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

MEETINGS AT LEUVEN, BELGIUM, 19 - 23 JULY, 1995

PROCEEDINGS

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Please note: the index of names is organized by first names.

WEDNESDAY, JULY 19 16:00

Registration opens at the Welcome Desk in Pope's College; Meet friends and colleagues at the Pope's College bar.

WEDNESDAY, JULY 19 20:30 - 22:00

Opening Reception

THURSDAY, JULY 20 8:30 - 10:00

Post-WWII Biomedicine. I: Viruses, Molecules and Disease. 1

Organized by Jean-Paul Gaudillière (INSERM, Paris)
Chair: Thomas Söderqvist (Roskilde University

Relations between the laboratory and the clinic have been radically altered since World War II by a rapidly growing biomedical research enterprise. A large biomedical complex linking state research agencies, pharmaceutical firms, medical schools and teaching hospitals emerged simultaneously with the transformation of the clinical and research agenda as evidenced by the systematic use of antibiotics as well as technological innovations such as the ultracentrifuge and the electron microscope. Medical frontiers shifted from diagnosis and treatment of diseases caused by bacteria - increasingly perceived as routine work by many biomedical researchers - to cancer and viral infections like polio and influenza. Part I of the session will explore changes in research practices resulting from an increasing interest in the pathological role of macromolecules and viruses. Case studies will cover the entire postwar period, ranging from early episodes, e.g. the work on vaccines against the polio virus in the 1950's, to more recent developments, such as the debates over the role of retroviruses in the causation of cancer and AIDS.

Blood Feuds: Chemistry, Physiology, and Medicine in a Controversy Between L.J. Henderson and Y. Henderson.

Olga Amsterdamska (University of Amsterdam)

The publication of Lawrence J. Henderson's 1928 Silliman Lecture on Blood in which Henderson reported the results of his and his collaborators' studies on blood as a physico-chemical system led to a brief but intense controversy involving a number of prominent American and British physiological chemists, among others Yandell Henderson of Yale, J.S. Haldane, and Donald Dexter Van Slyke. The controversy touched on a variety of issues, including such matters as proper citation parctices and style of writing, but most significantly it brought to the fore profound differences among competing conceptions of biochemistry as a discipline. These "philosophical" differences were associated not only with different evaluations of distinct research styles prevalent in interwar biochemical research, but also with views about relations between biochemistry and other disciplines and areas of practice, such as physiology, physical chemistry, and clinical medicine. Both Yandell Henderson and Haldane attacked as overly reductionsitic, mechanistic and "metaphysical" L. Henderson's assumption that blood should be studied primarily with the methods and in terms of physical chemistry. L. Henderson's defenders extolled the virtues of mathematical and chemical analysis, and insisted on the progress in the understanding of both normal and pathological processes that could be achieved with the help of physical chemistry.

On the basis of both published texts and the correspondance between the major actors of the controversy, the paper examines the differences in the conceptions of biochemistry as a discipline and their implications for the manner in which biochemical knowledge and techniques were to be applied to clinical medicine. The paper argues also that the difference kinds of reductionism mebrace dby the two

camps involved in the controversy were closely associated with different modes of understanding diseases.

Between the Clinic and the Laboratory: Haemoglobin and its Pathologies.

Soraya de Chadarevian (University of Cambridge)

In 1949, Pauling heralded sickle cell anemia as the first molecular disease. In the following years sickle cell heamoglobin and other "abnormal heamoglobins" became common laboratory tools. In this paper I will focus on Max Perutz's and Vernon Ingram's structural, chemical and genetical work on haemoglobin at the Medical Research Council Unit for Molecular Biology in Cambridge and on the role of Hermann Lehmann and his reference collection of abnormal heamoglobins as exemples of the circulation of tools and practices betwenn the clinic and the laboratory.

I will argue that the use of haemoglobin as a research tool and the clinical knowledge of its pathologies were crucial in the development of research strategies to determine structure-function relationships of proteins and nucleic acids which came to define molecular biology in post-war Britain. In turn, the adoption of new laboratory techniques changed the way in which clinical pathologies were defined and classified. This facilitated the exchanges of tools and knowledge between the clinic and the laboratory, most often, however, to the more immediate advantage of the research workers than of the patients.

Wendell Stanley and the Polio Virus.

Angela Creager (Princeton University)

When Wendell Stanley was recruited to the University of California at Bekerley in 1946 to establish a department of biochemistry and a first-rate Virus Laboratory, the state of California funded the institute on the basis of Stanley's claims that it would provide practical benefits on the struggle against viral diseases threateninig both state agribusiness and public health. Stanley also obtained funding from the National Foundation for Infantile Paralysis to investigate poliomyelitis virus. His coworkers contributed to knowledge about the structure of the virus throughout the 1950s, cristallizing the virus in 1957. However, polio researchers in the Virus Lab failed to garner credit for victory over the epidemic disease, particularly given the public reception of Salk vaccine in 1955.

The paper will focus on Stanley's attempt to lay claim to the polio virus, and will reflect on problems of his Virus Lab's mission to do both basic and clinical research in the charged political atmosphere surrounding polio research after World War II.

Adaptation and Optimality: The Meaning of History

Organized by Alirio Rosales (Instituto Internacional de Estudios Avanzados, Caracas)
Chair: Paul Griffiths (University of Otago)

The publication of the paper "Optimality Models and the Test of Adaptationism", by Steven Orzack and Elliott Sober (*American Naturalist*, 143 (3): 361-380, 1994), introduces fresh air into the long standing controversy over optimality and adaptation. In formulating what they hold to be the central statement of the optimality approach to explaining adaptation, they present a position which understands optimality as an evolutionary state of local optimality in a given current population. When tested in the proper quantitative way, such a state provides grounds for mantaining that natural selection is a sufficient explanation for the evolution of the trait which is at the state of local optimality. Orzack and Sober dilucidate the structure of a test for optimality models that is at the same time, a test for adaptationism. The goal of this session will be to discuss whether the claim of local optimality can be really linked to natural selection and to see if the soundness of Orzack and Sober's approach requires an ahistorical concept of adaptation. This considerations should shed light in the issue of equilibrium vs. dynamic models of adaptation and the role of history.

Whither Optimality? Process Approaches to Testing Adaptationism.

Robert Brandon (Duke University)

In a recent paper Steve Orzack and Elliot Sober offer a statement of adaptationism that they consider precise enough to be tested, and a method of testing the statement. Taking this as a starting point, I will argue that it is crucial to distinguish claims of optimality from claims about the historical processes that drove an evolutionary trajectory to its present state. On the one hand, even when natural selection is the sole evolutionary factor optimality is not ensured; and on the other hand observed optimality is no guarantee that natural selection has been the sole, to major, driving factor resulting in that observed state. I argue that adaptationism as a general thesis is most profitably thought of as a hypotheseis concerning the historical mechanisms that led to the state to be explained. Once so conceived, I argue that optimality models play little to no role in the testing of adaptationism.

The thesis os adaptationism and whether, and how, it can be tested are issues of considerable importance in the philosophy of evolutionary biology. But in discussing these issues some more general issues in the philosophy of science will arise that will receive some attention. In particular, I think this case illustrates some interesting, and perhaps unexpected, facets of testing statistical generalizations. Are statistical generalizations best tested "inductively", i.e., by looking at a number of individual instances of them? Are the most definitive tests of such hypotheses those that search cases deemed most likely to make them false? How, in a practical sense, can one deal with cases, such as the one at issue here, where an infinite number of models can describe an observed state or trajectory? I will offer answers to each of these questions as they apply to the thesis of adaptationism.

Optimality and Ecological Understanding.

Gregory Cooper (Duke University)

A good deal of the philosophical mystery surrounding optimality modeling has been removed. We have a much clearer picture of the structure and empirical assumptions of this kind of explanatory strategy. Optimality models attempt to find traits to stand in for fitness, and to show, given the ecological circumstances, that these traits occur in their fitness maximizing form. In the process, they make certain

presuppositions about heritable variation, epigenetic processes, historical contingency, and the like. Of course there is still the healthy internal controversy over what kinds of model to build, and there is substantial debate about the extent to which empirical presupposition are satisfied. But the overall nature of the enterprise is fairly clear.

There is less clarity, however, concerning the conceptual role (or roles) of optimality modeling. Why would someone want to embark on an optimality analysis in the first place? I think the usual presupposition is that the project is one of understanding why organisms have the properties they do, and the optimality argument is intended to establish the efficacy of natural selection in the causal history leading up to the trait in question. There is no doubt that this often is the project. But to suppose that this is always so is to miss another important role for what Mac Arthur used to call "selection thinking". Population and community ecologists are interested in patterns of distribution and abundance, and in the structure of ecological communities. Optimality analyses can play a role here as well, and the existence of a Darwinian history involving natural selection is more of a presupposition than an object of inference. This explains the frequently encountered, and otherwise perplexing, assertion that the optimality of organisms is an assumption, not a successful optimality analysis, even if laden with historical assumptions, can boost confidence in the ecological account as well as provide a more mechanistic understanding of the ecological processes at work.

This paper briefly surveys the emerging consensus about the nature of optimality modeling and argues that much of the controversy that remains traces back to a failure to appreciate the conceptual roles that optimality arguments play. In particular, it argues against the assumption-- which seems to pervade methodological critiques-- that the ultimate point behind optimality analyses is always to test some general adaptaionist thesis by establishing historical claims about the evolutionary trajectory leading up to some trait. The role of optimality modeling in ecological inquiry is developed as an alternative. Finally, since success depends on what one sets out to accomplish in the first place, the paper argues that methodological analyses must be sensitive to the different roles that optimality studies can play.

Are Optimality Models Ahistorical?

Alirio Rosales (Instituto Internacional de Estudios Avanzados, Caracas)

I reconstruct the general objection to optimality models (GOOM) as an objection to the historical attainability of an optimal state. This includes constraints due to genetic correlations, developmental constraints, and philogenetic history. The crucial point is that (GOOM) works under a construal of natural selection as a force of evolutionary change that produces adaptation. This is the prevailing conception of a natural selection-explanation (NS-E), which I will distinguish as NS-E (1). Since (OM) are understood here as basis for NS-E's, the question will be raised as to whether (OM) provide the basis for NS-E (1). This will lead to a reconsideration of the relation between adaptation, optimality, and natural selection. In particular, I argue that for that relation to hold explanatory, we need to determine the time horizon within which a given population is said to remain at ESS. It turns out that optimality cannot characterize the evolutionary process in general. OM apply only in episodes of phenotypic evolution in which evolutionary and ecological dynamics are causally coupled. OM work with a different conception of natural selection as maintaining adaptation, which will be distinguished as NS-E(2). OM are not ahistorical but they are looking at a partial dimension of evolutionary history of a trait in which ecological processes and interactions can equilibrate

different evolutionary dynamics. NS-E(2) are supposed to be compatible with NS-E(1), but that is largely not the case. Recent work, however has assessed the problem and I discuses some of their conclusions.

Quality of Risk-Assessment in Biotechnology: Theory, Practice and Politics of Deliberate Release. I. Theory

Organized by Ad van Dommelen (Vrije Universiteit, Amsterdam)
Chair: Ad van Dommelen (Vrije Universiteit, Amsterdam)

The issue of risk-assessment in biotechnology is fraught with problems of general methodology. Some important questions are: What can be done from a philosophy of science perspective to improve upon the scientific quality of risk-assessment? What can be done to improve the quality of debates about risk-assessment? What are sensible ways to deal with a lack of knowledge or with scientific uncertainty? What is a useful way of dealing with background theories from ecology, evolutionary biology and/or genetics?

The Role of Standardized Microcosms in the Risk Assessment of Genetically Engineered Microorganisms: From Genetic Reductionism to Ecological Modelling.

Sheldon Krimsky (Tufts University)

Two modes of thinking have framed the discussion of risks associated with the release of genetically engineered organisms (GEOs) into the environment. The genetic reductionist framework places greatest weight on understanding the foreign genes and the phenotypic characteristics of the host organism. The ecological framework is site specific and requires field testing of the GEO under the conditions of application. The former assumes a predictive theory between the genetic sphere and the ecological sphere, while the latter offers little credence to predictive risk assessment of GEOs and must accept the dismal conclusion that every ecological assessment experiment places the environment at risk.

The use of microcosms to test GEOs prior to release has been proposed as a bridge between the dogmatic genetic reductionists and advocates that claim that risk analysis begins and ends in the field. An intermediate step of microcosm analysis has been proposed that could provide reliable and useful information about the survival, competitiveness and dispersal of genetically modified microorganisms released in the soil. The paper examines the possibilities and limitations of microcosms, tradeoffs in microcosm structure between modeling actual conditions and gaining replicable and dependable results, and the problems of standardization.

Reductionist or Integrationist Biology: Who Will Define True Science and What Difference Will It Make?

Philip J. Regal (University of Minnesota)

Risk assessment policies and project evaluations in biotechnology are often only superficially based on the full range of available scientific knowledge. Regulators may not distinguish which parts of

their scientific reasoning are in fact distinguish which parts of their scientific reasoning are in fact abstract philosophy and which are empirically-based scientific knowledge.

Underlying philosophical concepts that have informed risk assessments in the belief that they are 'true scientific thinking' have included reductionism, idealism, essentialism, and even utilitarianism, and consequential ethics.

Idealistic and essentialistic models are used to argue that transgenic organisms will generically be safe, and reductionism to ignore the sciences that study higher levels of biological organization, such as modern ecology, population genetics, developmental biology, organismal physiology.

Philosophers and historians of science could improve risk assessment by identifying judgments based on philosophy rather than on mechanistic insights, and by clarifying what constitutes sound judgment in pressing, practical matters.

The biotechnology community claims to agree to safety regulations based on 'true science.' But who gets to define 'true science'? Should it be the academic community, scientists at large, or those who control science policy and funding?

Epistemic Discourse and Risk Assessment.

Rene von Schomberg (Tilburg University, The Netherlands)

Controversies in science go beyond the truth of statements by discussing the plausibility of knowledge-claims. Within an epistemic debate, which will be explained, we can only expect a reasonable dissent but not a reasonable consent. The conflicting knowledge claims of the experts constitute epistemic uncertainty. Decisions within the field of policy making realised against the background of such discussions will, therefore, be subjected to these conditions of uncertainty. I will start with a discussion of the structure of epistemic discussions. This I will do with the help of an example: the risks of the deliberate release of genetically engineered organisms. This explication will be followed arise in the context of scientific controversies. It can be shown that these problems can not be adequately dealt with in the usual policy procedures. I will try to show how discursive procedures of Risk Assessment could elaborate these problems.

Biology as Ideology and as Policy

Contributed papers
Chair: TBA

IQ Deja Vu: Three Phases of Hereditarianism, Nativism, and Deprivilegization.

Leonard Lieberman (Central Michigan University)

The Bell Curve represents the third major attempt in the 20th century to use IQ tests as a weapon to stigmatize immigrants, African-Americans, and/or other depriveleged groups with hereditary inferiority and to define them as consequently undeserving of environmental assistance. The first period in the early decades of the century involved eugenics and anti-immigrant legislation. In the late 1960's Jensen and

others used IQ tests to stimulate a backlash against the war on poverty and civil rights legislation. In the third phase in the 1990's Herrnstein, Murray, Rushton, and others once again brandished IQ scores as proof for their hereditarian position. Each of these phases occurred in reaction to the social context. Each time intellectuals responded to the hereditarian arguments with intense criticism and neutralized them for those willing to listen. The critique of the third phase is currently underway. These developments are reviewed with attention to some neglected but potent studies that effectively refute the basic assumptions of the new hereditarians. The viewpoint of the paper draws upon biological anthropology and the sociology of science.

Interpretations of Darwin's Theory of Evolution: How Theory of Evolution can be Brought to Correspond to Different Political Ideologies.

Kerstin Berminge
Department of Theory of Science and Research (Göteborg University)

Darwin's theory of evolution has been used in order to legitimate a liberal political system, so-called Social Darwinism. How classical liberalism was used by Herbert Spencer has been thoroughly investigated and discussed, and mostly it is this variety of politically influenced Darwinism we associate with the concept of 'social darwinism'. It is not so well known however, that ethologist and Nobel Prize winner Konrad Lorenz constructed a behavioural theory using and interpreting evolutionary theory in a way which fits in perfectly with Burkean conservatism, and also fascism. My aim in this paper is to demonstrate how well his theory regarding animal and human behaviour corresponds to Burke's ideology, and also to some extent to nazism and how his interpretation of theory of evolution differs from that used to legitimate liberal Social Darwinism

Biology as a Mechanism of Popular Education in the Third Reich.

Anne Bäumer-Schleinkofer (Johannes Gutenberg-Universität Mainz)

Biology gained enormous prestige in the Third Reich because it could be used to give the Nazi world view a pseudoscientific veneer. It was used to demonstrate the incontrovertible truth of Nazi plans and propaganda. The central issue of biological teaching became the concept of race studies originally developed by the medical profession. Biological education further aimed to inculcate the pupils with a love for their own home region and for nature and to train them to take a holistic view of the natural world. Two theses were basic to all curricula and directives on the teaching of biology: "national socialist thought must be biological thought" and "national socialism is politically applied biology". The Nazis were determined to anchor the political and ideological substance of bioldogy teaching in the hearts of young people, so that they would understand and support Nazi policies and laws. Teaching methods were altered to match these aims, indeed the Nazis proved to be as expert pedagogues as they were demagogues!

Biology and Religious Thought

Contributed papers
Chair: TBA

Human Thought Beyond the Natural Sciences.

Hai Kui Huang (Kunming Municipal Institute of Environmental Science, Kunming, Yunnan, PRC)

Two different methods of human thought are developed along with the evolution of the information system. One is the scientific thought, the other is the religious thought. The ecological crisis arose today because of the finitude of natural sciences. On the other hand, the religion have made a great progress in recognition of relationship between human and nature because of its universal quality, especially Chinese Taoism and Medicine. They have a long history, and not only have a great influence on astronomy, geography, chemistry, science of human body, prediction of events at future and Chinese culture but also make an important contribution to interaction between human and nature as well as to subconsciousness of human being. After combining religion with natural science the ecological problems can perhaps be recognized renewedly and a new philosophical way can be found to resolve these problems.

Evolutionary Ethics, Christian Ethics.

Patricia A. Williams (Virginia State University, Richmond)

According to evolutionary ethics, people naturally love themselves, their kin, and their friends. Christian ethics requires people to love their neighbor without regard to kinship or friendship, or, at least, beyond kinship and friendship. The gap between these two positions has been known in Christianity as "original sin," the disposition to love oneself instead of God and neighbor. Thus, evolutionary ethics offers support for the theological idea of original sin. There are implications for the doctrines of incarnation and atonement.

The True Spirit of Physico-theology: Gilbert White's *Natural Theology of Selborne*.

Nigel Cooper (Diocese of Chelmsford, Church of England)

Gilbert White (1720-1793) was a Church of England parson and conscientious pastor as well as a naturalist. He supported his natural history enquiries by references to the work of God in creation. He was particularly influenced by John Ray and William Derham's physico-theology, despite some of its weaknesses that later became evident. He was deeply influenced by the literary pastoral tradition that looked back to Virgil's Georgics for inspiration. This is associated with his approach to gardening which demonstrates his relationship to nature. The enduring strength of his book, *The Natural History of Selborne*, is explained by Gilbert White's quality of vision. This may not be his explicit theology, but it is his spiritual legacy to us.

Philosophy and Evolution I

Contributed papers
Chair: TBA

Recapitulationism in 20th Century Embryology.

Dominic Lewin (University of Leeds)

The changing fortunes of recapitulationism are discussed apropos the role of embrylogy in mid-twentieth century evolution theory. S. J. Gould finds support for the late nineteenth century's "unanimously upheld principle of recapitulation" (1977) within modern genetic and morphological theories. Rasmussen suggests a variety of non-Lamarkian supports for recapitulationism which itself "did not become substantially less tenable during the [early twentieth century] period of its decline" (1991). Finally Ernst Mayr tells us recapitulationisms's acceptance "documents the maturation of biology," (1994) and looks to developmental induction processes to provide support for a "somatic programme" theory in the spirit of Garstang's critique. We shall assert the historical and philosophical significance to neo-recapituationism of the synthetic evolutionary theory of C. H. Wassington. Genetic assimiliation provided a powerful theoretical mechanism linking Morgan's genetics to developmental phenomena, such as double assurance and the transition from regulative to mosaic development. It accommodated ultimate and proximate causation within its mechanism of selection pressures toward endogenous triggering of developmental competencies. Waddington's neo-Lamarkian overtones may explain hostility to a thesis of real importance to evolutionary embryology.

Bertalanffy's Organismic Concept in its Historical Significance.

Sabine Brauckmann (University of Muenster)

My research subject is the Austrian biologist Ludwig von Bertalanffy (LvB) who has developed the organismic systems concept in Austria in the 30os completing the theory during his Rockefeller fellowship at the University of Chicago in 1937/38. According to my research he was the first biological scientist who defined the organism as an open system hierarchically organized in steady state. For LvB the most fundamental problem of modern biology is to determine the organic system lawfulness, and his own biological research was dedicated to this problem. The organismic theory implies a methodological independence and the system state of a highly organized living matter. In other words: the localization of organ constructing tissues is a so-called "Schichtungsvorgang" based on the physico-chemical system conditions of the living organism. LvB conclude that for these reasons a specific organic factor (directivness) must exist. I suggest that the postulate of a theory of biology which LvB has tried to establish is still a desiderat for modern biology. I know that these few statements on such an elaborated theory could not satisfy the reader, but I suggest that for this letter a shortening of my research is necessary.

From Huxley to Gould: The Role of Personality and Popularization in the Debates Over Evolutionary Theory.

Sherrie L. Lyons (Daemen College, Amherst, NY)

The history of science looks very different from the founding days of George Sarton. We are continually reminded we must situate our scientific stories in their proper historical, social, political and economic contexts. Although many of us remain highly critical of social constructionist accounts, all of us would agree that science does not occur in a vacuum. I think that in the long term the "facts" exert their influence, i.e. blending inheritance, De Vriesian mutation, Lamarckism are shown to be untenable. The weight of scientific evidence is simply against these theories. However, in the short term this is not necessarily the case. Theories gain acceptance for a variety of complex reasons. Why theories enjoy the popularity they do at a particular point in history is an extremely difficult question to answer. Thomas Huxley warned Darwin on the eve of the publication of The Origin: "You have loaded yourself within an unnecessary difficulty in adopting natura non facit saltum so unreservedly." Over one hundred years later, Stephen Gould and Niles Eldredge began their 1977 article on punctuated equilibria with Huxley's warning. The problem of saltation vs. gradualism has been a recurring theme in the development of evolutionary theory. Why theories enjoy the popularity they do at a particular point in history is a very difficult question to answer. But certainly, science cannot be separated from the scientists who practice it. Style is inextricably linked to content; how something is presented and the personality of the presenter has a great deal to do with how it is accepted. In this respect some interesting parallels can be drawn between Thomas Huxley and Stephen Gould. In bringing Darwin's theory to the general public with his highly polemical style, Huxley influenced the scientific debates in ways that were difficult then if he had confined himself to more formal scientific writings. In a similar vein, one should not underestimate Gould's charismatic style and his prolific writing for the general public as well as the scientific community in trying to understand the attention punctuated equilibria has received.

While there have been numerous commentaries on the impact of science on the popular culture, this paper seeks to examine how that popularization then creeps back into the scientific community and influences the discussion. In particular, it will explore the role that the personality of two particular scientists played in the debates over evolutionary theory occurring within the scientific community. This adds yet another element to our understanding of the nature of scientific practice.

Goethe, Art Nouveau, and Cytoarchitecture: Historical Aspects of Biological Form

Organized by Robert Hendrick (St. John's University)
Chair: Robert Hendrick (St. John's University)

The participants in this session will examine biological form from three different historical viewpoints. John Jungck's presentation deals with the importance of mathematical metaphors in the development of concepts about the cell's internal milieu. He shows how such metaphors led to fundamental changes in the way in which the material of the cell was understood. Robert Hendrick's paper examines the use of biological forms in Art Nouveau, focusing on the work and writings of the artist, Emile Gallé. His main concern is to demonstrate how these images popularized biology with the French public while at the same

time they conveyed ideological messages. Maura Flannery presents the botanist Agnes Arber's views on plant morphology and how they relate to those of Goethe, whose botanical writings greatly influenced Arber. All three papers deal with how nineteenth-century views on form were influential in the development of biology and its relation to other aspects of the culture, specifically art, mathematics, and philosophy.

Mathematical Metaphors of Interior Milieu: From Protoplasm to Cytoarchitecture.

John Jungk (Beloit College)

(Abstract not supplied)

Emile Gallé and the Ecole de Nancy: Public Images of Biology in Fin-de-Siecle France.

Robert Hendrick (St. John's University)

Toward the end of the nineteenth century, the theoretical writings of French artists and aestheticians such as Hector Guimard, Eugène Grasset, and René Binet called for the extensive incorporation of biological forms into the art of the period, particularly art which would be on public display. They felt this would regenerate their own aesthetic sensibilities and provide the public with inspirational art forms. French Art Nouveau (1885- 1905) artists responded to these demands by adopting biological forms (especially botanical ones) to the decorative arts.

This paper focuses on the decorative arts (mainly glassware and furniture) and theoretical publications of Emile Gallé. Gallé, a trained botanist, was the leader of the Ecole de Nancy, the most important center of French Art Nouveau. The types of biological forms and images created for public viewing are examined, using both his work, which was world- famous, and his writings, especially his *Ecrits pour l'Art*. Also examined are the ideological messages embedded in these images, which served to do far more than simply popularize biology with the public.

Agnes Arber's Concept of Form: Plant Morphology in the Tradition of Goethe.

Maura C. Flannery (St. John's University)

Agnes Arber (1879-1960) was a noted botanist and only the third woman to be elected to the Royal Society. Her area of particular expertise was monocot morphology, but she was also interested in the importance of imagery in biological inquiry and in historical and philosophical issues relating to biology. She translated Goethe's works on botany into English and later wrote *The Natural Philosophy of Plant Form* which is a history of morphological concepts in botany. This paper will deal primarily with the views on form which Arber developed in this book where she discusses and then updates Goethe's view that all plant forms are variations on an archetypal leaf form. While he saw the type as an end in itself, she saw it as congruous with evolution, with forms related to a particular type by means of evolutionary descent.

THURSDAY, JULY 20 10:30 - 12:30

Research Programs at the Rouge-Cloître: From Chemical Embryology, Nucleic Acid Biochemistry and Cytochemistry to Molecular Biology. I

Organized by D. Thieffry (Université Libre de Bruxelles) and R. Burian (Virginia Polytechnic Institute and State University)
Chair: Maurice Errera (Université Libre de Bruxelles)

The purpose of these sessions is to examine the work on embryology, protein synthesis, and molecular biology at Rouge- Cloître, with emphasis on the programs of research initiated by Jean Brachet and Raymond Jeener. Although the occasion will serve partly to honor the work of the entire Rouge-Cloître group and the 90th birthday of Professor Jeener, the contributions to the symposium will not simply celebrate the accomplishments of the various research programs discussed as they look to us in hindsight. Rather, we seek to gain a broad understanding of how the problems appeared at the time they were undertaken, the ways in which Rouge-Cloître group was similar to and different from related laboratories elsewhere, the specific character of the working relationships there and the relationships with other groups and laboratories, and the ways in which the programs of work were transformed both by discussions and findings within the laboratory and in response to work done elsewhere.

The Role of Technique: Some Transformations Wrought by Use of RNAase and Staining Techniques, 1938-1952.

R. Burian (Virginia Polytechnic Institute and State University)

In 1952, Jean Brachet offered an account of protein synthesis with some affinity to Crick's "central dogma", enunciated publicly in 1958. This paper explores the importance (and limitations) of key techniques employed by Brachet and colleagues in their investigations of nucleic acids. Brachet's use of embryological and cytochemical techniques made his investigative pathway quite different than those of Watson and Crick, Delbruck and the phage school, Jacob and Monod, and classical biochemists such as Chargaff and Seymour Cohen. Time prevents exploration of these comparisons, but I will examine the importance of certain cytochemical techniques and findings in setting the problems that occupied Brachet and colleagues in their studies of nucleic acids. One reason for the rapid emergence of molecular genetics in the 1950s and 1960s is the robust concordance of results about nucleic acids obtained by use of radically different techniques in different laboratories. One reason for paying close attention to the work of such groups as that at Rouge-Cloître is the need to appreciate the specific contributions of different techniques to the robustness of new findings.

From Experimental to Molecular Embryology: the 'New' Embryology at the 'Rouge Cloître'.

H. Alexandre (Université de Mons-Hainaut, Belgium)

In the period 1930-40, Jean BRACHET was one of the few members of the young family of "Chemical Embryologists" who believed that morphogenesis cannot be understood without the help of biochemistry. At the time, the main exciting concepts of "causal embryology" (regulation, competence, determination, induction, organizer, evocation, morphogenetic gradients, ...) had already been proposed, but they all were poorly understood from a molecular point of view. In contrast to most of his colleagues, who were exclusively interested in energy production, intermediary metabolism, mechanisms of cellular oxidations, J. BRACHET cared about nucleic acids and protein synthesis, leading to the birth and the development of Molecular Biology at the "Rouge-Cloître".

RNA & Protein Synthesis: from Chemical Embryology to Molecular Biology.

B. Fantini (Institut Louis Jeantet d'Histoire de la Médecine, Genève, Switzerland)

In 1941, J. Brachet and T. Caspersson independently produced the hypothesis that RNA might play a significant role in protein synthesis. The study carried out by Caspersson and Schultz, using absorption measurements on the amount of nucleic acid in chromosomes, and the cytochemical observations executed by Brachet on the distribution of nucleic acids in the cell both suggested a link between ribonucleic acid and protein synthesis, but did not supply any biochemical evidence about the nature of this link. Any attempt to discuss the role of the nuclei acids in protein synthesis was difficult, owing to the scarcity of information about its mechanisms. Various hypotheses were produced in the following years in order to explain this link. The nucleic acids were in turn considered as a source of energy, a structural component of a respiratory subcellular structure or a catalytic agent. Another hypothesis, assuming the cell to be a colloidal matrix controlled by a cytoskeleton, considered nucleic acids as responsible for the spatial configuration determining protein specificity and producing the chemical differentiation of cells.

The most widely accepted view, however, considered nucleic acids as part of a self-replicating structure (ribonucleoprotein granules), that is, as "organised cytoplasmic particles" analogous to genes, plasmagenes, and viruses, and held them to be responsible for differentiation and cell metabolism. The nucleic acid was considered as necessary for the multiplication of those self- reproducing units.

The establishment of a link between the presence of nucleic acids and the protein synthesis has been often considered as an anticipation of Crick's "Central Dogma of Molecular Biology," as a correct hypothesis that needed only to be confirmed by facts. However, the clear distinction between the flow of information and the flow of energy and matter introduced by molecular biology produced a new "theoretical world" incompatible with the different hypotheses regarding protein synthesis proposed before 1953. Chemical embryology operated simultaneously with two systems of concepts, that of chemistry and that of morphology. Using this combination, chemical embryologists explored the possibility of rewriting cytology CHEMICALLY and explaining morphogenesis by means of chemical concepts (gradients, concentrations, reactions, migration of molecules). In contrast, molecular biology offered an explanation of the chemical behaviour of macromolecules within the cell applying the morphological concept of form to molecules, rewriting chemistry MORPHOLOGICALLY, and explaining protein synthesis and protein specificity as a transfer of information, produced by a programme encoded in the genome.

Darwin and 19th Century Philosophy of Science

Organized by David Hull (Northwestern University)
Chair: TBA

(Abstract not supplied)

Theory Construction and Theory Appraisal: Darwin and the Philosophers' Ideas of Evidence and Explanation, 1835 - 1842.

M. J. Hodge (University of Leeds)

During these years Darwin constructed many theories on many subjects. But his view was that one cluster of evidential and explanatory ideals was appropriate to his own and others' assessment of them all the cluster he took to be shared by Lyell and Herschel. However, Darwin did think his various theories differed in how fully they met the demands set by these ideals: natural selection did better than pangenesis, for example. In 1837 Whewell disagreed with the Herschel-Lyell line on verae causae (true causes) in general and in the appropriateness of theat ideal for geology (including theories about the origins of species) in particular. Darwin remained loyal to the Herschel-Lyell line. Darwin had debts to Whewell on several subjects, but in these years and hence in the structures and strategies of his public expositions later he continued to side with Herschel and Lyell against Whewell over the ideals definitive of good science.

John Stuart Mill's Influence on the Reception of Darwin's Theory of Evolution.

