and 60,000 years ago reflects a major change in human mental functioning and should be seen as the beginning of modern humans as we know them today. While MITHEN associates this with a sudden and unexplained disappearance of isolation mechanisms between skill domains, FAUCONNIER and TURNER relate this supposedly quantal leap with a gradual increase in the selectivity with which 'mental spaces' can be combined into new conceptual tools. As their examples include sensorimotor skills among the much more numerous conceptual metaphors, it seems indicated to compare their proposed integration schemes (blends) with PIAGET's description of sensorimotor development. This could clarify the issue to what degree sensorimotor development and conceptual metaphor can be situated on the same continuum.

Saturday, 9-10:30 AM, LR4
SESSION VI

Biology and Education I

Organizers: **Charbel Niño El-Hani** and **Steve Fifield**, Chair: **Steve Fifield**

• Socializing our View, **Karen Kastenhofer**, IFF Vienna and U of Vienna, Austria

During an interdisciplinary research project within the field of cultural studies, socialization (BOURDIEU) of biology students throughout their education at university has been examined. Methodologically, different practices of (re)presenting and explaining science and scientific expertise in biology, physics, literature and history were compared and analysed according to their specific contribution to the process of worldmaking (GEERTZ). The specific bioscientific culture how to view the world and how to (inter)act with it in a scientific way influences implicit epistemological, cultural and social assumptions of biological theory and practice prevalent in the scientific community. The paper is meant to adress three central questions: Which effect does bioscientific culture have on biological theory? Which effect does it have on interdisciplinary cooperation of biologists with other scientists? Which effect does bioscientific culture have on the social role of biology? All three questions shall be discussed on basis of results from the research project 'Science as Culture' on socialisation within universitary education.

• 21st Century Biology General Education, **Cheryl A. Kerfeld**, UCLA, USA, **Bruce McClure**, U Missouri, USA, **Nikki Whitley**, U Maryland, USA, **L. Jean Perry**, UCLA, USA, **Shari Freyermuth**, U Missouri, USA

The 21st century promises to be the century of the genome, and the sequencing of the human genome is arguably the most important biology project ever. Biotechnology is already having a profound impact on day-to-day life. Molecular biology and genomics are changing our understanding of life, affecting how we think about ourselves and our culture. And it is promising us the ultimate potential to take control of our biological destiny. We are designing biology general education materials that aim to prepare students to be scientifically literate citizens while simultaneously building a major research accomplishment: the sequencing a microbial genome. In our program students from all majors have the opportunity to isolate and analyze DNA, making a new contribution to genomic research while they learn. In the course materials we are developing, students become familiar with the techniques, concepts and emotions that accompany doing and contextualizing scientific research. As a collaboration among universities and industry (LI-COR Biosciences), our program itself is a model of 21st century biotechnology research.

Saturday, 9-10:30 AM, LR3
SESSION VII

Trajectories of Drugs

Organizer and Chair: **Jean-Paul Gaudillière**

• Drugs and Chronic Disease, **Carsten Timmerman**, U of Manchester, UK

The 1940s and '50s were a time of remarkable change in western ways of thinking about diseases and their treatment. It became an accepted fact that infectious diseases were a problem of the past and that future efforts had to be geared towards tackling chronic disorders, especially those that came with old age. The populations of Europe and North America no longer died young from tuberculosis, they lived long lives and suffered from heart disease and cancer. The success of the antibiotics, however, had led to the widespread conviction that any disease could be overcome if only the right chemical was found. This paper tells the story of one of the chemicals that contributed to the transformation of high blood pressure from a life-threatening disease into the physiological marker of a preventable risk.

Hexamethonium, a quaternary ammonium compound, belongs to a group of drugs that had long been part of the standard toolkit of pharmacologists and physiologists interested in nerve function. Only in the 1940s, however, these drugs were reinterpreted as ganglion blockers and their potential clinical implications seriously investigated, first in the US and then, more intensively, by pharmacologists and clinicians associated with the UK Medical Research Council. The paper analyses the trajectory of the ganglion blockers from the laboratory bench via the bedside in teaching hospitals to patients' homes, looking at the roles played by historical actors based in these different settings and their interactions with a pharmaceutical industry about to discover the potential of new drugs for chronic disease.

• From 'Inner Disinfection' to Antitoxin: Developing the Serum Therapy: 1885-'95,
Volker Hess, Institute for the History of Medicine, Germany

When Emil BEHRING published his famous reports about the facility of the serum therapy in 1890, in France and Germany scientists developed the experimental object into a commercial product in only some years. My paper will focus this process in which a weak experiment was stabilized and transformed into a stable equipment of manufacturing. In both countries the research had followed different ideas up to this moment: In Paris, YERSIN and ROUX handled to find an attenuated bacterium, while BEHRING haunted the idea to sanitize an infected organism with disinfectant substances. Both research groups transformed their experimental system in a short. At the end of 1894, both produced, traded and sold the diphtheria antitoxin. However, conforming an equalizing the experimental system were accompanied by diverging the manufacturing system. In Prussia, the state bureaucracy patronized the fabrication in free enterprise, while in Paris the Institut Pasteur kept the production private. The Institut Pasteur remained responsible for testing and controlling the produced sera, while the Prussian government established a state agency specialized only on controlling the new biological drugs. Comparing the development of the serum therapy in France and Germany my paper will deal with the production of a biological drug in which an experimental system was transformed into an industrialized process.

• Penicillin: Chemical/Biological? **Jean-Paul Gaudillière**, Cermes-Inserm, France

Penicillin may be seen as the emblem of the regime of medical innovation, which developed after WW II. The wonder drug was a product of the laboratory, a cure for a wide range of infectious diseases, and a commodity massively produced by the US industry. Penicillin exemplified the rising importance of a new category of drugs purified out of biological organisms. Following the development of steroids, hormonal preparations, and vitamins, antibiotics demonstrated that biological research could renew the promises of chemotherapy. Penicillin was however not always viewed as a biological. During the war, massive investments were put in the search for a path toward

complete chemical synthesis. The issue remained open after 1945. This paper will examine the conflicting views of penicillin, which guided the pharmaceutical research agenda in the 1940s and '50s. By comparing the French and the German practices, the paper will contrast scientific, Industrial and social configurations, which respectively led to develop antibiotics as chemicals, or as biologicals. By focusing on two countries where penicillin came late, as a challenge, this comparison also highlights the dynamics of the scientific reconstruction in continental Europe.

Saturday, 9-10:30 AM, LR5
SESSION VIII
Topographies of Knowledge Production in Aquatic Ecology: The German-speaking World, 1870-1940 I
Organizer and Chair: **Thomas Potthast**

• Exploring the Topography of the Living Sea Floor: The Community Concepts of Karl August Moebius and C.G.J. Petersen, **Kurt Jax**

Several concepts of ecological communities were developed in aquatic ecology during its early years. One of the most prominent and in the German-speaking world—even the dominant one for many decades—was that of the Biocoenose (biocoenosis) as developed in 1877 by Karl August MOEBIUS in connection with his investigations of oyster beds. Some years later, C. G. J. PETERSEN introduced his concept of marine benthic communities. In this contribution I will explore the origin and theoretical status of these two concepts, trace the question of their differences (or identity) and the background of their developments in terms of the habitats in which they worked, the methodology which these habitats necessitated and the socioeconomic boundary conditions of their investigations. Finally I will contrast and relate their ideas to other notions of community that were developed during the same period, and discuss the different heuristic values which these concepts had for further ecological research.

• Marine Ecology in the Natural History Museum, 1890-1925,
Lynn K. Nyhart, U of Wisconsin, USA

In this paper, I argue that in the decades around 1900, German researchers investigating marine ecology and biogeography played a significant role in injecting new interest in ecology into museum-based research, helping to reorient biogeographical and even evolutionary questions away from pure morphology and toward environmental-ecological factors. While I consider the work of several museum curators across Germany who specialized in marine organisms, I focus especially on Friedrich DAHL, curator at the Berlin Museum für Naturkunde and a student of Karl MÖBIUS (himself a fountainhead of marine ecology), because DAHL transferred the perspective he had gained as a marine scientist to land ecology as well. As I suggest at the end, not all marine ecological questions and approaches were equally pursued in the museum context; the culture of the museum and its intellectual traditions would shape which ones were taken up and which were not.

• Commentary and General Discussion

Saturday, 9-10:30 AM, LR9
SESSION IX

William Bateson and the Suppression of Epigenetic Biology

Organizers: **Gerd B. Müller** and **Stuart Newman**, Chair: **Stuart Newman**

• Diminishing Returns: William Bateson's Influence on 20th Century Evolutionary Biology, **R.G.B. Reid**, U of Victoria, Canada

With the 1894 publication of *Materials for the Study of Variation, with Especial Regard to Discontinuity in the Origin of Species* William BATESON was in a position to assist the launch of evolutionary epigenetics into the 20th century. However, his insistence on evolutionary discontinuity brought him to Mendelianism and mutation theory. His subsequent diversion into classical genetics, conventionally regarded as his best contribution to biology, took the fire out of his concept of the discontinuous evolution of form. And his "righteous anger" was destructively misdirected at neo-Lamarckism, which had strong evolutionary epigenetics components. BATESON's *meristic variation* was part of the conceptual foundation of homeotic evolution, but the tools for experimenting with such variation at the molecular level did not appear for another century. Consequently, his positive influence on evolutionary theory diminished. Although his students continued to accept evolutionary discontinuity and reject natural selection as a causal agent, these heresies were finally swamped by the modern synthesis. Unfortunately, alternative initiatives by GOLDSCHMIDT, the neo-Lamarckists and the Theoretical Biology Club suffered the same fate. And in the Soviet Union, VAVILOV, who had collaborated with Bateson, suffered worse.

• William Bateson's Physicalist Ideas, **Stuart A, Newman**, New York Medical College, USA

At the turn of the 20th century new attention to the discoveries of MENDEL and the rise of the Weismannist paradigm led to a parting of the ways of genetics and developmental mechanics. From that point on, the scientific analysis of transmission and population genetics proceeded with essentially no connection to embryology, and embryology receded from the mainstream of biological research until late in the century, when it gained new prominence as a subfield of molecular genetics. What was suppressed in this schism was a scientific approach to the origination and evolution of form that took into account processes beyond genetic affinities and change, evolutionary theory having become the exclusive province of geneticists. William BATESON was a key transitional figure in this century-old reorientation of biology. His *Materials for Study of Variation* (1894) featured analyses of the character and origination of organismal morphologies, such as segmentation and serial repetition, based on material properties of tissues and potentially fruitful physical models such as the 'Chladni patterns' formed by particles on vibrating surfaces. Just eight years later, in MENDEL*'s Principles of Heredity* (1902), he backed off of his earlier theorizing, stating "In the Study of Evolution progress had well nigh stopped" until the 1900 disclosure that MENDEL had "cut a way through." Here I will argue that a re-synthesis of genetics and developmental mechanics represented by recent strains of evolutionary developmental biology ('EvoDevo') is revisiting the phenomena and theoretical notions of concern to the pre-1900 BATESON in order to dispel the reductionist legacy of the post-1900 BATESON.

• Betrayed by Balanoglossus: William Bateson's Rejection of Evolutionary Morphology as the Basis for Understanding Evolutionary Change, **Brian K. Hall**, Dalhousie U, Canada

William BATESON (1861-1926) is most remembered for introducing Mendelian genetics into Britain, promoting heredity as the mechanism of evolutionary change, and coining the name 'genetics' for this new field of scientific endeavour. Less well known is that BATESON began his professional life as an evolutionary embryologist with studies on the worm-like marine organism, Balanoglossus, with the aim of uncovering the origin of the vertebrates; was Balanoglossus the last of the invertebrates or the first of the vertebrates? BATESON, however, abandoned Balanoglossus: "the Balanoglossus business … hasn't any bearing whatever on the things we want to know. It came to me

at a lucky moment and was sold at the top of the market" (22 Nov., 1886). How BATESON came to feel betrayed by Balanoglossus and how and why he made the switch to genetics and the analysis of variation from what he termed "the embryological method" as the means to understanding evolutionary change are the topics of this paper. It is intended as a contribution to the history of Evo-Devo, which is the current manifestation of a field with a long history seeking relationships between embryology and evolution. BATESON's reservations arose from the explicit application of embryology to phylogenetic reconstruction. He had observed quite different larval forms of Balanoglossus in the sands of Chesapeake Bay in the USA but saw no way of determining from their morphology which was ancestral and which descendant. Genealogical reconstruction might reveal a likely history of the vertebrates, even a history of life, but could not reveal how that history came to be. For BATESON, understanding how new forms arose became a matter for heredity not genealogy, especially not the speculative genealogy of the type required to establish links between groups such as hemichordates, chordates and likely (or even not so likely) ancestors. "Variation, whatever may be its cause, and however it may be limited, is the essential phenomenon of Evolution. Variation, in fact, is Evolution…. The readiest way, then of solving the problem of Evolution is to study the facts of Variation" (BATESON, 1894, p. 6). The significance of BATESON for the relationship between embryology and evolution was captured by Peter BOWLER in a Foreword to the reprinting of BATESON's *Materials for the Study of Variation*: "Biologists who retain an interest in the developmental process and its relationship to evolution may find his work illuminating for its ability to identify topics and problems which have been set aside in the construction of the genetical theory of natural selection. The few who wonder if a full recognition of the role played by development will threaten the basic principles of Darwinian adaptationism can see how far one of the pioneers of genetics was willing to carry this line of argument" (BOWLER, 1992, pp. xxiv-xxv).

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• William Bateson's Strife with the Vivarium Institute,
Gerd B. Müller, U of Vienna and KLI, Austria

BATESON's own conversion from an 'environmental inductionist', who looked for evidence for his theories in the steppes of Central Asia, to one of the major advocates of the centrality of Mendelian genetics and mutational variation in the early 20th century, led him to combat aggressively all notions of a causal role of the environment in the generation of variation. The Vienna-based Biologische Versuchsanstalt (Vivarium Institute) had its programmatic focus just on this kind of environmental causation. The institute was specifically designed and equipped to promote experimentation involving whole organisms and their entire life-cycles, over several generations and under variable environmental conditions. It generated extensive amounts of empirical data on the influence of environmental parameters on ontogenesis and inheritance. Thus the Vivarium became a prime target for BATESON's attacks against neo-Lamarkianism, which culminated in the well-known KAMMERER affair. Because of the loss of its scientific personnel (many of whom were Jews) and its complete dissolution during WWII, the Vivarium Institute fell into undeserved oblivion. In commemoration of the 100th anniversary of its opening in 1903, the unique role of the Vivarium as a large scale research institute devoted entirely to the experimental study of the relationships between environment, development, and evolution will be described. Because its theoretical foundations included a strong quantitative and mathematical orientation and its research program focused on epigenetic determination, it can be seen as a forerunner of contemporary systems biology and EvoDevo. I will summarize these positions and review some of the issues underlying the discordances between BATESON and the Vivarium.

• The 'Arbitrariness' of the Genetic Code, **Ulrich Steggman**, King's College London, UK

The 'arbitrariness' of the genetic code has been suggested as one of the features which may justify attributing semantic information to some macromolecules, especially to DNA (e.g., GODFREY-SMITH 2000). The 'arbitrariness' of the genetic code is, roughly, the idea that the codon-amino acid assignments could be different than they actually are. But it is not clear what this modal proposition entails and, so far, the concept has escaped a satisfactory explication. I will argue that a precise notion of the 'arbitrariness' between codons and amino acids can be gained from Jacques MONOD's work on enzyme regulation. His analyses of competitive inhibition and allosteric effects during the 1960s involved the concept of *chemical necessity*. I suggest that 'chemical arbitrariness' can be formulated in terms of both nomic necessity and necessary conditions. I try to show why the 'chemical arbitrariness' of the genetic code neither relies on CRICK's (1968) 'frozen accident hypothesis' nor on its being evolutionary contingent in BEATTY's (1995) sense. However, although 'arbitrariness' can be made precise, I will deny that this concept supports semantic claims. More specifically, the 'chemical arbitrariness' of the genetic code is neither sufficient nor necessary for attributing semantic information to nucleic acids.

• The Vertebrate Brain as the Subjective Value Generator: A Plea for Semiotic Conceptualization, **Andrzej Elzanowski**, U of Wroclaw, Poland

The subjective value assignments to items of the outside world, and thus the simple awareness (preconsciousness) of their presence, are relatively easy to explain in terms of ultimate causation. If an organism is intelligent enough and its environment is variable enough, it is a much better solution to make the organism aware of what matters for the reproductive success and let it choose currently available means to reach or avoid certain items than to program (hardwire) it for all possible situations (which may be impossible). This explains why the most intelligent animals (at least mammals, birds, and some other vertebrates) have been employed as managers of their own reproductive success by making them aware of the outside world and assigning positive value to whatever predictably promotes, and negative value to whatever reduces fitness (and thus, usually, survival). The value is assigned to an external item through an association with value-laden (positive or negative) experiences. We now know where in the brain most of negative and positive experiences are generated, but the way (or proximate causation) whereby some molecular or other physical configuration translates into a good or bad feeling defies so far a scientific conceptualization. This translation across the brain/mind interface must rely on subjective meanings being assigned to physical configurations, which makes it by definition semiotic.

• Death and Aging in a Systems Perspective, **Mia Krause**, U of Copenhagen, Denmark

Molecular biology research has revealed many interesting facts of biological systems, but our current understanding of the biological organism as composed of various molecular mechanisms has led to a fragmented understanding of the whole. A good example of a research area marked by this approach is the study of aging and death in biological organisms, a field currently dominated by molecular biology and mathematics. This presentation reviews current approaches and argues that shifting to a Peircean perspective in the understanding of the evolution of complex biological systems, may provide a new and promising way of comprehending the puzzling and still unsolved mystery of biological aging and death.

Saturday, 11-12:30 PM

SESSION I, LR1

Issues in Sociocultural Evolution II

Organizers: **Marion Blute** and **Sigrid Glenn,** Chair: **Marion Blute**

• Repeated Assembly, **Linnda R. Caporael**, Rensselaer Polytechnic Institute, USA

'Genes' are an awkward entity for evolutionary analysis at higher levels of organization, especially when we turn to psychological, behavioral, and cultural levels. Work at these levels acknowledge the necessity for evolutionary accounts that acknowledge developmental systems and interaction with environments. The recurrence of constructive interactions in phenotypic formation over generations suggests a vocabulary of 'repeated assembly' for evolutionary discourse. Repeated assembly is consistent with neo-Darwinism, but focuses on selective processes embodied at different levels of organization (from the cellular level to the group level), situated in specific contexts, and dynamic in operation. Unfortunately, the definition of repeated assembly is bulky and awkward: Repeated assemblies are recurrent, entity-environment relations composed of hierarchically organized, heterogenous components having different temporal frequencies and scales of replication. This paper will unpack this mouthful of a definition, provide some examples, and also discuss what is not a repeated assembly.

• Sociality as a Response to the Upper-Limits Problem, **James Phelps**, U of Connecticut, USA

Pleiotropy occurs when more than one trait is controlled by a single gene or gene complex. If pleiotropy were global along the genome, i.e., if every gene contributed to the formation of every trait, selection would be relatively impotent as a force for evolutionary change. The likelihood that any new mutation would be beneficial in this case would be vanishingly small since the new gene sequence would necessarily have to cooperate with every gene in the genome to produce a useful trait. This is one version of what I call the upper-limits problem. For any given level of biological organization, the greater the number and strength of connections between the units that constitute the level of organization the more constrained selection is to act upon that level of organization. This paper argues that selection faces this upper-limits problem in constructing brains as well. The more selection adds to our cognitive repertoire, the more difficult it becomes for selection to add psychological mechanisms in the future, as a prerequisite for such additions is the coordination of the new device with currently existing devices. Social organization is a response to this upper-limits problem as it makes possible higher-level cognitive divisions of labor. Thus while alterations of our cognitive repertoire by selection might reduce the effectiveness of some psychological mechanisms, the reduced effectiveness is compensated for by other members of the social group.

• Fitness in Explanations of Economic and Social Evolution,
Thorbjoern Knudsen, U of Southern Denmark

In the life sciences, the development and use of a technical fitness is associated with the advance of useful formal representations of selection. In the social sciences, the use of evolutionary models has increased dramatically over the last decades. Unfortunately, the usefulness of evolutionary models has remained somewhat limited because of conceptual confusion, disagreement, and a limited use of the mechanics of formal selection theory. Recently, the advance of core Darwinian principles has led to promising conceptual developments. Evolutionary economists are beginning to address core issues regarding the definition of social phenotypes and social genotypes as well as replicators and interactors. Further progress seems to lie in developing a rigorous and sufficiently general definition of selection. The purpose of the present article is to the help advance the use of formal selection theory in evolutionary explanations of economic and social change by addressing

unsolved issues regarding the definition of fitness. The article builds on previous work that advances PRICE's general definition of selection. The contribution is to identify a possible measure of fitness that can be used in economics as well as broader studies of social change.