David Hull (Northwestern University)

No evidence exists that John Stuart Mill played any direct role in how Darwin constructed his theory of evolution. However, after he published it, Mill did seem to endorse Darwin's theory in his famous footnote to his *System of Logic.* Upon closer inspection, however, Mill's endorsement cut deeply into Darwin's own claims for his theory. And in a paper published posthumously, Mill rejected both evolution and natural selection. The effect of 19th century philosophers of science like Mill was to determine the ways in which Darwin's theory had to be transformed before it could be accepted.

Darwin and the Consilience of Inductions.

Michael Ruse (University of Guelph)

In this talk I discuss Darwin's relationship with the important British philosopher and historian of science, William Whewell, arguing that Darwin solved the veracausa principle problem by relying on a Whewellian consilience of induction.

Darwin as a Romantic Biologist.

Robert Richards (University of Chicago)

In a recent volume devoted to "Romanticism and the Sciences," the historian David Knight made the commonplace judgment that "Darwin's ... *Origin of Species* was not rooted in Romanticism but in the very different tradition of Paley and Thomas Malthus." The impact of Paley and Malthus on Darwin's thought does ride the surface of his concerns. But below that surface, and the easy historical judgments based thereon, resides another, much more profound cluster of ideas and attitudes that gave shape to the thought of the young Darwin and that formed the contours of his theory in the *Origin*. That set of conceptions derives from the Romantic movement in Germany, mediated for Darwin by Alexander von Humboldt and Richard Owen. I will attempt, in this talk, to show exactly how Darwin's theory displays the stamp of German Romantic thought and how the implications of the theory appear radically different when the theory's deeper structures are revealed.

Developmental Systems Theory and Extended Replicators

Organized by Paul Griffiths (University of Otago)
Chair: Susan Oyama (John Jay College, CUNY)

Developmental Systems theorists have argued that the special status of the genes in development, and hence evolution, cannot be sustained. There is no notion in of developmental information such that only genes carry developmental information concerning evolved traits. Such 'localisations' of information in a single developmental resource are entirely conventional. These theorists have argued that the real unit of evolution is a developmental system consisting of all the stably replicated resources that interact to construct the life-cycle of each generation. Adherents of the 'extended replicator' view accept the critical part of this position, but argue for a more conservative replacement of the gene-centered view. They retain the replicator/interactor distinction ,but as a pragmatic distinction driven by decisions in theory construction. Different parts of a single developmental system may be regarded as the replicator for different theoretical purposes. This session examines the relative strengths and weaknesses of these two views.

The Extended Replicator.

Kim Sterelny (Victoria University of Wellington)
Kelly Smith (Trenton State University)
Michael Dickison (Victoria University of Wellington)

Our purpose in this paper is to evaluate a conception of evolution in general, and the units of selection in particular, that has been articulated by a group we will refer to as Developmental Systems Theorists. First, we will contrast their "take" on evolution with the other perspectives. Then we will compare and contrast our views of replicators with theirs - in resisting thier view that genes play no distinctive informational role in inheritance, it becomes clear that though genes are special, they are not unique.

Lastly, we argue that despite it insights Developmental Systems Theory has serious problems. Moreover, their insights can be captured by a less radical take on the units of selection problem. We endorse the distinction between replication and interaction but think that previous authors have underestimated the range of biological replication. We speculate on this extended cast and suggest that Bateson's famous reductio of the replicator is no reductio at all.

Replicator II: Judgement Day.

Paul Griffiths (University of Otago) and Russell Gray (University of Wellington)

Developmental Systems theory says that organisms inherit all the resources which plays a role in the construction of evolved developmental outcomes. It is this developmental system that is replicated in each generation. The extended replicator view admits there is no ontological basis for the replicator/interactor distinction, but attempts to save it as a pragmatic device for theory construction. The extended replicator view argues that the full developmental systems view is too holistic to be manageable. This worry stems from a failure to reject some of the apparatus which accompanies the old strategy of localising developmental information in the genome, such as the pretence that the 'environment' is a stable and relatively unstructured background against which the genome evolves. The extended replicator view also loses one of the main insights of the developmentalist approach: the replication and evolution of relationships between elements of the developmental system is as important as the replication and evolution of the elements themselves.

Discussion.

James Griesemer (University of California, Davis)

Discussion.

Lenny Moss (Northwestern University)

Studies of Medicine

Contributed papers
Chair: TBA

History of Chagas Disease: Success Story in the Periphery.

Marilia Coutinho and Jair D. Miguel (Universidade de Sáo Paulo)

Chagas disease is a major endemic disease in Brazil and in other Latin American countries. Its discovery in 1909 by Carlos Chagas had a special significance in the context of the institutionalization of

Brazilian biomedical science. Given its social and epistemological peculiarities, the discovery itself has been disputed soon after international consecration. Considering that the discovery of Chagas disease has been the product and catalyst of the first outstanding expression of Brazilian science and the fact that again today it shelters clusters of "excellence" in this country's science, we have analyzed the course taken by the intellectual production related to Chagas disease from 1909 to 1979. The analysis of the growth tendencies of each type of product, together with qualitative data obtained from interviews with relevant characters, indicates that the modes of reproduction of the original community associated to the theme led to theoretical diversification and corresponding divergence between rising groups.

The Treatment Of Mental Disease: Between Pharmacotherapy and Psychotherapy

Lauro Galzigna (University of Padua)

The type of treatment that most of the mental ailments receive at present is a mix of neuropharmacology and psychoterapy. As a matter of fact, mental functions are the result of the activities of a brain machinery together with those due to the interaction between individuals and their external environment. We now know the molecular mechanism of action of several psychoactive drugs but have scarce understanding of the correlation between molecular events and mental function. As for psychotherapy we know that it may have beneficial effects on patients' behaviour but ignore whether this has any correlate at molecular level. A black box still exists between drugs, neurotransmitters, receptors, and the higher brain functions defined as anxiety, emotion, arousal, etc. Yet mental treatments imply a therapeutic method in which the blend of drugs and words administered to patients is only determined by the common sense of the therapist. In this epistemologically confused situation the pharmaceutical industry is playing a major role in orientating the medical profession towards the use of more and more powerful substances with very detailed molecular actions and plenty of side-effects. On the other hand the use of psychotropic drugs has allowed the abolishment of psychiatric hospitals and the "liberation" of millions of psychiatric patients. This beneficial effect is counterbalanced by the dependency of millions of individuals on psychotropic drugs. The situation leads to a number of questions relative to the possibl deterministic links or the missing links among words, molecules and behaviours.

Cells and Cell Theories in Biological Research

Organized by Anne Mylott (Indiana University)
Chair: Marsha Richmond (Wayne State University)

We will situate cell research in broader biological contexts. From Schleiden's investigations of fertilization in flowering plants, through Bennett's inquiries into the "molecular theory of life" relating nutrition, cell formation, and pathology, up to Sonneborn's use of *Paramecium* to understand not only cytoplasmic genetics but all of cell differentiation, we will look at some of the interactions between work with cells and general extra-cytological concerns of the researchers involved.

The Developmental Significance of the Nucleus in Schleiden's Cell Theory.

Anne Mylott (Indiana University)

The co-founder of the cell theory, Matthias Jacob Schleiden, was investigating fertilization and early development in flowering plants when Robert Brown visited Berlin and demonstrated his work on fecundation in Orchids and Aesclepiads, including his observation of the nucleus. I will examine Schleiden's 1838 "Contributions to Phytogenesis" to see how his developmental concerns may have shaped his reception of Brown's account of the nucleus. I will contrast Schleiden with his adversary in Berlin, Franz Julius Meyen, who took much less away from Brown's visit.

The Molecular Theory of Cell Structure: John Hughes Bennett's Ideas, 1845-75.

Jim Strick (Princeton University)

Bennett was one of the most popular and successful teachers of the mid-19th century Edinburgh Medical School. Following upon Schwann and Schleiden's cytoblastema theory, Bennett carried on research on the basic relation between nutrition and new cell formation and developed his "molecular theory of life", which was widely disseminated through his teaching and, int he early 1860s, through writings he published in the Lancet. Bennett attempted to explain healthy cell growth as well as pathology. His theory was attacked in 1863 by Lionel Beale, but it continued to be influential in British medical and bio logical circles, peaking in 1869. After that, Bennett's rapid decline in health and the involvement of his theory in the controversy over spontaneous generation led to a precipitous decline in its fortunes.

Cellular Heredity in an Age of 'Cyto-ignorance': Tracy M. Sonneborn and *Paramecium* as a Model Organism for Cytogenetics.

Judy Johns Schloegel (Indiana University)

While American geneticist Tracy M. Sonneborn (1905-1981) is well-known for his research on the cytoplasmic inheritance, it is more accurate to describe his research programme as focused upon the entirety of cellular heredity; throughout his career Sonneborn sought fundamental knowledge about the heredity of cellular and nuclear differentiation and the interactive influence of nuclear genes, cytoplasmic elements, and the environment upon cellular heredity. This paper will explore the biological role played by the ciliate protozoan, *Paramecium*, Sonneborn's primary research organism, in the development of an expansive research programme in cytogenetics. It will illumninate the important role of the co-constructive relationship between researcher and research organism for Sonneborn's mode of experimental practice and the production of biological knowledge by *Paramecium*. Ultimately, this paper will explicate the modest impact of protozoan genetics within a biological context which Sonneborn believed characterized by widespread "cyto-ignorance."

'Schwann's Project': The Constitution of Microscopical Causality

Ohad Parnes (Max-Planck-Institut für Wissenschaftsgeschichte, Berlin)

Schwann's work in Berlin in the years 1836-38 had to do mainly with three research themes: digestion, spontaneous generation and fermentation, and cell theory. These topics, often discussed by historians separately, could be considered in the context of one of the central scientific problems of the time, namely the difference between the organic and the inorganic, and the consequent epistemological and methodological implications of this distinction for science. Specifically, I would like to consider Schwann's work as an attempt to identify 'organic agents'. Although the term itself was not used as such by Schwann, it will be argued that his work on digestion, fermentation and the cell theory all involve an intentional use of an heuristic strategy, aimed at pointing out uniquely organic agents. This research method was to become paradigmatic for the newly developing science of microbiology. The demonstration of the causative role of microorganisms in disease is usually associated with 'Koch's Postulates', themselves related to the more loosely formulated 'Henle Postulates', and the relation between these postulates and Schwann's method will be discussed.

Schwann left Germany in 1839 for the University of Leuven, Belgium.

Discussion.

Marsha Richmond (Wayne State University)

Tools, Instruments, and Materials

Contributed papers
Chair: TBA

The Irradiated Mouse: Early Debates Over the Construction and Meaning of Radiation Chimeras.

Craig Stillwell (Michigan State University)

One line of inquiry in post-World War II radiobiological studies focussed on ways to protect organisms from the deleterious effects of total body irradiation. The 1949 observation that mice could recover from lethal exposure to x-rays by shielding certain hematopoietic organs and tissues (e.g., bone marrow and spleen) with lead was soon followed by the discovery that post-irradiation injections of tissue homogenates from unirradiated donors could also rescue lethally irradiated animals from dying of radiation sickness. The potential medical benefits of such a "treatment" provoked vigorous research to understand the mechanisms and limitations of recovery by intravenous injections of cells and homogenates. Two hypotheses accounting for the recovery phenomenon--humoral factors stimulating hematopoiesis and cellular "seeding" by the donor cells--competed in the early 1950s before a consensus was reached in 1956 in favor of the cellular hypothesis. I will examine this debate and how the mouse "radiation chimera" emerged out of radiobiology to become an important research tool in transplantation biology and immunology.

Redesigning the Internal Environment: Instrumental Tracing and Calorimetry as Techniques for Biophysical Representation and Experimental Enclosure.

Mark Hamel (University of Pennsylvania)

Experimental physiology in the early 20th century stood at the crossroads of several research traditions. Techniques of experimental aperture such as fistulae and forms of vivisection had required skill-intensive, almost artisanal facility with basic tools; by the turn of the century, various instruments for reading traces of organic functions (e.g., kymographs, x-ray devices) offered the prospect of studying bodily processes with (relatively) less interventive disturbance of the living organism. Investigators such as Bernard and later Cannon and Henderson saw physiological functioning as constitutive of an internal environment in self-regulating equilibrium, while also conceiving of the body's operations along the lines of mechanical engineering -- the digestion studies of Cannon or Alvarez, for instance. From a predominantly medical institutional context,physiological work came to occupy diverse niches and to take on a wider array of interests, including industrial hygiene and ergonomics as biomechanical engineering programs for reciprocally adjusting organisms to artificial built environments.

Calorimetry, with its thermodynamic model of metabolism and its techniques for measuring exchange values of matter and energy, developed from rather different traditions in 18th-19th century thermogenetic physics and chemistry compared to the medical milieu of digestion and assimilation studies. By the 20th century, however, researchers such as F. G. Benedict, Graham Lusk, and H. P. Armsby used enclosed-chamber calorimeters for medical and veterinary-agricultural work. HVAC engineers also relied on calorimetric studies to develop a science of design for human comfort and efficiency.

The Emptying of the Botanical Jar.

Kaat Schulte Fischedick (University of Utrecht)

For centuries the botanical jar (or collecting tin) functioned as a handy tool for plant collectors, to keep their collected specimens in. Moreover, it functioned as a group emblem for plant collectors, both for insiders and for outsiders. In the first half of the twentieth century the botanical jar gradually fell out of use.

In order to pinpoint changes in botanical practices in the first half of this century, I explore here the (uses of) botanical jar as a new research object. The hypothesis is that in this period the content of the botanical jar changed, both in the qualitative sense (different specimen) and in quantitative (less specimen). These changes are understood in the context of the rise of the notion of plant protection, in the context of developments in the plant sciences, and in the context of changes in scientific tools. Research data mainly considers the Dutch situation, though also material from the United Kingdom is included.

The Analytic Geometry of Genetics: The Evolution of Punnett Squares.

William C. Wimsatt (University of Chicago)

A square tabular array was introduced by R. C. Punnett in 1906 to visualize systematically and economically the combination of gametes to make genotypes. This mode of representation evolved rapidly

and became standardized as the canonical way of representing like problems. Its advantages over other contemporary methods are discussed, as are ways in which it evolved to increase its power and efficiency, and responded to changing theoretical perspectives. (It didn't much have to, which probably facilitated its spread.) Extensions to additional kinds of problems show its fertility and one of its main strengths a natural visual decomposition of a complex problem into a number of inter-related stages. This last feature also explains its computational and conceptual power, for it allows one to simply "read off" answers to a quite astounding variety of questions simply from the "right" visual representation of the problem--and to represent multiple problems, and multiple layers of problems in the same diagram. Comparisons are made with prior work on the evolution of Weismann diagrams by Griesemer and Wimsatt.

THURSDAY, JULY 20 14:00 - 15:30

Post-WWII Biomedicine. I: Viruses, Molecules and Disease. 2

Organized by Jean-Paul Gaudillière (INSERM, Paris)
Chair: Alfred I. Tauber (Boston University)

How to Become Human? Cancer Viruses in the 1960's.

Jean-Paul Gaudillière (INSERM, Paris)

In the late 1940s, Ludwig Gross, then director of the Cancer Research Unit at the Veterans Administration Hospital in New York, defined cancer in animals and humans as an epidemic disease caused by vertical transmission of viruses. Gross used three resources to support his vision: inbred strains of mice showing high incidence of tumors, acellular extracts which induced cancer when inocculated in suspectible mice, and electron microscopic pictures of tumor tissues showing particles of viral appearance. If the mouse tumors were to be trusted the eradication of cancer may become feasible by simple preventive methods including the production of vaccines and sera. One caveat in Gross' argument was the problematic translation of experimental cancer in mice into models of human diseases. By the mid 1960s, however, the search for tumor viruses in mice and humans had become a major research program at the (US) National Cancer Institute.

The paper examines this consensus regarding the "viral hypothesis" in cancer research. It focuses on the development of two dispositifs employed by virologists and clinicians to link laboratory work and clinical circumstances: the production of new animal viruses and the immunological screening of human tissues.

Styles of Practice in AIDS research.

Joan Fujimura (Stanford University)

I use a scientific controversy, and the efforts to legitimize and undermine a theory, to examine the co-production of facts and the rules for verifying facts over time. I discuss these processes in terms of what

I call "styles of scientific practice." In contrast to the focus of idealist philosophers on theory production and validation as forms of logic or ways of thinking, styles of practice also include the activities of hands and eyes and the discourses between multiple actors in diverse situations. I discuss aspects of the different styles of practice deployed by opponents in a current controversy surrounding the etiology of AIDS to understand how the same data are interpreted in different ways to support diametrically opposed views. This study describes and examines rules of confirmation used by supporters of the theory that HIV causes AIDS. For example, I introduce a "mosaic" style of practice used by AIDS researchers to synthesize information to understand this disease. Styles of practice stress the historically located collective efforts of scientists, technicians, administrators, institutions, and various "publics" as they build and sustain ways of knowing. Yet, I also show that the "history" is also a contested construction, not a given in dusty archives. I describe the different versions of history constructed by various participants in the debate to validate their current constructions and definitions of the disease AIDS. Finally, I discuss the politics behind disease definitions and the consequences of different definitions.

Studies on Cancer Viruses by Peyton Rous and C. H. Andrewes in the 1930s.

Ton van Helvoort (Beek, The Netherlands)

During the 1930s there was great interest in the possibility of a viral cause of cancer. In the U.S.A., pathologist Peyton Rous was a protagonist of the view that tumors might have a viral cause while virus worker Christopher H. Andrewes was an influential defender of a similar point of view in the U.K. The results of experiments on tumors in chicken (e.g., Rous sarcoma) and those in rabbits (e.g., tumors resulting form papillomas) presented many anomalies to the accepted view on the nature of the (filterable) viruses. The disappearance of the virus as an infectious entity being one of the findings most difficult to explain. The experimental findings led to such concepts as virus latency and virus masking or in Rous's words, the notion of "toothless viruses". Discussions on the nature of cancer viruses in the 1930s constitute the early history of the findings on retroviruses and oncogenic DNA-viruses during the '60s and '70s.

Complexity I

Organized by Sandra D. Mitchell (University of California, San Diego)
Chair: TBA

Biological systems constitute the paradigm for notions of complexity - in terms of compositional (part/whole) relations as well as in terms of dynamic (non-linear) processes. The presumption of complexity and its various structures have important implications for our view of the nature of scientific theories, methodological strategies and explanatory standards. This session would focus on recent developments in our understanding of complexity in biology and their philosophical implications.

Biological Complexity and Theoretical Pluralism.

Sandra D. Mitchell (University of California, San Diego)

Biological systems are taken to be the paradigm of complex systems. The understanding of complexity, however, is diverse. Biological complexity refers to organization, dynamical processes, and the domain of objects studied. Organization is the structure of part/whole relations where complexity is often associated with hierarchy. Complex processes are non-linear, and the evolved objects of biology constitute a diverse and variable set. In this paper it will be argued that aspects of biological complexity provide grounds for defending a view of theoretical pluralism which I call integrative pluralism. In particular, I will discuss three models of self-organization of the division of labor in social insects. This will allow us to see when and where these alternative theories and explanations are competing and when and where they are compatible.

Evaluating Simulations, Models and Instances of Complex Phenomena.

Brian Keeley (University of California, San Diego)

What are the criteria for success in Artificial Life (A-Life)? How would we know whether it had achieved its goals? "Weak A-Life" is the claim that the goal of A-Life is to create accurate models of complex, biological phenomena. "Strong A-Life" is the claim that the goal of A-Life is to create actual artificial instances of biological phenomena. The difference between the two characterizations is that between "merely simulating" life and actually creating it. This paper argues that the intuitive distinction drawn between an instance of a complex phenomenon and a simulation of same is not as clear as one would hope. Instead, there is a range of possibilities with no non-arbitrary and firm line to be drawn between them. However, evaluation (even in this continuum of cases) is still possible. I argue for the importance of what might otherwise be considered "implementational details" in the case of systems like Tom Ray's tierra (and the related avida system). In other cases, I argue that the ability of an artificial system to interact seamlessly with existing biological systems is an important criteria. (This later criteria is called the "Purring Test.")

Explaining Complexity: Explanation And Complexity In Self-Organizing Systems

Robert Richardson (University of Cincinatti)

There are two broadly different approaches to understanding scientific explanation. The first approach encompasses what is sometimes called a "causal," or "mechanical," approach to explanation (e.g., Railton 1978; Salmon 1984, 1989). The goal is to reveal the causes, or mechanisms, responsible for the phenomena we observe. Causal realism becomes the central goal of scientific explanations. The second approach emphasizes the virtues of unification (e.g., Friedman 1974; Kitcher 1981, 1989, 1993). The goal is to develop a system of laws capable of describing observed phenomena in the most economical way. Organization and systematic unification become the central goals of scientific explanation. These approaches may not be inconsistent, but they do pull in different directions.

Explanations of in terms of self-organization promise to give us explanations of observed order. Thus, Stuart Kaufmann (1993) claims that the problem for twenty-first century science is to explain "organized complexity," including ecosystems, communities, organisms, genetic regulatory systems, and neural systems. His exploration of the "origins of order" emphasizes that across disparate domains simple general principles suggest that there is a natural and spontaneous order in complex systems, that it is systems which are at the edge of chaos" that are most evolvable, and that selection maximizes this evolvability. These claims have very different credentials, and fit with our explanatory aspirations in very different ways.

Social Nature of Genetic Disease

Organized by David Magnus (University of Puget Sound, Tacoma)
Chair: Jane Maienschein (Arizona State University)

Advances in genetic technology have the potential to recreate significant aspects of our culture. Philosophers, scientists, and historians have worried about the implications of the expanding estimation of the genetic basis of various traits and diseases. The concept of genetic disease has expanded from paradigm cases such as Huntington's disease or sickle cell anemia, to diseases with any heritable component, to "diseases" such as alcoholism. The papers in this session offer critical evaluations of the reductionist tendencies involved in this expansion. Criticisms include an attack on the validity of claims to a genetic basis for complex behavioral traits, worries about the way that ignoring the social and psychological aspects of even the paradigm genetic diseases may hinder full understanding and treatment, and concern that gene therapies may hinder consideration of the most powerful (i.e. social) determinants of health.

Philosophical and Methodological Problems with Genetic Theories of Human Behavior.

Garland E. Allen (Washington University, St. Louis)

The main point of my paper is to analyze the flaws in arguing for a specific genetic component to any complex human behavioral trait. I will look at various areas of the reasoning process: definition of traits, methods of measuring or identifying individuals with specific behavioral phenotypes, experimental design (twin/adoption studies, family studies, blind/non-blind experiments, etc.), data analysis (especially heritability), conclusions drawn, and, finally, the whole philosophical concept bound up with the phrase "The gene(s) for . . ." In this part I will discuss a levels of organization approach, as opposed to the traditional (mechanistic) approach, to understanding what genes do (and do not) do.

The Psychological Complexity of Predictive DNA-Testing For Late-Onset Diseases.

G. Evers-Kiebooms (University of Leuven)

DNA-testing for late onset diseases such as Huntington's involves far more than the genetic techniques and information that makes such testing possible. There are numerous psychological and social factors which enter into family decision-making, the ethical implications of testing, and the full effects and experience of a late-onset genetic disease.

Gene Therapy and the Concept of Genetic Disease.

David Magnus (University of Puget Sound, Tacoma)

Recent developments in gene therapy have the potential to dramatically expand the concept of 'genetic disease.' On this view, a genetic disease includes any disease which can be treated by gene therapy. However, new gene therapies have been developed to treat diseases whether or not the disease in question has any heritable component at all. This enables reductionistically minded biomedical practitioners to side step a host of difficult biological and methodological issues. However, the reinforcement of our reductionist tendencies may actually be counter-productive to improved health. Several studies indicate that however wonderful medical modalities are at the individual level, they are a relatively insignificant factor in improving the health of populations. Broader social factors (which reductionism leads us to ignore) have a much more significant impact on the health of human populations.

Ethics and Policy

Contributed papers
Chair: TBA

Effects of Scientific Efforts, Christian Ethics, State Policy, and Public Values– Analyses of Two Extended Swedish Nineteenth-Century Families and a Serious Hereditary Disease.

Ulf Drugge (University of Umeå)

Serious hereditary diseases like Huntington's Disease(HD) lead to considerable social relational, economical, and cultural strains and are often causing reactions from different authorities like courts, public assistent committees, or church councils. Conceptions concerning relationships between inheritance and environmental influences are often present then. The way theoretical and practical considerations in the 19th-century were coordinated or counteracted with respect to scientific standards, political efforts, religious claims, and public opinions is studied. This interplay is analysed from reactions related to members in different generations of large-scale families suffering from HD. The unique Swedish Parish Archives are utilized and contain systematically collected Swedish citizen records, and are combined with historical data from other judicial, fiscal and administrative archives.

Bioethics, Government Policy and Legislation: the Case of the Flemish and European Biotechnology Policy.

Dirk Holemans (University of Ghent)

This paper reports of a two years research project on the ethical aspects of non-human biotechnology commissioned by the Flemish government. Whereas the research led up to the need for an open-ended well structured public debate, policy makers demanded the short-term development of a set of fixed ethical criteria. It will be argued that the research project served as a legitimation for continued heavy funding of biotechnology. An analogous situation seems to take place at the level of the European Union. Social research projects, a specific ethical committee and active social movements seem to have only little influence on the European biotech policy. This can be characterized by the term 'repressive tolerance' (H. Marcuse). Social groups are e.g. invited to meetings with a fixed agenda that have little influence on policy and legislation. In this paper the position will be defended of a 'strong democracy' (B. Barber): a participatory mode of democracy where decision making is defined as an ongoing open process, and where serious ethical concerns of the public are incorporated in legislation (H. Arendt). The Danish biotech policy seems to come the closet to this form of democracy.

Incompleteness of Explanations and an Ethics of Attention.

Cor van der Weele (Vrije Universiteit, Amsterdam)

Every scientific explanation is necessarily limited and specific, due to inevitable choices concerning subject definition, concepts and methods. Since scientific choices influence moral problem definitions and their possible solutions in wider contexts, an 'ethics of scientific attention', that is, a moral philosophy of science, is needed. Taking developmental biology as an example, I will argue that different approaches to development are not morally neutral in their consequences, and that environmental influences in ontogeny deserve more emphasis. The more general issue is: how to develop analytical moral debate in the philosophy of biology.

Instrumentation in Neuroscience

Contributed papers
Chair: Helen Longino (University of Minnesota)

'L'instrument terrible': The Role of the Galvanometer in Emil Du Bois-Reymond's Demonstration of Electricity in Nerve and Muscle.

Maria Trumpler (Yale University)

During the 1840s, German physiologist Emil Du Bois-Reymond was able to convince his peers as well as a special committee of the Paris Academie des Sciences, that he had demonstrated the existence of electrical current in nerve and muscle. The French academicians as well as subsequent historians have cited his development of a very sensitive galvanometer as the critical element of his success. My paper will critically examine the role of the galvanometer both in Du Bois-Reymond's own research and then later in his attempts to convince others of the veracity of his effects. Sources will include Du Bois-Reymond's

laboratory notebooks of his investigations, his dispute with Carlo Matteucci wh who challenged the reliability of his instrument, and the deliberations and conclusion of the committee of the Academie des Sciences charged with watching him perform his experiments and evaluating his claims.

Vision: The Career of Single-Cell Recording and its Critics.

Robert Olby (University of Pittsburgh)

This paper outlines the history of the practice of single-cell recording in the study of the neurophysiology of vision, identifying the significant changes which have been successively introduced in the method. The limitations of the method, the nature of the criticisms levelled against it, and the extent to which the method determines the nature of the results are discussed. This leads to an evaluation of the relative contributions to the development of our understanding of vision made by the method and by non-physiological alternatives.

Ethology in the Twentieth Century: Comparative Perspectives

Organized by Richard W. Burkhardt, Jr. (University of Illinois)
Chair: Gregg Mitman (University of Oklahoma)

The approach to animal behavior studies known as ethology was developed on the Continent prior to the Second World War, thanks in particular to the efforts of the Austrian naturalist Konrad Lorenz and the Dutch naturalist Niko Tinbergen. The post-war period saw ethology make major strides in its institutional development. At the same time, its conceptual foundations underwent certain important modifications. The standard treatments of ethology's development have paid little attention to how much ethology's development, both before and after the war, involved contingent choices made in local settings. The aim of this session is to lay the groundwork for the systematic study of the different ways in which animal behavior studies in general and ethology in particular developed in different countries and at different research centers.

Tinbergen in Oxford: the Adaptation of Continental Ethology to a New Setting.

Richard W. Burkhardt, Jr. (University of Illinois)

Niko Tinbergen's development of animal behavior studies at Oxford affords a special window for viewing and analysing the kinds of factors that distinguished the major ethological research programs of the post-war period. Formed in the tradition of Dutch field biology, embracing in the late1930s and 1940s the major conceptual features of Konrad Lorenz's understanding of behavioral causation, Tinbergen's own conception of "the aims and methods of ethology" continued to evolve after he left Leiden for Oxford in 1949. His concern with making continental ethology visible to Anglo-American biologists is well known. Here I will explore how the choices he made in setting up his Oxford program can be seen in the same behavioral-ecological terms he came to use in explaining the behavior of organisms, namely, as

compromises worked out in the face of selective pressures in particular settings. This paper identifies how Tinbergen's thought and practice evolved with respect to (1.) the particular material, intellectual, and institutional resources Oxford and Britain had to offer; (2.) dividing ethological problems with research programs elsewhere (including distancing his own work from Lorenz's); and (3.) his emerging vision of what the aims and methods of ethology ought to be.

A Quest for a Unified Framework? The Development of French Ethology After the Second World War: an Analysis of Institutional and Intellectual Factors.

Philippe Chavot (University of Strasbourg)

After World War II, numerous factors -- ideological, social and structural -- stood in the way of attempts to provide ethology in France with a coherent and unified framework. While the influential P.-P. Grassé wished to develop ethology as part of naturalistic zoology and insisted on linking ethological studies to French traditions of research, the expanding CNRS favored experimental sciences and gave psychophysiology substantial means to start transdiciplinary research in behavioral and neuro-physiological sciences. In the late 1960s, French ethology profited from the expansion of psychophysiology. As early as the 1970s, however, internal conflicts restrained the discipline's influence. Controversies took place over the development of international exchanges, the need to link French researches to the theories of Lorenz and Tinbergen, and the questions of Lorenz's behavior during the Third Reich and the conservative nature of his philosophical writings.

I examine in this paper the different trends of research in French animal behavior studies in the late 1940s and the steps taken to unify French ethology. I then analyze factors that led such attempts to fail, giving particular attention to personal, political and institutional factors. I conclude by comparing French and British scientists' attitudes regarding the internationalization process of ethology.

THURSDAY, JULY 20 16:00 - 17:30

Waiting for Monod: History of Biological Regulation I

Organized by Evelyn Fox Keller (MIT)
Chair: Evelyn Fox Keller (MIT)

We wish to propose a double session (3 hours) on the conceptual history of biological regulation: How was the phenomenon of regulation formulated in relation to the models and experimental practices of physiology, genetics, developmental biology, and biochemistry? What (if any) help was provided by the operative models of mathematical biology? And how did these formulations change over time? Our primary focus will be on the disciplinary cross-currants during the period leading up to the operon model of Monod and Jacob, and especially following the introduction of cybernetics.

Reframing Regulation from the Body to the Cell: Articulations of Biochemical Feedback in the 1950's.

Angela Creager (Princeton University)

In 1956 H. Edwin Umbarger introduced the term "feedback" into biochemistry, adapting its prevalent usage in describing the "internally regulated machine" to living systems. While Umbarger alluded to feedback patterns of regulation in the human body (such as homeostatic maintenance of temperature) he argued that regulatory mechanisms could be best explored in simple single-celled organisms, such as E. coli. His own experience of negative feedback was drawn from the bacterial biochemical feedback caught on widely in the late 1950's. I will argue that in studies of biological regulation the cell became a model for the body, a contention which is also seen in the contemporaneous visualization of "organelles" in cell biology. In addition, there was no clear separation, conceptually or experimentally, or genetic regulation from enzymatic regulation until the mid-1950's, and feedback inhibition served as a critical tool for dissecting apart gene from enzyme regulation.

Waiting for Monod: The Temporality of Gene Action: 1938-1958.

Sahotra Sarkar (McGill University)

The construction of the operon model by Monod and his collaborators around 1959 provided the first explicit account of gene action that took its temporal dimension into consideration. This paper explores how that problem was treated--or, more accurately, avoided--during the previous two decades. Three pairs of books by, Richard Goldschmidt ("Physiological Genetics" from 1938 and "Theoretical Genetics" from 1955), C.D. Darlington (the first (1939) and second (1958) editions of "Evolution of Genetic Systems") and C.H. Waddington ("Introduction to Modern Genetics" from 1939 and "The Strategy of Genes" in 1957), prove particularly useful to frame that period.

The former works were written as the relations of genes and cellular components were beginning to be experimentally eludicated. The latter were written after molecular biology had already received what may be called its canonical formulation. In between were many books that explored the biochemical frontier of genetics including J.B.S. Haldane's "Biochemical Genetics (1954). It is suggested that concern with the temporal dimension decreased during this period even though, in the late 1930's there were hints that genes and physiology would begin to receive a more unified treatment. Much of this paper is devoted in attempting to understand the reasons for this development, which helped make the new molecular biology a "static" field. Finally, it is suggested that what the operon model amounted to was a transfiguration of the problem space.

The Life Cycles: The Regulation of Intermediary Metabolism.

F. L. Holmes (Yale University)

The rapid rise to prominence of the problem of metabolic regulation during the 1950's has been associated with the influence on biochemical research of concepts of feedback control emanating from communications engineering and cybernetics. A well-defined story of their impact on studies of bacterial

growth, where they give rise to the idea of "feedback control by endproduct inhibition", has recently received historical attention. These events, however, form only one strand of a broader, more diffuse development. Regulatory problems were never far beneath the surface during the decades, from the 1920's to the 1950's, in which the metabolic pathways themselves were mapped out. In this paper I will sketch some of the other investigative strands which converged by the 1950's to elevate regulation to one of the central questions within the maturing subfeild of intermediary metabolism.

Complexity II

Organized by Sandra D. Mitchell (University of California, San Diego)

Chair: TBA

Dynamics, Thermodynamics and Biological Complexity.

David Depew (California State University, Fullerton)

Bruce Weber and I have argued that there is a distinctly American strain of genetic Darwinism which stresses (1) the large amount of variation preserved in natural populations; (2) the hierarchical structure of the living world, together with a corresponding multiplicity of units of selection; and (3) a possible gap between microevolutionary process and macroevolutionary patterns arising from analysis of the conditions of speciation. Recent advances in the study of complex systems promise some insight into why nature be structured in these three ways and why Darwinism must be expanded. Studies in complexity in biology have focused on synchronic and diachronic aspects. Weber and I stress diachronic aspects and explore the thermodynamical conditions for the emergence, development, and senescence of complex systems. This paper will explore these issues. In addition it will be argued that the success of semantic accounts of scientific theories in the hierarchical expansion of Darwinism can be consolidated by bringing the same view to the biological aspects of complex system dynamics. In that effort I will suggest that natural selection is a definitional component of certain kinds of systems: of the bounded, informed, autocatalytic ecologically dissipative systems that are poised between crystalline order and ergodic chaos.

Evolution as Repeated Assembly.

Linnda Caporael (Rensselaer)

Would a recasting of evolution meant to accommodate the human case also accommodate views of other parts of the biological world? Although evolutionary theorizing has become a richer and more complex, contrasts between innate and learned, nature and nurture, and biology and culture, persist in evolutionary approaches to humans despite appeals to interactionism. In this paper, I propose the term "repeated assembly" to capture recent ideas about evolution, including hierarchical organization with multiple levels of selection, emphases on entity-environment relations (rather than entity traits), and heterogeneity of resources--DNA, biotic, symbolic and technological– in relations potentially available to

selection. Components of repeated assemblies (which may be other assemblies) may differ in the reliability of their replication and their durability over different scales of time, including ontogenetic, cultural and evolutionary time scales. I illustrate repeated assemblies with an example of four group configurations repeatedly assembled for human behavioral coordination.

Generative Entrenchment and the Evolution of Complexity.

William C. Wimsatt (University of Chicago)

Evolution is widely associated with increases in complexity, despite frequent counterexamples: parasitism, fusion of bones in some evolutionary lineages, loss of the ability to synthesize compounds which become readily available, etc. But in most lineages most of the time, there are obvious continued increases in complexity however defined over time. I explore consequences of an explanation for much of this complexity a particular kind of irreversibility in the evolution of generative structures generative entrenchment (GE), which affects biological, psychological, and socio-cultural processes alike. (Compare Brian Arthur's "lock in" and "path dependence" in economics.) I elaborate work suggesting that GE has both allowed and generated increases by several orders of magnitude in the size of genotypes maintainable by selection over evolutionary time, and look at the role of 'exaptations' as the creative edge of GE processes in micro- and midi-evolutionary time scales. Finally, problems arise in defining what evolution would be like without GE processes seemingly necessary to evaluate their contribution to increases in complexity. While local comparisons seem meaningful, global comparisons break down so fundamentally that GE must be seen as essential to evolutionary processes, and to the evolution of any significant degree of adaptive complexity.

Studies of Ecology

Contributed papers
Chair: TBA

Ecology and Government Before World War II: the Case of the Kaibab Deer.

Christian C. Young (University of Minnesota)

This paper will explore the relationship between science and the United States government in the early 20th century, focusing on conservation, land use, and wildlife management. By considering the emerging profession of ecology, especially as practiced by federal agencies, a richer view of that relationship emerges. Textbooks of ecology highlight one case that demonstrates some essential aspects of this connection. In 1906, President Theodore Roosevelt established a national game refuge on the Kaibab Plateau on the North Rim of the Grand Canyon. The refuge became neither a hunting preserve nor a haven for wildlife. Rather, battles between the Forest Service, the National Park Service, the state of Arizona, and various conservation groups made management of the deer herd virtually impossible. With previously unexamined data from government archives, conflicts within the government and disagreements

among scientists emerge. This paper will describe the context of ecology and government in the Kaibab controversy.

The Relation between Holistic Philosophy and Behavioral Ecology in Japan.

Osamu Sakura (Yokohama National University)

The development of ecology and evolutionary biology in Japan showed both of differences and similarities to that in Western countries. Those both features were clearly demonstrated among ecologists-evolutionists before or just after the WW II. One of the most eminent ecologist- evolutionist in Japan in those days was Kinji Imanishi (1902-1991) who developed quite holistic and harmonious theories. His name is not so popular among Western countries, but his influences on the biologist community in Japan was very large until just recent. Thus, he is one of the "key-person" to understand the dynamics of Japanese biologist communities. I will introduce his evolutionary and ecological theories, and will compare those with C. A. Alee's holism. They seemed to share some ideas about the harmonious structure of the nature, while opposed their views on the animal society. I will also point out that Imanishi's extravagant holism, accompanied with anti- Darwinism, suppressed and retarded the acceptance of behavioral ecology by Japanese younger ecologists.

A Semantic View of Ecology.

David G.A. Castle (University of Guelph)

An examination of some recent criticisms of the scientific status of ecology shows that they reflect the so-called received view of science. Critics demand that ecological models must correspond to the objective world in order to have explanatory and predictive power. It is here that they find shortcomings in current ecology. I argue that the received view is the inappropriate conception for theory modeling in ecology. Following the work of J.Beatty, P. Thompson and E. Lloyd in evolutionary biology, and through an analysis of equilibrium models, I show that the semantic conception of ecology must displace the received view. I conclude by offering a brief semantic account of scientific ecology.

Studies in the History of Natural History I

Contributed papers
Chair: Peter Stevens (Harvard University)

Linnaeus' Theory of a Natural System.

Staffan Mueller-Wille (University of Bielefeld)

The concept of a "Natural System" implies, that a classification system reflects the order inherent to the realm of organisms and resulting from objective relations ("affinities") between organisms. To achieve this, those characters have to be selected that indicate affinity. This affords the existence of a biological theory grasping the specificity of organisms. I will show that Linnaeus had such a theory centering around the concept of sexuality and of organisms as machines. His theory of classification is reread against this theory, which reveals that Linnaeus actually designed and followed a method for the establishment of a Natural System, the main conceptual achievements behind this being: the concept of species as reproductive communities and the concept of genus types as character complexes shared by several species and constituted by physiologically necessary relations. These achievements are the result of the introduction of techniques aimed at the manipulation of plant reproduction into botanical practice.

Metschnikoff on Metazoan Origins: The Naples Connection.

Michael T. Ghiselin (California Academy of Sciences)
Christiane Groeben (Stazione Zoologica 'Anton Dohrn', Naples)

Elias Metschnikoff developed a highly influential "parenchymella" theory as an alternative to Ernst Haeckel's "gastraea" theory. While gathering data in support of his views he spent two periods in residence at the Zoological Station that had been founded by Anton Dohrn -- from March 17 to June 3, 1878, and from December 4, 1879 to May 19, 1880. During these periods he worked in an environment surrounded by persons likely to be quite sympathetic to his endeavor, including Dohrn himself, as well as visitors and staff. Metschnikoff seems to have been encouraged to develop his alternative scenario, which had a strong functional component, during his periods of residence. Crucial work on sponges and on intracellular digestion as an ancestral metazoan trait were done at Naples. Such facts illustrate the point that the Zoological Station was an important center of phylogenetical research and that Anton Dohrn was a more highly influential zoologist than has generally been realized.

The Narrative Field Plowed by Systematists: Biologists Using Their History.

Mary P. Winsor (University of Toronto)

There have always been biologists contributing to the history of biology, that is, writing or speaking about past events in their discipline; indeed until recently this was the only history of biology that existed. Professional commentators upon science, whether trained as historians, philosophers, or sociologists, are good at pointing out the various social functions history can serve within science, such as pedagogy and the rhetoric of legitimation. The idea that history may play a cognitive function within science, however, is a very different, and epistemologically radical, proposal. Joseph Rouse claims that the intelligibility of scientific knowledge depends upon a narrative context (narrative constantly undergoing change). As a case study to explore this idea, I look at systematics in the 1930s and 40s, a period when the rebuilding of Darwinism upon a genetic foundation forced taxonomists to review their status. Much was written on the history of systematics in those years. Are sociological concepts like legitimation adequate to describe the role of history, or is there evidence for the kind of cognitive role Rouse proposes

The Invention and Transformations of a Research School– Delft and Microbiology

Organized by Susan Spath (University of California, Berkeley)
Chair: Jim Strick (Princeton University)

Microbiologists, biochemists, and molecular biologists recognize the "Delft School" as an important research tradition in microbiology and biochemistry. As conceived by scientists, the "Delft School" was founded in the late nineteenth century by M.W. Beijerinck (1851-1931), when he became Professor of Microbiology at the leading technical university in the Netherlands. His successor, A.J. Kluyver (1888-1956) continued the tradition from the 1920's to 1950's. Kluyver's protege, C.B. van Niel (1897-1985), brought the "Delft School" program to the United States in 1929.

This session brings together two scholars from the Netherlands and two from the United States, who are studying the "Delft School" from different perspectives. Some of the questions we will address collectively are -- What purposes have been served by the concept of the "Delft School"? How and why has its meaning changed over time? What kinds of research has been carried out in the name of the "Delft School"?

Bert Theunissen argues that perceiving Beijerinck principally as the Delft School's founder obscures the breadth of his research interests and his intellectual debt to the Leiden tradition in botany. Theunissen situates Beijerinck's research in the context of a larger movement among Dutch scientists to define a role for pure science in society. Olga Amsterdamska analyzes Kluyver's research program and shows how he skillfully took advantage of local institutional circumstances to prepare students for careers in industry while advancing his theoretical agenda. Doris Zallen examines the successful collaboration of Kluyver with physicist L. Ornstein and his group at Utrecht. She argues that their research on photosynthesis in bacteria contributed to formation of the field of molecular biology as it was first understood by practitioners. Susan Spath examines the translocation of the "Delft School" program to the United States, when van Niel moved to Stanford University's marine biology laboratory in 1929. She describes van Niel's conception of the "Delft School" and analyzes his attempts to propagate it. Taken together, these papers show that the "Delft School" has had a wide range of meanings. They give evidence for the view that scientific practice and the construction of legitimating histories are intertwined processes.

Dutch Biology circa 1900 and the Beginning of the Delft Tradition.

Bert Theunissen (Utrecht University)

Martinus W. Beijerinck is almost invariably described as a'pioneer' of scientific microbiology. For good reasons, it seems, because in an era in which the study of bacteria was dominated by practical concerns, Beijerinck started a succesfull tradition of pure microbiological research, a field that would only acquire clear discipline status several decades later. This perspective, however, although not entirely incorrect, does not enable us to get a clear view of the aims and intentions of Beijerinck's work. I shall argue that Beijerinck, besides being a pioneer, was also an exponent of a contemporary movement in Dutch biology that was rapidly gaining momentum at the turn of the century. Together with figures such as Hugo de Vries, Beijerinck was an exponent of what might be called the Leiden tradition in experimental

botany. A central issue for the botanists taking part in this movement was the role of botany (and of science in general) in society. In their view, scientific research was not merely an intellectual and cultural enterprise; it was also of direct relevance to society. I shall argue that Beijerinck's taking up the scientific study of bacteria, rather than foreshadowing later developments, provides an example of the different ways in which Dutch botanists tried to put the idea of the practical relevance of pure science to work. From this, it will also become clear that the traditional interpretation of what Beijerinck's work was about will have to be revised

Beneficent Microbes: Albert Kluyver, the Delft School of Microbiology, and Industry.

Olga Amsterdamska (University of Amsterdam)

The emergence of research schools and new research areas often depends on innovative exploitation of unusual institutional opportunities by entrepreneurial scientists. Combining a research program in microbial biochemistry with extensive industrial connections, A.J. Kluyver, a professor of microbiology at the Technical University at Delft from 1922 to 1956, managed to establish not only a thriving laboratory training a number of industrial microbiologists, but also an eminent research school. This paper traces the development of the Delft School paying particular attention to the manner in which the institutional setting of the technical university provided an environment in which the practice oriented work for industry could be used to support and legitimate a research program in fundamental biochemistry.

Microbiology As Pure Science: C.B. van Niel and the Reinvention of the 'Delft School' in California.

Susan Spath (University of California, Berkeley)

Originally from the Netherlands, C.B. van Niel brought his understanding of the "Delft School" of Microbiology to the United States when he joined Stanford University's marine biological laboratory in 1929. There he pursued research on the biochemistry and physiology of microorgansisms until the 1960's.

In the 1940's, general interest in microorganisms, especially bacteria, for the study of basic questions in biochemistry, physiology, and genetics increased substantially. In 1949, van Niel specifically attributed this new interest to the influence of the "Delft School," and its stimulus to the expansion of "general microbiology". This paper analyzes van Niel's conception of the "Delft School", and his efforts to secure an important place for this tradition and for microbiology in the history of the life sciences. However, some of the important research regarded by van Niel as "general microbiology" was later claimed for the "official" history of molecular biology. This paper suggests that microbiology was a crucial resource for the formation of molecular biology, but was unable to serve as its disciplinary locus.

Quality of Risk-Assessment in Biotechnology: Theory, Practice and Politics of Deliberate Release II. Theory and Practice

Organized by Ad van Dommelen (Vrije Universiteit, Amsterdam)

Chair: Philip J. Regal (University of Minnesota)

The usefulness of our perspectives is the more compelling since the quality of risk-assessment is of great practical importance, and not just theoretically interesting. Therefore, seperate consideration will be given to the specific problems of uncertain knowledge and expertise that arise in practical risk-assessment situations.

The Case for a Moratorium on the Large-Scale Release of Genetically Engineered Organisms into the Environment.

Brian Goodwin (Open University)

At the moment there are no adequate safety protocols governing the movement and dissemination of genetically engineered organisms (GEOs) between countries, now happening as part of the 'gene revolution' in agriculture, unlike those that protect most countries from the import of potential crop and animal disease agents. Yet the risks are equally high from GEOs. I shall present evidence on known cases of epidemics caused by GEOs and the intrinsic unpredictability of their ecological consequences, making the implementation of safety protocols an urgent issue. A moratorium would provide the much-needed opportunity to discuss and implement such protocols needed to reduce the risk of irrevocable and untraceable damage to ecosystems and human populations.

The Oxford Baculovirus Controversy: Safely Testing Safety?

Les Levidow (Open University)

In spring 1994, for the first time in Britain, there was a prolonged public controversy over the intentional release of a genetically modified organism (GMO). This GMO was a baculovirus with a gene inserted for scorpion toxin, designed to kill larvae more quickly than the wild-type baculovirus would do. The controversy arose partly from the value judgements implicit in safely testing safety.

This case proved difficult for the 'step-by-step' procedure of gradually relaxing containment on a GMO; indeed, the stepwise procedure entailed a circular logic.

Despite its unusual circumstances, the baculovirus case illustrates difficulties which pervade the stepwise procedure in general. That is, hypothetical hazards can be meaningfully tested only by enhancing the prospects for realizing them. Such releases in turn can be justified only by accepting biotechnological claims to provide 'environment-friendly products'.

In effect, 'safety' judgements bear the burden of adjudicating a contentious technological development, by default of any direct democratic means.

Conceptual Confusion in Risk Assessment and its Methodological Analysis.

Ad van Dommelen (Vrije Universiteit, Amsterdam)

Debates about the risk assessment of releasing genetically modified organisms (GMOs) into the environment seem to be making little progress. One important reason for this stall is that two of the most central dichotomies in the debate, viz. 'process versus product' and 'case-by-case versus generic', are used in ways that confuse the issue. This confusion is distracting attention from more fundamental problems that underlie risk assessment. I analyze the misguided use of these two dichotomies and present a more useful reconstruction of the risk assessment debate. As an example of a more adequate approach to the issue of risk assessment and its quality, I discuss the role of the concept of pathogenicity in risk assessments. Methodological analysis of the problem and the relevant biological knowledge shows that general claims about the safety of GMOs derived from nonpathogens are unwarranted.

Anthropomorphism

Contributed papers
Chair: TBA

The Problem of Anthropomorphism for Turn-of-the-Century North American Natural History Writing.

Robert W. Mitchell

Many naturalists at the turn of the century, following and Darwin's and Romanes' lead, took anthropomorphism for granted given the fact of evolution, and presumed that animals lived lives similar in many ways to those of people. Stories about animals depicting them as thinking, emotional and sentient beings were enormously popular. John Burroughs argued strongly against the anthropomorphic writings of Ernest Thompson Seton, Charles Roberts, and especially William Long, yet himself used anthropomorphic interpetation in his own writings. Although the controversy extended beyond natural history writers to include newspaper exposes and discussion in scientific journals, the rapidly developing behaviorist approach depicted all natural history as inadequately controlled and therefore suspect, and the issue of anthropomorphism was viewed in scientific doctrine as an unscientific approach to understanding animals. However, because the issue was dismissed rather than resolved, many nonscientists and some scientists continued to use anthropomorphism to understand animals.

Why Ethological Theories about Animal Welfare Need Hermeneutics or Phenomenology.

Susanne Lijmbach (Wageningen Agricultural University)

In order to conclude that animal emotions are not just theoretical concepts in the heads of ethologists, most ethologists use the argument of analogy. This argument has to narrow the gap between the objectivating language of the natural scientific, ethological theories and the subjectivating language in which we are speaking about emotions. However, this gap can not be narrowed by a scientific or logical argument.

In the human sciences the phenomenological and hermeneutical sciences are directed at knowledge from a subjectivating point of view. But, most phenomenological and hermeneutical scientists

have a philosophical concept of subjectivity which excludes animal subjectivity. However, the philosophical hermeneutics of Gadamer about human health and illness, which emphasizes the bodily character of experiences of health and illness, may open a possibility of animal subjectivity, which is based on their bodily way of being.

Writing about health and illness, Gadamer refers to natural scientific theories about health and illness. I will show how Gadamer narrows the gap between the objectivating language of the natural sciences and the subjectivating language of hermeneutics in a more justified way than most ethologists do.

Discussion.

Cecelia Heyes (University College, London)

FRIDAY, JULY 21 8:30 - 10:00

Visions of Life in the Nineteenth Century

Contributed papers
Chair: TBA

Johann Wolfgang von Goethe: Bones, Plants, and Color.

Ralph Troll (Augustana College)

Wherever German is spoken, Johann Wolfgang von Goethe enjoys undisputed renown. His name is synomymous with literary greatness; but to most of the world, he is virtually unknown as a scientist. Yet as the result of his unlimited curiosity about natural phenomena, the great Weimar edition of Goethe's works contains fourteen volumes of his writings in the various fields of natural science. Mechanics became highly quantitative in the seventeenth century. The view that animals and plants may be considered mechanisms naturally inspired the thought that the quantitative method might be useful in biology. Goethe, however, viewed nature not as a machine. He shunned mathematical analysis and excessive instrumentation. He was opposed to the analytic treatments of organisms and displayed fierce opposition to an ultra-mechanistic interpretation of nature. The value of his work has been estimated very differently at different times and the essence of his contribution to biology has not always been correctly identified. I maintain that Goethe made substantial contributions to osteology, botany, and optics.

Louis Pasteur– A Centennial Commemoration.

Robert Krasner (Providence College)

Louis Pasteur (1822-1895) is considered to be one of history's greatest scientists. He has been described as "the most perfect man who has ever entered the Kingdom of Science." This theme is developed in this lecture. While the focus is on the events leading to the development of the rabies vaccine and its first human trial (Joseph Meister, July 8, 1885), Pasteur's earlier studies, including fermentations, silkworm disease, spontaneous generation, cholera, and anthrax are most noteworthy. Recognition of Pasteur's achievements, particularly rabies, the world-wide tribute following his death, and his continuing legacy one hundred years later will be presented. This slide-illustrated lecture includes caricatures of Pasteur which appeared in French newspapers during his lifetime highlighting his contributions to science. The year 1995 has been designated by UNESCO and the Pasteur Institute as "The Year of Louis Pasteur" in commemoration of the one hundredth anniversary of his death.

Rudolf Virchow's Diffficulties in Becoming a Member of the Prussian Academy of Sciences.

Klaus Wenig (Berlin-Brandenberg Academie der Wissenschaften, Berlin)

In the middle of the 19th century the Prussian Academy of Sciences in Berlin, which had been founded by G. Leibnixz in 1700, had to consider radical changes in the field of natural sciences from romanticism to more exact methodology. Members of the Academy like J. P. Müller and E. du Bois-Reymond, who represented this development, proposed new candidates for membership, who were able to support this direction. Thus, the German anatomist and pathologist Rudolf Virchow was suggested for membership[in 1864. However, the Class of Physics and Mathematics rejected this proposal by E. du Bois Reymond. Taking into account that Virchow was already a scientist of international reputation, this rejection is surprising. My paper is focusing on this procedure and its background.

Post-WWII Biomedicine. II: Post-clonal Bodies: Immunology in the 1960s and Beyond 1

Organized by Alberto Cambrosio (McGill University) and Thomas Söderqvist (Roskilde University)
Chair: Olga Amsterdamska (University of Amsterdam)

(Abstract not supplied)

The Molecularization of Immunology: the Problem of Antibody Diversity.

Alfred I. Tauber (Boston University)

The dichotomy between the molecular and biological approaches to immune selfhood has persisted since the 1930s. Post-CST immunology has been characterized as dominated by the rise of the biological perspective in immunology. However, we would first state that CST itself was not proven by its biologically oriented proponents via unispecificity experiments; instead, the instructive hypothesis of antibody formation was disproven by molecular immunologists (what Jerne would term trans-immunologists), who put their own firm imprint upon the field. As such,

what made CST so attractive to Burnet in the first place - its ability to apply to questions of selfhood and tolerance - was rarely mentioned at the 1967 CSH Symposium apotheosis of the theory. Instead, the molecular immunologists placed their own primary question - namely, the mechanism of the generation of antibody diversity - at the center of the field, where it would stay for the next two decades.

Still more instructively, the "solution" of antibody diversity would be arrived at by molecular biologists such as Tonegawa and Leder who clearly did not perceive of themselves as immunologists in the first place. The early attempts at the solution of antibody diversity relied upon the analysis of genetic markers and protein sequences; the latter attempts, however, saw the introduction of recombinant DNA technology into the field, as the cloning and sequence of antibody genes led to the apparent solution of this most molecularly oriented of questions. The solution of antibody diversity was perhaps the most glorious intellectual application of recombinant DNA technology, and it is difficult to say whether Tonegawa received his Nobel Prize for his application of this methodology or for the importance of the questions he apparently answered. What is perhaps more relevant is not the impact of the solution on the acceptance of recombinant technology, but the impact of the technology on the molecularization of the field.

Conversational and Idiotypic Networks: the Romantic Foundations of European Immunology in the 1970's.

Thomas Söderqvist (Roskilde University)

Until the mid and late 1960s the emergence of immunology as a scientific discipline was largely an American affair. The situation began to change in 1969-1971 when the Basel Institute for Immunology was founded and became the organizational and intellectual center for European immunology throughout the 1970s and early 1980s. Basel was not just an organizational center, however. Basel was also placed on the theoretical map when, in 1973, Niels K. Jerne proposed the network theory of the immune system. In the immunological lore, the network theory was always seen as a particularly `European theory'. In this paper I will show that the two events--the foundation of the Basel Institute and the formulation of the idiotypic network theory--were closely related. I will also demonstrate the notion of the cybernetic, idiotypic network can be mapped on the basic vision of the Bael Institute as a `horizontal organizational network'. Finally, I argue that both the network theory and the Basel network organization are deeply rooted in one man's (Jerne's) Romantic ideas, and that, consequently, the foundation of European immunology in the 1970s was the expression of a Romantic streak in the contemporary life sciences.

The Immune Cell and the Flow Machine.

Alberto Cambrosio (McGill University)
Peter Keating (University of Quebec, Montreal)

Because of its biological focus, post-clonal immunology has moved away from an exclusive interest in humoral factors (antibodies) that previously characterized immunochemical

practices and has placed cellular and, more recently, subcellular elements at the center not only of its theorizing but also of everyday laboratory and clinical (diagnostic and therapeutic) manipulations. Thus, the lymphocyte, once a neglected cell, or, rather, lymphocytes as an increasingly differentiated array of cells have moved to the forefront. The paper will describe the development of the equipment and techniques that allow researchers and clinicians to interact with lymphocytes, in particular through the use of so-called flow-cytometric technologies. A central argument of the paper is that representations of the various lymphocyte subpopulations are dependent upon the availability of tools for intervening on them, and, conversely, that intervening is predicated upon representing.

Politics of Conservation I. Ecologist Visionaries/ Ecologized Visions 1

Organized by Peter J. Taylor (Cornell University)
Chair: Peter J. Taylor (Cornell University)

This session examines whether ecology has its roots in resource management and whether it is still connected with such roots and explores the emergence of ecology as ideology, and, in particular the interactions between commitments of "environmental scientists" to economic, technological, and environmental ideas and practices. Part A links up papers that broadly address issues about philosophical conceptions of nature and how these conceptions influence the way we as historians write histories of ecology and Part B examines three key contemporary figures in 20th century ecology, from "pure science" to applied science to social science.

'The Revolt of Nature': Exploring Themes of Domination, Agency, and Emancipation

Helen Denham (University of California, Berkeley)

Contained within the concept of the 'revolt of nature' is the dual notion of an internal and external nature, or rather the nature of humans and the nature outside of humans. Revolt, in this case, can be interpreted as affecting both the social and the natural worlds. This paper will explore the theoretical foundations of this concept as it was first presented by Max Horkheimer in 1947 and later elaborated upon by William Leiss in the early 1970s; it will then examine its strengths and limitations in particular as they relate to select developments in the fields of environmental history, political ecology, and social theory; finally, it will discuss the relevance of such themes for contemporary efforts to understand the complex interactions and relationships between human and non-human nature. Horkheimer and Leiss take as their point of departure a 'critique of reason' which for them incorporates a discussion of the domination, mastery, and liberation of nature. Implicit within their discussions is the idea that neither humans nor non-human nature can ever be entirely repressed, subdued, mastered, or controlled. Within this context, the revolt of nature suggests an inherent resistance to conditions of oppression; it also implies that non-human nature has agency. The purpose of this paper is to open a dialogue to discuss both the specifics of the

revolt of nature and the more general questions of social resistance, social movements, and nature as actor.

'Wilderness': Primeval Nature and Traditional Culture Glued Together Using Politics and Ideology.

Yrjo Haila (Turku University)

The talk will provide a brief history of the notion of "wilderness," examining how this notion came to dominate one portion of conservation thinking; the attributes of nature & culture accepted under the umbrella "wilderness"; how this is philosophically suspect; but how, nevertheless, is there something valuable in the talk about "wilderness."

Clarence Glacken and 'Nature and Culture in the 19th Century'.

S. Ravi Rajan (University of California, Berkeley)

Clarence Glacken is known for his book, *Traces on the Rhodian Shore* . Although Traces is more than 600 pages long and took a life-time to write, it was only one of Glacken's odysseys in to environmental history. In the four decades from his graduate student days at Johns Hopkins University to his retirement as a Professor of Geography at the University of California, Berkeley, Glacken erected a complex and fascinating theory of the development of environmentalism, which was kept from public view only because of his introverted nature and his commitment to perfectionism. This paper is an attempt to identify some of the key themes in Glacken's work. It has three parts: his view of history, especially on the importance of the history of ideas; the three themes of his early work: the Idea of a designed earth; Environmental Influence; and Man as a Geographical Agent; and the key issues underlying his unpublished sequel to Traces , especially, the importance of the concerns about posterity and aesthetics. The paper draws extensively from the unpublished and largely unused manuscripts and letters of Clarence Glacken at the Bancroft Library, University of California, Berkeley.

Perspectives on Museum I (Workshop)

Organized by James Griesemer (University of California, Davis)
Chair: James Griesemer (University of California, Davis)

The goal of this session is to develop and explore the philosophy of museum, focussing on natural history museums as examples and case study material. The aim is to formulate a set of issues about museums -- and related activities such as collecting, curating, and exhibiting -- as institutions that can serve to focus collaborative work by historians, philosophers, and social scientists interested in the full gamut of problems in science studies.

Jane Camerini (Madison, WI)

John Damuth (University of California, Santa Barbara)

Michael T. Ghiselin (California Academy of Sciences, San Francisco)

James Griesemer (University of California, Davis)

Giovanni Pinna (Museo Civico di Storia Naturale, Milano)

Barbara R. Stein (University of California, Berkeley)

Mary P. Winsor (University of Toronto)

Waiting for Monod: The History of Biological Regulation II

Organized by Evelyn Fox Keller (MIT)
Chair: Evelyn Fox Keller (MIT)

From *E. coli* to the Elephant; from Pasteur Institute to Cold Spring Harbor: How Biochemical Regulation Held.

Jean-Paul Gaudillière, (INSERM Paris)

Biologists working in the laboratories of biochemistry and bacterial physiology at the Pasteur Institute, respectively headed by Monod and Lwoff, focused on different experimental systems until the late 1950s. For Monod, the system of choice was *E. coli* exposed to chemicals that induced the synthesis of enzymes involved in lactose metabolism. For Lwoff, it was lysogenic *E. coli* producing phages when properly exposed to UV radiations or other triggering agents. According to legend, these different worlds met when Monod embarked on using Jacob's mating technique In two years, this collaboration resulted in a model for protein synthesis which explained the properties of both systems with genetic regulation circuits that are usually viewed as the basis for the conjunction of "life" and "information". A few years later, however, Jacob's and Monod's activities had drifted apart: Monod was emphasizing another "secret of life", namely biochemical regulation circuits exemplified by the properties of allosteric proteins.

The paper will address this paradox by examining Monod's attempts to preserve biochemical regulation while defining genetic regulation in collaboration with Jacob. In order to discuss the interplay between material, social and literary technologies, I will focus on two arenas : the experimental bench at the Pasteur Institute and the Cold Spring Harbor professional meetings.

Taming the Cybernetic Metaphor: Feedback and Genetic Control.

Evelyn Fox Keller (MIT)

The introduction of the term and concept "feedback" into biology in the mid 1950s is often taken as evidence of the influence on biological research of modern developments in electric circuitry, electronics, and cybernetics. Norbert Weiner's books helped enormously to popularize these ideas in the culture at large. At least on a terminological level. there is little question that they were also influential in biology. However, both the conceptual roots and the conceptual consequences of the introduction of feedback into biological research were considerably more\re complex than a simple reading of direct disciplinary incursion would suggest. Indeed, I want to suggest that the word itself, along with the various models invoked to illustrate the phenomenon of feedback, constituted a resource for researchers representing a number of quite different biological agendas competed. To illustrate this claim, I will trace the competing uses that were made by geneticists, biochemists, and developmental biologists of a feedback model for biological regulation introduced in 1949 by Max Delruck.

Discussion.