Saturday, 11-12:30 PM, LR2
SESSION II
Biology of Human Behavior II
Contributed Papers, Chair: **Lisa Gannett**

• The Politics of Group Versus Individual Differences in Behavioral Genetics,
Lisa Gannett, California State U, Chico, USA

The disciplinary history of behavioural genetics identifies Francis GALTON as its founding father, yet averts discussion of the eugenics movement he founded. Recent history is portrayed in a light that places behavioural geneticists in the position of underdogs—marginalized by the popularity of John WATSON's behaviourism, maligned by post-WWII opposition to eugenics, and unfairly targetted by politically inspired attacks on Arthur JENSEN. Recent successes in behavioural genetics, on this account, are due to truth's decisive victory over politics. It is recognised as well, however, that this victory was aided by a strategic focus on individual rather than group differences in the application of the new techniques of molecular genetics to variability in human behaviour. This historical narrative and this research strategy sidestep questions about racism and ethnocentrism, despite the racialisation of behavioural differences that has occurred in the fields of evolution, genetics, and anthropology ever since LINNEAUS, KANT, BLUMENBACH, DARWIN, GALTON, FISHER, and others. In this paper, I consider some philosophical questions implicated in population-based approaches to behavioural genetics research. I look at a couple of controversies that have occurred regarding the existence and investigation of behavioural differences between human populations—the 1950 and 1951 UNESCO Statements on Race and Arthur JENSEN's infamous 1969 article. I concentrate on points of consensus as well as dissension among parties to these controversies. This reveals that there is more proximity on a number of scientific and conceptual issues between opposing sides than is generally recognized—there is agreement, for example, about the existence of biological populations as objective entities, the distinction between facts and values, the objectivity of genetic differences between human groups, and the limited significance of these differences for making judgements about individuals. Many of the differences between sides are about values and politics. I defend the position, however, that these value and political differences are not external to, but inextricable from, the science of behavioural genetics.

• What are Natural Inequalities? **Tim Lewens**, Cambridge U, UK

People are not born with the traits that they need to secure access to desirable goods and positions. Moreover, in most cases there will be many possibilities for (say) the level of intelligence that a newborn might eventually attain (some likely, others much less so) that turn on what kinds of social and biological resources are made available to it through development and into adulthood. In this paper I show how these platitudes of developmental biology undermine the distinctions sometimes used by political philosophers between natural and social inequalities, and between natural and social goods, and why some conceptions of the equality of opportunity are consequently untenable.

• Evolutionary Psychology and Behavior Genetics,
Marko Barendregt, Vrije Universiteit, Amsterdam, The Netherlands

Evolutionary Psychology (EP) and Behaviour Genetics (BG) are two biological approaches to explain behaviour and both make reference to genes. Nonetheless, the development of these disciplines is anything but interdependent. This paper tries to locate and explore possible fruitful points

of contact between explanations offered by EP and BG. Firstly, a prominent view is criticised, which claims that any interesting connections are lacking, because EP focuses on *universal* adaptive design while BG focuses on adaptively insignificant *variation*. This view is untenable as genetic variation actually may be adaptive and recent molecular genetics does try to explain universal behavioural features. Secondly, an alternative view is developed on the basis of philosophical discussions on the nature of scientific explanations. Some philosophers of science rightly acknowledge that explanations do not necessarily involve universal laws, but requires invariant generalisations. In biology, invariant generalisations are often causal regularities (or mechanisms) that are invariant only within a certain domain. Successful explanation requires invariant generalisations *and* identification of the domain of invariance. I will argue that while BG attempts at causal mechanical explanations of behaviour, EP accounts for the domain of invariance of those causal generalisations. In this way, both BG and EP are needed for offering complete and successful explanations.

Saturday, 11-12:30 PM, LR6
SESSION III

Challenging the Essentialist Story about the History of Taxonomy II

Organizer and Chair: **Mary P. Winsor**

• The Functions of Myth: 'Essentialism' as the Foe of 20th Century Evolution Theory,
Ron Amundson, U of Hawaii, USA

It took nearly forty years, but historians of science have now pretty well established that pre-Darwinian taxonomic practices, and beliefs in species fixism, were not under the control of ancient Greek doctrines like Platonic typology and Aristotelian essentialism. It is now time to ask why. Why was the myth of essentialism as the foe of evolutionary biology so prevalent between 1959 and 2000? I believe that the answer has to do with the structure of Modern Synthesis evolutionary theory, with its commitment to phenotypic gradualism, adaptationism, and its emphasis on organic diversity. Evolutionary views that stressed the Unity of Type, and the commonalities of ontogenetic development, were seen to be inconsistent with the principles of the Modern Synthesis. The identification of essentialism as the antithesis of modern evolutionary thinking provided a useful category of analysis. The 'essentialist' category grouped non-Synthesis 20th century theorists (e.g., GOLDSCHMIDT) together with pre-Darwinian (alleged) anti-evolutionists (e.g., OWEN). It then blamed them all on a recognizably silly metaphysical doctrine. It is a significant historical coincidence that the myth of essentialism is being exposed at the same moment in history that the ontogenetically-based evolutionism of Evo Devo is on the rise.

• After 40 Years of Stasis. What Would It Mean to Overcome the Essentialism Story?,
Gordon McOuat, U of King's College, Canada

Essentialism is dead, both in late modern biology and now, it seems, in the late modern historiography of biology. This paper will explore the consequences of the 'end of essentialism' for writing the history of the life sciences. Three short case studies, set in the very thick of the struggle over essentialism, will be examined showing that essentialism was much more than just another myth to be overcome. This will be followed by a short manifesto for future histories of 'essentialism', classification, and the life sciences.

• The Essentialism Story and its Impact in Paleobiology and Macroevolutionary Theory,
Gunther J. Eble, KLI, U of Leipzig, Germany, and U of Burgundy, France

The essentialism story promoted the spread of population thinking among paleontologists, and encouraged microevolutionary research on the quantification of morphometric variation within fossil lineages. This contributed to the emergence and acceptance of Paleobiology as a discipline firmly

in the domain of evolutionary biology. However, most major research agendas in Paleobiology have concerned macroevolution and the long-term dynamics of species and higher taxa treated as discrete entities, on temporal scales in which population-level variation is often not informative. Thus, research themes such as theoretical morphology, punctuated equilibrium, particle paleontology, hierarchical sorting and selection, and the quantitative analysis of large-scale patterns of diversity, disparity, and morphospace occupation, among others, often use as basic data large numbers of taxa vetted, with the method of exemplars, by generations of paleontologists both before and after DARWIN. The investigation of questions adapted to the resolution of the fossil record and the routine use of statistics in paleobiological inference tend to further minimize biases of taxonomic practice, allowing robust generalizations about macroevolution to be produced without ad hoc ontological commitments. All in all, the essentialism story has been quietly and consistently challenged, through sensible indifference, in the practice of macroevolutionary research. Further, the purported moral of the essentialism story is at odds with much of current macroevolutionary theory, which pluralistically incorporates elements of both typological and population thinking, corrupts population thinking by coopting it into 'clade thinking' to support a hierarchical view of evolution, and ultimately advances a defensible notion of material, constructional essence with its recurrent emphasis on concepts such as constraint, stasis, homology, Bauplan, and frozen accident.

• Commentator: **Giovanni Camardi**, Universita di Catania

Saturday, 11-12:30 PM, LR9
SESSION IV

Perspectives on Population I

Organizer: **Michael Dietrich**, Chair: **Anya Plutynski**

• Rehabilitating a Democratic Eugenics in the Postwar USA: Its Implications for Relations Between Social and Biological Scientists of Human Populations,
Edmund Ramsden, Independent Scholar, UK

Considering the relevance of demographic statistics for population genetics, and of genetic structure to any population, the convergence between the disciplines of genetics and demography should be great. However, in practice, it was not until the 1960s that demography made any significant inroads into the study of human genetics, and genetics has only had a limited impact upon mainstream demographic study. This paper will first briefly outline how the growing stigma of eugenics in the 1940s was partly responsible for disrupting the growing connections between biological and social students of population that had developed in the interwar period. The paper will then explore how the reemergence of a eugenic agenda in the population field in the 1960s, was a crucial force in reuniting social and biological population students within a program of genetic demography. While many sought a new eugenics based upon the revolutionary potentials of molecular genetics, reproductive technology, or extreme measures of population control, my paper will explore how it was precisely these controversial concerns—with realizing an optimal genotype or an optimum population—that encouraged many to look for an alternative and more democratic program of 'population eugenics'. Such a program would analyze and influence the processes of assortative mating and differential fertility as both a precondition to and consequence of the success of the welfare democracy. In contemplating such a program of eugenics, leading population scientists were encouraged to reclaim the population field's interdisciplinary past and develop a genetic demography, through the influence of reform eugenicists such as Frederick OSBORN.

• Whatever Happened to the Unity of the Genotype? **Anya Plutynski**, U of Utah, USA

This paper will discuss the origins and fate of MAYR's influential idea of the unity of the genotype. Up through the early 1980's MAYR's influential concept of the unity of the genotype still held sway with many biologists (CARSON, TEMPLETON) who were interested in speciation. After about 1985,

the founder model of speciation seems to have lost it's momentum. Why? Was it simply the mathematical arguments proffered by population geneticists such as BARTON and CHARLESWORTH, or has there been a shift in the conception of the genome such that speciation no longer seems to require a 'genetic revolution'? What has been the fate of the 'balance' theory and how has shifts in our conception of the genome affected theories of speciation?

• Theoretical Articulation in Population Ecology: From Spreading the Risk to Metapopulation Dynamics, **Greg Cooper**, Washington and Lee U, USA

This paper examines the development of theoretical ideas in population ecology, from the deployment of fairly local verbal models, through the articulation of comprehensive, though still verbal, theoretical frameworks, to the development of a body of mathematical theory. I look at this process in the context of a specific case study: the transition from what was once known as 'spreading the risk' to the theoretical juggernaut we now call metapopulation dynamics. The analysis begins with the emergence of the idea of spatially structured populations in the work of Sewall WRIGHT in the 1930s. Owing to the isolation between population genetics and population ecology, WRIGHT's work did not take hold among the ecologists. The first indications that spatial structure might have significant demographic consequences appear in the work of Australian ecologists H. G. ANDREWARTHA and L. C. BIRCH in 1954. Dutch population ecologist Pieter DEN BOER was the first to give the idea a name: he called it spreading the risk. A year later, in 1969, Richard LEVINS provided the first mathematical articulation of the idea. Around the same time DEN BOER, together with Joannes REDDINGIUS, developed an alternative mathematical approach. The two theoretical treatments suffered very different fates. LEVINS' theoretical model went on to become the fountainhead for metapopulation dynamics, whereas the work of DEN BOER and REDDINGIUS did not have widespread impact. The paper speculates on the reasons for these very different historical trajectories and concludes with a discussion of the implications of the case study for our understanding of the explanatory power of theoretical ideas in ecology.

Saturday, 11-12:30 PM, LR3
SESSION V

Microbial Challenges

Organizer and Chair: **Robert I. Krasner**

• The Sociobiology of Vaccination, **Peter Nicholls**, U of Essex, UK

Since JENNER it has been generally accepted that vaccination protects the individual against smallpox in the community. Yet public acceptance of vaccination, especially compulsory vaccination for children, was slow in coming. The antivaccination league was one of the strongest social movements in late Victorian England with many resisters fined or even imprisoned for refusing vaccination for themselves or their children. It recruited some famous supporters, most notably George Bernard SHAW and Alfred Russel WALLACE. WALLACE was able to show statistically that whether or not the individual could be protected by vaccination the communities that embraced vaccination programmes were often more susceptible to smallpox attacks than those who took alternative prophylactic measures. More recently in the UK the multiple vaccinations of children for other diseases have come under criticism for their possible links to autism and bowel disease. The so-called Gulf war syndrome has been arguably linked in part to the multiple vaccinations undergone by the troops. Smallpox was declared abolished in the late 1970s by WHO and general anti-smallpox vaccination programmes ceased. But proposals have now been put forward for renewed vaccination of key medical and other workers, and perhaps of members of the general population, in the shadow of possible threats of terrorist release of smallpox virus. Yet vaccination against smallpox—and possibly the still current multiple vaccinations of children against diseases like mumps, measles and rubella—is not risk free. The most advantageous personal strategy is to try to remain unvaccinated amid a vaccinated herd—in game theory terms, to be a defector. The advan-

tage to be gained from this strategy depends upon the several risks involved as well as the strategy followed by the herd itself. In the UK the take-up of the MMR vaccine by parents hovers at the 85-90% level, too low, say epidemiologists, to control the spread of the diseases but interestingly somewhere near the Nash equilibrium strategies for the system with reasonable values for the pay-offs. The paper will examine this situation, the behavioural links to other strategies involving co-operation and defection in our evolutionary past, and the tensions between morality, politics and biology in assessing risk informally and socially, as well as formally and technically. It will be concluded that although it is impossible to determine the absolute risk of a recurrence of smallpox, such a risk must be less than that of vaccination to an individual member of the society chosen at random. The author makes this calculation as one whose parents, followers of Bernard SHAW, refused him smallpox vaccination as a child, yet who may today retain some residual immunity to the disease consequent upon adult vaccination for an international travel card in the 1950s and 1960s. Others may calculate differently.

• Interpreting the Plague After Yersin: Germ Theory and the Writing of History,
Richard Keller, U of Wisconsin, USA

As several historians of science and medicine have recently argued, the idea of a 'bacteriological revolution' in the late 19th century is increasingly in dispute. While the work of PASTEUR, KOCH, and others was enormously influential in some circles, many leaders in public health either resisted 'germ theory' or applied knowledge about microbes haphazardly until well into the 20th century. The third pandemic of bubonic plague, which engulfed South and East Asia and established a beachhead on every continent after 1894, heightened the intensity of debates about the microbial origins of disease. YERSIN's and KITASATO's independent discovery of the plague bacillus, followed by SIMOND's research on rat and flea vectors for the disease, indicated clear pathways for fighting the pandemic, yet many scientific and political authorities clung to environmental and often racial interpretations of the disease's origin.

Curiously, proponents on both sides of the debate frequently turned to history to as a means of supporting their cases. The spread of plague at the turn of the 20th century encouraged a wave of histories of previous pandemics, written by scientists, historians, and public health officers. Some asserted that the existence of *Y. pestis* explained formerly incongruous facts known about pandemics such as the Black Death of the 14th century, and argued that YERSIN had 'cracked' the mystery that surrounded plague. Others rejected such claims, stating that the reduction of plague to a microbe stripped the pandemic of its cultural significance as a metaphor for a society under siege.

The paper explores how anxieties about emerging and reemerging diseases give rise to new examinations of the past, and how history-writing can serve as a tool for both upholding and discrediting new scientific theories. Drawing on histories of plague that appeared in scientific and medical journals, public health reports, and popular literature, the paper indicates the ways in which epidemic diseases often acquire a mystique in the cultural imagination that both heightens interest in their origins and makes them resistant to new interpretations.

• Public Health Advances of the 20th Century in Infectious Disease Control,
Robert I. Krasner, Providence College, USA

A 17th century preacher stated, "Prevention is much better than healing because it saves the labor of being sick." It wasn't until the last century, however, that public health was practiced on an organized basis resulting in a better and a longer life in many nations. Public health measures aimed at society at large, more than curative medicine, have been responsible for retreat of the microbial diseases that have plagued mankind from antiquity. A report of the Centers for Disease Control and Prevention published Ten Great Public Health Achievements of the USA, 1900-1999. Sanitation and hygiene, safer foods and clean water, and vaccination were three achievements cited in the control of infectious diseases. What happened to make societies invoke public health strategies to control microbe-caused diseases? The answer is that the latter half of the 19th century was identified as the 'Golden-Era of Microbiology'. During that time, PASTEUR, KOCH, and other pioneer-

ing microbe-hunters extended their findings from the laboratories into the fabric of society. PASTEUR reasoned that since microbes caused diseases in wine, they could cause diseases in humans; KOCH's postulates established the link between a specific microbe as the causative agent of a specific disease. Prior to those years, and despite LEEUWENHOEK's early descriptions of 'animalcules' in 1658, the role of microbes as agents of disease was not hinted at. Early thinking as to the cause of plagues was acceptance that disease and death were a part of life caused by 'miasmas', a term vaguely described as 'bad air' or 'swamp air'.

Population growth and urbanization during the 19th century were major factors in the presence and spread of infectious disease. John CAIRNS, a British biologist, termed cities "graveyards of mankind"; filth and squalor prevailed. Towards the end of the century, on the heels of the germ theory, the sanitary reform movement gradually arose and focused on public health strategies; sanitation was 'in'. Upton SINCLAIR's powerful 1906 novel, *The Jungle*, and his description of "Packingtown," a filthy Chicago stockyard contributed to the passage of the Pure Food and Drug Act within a year followed by the enactment of other public health measures to protect society.

The decline of many bacterial and viral diseases, in some cases approaching 100%, in the past century is a tribute to new vaccines and vigorous immunization campaigns. Over twenty vaccines are now available, the majority being introduced in the 20th century. However, there is a serious backlash in terms of social acceptance. Antivaccination leagues have a long and controversial history; their voice is now particularly strident because of the claimed association of multivaccination and an increase in autism.

This presentation will emphasize the role of public health and its interaction with the public at large.

Saturday, 11-12:30 PM, LR8
SESSION VI

Biology and Education II

Organizers: **Charbel Niño El-Hani** and **Steve Fifield**, Chair: **Steve Fifield**

• Philosophical Contribution to a Better Clarification of the Conceptual Context and Goals of Environmental Education, **Kostas Korfiatis**, U of Cyprus

The science of ecology provides essentials concepts for the framing of the conceptual background of environmental education. Concepts like sustainability, balance of nature, complexity and stability, etc., are used in order to describe what is nature and how it works. The above concepts and the relevant worldview are considered legitimized because they relay on ecological science. That causes various distortions. The whole treatment of ecology into the educational literature is based on an old-fashioned positivistic approach of science, which is in contradiction with basic principles of environmental education, especially the ones concerning the development of critical thinking. Besides, the wrong use of scientific concepts in areas outside their domain of application leads to the creation of 'overloaded' concepts, i.e., concepts with many different interpretations and without explanatory value. Theoreticians, trying to adjust ecological scientific concepts into a sociological framework, and to give them meaning within it, render them to ill-defined concepts. The introduction of philosophical analysis into environmental educational process could contribute to the establishment of a better relationship between the science of ecology and environmental education, as well as a better clarification of the role and meaning of the concepts used in environmental education literacy.

• Natural History in Russia, 18th-19th Century,
Alexei Kouprianov, European U at St. Petersburg, Russia

When the first textbooks of Natural History appeared in Russia in the 1780s, they were full of utilitarian remarks on the 'economical' use of naturalia and fixed on the properties of every particular species of plants, animals, and minerals. By the end of 1850s, they undergone a profound

transformation turning more philosophical and concentrated on the higher levels of taxonomic hierarchy which were thought to reflect the indisputable Laws of Nature. The sociologists of knowledge say that every conceptual change has to be understood as a social change, a change in the networks of diverse actors involved in the production of knowledge. The present paper explores the interrelationships between the changes in the organisation of education and science in Russia (Gymnasia, Universities, the St. Petersburg Academy of Sciences) and the changes of the rhetorics in the textbooks of Natural History.

Saturday, 11-12:30 PM, LR7
SESSION VII

Bioinformatics and the Transformation of Biomedical Research

Organizers: **Joan Fujimura** and **Bruno Strasser**, Chair: **Joan Fujimura**

• Protein Biochemistry, Computational Biology, and the Construction of Molecular Phylogenies,
Joel Hagen, Radford U, USA

Phylogenetic analysis using fast computers, computer networks, and molecular data bases, is an important part of the interdisciplinary field that has come to be known as bioinformatics. Despite frequent claims that this area of science is in its infancy, the origins of computer-based phylogenetic analysis dates back to the mid-1960s. A decade before DNA sequencing became feasible, computational biologists focused on the rapidly accumulating data from protein biochemistry. Without the benefits of supercomputers or computer networks, these scientists laid important conceptual and technical foundations for phylogenetic methods used today. This paper examines the motivations for using computers to study the structure, function, and evolution of proteins. It also examines some early claims of computational biologists for revolutionizing evolutionary biology, and some of the consequences of their preliminary attempts to compute the evolutionary histories of protein molecules and the organisms from which they were drawn.

• A History of Sequence Databases,
Bruno J. Strasser, Institute for the History of Medicine and Health, Switzerland

Protein sequence databases have become an indispensable tool for contemporary biomedical research. They combine different types of information (protein sequences, related diseases and biomedical literature) and make them available on-line to researchers around the world. The history of this essential tool can be traced back to the immediate postwar years, when computers began to be used in protein crystallography. In the 1960s, however, three different initiatives emerged which were to play an essential role in the emergence of modern sequence databases. The first atlas of protein sequences was published in 1966, by Margaret DAYHOFF. It was made possible by the development of faster and easier sequencing methods. The same year, V. MCKUSICK published the first edition of the Mendelian Inheritance in Man, a catalogue of genetic disorders, At the same time, several projects were carried out to automate medical bibliographic information retrieval. My paper will bring the history of sequence databases in the context of these three initiatives.

Saturday, 11-12:30 PM, LR5
SESSION VIII

Topographies of Knowledge Production in Aquatic Ecology: The German-speaking World, 1870-1940 II

Organizer and Chair: **Thomas Potthast**

• Swiss Connections in Early Aquatic Ecology: In the Midst of Border Crossings,
Astrid E. Schwarz, Technical U of Darmstad, Germany

The very beginning of freshwater research, especially the 'invention' of the ecological lake, lies in Switzerland. A central role in this net of Swiss connections is played by the Vaudois François Alphonse FOREL, who was already enthroned as the founder of the new discipline limnology by his contemporaries. FOREL and his colleagues initiated and intervened in activities around lakes, by crossing a number of borders, e.g., those of laboratory and fieldwork, of technical and scientific interest, of French and German language, of political and scientific institutions, and of course several disciplines such as geology, hydrography, and botany. In this paper I will discuss how those border crossings created a zone of vagueness, a 'no man's land' in which aquatic ecology could arise and to what extent this may have been supported by the specific Swiss national context which also can be analyzed as a zone in the midst of border crossings.

• Topologies of Topology: Spaces of Research, Spirit of Place, and the Classification of Lakes,
Thomas Potthast, U of Tübingen, Germany

The unity of 'Lebensraum' and 'Lebensgemeinschaft' has become a prominent theoretical building block of ecology mainly, but not only, in the German speaking context. In this paper, the development of theories and research practices investigating this unity will be discussed with regard to the classification of lake types in freshwater ecology. Efforts to produce a typological system revealed a major tension between the place specificity of a 'regional limnology' and the aim of producing universal laws to build up a 'general ecology' of lake types on a global scale. Focussing on August THIENEMANN and his co-workers at the 'Hydrobiologische Anstalt' of the Kaiser Wilhelm Society at Ploen (northeast of Hamburg), these tensions can be reconstructed in a narrative both of success and failure of a spatially explicit ecology. Local studies of the lakes around Ploen, methodological innovation, and frequent visitor exchange fostered a strong research program and several refinements of the typology of lakes. It was partially the result of an expedition to the Indonesian archipelago that led THIENEMANN to abandon the attempts for global system of lake based geomorphological criteria. Typologies turned out to be applicable only regionally. However, the problem was not only that of conflicting data. The cultural context of a specific way of doing ecology as 'Wissenschaft', not only as a pure science, has to be taken into account to analyse the causes and consequences of topological typologies in German aquatic ecology in the first half of the 20th century.

• Commentary and General Discussion

Saturday, 11-12:30 PM, LR10
SESSION IX

Biology and Metaphysics I

Contributed Papers, Chair: **Jeremy Kirby**

• Metaphysical and Scientific Trends in the Italian Debate About Theoretical Biology and Life Sciences at the Beginning of the 20th Century, **Massimo Stanzione**

As elsewhere in the world, in Italy, too, the Darwinian heritage exerted a very strong impact on the intellectual community. Starting from the seminal works of Giovanni CANESTRINI and Michele LESSONA (both translators of DARWIN's works), many authors tried to (re)interpret the new evolutionary thinking, extending it to the fields of anthropology (Paolo MANTEGAZZA), psychiatry (Cesare LOMBROSO), as well as psychology and philosophy. At the same time, Darwinism provoked the polemic reactions of many Catholic writers (as the great naturalist Padre Antonio STOPPANI, who cried against "the natural sciences corrupted by sensism," i.e., against any approach to nature, man, and society "supported by a naturalism which is the father of socialism and nihilism"). Therefore, any attempt to make the faith in Biblical narratives and the evolutionary paradigm agree, as proposed firstly by Filippo DE FILIPPI and then by the founder of the so-called Italian 'modernist' movement, Antonio FOGAZZARO, was doomed to fail. This was the intellectual framework from which arose, from 1890 up to the third decade of the new century, the philosophical battle of the neo-idealists Benedetto CROCE and Giovanni GENTILE against the 'old positivism' and its misleading and a-theoretical 'evolutionary metaphysics'. The main critical argument used by CROCE and GENTILE in their anti-positivist crusade was the "lack of theoretical (i.e., conceptual) clarity" in defining life and its specificity with regard to the physical world. In this vein, the biological sciences represented for them a good opportunity to reassert the pseudo-conceptual character of naturalistic and historical-naturalistic explanations. Even if, notoriously, these neo-idealistic positions prevailed in the Italian philosophical culture, many prominent scientific scholars exerted a long lasting resistance to it. This resistance, even when based on different philosophical and ontological tenets, was always primarily founded on epistemological considerations. They were: the mathematician Federigo ENRIQUEZ, the psychologist and philosopher Francesco DE SARLO, the neurologist and psychologist Padre Agostino GEMELLI (who founded the 'Sacro Cuore' Catholic University), the sociologist Eugenio RIGNANO (who, together with ENRIQUEZ, in 1907 founded *Scientia*, the first Italian review in epistemology and philosophy of science), the physiologist Filippo BOTTAZZI. Some of them (BOTTAZZI, GEMELLI, and RIGNANO) were primarily interested in the philosophy of biology. In my paper I will illustrate their contributions to some crucial questions that are still open regarding this field of study (mechanistic vs. vitalistic explications of biological organisms, the peculiar character of biological explanation in relation to the laws of physics, the debate about the neo-Lamarckian and neo-Darwinian evolutionary paradigms, the mechanisms of biological memory and of the supposed hereditary transmission of physical and mental traits, and some major themes of evolutionary epistemology). I will try to demonstrate that, at the beginning of the 20th century, the best part of Italian scientific culture was up to date with the more advanced research lines in empirical biology, and, at the same time, firmly convinced of the epistemological feebleness of the prevailing neo-idealistic trends.

• Living Matters, **Jeremy Kirby**, Florida State U, USA

ARISTOTLE thought that living things were compounds of essence and matter. And he famously held that a body that is not alive is a body in name only. J.L. ACKRILL has argued that ARISTOTLE cannot pick out the material constituent of a living thing independently of the essence. For without the essence, there is no life. And without life, there is no body to which one might refer. Hence, ARISTOTLE's hylomorphic theory cannot, as it seems, make room for the *hyle*. The project I am engaged in here is that of locating the extent to which a key assumption in ACKRILL's argument can be challenged. Can one specify the material of a composite qua material, when its identity depends on the essence of the thing of which it is a constituent?

• Muscular Psychophysics Continuum, **Jorge de Barros Pires**, UNOPAR, Brazil

This contribution proposes an analysis the human skeletal muscle physiology as a sign process, based on Charles S. PEIRCE's thought. It contributes to understand the psychophysical relations in the human body by considering muscles an including sphere of mental processes. This paper first discusses the nature of the muscular conducts. Following PEIRCE's Objective Idealism, it is argued that muscle contraction is a typically mental process, general and eidetic. In the second part, the muscular physiological conduct is discussed as experience based habits, anticipated by PEIRCE. In the third part, the relation between final and efficient causation in muscular conduct is examined, and in the fourth, the phenomena of indeterminacy in muscular contraction. It is argued that objective chance is solely responsible for originating spontaneous and creative muscular conducts. In a fifth part, I discuss the relation between the acquired auto-controlled muscular habit and the three different types of reasoning proposed by PEIRCE: Abduction, Deduction and Induction. I conclude by suggesting the hypothesis that the basis of all muscular cell conduct is evolutionary rationality. The triadic model of Sign proposed by PEIRCE allows us to observe a wider rationality in muscular conduct. It shows us the higher and refined properties of a Mind that learns by experience.

• Towards a Post-mechanistic Nature? Biotechnology and the Contemporary Body,
Marko Monteiro

This paper aims at a discussion of how contemporary biotechnological interventions in the body, through techniques such as genetic engineering, production of bioreactors and of transgenic organisms, has the potential of shifting our prevailing views on nature and its relation to the social sphere. Through a reading of how modernity, through such authors as Francis BACON and DESCARTES, was founded by establishing a particular form of conceiving reason, and a view of nature as eminently material in contrast to spiritual or mental worlds, I intend to debate how far these representations or worldviews have changed in light of current developments in biotechnology. The main objective is to speculate on how modernist worldviews do not allow for a thorough comprehension of the extent to which the sphere once known as 'nature' has been colonized by 'culture'. Thus maintaining this distinction in modernist terms may be counterproductive for an understanding of contemporary times. Feminism and current studies on biology and the body show that a reworking of the natural through the technological may require that we propose new cosmologies and new forms of reason. The body as site of intervention and as material base of human consciousness is a privileged battle ground for these distinct ways of perceiving reality, and will be the focus of the paper.

This paper is a smaller version of my PhD research in Social Sciences being developed at the State University of Campinas, Brazil.

Saturday, 2-3:30 PM

SESSION I, LR6

The Romantic Conception of Life

Organizer: **Phillip Sloan**, Chair: **Manfred Laubichler**, Arizona State U, USA

• Romantic Biology and British Life Science: Does Darwin Belong in this World?
Phillip Sloan, U of Notre Dame, USA

Robert RICHARDS has made some bold claims about the affiliation of DARWIN's project to the development of German Romantic biology. In this paper I will sketch out some key features in the

British reception of German Romantic biology in the period relevant to the origin of DARWIN's ideas and the ways in which DARWIN may and may not be related to this tradition.

• Why Bob Richards is Completely Wrong in Making Darwin into an *Ersatz* German,
Michael Ruse, Florida State U, USA

In his impressive new book, Bob RICHARDS argues that German ideas were the chief influence on Charles DARWIN in the writing of his *Origin of Species*. I argue that he was completely wrong on this, and that DARWIN was the quintessential Englishman, in his science as in all else.

• Response to Warranted and Unwarrented Criticisms of *The Romantic Conception of Life*,
Robert J. Richards, U of Chicago, USA

I am delighted that such astute historians and philosophers of science as Phillip SLOAN, Michael RUSE, and Manfred LAUBICHLER wish to provide critical evaluations my book. In response, I will, very briefly, attempt to state its central argument, namely: the usual assumption about Romantic science—that it formed a backwater in the history of 18th and 19th century thought—cannot be sustained. I rather maintain that major themes in Romantic biology formed the deep currents of 19th century science and carried even DARWIN's theory of evolution into the modern age. From the general conception of nature to more particular assertions about embryology and morphology, DARWIN, I believe, so utilized the resources of German Romanticism, that it would be fair to characterize that icon of English sobriety a Romantic biologist. The panelists undoubtedly will have probed some tender spots in the text, and I will, consequently, have to cauterize the wound—and fend off further infection with sterile patches and a hot poker.

Saturday, 2-3:30 PM, LR8
SESSION II

Ecology and Environmental Values

Organizer: **Heather Douglas**, Chair: **Matt Chew**

• Invasion Ecology: Bates or Elton? **Matt Chew**, Arizona State U, USA

In the 1950s, two significant syntheses were undertaken regarding the phenomenon of species transport and introduction by humans. The first was Marston BATES' "Man as an Agent in the Spread of Organisms," given in 1955 at the Wenner-Grenn Foundation (for Anthropological Research) International Symposium, "Man's Role in Changing the Face of the Earth" at Princeton, NJ, and published in its 1956 proceedings. The second was a short, semi-technical book by Charles ELTON of the Oxford Bureau of Animal Populations, titled *The Ecology of Invasions by Animals and Plants*, published in 1958 and based on his series of BBC Radio talks called "Balance and Barrier." Both the American and the Englishman were well-known in academic and popular circles. BATES's paper preceded ELTON's book by two years, appeared in an interdisciplinary academic context and covered, more succinctly, many of the same cases and concepts, but *The Ecology of Invasions…* has been cited over ten times more often, and was recently 'rediscovered' and republished. I compare and contrast the works and show how ELTON's treatment appealed (and appeals) more than BATES's to environmental activists, and exemplifies a diminished concern for uncertainty and objectivity among scientists enlisting to fight 'the second greatest threat to biodiversity'.

• Environmental Protection in a Value-neutral World, **Laura Landen**, Providence College, USA

Many environmental philosophers develop arguments for intrinsic or inherent value in the nonhuman world. These arguments frequently prove useful for those persons seeking to effect policies for environmental protection. Most ecologists and evolutionists today, however, emphasize that

the natural world is value-neutral and maintain that natural processes are not directed toward some ideal, climax state which ought to be preserved.

In this paper I examine the basic approaches taken in developing philosophical arguments for the intrinsic or inherent value of nature. In doing so, I show that, ultimately, all such arguments rest upon a single metaphysical principle: To exist is better than not to exist. Next, I examine the basis for this principle and the extent of its validity, concluding that its application in establishing a basis for normative behavior entails an equivocation about the good. Finally, I lay out an alternative way of approaching environmental protection that coheres with developments in ecology and evolution without falling into the trap of relativism.

• Ecology and Other Values in Dutch Nature Conservation,
Henny van der Windt, U of Groningen, The Netherlands

In this paper it is shown how ecological approaches are linked to other (normative) considerations. Based on current philosophical schemes and reflections an analytical framework is proposed, presenting three main visions on nature. These are a wilderness, an arcadian and a functional perspective. Each of them can be seen as a specific arrangement of ecological, ethical, and aesthetic considerations. Different ecological approaches here are production-oriented ecology, structure ecology, and process-oriented system ecology, which are linked, respectively, to strong anthropocentric, moderate anthropocentric (stewardship/partner) and ecocentric ethical viewpoints. These 'valuation arrangements' can be recognised in Dutch papers of governmental and nongovernmental conservation organisations and in texts of local stakeholder groups, such as farmers, environmentalists, and politicians. It is asked whether these arrangements are stable or not, and to what extent ecology enables or complicates communication and consensus in decision making processes on nature conservation.

Saturday, 2-3:30 PM, LR7
SESSION III

Inheritance and Evolution

Organizer and Chair: **Peter Godfrey-Smith**

• The Benefits of Infidelity, **Eva Jablonka**, Tel Aviv U, Israel

The paper will examine the widespread assumption that fidelity through inheritance is a necessary property of a system that evolves through natural or artificial selection. I shall compare the genetic and the nongenetic inheritance systems and show that when the generation of variation is targeted and constructed, as it is in most nongenetic inheritance systems, fidelity is not necessary, and may in fact be detrimental. The meaning of infidelity and its effects on the nature and direction and of evolutionary processes will be examined.

• Mechanisms of Heredity and Evolutionary Theory, **Patrick Forber**, Stanford U, USA

The efficacy of natural selection depends upon a system of inheritance. Until recently evolutionary theory assumed that genetics described the sole mechanism of inheritance. Now, as JABLONKA argues, there are a myriad of alternatives. To help evaluate the evolutionary significance of these alternatives I will follow an approach proposed by STERELNY and identify the features of an ideal inheritance mechanism from the perspective of evolutionary theory. The analysis aims (1) to establish that the evolutionary significance of an inheritance system depends more on the properties of the whole system than on the properties of the replicators involved, and (2) to uncover the empirical assumptions behind the arguments for the relevance of nongenetic inheritance.

• Inheritance and Evolution, **Peter Godfrey-Smith**, Stanford U, USA

This paper will be a general discussion of the relation between inheritance mechanisms and evolution by natural selection. In particular, I will focus on the class of inheritance mechanisms that involve 'replicators', in roughly the sense of DAWKINS and HULL. Evolution does not require replicators in this sense, but mechanisms of this kind might have a special evolutionary role. Recent work by JABLONKA, LAMB and others on 'epigenetic inheritance systems' will also be discussed.

Saturday, 2-3:30 PM, LR9
SESSION IV

Perspectives on Population II

Organizer and Chair: **Michael Dietrich**

• Looking for a Few Good Males: The History of Sexual Selection, 1938-'72,
Erika Milam, U of Wisconsin, USA

The history of sexual selection is traditionally divided into three phases: the glorious days of debate between DARWIN and WALLACE, the dark decades following the modern synthesis in which it received little notice, and the blossoming of the theory in the 1970s with sociobiology. However, in the 1940s, '50s, and '60s, some ornithologists, ichthyologists, and even drosophilists were investigating the role of sexual selection and female choice in maintaining reproductive isolation between species. Their work was largely ignored by field biologists in the 1970s, and thus disappeared from the pages of history. This new history of sexual selection suggests that the rhetoric of biological unity surrounding the modern synthesis did not ultimately provide for unity or equality of practice.

• The Measurement of Biological Diversity, **John M. Drake**, U of Notre Dame, USA

The word 'biodiversity' is an essential component of conservation biology's technical lexicon and is deeply ingrained in contemporary environmental rhetoric. Though 'biodiversity' is a relatively new word (it was invented around 1984), a sustained effort to provide a general theory for the quantification of biological diversity has existed in ecology at least since the 1940s. I trace the genealogy of the modern concept, which is usually associated with the contemporary entomologist E. O. WILSON, from the seminal paper by FISHER, CORBETT, and WILLIAMS (1943) to the paper "Diversity as a concept and its measurement" by PATIL and TAILLIE (1982). In current usage, biodiversity is usually understood as an immeasurable, but inherent property of ecosystems, surrogate measures for which include species richness and other 'diversity indices'. However, I suggest that for the purpose of developing meaningful scientific theory, the current view is impoverished compared to earlier uses because it fails to provide a way of operationalizing the intuition that biological variety affects the measure and mode of natural processes. In conclusion, I contend that 'biodiversity' is not a property of systems to be measured (or for which we might find an index), but is implicitly defined with respect to particular discourses, e.g., a theoretical discourse about the measure and mode of natural processes, a statistical discourse about sampling theory, or a political discourse. These different uses of 'biodiversity' should not be confused.

• The Justification of Balance, **Christian Haak**, Dalhousie U, Canada

I first describe my work on the concept of a balance of nature in population biology. I claim that the notion of balance has changed over time. From a postulated mechanism in nature it has become operationalized into a statistical concept. How is this change to be interpreted? I discuss different approaches to this interpretation. One is that the debate about a balance of nature should be decided empirically. I doubt this and suggest that the justification for the concept has changed. Here I mean the reason the concept is used is different today than for example in the 1930s. I then

describe why justification is important to investigate, where it connects to the work done by philosophers and epistemologists, and what this means for the philosophy of biology.

Saturday, 2-3:30 PM, LR5
SESSION V

Naples as an Evolutionary Niche

Organizer and Chair: **Christiane Groeben**

• Catalyzing Science: The SZN as a 'Hotel of Science',
Christiane Groeben, Stazione Zoologica Anton Dohrn, Italy

The Stazione Zoologica di Napoli (SZN) has always been a privileged place for the circulation of scientific ideas. This purpose has been obtained in different periods through different vectors. In the first place the SZN has offered hospitality to guest investigators from different parts of the world. As a true 'hotel of science' it favoured communication and the exchange of ideas. Secondly, it developed vectors for long distance communication such as journals, monographs and animal supply, and lastly, it has promoted and organized important international meetings.

• The Role of the SZN for the Introduction and Spreading of (Classical) Genetics in Italy,
Alessandro Volpone, Stazione Zoologica Anton Dohrn, Italy

During the first decades of the 20th century the SZN has played—together with the Institute of Zoology at Bologna—a fundamental role in the introduction and diffusion of genetics in Italy. It came essentially from the Anglo-American context and was accepted as a 'new' (or 'special') branch of zoology, comparative anatomy, as well as botany.

• The SZN as a Special Place for Researchers,
Ariane Dröscher, Stazione Zoologica Anton Dohrn, Italy

One of the characteristics that distinguished the SZN from other research institutions was its liberalism towards scientific trends and approaches. It never represented a unique 'school', nor was any researcher rejected because his project did not fit in with the conceptions of the director. Proposing the case study of colloid chemistry between 1910 and 1940 the pluralism of styles and goals, and the interaction between researchers will be presented.