René Thomas (Université Libre de Bruxelles)

Positioning Developmental Systems: Convergences and Choices

Organized by Susan Oyama (John Jay College, CUNY)
Chair: Paul Griffiths (University of Otago)

Traditional approaches to both development and evolution have tended to be genocentric. Conceptualising development as the unfolding of a genetic program fits neatly with post-synthesis definitions of evolution as change in gene frequencies. Workers taking a developmental systems approach have elaborated alternative views of both processes and of the relationship between them. At points these efforts have converged with critical projects with largely independent histories, including that of dynamical systems, process structuralism, and Gibsonian direct action. How are these critical projects related to each other? Are they to be seen as alternative treatments of the same problems, or are the problems themselves subtly (or not so subtly divergent).

Organisms as Meaningful Agents.

Brian Goodwin (Open University)

Organisms have a property of dynamic closure which underlies their capacities for regeneration and reproduction, making wholes from parts. Hence the definition of organisms as life cycles, which includes dependency on a particular habitat. Dynamic closure also gives organisms agency, and their internal and external relationships can be said to have meaning in relation to maintaining and perpetuating a whole. However, organisms are also open to environmental influence, and they can change in ways that maintain dynamic closure in altered environments.

These concepts will be explored in relation to organisms as complex systems that generate emergent properties.

Relationships as Dynamic Systems: Information and Development.

Alan Fogel (University of Utah)

Relationships, as living systems, have the ability to self-organize, self-maintain, self-renew and self-develop. A relational perspective shares world views and concepts with a number of theories that take seriously the connections between things, most notably developmental systems theory, dynamic systems theory, and Gibsonian ecological theory. I propose that the communication system, rather than its individual components, is the proper unit of analysis in a relational perspective. Explicitly focusing on relational processes -- the communication system and how it transforms over time -- can highlight the similarities and differences in these approaches and at the same time ground an emprically verifiable systems theory of development. A relational approach can be applied to all levels of biological communication -- molecular, cellular, behavioral. I draw on examples from my own research on interpersonal communication in the development of the human mother-infant relationship.

Discussion.

Cor van der Weele (Vrije Universiteit, Amsterdam)

Discussion.

Ron Amundson (University of Hawaii, Hilo)

Continental Influences on British Natural History, 1800 - 1860

Organized by Philip Rehbock (University of Hawaii, Manoa)
Chair: TBA

As has been so often noted– by historians of the Darwin Industry on down to the chroniclers of local natural history societies– the 19th century was the heyday of British natural history. Far less often have historians noted just how uninspired the 18th century was by comparison. The more reflective observers of the early 19th century were well aware, however, of the slump into which British natural history had sunk since the days of John Ray. The Scottish zoologist John Fleming (1785-1857) was not overstating the case when, in 1822, he referred to the 18th century as the "dark age" of Britain's natural history tradition, and to the forty years previous to his writing as a period of "zoological listlessness."

It is not the intent of this session to debate whether natural history in the Age of Joseph Banks was retarded, or indeed kept alive, by its most famous patron. Rather we hope to examine some of the routes by which British natural history so rapidly emerged from its "dark age." How did Georgian Britain, whose roster of philosophical naturalists included few beyond Gilbert White, and James Hutton, give way to Victorian Britain, with not one but two conceivers of evolution by natural selection, a raft of geological theorists, and naturalist entrepreneurs like the Hookers and Richard Owen? One might point to institutional factors such as the rise of the specialist societies in London; or to extrinsic factors such as the new accessibility of the countryside wrought by the railroads. But in this session we look abroad to the Continent in the hope of understanding the sudden progress of Victorian natural history. Bluntly stated, to what extent was Victorian natural history built upon an imported foundation? To what extent did Victorian naturalists correspond with, obtain ideas, books, papers, or specimens, or draw their enthusiasm, from their counterparts in France in Germany?

In one brief session of three papers, we can only hope to demonstrate the viability of this topic as a field for further research. We welcome suggestions from our listeners, however, of other promising cross-channel routes to explore. And we expect that in the course of time other scholars will contribute additional chapters to what will eventually be a book on the subject.

Extinction: Its Recognition on the Continent and its Acceptance in Britain.

Philip Rehbock (University of Hawaii, Manoa)

From the awakening of natural history in the Renaissance until the close of the 18th century, the impossibility of species extinction was a well accepted corollary of the fixity of species doctrine among European naturalists and , especially, natural theologians. John Ray, Carl Linnaeus, William Paley, and even Thomas Jefferson in the New World, all concurred that no rational and omnipotent Creator would allow His harmonious economy of nature to be disrupted by the loss of all the occupants of a particular station.

In Enlightenment France, as natural history became less dominated by theological considerations, the possibility of extinction began to be entertained. According to traditional historiography, Buffon first threw doubt on the anti-extinction principle; and Cuvier, with his analysis of the remains of extinct pachyderms, then demonstrated the reality of extinction conclusively at the turn of the 19th century. With species extinction established as one of the major events in the history of life, its opposite--species emergence--had now to be explained, and the saga of creation vs. evolution was underway.

If extinction was largely a French discovery, why was it left to the British to conceive an acceptable emergence theory? (Surely Lamarck was not entirely to blame?) More importantly, why was Cuvier's discovery so easily absorbed into British natural history at the beginning of the 19th century, when, a few decades earlier, British naturalists had been among the staunchest advocates of the anti-extinction principle? Why is there so little evidence of controversy over this revolutionary discovery, especially at the moment when a reactionary Britain was least apt to look kindly on any revolutionary ideas--regarding either the Order of Nature or the Order of Society--from a revolutionary France?

This paper will outline the dimensions of the extinction episode and propose provisional solutions to its enigmas by an examination of the work of turn-of-the-century naturalists in Britain. It

is also hoped that the topic will provoke discussion of the larger questions of (1) the flow of natural history data and theories from the Continent to Britain during the crucial, pre-Darwinian period; and (2) the prevalence of "non-controversie" elsewhere in the history of biology.

The 'Silent Revolution' in Pre-Darwinian Biology: The Transfer of 'Wissenschaftliche Zoologie' to Britain in the 1840s and 1850s.

Marsha Richmond (Wayne State University)

In the mid-1840s, the first traces of a "silent revolution" that would challenge the traditional methodology of British natural history can be discerned. Visible in the early work of Thomas Henry Huxley, the new methodology also was taken up by a number of prominent naturalists, including Charles Darwin after 1850. With roots in the embryological work of Karl Ernst von Baer, this new approach to biological investigation also was evidenced in France, especially in the writings of Henri Milne-Edwards. Under the banner of "wissenschaftliche Zoologie" (scientific zoology), the new methodological movement experienced its fullest expression in Germany. The present paper will examine the roots of this new program for zoological research, particularly focusing on the work of Karl Theodor von Siebold (1804-1885), and trace its spread to the British natural history community.

In the early 1840s, Siebold, professor of zoology, physiology, and comparative anatomy at Erlangen, prepared annual reports on new developments in German natural history for the *Archiv für Naturgeschichte*, which were translated into English and published by the Ray Society. In 1845 Siebold's influential textbook on the comparative anatomy of invertebrates was published, an English translation of which appeared in 1854. In these works as well as numerous scientific papers, Siebold thoroughly embraced the use of the "embryological criterion" in classification (as formulated by von Baer), as well as stressed the necessity of detailed microscopical study in comparative anatomy and physiology. Consciously coining the polemical phrase "scientific zoology" to denote this new approach, Siebold and the comparative anatomist Rudolf Albert von Koelliker founded the new journal *Zeitschrift für Wissenschaftliche Zoologie* in 1848 to publicize the work of fellow practioners of the new zoological program.

In England, one of the earliest and most fervent followers of the new scientific zoology was the young T. H. Huxley, who was first introduced to the new methodology during his voyage as ship's physician to HMS Rattlesnake, 1846-1850. Huxley's role in popularizing the new methodology among British natural history circles, both in his earliest scientific papers and through his public proselytizing, was significant. It was featured in a number of prominent works in Britain in the mid-1850s, being particularly visible in Darwin's *Monograph on the Sub-class Cirripedia* (1852-1854) and in *Origin of Species* (1859).

As scientific zoology came to be adopted by more and more of the rising new professional scientific elite in Britain, and by the new generation of German academic zoologists, it gradually came to replace, or at least to supplement, the traditional methodology of natural history in both countries. The process of its assimilation in the 1840s and 1850s can be viewed as a "silent revolution," it is argued, since the new perspective it represented subsequently became subsumed within Darwin's more radical revolution in biological investigation, which was embraced by both Huxley and Siebold.

Historicism in German Biology: From Romantic Naturphilosophie to Darwinism.

Thomas Junker (Munich, Germany)

The reception of Darwin's theories by German biologists has been traditionally depicted as positive and even enthusiastic. Especially the comparison to the rather cool reaction of French biologists gives plausibility to this assessment. From a sociological point of view, Darwin's theories were most readily accepted by German biologists who held a liberal political outlook, who were leaning towards an agnostic or atheistic view of religion, and who had a materialistic rather than an idealistic conception of nature. Darwinism, however, was not the first scientific research program that was based on a historicist understanding of nature. During the first decades of the nineteenth century in the German countries a historicist philosophy of science was promoted in various scientific fields, from law to the humanities and political and natural sciences. The scientists who promoted the scientific programs of the 'Historische Schule,' 'historicism,' and romantic Naturphilosophie, were predominantly political conservatives and had a religious and idealistic outlook. Between 1830 and 1860, historicism - defined as an attempt to understand nature in terms of its historical development - was shifted from its original conservative sociological basis to a liberal one. I will discuss various causes that may explain this shift and some of the theoretical changes that occurred when the historicist idea was incorporated into the new theoretical environment.

FRIDAY, JULY 21 10:30 - 12:30

On Species Concepts, Theory and Practice, Past and Future

Organized by Mary P. Winsor (University of Toronto)
Chair: Mary P. Winsor (University of Toronto)

The key to the species problem has been clear, or should have been, ever since Darwin made natural history into something not only fully natural but also historical. The origin of a new species occurs over time, out of the transformation, and often the splitting apart, of a previous species. This truth, however, does not hand us a definition of species, what it does it explain why definitions are so elusive. Tate Regan's 1925 presidential address symbolizes the ongoing turmoil; he was explaining the biological dynamics that had already been sketched by Darwin and would later be expounded by Mayr, but cynics ridiculed him for defining "species" as whatever the experienced taxonomist may choose to name. Still, human needs, both practical and psychological, continue to keep alive the interest in definition, while the success of cladistics (phylogenetic systematics) may have increased the demand.

The pragmatic dimension exposed in Tate Regan's notorious definition is exemplified in the cases researched by McOuat and Stevens. If the competent taxonomist decides on species, who decides who is competent? At Kew and the British Museum, the imperial centers of Victorian natural history, authority was firmly held by the men in power, at the expense of field naturalists

closer to the living plants and animals. Counterbalancing this historical perspective, Kornet's "composite species" concept points to the future. Based on rigorous formal analysis, she claims that the ambiguity rife in earlier definitions can be avoided if careful attention is paid to the difference between splitting and transformation. O'Hara, who has published on the past and future himself, will conclude our session by knitting these threads into a seamless fabric.

The Cynical View of Species: Social Spaces and the End of Definition.

Gordon McOuat (University of King's College, Halifax)

This paper will identify two places within which discourse about species gets stabilised in the nineteenth century: 1) Rules of Zoological Nomenclature, and 2) Social places of museums. One situated species demarcations solely in the hand of "competent published naturalists", while the other claimed species competence for metropolitan museum naturalists. Both these sites claimed sole authority for species demarcations while consciously working against the idea of a "definition" of species. The "species definition" problem, then, did not exist until the population biologists of the 1940's created it along with its history. Insofar as Darwin was instrumental in the establishment of both of these sights, this should tell us something about Darwin's supposed "failure" to define species and his adoption of the so-called "cynical view of species", where "species are what naturalists say they are."

J. D. Hooker and Species: a Consummate Naturalist or a Consummate Politician?

Peter F. Stevens (Harvard University)

J. D. Hooker was perhaps unique among his contemporaries in being an active administrator, a major botanical explorer, a statesman of science, a monographer, a flora writer, and a strong proponent of evolution. Nevertheless, many of his views on species were those of his colleagues elsewhere in Europe and North America. Hooker's approach to species devalued the value of field data, the only botanical information obtained during exploration other than the plants themselves. Field work was, however, particularly important to two sets of people Hooker inveighed against: naturalists describing hybrids and even new species from the European flora (and often publishing in obscure journals), and those describing species from the colonies in which they were living. Both posed problems for the kinds of projects Hooker and his colleagues were most active in: the inventory of nation and empire. A stable nomenclature was a necessity for inventory; change, either occasioned by evolution or by new observations, militated against its successful completion. Darwin hoped that disputes about what was or was not a species should cease, however, his solution - reliance on the experts - was not satisfactory. Most systematists wanted species to be both stable and more "real" than a variety or a genus, so they would either have to find features that allowed them to rank the species (contra Darwin), or be forced to maintain species limits by invoking superior knowledge, convention and tradition. The latter was by necessity the tactic most often used, setting up the lines of later conflict with those who wanted unambiguous criteria for the rank of species as well as those who wanted species to be the unit of evolution.

Species Boundaries in Time: Introduction to the Concept of Composite Species.

Diedel J. Kornet (Leiden University)

In calling for the reconstruction of the historical sequence of speciation events, phylogenetic systematics has made unavoidable the question when exactly does a new species arise. The morphological and biological species concepts fail to give an answer. Organisms tend to look alike and are capable of interbreeding with their parents: these concepts therefore cannot trace boundaries between species in time. If Hennig's suggestion that new species arise with splits in the genealogical network is followed to its conclusion, species are parts of the network bounded by two splits. We have constructed a formal definition of those parts and proved mathematically that this definition indeed partitions the genealogical network exhaustively into historically continuous entities with sharp boundaries, also in time. However, the abundance of these entities (which we call internodons) , related to their short life span, makes their interpretation as species implausible. Not all splits should count as speciation events. Grouping internodons on the basis of an inherited fixed character state into "composite species", may herald a solution of the species problem. Since temporal continuity is characteristic of composite species their ontological status is that of historical entities rather than natural kinds.

Discussion.

Robert J. O'Hara (University of North Carolina, Greensboro)

German Genetics - A Different Style Of Thought?

Organized by Manfred Laublicher (Yale University)
Chair: TBA

Jonathan Harwood supported his application of the concept of style for the analysis of variations in the history of scientific practices with an analysis of the German genetics community during the first decades of this century. The German genetics community itself was remarkably diverse and so far, with the exception of the eugenics movement, relatively little is known about its history and the philosophical convictions and sociological factors that influenced those scientists. The papers in this session will deal with some of these aspects. One of the objectives of this session is to present more material that should further stimulate discussion of whether specific styles of scientific thought exist and whether the German genetics community would be a good paradigm case for this concept.

Hans Winkler and Gene Conversion: Recombination in a German Context.

John Simmons Ceccatti (University of Chicago)

In 1930, a German botanist named Hans Winkler published a theory to explain genetic recombination he and others observed in lower plants and fungi. The theory -- gene conversion -- was proposed as an alternative to the theory of crossing-over developed primarily by the Morgan group in the United States and was intended explicitly to account for recombination without the added assumption of the linearity of genes on chromosomes. Winkler's theory came under immediate criticism in Germany from Curt Stern, who had spent time in Morgan's lab at Columbia University, but it had little impact among other geneticists, especially those in the United States who were developing materialist accounts of genetic recombination. In this paper, I will examine the content of Winkler's theory of gene conversion and will explore the several reasons behind its failure to be incorporated into mainstream genetics theories of the 1930s. Two issues that will be addressed are the role of the organism in theory construction and the influence of national context on the style of scientific research.

On the Mutability of Genes and Geneticists: The Americanization of Richard Goldschmidt and Victor Jollos.

Michael R. Dietrich (University of California, Davis)

In the late 1920s Richard Goldschmidt and Victor Jollos began to experiment with the fruit fly *Drosophila* and so tried to enter that distinctly American world of *Drosophila* genetics. Both quickly found themselves embroiled in a controversy over temperature effects and later spontaneous mutation. I claim that an important part of the difference between these German geneticists and their American counterparts was based in differences in style. These differences were made even more accute by Goldschmidt's and Jollos's immigration to the United States after their expulsion from Nazi Germany. For both Americanization became a process of altering their scientific styles.

Oskar and Cecilie Vogt: Crossing Boundaries, Brain Research and Genetics.

Manfred Laublicher (Yale University)

When Timofeeff-Ressovsky arrived in Berlin he found a position in the "Kaiser Wilhelm Institut fur Hirnforschung" led by Oskar Vogt. That was by no means an accident. As early as 1909 Oskar Vogt had tried to utilize principles borrowed from other biological disciplines, such as systematics and genetics, for application in psychiatry. This paper analyzes the problem that Vogt wanted to solve, namely the classification and explanation of pathological conditions of specific regions of the brain. How his application of genetics that helped him to come up with a system of classification of "pathological" and "natural" variation influenced in turn the development of

genetics in Germany will be investigated. In addition special attention will be paid to the logical structure of Vogt's "reasoning across disciplinary boundaries".

Morgan's Uncertain Legacy: Aspects of German Genetics, 1920-1930.

Sahotra Sarkar (McGill University)

The Morgan school created classical genetics from the turn-of-the-century Mendelism shortly after 1910 by: (i) demonstrating the linear order of genes on chromosomes; (ii) explaining some phenotypic traits by invoking localized genes for them; and (iii), in a brilliant dialectical move, exploring the fine structure of chromosomes using the genes localized in (ii). This program works so long as there is a one-to-one correspondnece between genes and traits, or at least a clear correlation between traits and small numbers of loci that determine them. Neither all genes, nor all traits, exhibit such benevolence. In the 1920's, the Morgan program begins to fall apart, especially in Germany. One result is the addition of an increasingly cumbersome superstructure on classical genetics, which ultimately became the baroque genetics that molecular biology attempted to sanitize in the 1950's. This paper looks at the beginnings of these developments in the 1920's, which took place in Germany, especially with the work of Timofeef- Ressovsky and the Vogts. In particular, it looks at how the concepts of expressivity and penetrance were introduced and how (gene) specificity was borrowed from a biochemical/physiological context in an attempt to preserve classical genetics.

Research Programs at the Rouge-Cloître: From Chemical Embryology, Nucleic Acid Biochemistry and Cytochemistry to Molecular Biology. II

Organized by D. Thieffry, (Université Libre de Bruxelles) and R. Burian, Virginia Polytechnic Institute and State University
Chair: Paulette van Gansen (Université Libre de Bruxelles)

Cytoplasmic Particles in Brussels (Jean Brachet, Hubert Chantrenne, Raymond Jeener) and at Rockefeller (Albert Claude), 1938-1950.

H-J. Rheinberger (Universität Salzburg, Austria)

The present contribution aims at elucidating the different research backgrounds that, towards the end of the 1930s, led Albert Claude and his colleagues at the Rockefeller Institute in New York, and Jean Brachet and his coworkers at the Free University of Brussels to focus their investigation on macromolecular structures ("microsomes," "particules macromoléculaires") of the cytoplasm, their microscopic and ultramicroscopic identity, their structure, and their function. What were the experimental systems, research traditions and background assumptions that were instrumental in shaping the cytoplasmic research program of the two groups? How did these systems, traditions, and assumptions influence the respective research activities throughout the

1940s? Comparing the two 'investigative enterprises' (Holmes) embedded in two different institutional settings in Europe and the US during War time, should lead to a better understanding of how, in terms of techniques and experimental systems, the stage was set for a macromolecular dissection of the cytoplasm that eventually became connected to the elucidation of the basic mechanisms of protein synthesis in the 1950s. And it should lead to an understanding of why, in terms of research policy in a wider context, the work of Claude came to occupy a position of general reference for the scientific community involved, whereas the work of the Brussels group remained much less visible.

Contributions of the 'Rouge Cloître Group' to the Notion of mRNA.

D. Thieffry (Université Libre de Bruxelles)

As a starting point, we will briefly review a set of historical accounts of the origin of mRNA notion. Referring to these accounts, a set of presumptive founding contributions will be identified, including Crick's well known paper on protein synthesis and Jacob & Monod's papers on the regulation of lac operon. These contributions will then be situated in the wider context of various early work involving a possible genetic or "messenger" role for RNA, including work on RNA location and turn-over, RNA viruses, characterisation of different RNA species, role of RNA indevelopment and regeneration, etc. In particular, we will discuss different sets of experiments conducted at the Rouge-Cloître by Jeener & Szafartz, and by Chantrenne, Marbaix, Burny & Huez. The analysis of all these different contributions will lead us to question the existing accounts of the emergence of mRNA concept. We will end with a discussion of the construction process of the "mRNA story" in regard to the diffusion and influence of the different scientific contributions.

Jean Brachet, Plasmagenes and Viruses.

J. Sapp (York University, Ontario)

The present contribution analyses the intellectual traditions and theoretical assumptions underlying Jean Brachet's research on microsomes during the 1940s and 50s. In particular, Brachet's discussions of microsomes are placed in the tradition of embryological views of the role of the cytoplasm in development and heredity, as well as plasmagene theory and investigations of cytoplasmic genetic entities led by Tracy Sonneborn in the United States, and by André Lwoff, Philippe L'Héritier and Boris Ephrussi in France. Brachet's theorizing on plasmagenes, cytoplasmic organelles, and viruses are further situated in the bio-political context of discussions of the inheritance of acquired characteristics at the time of the Lysenko controversy.

The Brachet-Jeener-Chantrenne Group: a Genetic Viewpoint.

René Thomas (Université Libre de Bruxelles)

This paper addresses contributions of the Rouge-Cloitre Group which deal with four aspects: 1) Contribution of Brachet to the idea that DNA is the genetic material. 2) Concrete contributions of Brachet, Jeener, Chantrenne, et al. to the concept later known as "flow of genetic information from DNA to RNA to protein". 3) Plamagene theory and the debate on nuclear vs cytoplasmic heredity. 4) Discovery of DNA denaturation. These contributions will be briefly discussed regarding their role and influence in the perspective of the developing Molecular Genetics.

Politics of Conservation II. Ecologist Visionaries/ Ecologized Visions 2

Organized by Peter J. Taylor (Cornell University)
Chair: Peter J. Taylor (Cornell University)

Plant Ecologists in 20th Century U.S. Conservation/Environmental Movements: The Case of Paul B. Sears.

Juliana Mulroy (Denison University)

Paul B. Sears (1891-1990) was an important figure in academic plant ecology, studying or working at three of the major U. S. centers of the discipline (Nebraska, Chicago, and Ohio State) and making pioneering research contributions to the field of paleocology from 1919 through the 1950s. Elected to boards and presidencies of most scientific organizations to which he belonged, including the Ecological Society of America and American Association for the Advancement of Science, Sears used his positions to advance science, science education, and conservation. At the same time he developed a popular following as a writer and lecturer on conservation topics. Beginning with radio programs in the 1920s, the environmental classic Deserts on the March and pioneering state soil conservation legislation in the 1930s, committee work with state and federal agencies as well as national conservation groups such as Friends of the Land in the 1940s, establishment and direction of the Graduate Program in Conservation at Yale in the 1950s, and a variety of visiting professorships across the country in the 1960s, Sears became a nationally known spokesperson on conservation issues and conservation education. Sears thus occupied a pivotal position at the intersection of academic ecology and conservation activism during a major portion of the 20th century. I propose to examine his roles as an ecologist within grassroots conservation organizations, and as a conservationist within scientific organizations, as a means of illuminating the development and interrelationships of ecology and conservation during this period.

'Potatoes Made of Oil': Eugene and Howard Odum and the Origins of American Agroecology.

Mark Madison (Harvard University)

Eugene P. Odum and Howard T. Odum were at the forefront of the "new ecology" of ecosystems in the 1950s and 1960s. As part of their program, the Odums were firmly committed to

bringing both natural and human ecosystems into accord with the laws of ecoenergetics, as they understood them. American agriculture struck the Odums as a particularly egregious violator of all the laws of ecoenergetics and hence a dangerous paradigm for world development. By reducing the complexities of American agriculture to a circuit diagram of energy inputs and outputs, the Odums concluded that the present system of "fossil fuel farms" was dangerously unstable. As a remedy they suggested an end to the Green Revolution and a modification of human society so at to more nearly approach the steady-state of a mature ecosystem. Paradoxically, the Odums' program was poorly received by the ecologists whom they sought to empower, and widely accepted by organic farmers, environmentalists and other "amateur" groups whom the Odums' had meant to supercede with their more rigorous analysis of agriculture. While the Odums agroecological language and tools continue to persist in agroecology, they have largely been divested of the Odums' broader program of bringing the entire human ecosystem into accord with natural laws. By re-examining the social and scientific context of the Odums' early work it may be possible to better understand agroecology as both a tool and a social program.

Ecology and Ideology in the General Systems Community.

Debora Hammond (University of California, Berkeley)

Focusing on the founders of the Society for General Systems Research, and specifically on the work of Ken Boulding, this paper will examine the ways in which ecological models shaped social theory in the general systems tradition, the extent to which the founders addressed the potential for coercive approaches to both resource and human management, and how they addressed issues of power and ideology. There is considerable diversity within the systems community on this issue; the primary aim of this paper is to demonstrate that a number of individuals were aware of the dangers and sought to balance environmental constraints with more participatory forms of decision making, drawing support for this position from their perception of the meaning of ecology, defined in terms of the dynamics of interacting populations, with evolutionary change based on adaptation and learning, which is inherently responsive to all segments of the population. For these individuals, an ecological systems view highlighted the necessity of responsiveness to all elements of the system, both through the insights of ecology as a science, and the active and informed participation of the human community.

Discussion.

Peter J. Taylor (Cornell University)

Philosophy and Evolution II

Contributed papers
Chair: TBA

Metaphysics and Vitalism in Henri Bergson's Biophilosophy: A New Look.

Spas Spassov (University of Montreal)

A necessary condition for understanding the insights and contemporary value of Bergson's biophilosophical views is to recognize the logical independence between his vitalistic model of life and the philosophical and metaphysical ideas and intuitions which underly this model. The paper purports to show that the vitalistic principle in Bergson's biophilosophy is not a necessary implication of his metaphysics, i.e. that it is not deducible from the philosophical and metaphysical premises forming its conceptual basis. The essential difference and logical independence between Bergson's concrete vitalistic model of life, and his more general biophilosophical ideas, only confirms the purely speculative character of the vitalistic principle. At the same time, this unnecessary hypothesis, today completely discredited, should not prevent us from recognizing the contemporary value of some of Bergson's metaphysical and methodological ideas.

Donald Davidson and the Structure of Evolutionary Explanation.

Denis F. Sullivan (St. John's University)

To those familiar with the work of Davidson, it might seem strange to see a title which couples his name with evolutionary theory. Davidson only mentions biology a couple of times and, as far as I know, has nothing to say about the theory of evolution. But in a number of ways his work on the nature of psychological explanation can be related to what goes on in evolutionary explanation. Furthermore this relationship is worth noting both for the light it sheds on the nature of evolutionary theory and also for suggesting the necessity of a reconsideration of the scope and complexity of the scientific enterprise.

Davidson has argued that psychology is radically incapable of formulating anything like the laws we find in physics. Furthermore, on a certain interpretation, his work implies that psychological explanations deal with contingent events and thus are fundamentally retrospective. But these three elements: the absence of law, the contingent nature of what is to be explained, and the fundamentally retrospective nature of the explanations to be provided, are the very elements Mayr and Gould pick out as characteristic of evolutionary explanation. Thus Davidson's work enables us to construct a parallel between the structure of psychological explanation and the structure of evolutionary explanation.

Darwin and the Eighteenth Century British Moral Tradition.

Michael Bradie

Hobbes set the stage for modern moral philosophy by constructing a model of human nature according to which human beings acted solely from considerations of self interest. The moral philosophers who followed read Hobbes as denying the reality of truly benevolent and disinterested human motivation. This struck many as contrary to the empirical evidence. A central theme running through 18th century British moral philosophy involved attempts to construct alternative conceptions of human nature which provided for both self interested and disinterested or other regarding motivation. Darwin inherited this problem and, in his moral theory, gave it an evolutionary twist. In this paper, I trace the connections between the 18th century British moral tradition and the emergence of the "problem of altruism" in Darwin and contemporary sociobiological theory.

Experience and Analogy: Darwin and Dugald Stewart.

Daniel Becquemont (Université Lille III)

In his Essay of 1844, Darwin builds his theory on a close analogy between domestication and "natural means of selection". This presentation, which agrees with the "universal analogy of nature" on which the idea of natural religion was founded, lays stress on the image of a creator. In the Origin of species, Darwin displays his analogy on three different levels, and tends to substitute the image of an omniscient creator for natural selection.

In both works, the presentation of the theory and its division into two parts are inspired by Dugald Stewart's theories - supported by Hartley and Prevost - on the the role played by analogy in scientific research, , his views on the relationship between analogy and experience, and his concepts of "negative evidence" and "convergence of probabilities".

Depositions of Paleontology

Contributed papers
Chair: TBA

'One of the most complete and satisfactory chains of consistent circumstantial evidence': William Buckland (1784 - 1856) and the Foundations of Quaternary Taphonomy and Environmental Interpretation.

Patrick J. Boylan (City University, London)

Though transforming geological data into vivid interpretations and reconstructions of past environments and faunas is nowadays regarded as an obvious objective of taphonomy and palaeoecology studies, this was not at all the case in the early years of geology. Like other distinctive visual geological languages used routinely today (such as the creation and interpretation of geological maps and sections) taphonomic and palaeoecological interpretation,

turning often small fossil fragments or apparently insignificant sedimentary or other features into living, roaring or rampaging animals or vivid visual images of past environments, was far from instinctive. The foundations were laid in the early 19th century, above all by William Buckland, the first Professor of Geology at Oxford, the teacher of Charles Lyell, and later a controversial Dean of Westminster. Some of Buckland's series of classic Quaternary case studies are reviewed, including his work in 1821 on the last interglacial (deep sea core zone 5) fossil hyaena den of Kirkdale Cave, Yorkshire, the reconstruction of extinct vertebrates from skeletal remains such as Megatherium, and the interpretation of the environment and fauna represented by Quaternary deposits of northern Britain - first (logically though incorrectly) identified by him as 'diluvium', but later as evidence of a massive continental scale glaciation.

Illustrating Fossil Invertebrates After Darwin: The Stability of Practice.

Mark Hineline (University of California, San Diego)

Descriptive practice in the field of invertebrate paleontological has shown a marked stability through the late nineteenth and much of the twentieth centuries. While the acceptance of evolutionary species change had little effect on description, rapid technological changes in the production of what William Ivins has called "exactly repeatable pictorial statements" opened a number of options for reforming the practice of producing and reproducing illustrations to accompany verbal descriptions of fossil specimens and fossil taxa. Because of their concern for mimetic fidelity in representation, paleontologists have shown a variable but explicit interest in skills and techniques for making illustrations, and in the constraints of printing technologies. Unlike those of other branches of of systematics (such as ornithology), however, illustrations in invertebrate paleontology do not show a clear historical drift to increased schematization or idealization.

Breaking Barriers and Crossing Boundaries: Everett Olson and a Transformation of Twentieth-Century Paleontology.

Ron Rainger (Texas Tech University)

This paper will examine questions of how and why scientists cross disciplinary boundaries by discussing the work of paleontologist Everett C. Olson. Originally a systematist, Olson in the 1940s and 50s helped transform paleontology by developing paleobiology and taphonomy --the study of factors that affect the burial of fossils. Although not the first to examine such problems, Olson played a leading role in developing new approaches and methods and bringing European work, particularly Soviet studies in taphonomy, to the United States. This paper will not focus on Olson's contributions; rather it will examine what social, scientific, and personal factors led him to develop new approaches, and to explore how and why he crossed disciplinary and geographical boundaries to do so. Through analysis of Olson, this paper will raise questions concerning what leads scientists to cross boundaries, what kinds of scientists are boundary crosses, and what circumstances promote boundary crossing.

Leibniz' *Protogaea*: Patronage, Mining and the Meaning of Fossils

Claudine Cohen (EHESS, Paris)

At the turn of the seventeenth and eighteenth century, the question of identification of "fossil objects" as remains of animals which had once lived and as clues for a history of the earth was widely discussed in whole Europe. The interest for "antiquities" buried in the earth met with theological as well as historical concerns. The possibility of building "Theories of the Earth" in the second half of the XVIIth century had followed the Copernican Revolution, and was with no doubt a consequence of a new vision of the world.

Leibniz' *Protogaea* was written between 1691 and 1693, but published posthumously in 1749. Since 1676, Leibniz had been appointed the librarian and counselor of Johan Friedrick, Duke of Hanover, and after his death, of Ernst August of Hanover. In 1680, he undertook to write a history of the Princely House of Brunswick-Luneburg, his charge being to establish its rights and perogatives through genealogy. *Protogaea* was intended to be the preface of this monumental work, and eventually was separately published in its original Latin version in 1749. This text, as indicated in its title, dealt with "the first aspect of the earth," and studied the remains of its most ancient history.