• The Neapolitan Biological Research Center,
Fabio De Sio, Stazione Zoologica Anton Dohrn, Italy

Between the end of WWII and the 1960s the city of Naples became the centre of an intense program of development of research in biology, due to the presence of the SZN and the politics in favour of the southern regions. Gustavo COLONNETTI and Giovanni POLVANI, presidents of the Italian National Research Council (Consiglio nazionale delle ricerche—CNR) during the 1950s and '60s gave to Naples the LIGB LIGB (Laboratorio Internazionale di Genetica e Biofisica/International Genetics and Biophysics Laboratory) in 1962 and the LEM (Laboratorio di Embriologia Molecolare/Molecular Embryology Laboratory) in 1970. The original project aimed at creating a network of biological research among the SZN, the CNR research centres in the Region of Campania and the University of Naples. This project or the first implemented fragments gave proof of great vitality and intense productivity. However, the project was discontinued for reasons still to be investigated.

Saturday, 2-3:30 PM, LR1
SESSION VI

Beyond Lorenz

Organizer and Chair: **Richard W. Burkhardt, Jr.**

• "The Father of Ethology and the Foster-Mother of Ducks": Ethology and Parental Behavior, 1950-'70, **Marga Vicedo**, Harvard U, USA

In this paper, I examine Konrad LORENZ's views on parental behavior and their reception among biologists and psychologists, especially in the USA In the 1950s and '60s, LORENZ's and TINBERGEN's ideas became a frame of reference for studies of infant development and parental care, both in human and nonhuman animals. LORENZ's views about imprinting had a profound influence on psychoanalyst Rene SPITZ and on child psychologists John BOWLBY and Mary AINSWORTH, whose theory of attachment was based on what they called 'an ethological approach to personality development'. Harry HARLOW's experiments with rhesus monkeys were also influenced by LORENZ's views, although HARLOW did not always agree with them. Finally, some biologists working on parental behavior, such as Daniel LEHRMAN, disagreed with LORENZ's methodology and ideas. I analyze the influence of LORENZ's views and explore the factors that led several scientists to different interpretations of parental behavior. I examine those scientists' views about the value of description versus experimentation, as well as their different ideas on instinctual behavior. To understand them, I look at the interplay of their diverse personal beliefs, institutional settings, disciplinary traditions, and cultural values.

• Ethograms and Epideictic Behavior, **Mark Borello**, Michigan State U, USA

In this paper I will explore the relationship between evolutionary theory and ethology. I am especially interested in the idea of the ethologists, particularly Konrad LORENZ, that animal behavior and human behavior were equally appropriate subjects of biological analysis. I introduce this connection to contrast WYNNE-EDWARDS' explicit evolutionary approach (ultimate explanation) with the more physiological (proximate) approach of Lorenzian ethologists. LORENZ's work emphasized the way in which special structures and behavior patterns had evolved in the service of intraspecific communication. Given the focus of WYNNE-EDWARDS' theory on exactly this phenomenon, one might expect their work to overlap more obviously. WYNNE-EDWARDS', however, was interested in these behaviors purely in an evolutionary context, particularly the variation of responses to signals within a population. LORENZ, in contrasting his position with American behaviorist psychologists, stressed the invariabili!ty of instinctive behavior and failed to attend to the phenomenon of intraspecific variation. WYNNE-EDWARDS also was not interested in the behaviorists' problem of behavioral conditioning, but in the evolutionary importance of the ability of the group to adjust behavior to given environmental conditions (and he found individual differences in behavior to be an important part of this process).

• Does Animal Learning Belong to the History of Ethology? A Case Study from France, **Marion Thomas**, U of Manchester, UK

Historians of animal behaviour studies have focussed either upon ethology (the study of innate behaviour in the field) or animal psychology (the study of learned behaviour under restricted conditions in the laboratory). In doing so they seem to have taken for granted the polarization of the field of animal behaviour studies that arose when Konrad LORENZ championed his ethology in opposition to J. B. WATSON's Behaviourism. The case of the French zoologist Louis BOUTAN (1859-1934), however, illustrates an alternative way of looking at animal behaviour, which makes it clear that Lorenzian or Watsonian approaches were not the only approaches to animal behaviour in the first half of the 20th century. I argue, therefore, that the history of animal behaviour studies ought to be more broadly demarcated in order to embrace all of these approaches. In addition to discussing such disciplinary issues, I will also show how socio-cultural presuppositions informed

BOUTAN's choice of task, and thus indirectly the behaviour of the apes he studied. Namely, the Cartesian legacy, evolutionary debate and the requisites of the French Third Republic all shaped BOUTAN's comparative studies of apes' and children's intelligence. Although BOUTAN infantilized his gibbons in order to explore the evolutionary stage where children acquire language and then diverge from animality, he nevertheless also provided evidence that apes may be intelligent.

Saturday, 2-3:30 PM, LR2
SESSION VII

Sex, Gender, and Immunology

Organizer and Chair: **Moira Howes**

• Gender and Autoimmunity, **Bernice Noble**, The State U of New York at Buffalo, USA

The possibility of gender differences and/or sexual dimorphism in the expression of immune responses has been almost entirely ignored until now. Immunity is not normally expressed against molecules produced by healthy individuals themselves, a phenomenon called 'self-tolerance'. Self-tolerance may break down allowing producing diseases described as 'autoimmune', which include hyperthyroidism, hypothyroidism, rheumatoid arthritis, systemic lupus erythematosus, multiple sclerosis and insulin-sensitive diabetes. As a conservative estimate, in the US, 8,500,000 individuals are affected by autoimmunity. Of those, nearly 80% are women. Most autoimmune diseases lead to considerable lifestyle impairment, including significant morbidity, chronicity and, in many cases, premature death. Although the social and economic impact of autoimmunity is enormous, the medical science establishment has, until recently, expressed little interest in those diseases. Biological factors unique to women appear to have overwhelming importance, however, except for the sex steroid hormones, none has been convincingly identified. This paper will review critically the history of immunological concepts of self-recognition, the biological, medical and social implications of failure to study gender differences in immunity, and the present status of research and clinical practice in autoimmunity.

• Modeling the Maternal-Fetal Immunological Relationship, **Moira Howes**, Trent U, Canada

Two questions guide my present inquiry: First, why are models of the maternal-fetal immunological relationship so rare in immunology? And second, what explains the stability of the rudimentary assumption that this relationship is antagonistic? A standard feminist approach to the maternal-fetal relationship in other contexts—such as the bioethical or social—is to inquire into the theoretical or ontological assumptions that structure commonplace views of this relationship. One merit of such an approach in the present immunological context is that it could be used to critically examine the theoretical justifications some immunologists give for maternal-fetal antagonism. However, while investigating the theoretical dimensions of this issue is certainly important, developing a full answer to the above two questions requires an evaluation of experimental practice and its relation to model development in immunology. Closer attendance to experimental practice not only strengthens critical evaluation of the rare and rudimentary maternal-fetal immunological model; it suggests avenues for a more deeply critical immunological inquiry in general.

• Artificial Insemination and Sperm Banks in USA, **Alicia Villela**, UNAM, Mexico

The developing of new reproductive technologies in the last four decades such as artificial insemination, semen preservation, embryos transfer and in vitro fertilization have generated new ways of thinking in our cultures. Reproductive technologies are influential and striking because they are not only concerned with biological events, but additional involve a series of different thoughts and interrelated behavior among a wider public. In trying to present some of the public-scientific discussions and attitutes generated with the development of the sperm banks in the USA from 1945 until now. This paper seeks to show some of the possible opinions and changing people and scien-

tists attitudes toward the creation of sperm banks as a consequence of the artificial insemination assay within eugenics ideas.

• Is The 'Self/Nonself' Model of the Immune System at Odds with Feminist Epistemic Virtues in the Philosophy of Science? **Charissa Varma**, KLI, Austria

In her paper "Dismantling the Self/Other Dichotomy in Science: Towards a Feminist Model of the Immune System" WEASEL argues that MATZINGER's 'danger' model, while not designed with feminism in mind, is consistent with different feminist approaches to epistemology and philosophy of science, and thus provides biology with a feminist model of the immune system. A crucial move in WEASEL's argument involves contrasting MATZINGER's model with the traditional self/nonself (SNS) model, arguing that the SNS model of the immune system cannot be understood as a feminist model of the immune system. In this talk I argue this interpretation of the SNS model might be a too hasty and too superficial an analysis. Drawing from the work of early immunologists METCHNIFKOFF, BURNET, and MEDAWAR, TAUBER and CRIST argue in their paper "Selfhood, Immunity, and the Biological Imagination: The Thought of Frank Macfarland Burnet" that the SNS as developed by these biologists is not only consistent with, but has biological holism (or sometimes called 'organicism') in mind. In my talk I will argue that biological holism is not at odds with feminist epistemic virtues in the philosophy of science, and if the original version of the SNS model shares these virtues, then WEASEL's argument is importantly flawed, as she is not sensitive to the important historical context in which the rhetoric and metaphors of the SNS model are situated. This argument is not designed to dissolve the debate, deprecate the theoretical and practical value of MATZINGER's model, show that MATZINGER's model is inconsistent with feminist epistemic virtues, or even to say that the self/other dichotomy (generally speaking) is not at odds with feminist values. Instead, I want to suggest an alternative way to approach this debate. I suggest that in some cases the self/other dichotomy is not at odds with feminist virtues, and this is such a case. To do this, I argue, will involve first looking at the way these early immunologist characterised this distinction (with respect to the immune system) and the subsequent role this model played within the history of biological thought. Then, with this context in mind, one is better equipped to evaluate it within a feminist context. In addition, I suggest this approach provides an interpretation of the debate between these two models of the immune system that is more in keeping with the spirit of MATZINGER's own criticisms and innovations.

Saturday, 2-3:30 PM, LR3
SESSION VIII

Emergence and Mechanisms in Biochemical Networks

Organizer and Chair: **Robert C. Richardson**, U of Cincinnati, USA

• Broad Emergence, **Achim Stephan**, U of Osnabrück, Germany

Widely accepted is a distinction between *strong* and *weak* emergence. The stronger version of emergentism is based in part on a weak theory, which at the present pervades emergentist theorizing in various scientific approaches, *e.g.*, connectionism, artificial life, and theories of self-organization. Its three basic features the thesis of *physical monism*, the thesis of *organizational* (or collective) *properties*, and the thesis of *synchronic determinism* are compatible with reductionist approaches without further ado. The essential features of more ambitious theories of emergence are the theses of *irreducibility* and of *unpredictability*, respectively. Both are closely connected: irreducible organizational properties are eo ipso unpredictable before their first appearance. We here concentrate, however, on the notion of irreducibility. In particular, two different types of irreducibility—horizontal and vertical—should be distinguished. Both were implicitly discussed by BROAD in the 1920s. Vertical reduction is given if both the property to be reduced can be functionalized and if it can be shown that the specified functional role is filled by the system's parts and their mutual interactions. Failure of one of the two conditions brings vertical irreducibility. It is

this notion of emergence that many philosophers of mind are interested in. In contrast, it is mainly the horizontal type of emergence that is of interest to natural scientists. Horizontal reduction affords that the behavior of the system's parts can be shown to follow from the behavior they show 'in isolation' or 'in other systems' than the system in question. This leaves room for a variety of 'reduction bases', giving us some 'measure' of emergence.

• Emergence in Biochemical Networks: The Role Played by 'Systemic' Properties of Components in Complex Systems, **Frank J. Bruggeman**, Vrije Universiteit, Amsterdam, The Netherlands, **Achim Stephan**, Vrije Universiteit, Amsterdam, The Netherlands, **Robert Richardson**, U of Cincinnati, USA, **Hans Westerhoff**, University of Osnabrück, Germany, and **Fred C Boogerd**

In molecular cell physiology, cells are studied as complex systems of nonlinearly interacting (macro)molecules. All behavior found at the system level (cellular level) is explained in terms of the properties and the interactions of the (macro)molecules. We discern two types of properties of (macro)molecules as parts of a complex biosystem; (i) the properties that do not depend on the systemic context and that they display in isolation (*intrinsic* and *relational* properties) and (ii) the properties that depend on the systemic context (*component* properties). A relational property is a property determined by two parts. It characterizes through a set of constants the mechanism with which two parts interact. A component property of a particular part is determined by its relational properties and, additionally, the systemic context. To reconstruct a system from the relational properties of its parts one also needs composition relationships. Systems have two kinds of properties; state-independent and state-dependent properties. The state-independent systemic properties can be determined from a analysis of the system after it has been reconstructed. In order to determine the state-dependent properties, additionally, initial and boundary conditions have to be taken into account. Some of the system properties are not displayed by the parts (or any of its subsystems in isolation). We term these properties emergent properties even though they are reducible to the component properties of the parts. All of the concepts introduced above shall be illustrated with examples from kinetic models of biochemical networks. It will be shown that their emergent physiological properties can be deduced from component properties of their parts. Additionally, nonlinearity and mutual interactions among parts will be illustrated to be necessary for a particular kind of emergent properties that has to do with the stability of biochemical networks.

• Towards a Mechanistic Explanation of the 'Live' State Through a System Biological Approach, **Fred C. Boogerd**, **Frank J. Bruggeman**, Vrije Universiteit, Amsterdam, The Netherlands, **Hans Westerhoff**, University of Osnabrück, Germany

The biological sciences study (bio)complex living systems. Research directed at the mechanistic explanation of the 'live' state truly requires a pluralist research program; that is, Biocomplexity research. Combining reductionist and antireductionist strategies, the program should apply multiple intralevel and interlevel theories and methodologies. Here, attention will be paid to the interlevel approach in particular. Three examples of kinetic modelling serve to illustrate the power of linking global (physiological) phenomena to the underlying local (biochemical) processes. First, metabolic control analysis embodies a rigorous mathematical framework for studies of control and regulation of biochemical systems. It links two adjacent levels in the biological organizational hierarchy; that is, the level of metabolic pathways to the level of enzyme-catalysed reactions. Hereby, global (systemic) behavior is explained in terms of local (constituent) properties. Second, in silico experimentation with a precise kinetic replica of glycolysis reveals the physiological functions of a tight biochemical regulation of the pathway in a cellular context. Third, oscillation in a metabolic network consisting of two non-oscillating subsystems in vitro serves as an example of an emergent property. It arises at the systemic level, but is mechanistically coupled to the properties of the subsystems. In summary, contemporary cell biology is growing from a qualitative to a quantitative science that progresses through precise and pluralist experimental approaches.

• Commentary, **Marcel Weber**, Universität Hannover, Germany

Saturday, 2-3:30 PM, LR10
SESSION IX

Biology and Metaphysics II

Contributed Papers, Chair: **Andrew Hamilton**

• Colonial Medicine and Semiotics, **Jayanta Bhattacharya**, Independent Scholar, India

The human body has been of primordial interest as a space of imagination, representation and investigation across millennia. But a prosaic toward sameness was promoted by the mass-production, standardization and colonial expansion making scientific explanation the authoritative cultural narrative of modernity. That all disease is semantic, that disease is a symbol of a sign which reveals a particular meaning becomes a point of oblivion in the specific Indian context through colonial encounters at the level of the epistemological space of the body. Pre-colonial polysemous nature-being interaction signified through metaphoric floating signifiers has been reconstituted metonymically by the sign-system of colonial medicine. Patient as embodied subject has become the object of biomedicine permanently metamorphosing the physician-patient-community intersubjective network. It is another kind of semiosis through which we can explore into different epistemologies, corporeities and selves in a post-colonial semiotic setting through medical discourses around the colonized, also socialized, body.

• Pure Forms in Biology, **N. Milkov**, Bielefeld U, Germany

The task of the paper is to develop (following some ideas of WITTGENSTEIN) a programme for investigating the pure (logical) forms of biological objects (species). These 'natural forms' (as different from the 'natural kinds') can be seen as shapes; biological metaphysics studies their transformation. This is a type of morphology indeed—not a morphology of GOETHE's type, however. The morphology of pure forms in biology tries to find the forms of the species with the only aim to analyzing them. In contrast GOETHE's morphology is not analytical but complementary.

• Biology: Unity and Disunity, **Andrew Hamilton**, U of California, San Diego, USA

One important result of recent thinking about biology is the disunity thesis—the claim that biological science is importantly different from the nonbiological sciences for metaphysical, epistemological, or sociological reasons. In addition to investigating what the disunity thesis comes to and the arguments surrounding it, this session will also investigate its larger implications, including perhaps concerns about reductionism, natural laws and contingency, and the relative epistemic standing of biological sciences.

Saturday, 4-5:30 PM, LH

Evening Plenary
Stephen J. Gould's *The Structure of Evolutionary Theory*
Organizer: **Vassiliki Betty Smocovitis**

• Introduction to the Session: **Lindley Darden**, U of Maryland, USA

• Introductory Remarks and Tribute to Gould, **Vassiliki Betty Smocovitis**, U of Florida, USA

• Panel: **Jean Gayon**, Université de Bourgogne, France; **Elisabeth Lloyd**, Indiana U, USA; **Robert J. Richards**, U of Chicago, USA; **Michael Ruse**, Floridate State U, USA; **Kim Sterelny**, Australian National U, Australia
Moderator, **Richard M. Burian**, Virginia Tech U, USA

Sunday, July 20

9-10:30 AM

SESSION I, LR1

Darwin

Contributed Papers, Chair: **Juan Carlos Zamora**

• Functionism in Darwin's Theory of Descent with Modification,
Juan Carlos Zamora, UNAM, Mexico

At the beginning of the 19th century CUVIER and many other naturalists conceived that the Creator designed the different organisms perfectly adapted to their environment. They affirmed that the Conditions of Existence of living beings determine the functions of all of their parts and organs and, consequently, the form of their adaptations. This conception, based on how function establishes form, has been named *functionalism*. Strict functionalism considered organisms so well adjusted to their environment that modifications in their structure would cause them to die. Thus, it opposed evolution. DARWIN recognized that his theory of descent with modification was not compatible with this type of functionalism. Even so, he concluded Chapter Six, "Difficulties on Theory," of *The Origin of Species* stating that: "It is generally acknowledged that all organic beings have been formed on two great laws—Unity of Type and the Conditions of Existence… The expression of conditions of existence, so often insisted on by the illustrious Cuvier, is fully embraced by the principle of natural selection… in fact, the law of the Conditions of Existence is the higher law; as it includes, through the inheritance of former adaptations, that of Unity of Type." DARWIN affirmed that when the environment changes, the new Conditions of Existence of an organism determine the new functions that certain parts or organs have to satisfy so the organism can survive. These new functional constraints establish how natural selection operates to give rise to new adaptations. Through natural selection function determines form. Thus, even though DARWIN opposed a strict functionalism, a different type of it is present in his theory, one that considers that living beings have some parts and organs that are not adaptations to its present environment but to past conditions of existence, a functionalism limited by historical considerations.

• Darwin and Inheritance, **Ricardo Noguera** and **Rosaura Ruiz**, UNAM, Mexico

One of the most important problems in biology is inheritance. Nevertheless, only in this last century has it been possible to consolidate it as an independent research program. Many phenomena connected with heredity were contained in the generation (reproduction) program that survived until the second half of the 19th century. Before this century the hereditary problem was tied to studies on explanations of the monstrosities and abnormalities generated by environmental influences, meaning it relates to acquired hereditary characters. The transformations from the hereditary (as an adjective) to inheritance (as a noun) was a great conceptual change. In this paper we try to understand in which of the conceptual programs DARWIN was working. In our point of view DARWIN's ideas go from one conceptual field to another. In the first stage, DARWIN's ideas on heredity is connected with the generation program, in the second stage, heredity is an autonomous program. Our historical analysis is based on DARWIN's work from his notes written during the *Beagle* journey (1831-1836) until the last edition of *The Effects of Cross Fertilization of the Vegetable Kingdom.*

• The Vestiges of Creation: The Phrenological Connection to Evolutionary Theory,
Sherrie Lyons, Empire State College, USA

This paper argues that Charles DARWIN owed an enormous debt to the phrenology-based argument presented in Robert CHAMBERS' *Vestiges of Creation* (1844), both as a direct influence on his *De-*

scent of Man and in paving the way for the public acceptance of *Descent*'s argument. The importance of *Vestiges* in acclimatizing Victorians to the idea of evolution is well documented, and it was specifically phrenological concepts, borrowed from Franz GALL, that allowed CHAMBERS to include humans in his grand evolutionary epic. DARWIN would use concepts strikingly similar to CHAMBERS' in chapter 3 of *Descent* to argue that humans were not exceptions to his own evolutionary theory. It is true that we have no clear documentary evidence that DARWIN drew signifycant support from phrenology, and many of the examples given in this paper can be found in the natural theology literature that DARWIN did acknowledge in his private notebooks. Yet while DARWIN may have borrowed certain ideas from the natural theologians, his account of animal behavior ultimately had far more in common with GALL's naturalistic theory, even if he refused to acknowledge it. This is not surprising: DARWIN would have wanted to distance himself from phrenology, which had lost virtually all of its reputable support by the time he wrote *Descent*.