On the other hand, between 1679 and 1684, Leibniz had worked as an advisor and as a mining engineer in the mines of the Harz mountains. Leibniz supervised the draining of mines and tried to imporve production by designing windmills, gearing mechanisms and suction pumps. Although his projects all ended in failure, his mining experience was essential to the gestation of *Protogaea*.

My paper aims to examine the way the writing of this text was not only related to debates which occurred in the natural sciences during this period on the question of the nature of fossil objects and of the history of the earth--but also bears strong connections with Leibniz' historical works and his practices as an engineer of mines.

FRIDAY, JULY 21 14:00 - 15:30

Philosophy of Biological Form I

Organized by Dr. Henk Verhoog (Leiden University) and Brian Goodwin (Open University)
Chair: TBA

Recent discussions within the philosophy of biology show a strong focus on the neo-darwinistic theory of evolution. In this theory biological form is explained as the result of a genetic program designed by natural selection, operating upon hereditary variation in populations of organisms. At least two schools of biological thought believe that natural selection is not the whole story: the structuralist school and the Goethean phenomenological school. Both emphasize the need of a special 'formal cause' or order to explain the origin of biological form. Some structuralists speak about a morphogenetic field of an energetic nature. The phenomenologists speak about the

essential nature of a thing; the order involved is of an ideal, typological nature. In this session we want to explore the differences and similarities between these two views.

Adolf Portmann and the Philosophy of Biological Form.

Henk Verhoog (Leiden University)

There are tensions between the (goethean) phenomenological approach to living form and the structuralist approach. In both approaches 'transformation' of form plays an important role. The phenomenologist emphasizes transformations of form which can be seen outwardly and/or inwardly (imaginatively). Structuralists, on the other hand, tend to focus on morphogenetic fields, which 'structure' material processes in the course of morphogenesis. These fields tend not to be (immediately) accessible to experience. In this paper these tensions will be explored on the basis of the comparative morphological work of Adolf Portmann (1897-1982). In a lecture about Goethe he uses the metaphor of a drama to illustrate two different approaches to nature. The goethean view focusses on what is happening on the stage (gestures, meanings). The other (structuralist?) view looks behind the stage and studies material processes and mechanisms. The way how one relates these views is dependent on philosophical positions.

The Intentional Context of Living Forms.

Ron Brady (Ramapo College)

When Goethe coined the term 'morphology' he said that this science approached organisms as wholes and attempted to grasp their parts in context. The proposed phenomenological problem is a subtle one. Goethe was intuitively aware that nothing is perceived except by the grace of contexting intentions provided by the perceiving subject. He argued that one cannot approach living phenomena with the mode of thought used to grasp the inorganic. Thus, he continued the discussion above by noting that in morphology one did not deal with 'Gestalt' (fixed shape), but with 'Bildung' (formation), which is something quite different. When we examine the intentional contribution required to make this shift, it appears that the problem of organic "form" is not the simple production of shape, but the relations of shape within that context by which formis recognized as organic in the first place, and 'Gestalt' is cancelled.

A Realist Approach to Biological Form.

Gerry Webster (University of Sussex)

Biological taxa are largely constructed on the basis of morphology and such taxa have traditionally been conceived as classes, hence putative natural kinds. This position has raised major practical problems, not least, how to deal with individual variation. David Hull has argued that species and other taxa cannot be conceptualized as natural kinds and that within the theory of

evolution species taxa are conceptualized as individuals. From this perspective, it would appear that a SCIENCE of form is ruled out; the morphological features specific to particular taxa are not susceptible to scientific explanation but must be explained in terms of historical narrative. In this paper I will present an outline of a realist approach to the problem of form in which, via a theory of morphogenesis, taxa might be conceptualized as natural kinds - hence the specific characteristics of their members explained scientifically - and in which variation might be understood as transformation.

Post-WWII Biomedicine II. Post-clonal Bodies: Immunology in the 1960s and Beyond 2

Organized by Alberto Cambrosio (McGill University) and Thomas Söderqvist (Roskilde University)
Chair: TBA

Policing Deviancy: Cytotoxic T Lypmhocytes and Cancer.

Ilana Löwy (INSERM, Paris)

The "cytotoxic T lymphocyte" became prominent in the oncology clinics in the 1970's. However, the proposal that lymphocytes are involved in the destruction of malignant cells was not new: it was made - and confirmed in the laboratory - in the 1910, and then neglected until the 1950's. The growing interest at that time in mechanisms of graft rejection led to studies of cells involved in rejection of grafted tumors. The attempt to introduce bacterial vaccines to cancer therapy favorites the study of cytotoxic lymphocytes, the stimulation of which, it was assumed, explained the anti-tumor properties of bacterial vaccines. The introduction of interleukins to oncology clinics in the 1980's - combined with diffusion of flow cytometry - contributed to the continuation of studies "cytotoxic T lymphocytes", the difficulty to define their contribution to the control of cancer in human notwithstanding. At the same time the focus of researchers' interests shifted from cells to receptors, and from mechanisms of killing to morphological classification. My paper will follow the classifications and definitions of cytotoxic T lymphocytes and relate them to developments in cancer therapy and to industrial practices.

How to Organize an HIV Virus.

Antoinette de Bont (University of Limburg)

In 1989 the World Health Organization (WHO) invited several laboratories to discuss the standards and criteria for the isolation and characterization of HIV. As the identity of the HIV virus remained ambiguous, the laboratories could not decide on a set of standard experiments for identifying the virus. Instead, they developed a database to map all the epidemiological, biological, antigenic and genetic differences among HIV viruses. For the development of HIV vaccines to be possible, the database should not distinguish between more than five or six clearly identified,

representative HIV viruses. The development of an international registry involved a lot of work. To solve the many problems that came up, the WHO showed an unexpected preference for genetic technology. Viruses were selected on the basis of genotype, they were produced by cloning rather than by using infected cells, and all the data were held as flat files, like all well-known genetic databases. Why did the WHO resort to genetic technology? This did not solve the controversy about the efficacy of potential HIV vaccines. Moreover, in comparison with disciplines like epidemiology and immunology the position of molecular genetics within the WHO is rather weak. In this paper I describe how geneticists achieved their central position in the WHO-program: they were able to tame differences.

Discussion.

All Speakers

Politics of Conservation III. Coercive Conservation & Subversive Science 1

Organized by Gregg Mitman (University of Oklahoma)
Chair: Gregg Mitman (University of Oklahoma)

This session explores the relationship between the history of ecology and the domestic/colonial projects of control over territory, resources, people; examines whether ecology has its roots in resource management and whether it is still connected with such roots; reflects critically on whether ecologists are lending themselves to projects that turn out to be "coercive conservation" (Nancy Peluso's term); and explores the emergence of ecology as ideology. Each of the papers in part 1 deals with ecology as a means of political coercion in the first half of the twentieth century, while each of the papers in part 2 addresses the tensions between ecological science and the ideological use of ecology as an instrument of radical politics in the 20th century.

Applied Ecology and Politics: The Ambiguity of Knowledge as Power in Range Management .

Maarten Heyboer (SUNY Institute of Technology)

Ecology and politics have been intertwined in range management since the early twentieth century because the U.S. Forest Service relied on applied ecology to manage ranchers who used the range and to justiy its authority. However, knowledge developed by Forest Service range researchers did not reinforce the Forest Service's political agenda in a straightforward manner because ranchers did not just passively accept that knowledge. In the 1920s, cattle ranchers and sheep ranchers questioned what should count as valid knowledge in range management during a controversy in Utah that pitted both cattel and sheep ranchers against the Forest Serice, and which also continued a long- running struggle between cattlemen and sheepmen over access to the range. The controversy finally ended in 1934, when sheepmen adopted knowledge Forest Service researchers had developed to force the Forest Service to change its policies with respect to cattle

grazing on the range. Analysis of this controversy reveals the ambiguity of knowledge as power because ranchers in effect became members of the community that developed knowledge about the range, a role that was institutionalized in 1948 with the establishment of the American Society for Range Management.

High Over the Borders: Visions of Conservation from Pan-Americanism to the Arusha Conference.

Gregg Mitman (University of Oklahoma)

In the early 1940s, Fairfield Osborn, president of the New York Zoological Society (NYZS), served in an advisory role along with the acclaimed documentary filmmaker John Grierson, for a Columbia Pictures release entitled High Over the Borders. This film was the first of many conservation films Osborn became associated with in the 1940s and 1950s as president of the NYZS and director of the Conservation Foundation. Through the story of bird migration, the film promotes a message of internationalism and thus played an important propaganda role in legitimating the interests of the Pan-American Union that had ratified an agreement in 1940 outlining the establishment of national reserves for resource exploitation and preservation. This paper explores the implicit political ideologies of conservation embedded in the films of the Conservation Foundation and the New York Zoological Society from 1940 to the 1961 Arusha Conference on the Conservation of Nature and Natural Resources in Modern African States and the ways in which these films helped influence within the United States the general public's understanding of the international politics of conservation.

Brushpicking and Biodiversity: The Struggle for Control over Non-Timber Forest Products in the Pacific Northwest.

Rebecca McLain (University of Washington)

Non-timber forest products (i.e. understory plants and fungi) have long been the neglected stepchildren of the forest products policy arena in the Pacific Northwest. Until recently, ecologists focused their attention on the more glamorous woody species, and land managers did little to enforce the few regulations that governed access to NTFPs on state and federal lands. In this climate of benign neglect, local harvesters, including settlers of European descent and Native Americans, staked informal "claims" to huckleberry, mushroom, and ferns patches and developed sophisticated methods of managing these wild plants. In the last decade, however, ecologists' concerns for maintaining biodiversity, coupled with the land management agencies' needs for new sources of revenue, has led to increased interest in expanding and strengthening regulatory controls over non-timber forest products. In my presentation, I will explore the extent to which the proposed new NTFP policies in Washington State and Oregon can be interpreted as yet another example of coercive conservation on the part of ecologists. My work will draw primarily upon field work conducted during 1994 as part of a U.S. Forest Service funded project on NTFP policy development.

Given the growing recognition among ecologists and foresters around the world that NTFPs are often critically important resources for rural community dwellers, I believe that the topic outlined above will interest people from a variety of disciplines and cultural backgrounds. It also is well-suited to illuminating the intersection between technical, social, and environmental concerns.

Perspectives on Museum II

Organized by James Griesemer (University of California, Davis)
Chair: James Griesemer (University of California, Davis)

Philosophy of Museums: Cultural, Historical and Scientific Aspects.

Giovanni Pinna (Museo Civico di Storia Naturale, Milano)

(Abstract not supplied)

Vision, Money, But Above All Science: Creating the Museum of Vertebrate Zoology.

Barbara R. Stein (University of California, Berkeley)

A unusual example of collaboration in biology, and one that departs radically from the notion of scientists with shared goals working together, is the unique relationship that existed between Annie Montague Alexander and Joseph Grinnell. The Museum of Vertebrate Zoology owes its prominence as a center for research in evolutionary biology to the collaboration between these two individuals. Alexander, its founder and benefactress, and Grinnell, the biology teacher who became its first director, recognized the need for a major scientific collection and research museum on the west coast. In addition to this shared vision, each brought to the collaboration flexibility, trust, mutual respect, and strong personal commitment. Each partner also made unique contributions to the effort. What both sacrificed for the venture was autonomy. Alexander, who had no formal training or credentials in science, was dependent on Grinnell to develop the Museum intellectually. Throughout his tenure as Director, Grinnell was required to consult with Alexander on all matters pertaining to the Museum's focus and fiscal management. While significant contributions to science generally are viewed as the result of collaborative efforts among scientists, the successful creation of an intellectual center by Alexander and Grinnell suggests that a re-evaluation of this notion is warranted.

Biology in Nineteenth Century America: The Wistar Museum Of Anatomy.

Simon Baatz (University of Sussex)

The Wistar Museum of Anatomy, originally part of the University of Pennsylvania Medical School, formed the centerpiece of a research organization in Philadelphia created in 1892 under the guidance of Edward Drinker Cope, leader of the neo-Lamarckian school in America. The museum was re-arranged and became the basis for research on Lamarkcian principles led by Cope and John A. Ryder. By 1905 the research program had collapsed and the Wistar Institute came into the mainstream of American biology, focussing on cytological and embryological research.

Biology and Society in an Ethical Perspective: Population, Purity, and Limits to Growth

Organized by David A. Valone (Hamden, CT)
Chair: TBA

This session will seek to explore the ways in which explicitly ethical concerns have been brought to bear on issues that bridge the divide between the biological and the social in three historical contexts. The unifying theme will be the critical examination of the way in which ethics can be used as both a weapon of political partisanship and as a means of defining scientific norms. Kathy Cooke will examine the idea of "purity" as it was used in biological and social thought during the progressive era in America that draws on her research on American agricultural breeders and eugenicists. Irving Elichirigoity will examine the move from an ethics centered around the concept of "man" to one centered on the concept of the "biosphere." David Valone will examine the moral dimensions of T. R. Malthus' *Essay on Populaton*, and explore the ways in which Malthusian ethics were used as the foundations for colonial policy and Victorian "sexual science."

Life Against Life: Sex, Reproduction, Death and Malthusian Morality.

David A. Valone (Hamden, CT)

Malthus's Essay on the Principle of Population has set the tone for discussion of human population growth for the past two centuries. Although the focus of these discussions has tended to fall on Malthus's assertion of the geometrical and arithmetical growth ratios for population and food supply, Malthus repeatedly argues that these rations rarely apply with full force to human societies due to the prevailing "manners and habits." Much of Malthus's concern, therefore, is focused on the creation of a "human science" that can scientifically analyze these factors. This paper will explore Malthus's moral views in their historical context, particularly his easy migration between biological and social ethics focused on considerations of the sexual instinct, the economy of human reproduction, and Malthus's views on death and salvation. The paper will conclude with a brief consideration of the enduring influence of Malthus's moral views, including a brief consideration of the 1994 Conference on Population and ongoing controversies over reproductive technologies.

The Drive for Purity: Agricultural Breeding and Social Reform in the Progressive Era.

Kathy Cooke (Quinnipiac College, Hamden, CT)

In this paper I will examine the idea of "purity" as it was used in biological and social thought during the progressive era in America. The notion of purity is fundamental to eugenic reform efforts as well as agricultural improvement. I will highlight that connection, relating it to changing notions of "good seed" in late nineteenth and early twentieth century agricultural science. Originally good seed was seed that was free of disease and uniform in terms of the kind of plant it represented. However, toward the late nineteenth century the notion became transformed into one of gametic purity, that is, seed of the proper and most pure variety. A similar process took place within stock breeding. The value of such purity in agriculture, I will argue, impacted the perceived need for "purity" in morality and ethics in American society during the progressive era.

The Machinic Components of Planetary Ethics.

Irving Fernando Elichirigoity (California Institute of Technology)

The 17th century poet John Dunne stated that "man is all. He is not a piece of the world, but the world itself; and next to the glory of God, the reason why there is a world". This anthropocentric worldview has dominated Western thinking for the last four hundred years. Recently it has come under increasing challenge. New views, centered on the necessity of preserving the health of the biosphere, have displaced "man" from the center to, at best, the periphery of of more central concerns about the longterm fate of the planet. My paper wil explore some of the strands contributing to this shift, particularly the machinic components underlying this shift, such as computer modeling, systems thinking, and satellite imaging. It wil be my contention that the technoscientific apparatus underlying the new conceptualizations have been underrated.

Racism and Biology (Roundtable)

Organized by Bonnie Blustein (Chicago, IL)
Chair: Bonnie Blustein (Chicago, IL)

During recent years we have witnessed a resurgence of racism and ethnic violence in parts of Europe, Africa, Asia, Australia, and North America. At the same time, academics such as J. Philippe Rushton (Canada) and Charles Murray and Richard Herrnstein (US) have attracted much public attention with purportedly scholarly works promoting biologically-oriented theories of "racial" difference and calling attention to perceived policy implications of their analysis.

Historians and philosophers of science, among others, have meanwhile produced a substantial body of scholarship on what Sandra Harding has called the "'racial' economy of [western] science." We have become more aware of the ways in which the concept of race itself has been socially constructed, of how it has helped to shape scientific practice, and of the profound social costs that have been incurred in its name. This workshop wil incorporate scholarly

and activist perspectives on the uses of biological language and argument in both racist and anti-racist movements today. We are especially interested in making international comparisons, contrasts, and contacts.

Bonnie Blustein (Chicago, IL)

Garland E. Allen (Washington University, St. Louis)

Peter Weingart (University of Bielefeld, Germany)

Leonard Lieberman (Central Michigan University)

Carlota Sole (Universitat Autonoma de Barcelona)

Studies in the History of Biochemistry

Contributed papers
Chair: TBA

The Handicap of Hindsight: Macromolecules and the History of Colloid Chemistry.

Geert Somsen (University of California, San Diego)

The first step on the Path to the Double Helix was the establishment of the concept of the macromolecule. This seems a reasonable statement, and a good starting point for a book on the developments that followed. It does much less so, however, for the history that directly preceded it. Yet, besides Robert Olby, most historians of molecular biology and also polymer chemistry, have proceeded in that way: starting from the acceptance of the macromolecule around 1930, and tracing its pre-history backwards from there. The picture that thus arises is one of a long-fought, difficult struggle of the macromoleculists against their mighty (but mistaken) opponents: the powerful school of colloid chemists, who claimed that the particles under consideration were aggregates of small molecules.

In my paper I want to argue that this retrospective story gives a distorted picture as to the colloid chemists' views and the make-up of their community. It will appear that neither an `aggregate theory' nor any coherent `colloid school of thought' existed. Instead, the colloidolists formed a very diversified group, as a result of their widely differing backgrounds. Using these origins as a point of departure, I will try to get a new understanding of the views of some of the colloid chemists: Wolfgang Ostwald, trained zoologist, `son of', and undisputed leader of the colloidalists; and Jacques Duclaux, his main opponent, but nevertheless not much of a macromoleculist.

Jacques Loeb and the entry of Otto Warburg into the Field of Biological Oxidation.

Petra Werner (Academy of Science, Berlin)

The central intellectual inspiration for Warburg's venture into the field of biological oxidations, appears most likely to have been Jacques Loeb. Warburg took up almost exclusively problems Loeb had defined, organism on which Loeb had performed many of his experiments, he investigated the same developmental phenomena, under the same conditions to which Loeb had subjected them. But Warburg went straight to the most direct quantitative means possible to test Loeb's conclusions. In 1910 Warburg moved away from Loeb's point of view, because he denied the interdependency between oxidations and nuclear and cell divisions which lay at the heart of Loeb's approach; he asserted the primacy of chemical processes over morphological processes. He left the field of developmental biology. The investigation of the process of biological oxidation became the main topic of his research program and further scientific career.

The Centro de Investigaciones Biológicas (CIB) and the Establishment of Biochemistry and Molecular Biology in Spain: Academic Legitimation and Political Support.

María Jesús Santesmases
Emilio Muñoz (Instituto de Estudios Sociales Avanzados, CSIC, Madrid)

Born as a biological research institution in its classical sense in the fifties, the CIB became soon a center for the establishment of modern biochemistry and molecular biology in Spain in the sixties. Political and scientific actors worked together making possible a unique research initiative. Connected with political authorities and competing for resources and academic legitimation with classical disciplines, a set of research groups started doing research in biochemistry and molecular biology at the CIB after the coming back of some scientists from their training period abroad and before their moving to the University. International and national scientific ties made possible this first effort to be part of the international scientific community. The introduction of international norms and standards was the main role played by the first Spanish biochemist and molecular biologists.

FRIDAY, JULY 21 16:00 - 17:30

Philosophy of Biological Form II

Organized by Henk Verhoog (University of Leiden)
Chair: TBA

Thoughts about the Conflict between Structuralism and Neo-Darwinism.

Mirjam Maas (Leiden University)

Structuralism is sometimes interpreted as a theory that can explain the stasis of biological forms through extended periods of time, but unable to account for the rapid evolutionary changes leading to adaptation to the environment. By combining structuralist ideas about individual development with the principle of natural selection, attempts have been made to overcome the felt lack of 'evolutionary dynamics' in structuralism. History is introduced to complete the picture. Although such a move is possible,it is questionable whether this does justice to the more philosophical point that at least some structuralists seem to be making. It could well be that at a deeper level the conflict between structuralism and neo-darwinism is about different metaphysical positions, with different and perhaps incompatible notions of evolution, one seeing it as a creative process, resulting from the activities of individual organisms; the other as a stochastic process.

Selectionist Reservations.

Paul Griffiths (University of Otago)

There are reasons to be concerned about the percieved opposition between structuralist and selectionist explanations of biological form. First, this dichotomous approach to biological causes threatens a recapitulation of the various unproductive attempts to come to terms with the interaction of two classes of causes which was seen in the nature/nurture debate. Second, the erection of this opposition leads adherents of existing Darwinist research programs to treat structuralist approaches as threats to be countered, rather than sources of insight. This is one factor helping prevent the much needed integration of developmental and evolutionary thought.

Fields as Causes of Biological Form.

Brian Goodwin (The Open University)

Within a realist philosophy of science, the explanation of a phenomenon is to be found in the generative processes that produce the phenomenon. One way of understanding biological forms is in terms of morphogenetic fields, whose properties are described in terms of the dynamic behaviour of cells and their molecular constituents. Form is then often described as an emergent property that arises from the activity of this field, specifity of form resulting from the influence of particular genes and environmental influences on the generative field. However, in a phenomenological description form does not emerge out of a lower level of activity but exists already as a member of the transformation set which defines a coherent whole, so that form is made intelligible in terms of relationships within this set. Possible ways of resolving these differences of description and explanation will be discussed in terms of specific biological examples.

European Zoos in the 19th Century

Organized by Donna Mehos (University of Pennsylvania & University of Amsterdam)
and Lynn K. Nyhart (University of Wisconsin-Madison)
Chair: Robert E. Kohler (University of Pennsylvania)

Zoos constitute one of the least-studied institutions in the history of biology, yet they are important sites for historical investigation, for they offer a unique meeting-ground of scientific and popular ideas about animal life. The aim of this session is to explore the place of zoos in the history of nineteenth-century European natural history and zoology. In particular, we propose to look at the ways in which the scientific activities associated with zoos were molded by the expectations of the researchers, patrons, and audiences for what a zoo should be, as well as by the financial, political and practical exigencies these institutions faced. By covering different time periods (early, mid-, and late nineteenth century) and different local stories (Paris, Amsterdam, Frankfurt, and Hamburg), we hope to generate discussion about the variety of roles zoos have played in the development of European biology.

Bringing Life to the Museum: The Early History of the Paris Menagerie, 1793-1838.

Richard W. Burkhardt, Jr. (University of Illinois)

The Paris ménagerie, the first public zoo in Europe, came into being abruptly in 1793 when live animals exhibited in the streets of Paris were confiscated by the police and delivered to the Muséum d'Histoire Naturelle. This necessarily brought a special urgency to the already-debated question of the aims and functions a ménagerie might serve. It is worth contrasting the early answers to this question with actual events and practices at the ménagerie, for neither the animals nor the humans in this story behaved according to script. The topics addressed here will include discussions of the public, political, and scientific functions a ménagerie could serve; the sources from which the animals came; the concert for the elephants; and Frédéric Cuvier's attempts to establish a science of animal behavior under the dual constraints of (1.) studying animals in captivity at (2.) an institution where the observation of living animals was valued less highly than the anatomical study of their cadavers.

Destined for Dissection: Uses of Zoo Animals in Mid-Nineteenth Century Dutch Natural History.

Donna Mehos (University of Pennsylvania and University of Amsterdam)

The course of nineteenth-century European natural history was dependant, in part, upon the availability of zoological specimens. For Dutch researchers, the Amsterdam Zoo became an important source of rare specimens. Like lay members of the Zoo, Dutch naturalists observed the living animals in their zoo lodgings. For these researchers, however,the animals were more valuable upon their deaths when they became available for dissection. They were then compared with similar prepared and mounted specimens in both the Zoo's natural history collection as well as

those in other European natural history institutions. In this paper, I will explore the scientific uses of zoo animals as living specimens, as objects for dissection, and as material complimentary to natural history collections. I will also discuss the ways in which shared hopes for scientific research brought together the Dutch bourgeoisie--members of the private zoo--with local experts who, in turn, forged professional relationships with European naturalists as a result of their access to the collections at the Zoo.

For Heimat and Empire: German Zoos, 1860-1880.

Lynn K. Nyhart (University of Wisconsin-Madison)

Before 1858, the only zoos in the German-speaking lands were those in Berlin and Vienna.In the next two decades, however, German city dwellers caught zoo-fever:groups of enthusiastic citizens founded zoos in Frankfurt am Main, Hamburg, Cologne, Dresden, Düsseldorf, Hannover, Karlsruhe, Münster and Breslau. Focusing primarily on the leading zoos (Frankfurt, Hamburg, and Berlin), this paper explores the meaning of this rash of zoo-development and the ways that it was shaped by scientific questions and theories, the ideological concerns of animal-lovers in a rapidly industrializing economy, and the technical problems of keeping wild animals in captivity.If zoos were founded with such explicit missions as the acclimatization of potentially useful exotic animals or the study of animal behavior, they also contained implicit messages about the place of humans in nature--and the place of German city-dwellers in an increasingly cosmopolitan society.

The New Biology of Development 1950-1985: Historical and Epistemological Approach

Organized by Michel Morange (Ecole Normale Superieure, Paris)
Chair: Charles Galperin (Université Charles de Gaulle, Lille)

The history of the relationship between genetics and development is an old one. However, the work of E.B. Lewis (1951-1963) will mark a breakthrough. Genetic studies of the pseudoallelic series bithorax of *Drosophila* will permit the application of the bacterial operon models.

After an initial phase of enthusiastic reception, these models were harshly criticized. They were considered too simple and conceptually insufficient to explain the mechanisms involved in determination. These criticisms were swept away by the new tools of genetic engineering and the fine dissection of developmental genes by *Drosophila* geneticists.

From homeotic genes to homeoboxes. The main categories of genes which control the steps of development are established between 1978 and 1985. New techniques of genetic analysis reveal a sequence in the homeotic genes: the homeobox. New perspectives will be examined.

The Beginning of Developmental Genetics.

Charles Galperin (Université Charles de Gaulle, Lille)

We intend to present a primarily epistemological inquiry into the beginning of developmental genetics. In spite of the separation between the study of the mechanism of heredity during the first quarter of this century and concepts and methods of the embryologists, the hope to link the study of the genes' action and that of the developmental pattern was not always present. This is true of T.H. Morgan and A.H. Sturtevant among others. We shall recall the main directions of research. We shall discuss only the studies on homeotic mutants by R.B. Goldschmidt and C.H. Waddington. We shall follow the long effort of Ed. Lewis to understand, chiefly through the technique of crossing-over, the mechanisms of the "pseudo-allelic" series bithorax. Lewis' work will really open the way to the new study of developmental genetics. What are the reasons of this achievement? What are its limits? We shall conclude with the models proposed by Lewis to understand the genes' action and especially the application of the regulatory bacterial model of the operon to *Drosophila* (1963).

The Operon Model and Developmental Genetics (1960 - 1980).

Michel Morange (Ecole Normale Superieure, Paris)

In 1961, Francois Jacob and Jacques Monod proposed a model for the mechanisms regulating gene expression in bacteria, called the operon model, which was immediately modified by its authors to explain cellular differentiation.

This model constituted an attempt to fill the gap existing between genetics and embryology. After a short period of enthusiasm, it was harshly criticized because it was considered as unable to explain the phenomena specific of embryonic development, i.e., cell determination.

Between 1970 and 1980, *Drosophila* geneticists established that genes, called selector genes, were responsible for the determination process and for the formation of cell compartments, first sep toward morphogenesis. After 1980, the tools of genetic engineering allowed the isolation and characterization of the first developmental genes which were demonstrated to be, in most cases, genes involved in transcriptional regulation.

Homeosis, Homeotic Genes, Homeobox as Part of an History of Developmental Biology.

Nadine Peyrieras (Université de Marseille)

Molecular Genetics stands at the junction between Genetics and Embryology providing them with tools for common investigations. Developmental Biology emerged from this synergy. The study of homeosis with emphasis on the characterization of the homeobox DNA motif illustrates the history of this novel discipline. William Bateson first introduced the term of homeosis in 1894 in his book, "Materials for the study of variation." Recent work commonly refers to his description of homeotic transformation and acknowledges the later contributions of E.B. Lewis, who has been

studying the logic of expression of homeotic genes lying in the *Bithorax* (BX-C) complex of the fruitfly *Drosophila melanogaster*, for the last fifty years. Beginning in 1984, molecular elucidation of these genes prompted a conceptual revolution in the approach of both ontogeny and phylogeny of organisms.

Politics of Conservation IV. Coercive Conservation & Subversive Science 2

Organized by Gregg Mitman (University of Oklahoma)
Chair: Gregg Mitman (University of Oklahoma)

Conceptual Support Given by Ecology to Alternative Perspectives on Environmental Issues.

Marilia Coutinho (Universidade de São Paulo)

There is evidence of extensive conceptual appropriation between ecology and political discourses committed to alternative political projects. One of them claims the necessity of radical substitution of current patterns of production and transformation of the social structure, since they would be at the roots of our environmental as well as social crisis. The other rejects this thesis and prescribes minor administrative changes, regarding the environmental problems as management problems with management solutions. I call them respectively environmentalist thought and technocratic sustainabilism. The connections between these views and ecological discourse go beyond contingent uses of terms, concepts and ideas and should be referred to the deep structure of these discourses. Analysis of "key concepts" of metaphorical expressions help to highlight such connections.

Radical Ecology and Conservation Science: An Australian Perspective.

Libby Robin (Sir Robert Menzies Centre for Australian Studies)

Scientific ecologists have frequently expressed discomfort with radical ecology and the new environmentalism. It is contended that this is not a matter of `hard science' versus `soft ethics', but rather that the traditional ethical basis for scientific ecology in Australia is incompatible with radical ecology.

Ecology, Science, and Politics in Australia: Green Values in the Lucky Country.

Judith Homeshaw (University of Tasmania)

Australia is a continent the size of the contiguous states of the USA with a population roughly that of the Netherlands, or one state of Germany - Nordrhein-Westphalia. This emptiness has brought special challenges for ecologists. Science has been practised in Australia since white settlement in 1788. Ecologism dates from the nineteenth century when Australia followed

America's lead in establishing National Parks. Paradoxically in Australia, ecologism entered politics before it was embraced by mainstream science. Tasmania, the smallest state of Australia, lays claim to the world's first green political party in 1972. This paper traces the development of ecologism in Australia from the establishment of the first parks to the way the greens saved the Labor government from defeat in 1990.

Discussion.

S. Ravi Rajan (University of California, Berkeley)

Biological Thought in the Social Sciences

Contributed papers
Chair: TBA

The Ecological Legacy of the Chicago School of Sociology during the 1920-1940's.

Stefan Timmermans (University of Illinois)

This paper discusses how the introduction of biological concepts in sociology by early Chicago Sociologists of the period between 1920 -1940 prompted a research tradition in human and urban ecology. In their influential sociological textbook, "Introduction to the Science of Sociology" (1921) Ernest Burgess and Robert Park Attempted to distinguish sociology from the biological sciences by defining human nature as a product of social living. Still, at the same time, these authors introduced biological concepts to describe a sociological research agenda. The most prominent concepts were: ecology, and symbiosis. Students of Burgess and Park, notably Hughes and McKenzie, developed an ecologically based conceptual framework and laid the groundwork for urban and human ecology. In this paper, I research the origins of the ecological concepts, the role they played in sociological debates, and their legacy for the field of sociology.

From the Apes to Angell: Developing a Science of Man at Yale's Institute of Human Relations.

Nadine Weidman (MIT)

I propose to discuss the role of the psychologist Clark Hull in the development of an interdisciplinary science of man at Yale's Institute of Human Relations, from its founding in 1929 to its dissolution in 1952. Hull's discovery of the basic laws of conditioning behavior was the centerpiece of the Institute, its program of unifying the social sciences and its aim of developing more efficient methods of social control. The Institute's sociologists, anthropologists and psychiatrists used Hull's laws of behavior, which they believed to be universal, as the basis for their sciences of society and culture. I will explain how Hull and his fellow social scientists derived

the moral values of human society from Hull's supposedly value-neutral laws of behavior. I will also examine the contested place of biology in the Institute's program of research.

The Role of Selection in the Coevolutionary Process of Gene and Culture.