• Progress and Degeneration: Romanes, Lankester, Spencer, Wallace,
Daniel Becquemont, University Lille III, France

After DARWIN's death, two of his successors, G.J. ROMANES and Edwin Ray LANKESTER—joined by the co-founder of the theory of natural selection, A. R. WALLACE—questioned and rejected the idea of an inheritance of acquired characters, inspired by WEISMANN's theories. They all claimed to be the rightful inheritors of DARWIN. ROMANES coined the word 'neo-Darwinians', WALLACE spoke of 'pure Darwinism'. Herbert SPENCER severely criticized the rejection of the inheritance of characters. Though the three men agreed on that rejection, and were all interested in extending the action of natural selection not only to evolutionary progress, but to cases of degeneration, though more or less agreed on the Weismannian concept of 'panmixia', they were at odds on many important issues. WALLACE claimed to develop DARWIN's theories on a strict utilitarian basis. ROMANES, who claimed to have been the first biologist to question the inheritance of acquired characters, considered WALLACE's views as rigidly utilitarian leading to a theory of 'natural causation' rather than 'natural selection'. LANKESTER critized ROMANES's views on the 'cessation of selection'. The three men claimed to be the true inheritors of the pure Darwinian theory in the 1880s and '90s.

Sunday, 9-10:30 AM, LR2
SESSION II

Biohistory—Neo-Darwinism's Last Frontier

Organizer and Chair: **Robert S. McElvaine**

• Is the Sexual Arena A Battlefield or a Marketplace? **Marion Blute**, U of Toronto, Canada

In one sense the problems of genetic recombination (sex), of the origin of gender differences (anisogamy), of the nature of secondary sexual characteristics (sexual selection), and of sex allocation (e.g., the sex ratio) are all distinct problems. In another sense, they reduce to the same problem—the cost to females of male parasitism. In populations in which males make no contribution to parental care (which is commonly the case in mammals), it is unclear why females do not evolve to be parthenogenetic (sex), why macrogametes did not evolve to fuse preferentially with other macrogametes (anisogamy), why females do not always choose for resources (sexual selection), and why such populations do not evolve to become composed exclusively of females or to include males who do contribute equally parentally. This deep problem undermining our understanding of the origin and nature of gender differences can only be resolved by positing either historical obstacles or by adopting a view of the sexual arena as a marketplace rather than a battlefield.

• The Evolution and Ecological Variations of Sex Differences,
Bobbi S. Low, U of Michigan, USA

This paper will focus on my work on the application of evolutionary and behavioral ecological theory to questions concerning human history and particularly demographic questions. In the past, evolutionary approaches have been used with success in examining the demography of traditional societies. This paper will discuss population-environment interactions as they illuminate historical phenomena in advanced 'modern' societies. Such investigations offer one of the more promising avenues in which a biohistorical perspective can illuminate our understanding of human history.

• Sex as the Basis of Biohistory, **Robert S. McElvaine**, Millsaps College, USA

In my *Eve's Seed: Biology, the Sexes, and the Course of History* I introduced a new historical approach I call 'biohistory'. It is an attempt to inform historical inquiry by taking serious account of the fact that the creatures we study as historians are in fact biological species with certain traits. Biohistory takes a radically different approach from that of social Darwinism. Social Darwinism focused on the putative differences among groups of humans; biohistory seeks to utilize a better understanding of human nature or the human biogram—the fundamental traits and predispositions that all humans share and that make us alike—to illuminate aspects of our history. This paper will focus on the biological condition that has had the greatest effect on human history: the fundamental fact that our species reproduces sexually. Because women can do certain things that men cannot—carry, give birth to, and nourish offspring—men have always, to varying degrees, experienced feelings of inferiority that might be called, reversing FREUD, womb envy and breast envy. As a result, men in cultures around the world have developed definitions of manhood based on the false notion that men are the opposite of women. The particulars vary from culture to culture, but a 'real man' is always seen as the negative of a woman: 'notawoman'. Men have avowed not only their polar opposition to women, but also their superiority in the false sexual dichotomy they have set up. This concept of male over female has, in turn, been used as the model on which all other forms of inequality, domination/subordination, and hierarchy have been constructed. Men as individuals or groups who assert their superiority over others always claim that they are to the alleged inferiors as man is to woman. The paper will explore how these sexual differences played out during the 'prehistorical' period following the development of agriculture and so set the stage for much of recorded history.

Sunday, 9-10:30 AM, LR5
SESSION III

Alternative Evolutionary Theories I

Organizers: **George Levit** and **Uwe Hossfeld**, Chair: **George Levit**

• Some Remarks on Lysenkoism at Jena University,
Uwe Hossfeld and **Lennart Olsson**, Jena U, Germany

After the end of the WWII, German scientists hoped for a strict subdivision between science and ideology. They hoped for a new beginning without misanthropic political doctrines, but this hope was thwarted in East Germany. There it soon became clear that the Communists wanted to decide in which direction scientific research should go, just like the National Socialists who had ruled before them. This was true especially of biology and philosophy. In the 1950s and '60s, the attitude to the mode of thought encapsulated by Lysenkoism and to 'the socialist achievements of the Soviet Union' was used as a measure of a scientist's political stance. In this period, some scientists from Jena University (Georg SCHNEIDER, Georg USCHMANN, Hans WARTENBERG, Jürgen W. HARMS, and Manfred GERSCH) played important roles pro and contra in the debate about the non-Darwinian theories of LYSENKO and his circle (e.g., PREZENT, LEPESINSKAJA). In our talk, we will

give an overview of this debate, present the leading protagonists, and discuss the influence (or lack thereof) of LYSENKO's ideas on the development of the biological sciences in the GDR.

• Alternatives to Darwinism in Sweden, **Lennart Olsson**, Jena U, Germany

Both Darwinism and the later so-called Modern Synthesis were met with resistance in Sweden. Older traditions like idealistic morphology remained influential, for example in the work of the 'Stockholm School' of palaeozoology. This was founded at the Natural History Museum in Stockholm in the first third of the 20th century and persists today in that Hans C. BJERRING, although retired, is still active. The Stockholm researchers became famous for their detailed reconstructions of the internal anatomy of fossil fishes and for pioneering work on early tetrapods like *Ichthyostega*. Partly the success was based on using a basically ahistorical approach, which made it possible to compare the fossil material with the anatomy of extant vertebrates. Researchers like Erik STENSIÖ (1901-1984), Erik JARVIK (1907-1998), and their students often worked on classical problems in idealistic morpology like the 'head problem' (head segmentation, evolution of head structures, homologization) and the evolution of limbs. Lamarckian approaches were influential, especially before Mendelian genetics became influential. The Uppsala physiologist Frithiof HOLMGREN (1831-1897) famously tried to transform doves into flesh be feeding them flesh. The transformational part (that species change into new species) of Darwinism was strongly opposed by Nils HERIBERT-NILSSON (1883-1952), a botanist at Lund University who believed in the constancy of species despite the existance of variation. The most widely criticized part of Darwinism was perhaps the theory of selection. Many researchers did not believe that natural selection could have the power to be important for evolutionary change. An outspoken critic of natural selection was the zoologist Douglas MELIN (1895-1946), active at Uppsala University, who thought that direct environmental influences and instinct were important factors.

• The Concepts of Directed Evolution, **Igor Popov**, Russian Academy of Sciences, Russia

The basic idea of Darwinism is that evolution is based on natural selection from very extensive or even inexhaustible material of variability. The opposite view suggests that the material of variability is limited. According to this view living organisms are predisposed to vary only in the certain direction, and this very predisposition determines major transitions in evolution. The idea of directed evolution is a core of about 20 evolutionary concepts (heterogenesis, batmism, orthogenesis of W. HAAKE, orthogenesis of Th. EIMER, ologenesis, nomogenesis, historical biogenetics, organicism, typostrophism, Frankfurt school of engineering morphology, etc.). Most of them claim the saltatory character of evolution. Some concepts combined the idea of directed evolution with Lamarckian ideas like the theory of exercises or the concept of directed influence of environment. Some concepts contain specific ideas emphasizing either the polyphyletic or the regressive character of evolution. The common features of the various orthogenetic concepts allow to consider them as unite methodological platform in evolutionary biology. They are variations of the idea of directed evolution. However, in most cases authors introduced their concepts as absolutely new ideas and coined exotic terminology that makes the overview and comparative research especially difficult.

Sunday, 9-10:30 AM, LR6
SESSION IV

Autonomy: A Key Concept I

Organizers: **Alvaro Moreno** and **Kepa Ruiz**, Chair: **Alvaro Moreno**

• Biological and Cognitive Autonomy as an Interplay Between Population and Individuals,
Bernd Müller and **Andreas E. Kilian**, Fraunhofer Institute AIS, Germany

Biological and cognitive autonomy cannot be separated from each other, they build on each other and are tightly connected. Cognitive capacities are part of the autonomy subsistence tools of living beings. Biological autonomy starts when the 'selfish' gene forms a containment, a body, for itself in order to survive. Stated more correctly, the rules for the construction of the body survive which allow to build a body which survive in a balance between rules and environment. Complete autonomy cannot be ascribed to lower organisms that are selected by the environment. As individuals they are subjected to environmental occurrences in a more passive way. In evolution, increasingly more efficient adaptations lead to higher forms of autonomy which give more flexibility to organisms for actions under varying environmental conditions. Such adaptations take on the form of genetic mutations in lower organisms. Changes, actions, and reactions in the environment are a matter of generations. The autonomy of lower organisms is thus limited by the reproduction of cellular lines and populations. The evolution of the ability to learn may be considered as a leap in the evolution of autonomy. With imprinting, as a form of learning, the evolutionary interplay between stochasticity and selection is again applied in epigenesis. Provision is made for a surplus of synapses in preparation of imprinting. Following an environmental stimulus, only few neuronal connections are selected during a predefined temporal slot. These selected connections remain after imprinting, whereas the surplus of synapses that are no longer necessary is retracted. This interplay between 'producing a surplus' and 'selecting according to environmental stimuli' in and individual is a kind of evolution at increased speed. In this way, the biological bodily adaptation via genetic proliferation is circumvented in the first place. Cognitive processes allow an adaptation of an individual during its lifetime. The fitness of the individual to resist selective processes may also be increased in such a way. The autonomy of such an organism increases through this ability to act in and adapt to its individual environment. The individual is able to adapt in a way independent from sexual reproduction, but the results of learning cannot be inherited to the next generation via its genes. Such an organism can draw both on a form of biological autonomy of the population and on an individualized autonomy during its lifetime. The next step in the evolution of autonomy lies in the selection of systems, which are able combine the population autonomy of sexual reproduction with the individual autonomy of cognitive processes. These are family groups, which are able to transfer the results of learning to the next generations. This not a matter of biological genetics but of cognitive genetics in the form of culture. Whatever drives the 'selfish' gene towards increasing the autonomy, the outcome is that the evolution of autonomy is separated from its original carrier, the gene, and that different genetic lines can be reunited via culture.

• Autonomy in Dynamical Adaptive Systems: Towards a Naturalized and
Biologically Inspired Definition of Cognition and Adaptive Behavior,
Xabier Barandiaran, U of the Basque Country, Spain

Autonomy is defined as a property of adaptive systems characterised by: 1) satisfaction of self-maintenance, 2) operational closure, 3) plasticity and 4) self-directedness. More specifically, in dynamical terms, autonomy can be conceptualized as homeostatic maintenance of essential variables under viability constraints (ASHBY) by means of behavioural interactions with the environment. The autonomy of the nervous system, thus described and dynamically decoupled from metabolic processes, becomes the key notion to define normative functionality and the specificity of behavioural adaptation and cognition. We illustrate our approach with recent work on homeostatic plasticity in evolutionary robotics and computational neuroethology, where dynamic recurrent neural networks with homeostatic synaptic plasticity are artificially evolved. This artificial life technique

allows for a dynamical, bottom-up and biologically inspired grounding and naturalization of cognition and adaptive behaviour.

• Autonomy and Functional Change, **Wayne Christensen**, KLI, Austria

I briefly introduce a theory of autonomous systems as systems that interactively generate the conditions required for their existence. Such systems are 'self-governed' in the sense that they shape the conditions involved in maintaining their integrity. This theory describes a very general property of living and some non-living systems, but it has many important and detailed applications. One of the most fundamental is that it provides a dynamic rather than structural definition of system identity, and this is essential for characterizing systems that undergo extensive functional reorganization and shifting interdependencies with the environment. Functional reorganization is an extremely important biological phenomenon, associated, for example, with evolutionary innovations, developmental, and learning processes. I will discuss some of the advantages that the dynamic autonomy-based conception of system identity has for conceptualizing open-ended learning processes in cognition, which cannot be adequately modeled using standard assumptions of a fixed functional architecture and problem space. Open-ended learners can reconfigure themselves and the problems they deal with.

Sunday, 9-10:30 AM, LR7
SESSION V

'Race' in Theories of Human Origins and Diversity, from the 17th Century to the Present

Organizer and Chair: **Marianne Sommer**

• "Ancient Hunters and Their Modern Representatives": Evolutionary Racism, Imperialism, and War, **Marianne Sommer**, Pennsylvania State U, USA

Drawing upon the tradition of his instructor of paleontology at the Muséum National d'Histoire Naturelle, Albert Jean GAUDRY, Marcellin BOULE formulated his influential theory of human evolution as a branching tree similar to those drawn for mammalian evolution. This meant that the hominid fossils so far discovered, such as *Neanderthal* and *Pithecanthropus*, represented dead-end side branches rather than stages in the evolution of modern *Homo sapiens*. BOULE's work led to a general change in opinion, also in the case of William SOLLAS, professor of geology and paleontology at Oxford University, who had formerly envisioned *Neanderthal* and *Pithecanthropus* as 'the piers of a ruined bridge which once continuously connected the kingdom of man with the rest of the animal world'. In his *Ancient Hunters* (1911, 1915, 1924), SOLLAS presented a race succession paradigm according to which natural selection, "the stern but beneficent tyrant of the organic world," had led to the successive marginalization or extinction of 'inferior races' of man. Though not everybody agreed with SOLLAS' identification of fossil human 'races' with modern 'races' of man, the white supremacist view that the line of modern Europeans went far back in time and did not include the fossil or modern 'races' was widespread. The paper will address this evolutionary racism and its mutual feedback with eugenicist and imperialist ideology as well as the uncertainty generated by a war between 'the master nations'. Besides discussing the theoretical aspects, it will tackle the everyday problems or opportunities that arose for the work of human paleontologists through imperialism and war.

• Black Skin and the Origin of Physical Anthropology, 1640-1850,
Renato G. Mazzolini, U di Trento, Italy

During the 17th and 18th centuries the question 'Why are Africans black?' was posed with increasing frequency. Papers and books were devoted solely to this question by a considerable number of

European scholars: in the period 1640-1849, for instance, at least 111 independent and specialized contributions appeared, most of which made direct reference to it in the title. The question, however, was raised and discussed—at times in entire chapters, on other occasions only in a few pages—in a far higher number of works such as textbooks of anatomy and physiology, contributions to the natural history of man, books by historians, theologians, travelers, and philosophers and during the second half of the 18th and early 19th century by members and opponents of the anti-slavery movement. Though the above question may appear limited, it had, in fact, considerable impact: religious, moral, historical, political, legal and scientific. At the religious level, for instance, it led inevitably to a discussion on whether Adam had been white or black; at the historical level, when and how physical differentiation came about; and at the legal level, by which principle a physical feature could determine a man's future social condition. Nine theories concerning the cause of black pigmentation were advanced and discussed: (1) Noah's curse of Canan, (2) climate, (3) blood, (4) [missing], (5) the imagination of mothers, (6) chemical substances, (7) nervous fluid, (8) 'genetic' particles, and (9) a past pathological condition. In the paper I will emphasize how the debate on pigmentation may be considered the historical cradle in which physical anthropology had its origin and formulated some of its methods of inquiry, intellectual commitments, and general problematic.

• The Odd History of Human Paleontology, **Jeffrey H. Schwartz**, U of Pittsburgh, USA

Although the late 17th century saw the recognition of fossils as the remains of extinct organisms—because they could be incorporated into the creation story embodied in the Great Chain of Being—acceptance of human antiquity through the indisputable demonstration for the contemporaneity of human bones, stone tools, and accepted fossils was not forthcoming for nearly two centuries thereafter. When it did, however, ancient humans were not generally seen as presenting a pattern of diversity as seen in the fossil records of nonhuman organisms. The origin of unilinear/anagenetic thinking about the course of human evolution lies, I believe, in T. HUXLEY's *Man's Place in Nature* (1863). There, HUXLEY argued that the Feldhofer Grotto Neanderthal skull cap was merely an extension into the past of morphology seen in the Australian aborigine, whom he took to represent the primitive end of an extreme range of variation he thought characterized *Homo sapiens*. During the mid-20th century, MAYR and DOBZHANSKY (mis)used their prominence in evolutionary biology to argue without basis that, for most of its history, human evolution was unilinear. Taxic diversity was diminished to the status of individual variation through, and at any point in, time. Lack of understanding this history, and the biases underlying it, continues to affect the ways in which most paleoanthropologists pigeonhole human fossils.

• Commentary, **Claudine Cohen**, École des Hautes Études en Sciences Sociales, France

Sunday, 9-10:30 AM, LR3
SESSION VI

Science and Policy: International Perspectives I

Contributed Papers, Chair: **Christine Keiner**

• Brazilian Biotech and IPR, **Marilia Coutinho**, U of São Paulo, Brazil

Considering the national innovation system, Brazil is among the immature countries for many different reasons. Scientific research and inventive activity are heavily concentrated in academic environments, while the private sector has very little access to them. Even if well-meaning legal actions are taken, scientists' attitudes towards the new situation will be instrumental for the success of such measures. For this reason we have studied the behavior of Brazilian scientists from the biotechnological fields concerning Intellectual Property Rights. In this research 1,032 researchers were electronically contacted and 150 correctly responded (14,5%). They were selected among the biotechnology research leaders registered at the National Research Council in Brazil. The 41 ques-

tions include indicators about the interviewees' perceptions about their institutions' support for patenting research results, their attitudes towards recent changes in Intellectual Property Rights legislation, and about the interaction of researchers with demands from external interests. The results show contrasting patterns concerning the interviewed researchers' attitudes. In spite of the fact that they tend to behave rather defensively towards their institutions, it is possible to identify a group of researchers that strongly diverge, following other perception patterns. These opinions are associated to new research strategies which include new forms of interaction between these groups and demands external to the academic environment. Qualitative research supports this argument.

• Urban Ecology in Action: The Baltimore Rat-Control Program, 1942-'45,
Christine Keiner, Rochester Institute of Technology, USA

This study focuses on a unique rat-control campaign that took place in Baltimore during WWII. The campaign was led by two Johns Hopkins biologists who later achieved fame in their subfields: Curt RICHTER (1894-1988), who made groundbreaking contributions to the study of dietary self-section and biological clocks, and John T. EMLEN, JR. (1908-1997), who advanced the evolutionary study of animal ecology and behavior. The Baltimore rat-control campaign of 1942-'45 grew out of wartime experiments by RICHTER to test a new synthetic rodenticide, alpha-naphthyl thiourea (ANTU). The powerful new compound quickly attracted the attention of federal and municipal authorities. With funds from the Office of Scientific Research and Development, National Research Council, and Baltimore city officials, RICHTER and EMLEN developed new methods for attacking the urban rat problem. They pioneered the concept of using a city block as an ecological unit for rat control, and developed new methods for studying rat population dynamics. The campaign was marked by political infighting and rancorous debates over whether rodent control belonged to the city health department. RICHTER, who believed the best way to control rats was via chemical warfare, feuded publicly with the city health commissioner, who asserted that eliminating rat habitat was the best solution. The Baltimore rat-control campaign and its accompanying debates highlight early efforts to apply an ecological approach to the control of urban nature. This paper analyzes those efforts by integrating themes from the history of urban ecology, public health, and pesticide technology.

• Biomedical Research in the USSR, **Oxana Klimkova**, Central European U, Hungary

This paper explores a specific kind of biomedical research in the Soviet Union, conducted under conditions of high secrecy in special institutions well known in Russian public discourse as 'sharashki'. Since the scientists were often compelled to conduct research in biology and medicine under such conditions for the interests of the state, this practice might be best understood as a unique kind of 'forced intellectual labour' in a totalitarian state. The paper focuses on the areas of such biomedical research, the biographies of the major scientists involved, the conditions under which they worked, the reasons and historical significance of the evolution of this kind of research practice, and its implications for the development of the biomedical research in the Soviet Union.

Sunday, 9-10:30 AM, LR9
SESSION VII

Dimensions of Genomics

Organizer and Chair: **Jane Calvert**

• Sequencing Human Genomes, **Adam Bostanci**, U of Exeter, UK

The efforts by publicly and privately funded laboratories to sequence the human genome continue to make headlines world-wide. In this paper I describe the moral and productive economies of the DNA sequencing operations of the Human Genome Project (HGP) and The Institute of Genomic Research (TIGR). The HGP pieced together its version of the human genetic code in a 'map-based

sequencing' operation. TIGR, in contrast, pioneered the 'whole-genome shotgun', which was later controversially employed by the company Celera Genomics. In this paper I suggest that the draft genomes produced by the HGP and Celera Genomics differ not only by virtue of superficial sequence discrepancies but also in terms of their topology. Given that the human genome has commonly been conceptualised and idolised as a single natural object, it is of course ironic that current research has produced two dissimilar versions of our genetic blueprint. I conclude by making tentative suggestions about how the two drafts of the human genomes might be reconciled in scientific practice.

• Negotiating and Interpreting the Science/Technology Boundary in Genomics,
Jane Calvert, U of Exeter, UK

This paper is concerned with how scientists engaged in genomics negotiate and interpret the science/technology boundary, by drawing on in-depth interviews with university researchers on the nature of their work. The focus is on how scientists manage both public and private interests in their research. For example, how do they decide whether they should publish or patent their findings? Does this depend on whether the research itself is considered 'basic' or 'applied'? To what extent do economic interests infiltrate the research and influence its dissemination? The implications of these findings for the commodification of genomic knowledge and for the changing nature of university research are addressed. Interview material is drawn on to examine the rhetorical strategies used by scientists in these contexts when negotiating, interpreting and justifying their activities, in order to uncover the meanings and values they attach to their research. Finally, taking these rhetorical strategies into account, the paper asks if genomics epitomises 'technoscience' and whether traditional distinctions between science and technology are no longer appropriate in this fast-moving field.

• Genomics and International Agricultural Research: A Dialogue of the Deaf?
David Reece, U of Exeter, UK

This paper distinguishes between the cognitive and institutional character of research, and suggests that the institutional characteristics of International Agricultural Research may limit its capacity to make use of the (cognitive) content of genomics. Advances in genomics could, at least potentially, contribute to the development of new agricultural technologies for use in the 'Third World'. Any such contribution would necessarily be mediated by the institutions, led by the International Agricultural Research Centres (IARCs), that apply science and technology to developing-country agriculture. This paper therefore elucidates the structure and function of the IARCs and their principal stakeholders. It emphasizes that, in stark contrast with genomics, the IARCs display a structural separation between research ('knowledge search') and production ('knowledge use'), while the relationships between the IARCs and their stakeholders exhibit a unique blend of market and non-market characteristics. These arrangements, it is argued, have hindered attempts to incorporate a 'client focus' into the work of the IARCs, instead sustaining a paradigm that is becoming progressively less relevant to the needs of the developing world. Finally, the paper asks whether the structure of the IARCs will prevent them from benefiting from advances in genomics.

• Claims to Knowledge in Behavioural Genetics: The Problem of Mapping the Field,
Richard Holdsworth, U of Exeter, UK

The prospect that human genome research will be used to support claims for genomic influence on the development of behaviour has given rise to ethical debate (examined, e.g., in the report by the Nuffield Council on Bioethics on 'Genetics and human behaviour—the ethical context', London, October 2002). However, this underlines the fact that philosophy also has the epistemological task of analysing claims to knowledge in this area. But first: what is the extent of 'this area'? Its scope is potentially so wide, and the approaches within it so disparate, that it is far from self-evident what disciplines and research programmes comprise the field of behavioural genetics, which must in-

clude the investigation of interactionist and environmentalist theses. Trying to map the territory gives rise to the problem of comparing like with like, and the problem of choosing criteria for making such identifications. The paper discusses the validity of a descriptive approach designed to lay the basis for an ostensive definition of the research categories.

Sunday, 9-10:30 AM, LR8
SESSION VIII

Biology and Meaning I

Contributed Papers, Chair: **Jesper Hoffmeyer**

• Semiogenic Scaffolding in Living Agents, **Jesper Hoffmeyer**, U of Copenhagen, Denmark

Organisms, and the cells and tissues of which they are built are not just objects but also subjects in the sense that they are semiotic agents capable of interacting with their surroundings in 'clever' ways. The history of how these semiotic interaction patterns have been scaffolded into the myriads of ontogenetically consistent dynamics of this world, i.e., the life cycles of organisms, is what evolution is about. Genetic fixation of course plays a crucial role in such scaffolding but it has been claimed that there are countless semiotic ways of obtaining a relatively secure scaffolding of intra- and interspecific interaction patterns.

• Where Does Pattee's "How Does a Molecule Become a Message?" Belong in the History of Biosemiotics? **Jon Umerez**, U of the Basque Country, Spain

Recalling the title of YOXEN's classical paper on the influence of SCHRÖDINGER's book, I will try to analyze the role that might have played, if any, the work of H. PATTEE in the development of Biosemiotics. I will take his 1969 paper "How does a molecule become a message?" as a first target due to several circumstances that make it especially salient, but will also consider his more general contribution. On the one hand, even if PATTEE has obviously developed further his ideas in later papers, the significance of this one springs out right from the title, the journal and date of publication and, of course, its content. On the other, this paper in particular has been somehow rediscovered recently and not only within the frame of biosemiotics (for instance, Evelyn Fox KELLER has repeatedly mentioned it lately and other authors working in the history and philosophy of biology as well). Following the parallelism with YOXEN's paper we could say that PATTEE's work was relatively influential with respect to a good amount of attempts to rethink living systems within theoretical biology around the 1970s. This influence diminished together with the decay or even collapse of those attempts under the impact of molecular biology as it was been developed those years. Eventually, PATTEE's work has been taken up again. Nevertheless, it is quite clear that PATTEE himself did not intend to contribute specifically to Biosemiotics in any way, even to the point that he was probably unaware of any such discipline, at least until recently. We should as well ask, then (as YOXEN wonders with respect to SCHRÖDINGER) to which extent PATTEE's influence has been a direct one or rather an indication of the relevance of his ideas and the resonance of his hypothesis with those of biosemiotics. For this task I will examine the work of authors such as HOFFMEYER, EMMECHE, or KULL who either address directly this issue or have contributed significantly to building up the history of Biosemiotics.

• Biolinguistics and the Causal Theory of Reference,
Wolfram Hinzen, U of Regensburg, Germany

Biolinguistics, as inaugurated by CHOMSKY, is an attempt to study human language as an aspect of the natural world, i.e., human biology. The program attempts to explain how it happens that an organism takes up referential relations to the world using particular structured sounds and meanings. For most philosophers in the functionalist and 'teleosemantic' tradition, this has meant that one has to answer the question what converts purely physical states into 'mental states', i.e., states that have

a certain representational content. The standard answer to this question appeals to causation in conjunction with a notion of biological function (natural teleology). The causal part of the story is essentially the commitment that we come to have a concept of a thing, X, because of that thing, X, and our causal encounters with it ('learning'). The teleological part is a reaction to the fact that we are causally induced to entertain certain concepts by many other things than the ones that these concepts intuitively denote. For example, the causal trigger that leads the frog to have a mental representation that causes it to snap can be both a fly and a black moving food pellet that someone inserts in the frog's environment. By a common perception, this indeterminacy of meaning has seemed an unacceptable result as regards human meaning. The verdict in the frog's case is that it is not a matter of the frog's biological design to snap at artifical black food pellets. The frog malfunctions in such cases, and the intuition is that its mental representation had been one of flies, had the frog been functioning properly. The same solution in the case of human meaning, I argue, leads nowhere and thereby also shows interesting features of human meaning that are discontinuous with meaning in animal communication systems generally (vervets, ants, bees, etc.). At first, a causal view seems exceedingly plausible: what on earth except for doorknobs should possibly explain why we come to have a concept of doorknobs? What on earth if not how we learned about doorknobs in the course of our life history, or how we were causally affected by them should explain what doorknob, as we use that concept, means or refers to? But DARWIN taught us that in order to explain a perfect adaptation of a character to an environmental feature, we need not assume any directed variations—any environmental pressures that directly induce a character adapted to it. The environment does not drive the variation, it selects from given forms that arise by internal cauation. Theories of human meaning that derive human concepts from what they are used to refer to directly ignore the basic point that the current function of a trait does not need to tell us anything about how or why it came to be. This is a first point against the causal-teleological view, the second one being that on closer inspection, the meaning of human words indeed cannot be defined in terms of the worldy referents they are used to refer to and stand in causal relations with. The simplest substantive lexical items, such as house, person, or city, reveal an inner complexity raising 'poverty-of-the-stimulus' problems as vast as in the case of syntax, where they are widely regarded to force internalist conclusions. Unintuitive as the claim may seem that human concepts have internal rather than external causes, this Darwinian (as opposed to Spencerian or Lamarckian) suggestion seems as good a suggestion as one can currently make. A look at the most recent empirical data and theoretical research on concept acquisition confirms this impression. So does the point that communicative pressures appear to have nothing to do with the structural features of human languages that we universally find, and that the features of elegance and minimality in human language design has anything to do with the shaping influence of an environment 'molding' the human organism, in either phylogeny or ontogeny. By and large, then, an 'internalist' and 'Darwinian' perspective of meaning in conjunction with the biolinguistic program suggests quite the opposite of the causal-externalist view of meaning that is an orthodoxy in American philosophy today. None of this will entail that meaning is deprived of any externalist elements: how a given concept functions with respect to an environment cannot obviously be determined without looking at that environment. Such an externalism does nothing to imply the kind of causal flow from the environment to the organism that I centrally dispute, or the idea that meaning is a direct relation between 'words and objects'.

Sunday, 9-10:30 AM, LR10
SESSION IX

Biological Hierarchies I

Contributed Papers, Chair: **Bence Nanay**

• Evolutionary Processes Among Replicators, Interactors, and Transactors,
Donato Bergandi, Florida State U, USA

In evolutionary biology and ecology the traditional hierarchy of genes, organisms, demes, populations and species, that have been the imaginative ontological scenery of the scientific community for decades, have met a deep metamorphosis. Among the scientists more sensitive to the epistemological dimension of their work, all these entities can be seen through the interpretative filter of replication and interaction. The replication concerns the passage of genic information from generation to generation. While the interactive dimension of evolution would concern the interaction of all organizational levels, from genes to species, with their environments. The replication and the interaction processes have been materialized in the 'replicator' and 'interactor' entities. At first, these terms can appear bizarre neologisms to exemplify entities that have complementary roles in the evolutionary process. But I suppose the picture is not totally complete if we do not add another term: the 'transactor'. In the wake of the transactional approach of DEWEY and BENTLEY, and the emergentist conceptions of FEIBLEMAN and CAMPBELL, and as the logical complement of HULL's concept of interactor, the introduction of the term and concept 'transactor' is suggested. In the debate over selection units, the concept of 'transactor' offers a potential conceptual instrument that is functional with regard to the representation of systemic-emergentist positions concerning the higher levels of organization of the evolutionary process, including populations, communities, and species. The theoretical core of a transactional perspective is the idea that the intimate connection existing in the transactor between the entity selected and the ecological-selective environment exists such that this latter is not set once and for all, but instead is constructed from the organisms at least as much as that same environment contributes to constructing them. As a whole, the transactors would constitute a transactional hierarchy in which the emergent properties of every level certainly have their roots in a lower level of organization, but 'take roots' also to a higher level. Since the higher levels participate as well, the inferior levels participate in the determination of the emergent properties of a specific level. Moreover, it is important to emphasize that in every type of transactor, the environment and the entities, which in an atomistic and/or interactional perspective have external relations, should instead be considered directly and closely integrated or connected. In this respect, it is important to notice that HULL recognizes that "the distinction between an organism and its environment is … artificial." The transactor must be understood as a methodological construct implying that (i) the attribution of specific emergent properties may express specific adaptations, and (ii) in the search for the causal explanation of adaptations, we can avoid resorting to the analytic decomposition of the bottom transactors. Finally, (iii) the need to take into account the upper transactor is revealed when the differential frequency of the proliferation of an entity (gene, organism, deme, population, species) is sensitive to, or depends on, its 'context'. In other words, the fitness of such entities can depend on specific selective-environmental factors and on the influence of the higher transactor, at least as much as on the specific properties of the entities in question.

• Is Replication a Philosophically Interesting Concept?
Bence Nanay, U of California, Berkeley, USA

I question the importance of the concept of replication by suggesting the possibility that the various definitions of replication either do not prove to be philosophically interesting (i.e., they cannot serve as the basis of adaptation-explanations) or they are so narrow that only the genotype counts as replicator, which would make the general category of replication useless. After analysing various definitions of replication, it is argued that only one of these could be used in adaptation expla-

nations, namely, the one that is involved in cumulative selection. Finally, I question that anything else than the genotype may belong to this category.

• Models and Experiments that Introduce the Relationship Between Genes and Time in Developmental Biology, **Charles Galperin**, Société d'Histoire et d'Èpistémologie des Sciences de la Vie, Paris, France.

We plan to present a conceptual analysis, both historical and critical, of some aspects of developmental biology from experimental embryology to molecular genetics. We shall concentrate on Lewis WOLPERT's models during the seventies and Denis DUBOULE's around 1990. WOLPERT invented the concept of 'positional information'. He applied its modelisation to sea urchins, hydra, and the early development of the chick's wing. We shall underline this last example. DUBOULE is an eminent master of the Hox genetic system in the development of vertebrates' limbs. We shall deal with the following items: (1) Positional Information and the model of regulation in experimental embryology—Model 1; (2) Introduction of time in the growth of vertebrates' limb. Positional Information and morphogenesis—Model 2; (3) Hox genetic system and relationship between growth control and pattern formation in the work of DUBOULE and his collaborators—Model 3; and (4) Notes on relations between theoretical work and experimental systems.

• Does the Theory of Cultural Evolution Need the Replicator-Interactor Distinction? **Jean Lachapelle**, Champlain Regional College, Canada, **Luc Faucher**, U of Quebec, Canada, and **Pierre Poirier**, U of Quebec, Canada

Ever since DAWKINS' construal of cultural evolution in terms of memes in *The Selfish Gene*, the replicator/interactor has been at the heart of several models of cultural evolution. Yet the very utility of the replicator/interactor distinction to understand biological evolution has recently been questioned, most notably by developmental systems theory (DST). Drawing on some insights of DST, we will argue that the replicator/interactor distinction has a very limited role to play in a theory of cultural evolution. First, we will suggest that some of the problems plaguing a gene-centered approach to biological evolution also affect a meme-centered approach to cultural evolution. This will then lead us to reconsider some aspects of the theory of cultural evolution. Specifically, we will show (1) how cultural evolution is possible without a specific element playing the role of a replicator; (2) that culture is a system of inheritance in which the notion of information transmission plays a fundamental role, but one that need not postulate the transmission of particulate replicators. We will conclude by outlining the criteria that make culture a genuine system of inheritance that is highly evolvable.

Sunday, 11-12:30 PM

SESSION I, LR1

Boundaries in Biomedicine

Organizer and Chair: **Paula Saukko**

• Troubling Risk. **Paula Saako**, U of Exeter, UK

This paper examines the calculations or constructions of 'risk' that constitute a threshold for prescribing genetic testing for factor V leiden and prothrombin G20210A, the two most common known mutations that predispose a person to deep vein thrombosis (DVT). These two mutations have been chosen for study because of their fuzzy and mundane nature, i.e., they are quite common but the absolute risk that they pose is relatively low. Decisions about screening for these factors as well as decisions about actions they require are also often made in the routine context of

general practice, for example, when prescribing oral contraceptives. As such, screening for the genetic predisposition for DVT provides a good case study for exploring the personal and political implications of the increasing use of genetic testing in mainstream medicine. The paper examines how the 'risk' posed by these factors is perceived by the doctors referring for screening, people being tested, the laboratory processing the tests, and genetic literature. It does not approach 'risk' from either the realist/expert perspective (measuring views in relation to a 'correct' understanding of risk) or from the pluralist/interpretive one (multiple equally good understandings of risk). Rather, it explores different notions of the threshold risk for screening in order to bring them into conversation with one another and point toward a multi-dimensional understanding and 'governing', in the Foucauldian sense, of this emerging technology.

• Facts and Values, **Christine Haukseller**, U of Exeter, UK

During the last five years stem cell biology has become a field of major interest for ethicists and even for some philosophers of science. After the birth of Dolly the sheep and the stem cell hype in 1998 new research programs provoked resistance from the public. Researchers suddenly saw themselves confronted with what they interpreted as caricatures of their work, linking it to fields that seemed to be topics only of science fiction until then. The strong public concerns about biotechnological research pushed researchers to develop their scientific language in interesting ways. Semantic distinctions and new speech was created to lead ethically concerned viewers away from important research projects and fix their focus onto sidepaths, especially in countries like Germany were the media made stem cell research a major issue. The following distinctions are typical examples of this semantic productivity: we learned to distinguish between toti-, pluri- and multipotent developmental states of early embryonic cells, resp. stem cells, between therapeutic and reproductive cloning or between an embryo and a pre-embryo. To lay people these distinctions suggest a serious and substantial difference. But once we take into account the point in time in the science-public-relation when they became important, what their function was at that time and how their meanings eventually shifted, it becomes quite obvious that the language they created was something different from just marking newly discovered facts. There is no practical difference between reproductive and therapeutic cloning. Technically it is just the same thing that is done and the criteria for success are the same until the blastocyste is developed. The reason why they make this distinction is to get rid of the fundamental ethical and moral sentiments and critiques that draw the work of scientist who work not on but with the cloning of humans into question. The focus of this talk is to give some examples of this strategic use of semantics in biological scientific language and to show thereby the moral baggage that this language carries.

• Changing Diseases, **Sara Melendro-Oliver**, U of Exeter, UK

With the completion of the analysis of the human genome, hopes have been raise that the genetic basis for many diseases will be revealed, encouraging in turn more genetic research for a multitude of diseases. This growing 'geneticization' assumes that most disorders have a genetic cause and makes the concept of 'genetic diseases' every day more common. In the last 30 years the category of genetic disease has grown continually. Today, genetic diseases number close to four thousand for single-locus syndromes alone; the ailments that have been described as multilocus have not even been counted. This conceptual change on the aetiology of many diseases and the expansion of the category of genetic disease can only in part be attributed to the development of scientific knowledge. In part, is a result of the ideological and institutional expansion of molecular genetics and at the genetic reductionism present in the popular discourse of genetics. The introduction of new technologies in genetics is closely linked to the construction of new accounts about the origins of disease. Every day more common acquired pathologies are been explained in terms of error in the way genes are regulated. Rarely is the talk about how genes can be expressed in different environments; the assumption is that genes, like microbes, can be determinant apart from context. With the advent of genetic explanation of diseases, the external environment is becoming largely superfluous, the clinical focus shifted entirely to the internal environment. One common disease

that has recently undergone a conceptual change and is now routinely defined as a 'genetic disease' is cancer. Here, I will look at how cancer has changed to be explained as a 'genetic disease' and what is the relevance of this conceptual change. The case of cancer will act as an example of how many common acquired diseases are increasingly being constructed as essentially genetic in origin.

Sunday, 11-12:30 PM, LR2
SESSION II

Scientific Change from a Biological Point of View

Contributed Papers, Chair: **Sheldon Richmond**

• The Impact of the Debate about the Interpretation of Quantum Mechanics on the Problem of Science as Revolutionary of Evolutionary, **Sheldon Richmond**, Canada

Is the history of science revolutionary or evolutionary? The common view is that science is marked by several major revolutionary episodes such as the Copernican, Newtonian, Einsteinian, and Heisenbergian-Bohrian. This view contradicts the general model of evolutionary epistemology (EE) developed by Karl POPPER. Evolution involves the inheritance of various characteristics from earlier 'species', or a large degree of continuity, and competition among species relative to a common eco-niche. Furthermore, according to POPPER's version of EE, scientific knowledge itself is an evolutionary product of common sense knowledge. Moreover, scientific knowledge involves comparison across competing cosmologies in the eco-niche of common problem situations. However, POPPER's theory of scientific knowledge as evolutionary is putatively refuted by the actual history of science. Does the history of science actually contradict EE? No.