Jean Lachapelle (University of Guelph)

Most models of the coevolutionary process of genes and culture focus on the mode of transmission and inheritability of cultural traits (Cavalli-Sforza) or memes (Dawkins). But very few of these models try to account for the mode of selection of cultural traits. This is partly due to the fact that we as yet haven't found a causal mechanism that would be to cultural evolution what natural selection is to biological evolution. I argue that, unless these models attempt to explain how selection operates on cultural traits in the coevolutionary process, their explanatory power will remain very limited. Moreover, I argue that it is only by explaining the mode of selection of cultural traits that these models will be able to explain how, and to what extent, the processes of biology and culture overlap and are embedded one in the other.

The Nature of Explanation in Biology

Contributed papers
Chair: TBA

Causal Role Functions in Biological Explanation.

Arno Wouters (Utrecht University)

In his well known paper on "Functional Analysis" (1975), Cummins rejects the idea that function attributions help to explain the item to which the function is attributed. According to Cummins, function attributions describe the role of a certain part or activity in maintaining a certain capacity of a system to which that part or activity belongs.

For example, to say that hearts have the function to propagate the blood is to say that propagating the blood is what the heart does that accounts for the organism's capacity to circulate the blood. I will show that and explain how, in contrast with Cummins's own views, attributions of functions in Cummins's sense are used by biologists to explain the item to which the function is attributed.

From Story-telling to Statistics; the Narrative Nature of Hereditary Explanations.

Carlos Lopez-Beltran (UNAM)

This paper focuses on the replacement of narratives in the field of hereditary transmission of biological and psychological traits by more objective means of securing causal connections during the 19th century in France and England. It tries to show how there is a basic narrative structure underlying all the initial attempts to demonstrate hereditary causal links in the work of authors like Francis Galton and Alphonse de Candolle.

In previous work (Lopez-Beltran, "Forging Heredity...", *Stud. Hist. Phil. Sci.* 25(2), 211-235), I showed that the basic conceptual structure of biological heredity was shaped during the 18th century by medical men around the disputes they had concerning hereditary transmission of disease. I now want to show how the medical tradition of case stories (an excellent conceptual device for handling uncertainty and unpredictability of disease outcomes) was also used for hereditary transmission. Even after the arrival of the "statistical revolution" in the 19th century, when story-telling began to be seen as a bad explanatory strategy, a deep narrative structure always remained in attempts by theoreticians to provide objective, statistical bases for their hereditary claims.

Generalization in Natural History.

Elihu M. Gerson (Tremont Research Institute, San Francisco)

Traditionally, we distinguish between particular circumstances and general principles ("laws of nature"). Such principles are held to be true for all times and places, do not refer to particulars, and are explanatory. Descriptions of particular circumstance, by contrast, refer to individuals, do not apply universally, and are not considered explanatory. This distinction is ineffective as a description of natural history, which has general explanatory principles which refer to individuals. For example, "If x is a mammal, then x has a four-chambered heart" is both a good generalization and a statement about Mammalia, a historically unique clade.

This problem arises because the traditional distinction confounds two different ways of generalizing. One basis for generalization is abstracting (Cartwright 1989). Abstracting creates new kinds of things by (conceptually) removing properties from individuals or more concrete kinds. A different basis for generalization, called "buffering", is typical of natural history. Rather than relying on removing properties from instances and kinds, buffering relies on removing parts from wholes while retaining their individual identities and particular character. The results of buffering are thus concrete individuals (such as holotype specimens). These act as the basis of generalizations through their use in systematic comparison. Recognizing two ways of reaching general conclusions enables a much more effective analysis of the differences among natural history and other modes of research.

Information Generation from Historical Perspectives

Organized by Koichiro Matsuno (Nagaoka University of Technology) and Stanley N. Salthe (Natural Systems, Deposit, NY)
Chair: Koichiro Matsuno (Nagaoka University of Technology)

Information is one of the key concepts in biology, though muddled with much confusion. We will try to put biological information in a perspective that may help us better understand its conceptual uniqueness and versatility. Toward this objective, we distinguish between synchronic and diachronic information, keeping the generative aspects of each in focus. We ask each speaker to address the extent to which the kinds of biological information met with in a particular sub-discipline could be viewed as either synchronic or diachronic. The organizers' hope to achieve a better grasp of biological information of historical origin within the framework of the synchrony-diachrony dichotomy.

New Approaches in Mathematical Biology: Information Theory and Molecular Machines.

Intrinsic Information.

John Collier (University of Newcastle, Australia)

Standard information theory (more correctly communications theory) deals with the syntactic (or formal) information carrying properties of a channel, where information is a reduction of uncertainty. It implies that there is something to be uncertain about, but communications theory never tells us what the object is. A popular version of contemporary genetics maintains that our genes contain information transmitted (with noise) from our ancestors,and that this information is transmitted through development into our traits. Genetic information is partly derived from past environmental conditions, and partly from past self-organisation of the genome, as well as accidental causes that slip through. Similarly, some accounts of knowledge say that information is transmitted from the world to our brains, where it is stored in order to serve our future purposes, along with all our misperceptions and misconceptions. We might ask, where does the information come from, and where does it go? Is information just a relation between transmitter and receiver, just an ephemeral epiphenomenon of a causal process, or is it something more concrete? What is the difference, if any, between storing information and using it to determine the form of some structure? I am going to argue that these mysteries can be solved if we first broaden the notion of information to apply to any determinate form (not just the transmission of distinctions), and then narrow it to include only those forms which have direct physical manifestations. On doing this, we can first objectify relative information as a genuine phenomenon, and then we can define the intrinsic information in an object as its capacity to serve as a source or receiver of relative information. Intrinsic information is an objective property of physical objects, just like their energy, entropy or temperature.

From Absolute to Relative Information: A New Conceptual Challenge for Evolutionary Biology.

Bernd-Olaf Kuppers (University of Jena)

An analysis of the concept of information in biology shows that every kind of genetic information is related to its context, as its meaning can only be expressed in relation to some other information. If the question of the origin of genetic information is not to lead into an infinite regression, we shall have to abandon the idea of absolute information and instead speak of information in relation to its context. It may be expected that this will have far-reaching consequences for the theoretical basis of evolutionary biology.

Naturalising and Explaining Cognition?: Aristotelian Four Causes.

Gertrudis Vandevijver (University of Ghent, Belgium)

The current naturalistic theories of meaning and cognition were developed first and foremost against the background of the formal approach in cognitive sciences. They aim at explaining cognitive phenomena and phenomena of meaning proceeding from their physical-material basis. We propose to analyze, with regard to a subpart of the latter theories, namely those that conceive of the physical and/or biological genesis of meaning and cognition in terms of emergence, the specific interpretation of causality. We start from the four Aristotelian causes, and more in particular from the difference between material and formal causality, as it is currently used in the general theory of evolving systems (cf. S. Salthe, 1993; Development and Evolution. Complexity and Change in Biology). This analysis brings us to a reconsideration of the relation between causality, law and explanation.

SATURDAY, JULY 22 8:30 - 10:00

Quality of Risk-Assessment in Biotechnology: Theory, Practice and Politics of Deliberate Release. III. Practice

Organized by Ad van Dommelen (Vrije Universiteit, Amsterdam)
Chair: Ruth McNally (University of the West of England)

The usefulness of our perspectives is the more compelling since the quality of risk-assessment is of great practical importance, and not just theoretically interesting. Therefore, seperate consideration will be given to the specific problems of uncertain knowledge and expertise that arise in practical risk-assessment situations.

Public Information and Participation in the Context of Directive 90/219/EEC on the Contained Use of Genetically Modified Micro-Organisms and Directive 90/220/EEC on the Deliberate Release into the Environment of Genetically Modified Organisms.

Piet Schenkelaars (MEBO Environmental Consultancy, Voorschoten, The Netherlands).

An overview of legal provisions and other mechanisms for public information and participation within the framework of national regulations on safety in biotechnology in European countries will be presented. The overview also includes the views of national and non-governmental organizations (NGOs) on these possibilities. The overview was discussed at a seminar for representatives of national Competent Authorities (CAs) and NGOs, held in 1994 in the Netherlands. Important issues discussed were harmonization of provisions for public information and participation in the EU member states, approaches in risk assessment and risk evaluation and the potential of biotechnology to contribute to sustainable development. This contribution will give a description of these discussions. Suggestions to improve the quality of debates on risk assessment in biotechnology will be put forward.

Risk Perception and Risk Evaluation.

Beatrix Tappeser and Manuela Jaeger (Institute for Applied Ecology, Freiburg, BRD)

Background of the presentation will be the obvious shifting of risk perception and risk evaluation in the scientific community throughout the last years and the impact this has for the public debate.

The safeguards for production plants are based on assumptions on the viability of GMOs, stability of nucleic acids and the possibilities of gene transfer to endogenous microorganisms. Ten years ago the general view stated, that microorganisms adapted to the laboratory environment will never survive in natural (more hostile) environments for longer periods of time because of their reduced fitnisolated nucleic acids which had been thought to be degraded rapidly in different environments.

The early assumptions on viability and persistence of DNA cannot be maintained. Nevertheless deregulation and reduced safeguards are heavily discussed because of a new interpretation of risks associated with releases of GMOs. The presentation will show the difference between scientific knowledge and the evaluation criteria (the interpretation of scientific knowledge) in the context of risk assessment as a basic reason for mistrust in the public debate.

Commercializing Herbicide-Resistant Crops: A 'Useful Experiment'?

Les Levidow (Open University)
Rene von Schomberg (Tilburg University)

During 1994 the market approval of herbicide-resistant crops sharpened a long- standing controversy on the safety of releasing genetically modified organisms (GMOs) into the

environment. The regulatory procedure entailed arguments among government departments in each country, among member states of the European Union, and among various lobbying groups (industrialists, scientists, environmentalists). For the purpose of reaching an EU-wide decision, how was the 'risk' problem defined? How was 'the environment' conceptualized? What potential effects were considered acceptable or unacceptable? And how does this market approval affect the longer-term prospects for commercializing herbicide-resistant crops? Such questions will be discussed in this paper, which arises from a DGXII-funded study of GMO regulation.

Studying the History, Philosophy and Sociology of Biology

Contributed papers
Chair: TBA

Science and Historicity.

Pascal Nouvel (France)

A comprehensive description of biological objects, such as cells, organisms or species includes an historical dimension. To invoke a single example, the correspondence between a given codon and a given amino acid, the so-called genetic code, is not under a physical constraint but rather appears to derive from events that have been selected early in evolution. On the other hand, the Schrödinger equation, which describes physical objects such as particles or atoms, does not contain any term related to the history of the object. Thus, we propose to discriminate between historical and anhistorical sciences among natural sciences. According to this distinction, biology and physics are "historical" and "anhistorical" sciences respectively, while thermodynamics appears dual. Classical thermodynamics, which focus on stable systems, appears anhistorical, while thermodynamics of irreversible process, which focus on unstable systems, appears historical. The relevance and the consequences of the distinction between historical and anhistorical sciences will be discussed.

Studying Natural Science Without Nature.

Nils Roll-Hansen (University of Oslo)

The paper sketches a critique of so-called laboratory studies of biological science. Attention is focused on the role of the theoretical problems of the scientists and their relationship to the objects under investigation. It is my thesis that both theoretical problems and objects are essential factors in an account or explanation of scientific events. Laboratory studies concentrate on observable social behaviour and tend to overlook these aspects of scientific research. Examples are Latour and Woolgar's *Laboratory Life*, Knorr-Cetina's *The Manufacture of Knowledge* and Michael Lynch's *Art and Artifact in Laboratory Science*. I will argue that behaviourist ("naturalistic") accounts of science often block adequate understanding of scientists'

actions. Without taking seriously scientists' intentions to discover what nature is like much of natural science makes little sense.

Discussion

TBA

Conceptualizing Selection

Contributed papers
Chair: TBA

Sorting Out Optimality Models.

Alirio Rosales (Instituto Internacional de Estudios Avanzados, Caracas)

Optimality models do not come as a single type of model. They are supposed to provide the basis for explanations of adaptive traits in organisms representing a certain relation between natural selection and adaptation under which the evolution of the trait in question remains at a stable rate, such as an ESS. In this paper, I try to characterize optimality models in terms of the way in which fitness is construed in the different causal contexts, life history theory or optimal foraging, for example, with the goal of determining how unified is the picture they provide and how stability criteria capture the causal structure of adaptive evolution. Special attention is paid to the special assumptions under which optimality models are supposed to apply.

The 'Paramount Power of Selection': from Darwin to Kauffman.

Jean Gayon (Université de Bourgogne)

The purpose of the paper is to evaluate in what sense Stuart Kauffman's views on self-organization and selection can or cannot be qualified as "Darwinian".

A first section will recall the meaning of Darwin's statements on the "Paramount Power" of selection, especially in relation with his philosophical conceptions on the status of natural selection in natural history. Darwin used to distinguish between "the mere hypothesis" and the "well-grounded theory" of natural selection. The "paramount power" thesis went beyond the "mere hypothesis", but not as far as the "theory", which implied a full reconstruction of natural history.

In a second section, Kauffman's views on natural selection will be compared with the general strategy used by the pioneers of theoretical population genetics in order to reconcile the Darwinian view of modification of species with modern experimental knowledge. Fisher and Wright thought that they had defined the most general conditions of evolvability. In their (largely common) model, natural selection was not taken as being a priori the major driving force of evolution; but of course, the model left room for interpreting natural selection has being possibly such a paramount

power, under certain conditions. Fisher's and Wright's formal schemes had the major interest of being based on concepts which were all truly biological (mutation, migration, mating system, population size and structure, selection...), but they were rather immaterial in their possible applications to grand scale evolution. In comparison, Kauffman's formal schemes (mainly his "fitness landscapes" and "regulatory networks") are quite independent from biology in terms of their basic conceptual ingredients; they could apply in fact as well to other areas of knowledge. But they look illuminating for the understanding of major features of large scale evolution. Especially, they enlighten problems on which the classical paradigm seemed unable to make predictions, such as: relation between embryology and evolution, genome complexity, major macroevolutionary disruptive events (such as radiative adaptations) or continuous trends (von Baer's law). Kauffman's views on the "limits" of the power of selection will be encompassed in that perspective: the perspective of Darwin's grand "theory".

A conclusive section will briefly compare Kauffman's departure from Darwin with other similar claims in contemporary literature.

After the Special Nature of the Organism: Beyond the Critics of Orthodox Neo-Darwinism.

Elias L. Khalil (Ohio State University)

The paper reviews the unit of selection and the notion of organism debates in relation to the question of whether there is a difference between "organism" and "structure." The term "structure" is used to denote miniature ecosystems of interdependence as manifested in symbiotic relation (like the interdependence of intestinal bacteria and the organism). The paper lays out the classic position of neo-Darwinism as epitomized in the work of Richard Dawkins. It identifies three main challenging approaches: ecological Darwinism, organic- bundle theory, and the punctuated equilibria/developmentalism. The paper argues that while each heterodoxy offers some insight, they generally fail at sustaining the organism/structure difference. There are two options: Either we dispose of the concept of the organism or we have to embrace a new metaphysics which recognizes final causality (a la Aristotle) as the ultimate arbiter between the organism and structure.

Information and Coordination in the Study of Life

Contributed papers
Chair: Helen Longino (University of Minnesota)

Information Technology as an Instrument of Genetics.

Christine Hine (Brunel University)

This paper describes an ethnographic study of the use of information technology (IT) in a laboratory engaged in research related to the Human Genome Project. The use of IT is portrayed

as strategic, facultative and motivated, in contrast to the assumptions of IT developers about their users. The implications of this type of IT use for knowledge in human genetics are discussed, through a consideration of the extent to which IT can be viewed as a scientific instrument. This analysis draws on stories about the role of instruments in scientific discoveries, and on attention paid to scientific instruments by previous laboratory ethnographers.

The Emergence of Artificial Life as a Challenge to the Philosophy of Biology.

Claus Emmeche (University of Copenhagen)

Artificial Life (AL) presents itself as an intellectual emancipation from classical descriptive and experimental biology. The computational study of biology by simulating emergent life-like structures has attracted attention as a promising interdisciplinary field related to physics, biology, complex systems research and computer science. A challenge of AL is to understand the construction of a new object of investigation, not merely life as it accidentally came to be but "life as it could be" or general principles of order in living systems. The strong version of AL, which claim not simply to simulate life, but to realise medium-independent life in emergent computational structures, contradicts the scientific intuition of life as a material phenomenon. The notion of emergence, which plays a role in AL as well as in evolutionary (proto)biology and the philosophy of biology, is examined.

Discussion

TBA

Studies of Genetics

Contributed papers
Chair: TBA

The Mendelian Explanation of Continuous Characters Around 1910, with Special Attention to the Contribution of Tine Tammes.

Ida H. Stamhuis (Vrije Universiteit, Amsterdam)

From 1908 until about 1915 a group of papers was published which argued that some characters are determined not by one but by various segregating Mendelian factors. By this so called multiple factor theory, phenomena, which until then could not easily be brought in the Mendelian scheme, now could be explained in a Mendelian way. The heredity of continuous characters was one of these phenomena. There is said that the Swede H. Nilsson-Ehle has proven this hypothesis, which is not true. Neither the publications of the Americans E.M East or R.A.

Emerson of 1910 were satisfactory. In 1911 an article of Tine Tammes (1871-1947) was published. This publication presented and interpreted experimental data of continuous characters on the basis of the Mendelian multiple factor theory convincingly.

Mutations in the Tropics: Genetics at Private Agricultural Experiment Stations in the Dutch East Indies 1885-1940.

Wim J. van der Schoor (University of Utrecht)

The "rediscovery" of Mendel's laws and Hugo de Vries' mutation theory of 1900 caused much excitement among both scientists and agricultural breeders. Recent literature stresses the importance of the agricultural context in the disciplinary development of genetics.

Many of De Vries' Dutch pupils made their career in tropical, colonial agriculture. They were employed by private agricultural experiment stations in the Dutch East Indies (present Indonesia), which were established from the 1880s onwards on behalf of "European" large scale agriculture.

This paper examines the reception of new theories in genetics at the experiment stations and the way this particular context affected the scientists' research and attitudes.

What is a Gene? A Unified View from the Perspective of Population Genetics.

Peter J. Beurton (Max-Planck-Institut für Wissenschaftsgeschichte, Berlin)

As a result of the discovery of overlapping genes, reading-frame shifts, genes-in-pieces, alternative splicing, and the like, a conceptual crisis about genes has arisen during the last two decades. A single gene may be more or less scattered across the genome and is "gathered together" by the "clever" genome. Because the genome defines the gene in various ways during development, genes are really anonymous stretches of DNA which the experimenter, depending on how he chooses to manipulate the genome, makes use of in various ways and calls genes according to his own ends. The gene is devoid of special reality and is just a word.

This is the view I contest. A more unified view of the gene which harmonizes well with the more recent findings of molecular biology can be established by introducing a population genetic dimension of the gene. A gene is defined as the genetic underpinning of the smallest possible difference in adaptation which selection can detect. Differences in adaptation among individuals, by directing selection towards the genetic underpinning of such differences, may be instrumental to the formation of genes.

Applying Developmental Systems Thinking: Research with Humans

Organized by Susan Oyama (John Jay College, CUNY) and Paul Griffiths (University of Otago)
Chair: Susan Oyama (John Jay College, CUNY)

How do Relationships Grow?: Approaches to Social Systems Research.

Alan Fogel (University of Utah)

The study of embryonic development is focused on an understanding of key events that allow one structure to transform into another. Such key events may be seen as objective entitities standing apart from the embryo or the cell as a coherent structure in itself (mechanistic). Or, the embryo may be seen as incompletely defined, as an open system that requires elements from the context to establish its integrity and allow particular developmental patterns to emerge (transactional). Similar perspectives are found in the study of behavioral development. Yet another conceptual viewpoint may apply both to developmental biology and developmental psychology: one that focuses on the relationships between individual and context (relational). Thus, what develops is not an embryo or a person, but a set of flexibly connected communication processes that over time take on particular forms, leading to the stabilization of dynamically maintained patterns of relationship. I present the details of a research methodology for the study of interpersonal relationship change during human mother-infant interaction. I show how patterns of communication form, stabilize, transform and bifurcate; how each dyad develops a unique character based on the locally emergent features of their relationship; and how we might better understand the key relational events that alter the developmental pathways of the social system. I suggest implications of this method for other levels of biological systems research.

On Cooperative Behaviour: From Physical Systems To Biological Groups And Social Interactions In School

Michela Galzigna (University of Padua)

The most important feature of complex systems is that their function exceeds the simple sums of individual components function. This behaviour characterizes physical systems as lasers, chemical systems such as water and inreasingly complex systems as animal societies. Theoretical paradigms have been worked out to describe cooperativity, independently of the type of individual components of the cooperative system, wich can be composed of atoms (lasers), molecules (water) or living organisms (animal societies) (haken, 1978). As far as social systems are concerned , the effect of cooperation (collaboration) in education seems to enhance learning and increase academic achievement. However, as Olser and Kagan notes "not all types of group work are necessarily cooperative" (1992, p.1), and cooperative learning must be "carefully organized so that each learner interacts with others and all learners are motivated to increase each other's learning" (ibid.). Moreover, educational research has stressed the fact that effective learning involves the so-called "triple alliance", i.e. cognition, metacognition and motivation (Short and Weissberg-Benchell, 1989) wich seems to to be particularly emphasized in the implementation of cooperative or collaborative systems of learning. These seem to be based on forms of altruism and low-anxiety contetxt, in contrast with the traditional "teacher frontal-approach" and with authoritarian methods. Recently Clutton-Brock & Parker (1995) have compared and contrasted altruism with punishment in the enforcement of cooperative behaviour in animal societies. They found that punishment is utilized when dominant breeders coerce subordinated to behave

cooperatively. To what extent this ethological model can be utilized or is even implicit in learning environments will be discussed.

Clutton-Brock T.H. & Parker G.A. "Punishment in animal societies", *Nature*, 373, pp 209-216, 1995
Haken H. *Synergetics*, Springer, Berlin, 1978
Olsen R.E.W-b & Kagan S. "About cooperative Learning" in Kessler C. (ed.)*Cooperative Language Learning*, Englewood Cliffs N.J., Prentice Hall, 1992
Short E.J. & Weissberg-Benchell J.A. "The triple Alliance for Learning: Cognition, Metacognition and Motivation" in McCornick C.B., Miller G. & Pressley M. (eds) *Cognitive Strategy Research: From basic Research to Educational Application*, New York, Springer, 1989

Discussion

Susan Oyama (John Jay College, CUNY)

Studies in the History of Natural History II.

Contributed papers
Chair: TBA

The 'Wider Teleology' and Natural Selection: the Construction of a Darwinian Natural Theology at Oxford 1860 - 1909.

Richard England (University of Toronto)

The relations between clergymen and evolutionists at Oxford changed dramatically in the years between the Huxley-Wilberforce debate (1860), and the 'eclipse of Darwinism' (1890-1920), when Oxford was home to the only selectionist school in England. In reconciling high Anglican theology to Darwinian science, biologists and theologians shared their specialized knowledge in order to reform natural theology. The result was a popular religious interpretation of Darwinism that won support from clergy, scientists, and students, thereby contributing to the success of the selectionists at the university.

I examine this change in various lights: the sharing of professional knowledge to bring about a popular 'synthesis', the interaction between the 'wider teleology' and the development of Darwinian instruction at Oxford, and the passing of the "mantle of cultural authority" (to use Robert Young's phrase) from theologians to scientists in this period. I conclude by describing difficulties with the label "Christian Darwinism", and by suggesting that the relationship between popular and professional knowledge is critical to our understanding of the 'reconcilers' of religion and Darwinian science.

Degenerate Crossings: The Hybridization Debate within British Anthropology, 1863 - 1871.

Vincent Groh (University of California, Berkeley)

Anthropologists and ethnologists in London (ca. 1865) were divided on a seemingly straightforward question: could all human races successfully 'interbreed'? The available evidence, taken mostly from colonial reports, was inconclusive. The issue was fueled by competing interpretations of human taxonomy, and a number of conflicting ideas on inter-racial fertility were developed to support or refute the unity of the human species. The human hybridization issue was given salience by 1) the implications of the Darwinian model had for clarification of Homo Sapiens, 2) close connections with contemporary (non-Darwinian) French anthropologists working on the topic and 3) high profile race struggles in British colonies and race migration concerns in the post Emancipation United States. The controversy was a reflection of larger difficulties between the two existing anthropology societies in London and illuminates the political nature of such organizations in Victorian Britain.

Getting Organized: Nineteenth Century Mediterranean Seaside Studies.

Christiane Groeben (Stazione Zoologica 'Anton Dohrn', Naples)

During the first half of the 19th century a considerable number of German scientists came to Italy to conduct research on the shores of the Mediterranean. These travelers were part of the long tradition of artists, poets, scholars and aristocrats attracted to Italy by traces of the past, art, natural beauty and the Mediterranean way of life. Scientists were drawn to places that allowed them to combine their research with sightseeing. Four cities in particular were favored in this sense: Nice, Trieste, Naples and Messina. Visiting scientists had to face such difficulties as locating equipment and books. In addition, each new arrival had to establish contacts with local fishermen and colleagues.

Among the local contacts in Naples there were Stefano Delle Chiaje, Arcangelo Scacchi, Guglielmo Guiscardi and the two German scientists August Krohn and Rudolph Amandus Philippi. Around 1870 study periods of marine life began to turn from individual explorations into organized and somewhat stereotyped research stays, as individual makeshift labs began to be replaced by seaside institutions such as the Stazione Zoologica.

SATURDAY, JULY 22 10:30 - 12:30

Politics of conservation V

Organized by S. Ravi Rajan (University of California, Berkeley)
Chair: S. Ravi Rajan (University of California, Berkeley)

This session explores the origins and politics of nature conservation in different European contexts. The discussion that follows will examine attempt to locate the loci of interaction between

work on Europe with work on other areas, on the one hand, and between hpssb'ers and environmental historians, on the other.

The Origins of Nature Conservation in Spain.

Santos Casado (Madrid)

I will examine the first initiatives for the protection of natural areas in Spain from 1900 until the Spanish Civil War of 1936-1939, discussing the participation of both scientists and politician as well as the changing ideologies involved in the process, first mostly of a conservative tendency and later more influenced by social thought, specially during the period of the Spanish Republic (1931-1936). I also will study the influence of the North American model of National Parks in the first Spanish Parks and the problems aroused in the adaptation of such ideas to the Spanish context.

The Politics of Conservation in France in the 19th Century.

Patrick Matagne (Tours)

In France, the first set of laws to protect animals, landscapes, and historic buildings were passed between 1850 and the beginning of the 20th century. But naturalists, writers, foresters, and agronomists were worried about deforestation and conservation of plants and animals since the 1820s. They were influenced by romanticism, but the political and economic reasons were also important. Before the First World War, these questions were included in nationalistic propaganda.

An Integrated Conservation Policy for Scotland: A Rhetoric which Belies Practice.

A.M.M. Samuel (University of Lancaster)

Recently the conservation of Scotland's natural heritage has taken on a new direction with the cretaion of a new integrated policy bringing together countryside and nature conservation practices. However, I would like to suggest that a rhetoric of integration belies the social limitations of conservation practice which are creationg the situation where scientific nature conservation techniques are beginning to dominate conservation work at the expense of philosophical countryside conservation tecniques. It is my contention that the interaction between ecological scientists and local interests help maintain nature conservation techniques. I would like to expand and clarify this argument in my paper.

Discussion.

Richard Grove (AustralianNational University)

History of Entwicklungsmechanik I

Organized by Jane Maienschein (Arizona State University) and Garland E. Allen (Washington University, St. Louis)
Chair: Jane Maienschein (Arizona State University)

Commemorating the 100th anniversary of the publication of the first volume of Roux's *Archiv für Entwicklungsmechanik*, and the 95th birthday of Viktor Hamburger

In 1895 Wilhelm Roux published the first volume of his *Archiv fur Entwicklungsmechanik der Organismen*, with its Introduction calling on investigators from many fields to join in the experimental study of developmental phenomena. Roux' program emphasized not only experimental, but also mechanistic approaches to embryology, to the rigorous and analytical dissection of development, particularly differentiation, into its component parts. What exactly did Roux have in mind about his approach, how was the *Archiv* influential in promoting that program, and what were the consequences of Roux' approach on the later development of embryology in the work of Hans Spemann and his students? These two sessions will explore some of these questions as a retrospective on a century of experimental embryology.

Introduction: One Hundred Years of Experimental Embryology.

Garland E. Allen (Washington University, St. Louis) and Viktor Hamburger

In his "Introduction" to Volume 1 of the *Archiv fur Entwicklungsmechanik der Organismen*, Wilhelm Roux called for a reorientation of biologists to the study of embryology: from the descriptive to the experimental approach, and from the qualitative to the use of quantitative and especially analytical methods. In his other writings of this period as well, Roux lays out what amounts to a new research program (paradigm) for developmental biologists. His "Introduction" calls on biologists of all sorts -- botanists, histologist, anatomists (but no mention of embryologists as such) -- to take up the new call and approach developmental phenomena as dynamic, physical-chemical processes, to be studied by the same methods as those employed by physicists and chemists. In this paper we explore the exact nature of Roux' call for a new program - - what exactly he had in mind and how he proposed to gather a following for his approach -- and raise the question of how successful the program ultimately was. To what extent, for example, did it influence the development of Hans Spemann's most influential students, will evaluate the relationship between Roux' program, as laid out in his early writings (c 1888-1895), and the work of Spemann and his students.

The Organizer Effect and Induction: Two Different Morphogenetic Models in Spemann's Scientific Thought.

Peter Fässler (Institut für Geschichte Wirtschafts- und Sozialgeschichte, Dresden)

The discovery of the organizer effect and its conceptual demarcation from the embryonic phenomenon of the induction by Hans Spemann have been insufficiently analyzed by historians of science. This paper shows that Spemann switched to a holistic point of view when he discovered and analyzed the later so-called organizer effect. This switch is only comprehensible by taking the intellectual and social background of that time in consideration. This paper shows also that Spemann did not uphold a consequent demarcation between the organizer effect and induction as biologists and historians maintain.

Julius Schaxel and the Social Relations of Entwicklungsmechanik.

Nick Hopwood (University of Cambridge)

Julius Schaxel, the last of Ernst Haeckel's pupils to embrace Entwicklungsmechanik, announced in 1918 that biology, including his own discipline, was in crisis. He offered a trenchant critique. But Schaxel is of special interest because during the post-war revolutionary crises he came to argue that theoretical reform was not enough: he reckoned the crisis in biology, and specifically in Entwicklungsmechanik, could be solved only by social change. As a rare socialist professor he claimed that only the proletariat (instructed by intellectuals such as himself) should save biology from disaster. I discuss Schaxel's writing on Entwicklungsmechanik both in specialist journals and in the socialist press, and argue that we cannot grasp what was at stake in the discipline unless, like Schaxel, we take these different arenas and the relations between them seriously.

Phylogenetic Developmental Mechanics.

Scott F. Gilbert (Swarthmore College)

In his prologomena on "The Problems, Methods, and Scope of Developmental Mechanics," W. Roux noted that development has both ontogenetic and phyletic components. "Hence," he wrote, "an ontogenetic and phylogenetic developmental mechanics are to be perfected." Most of experimental embryology and subsequent development biology have been concerned with the first of these paths. The latter path, which Roux admitted would have to come subsequent to the ontogenetic approach, has been largely ignored. The reasons for the eclipse of the phyletic program of developmental mechanics and its resurgence within recent years will be the focus of this talk.

Generalization and Unification in the Biomedical Sciences: Prospects and Limits

Organized by Kenneth F. Schaffner (George Washington University)
Chair: Kenneth F. Schaffner (George Washington University)

There has been considerable interest in recent years in the extent to which there are or are not generalizations of broad scope in the biomedical sciences that are analogous to universal "laws" in the physical sciences. The problem has been addressed by biologists, sometimes from the point of view of unification (Kandel, 1987), and sometimes under the heading of "theoretical pluralism" (Beatty, in press). Kandel argues that advances in molecular biology are disclosing a new unity and uncovering common mechanisms, especially in the field of learning. that "can relate what is discovered in *C. elegans*, *Drosophila*, and *Aplysia* to human biology." Other authors, including Beatty, Culp and Kitcher, and the individuals who are cooperating in the proposed symposium, are more skeptical, and see a picture which is much more mixed. Preliminary abstracts from each of the symposiasts sketch their different perspectives on this theme of generality and unification in the biomedical sciences. These studies range from biochemistry and molecular biology through the neurosciences and behavioral genetics to evolutionary theory. Extrapolations from one type of organism to others will be extensively discussed, as will the possibility of applied knowledge, with a special focus on the pharmaceutical sciences. An underlying theme in each of the presentations is that generality, though it figures in much of the rhetoric of the biomedical sciences, frequently has to give way to domain-specific mechanisms, causal systems, and historically-dependent narrative explanation.