The common view of the dominance of the Copenhagen Interpretation (CI) of Quantum Mechanics (QM), on the surface, appears to bear out the notion that science is dominated by paradigms, and that normal science involves consensus on fundamental principles. Debates concerning the fundamentals of QM are apparent and merely reinforce mainstream physicists in their agreement to CI. Moreover, EINSTEIN and his various challenges including the EPR paradox and David BOHM with his hidden variables theory represent a fringe minority of the old guard dominated by the defunct and superseded determinist model of Classical Physics. This common view has the ring of truth about it in that the old wave of criticism by EINSTEIN and BOHM has collapsed, especially given the success of the particle entanglement experiments. However, the common view is falsified by the occurrence of the new wave of contemporary critics who seek to improve upon CI by evolving QM in the direction of a better solut!ion to its main problem in physics.

The main problem in physics which the CI of QM attempted to solve is the problem of the observer in physical systems. POPPER and Michael REDHEAD developed a realist interpretation to argue for the irrelevance of the observer in QM. Hugh EVERETT III developed the relative wave theory (later dubbed the many worlds theory by Bryce DE WITT, and recently advanced by Max TEGMARK and David DEUTSCH) to include the observer within QM as opposed to leaving the observer within the classical world. Roger PENROSE argues for developing an entirely new non-classical physics to resolve the problem of the observer in QM. In general, criticisms of QM by the new wave of critics is based on resolving the failure of CI to resolve the problem of the role of the observer in physical systems that was created by QM. Contrary to the revolutionary hypothesis about the history of science, the evolutionary hypothesis conforms to the actual history of science, and so the actual history of science, at least in the case of QM is consistent with EE.

• An Evolutionary Analysis of Scientific Change: The Case of Genomics,
Maureen O'Malley, Dalhousie U, Canada

Evolutionary accounts of science are historical, sociological, and evaluative. They offer novel insights into scientific change in general, as well as explanations of success or failure in specific scientific research programmes. Our approach integrates and builds on David HULL's and John ZI-

MAN's evolutionary accounts of science. Their chief claims are that scientific change entails a selection process, and that the success of particular theories and practices depends on their relationship with their scientific and social niche. From this basis we develop a multi-level Darwinian model of science, which we then apply to the discipline of genomics. Our analysis of the field shows that many traditional scientific practices, organizational structures and motivational goals are undergoing rapid transformation. We discuss the implications of these changes in terms of our model, and what they mean for conceptions of scientific success in general.

• Evolution and the Historical Sciences, **Aviezer Tucker**

The cladistic modeling and inference that are characteristic of Darwinian Biology are shared by a number of sciences that have been evolving since the middle of the 18th century: Biblical Criticism, Classical Philology, Comparative Linguistics, and scientific (Rankean) Historiography. I explicate the structure of inference of a common cause from similar effects in all these sciences in Bayesian terms. I argue that we find in all these sciences a two stage argument logically and historically: First that the properties of the evidence scientists study (species, texts, languages, content of testimonies) is more likely given a common cause than given separate causes. Second, assuming a common cause, that the evidence is more likely given a particular common cause than given alternative concrete common causes.

Sunday, 11-12:30 PM, LR5
SESSION III

Alternative Evolutionary Theories II

Organizers: **George Levit** and **Uwe Hossfeld**, Chair: **Uwe Hossfeld**

• "The Nomogenesis," **George Levit**, Jena U, Germany

The theory of nomogenesis was founded by a Russian biologist Lew (Leo) BERG (1876-1950). In 1922 BERG published a book, *Nomogenesis or Evolution Determined by Law*. BERG's basic objective was to show the directed and law-governed character of evolution. He compared the taxons with the individuals and stated that there are laws controlling the rise and fall of any taxonomic group. He did not reject natural selection, but restricted its role in evolution. An important point of his concept was the rejection of Darwinian gradualism. Nomogenetic evolution is a spasmodic process. Although most of BERG's friends and colleagues were fascinated by his personality and scientific erudition, the idea of nomogenesis found only few followers. The first English version of *Nomogenesis* appeared with the introduction of D'Arcy THOMPSON in 1926. The second English edition was promoted by T. DOBZHANSKY, who did not accept nomogenesis but was fascinated by BERG's ideas and tried to initiate a discussion of them in the Western world. Despite such prominent support, BERG's ideas drew little attention in the English-speaking world. Yet in Russia the nomogenesis has been never completely forgotten. A. A. LIUBISTCHEV (1890-1972) and S. V. MEYEN (1935-1987) based their concepts on BERG's nomogenesis. I will describe nomogenesis theory, sketch its development and influences throughout the 20th century, and define its place in the history of evolutionary ideas.

• Engineering Morphology for Evolutionary Theory, **Michael Gudo**, Frankfurt U, Germany

Anatomical structures of organisms were described by morphogy in a traditional way. But it is a different approach to study organisms under structural-functional and biomechanical aspects in a similar way an engineer would analyse a technical apparatus. The arrangement and the biomechanical coherence of anatomical structures (i.e., structural-functional organisation) and the processes of generation and preservation of form during individual development and evolution are in focus of interest (engineering morphology). According to this, organisms are dynamic, energy-transducing machines and hydraulic entities; they are complex functional units which could not

evolve arbitrarily. Due to structural-functional, biomechanical, and hydraulic principles evolution has to follow natural laws limiting evolutionary changes. Therefore evolutionary research has to deal with two major aspects: (1) What are the constraints of the evolutionary process?, and (2) How can evolutionary pathways be reconstructed? In the so-called Frankfurt Theory of Evolution (originally conceptualized by Wolfgang F. GUTMANN) a promising post-Darwinian approach for such evolutionary research has been developed.

• Bryozoan Individuals and Bryozoan Individualization,
Joachim Scholz, Senckenberg Research Institute, Germany, **George Levit**, Jena U, Germany

It is well known that classical Darwinism is an organism-centered concept, i.e., Darwinian individuals are organisms. Some recent modifications of Darwinism like the theory of punctuated equilibrium distinguish different levels of Darwinian individuality: genes, cells lineages, organisms, demes, species, and clades. Both classical Darwinism and the hierarchical theory of selection suppose that the agents of selection (elements of the hierarchy) and their environments are clearly definable entities. Yet in some 'marginal', slightly individualized biosystems there is no clarity about how to define agents and environments. Where exactly do we mark our boundaries in the biological hierarchies of genes, cells, organisms or colonial modules, populations, metapopulations, species and ecosystems? The example of bryozoans shows that there is actually nothing like an exact boundary between colonial modules, colonies, and populations. Bryozoans represent a phylum of aquatic colonial and clonal animals. Colonies are constructed by repeated budding of genetically but not always morphologically identical, physiologically connected zooids. In life history paterns some colonies act like a single organism, whereas other colonies rely mainly on the biological potentials of each colonial module. Some bryozoan species are similar to a microbial mat consisting of many different species with underlying bryozoan zooids. In such cases it is extremely difficult to distinguish 'organisms' from their 'environments', because the structural relationships are much more complex than can be represented by the traditional schemes. A revised concept of biological individuality is thus required to understand certain aspects of coloniality and bryozoan growth. The question whether the required revision would influence the existing selectionist doctrine can be posed.

Sunday, 11-12:30 PM, LR6
SESSION IV

Autonomy: A Key Concept II

Organizers: **Alvaro Moreno** and **Kepa Ruiz**, Chair: **Kepa Ruiz**

• Chemical Autonomy and Stoichiometric Freedom: Toward a General Theory of Genetic Information, **James Griesemer**, U of California, Davis, USA

I seek a merger of theories of physical, chemical and biological autonomy (HOOKER, CHRISTENSEN, COLLIER, VARELA, GÁNTI) on the one hand with theories of generative structures (WIMSATT, SCHANK) on the other. My work on the nature of reproduction processes leads to the view that evolution requires multiple inheritance systems if it is to achieve the complex organizational forms characteristic of life on earth. Current evolutionary research on epigenetic molecular inheritance systems (JABLONKA and LAMB), dual inheritance of cultural and biological traits (BOYD and RICHERSON), behavioral inheritance systems (JABLONKA), and others all lend support to this pluralist view of inheritance systems. Recent arguments by developmental systems theorists, however, suggest that multiple inheritance channels depend on a flawed conception of hereditary systems as carriers of genetic information and that explanatory parity or causal symmetry arguments defeat the causal privilege usually accorded to genes in information-based accounts (OYAMA, GRAY, GRIFFITHS, KNIGHT). These arguments assume an account of the information relation between sources and receivers based either on the notion of systematic statistical dependency or on teleosemantics. In this paper, I pursue an alternative strategy based on an account of evolutionary proc-

esses that distinguishes autonomy—the basis for causal-mechanical arguments about inheritance systems as information carriers—from statistical independence—the basis for parity arguments against explanatory privileging of the causal role of genes in heredity, development, and evolution. The view that only whole developmental systems can be 'replicators' (hereditary units of evolution) and its support for only a single inheritance system, I argue, conflates these two very different conceptual bases for the analysis of inheritance. I propose a concept of chemical autonomy, expanding on Tibor GÁNTI's notion of stoichiometric freedom.

• Steps Towards Life: A Perspective on Basic Autonomy, **Kepa Ruiz-Mirazo**

The way science has tackled the problem of the origins of life during the last century is to search for the abiotic synthesis of the fundamental organic molecules that constitute all present living beings. The emphasis has been put on the properties of biomolecules, like proteins or nucleic acids, under the assumption that they are the key to trigger off biological behaviour (and, in particular, a process of Darwinian evolution). Accordingly, the most popular theories in the field (i.e., those behind the 'RNA-world' hypothesis) put forward a prebiotic scenario in which a population of complex organic molecules—of the 'nucleic acid' type—would be able to set up some sort of 'biological evolution' process as a result of their inherent replicative-competitive dynamics. However, as MOROWITZ, WÄCHTERSHÄUSER, SHAPIRO and several other authors have pointed out, that scenario is not very realistic, given the high molecular complexity it presupposes. In fact, the synthesis of biopolymers, or even their—relatively more simple—monomers, proves quite difficult in free-solution, abiotic conditions. Besides, the phenomenon of life cannot be reduced to the molecular components of biological systems, or to their structural properties; rather, it involves a whole process—or set of processes—of interaction and transformation in which the components of the system get functionally integrated. In this paper I argue that such a functional/organizational integration must come about quite early in the sequence of transitions towards the emergence of life, when some chemical self-maintaining networks create their own boundaries and develop an autonomous machinery of production of components (of still low molecular complexity), establishing a set of couplings between endergonic-exergonic processes and managing that way the flow of matter-energy through them so as to achieve self-construction. The authors of the theory of 'autopoiesis' even claim that a basic autonomous system of this kind would gather all what is needed to define life in its minimal sense. I will show why I disagree with this idea, as well as why the classical autopoietic criteria have to be profoundly reviewed, introducing a new, physically better grounded, conception of 'basic autonomy'. According to my account, although 'basic autonomy' is not sufficient for life, it constitutes a crucial step in any prebiotic scenario that we could think of, and one of the first stages that can and should be explored by researchers in the field.

• Autonomy and Information: Two Fundamental Principles in Biological Organization,
Alvaro Moreno, U of the Basque Country, Spain

Autonomy and information appear as two fundamental but opposite principles in biological organization. The origin of autonomous systems is conceivable as some natural development of the physico-chemical mechanisms that rule self-organizing processes, whereas information seems to be an organizational principle incompatible with the former, because it establishes a non dynamical (discrete, rate independent, physically arbitrary) causal relation between the informational components and their meanings. However, the origin of information can be linked with the development of a new stage in the evolution of autonomous systems, that of their insertion in a historical and collective meta-system. Thus, information becomes fundamental in the generation of new ways of functional inter-dependence among individual autonomous systems, bringing about a wider spatial and temporal organization.

Sunday, 11-12:30 PM, LR9
SESSION V

Is There a Kantian Tradition in Biology?

Organizer and Chair: **Phillippe Huneman**

• Functions' Succession and Classification in *Naturphilosophie*, **Stéphane Schmitt**, CNRS, France

The concept of function plays an essential role in the classifications worked out by German naturalists at the turn of the 18th century. KIELMAYER (1793) talks of a gradation of 5 cardinal functions along the animal scale. OKEN (1804) asserts a classification of animals based on a progression of sensitive functions. Here, we intend to describe the relationships of those theses with the Kantian tradition, and to show how some *Naturphilosophen*, mostly SCHELLING's students, assume this 'teleomechanist program' (LENOIR) in their interpretations of the diversity of beings.

• Caspar Friedrich Wolff's *De Formatione Intestinorum*, 1768-'69, or the Beginnings of Modern Embryology, **Jean-Claude Dupont**, CNRS, France

In 1768, the German embryologist Caspar Friedrich WOLFF (1733-1794) feels the impasse that would represent a supplementary discussion about *vis essentialis* and formative causes, problems he will bring up again in his discussions with BLUMENBACH, as he feels the uselessness of a theoretical argumentation against BONNET and HALLER on invisibility. It was necessary to produce a work whose professional skill would equal that of HALLER on the heart: it will be *De formatione intestinorum,* that he will publish in the Reports of the Academy of the Sciences of Saint-Petersburg. Having reminded the ideas about the generation which he had already explained his thesis of medicine in 1759 (*Theoria generationis*), WOLFF follows minutely the development of the internal organs, mainly digestive system. One suggests explaining the genesis of this work, which, because of it describes intermediate embryonic forms, and because it breaks with the dominant preformationism and with the ancient epigenesis inherited from ARISTOTLE appears to be the first great text of modern embryology, setting the teleology problem in embryology apart from the *vis esssentialis* debate.

• Naturalizing Purpose: From Comparative Anatomy to the Adventures of Reason, **Phillippe Huneman**, CNRS, France

By ruling out 'relative purpose' from science in the *Critique of Judgment* (§63), KANT elaborates as 'natural purpose' a concept of organism as a continuous and reciprocal production of a whole by its parts and of the parts by the (idea of) the whole. As a regulative principle, it lies at the basis of a biological methodology. Grown up in the German naturalistic tradition like his friend PFAFF, CUVIER, whose comparative anatomy rests on his 'principe des conditions d'existence'—which translates into scientific language the old notion of 'final causes'—states a teleology analogous to Kantian methodology; its regulative dimension is mainly relevant to paleontology. But at the same time the Kantian question of an articulation between teleology and mechanism raised the question of the possibility of an 'archeology of nature' (CJ, §80), moving back to a unique and material origin of all living forms: according to KANT, this is the 'adventure of reason'. Such monistic conception lies on the problematic notion of an archetype; and at the end of the century GOETHE, after HERDER, will get involved in this adventure. Through his comparative anatomy and botanics, GOETHE's work establishes the concept of a morphotype, or Ur-*Typus*, that Geoffroy SAINT-HILAIRE will work through—and we know his growing opposition to CUVIER's science. We will ask in what extent this program, launched by GOETHE, vindicates against Kantian epistemology and CUVIER's comparative anatomy and until what point they could share some presuppositions.

Sunday, 11-12:30 PM, LR3
SESSION VI

Science and Policy: International Perspectives II

Contributed Papers, Chair: **Patricia Bunner**

• Postwar Conservation Management in British East and Central Africa: A Tale of Science, Law, Capitalism, and Development, **Patricia A. Bunner**, West Virginia U, USA

As WWII came to a close, there was neither any national park legislation nor any national park department in British East and Central Africa (comprising the Anglo-Egyptian Sudan, Uganda, Kenya, Northern and Southern Rhodesia, and Nyasaland). Not all of the colonial territories even had a 'game' department. The existing departments were minute in comparison to their territories and none possessed any sort of research agenda or scientifically trained staff member. Fifteen years later, the situation had shifted 180 degrees. Colonial development funds poured into the improvement of new national parks across the region, airplanes criss-crossed the skies overhead to count wildlife herds, and scores of biologists and ecologists moved over the landscape to analyze the habits and histories of the parks' 'faunal residents'. The shift represented a veritable conservation boom, and dozens of new parks appeared as bright green patches on previously monochrome maps.

What had happened in the immediate postwar years to change the status of national parks and wildlife conservation so dramatically? Some historians have traced the origins of 20th century wildlife conservation to the psychological and emotional importance of the African environment for European colonizers. Accordingly the roots of British colonial national park and wildlife laws have been trace to 'western' ideas of nature in Africa. Historians have offered concepts such as the 'Eden complex' or the 'cult of the hunt' to explain European, particularly British, fascination with African wildlife and anxieties over its appearance. The core of their argument is that the idea of Africa as symbolic Eden has stimulated western interests in African conservation through the colonial period and into the present.

International conservation activists offer another explanation, one that attributes the postwar conservation legislation to the efforts of concerned lobbyists, alarmed by the decline of wildlife over the first half of the 20th century.

The post war conservation boom is best understood within the context of British political economy and African history, not as an environmental anomaly. Concentrating on British political economy and African history reveals the pivotal role that British colonial Africa played in both modern international scientific and conservationists organizations and the global standardization of wildlife and national park codes. From the rise of international commercial airfare to stories in women's magazines, such as *House Beautiful*, my paper explores many factors that contributed to the growth of conservation practices in British East and Central Africa.

• Early Wadden Sea Research in Germany: Application-oriented Biologists Lost Contact with New Theoretical Approaches in Marine Ecology, **Hauke Bietz**

At the south-eastern coast of the North Sea occurs an extensive coherent transition zone between land and sea, termed the Wadden Sea. It is administered by Denmark, Germany, and the Netherlands. The diurnal tides contribute to a landward directed deposition of sediment particles. This process is augmented by certain plants and benthic animals, and was artificially enhanced by land reclamation works. To gain agricultural land, much of the inshore salt marshes and mud flats have been embanked in the course of the last centuries. In the 1930s and early '40s extensive efforts were made to understand the sedimentological developments and the changing benthic patterns. The biologists involved in this task sought for indicator species and analysed in detail the distribution and abundance of the benthic organisms in relation to sediment composition and other habitat properties. After WWII new ecological concepts with ecosystematic approaches were developed by marine scientists outside Germany. These ideas were somewhat neglected by German applica-

tion-orientated coastal research. It is argued that involved scientists were trapped in a dilemma: on the one hand was their desire to make economic use of natural processes, on the other hand was the elimination of their very study objects by further embankments. Today, the natural value of this coastal area has surpassed the potential for agriculture. Large parts of the Wadden Sea have been designated a National Park. Although the pioneering research on the biota of the Wadden Sea tidal flats still offers a wealth of knowledge and insight, these studies have become almost forgotten because of the present environmentalist attitude contrasts with the coastal engineering approach seven decades ago.

Sunday, 11-12:30 PM, LR7
SESSION VII
Between Gender, Politics, and Biology
Contributed Papers, Chair: **Carla Fehr**

• Feminist Politics Improving Biology: Where is the Objectivity? **Carla Fehr**, Iowa State U, USA

Although very few philosophers would call themselves positivists, logical positivism remains one of the strongest influences on scientific methodology. This legacy of positivism has been the focus of many criticisms of science because in its focus on the logical character of science, it tends to erase social factors of race, class, and gender. Although these criticisms of positivism are well justified, many feminists would be surprised to find out that they share the political intentions of these early positivists. Many positivists fled Europe in the face of the Nazi threat, and knew first hand the dangers of politically influenced science. They hoped that their focus on logic would make anti-Semitic science impossible.

These intentions were good, but the project failed leaving behind a problematic legacy in the view that good science is value free, that it exists without bias, and that results depend solely on the nature of the world as it is revealed by impartial experiment, rather than by the culture that produces the science or the particular position of the scientist. Feminist philosophers of science have argued that science, because of its homogeneous culture, disguises rather than erases its social influences. Scientists repeat experiments in order to determine if the results obtained were due to nature or due to the peculiarities of a particular researcher. But, if all of the researchers are similar with respect to the culture that they work in, then the possibility of cultural influences cannot be detected in this manner.