The Science and Rhetoric of Unification: Hopkins and Biochemistry.

Harmke Kamminga (University of Cambridge)

Assessments of Cambridge biochemist Frederick Gowland Hopkins have thus far focused primarily on his activities as a discipline builder and advocate of a particular style of 'general biochemistry'. (See especially R. Kohler, 1981: *From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline*.) The chemical study of fundamental biological processes across the living world was promoted passionately by Hopkins in his many public addresses, and became the hallmark of Hopkins' research school in the interwar years. What has received less attention is the way in which Hopkins' vision of biochemistry in practice informed the research being done in his institute in Cambridge, and how that research, in turn, was used by Hopkins to give substance to his promotion of a dynamic biochemistry. A striking feature of both the research programme and the rhetoric is the centrality of unification for Hopkins, at several levels: the unification of chemistry and biology; the unification of organic and physical chemistry within biochemical research; the unification of the whole of biochemistry across the living world; and the unity of science at the level of methodology. Hopkins' endeavors to create and promote 'dynamic biochemistry' as the fundamental science of life will be assessed in terms of his programme's aims, achievements and limitations. The open-ended nature of the unification issue will be explored briefly by way of a comparison with later claims made for molecular biology.

Model Organisms and Generalizations in Behavioral Genetics: the Case of *C. Elegans.*

Kenneth F. Schaffner (George Washington University)

The use of what are termed "model organisms" play a major part in current biomedical research in both its basic and applied forms. *E. coli*, *C. elegans*, *Drosophila*, yeast, and the mouse are also often cited as models for the human genome project. Recent discoveries in cancer genetics exploit DNA sequence homologues in *E. coli* and yeast. All of these inquiries seem to be looking for biological generalizations of some sort, or for useful analogies. The present paper examines one of these model organisms, the nematode *C. elegans*, looking specifically at recent advances in the behavioral genetics of the "worm." Recent advances in the response of the nematode to environmental oderants are described, and the fundamental mechanisms of neuronal function that are shared with other animals are sketched. The behavior is surprisingly complex, and raises questions concerning the easy generalizability to other model organisms, and especially to humans. Some discussion of the special character of the *C. elegans* research community will be addressed, as will the strengths and the limitations of using different model organisms in the light of this exemplar.

Evolutionary Theory as Natural History.

Wim J. van der Steen (Vrije Universiteit, Amsterdam)

Some central concepts of biology do not directly refer to organisms, or features of organisms (or relations among them). Instead they cover diverse features that cannot be specified by definitions. Examples are stressor, natural selection, fitness in the engineering sense, also called fitness in the propensity sense, a so-called supervenient concept.

General statements containing such concepts are best interpreted as statement schemes which need implementation before they can do real explanatory work. Upon implementation, the statements loose their apparent generality. This restricts the generality of theories and explanations in biology, not least evolutionary biology.

Take engineering fitness. (This concept must be distinguished from "fitness" in population genetics, which is in some way defined as reproductive survival.) The thesis that differences in fitness cause differences in reproductive survival can be unpacked as follows. If organisms of type A (in any population, in some environment) have a higher fitness than organisms of type B (in the same population and environment), then there are features of A and of B such that reproductive success will be greater in A than in B. To avoid circles, "reproductive success" must not be mentioned in the definition of engineering fitness. In fact we can't give any definition since the concept is supervenient.

The existential quantifier shows that we are indeed dealing with a statement scheme. To get a real statement, and a real explanation, we have to mention particular features. Thus we get natural history, not general theory. Likewise for the idea that natural selection causes evolution. The phrasing suggests that selection is a factor that causes evolution. It isn't. Factors such as temperature and humidity do the causing. Theses about natural selection point to diverse factors

and diverse processes in an abstract way. Instead of qualifying as natural laws they summarize natural history.

Unification and Diversification in the Biological and Biomedical Sciences: Profiles as Communication Nets Between Scientists.

Rein Vos (Groningen University)

Recently, philosophers such as Hacking (1983), Schaffner (1986,1993), and Culp and Kitcher (1989) are calling for the exploration of the complex structure of scientific practice. Culp and Kitcher (1989) argue that current approaches to theory structure and change in science do not hold when confronted with the practice of science, most notably the practice of the complex science of molecular biology. In fact, they stress that the word 'theory' should be abandoned and that what seems to them "vastly more significant is the delineation of the ways in which the practice of biological science achieves generality" (Culp and Kitcher, 1989, p. 461).

This quotation might be understood in two principally distinct ways. One is monistic, i.e. that there is one sort of generality to be found characteristic for the biological sciences, albeit (more or less) different from the theoretical structure of physics and chemistry. The other way is pluralistic. The latter one is essentially implied by Culp and Kitcher. Once the epistemic space in biology is differentiated and compartmentalized, the question becomes what different ways of achieving generality are conceivable and how scientists communicate with each other in performing these different activities.

This paper deals with an important way of achieving generality in the biomedical and pharmaceutical sciences, i.e. the representation and use of knowledge with the help of drug and disease profiles. The characteristics of storing knowledge and experiences about disease and therapeutic outcome in profiles are analyzed. It is claimed that profiles as one specific form of cognitive structures and procedures are used as 'communication nets' between different specialists in the life sciences for bringing presumably relevant but distinct items together. That is, information collected at different switches in the causal chains and at different levels in the organism which form the linking fabric between the biochemical, physiological and environmental processes, can be interrelated in this way.

Molecular Sociology: Developments, Applications and Controversies Associated with DNA Technologies

Organized by Michael Lynch (Brunel University)
Chair: Michael Lynch (Brunel University)

The four papers in the session are ethnographic studies which examine specific sites where new DNA technologies are being developed and applied. Consistent with the perspective taken in recent sociological and anthropological investigations of science and technology, the studies show how contexts of 'application' are also sites where novel initiatives and problems

arise. Substantive medical or legal problems are not simply 'external' or 'secondary' to molecular biological knowledge. Instead, a variety of ethical, legal, and argumentative considerations associated with medical or legal practice are responsible for reconfiguring the significance and substantive organization of DNA technologies.

The Management of Diversity in the Development of Gene Therapy for Cystic Fibrosis.

Alan Stockdale (Brandeis University and Dibner Institute)

Research into the development of gene therapy for the treatment of an assortment of diseases has burgeoned over the last few years. In this paper, I examine the development of gene therapy for cystic fibrosis, one of the first diseases to be targeted by this nascent therapeutic strategy. The development of this technology is located in the interplay of a complex variety of relationships. The methods that different groups and individuals use in attempts to integrate and manage this diversity of relationships are described and analyzed.

DNA Profiling in Criminal Forensics: Practice & Controversy.

Kathleen Jordan (Boston University)

In the United States, DNA profiling techniques have been used in the practice of criminal forensics since 1988. Small traces of biological substances in association with crime scene objects and fluids are now recognized as being potentially viable evidence. This changes and challenges the course of forensic investigation for evidence-response teams and laboratory research teams who collect, examine, and analyze criminal samples. The crafting of narratives in legal testimony about evidentiary representations has also changed in the wake of these novel procedures. DNA profiling is fraught with a series of controversies. Issues of contamination abound, whether at the crime scene, through the 'chain of custody' in the transfer of evidence, and at the laboratory bench. DNA profiling is also subject to robust debates in population genetics. Lively argument are pursued regarding statistical calculations and measurements used to arrive at estimates on the frequency of shared prototypes. Furthermore, the admissibility of DNA evidence, as a new scientific innovation, remains contentious in the courts. This research is an account of the present state of DNA profiling techniques, and gives an historical overview of how controversies surrounding these technologies evolved and developed, how they were resolved by different groups, and how they have been resolved (or not).

Cross-Examining Science: DNA Evidence and the Courts.

Michael Lynch (Brunel University) and Kathleen Jordan (Boston University)

DNA 'fingerprinting' and other methods of genetic profiling have been used in hundreds of US and UK criminal trials over the past eight years. Although in the vast majority of cases, results

of DNA profiling have been accepted as evidence, successful challenges occurred in several well-publicized cases. Transcripts of such cases provide rich sources of data on how the 'same' scientific technique can be made out to be a simple, carefully performed, and certain procedure, or a comedy of errors and a locus of methodological horrors. These opposing versions often arise in vivid detail when expert witnesses are cross-examined by well-informed attorneys. Such exchanges are especially interesting in light of science studies debates about the 'construction' of facts and artifacts. We shall try to avoid discussing the O.J. Simpson trial, but we may be unable to resist.

Mapping the Human Genome at Genethon Laboratory: A New Organisation of Work in Biology for a New Patient.

Alain Kaufmann (Université de Lausanne)

The most important results of the French human genome project come from an unprecedented collaboration between a patient organization, the French Dystrophy Association (AFM), and the Centre for the Study of Human Polymorphism (CEPH). These two organizations have established Genethon Laboratory, which is responsible for the production of human genome maps of a quality and a performance unrivalled in genetic research. This work is financed through the French Telethon: a yearly, 30-hours-at-a-stretch, TV show aimed at the collection of private funds. Our paper is based on a one year field study at Genethon Laboratory. The analysis of the production, use and communication of the human genome maps shows that a completely new type of semi- industrial laboratory has emerged. The "Genethon adventure" results from the unique convergence between a specific social group--patients affected by muscular dystrophy and their families--and the "universal" goals of a researchers' and clinicians' community. Hence, these maps of the human genome can be considered as a hybridization between a new way of doing biology, a wide mediatic enterprise and the emergence of a "new patient," who actively participates in the production and transformation of its genetic identity.

Neuroscience

Organized by C. U. M. Smith (Aston University)
Chair: TBA

This session considers aspects of the history, philosophy and sociology of the neurosciences. We start with a case study of work at the interface between neuroscience and psychology. Ed Manier will discuss the philosophical and social dimensions of Kagan's early studies of the inheritance of 'shyness' with reference to possible underlying neural mechanisms. Zlatko Anguelov takes another case study which illuminates the social dimensions of neuroscientific research. He will discuss how neuroimagers, especially those using PET, work to shape their communications to serve a double role: to fit the great corpus of received knowledge of neuroanatomy and neurophysiology and to develop new more mathematical interpretations. Finally, Chris Smith re-examines the issue of brain complexity and consciousness. Starting with a

brief historical review of nineteenth century thought, he considers how our understanding has grown in the late twentieth century. Are we any better able to answer Coleridge's ridicule in the *Biographia Literaria*?

Biopsychiatry and the Development of Temperament.

A. Edward Manier (University of Notre Dame)

"Biopsychiatry and the development of temperament" deals with the epistemic and social contexts of Jerome Kagan's research on the early epistemic and social contexts of Jerome Kagan's research on the early expression and persistence through adolescence of a "behavioral inhibition to the discrepant" or shyness. Early publications of the results of the MacArthur Longitudinal Twin Study indicate behavioral inhibition is more highly heritable than any other trait studied and Kagan is proposing a central nervous system mechanism underlying this disposition. Kagan's work offers an opportunity to examine "unstable ideas" at the frontiers of neuroscience and psychology.

Technological Tides: Are There any Asylums of Uncertainty Left?

Zlatko Anguelov (McGill University)

My talk will be a reflexion on how images in brain research generate a neoanatomical discourse. Data collected from the Brain Imaging Centre at the Montreal Neurological Institute enable me to argue that users of PET struggle to keep their dominance over the powerful messages that PET functional images convey. The meaning they assign to those messages has a double role: to justify interpretations, which fit the previously accumulated knowledge about brain cytology and chemistry, and to protect the high social status of the imaging community.

The context in which they have chosen to locate this meaning is anatomy. But in contrast with classical anatomy, the neoanatomy is uttered in a mathematical vocabulary, which reflects the measuring and quantifying nature of the image-producing technique. This anatomy is ambiguously functional, but its epistemological strength seems to underlie a strategy for coping with the metamorphoses medicine undergoes in an era of knowledge uncertainty.

Brain Complexity and Consciousness.

C. U. M. Smith (Aston University)

The problem of mind has recently been subdivided into 'hard' and 'soft' questions (1). The soft questions have to do with the explanation of behaviour in terms of the neuroscience of the central nervous system. The hard questions have to do with the relationship of subjectivity, qualia, to the goings-on inthe brain. This paper addresses the hard questions. It provides an historical review of the argument that consciousness is in some way connected to brain complexity. It starts

with the work of Oken, Spencer, Lewes, Owen, Huxley and others in the nineteenth century and ends with a review of late twentieth century insights into brain complexity. Has the argument become more convincing in the nearly two centuries since it was ridiculed by Coleridge? Is the state of matter in the brain so different from that found elsewhere that consciousness 'emerges'?

Neurosciences and Reductionism. An Edelman's Theory of Consciousness Analysis.

Bernard Feltz (Université Catholique de Louvain)

Neuroscientist G.M. Edelman recently proposed a book where he developed *A Biological Theory of Consciousness*. While the title could indicate a reductionist position, a most precise analysis could conduct to other conclusions. First, we will propose a synthetic presentation of Edelman's scientific conception. Second, we will analyse the Edelman's philosophical conceptions at the anthropological and epistemological points of vue. Third we will confront Edelman's conception to the reduction's problematic in relation with the concept of idiosyncrasy and intentionality. The hermeneutic circle will be finally proposed for a characterisation of the global Edelman's approach.

Biology, Medicine, and Their Interface: A Retrospect on the Work and Life of Roselyn Rey (1951 - 1995)

Organized by Pnina G. Abir-Am (Boston University)
Chair: F. L. Holmes (Yale University)

The recent death (Paris, January 15, 1995) of Roselyne Rey, a CNRS senior research associate affiliated with the Centre Koyre for History of Science in Paris and a teacher of the history of biology at the Ecole des Hautes Etudes en Sciences Sociales, has deeply shocked her colleagues not only in her native France, but also in UK (esp. London, Manchester, Oxford) and US (esp. Yale, UCLA, Chicago) where she gave lectures in 1993 and 1994 and enjoyed wide admiration.

Roselyne Rey's versatility ranged from the history of French biology, expecially in the 18th and 19th centuries, to the history of the interface between biology and medicine, most notabliy in her book, A History of Pain (1993). A conceptual histoyr of pain from ancient times to the present (which takes renewed relevance from the author's own heroic struggle with a malignant affliction for the preceding four years), was published by Decouverte in both French and English; an American edition is under way by Harvard University Press. Reviews have begun to appear, just when the author is no longer alive to enjoy her colleagues' written appreciation of her accomplishment and scholarly dedication (the 1994 ISIS Cumulative Bibliography lists no less than five items under her name; most historians of science have one entry).

Introduction.

F. L. Holmes (Yale University)

Charles Darwin and Charles Naudin: Mosaic Inheritance and the Question of Hybridity.

Joy Harvey (University of Cambridge)

The complexities of the relationship between hybridism and inheritance have been studied by Robert Olby among others. Many nineteenth century figures tried through experimentation on plants to map out these complexities and arrive at an adequate theory that would explain the reappearence of parental characters. Naudin was one of the first to insist on the importance of segregation of characters in a pattern that he called "mosaic." Recent work on the correspondence between Charles Naudin and Charles Darwin and the experimental notes and book and pamphlet annotations made by Darwin have illuminated some of the problems inherent in the work of these two men. This paper attempts to use that new material to understand how Darwin, tied into one style of experimentation, responded to the different experimental style and conclusions of Charles Naudin. This paper is also conceived of as a memorial to Roselyne Rey, an historian of science and medicine with an unusually wide range to her interest and her work who began but did not complete her work on Naudin.

Drug Discoveries Between Biology and Medicine: The Case of Vitamins and Hormones.

Christiane Sinding (INSERM, Paris)

It is often said that drug discovery should and will be an entirely rational process based on results produced by basic research. Discovery accounts by scientists and some historians are generally constructed in such a way as to make the "discovery" look like a direct and logical outcome of basic research. By analyzing briefly the case of hormones and vitamins discovery, I intend to show that 1) very often drugs and remedies come first, rationalization second; 2) drugs are often used as "dissecting" tools for diseases, so that morbid entities are constantly reconstructed according to the effects of drugs; 3) in turn the effects of drugs are redefined by the results obtained. Finally, drugs are not stable substances which are produced by goal-directed research and possess precise and well defined effects. On the contrary, they are very often discovered empirically and possess a wide range of effects, some of which are emphasized by physicians as specific positive and negative, and constantly reshaped.

The Making of the First Miracle Drugs: A Comparative Perspective.

Jack Lesch (University of California, Berkeley)

It took two years for Prontosil and sulfanilamide to become miracle drugs. The end points of the process are roughly Gerhard Domagk's first publication on Prontosil in February, 1935, and the Paris international exposition and American Medical Association Atlantic City meeting in 1937. The process of becoming was different in France, Britain, and the United States. The differences are revealing of national differences in the relative closeness with which pharmaceutical developments in Germany were followed by medical communities in the three countries; the relative credence given to claims made for new drugs of whatever provenance; the way new drugs were evaluated; relations between the pharmaceutical industry and the medical community; the status of German patent law; and the role of the popular media.

The Construction of Collective Memory in Molecular Biology: Historiographical Implications of Anniversary Rites in US, France, and UK.

Pnina G. Abir-Am (Boston University)

The role of scientific anniversaries and commemorations in the construction of scientific memory has only recently been systematically examined from a joint historiographical and ethnographic perspective (e.g. the special issue -#4- of *Social Epistemology*, 1992). Roselyne Rey was a speaker in a 25 participant symposium on this topic, held in Paris in December 1993, while also serving as a valued advisor. Her highly original contribution discussed the French Assembly's votes against commemorating the 18th century encyclopedist Dennis Diderot, thus highlighting the political uses of scientific commemorations. As late as December 18, 1994, or less than a month before her swift death, she expressed a firm desire to participate in a similar symposium, held in Boston in April 1995.

This paper explores the genre of anniversary and commemorative rites in molecular biology, while focusing on a comparison of anniversary rites held in US, UK, and France for founders of research schools. The paper pays attention to historicity or the meaning of temporal distance between the timing of the commemorative occasion versus that of "historical" event selected as an object of commemoration; cultural context, or to what extent commemorative rites in science reflect not only its universalistic ethos but also cultural-national traditions; and political usage, or why and how certain groups and individuals are excluded from the construction of collective memory.

SATURDAY, JULY 22 14:00 - 15:30

Botanical Field Work in the 19th Century: Practices and Understandings

Organized by Jane Camerini, Madison WI
Chair: Malcolm Nicolson (University of Glasgow)

These papers each deal with a different pieceof fieldwork practice, ranging from the question of what actually goes on in the field, to how data are collected, compiled, and used, to a

comparison of the observational and experimental methods in the analysis of plant distributions. While natural history is commonly understood as a science of observation, these papers taken together spell out a complex process that begins with what people see when they "botanize" and follows representations of plants from a particular place through their transformations to numbers and indexes and thence to comparisons, interpretations and understandings. What it is that 19th century Europeans scientists and field workers came to know about the vegetation of the world is examined from several points of view, with a common interest in the transformation of local to global visions of plant geography.

Quantification and Plant Geography in the 19th Century.

Jean-Marc Drouin (Museum National d'Histoire Naturelle, Paris)

This paper analyses some of the attempts of the botanists of the nineteenth century to introduce quantification in plant geography. Humboldt's tables of distribution of the families in the different parts of the world and Watson's statistics of British Flora are two outstanding landmarks in this program of research, but some French speaking authors also made interesting proposals. The navy officer and traveller naturalist Dumont d'Urville in his *Flore des Malouines* (1825), intended to find numerical ratios to quantify distribution of botanical taxa in a given country. Henri Lecoq, professor of natural history in Clermont-Ferrand, proposed in his *Etudes sur la géographie botanique de l'Europe* (1855, vol.4) to reduce the area of a species to a theoretical square measured in degrees of latitude and longitude. The same year, Du Colombier, a telegraph engineer, published in the *Bulletin de la Societé Botanique de France* a more sophisticated formula which was supposed to allow a comparison between the richness of the floras of two different countries. Most of these authors are reviewed in a thorough and comprehensive paper by John Briquet of Geneva, published in 1893 in the *Bulletin del'Herbier Boissier*. The point of my paper is: were all these indexes used in fieldwork or were the only means of interpretation of already obtained data? Were they theoretical achievements or just a way to mimic physical sciences?

Alexander von Humboldt and the Epistemology of Natural History and Experimentation.

John Huss (University of Chicago)

Many historians have linked Humboldt with the Natürphilosophen both for his personal ties to Schiller and Goethe and his appeals for an aesthetic appreciation of nature. Yet his innovations in method and his thorough-going empiricism mark him as perhaps Europe's pre-eminent natural historian of the early to mid-nineteenth century. Historians have used his double-barrel epistemology to ally Humboldt with the Natürphilosophen, but also to distance him from them. In my paper, I explore the basis for the distinction Humboldt scholars have made (sometimes tacitly, sometimes explicitly) between what seem to be straightforwardly empirical observations (e.g., temperature readings) and emotional or aesthetic responses in his work. His very desire to unite the empirical, emotional, and the aesthetic illustrates that Humboldt was aware of the differences between them, but what was their basis? On the one hand, perhaps it was a cultural difference

between Natürphilosophie and Natürgeschichte. Schelling reacted against the cold meticulousness he saw in Humboldt's careful compilation of measurements, but for Humboldt, emotional responses to nature were a complement to, not a substitute for the measurements and other empirical observations whose synthesis he sought to achieve. On the other hand, the distinction between different modalities of observation may have reflected the methods and technology of the day. Humboldt's painful self-experimentation in his galvanic research suggests that the body itself could serve as a measuring instrument. I situate the status of objectivity and subjectivity in relation to the use of techniques of measurement and observation at this time. My analysis of Humboldt's experimental and natural history researches grounds his epistemological project in the changing practices of quantification, measurement, and description that characterized the early nineteenth century episteme.

Discussion.

Jane Camerini (Madison WI)

Biosemiotics I

Organized by Manfred Laublicher (Yale University)
Chair: TBA

Phenomena such as the context dependency of biological processes, the issue of emergent properties and the hierachical nature of biological systems, the interlevel character of biological theories and the discussion surrounding reductionism in biology are constantly attracting attention. In recent years some of these questions have been dealt with from a semiotic perspective. Semiotics, the study of signs and their action, can provide some fresh methodological insights as to how the issue of context dependency can be approached. The objective of this session is to explore the potential gains for our understanding of biological processes that could arise out of an application of semiotic principles such as the Peircean triadic model of the sign. Furthermore, the implications for the philosophy of biology will be discussed. In addition, related approaches to the problem of context dependency should be investigated.

The Riddle of Context Dependency: How Semiotics can Inform Biological Theory.

Manfred Laublicher (Yale University)

Context dependent phenomena are universal in biology. The standard procedure for dealing with the issue of context depency has largely consisted of concillatory lipservice and otherwise ignored the issue. Recent developments in various biological disciplines point towards a reversal of that position. This paper will investigate one issue central to many biological theories, namely, the relation of entities and processes. It will be argued that the context of the specific

processes defines the entities involved in that process. As an example, the process of selection will be analyzed. It will be argued that the "unit-centered" view of selection is the product of a conception of biology that largely diminishes context dependency. An alternative based partly on semiotic principes will be presented.

A Structuralist Approach to the Character Concept in Evolutionary Biology.

Gunter Wagner (Yale University) and Junyong Kim (Yale University)

In this contribution an approach to the clarification of biological concepts is presented. This approach has been informed by paradigms of successful concept formation, in particular the species concept and the fitness concept. The unique aspect of the present approach is the consequent structuralist style, which seeks to identify the meaning of a concept in formal structures regardless of their ontological implications. The basic assumption is that characters need to be identified in the context of the processes in which they act as an unit. In evolutionary biology, characters are partitionings of the organism which act as units in three contexts: variation, selection and transmission. In each of them a topological structure can be constructed on the set of organisms and the meaning of the character concept is derived from the geometrical features of the induced topological structures.

Semiosis and Coadaptation.

Kalevi Kull (University of Tartu)

According to J.Lotman, semiotics should become into exact science, connected to biology and history. By T.v.Uexkull, the dualism of body and soul might find a biosemiotic answer. After E.Baer, adaptation is a semiotic phenomenon, a process of signification.

The darwinian description of the mechanisms of adaptation is too simple to be able to explain the evolution of organisms towards the evolvement of languages. All known biological mechanisms which stand behind progressive development are coadaptive and have much more complicated structure, containing, at least, (a) two reciprocally recognizing adaptive devices, (b) dynamic memory(ies) with code duality, (c) two-level self-reproduction, (d) selection minimizing the negative effects of competition. Thus, we may get at a construction of semiosis. The organism as a semiotic integrity contains several levels of semiosis (enzymatic, clonal (e.g., immunological), sensoric, emotional, representational), each of them using different memory and giving a new dimension of freedom to connect things which would not be connected before or through their physico-chemical affinity.

Politics of conservation VI

Organized by S. Ravi Rajan (University of California, Berkeley)
Chair: S. Ravi Rajan (University of California, Berkeley)

Historical Dilemmas in Conservation of Common Nature.

Jos Dekker (Utrecht University)

In the Netherlands conservation strategies for common nature are a subject of debate. Different strategic choices have their roots in historical dilemmas. Nature conservation policy concentrates on preservation and development of (semi) natural areas in the main ecological framework at the benefit of nature of special interest. Outside these areas, there is common nature in the countryside, in built up areas and along transport infrastructure. This common nature has been valued for a lot of reasons but it is in danger of being neglected. In this paper we focus on the strategic debate about the management of common nature in the countryside. Conflicting strategies are proposed and pursued. Main strategic choices concerns are: is common nature of any value or not or isn't it nature at all; ecological versus activity standards; the reference (past, present and future); standards versus (other) instruments; voluntary versus compulsory; with payment or without. The debate has a long tradition in private conservation organisations. The value of nature in agricultural landscape and the kind of management strategy have always been points of debate. These questions reflect different concepts of nature in relation to different kinds of strategies towards agriculture which change in time. The basic dilemma seems to be about the relation man may have with nature.

Discussion.

Gregg Mitman (University of Oklahoma)

Discussion.

S. Ravi Rajan (University of California, Berkeley)

Discussion.

Peter J. Taylor (Cornell University)

History of Entwicklungsmechanik II

Organized by Jane Maienschein (Arizona State University) and Garland E. Allen (Washington University, St. Louis)
Chair: Garland E. Allen (Washington University, St. Louis)

The Experimental Mission in Embryology: Two Waves of Contrasting Concerns.

Frederick B. Churchill (Indiana University)

The onset of modern experimental embryology has been closely associated with the investigation of Wilhelm Roux, Hans Driesch, Thomas Hunt Morgan and Oscar Hertwig. Later practitioners in the field, such as Ross Harrison, Hans Spemann, and Charles Manning Child have been conventionally portrayed as heirs to this tradition. The author intends to challenge both of these historical claims. First, he will argue that the initial thrust into experimental embryology was not confined to a few well known and highly publicized events and practitioners but occurred on a broad research front that consisted of dozens of investigators pursuing a set of common goals. Second, he will argue that the second "generation" of investigators, representing even a broader wave of experimenters, had their own quite distinct agenda. This second wave changed experimental embryology from a nineteenth century subfield of morphology to a twentieth century subfield of physiology. The author will draw on both narrative and statistical information in his presentation.

Hunting the Organizer: They Sought It With Care, They Pursued It With Hope.

Jan Witkowski (Cold Spring Harbor Laboratory)

There are some experiments and discoveries in biology that seize the imagination. One such set of experiments were performed in 1921 and 1922 by Hilde Proescholdt in the laboratory of Hans Spemann, her Ph.D. supervisor. Here the transplantation of a small portion of tissue from the dorsal lip of the blastopore to another embryo led to the development of second body axis. This was a remarkable finding and one that promised to open up the secrets of embryonic development. Key questions concerned the nature of the organizer and how it produced its effects, and searching for the answers to these questions has taken 70 years. The search has depended on an interplay of classical techniques for manipulating amphibian embryos with successive new technical developments. This has culminated in analyses using recombinant DNA tools to dissect the complexities of early embryonic induction, revealing Spemann's organizer in undreamt of detail. Viktor Hamburger wrote of the challenge of "...redefining the old problems in the new language of cellular and molecular developmental biology..." and the need for scientists "...to address them with the sophisticated methodology at their disposal". The story of Spemann's organizer shows that while the problems were couched in different languages at different times, scientists, including Hamburger himself, have repeatedly risen to the challenge.

Discussion.

Jane Maienschein (Arizona State University)

***The Advancement of Science (AS)*: Where's the Scoreboard?**

Organized by A. Edward Manier (University of Notre Dame)
Chair: TBA

P. Kitcher (in *The Advancement of Science*, 1993) "dissolves" a rationalist model of the closure of scientific disputes into two stages: one proliferates a variety of plausible candidates, reflecting encounters between scientists and nature and among scientists themselves; stage two closes disputation when a consensus practice incorporates those empirical, technical, and methodological assets judged most likely to sustain or promote the advance of inquiry in the field (the "External Standard," ES). We offer fresh case studies to probe the perspectives from which ES is formulated and used to assess the disputants. Second, the closing chapter of *The Advancement of Science*, unlike every other, is a formal argument, unsupported by any case study. The chapter defines several social categories, e.g., 'cynic,' in theoretical terms without addressing issues which arise in their empirical application. A third paper examines such difficulties and the role of chapter 8 in *The Advancement of Science.*

'Minimal Darwinism' and the Rise of Experimental Embryology.

David Walton

In *The Advancement of Science* Philip Kitcher argues that biological practices between 1860 and 1930 were shaped by an emphasis upon historical narrative (phylogenetic histories) as the method and goal of biological research. This Minimal Darwinism, Kitcher argues, drove biological research toward the post-1930 synthesis of embryology, genetics, and evolution. I argue the dispute concerning the epistemic status of historical narrative as a guiding principle in biology continued after 1880. The increasing emphasis on experimental work largely abandoned historical narrative for analysis of the immediate causes of reproducible phenomena; and many young biologists sought distance from the excesses of earlier, historically oriented work. This paper examines the rise of experimental techniques in embryology to address more directly possible limits upon the influence of the historical approach during the period in question.

Memory, Cognitive Neuroscience & Kitcher's Account of Scientific 'Cynicism'.

A. Edward Manier (University of Notre Dame)

The concluding chapter of *The Advancement of Science* uses decision theory to appraise the epistemic impact of *cynical* scientists who promptly invest in research programs attracting general support ("bandwagon effect") rather than defer judgment concerning competing research programs until merited by the strongest methodological criteria, and claims that communities most of whose members are cynics achieve a "reasonably good distribution" of cognitive labor . I examine difficulties which arise in an effort to interpret the role of "cynicism" in a dispute (Schacter v. Roediger) concerning models of memory in cognitive neuroscience.

Wynne-Edwards and the Compromise Model of Scientific Closure.

Tim Shanahan (University of California, San Diego and Loyola Marymount University)

In his book *The Advancement of Science* (1993), Philip Kitcher proposes a "Compromise Model" for the closure of major scientific debates, according to which scientific debates reach closure through the joint contributions of (1) the articulation and acceptance of decisive arguments and (2) the operation of nonepistemic (e.g., social) factors. The model succeeds to the extent that transitions in the history of science satisfy its conditions. I evaluate Kitcher's model through an examination of a major transition in biology. V.C. Wynne-Edwards's book Animal Dispersion in Relation to Social Behaviour (1962) is widely regarded as both the high point and last grand gesture of group adapationist explanations in evolutionary biology (and ecology). Critical reactions to this book marked the end of "naive" group selectionist theorizing, and paved the way for the genic selectionist approach common to much current explanatory practice. I analyze the process by which discussions of Wynne-Edwards's work closed in a consensus against it, and examine the possibility that different, perhaps equally warranted, problematics, explanatory ideals, and epistemic priorities could have produced a quite different outcome. The implications for Kitcher's Compromise Model are then assessed.

Self-Organization vs. Optimization in Nonhuman Behavioral Biology I

Organized by Werner Callebaut (Limburgs Universitair Centrum and Rijksuniversiteit Limburg) and Jean-Louis Deneubourg (Université Libre de Bruxelles)
Chair: TBA

Two fundamental and complementary questions are at stake in biological research: (1) WHY does it occur: what is the structure's function and its adaptive value? (2) HOW does it occur: how do structures organize their building activity? The second question explores the link between the behavior of the building units or agents and the characteristics of the structures that are being produced. and the characteristics of the structures that are being produced.