Many feminist philosophers of science have argued that the invisibility of social values in science has produced a pernicious androcentric bias. Many of these problems have begun to be addressed as a result of feminist political activity. But this issue raises a crucial question about the nature of science that I will address in this project. In many of these cases, science was improved by the direct influence of political values. The intention of the positivists was to protect science from bad political influence, out of their good intentions came our ideal of objectivity. According to this traditional view, a researcher uses the scientific method to put himself in a position that has been called the 'view from nowhere', or 'God's eye view', which is a position without any particular perspective. The idea was to find a way to investigate the world that was free of any social or political sway. But, if as many feminists claim, there is no 'view from nowhere' and that what has been called the view from nowhere is really the view of educated upper-middleclass white American men, there is a substantial problem. If we allow social and political ideals to determine what science is being done, how can we be assured that we will choose the correct political and social ideals to guide us? The influence of social ideals has improved many areas of biological and medical research, but it was also the influence of political and social ideals that motivated such things as Nazi science. While it is obvious now that feminist medical research on women is good and that anti-Semitic science is bad, our culture, and our scientists, do not have a good track record of evaluating harmful cultural influences at the time that there were occurring.

A positivist critic of feminist views of objectivity could convincingly argue that even though we may not be able to achieve a traditional version of objectivity, at least in aspiring to it we can

attempt to protect science from what may turn out to be perilous political and social influences. As of now, feminist philosophers of science don't have a satisfactory response to this criticism. But, these views do not address the question of whether or not the social influences that we choose to be guided by are morally justified, and in fact, even among feminists there is disagreement concerning which social values ought to be promoted.□

This project is designed to fill this serious lacuna in feminist philosophy of science and in the philosophy of biology.□I will compare androcentric developmental biology with the case of the evolution of sexual reproduction. The literature comprising these cases, because of their focus on sex, are likely loci for bias. Yet in studies of the evolution of sexual reproduction this bias is surprisingly absent. Also, this developmental biology has been encouragingly impacted by feminist politics. As a result, this comparison is a unique opportunity to investigate how androcentric bias enters science, how if affects the objectivity of the research, and hence, how to formulate a concept of objectivity that can protect against it. It is also a chance to observe the effects of a conscious choice of feminist social values in action.

• The Thwarting of a Promising Project: A Joint Laboratory Between the Institute for Radiobiology and the Vivarium in Vienna, **Maria Rentetzi**, Virginia Tech U, USA

In 1932 the directors of the Biological Institute of Vienna, known as Vivarium, proposed to the Austrian Academy of Science the establishment of a joint radium laboratory for biological research. Prompted by the developing interest in the application of radium in biology, the researchers of the Vivarium wished to contribute to the relevant scientific investigations but also envisioned their institute as the regulator and promoter of radium in Austria. The proposed scientific partner, the Institut für Radiumforschung, was one of the three most important radioactivity research centers in Europe with a long tradition of working in the field. They both supported the educational and welfare reform projects of the Social Democrats who had the control of the city from 1919 to 1934. Additionally, they hosted an appreciable number of Jewish and liberal scientists and surprisingly a large number of women researchers which reached 30% during Red Vienna. Eventually, the proposal was never realized. In my paper I explore the reasons behind the failure of the promising project focusing on gender, politics, and radiobiology in Vienna from 1920 to 1934. Common interest in radioactivity research among the researchers of the two institutes and a number of political reasons prompted the directors to envision and suggest the plan of a joint institute on radiobiology. The failure to establish it was a symptom of the decline of the Social Democratic reforms in the city of Vienna and the advent of the fascist regime in Austria while it marked the thwarting of a promising scientific collaboration.

• 'Fitra' (Created Nature): Premodern Concepts of Gender Biology in Current Popular Islam, **Amy Bix**, Iowa State U, and **Taner Edis**, Truman State U, USA

Historians of life sciences have long been familiar with ARISTOTLE's comments in *The Generation of Animals* referring to the female as a departure from type, a 'mutilated male'. Starting from the assumption that females were weaker and less fully-developed, ARISTOTLE derived theories of women's physical, emotional, and intellectual inferiority. Echoed and extended by the influential physician GALEN, such arguments carried into medieval Europe, where they were reinforced by superstition and misogynist theology. Into even the 20th century, some Western biology clung to presumptions of female passivity, weakness, 'hysteria', and reproductive disorder.

Historians generally end this discussion there, implying that the 21st century has finally moved beyond stereotypes of gendered physical differences. Yet looking beyond American and European biology, we find that premodern ideas about female bodies retain an amazing hold in the Islamic world. In popular writings about the nature of women, present-day Muslim apologists occasionally repeat Aristotelian precepts word for word. This paper examines this phenomenon, showing how early biological assumptions still have a place in current Islamic rhetoric.

As we will demonstrate, premodern concepts of gendered physical differences reinforce other elements of Islamic thinking about the proper relationship between the sexes. In trying to validate

Muslim social norms as a divine order, prescriptive behavioral literature draws on the Islamic philosophical heritage, with its traditional Aristotelian emphasis on the created natures of women and men. By investing created physical ideals with moral significance, Islamic apologetics defines any deviation not as natural biological variation, but as sin. Gender biology thus becomes part of Muslim discourse on the status of women, with its emphasis on symbolic issues such as dress. In Turkey, with its recent Islamic revival, some apologists use biological rationales to convince followers of the harmony of nature and God's will. Odd as it may sound to those raised amidst modern Western medicine and feminism, conservative Islam thus still invests Aristotelianism and premodern biological concepts with cultural and intellectual power.

• From Thelyplasm to Theology: The Biological Basis of Weininger's Sexology and Its Reception in Russia, **Joanna Trzeciak**, U of Chicago, USA

In 1903 Viennese sexologist Otto WEININGER published his magnum opus *Geschlecht und Charakter* (*Sex and Character*). Shortly thereafter he committed suicide at the age of 23. *Geschlecht und Charakter*, marked by extreme antisemitism, put forth critiques of modernity, female emancipation, and ultimately, of sexuality itself. WEININGER's general influence on Western intellectual discourse on sexuality has received considerable attention. In this paper, I focus on WEININGER's agenda-driven, selective use of the biological discourse of the period. In particular, he drew upon the observations of three biologists—Karl Wilhelm VON NÄGELI (1817-1891), August WEISSMAN (1834-1914), and Johannes Japetus Smith STEENSTRUP (1813-1897)—which he used to support such ideas as the innate homosexuality of human beings, the intermediacy of human sex, and the radical inferiority of women. What has received scant attention is the influence WEININGER's work exerted on Russian discourse on sexuality. The biological basis of WEININGER's views provided a ready-made justification for their incorporation into Russian theological and philosophical discourse on androgyny, such as the work of Nikolai BERDYAEV (1874-1948).

Sunday, 11-12:30 PM, LR8
SESSION VIII

Biology and Meaning II

Contributed Papers, Chair: **Edwina Taborsky**

• The Semiotic Dynamics of Societies, **Edwina Taborsky**, Bishop's U and U of Toronto, Canada

This paper is focused around my research in semiotic realism, which refers to the codal measurement of energy/mass interactions found within all realms of our universe, the abiotic, biotic and socioconceptual. Our universe is composed of energy and mass; the one is a version of the other. My basic axiom is that energy is transformed to mass within codification, and as such, is to be considered 'information'. I am postulating that social organization, that is, the symbolic organization of energy/mass by the species *Homo sapiens*, is not separate from but is an integral and integrated operative part of this universe of information dynamics. Using a semiotic infrastructure of codification, which includes a level of codification that establishes normative habits (symmetry enforcing) and a level of codification that establishes individual instances (symmetry breaking), I am examining societies as complex processes of energy-to-information dynamics. A basic postulate is that societies, as operative within this theory of information dynamics, function as distinct organized morphologies that can be classified according to their energy resources and modes of energy processing (ecological resources, population carrying-capacity, technology). The questions that arise are: What are the organizational requirements that develop as the information dynamics increase? How are the organizational infrastructures of a low energy/small population morphology different from those of a high energy/high population? Importantly, what causes change? I examine three basic types of social morphologies, analyzing them within codifying capacities, and then examine the morphologies of dysfunctional societies, i.e., what happens when a society with X

amount of energy tries to organize its population within a morphology that functions only within a society with Y amount of energy.

• Memetics, Memecology, and Memevolution, **Kastytis Beitas**, Vilnius U, Lithuania

The need to understand cultural evolution generated the idea of meme. Often meme is understood as the cultural equivalent of gene or of virus. But the meme concept is more complex, because it includes some aspects of biological organism. The most interesting aspect of memes is their ability to form 'ecological systems'. The ecology of memes can be studied as analogy of populations ecology or of coenotic (communities) ecology. Populations memecology deals with the spread of memes in human populations. Coenotic memecology concentrates on ontogenetic development of meme systems in the human mind, where ecological interactions such as mutualism, competition, and some others can be found. So this branch of memetics must be most interesting for cognitive scientists. The evolutionary aspect of memes is related to many cultural phenomena. For example, lexical evolution can be interpreted as equivalent of biological evolution. And an equivalent of evolutionary canalization can be found in lexical evolution: Concepts and their semantic content are internal canalization factors of lexical evolution. And cultural and environmental aspects of human life are external ones. Wide use of computers in today's education begins convergence of neuromemes and compomemes ('computeric memes'). Genomics and biotechnologies portend confluence of compomemes and genes. Broader look to memes and genes as to replicators gives us a possibility to interpret this confluence as a new evolutionary event, unseen in the biological history of Earth—the confluence of two 'biospheres', the ideosphere of memes and the infosphere of computers.

• The Genetic Nature of Culture, **Adolf Heschl**, KLI, Austria

Up to this day, a consistent evolutionary, that means both materialist and nonmetaphorical explanation of human behavior still must fail in front of the supposed special position of *Homo sapiens* regarding his unique cultural achievements. 'Culture', reads the most general definition, is the acquisition and subsequent transmission of newly acquired information from generation to generation, and this completely independently of purely genetic transfer processes occurring along WEISMANN's germ line (egg/sperm cell). By definition, such a characterization precludes a conceptual synthesis between cultural development and biological evolution, since in either cases fundamentally different sources of information are assumed. However, this clearly unsatisfactory situation changes decisively as soon as one brings into play two easily overlooked empirical facts: (1) most (hypothesis: all) individual behavioral achievements, including learning, imitation and tradition, depend on concrete genetic instructions in either a direct or indirect causal manner, and (2) most (hypothesis: all) cultural achievements are adaptive with regard to biological fitness defined as differential reproduction. Some selected examples from quite different areas of human behavior are given to document this situation.

Sunday, 11-12:30 PM, LR10
SESSION IX

Biological Hierarchies II

Contributed Papers, Chair: **Toshiyuki Nakajima**

• On the Compatibility of Neo-Clementsian and Neo-Gleasonian Approaches to Ecological Succession, **Kevin de Laplante**, Iowa State U, USA

There is a tradition of 'neo-Clementsian', 'neo-organismic' holistic ecosystem science that argues for the existence of ecosystem-level 'goal functions'—thermodynamic, cybernetic or information-theoretic functions that are said to describe and/or govern the process of ecosystem development. This view is contrasted with the more dominant (in the USA, at any rate) 'neo-Gleasonian' tradi-

tion that emphasizes the fundamental role of stochastic, 'bottom-up' mechanisms in ecological succession, and whose proponents are deeply skeptical of the utility or reality of 'top-down' organizing principles in community and ecosystem ecology. These two approaches are prima facie incompatible. In this paper I evaluate various attempts to argue for the fundamental compatibility of neo-Clementsian and neo-Gleasonian approaches to succession.

• Synchronic and Diachronic Hierarchies of Entities in Living Systems,
Toshiyuki Nakajima, Ehime U, Japan

Living things (or systems) are not entities like crystals or man-made machines like watches. They possess the property of maintaining their organization by continual renewal or production of components by themselves. Another property of great importance is self-reproduction, i.e., replication of systems of similar organization, based on the above self-maintenance ability. This twofold property of living systems forms unique hierarchical organizations of entities as living systems at higher levels, which are not seen in physical, abiotic worlds. This talk focuses on how biological entities at various levels of organization are produced in terms of the twofold property. In this argument, two types of hierarchical organization (or part-whole relationship) are distinguished. One is synchronic participation in organizing an entity (or system), and the other is diachronic participation. In the former organization, the component membership of an entity (system) is fixed through time, regardless of their organizational patterns; whereas the diachronic organization is formed by diachronic participation of parts to form a particular internal relation among components. Extending the framework of the cognizer's system model, both types of organization are formalized. Based on this formalism, biological entities and organizational hierarchies are analyzed. The analysis reveals that biological entities are arrayed in the two-dimensional hierarchical matrix of synchronic and diachronic organization, and that entities occupying different ontological positions in the hierarchical matrix, have been confused and called under the same name, such as cell, organism, and population, in biological discourse. For example, the matrix distinguishes 'organism' as a molecular machine and 'organism' made by diachronic participation of cells.

• Group Selection in a Changing Selective Environment, **Alex Dajkovic**, U of Kansas, USA

Models of evolutionary change generally assume constant selective environments where selection coefficients are assigned to different types and do not vary across generations. While useful, this simplification avoids the question of the effect of changing environmental conditions on fitness of organisms and groups. In most natural ecological situations it is often the case that selective pressures in the environment change over time, setting the stage for differential reproductive success of interactors at various levels depending the presence of adaptations specific to the given environmental conditions. I will discuss cases of genes in enterobacteria whose function is related to specific environmental conditions (e.g., DNA damage) which cause intense selective pressures of short duration. This provides an appropriate scenario to test adaptedness under particular selective circumstances and to analyze fitness contributions by individual genes. Observations of actual survival rates of mutant alleles under these selective conditions reveal existence of deleterious genes whose expression kills individual bacterial cells, but is appears beneficial at higher levels in the hierarchy of interactors. I will discuss the relevance of this case to the levels of selection debate, especially to the theory of hierarchy of interactors.

• Gene Expression and Phylogenetic Inference,
Sohrab Aghabozorgy, Institute of Molecular Biology, Iran

A thermophilic bacteria, *Thermotoga maritima* which, based on current data, has been considered an ancient microorganism was compared with *E. coli* and Enterobacteriae. This comparison was based on the expression of rplJ gene which encodes ribosomal protein L10. rplJ has two parts, one part encodes L10 protein and the other part has a regulating function which is affected by L10 protein through a negative feedback mechanism. In the first experiment we used that part of rplJ gene

which encodes L10 protein from *E. coli* and the regulating section of the gene from *T. maritima*. In the second experiment we used the regulating part of the gene from *T. maritima* and the encoding part of rplJ gene from *E. coli*. In both experiments the result was the same as normal situation when the entire gene (including both the regulating and encoding part of the gene) is from one organism doing its normal production and regulating activities. Based on the function of this gene in *E. coli* and ancient *T. maritima* and their close relation we concluded that *T. maritima* can be the old ancestor of *Enterobacters* including *E. coli.*

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Thursday	11:00 - 12:30pm	2:00 - 3:30pm	4:00 - 5:30pm
Session I	Interpreting Evolutionary Theory (LR1)	Experiments in Experimentalism I (LR4)	Experiments in Experimentalism II (LR8)
Session II	Biologists in Policy-Making (LR4)	History of 20th-C. Biology (LR8)	Contingency and Explanation (LR6)
Session III	Biological Information (LR7)	A Cultural History of Heredity I (LR1)	Cultural History of Heredity II (LR1)
Session IV	Reconfiguring Knowledge (LR2)	Picturing Eggs, Embryos, and Cells I (LR2)	Picturing Eggs, Embryos, and Cells II (LR2)
Session V	Biology and Gender (LR8)	Complexity: What is it Good For? I (LR5)	Complexity: What is it Good For? II (LR7)
Session VI	Zoos in Central Europe (LR6)	Lorenz and Co. (LR6)	Women in Early Genetics (LR3)
Session VII	Making Sense of Interlevel Causation (LR9)	Genetics and Policy-making (LR10)	The Extended Phenotype Revisited II (LR9)
Session VIII	50 Years of the Molecular Biology of Behavior (LR10)	The Extended Phenotype Revisited I (LR9)	History and Philosophy of Anthropology (LR5)
Session IX	History and Philosophy of Neuroscience (LR3)	Biological Modeling I (LR7)	Biological Modeling II (LR10)
Session X	Genetics in History (LR5)	Ecology Transformed (LR3)	Biology and 'Naturalness' in Organic Agriculture (LR4)

Friday	9:00 - 10:30am	11:00 - 12:30pm	2:00 - 3:30pm	4:00 - 5:30pm
Session I	Radiobiology in the Atomic Age (LR1)	Pearson, Fisher, and the Statistical Roots of Biology (LR7)	Biology and Anthropology I (LR8)	Biology and Anthropology II (LR8)
Session II	Evolution and Devolopment I (LR7)	Evolution and Devolopment (LR1)	The Mutual Shaping of Science and Science Education I (LR5)	The Mutual Shaping of Science and Science Education II (LR5)
Session III	Philosophy of Cognitive Science I (LR8)	Philosophy of Cognitive Science II (LR8)	Visual Zoology on Wall Charts (LR6)	Exhibiting Humans and Animals (LR7)
Session IV	Modes of Research in Biology (LR2)	Endangered Species, Threatened Paradigms (LR4)	Biology, Biotechnology and Policy (LR4)	Digital History of Biology (LR6)
Session V	Animal Communication (LR6)	Recent Work on Pluralism and Levels of Selection (LR6)	Evolution and Politics I (LR9)	Evolution and Politics II (LR9)
Session VI	Metaphor and Communication I (LR10)	Metaphor and Communication II (LR10)	Lorenz' Concept of Instinct (LR7)	Philosophy and Ecology (LR3)
Session VII	Genes, Genomes, and Genetic Elements I (LR9)	Genes, Genomes, and Genetic Elements II (LR9)	Life, Metaphysics, and Biosemiotics I (LR10)	Life, Metaphysics, and Biosemiotics II (LR10)
Session VIII	Functions and Teleological Explanation I (LR3)	Functions and Teleological Explanation II (LR3)	Developmental Regulation I (LR1)	Developmental Regulation II (LR1)
Session IX	Biology and Ethics/Epistemology I (LR5)	Biology and Ethics/Epistemology II (LR5)	Fitness, Drift, Evolutionary Theory (LR2)	Science in Social Context (LR4)
Session X		18th-19th Century Biology I (LR2)	18th-19th Century Biology II (LR3)	Metascience from a Biological Point of View (LR2)

Saturday	9:00 - 10:30am	11:00 - 12:30pm	2:00 - 3:30pm
Session I	Issues in Sociocultural Evolution I (LR1)	Issues in Sociocultural Evolution II (LR1)	The Romantic Conception of Life (LR6)
Session II	Biology of Human Behavior I (LR2)	Biology of Human Behavior II (LR2)	Ecology and Environmental Values (LR8)
Session III	Challenging the Essentialist Story about the History of Taxonomy I (LR6)	Challenging the Essentialist Story about the History of Taxonomy II (LR6)	Inheritance and Evolution (LR7)
Session IV	Indeterminism and Evolution (LR7)	Perspectives on Population I (LR9)	Perspectives on Population II (LR9)
Session V	European Roots of Evolutionary Psychology (LR8)	Microbial Challenges (LR3)	Naples as an Evolutionary Niche (LR5)
Session VI	Biology and Education I (LR4)	Biology and Education II (LR8)	Beyond Lorenz (LR1)
Session VII	Trajectories of Drugs (LR3)	Bioinformatics and the Transformation of Biomedical Research (LR7)	Sex, Gender, and Immunology (LR2)
Session VIII	Topographies of Knowledge Production in Aquatic Ecology I (LR5)	Topographies of Knowledge Production in Aquatic Ecology II (LR5)	Emergence and Mechanismsi n Biochemical Networks (LR3)
Session IX	William Bateson and the Suppression of Epigenetic Biology (LR9)	Biology and Metaphysics I (LR10)	Biology and Metaphysics II (LR10)
Session X	Biosemiotics (LR10)		

Sunday	**9:00 - 10:30am**	**11:00 - 12:30pm**
Session I	Darwin (LR1)	Boundaries in Biomedicine (LR1)
Session II	Biohistory—Neo-Darwinism's Last Frontier (LR2)	Scientific Change from a Biological Point of View (LR2)
Session III	Alternative Evolutionary Theories I (LR5)	Alternative Evolutionary Theories II (LR5)
Session IV	Autonomy: A Key Concept I (LR6)	Autonomy: A Key Concept II (LR6)
Session V	'Race' in Theories of Human Origin and Diversity (LR7)	Is There a Kantian Tradition in Biology? (LR9)
Session VI	Science and Policy: International Perspectives I (LR3)	Science and Policy: International Perspectives II (LR3)
Session VII	Dimensions of Genomics (LR9)	Between Gender, Politics, and Biology (LR7)
Session VIII	Biology and Meaning I (LR8)	Biology and Meaning II (LR8)
Session IX	Biological Hierarchies I (LR10)	Biological Hierarchies II (LR10)