Self-organization (SO) is one particular script of the dynamics of biological organization. It is a process in which pattern emerges at the global (collective) level through interactions among the components of the system at the individual level, without these interactions explicitly specifying the global pattern.

Very often, SO is erroneously presented as a "heretic approach" ignoring natural selection or being a substitute to it. This is certainly not the case. SO is only one of the mechanisms subjected to natural selection and one actor of natural selection.

Numerous misunderstandings occur between different groups of scientists; e.g., the "self-organizers" are being perceived by "the others" as physicists ignoring biology, while "the others" have sometimes been regarded as unaware of relevant advances in certain scientific disciplines. As a result, the SO approach is still marginal in biology; and fundamental questions such as the interplay between SO on the one hand, and natural selection, optimality, individual complexity, and information coding (genetics), on the other, are not really discussed.

The session will deal explicitly with these and related methodological and sociological issues.

Introduction.

Werner Callebaut (Limburgs Universitair Centrum and Rijksuniversiteit Limburg)

Self-Organization and Alternative Explanations in Insect Societies.

Jean-Louis Deneubourg (Université Libre de Bruxelles)

A key problem in the study of social insects is to establish a link between individual behavior and global structures. Of particular interest is the question of how much individual complexity is required to produce the observed global complexity. This question is not specific to social insects and this group provides us with a useful model to discuss this question in biology. In many cases, self- organization can provide a powerful mechanism for generating collective structures. An important feature of these self-organizing systems is that natural selection often produces simple behavioral algorithms that allow social insects to exploit physical constraints in the environment. Individual behaviors modify the environment, feeding back on individual behaviors, resulting in complex global structures. This is not a new idea but today it appears that these mechanisms are more powerful than we imagined. In fact, we find similar strategies among many different social insect activities. We shall discuss: (1) How collective decisions arise through the amplification of environmental heterogeneities; (2) How physical constraints, such as available space, affect global patterns without the need to modulate individual behaviors; (3) How, in other cases, insects modulate their behaviors and communication by exploiting simple, information-rich cues; (4) What are the alternatives to self-organization?

Supramolecular Aspects of Morphogenesis.

R. Lefever (Université Libre de Bruxelles)

(Abstract not supplied)

What is evolutionary adaptation?

Peter Hammerstein (Max-Planck-Institute, Seewiesen, Germany)

Phenotypic approaches to evolutionary adaptation are commonly used in theoretical studies of animal behavior. However, a look at n-locus population genetics theory shows that the effects of recombination may severely disturb the phenotypic picture of evolution. This leads to the following puzzle: why can phenotypic analysis of behavior be so successful if it is not safely based on genetic grounds? A new line of reasoning helps to answer this question and leads to the 'streetcar theory of evolution.' Using the streetcar paradigm it can be demonstrated that phenotypic fitness maxima and evolutionarily stable strategies have a solid foundation in population genetics theory. In this sense the new theory extends the evolutionary synthesis that took place during the first half of this century.

SATURDAY, JULY 22 16:00 - 17:30

Social and Cultural Studies of Science (Roundtable)

Organized by Michael Lynch (Brunel University)
Chair: TBA

Recently, social and cultural studies of science (including sociological, anthropological, feminist, social-historical, and other approaches, often in combination) have become a subject of heated controversy. A few prominent scientists have gone on record denouncing social constructionist and feminist approaches, linking them to a broader anti-scientific movement which includes creationists and animal-rightists. The most prominent source of these criticisms is *Higher Superstition* (Johns Hopkins University Press, 1994) by Paul Gross and Norman Levitt. This roundtable is organized to discuss the arguments made by Gross and Levitt, and some of the other critics. The aim of the session is to clarify what social and cultural studies of science are saying in light of the criticisms. Are they actually opposed to science, and do they really threaten to undermine public respect for science?

Elisabeth A. Lloyd (University of California, Berkeley)

Michael Lynch (Brunel University)

Nils Roll-Hansen (University of Oslo)

Elihu M. Gerson (Tremont Research Institute, San Francisco)

Others TBA

Biosemiotics II

Organized by Manfred Laublicher (Yale University)
Chair: TBA

Biosemiotics: Towards a New Synthesis in Biology?

Jesper Hoffmeyer (University of Copenhagen)

The old 'new synthesis' from the 40ties unified most of the biological disciplines into one nearly mono-paradigmatic science. In spite of its indubitable success the weaknesses of the old synthesis have become more obvious in recent years. One need only to look at the list of content in a journal like TIBS (`Trends in Biochemical Sciences`) to see the extent to which communicative processes have come to the focus in modern biology. The old synthesis was not constructed to cope with this aspect of life and it tends to misguide us in doing so. The reification of communication to 'nothing but' transmission of signals (like genes) implies a grave underestimation of the interpretative or semiotic competence of living systems. It is claimed that a new semiotic synthesis is needed for biology to confront nature at the proper level of sophistication.

Decoding 'Coding': Text, Context and DNA.

Sahotra Sarkar (McGill University)

"Information" was introduced into genetics only as late as 1953, one week after the publication of the double helix model of DNA. By 1958, it had become a standard term, replacing "specificity." The "code" was what was deciphered in the 1960's. Yet, much of the dogmas about information, its transfer, its storage, falls apart in the eukaryotic context. No proper theory about biological information has ever emerged. Worse, the genetic code has revealed very few facts that can be connected to the systematic use of "information," its surrogates or related terms. This paper takes a skeptical look at the talk of coding and Information, and suggest that these metaphors only emerged because of the accidental linearity of the two most popular kinds of biological macromolecules, proteins and DNA. Metaphors can transform problem-spaces, and give rise to models. These metaphors never did. Perhaps they should be interpreted as biological manifestations of the linguistic/semiotic turn of the 1950's.

Neurosemiotics: Mechanisms of Meaning Assignment to Endogenous and Exogenous Signals in the Brain. How Do They Relate to the Multilayered Organization of Brain Functions?

Joachim Wolff (University of Gottingen)

In the nervous system, directed signal transduction via chemical synapses does not carry complex information from one neuron to another. Synapses rather transmit event-like graded messages, such as "whatever is your reactivity based on previous and present conditions, react now with a strength proposed by the FREQUENCY CODING of this signal". By coincidence with several other codes, functional meaning is assigned to these quantified, but meaningless signals. CHEMICAL CODING adds some functional aspects (effects on metabolism and/or membrane potential; excitation vs. inhibition) to effects on the postsynaptic target cell. This code is only partly determined during ontogenesis; pathophysiological mechanisms (repair and adaption to pathological conditions) may modify not only the expression of transmitter substances but may also influence the efficacy and even change the function (e.g., excitation vs. inhibition) of corresponding receptor systems. Biochemical plasticity then introduces into actual synaptic transmission certain aspects of the recent history (min. to hrs.) or previous activity in that part of the nervous system. LATENCY CODING, depends on : (1) differences in conduction velocity along nerve fibers, (2) fiber courses of different length, (3) different numbers of intervening synapses (number of synaptic delays). Ongoing summation or integration of postsynaptic effects makes latency coding an extremely important factor in determining the cite of occlusion between various excitatory and inhibitory inputs. Elimination of evoked potentials actually characterizes sites of "analysis" in the brain. Finally, TOPOLOGICAL CODING adds qualitative meaning to the signal. It is based on mapping the spatial order of receptors and targets in the organism to brain architecture. In this way, different sensory modalities originating from various sensory organs or other receptor systems are conducted to specific parts of the brain. This species-specific "bauplan" including the "wiring pattern" between remote neuron populations develops according to phylogenetically determined rules, it is ontogenetically modulated by genetic-epigenetic interactions and may be biographically modified by physiological use (e.g. learning). Thus frequency, chemical, latency and topological (and possibly other) coding systems cooperate in defining the actual "meaning" of a signal received by a specific set of neuron in the context of other signals. COOPERATION OF PARTIAL CODES IN MEANING ASSIGNMENTS TO SIGNALS suggests that separation of different levels of brain function (e.g., "trophicity" or maintenance function and epifunctions like the "millisecond business") may prohibit analysis of more complex brain functions, such as cognition and learning.

Crossing Interdisciplinary Boundaries (Octavian session)

Organized by Linnda Caporael (Rensselaer) and Cecelia Heyes (University College, London)
Chair: Linnda Caporael (Rensselaer)

ISHPSSB's commitment to interdisciplinary research is embedded in the society's name and mission. Yet, "interdisciplinary" can mean several methodological approaches. For example, one discipline may be used to explore another, as in the history and/or philosophy of biology; other disciplines may be "adjoining" as when naturalistic epistemologists use research in cognitive science. How do interdisciplinarians negotiate the boundaries between disciplines and the gaps between boundaries? Are there shared strategies, methods and standards for interdisciplinary research? Can there be/should there be/are there interdisciplinary communities similar to specialist

communities? We invite you to join us as speakers and discussants in this structured forum ("Octavian session") on the practice and prospects of interdisciplinary research.

Introduction.

Ron Rainger (Texas Tech University)

Introduction.

Cecelia Heyes (University College, London)

Discussion

The Audience

Self-Organization vs. Optimization in Nonhuman Behavioral Biology II. Metaperspectives.

Organized by Werner Callebaut (Limburgs Universitair Centrum and Rijksuniversiteit Limburg) and Jean-Louis Deneubourg (Université Libre de Bruxelles)
Chair: TBA

Self-organization Between Teleologism and Naturalism.

Werner Callebaut (Limburgs Universitair Centrum and Rijksuniversiteit Limburg)

(Abstract not supplied)

Epistemological and Methodological Comments on the Self-Organization Debate.

Isabelle Stengers (Université Libre de Bruxelles)

(Abstract not supplied)

Discussion.

All Speakers

Nineteenth Century German Natural History

Contributed papers
Chair: TBA

Charles V. Riley in Bonn, 1856-1860.

Edward F. Smith (Cornell University) and W. Conner Sorensen (Wuppertal, Germany)

The enduring reputation of Charles V. Riley (1843-1895) as the nineteenth century's most prominent agricultural entomologist rests to a considerable extent on the insect drawings with which he illustrated his reports as Missouri State Entomologist(1869-1877), as head of the U.S. Entomological Commission (1876-1881), and as chief Federal Entomologist (1878-1895). Riley's talent as a nature illustrator can be traced to his boyhood in England where he became interested in insects and in drawing, and to his instruction at art academies in Dieppe, France, and in Bonn, Germany. In Bonn (1856-1860), Riley studied under Christian Hohe, an artist with a reputation for exact replications of nature, portraits, and scenes along the middle Rhine. Through the use of Riley's sketches and drawings made in Bonn and other materials, we will attempt to trace Riley's footsteps in Bonn and to assess the influence of Hohe and the Bonn environment on his development as a master illustrator of insects and entomologist.

Was there a Darwinian or a Non-Darwinian Revolution in 19th-Century German Biology?

Eve-Marie Engels (Universität Gesamthochschule Kassel)

Taking up Peter Bowler's argument of a "Non-Darwinian Revolution", I want to discuss the question if there was a Darwinian or a Non-Darwinian revolution in 19-century German biology. Germany was one of the leading countries in picking up evolutionary ideas, particularly Darwinian ideas. Darwin's theoretical views also caused a widespread reaction in German periodical journals in a variety of areas. In this process of popularization and application to other areas however Darwin's theory was transformed many times in a way that it did not match any more Darwin's original intentions. The answer to the question if there was a Darwinian revolution therefore will vary in a context-specific way. My aim is to show that there was a Darwinian revolution in German biology. Even those of Darwin's contemporaries who critically discussed the essentially new element of his theory, the principle of natural selection, stressed the heuristic importance of this principle for the success of the theory of descent as a *scientific* theory. Therefore, the Darwinian revolution of the 19th century can be viewed as a revolution at the *methodological level.*

Representing the Unrepresentable: *Lebenskraft* as an Expression of Organic Activity.

Joan Steigerwald (York University, Toronto)

At the turn of the nineteenth century, the expression Lebenskraft (vital force) was not regarded as referring to an actual force by most investigators, but as a summary statement for the complex organic alterations held to underlie appearances of irritability, sensibility and generation. In my paper I will examine different strategies for representing these organic changes, contrasting a morphological approach, which focused attention upon static spatial structures, and a dynamic approach, which attempted to represent the linkages between organic events. I will suggest ways in which Naturphilosophie and Romantic theories of narrative influenced the growing emphasis on dynamic organic change in German physiology at this time. I will also argue that the interest in dynamic processes, as expressed by Lebenskraft, radically altered the conception of physiology as animated anatomy that had dominated in the eighteenth century under the influence of Albrecht von Haller.

Applying Developmental Systems Thinking: Research with Non-Humans

Organized by Susan Oyama (John Jay College, CUNY) and Paul Griffiths (University of Otago)
Chair: Susan Oyama (John Jay College, CUNY)

Biologists and Psychologists who have been drawn to developmental systems critiques and reconceptualisations of developmental and evolutionary processes have frequently been challenged to show how this approach might function in the formulation and conduct of empirical research. This session presents a sampler of work at a variety of levels of biological organisation. The nesting of systems at different levels is central to the developmental systems perspective, as is the interplay amongst levels. As the speakers describe their work, another interplay may become evident, between theory and practice, conceptual framework and research. Participants have been asked not only to speak about their work, but also to reflect on the roles played by the notion of developmental systems in that work.

Developmental Systems and Cancer.

Lenny Moss (Northwestern University)

The preponderance of the gene-centered perspective in biology is reflected in the field of oncology as a conventional view which looks to genetic mutations as the cause of cancer. The present paper will provide evidence from both past and current work which supports the theory that cancer is a kind developmental disorder, that it is based in a history of cell-tissue interactions, and that it is rooted in the ability of cells to adaptively acquire heritable epigenetic alterations of state. Cancer progression will be viewed as a kind of evolutionary microcosm in which heritable changes by way of "progressive state selection" may include but are neither dependent upon nor determined by genetic alterations.

Models of Biological Pattern Formation and the Context-Dependent Effects of Genes in Development.

Frederick Nijhout (Duke University)

Theoretical models of pattern formation and of signal propagation in development can be used to study the putative effects of genes on form. In such models genes essentially set the value of reaction constants (as they could, by virtue of the fact that many genes code for enzymes), and it is therefore possible to study the effects of point mutations on emergent form. Results of studies with developmental models demonstrate that mutations in a given gene can have quantitatively and qualitatively different effects on the phenotype (or no effect at all) depending entirely on the state of other genes and developmental parameters. Studies of the evolution of model parameters in a microevutionary model of classical population genetics reveals, likewise, that when the phenotype is under directional selection the effect of selection on the frequency of any one gene is completely context-dependent and independent of the nature of the gene. Such models provide easily-understandable demonstrations of the inappropriateness of the various genetic-programs metaphors.

Towards a More Dynamic Developmental and Evolutionary Biology.

Rolf Sattler (McGill University)

In as much as evolutionary biology deals with change during evolution, it is dynamic. The dynamics is, however, to some extent obscured and ignored due to its description in terms of categories which, as categories, are static. Evolutionary plant morphology is used to illustrate this situation. Usually evolution of vascular plant form is described in terms of structural categories such as root, stem and leaf. Accordingly, evolutionary change occurs within these categories. The categories themselves are exempt from change, at least within major plant groups such as vascular plants or flowering plants. This basic statics is, however, questionable. There is evidence that morphological change cannot be always contained within structural categories. For this reason, a more dynamic approach to the description of ontogeny and phylogeny is outlined. In this approach, the antithesis of form and process is eliminated. Form is not only seen as the result of process(es) but as process(es). Accordingly, any particular form is a process or process combination and evolution is a change in process combinations. From the point of view of this process morphology, [last 2 words underlined: SO] the fundamental question is no longer how, for example, roots, stems, and leaves have changed during evolution. The question is how and why process combinations have changed.

SUNDAY, JULY 23 8:30 - 10:00

Modern Biology and its Predecessors

Organized by Kelly Smith (Trenton State College)
Chair: TBA

Our session will address some of the reasons modern biologists must take the study of history seriously. We will discuss three different areas in which an understanding of the history of biology sheds new light on contem- porary studies. First, Kelly Smith will illustrate how Aristotle's "formal monistic" approach to embryology has had a powerful impact on the history of developmental biology culminating in modern gene-centric views. Ron Amundsen will analyze the transcendental anatomists of the 19th century and draw parallels between thier views (and the critiques of them as essentialistic) an modern biological ontologies which stress the centrality of development.

Aristotle and Genetic Explanation.

Kelly Smith (Trenton State College)

This paper will critically examine Aristotle's "genetic" theory of heredity and show how the sorts of inappropriate causal simplifications he makes have been echoed by later thinkers. Of the formal and material causes, Aristotle sees the formal cause as the primary means of constructing causal explanations of developmental outcomes. He deduces that the formal cause must reside exclusively in the semen of the male, with the female menstrual fluids providing only material to be worked on. Unfortunately, not only are his arguments for this conclusion inadequate, but accepting this as true makes it impossible to explain the all too common phenomena of maternal inheritance.

This general approach-- focusing on a single explanatory cause in spite of theoretical and empirical difficulties-- constitutes a leitmotif in the history of developmental biology. Preformationists argued that the complexity of the adult could be explained as the unfolding of a tiny person or "homunculus" within the egg. Haeckel's recapitulation explained the sequence of developmental stages entirely in terms of past evolutionary stages. While the nineteenth century sees a major revolt against reductionistic pictures of development, the emergence of Mendelian genetics and the explosion on biological knowledge it made possible clothed the reductionists' ideas in a newly respectable guise. Thus, modern biology recreates the analysis of Aristotle: genes are the formal elements that explain development. Not surprisingly, it inherits the problems he encountered as well: there is no convincing evidence for this claim and such common phenomena as genetic induction and pheontypic plasticity can not be adequately explained.

Necessity, Contingency and the Bauplan.

Ron Amundson (University of Hawaii, Hilo)

This paper discusses 1) the range of ontological and metaphysical stances taken within embryology and morphology during the past 200 years, and 2) the ways in which these stances have contrasted with teleological and adaptationist biological theory, especially modern neoDarwinism. Broadly stated the question is: Does metaphysics matter?

A group of 19th century biologists labeled transcendental anatomists have been widely interpreted as univocally "essentialist" or "typologist" in metaphysics. These views are often considered among the strongest metaphysical barriers to the acceptance of evolutionary change. The tension between historical contingency and typological necessity persists in some modern embryological thought. An examination of transcendentalist thinkers (including Goethe, Geoffroy, Owen, and Agassiz) reveals considerable diversity of metaphysical views. It will be suggested that these transcendentalists appear unified only from non-morphological, or adaptationist, perspectives. The relevance of the critique of essentialism to modern debates on adaptationism will be considered.

Quality of Risk-Assessment in Biotechnology: Theory, Practice and Politics of Deliberate Release. IV. Politics

Organized by Ad van Dommelen (Vrije Universiteit, Amsterdam)
Chair: Rene von Schomberg (Tilburg University, The Netherlands)

Theoretical and practical considerations are embedded in political processes, which require separate attention. Questions of concern are: What aspects of democratic procedures are relevant for the quality of risk assessment and how can they be improved? What are appropriate ways of making the required decisions and devising adequate policies?

Risk-Communication and the Ethos of Democracy.

Christoph Rehmann-Sutter and A. Vatter (University of Basel)

The ethos of democracy can be conceptualized as the idea realized in different concrete democratic legal (or extra-legal) rule systems. Each lingual formulation of that idea, i.e. each distinct political philosophy of democracy is a trial to interpret and takes place in the context of specific and practical problems. The aim of interpretation can be the modification of the rule system, in order to adapt it further to a changing environment. We propose (i) an interpretation of that ethos in the context of the question, how technological risks affecting others can be evaluated and legitimized. And we draw (ii) some rough lines of possible risk-communication methods for decision-making within existing parliamentary, majority-ruled legal systems, and (iii) formulate criteria of a political ethics for evaluating such methods.

The quality of risk-communication, -evaluation and -decision could be improved if the participants could trust the process being really based on the democratic idea, and has not only maintenance function in existing arrangements of power.

Biotechnology and the Precautionary Principle.

Soemini Kasanmoentalib (Vrije Universiteit, Amsterdam)

In environmental protection policy and law there exists broad international consensus in favour of the precautionary principle. Key elements are 1. it takes explicitly into account the interests of future generations, i.e. it is addressed to prevent irreversible and long term effects and 2. it places the burden of providing 'conclusive evidence' on the actors that argue that preventive action is not necessary, i.e. reversing the burden of proof. As ethical guideline not self a part of science, it focuses on the uncertainty unavoidably attached to any scientific assessment and the recognition of the limi-tations of data, models and paradigms. This paper questions the implications of the precautionary principle on the metho-dology of environmental risk assessments for the deliberate release of GMO's. What kind of research questions are relevant in the light of the principle and which scientific data arepresent ecological models and the impossibility to assess long term effects of introductions of particular transgenic organisms.

Lacking Scientific Knowledge or Lacking the Wisdom and Culture of Not-Knowing? Comments on the International Biosafety Debate.

Christine von Weizsaecker (Bonn, BRD)

Examples are presented and underlying conflicting concepts are analysed of the scientific and political debate in Germany, in Austria, in the European Union and in the United Nations. These underlying concepts are decisive for the basic structuring of the political debate. They lie in the interface between concepts of the very nature of scientific prognosis and of the political role of science and technology. The following conflicts are discussed in some detail: 'reproducibility' versus 'historical uniqueness' in scientific biology; 'hypotheticality' versus 'error-friendliness' in technology; 'engineering of public acceptance' versus 'democratic political debate'; 'technology-centered risk-assessment' versus 'problem-oriented comparison of different technological options'.

Gender in Biological Explanation

Contributed papers
Chair: Helen Longino (University of Minnesota)

Can Chaos Theory Help the Feminist Critique of Science?

Muriel Lederman (Virginia Polytechnic Institute and State University)

One of the hallmarks of the feminist critique of science is the identification of the androcentrism introduced at the Scientific Revolution. This is exemplified by the acceptance of experimentation as a component of contemporary science. I will explore whether chaos theory can substitute for the scientific method in some biological sciences, perhaps even giving a more accurate description of biological phenomena, and if this substitution overcomes other problems ointed out by the feminist critique.

Regendering 'Hysteria' in 19th-Century Medical Textbooks.

Estelle Cohen (University of Minnesota)

Textbooks on women's disease from around the middle of the nineteenth century often represented women as governed by sexual organs that were likely to malfunction at any time between puberty and menopause. The recent association of "hysteria" with newly identified ovarian disease allowed one physician to conclude: "Some disposition to hysteria is inherent, if not to all women, at least in the vast majority." But attempts by gynecologists and psychologists in particular to emphasize the biological and emotional distinctiveness of women not only overlooked their own and others' clinical experience of male hysteria but misrepresented the historical record as well, distorting the very scientific traditions they cited to legitimate their own theories. The evidence shows that nineteenth century medical writers frequently read back into earlier periods of disease entities and categories of difference that had only been formulated in their own time. Moreover, twentieth century historians of medicine have not always reported scrupulously the lively debates about hysteric diseases in the seventeenth and eighteenth centuries either, and have exaggerated the extent to which these disorders had been degendered by neurogenic theories that rejected a uterine model of causation.

Men, Insects and War: Applied Entomology in Germany, 1900 - 1920

Sarah Jansen (Harvard University)

The discourses that were formative of applied entomology in Germany between 1900 and 1920 will be examined. Special consideration will be given to the role of gender symbolism in the self-construction of this field. Topics to be examined will include use of metaphors, specific terminology and other language forms. For example terms such as "male soldiers combatting armies of insects" and "the hard methods required in the war against insects" used by Karl Escherich and Albert Hase. Among the approaches utilized will be that of Michel Foucault as found in "*A Discourse on Language*" and Klaus Theweleit in "*Male Phantasies*".

Experiment and Explanation in Biological Research

Contributed papers
Chair: TBA

Causal Theories, Justification and Experimental Science: a Case Study from Bacteriology in the Late 19th Century.

William C. Summers (Yale University)

The notion of causation has been of concern to philosophers at least since Hume, yet this notion of cause has rarely seemed problematic to the working scientist. In the case of bacteriology in the late 1800's, however, the meaning of causation and the criteria for justifying causal beliefs were explicitly discussed and debated by working scientists. This explicit concern for the problem of causation and its direct relation to experiment, rather than implicit or purely philosophical concern for this notion, seem unusual and perhaps unique to bacteriology in this period.

While Koch refined ideas about causation of disease previously advanced by Henle and Klebs, differing approaches to this problem were advanced by other bacteriologists. Daniel Salmon in America developed his own criteria with which to justify the notion of disease causation. Salmon argued for criteria which overlapped with those of Koch and others, but had substantial differences. As it would turn out, Salmon's research on hog cholera justified his position, when Koch's criteria failed.

This paper will examine the historical context of these debates on causation with special reference to Salmon's work. Although at times he rhetorically abandoned his theoretical position, his experimental work continued to be driven by the unfulfilled criteria of his theory of causation.

Authority of Experimentation: The Case of T.H. Morgan and his Group.

Neelam Sethi (Cornell University)

By 1900 most biologists admitted the appropriateness of experimentation in their practice. But experimentation meant different things to different biologists and held varying degrees of authority for them. My purpose is to examine the different arguments offered by biologists for admitting experimentation in their practice and also to identify the different tools and strategies utilized by them to substantiate their claims about the authority of experimentation. I will also attempt to uncover the ends they hoped experimentation would achieve for them. In this connection, I examine the work of T.H. Morgan and his group. More specifically, I wil explore how and why over time the authority of experimentation varied within this group and how different actors perceived it.

Explanation in Biology and the work of Barbara McClintock.

Ana Barahona (University of California, Irvine)

Usually the work of Barbara McClintock has been understood as a challenge to the paradigmatic principles of Morganian genetics. Although this idea is right, it is no enough to explain McClintock's idea of transposition. In this work I propose that McClintock's work can be explained if one assumes that "explanation by mechanisms" is the association of a structure or material configuration with some kind of stability, that is manifested by regularities. In this work I will try to show that McClintock's idea of transposition was to "explain" the somatic regularities of variegation and mosaicism in relation with the material chromosome configurations during mitosis. At the same time the movement of segments of DNA as thought by McClintock relates the occurrences of aberrations (mosaicism) as consequence of unstable genes with the origin of mutable genes. Transposition was seen as a mechanism so integrative that it will account for all of the numerous seemingly complex types of phenomena at different levels of organization that were associated with the presence and behavior of unstable genes. Between 1942 and 1948 a concept of this primary event has been developed and nowadays it is part of the theories at the molecular level.

Studies of Molecular Biology

Contributed papers
Chair: TBA

A Fundamental Chapter in the History of Life Sciences: The Theoretical and Experimental Contributions of Sol Spiegelman to Molecular Biology.

Juan M. Torres (Universidad Nacional del Sur, Bahia Blanca, Argentina)

Discoveries and accomplishments have succeeded one another with astonishing rapidity in the field of life sciences, especially in molecular biology, during the last 40 years. Due to this circumstance we disregard frequently the name of the scientists involved in such achievements. Attempting to repair this fault, our historical contribution deals with Sol Spiegelman's accomplishments, which were mostly carried out at the University of Illinois, Urbana (Department of Microbiology) and at Columbia University, New York City (Institute of Cancer Research).

Among other outstanding Spiegelman's contributions, we point out the idea of the gene as the unity of selection in evolutionary process of life (advancing clearly Dawkins' hypothesis of selfish gene), the theory of the origin of life from RNA entities, the isolation of the first self-replicating system that could be studied in vitro, the reproduction of selective Darwinian processes at the molecular level (providing experimental basis for M. Eigen's theory of hypercycle), and the discovery of the mechanisms used by bacteriophages for their own reproduction.

Pangloss Goes Molecular: On the Purpose of Genetic Errors.

Marcel Weber (University of Basel)

There has been much speculation on the question of whether genes that cause random DNA sequence alterations ("mutator genes") confer evolutionary advantages to their host organisms. In this paper, I provide a conceptual analysis of this problem. Focusing on prokaryotic micro-organisms, I shall examine some recent claims made by microbiologists on the function of, e.g., mobile genetic elements in microbial evolution. I contrast these claims specifically with the views of G.C. Williams, the well-known critic of theories of group selection. My thesis is that group selectionist fallacies can be avoided in this case by construing selection for genetic variability properly, namely as the differential survival of clonal lineages of bacterial cells due to the effects of mutator activity. Such a second-order selection process has strange characteristics, for instance, mutation rate becomes a direct fitness component. The question arises if this process, provided it exists, should be classified as selection at all. Paralells will be drawn to the concept of species selection in macroevolutionary studies of metazoans.

Molecular Biology in Action.

Catharina Landström (Göteborg University)

Molecular biology is a large scientific field where research practices vary according to specific cognititve aims and institutional structures. The local variations and the dynamic character of the field renders it suitable for laboratory studies, an approach that can illuminate actual research practices. Howver, present development in laboratory studies requires some rethinking before they can be used to capture molecular biology adequately. Laboratory studies today appears to be caught between an apprehension of science as a matter of individual skill or as the result of a seamless web of actants. In their present articulations these two approaches exclude each other. The discussion in this paper is an attempt to view molecular biology as local practice with cognitive implications beyond the local, which requires a conceptual bridge between the two mentioned science studies approaches.

Studies in the History of Botany

Contributed papers
Chair: TBA

Controversies on Plants in the Second Half of the Eighteenth Century.

Gilles Denis (Équipe REHSEIS)

With the development of the physiocratical ideas, around 1740, several important people, concerned with the rationalization of agricultural practices take an interest in corn diseases. Initially, this group is essentially made up of administrators, magistrates, landowners or educated farmers. We name them the << agriculturists >>. They enter into debates over plant diseases,

which took place, up to this time, in texts on meterology, plant physiology or in books of << simples>>. A confrontation is made possible between these studies, the learnings and practical uses of the peasants and the observations and experiments the << agriculturists >> make in the fields. After 1770, chemists seek to perfect, essentially through analysis the explanatory models of the << agriculturists>>. In parallel to this model- building activity, the naturalists' models emerge, but in a different historical context: that of the classification of the imperfect plants and of microscopic beings, the reflection upon parasites, the nature of living matter and the links between plants, soil and climate. These models may be distinguished as zoological or botanical according to the supposed presence of a small animal or a microscopic fungus. Botanical models gain credence little by little, from the end of the century, advancing the concept of microscopic parasitism and associating it with that of disease.

From Agriculture to Botany: The Foundations of a Science in Seventeenth-Century England.

John J. Butt (James Madison University)

Historians have long marvelled at the sixteenth-century interest in plant collection and description, at the flurry of activity in agriculture in the first half of the seventeenth century in England, and then at the fresh creation of theoretical, experimental botany in the late seventeenth century. Few have seen the connection between the agriculture and the botany.

The origins of botany, according to historians, are a change from the medieval formulaic to naturalistic descriptions and illustrations of plants in Renaissance herbals focusing on plant anatomy, varied attempts at classification, and seventeenth-century theoretical and experimental science in institutions such as the Royal Society of London; with the growth of scientific botany in the nineteenth century practical applications to agriculture were made possible. Botany, however, originated from practical agriculture, but this has been overlooked in the existing historiography. Changes in agricultural texts and practice forced close observation of and experimentation on plants which led to plant anatomy and physiology by the late seventeenth century. Agriculture played as large a role in the formation of botany as did taxonomic and descriptive botany.

The Gendering of Pistil and Stamen in the History of Botany.

Christien Brouwer (Belle van Zuylen Institute, Amsterdam)

A starting point to study gendering, the historical process in which natural objects that first had no gender acquire one, forms the gendering of two plant parts, pistil and stamen, in the history of botany. In my paper I want to argue why the 'theory' on the gendering of pistil and stamen of the American historian Laqueur needs to be adjusted.* According to Laqueur the pistil is constructed as a female part by equating it with the ovary of animals and the stamen is constructed as a male part by equating it with the animal penis. In my opinion, Laqueur fails to notice that in the construction of the pistil as a female part and of the stamen as a male part, not one but several reproductive parts have played a part. Moreover, he fails to see that this particular construction was facilitated by representing the behavior of the pistil and stamen in gender specific terms.

* Thomas Laqueur, *Making Sex, Body and Gender from the Greeks to Freud*, Cambridge, MA: Harvard University Press, 1990, p. 171-173.

Ecology and Evolution

Contributed papers
Chair: TBA

From Natural Selection to Natural Construction: The Challenges of Integrating Ecology into Evolutionary Theory.

Peter J. Taylor (Cornell University)

In the third chapter of *On the Origin of Species* Darwin identified a fundamental ecological problem for evolutionary biology. In the "Struggle for Existence," he noted, "any variation... profitable to an individual..., in its infinitely complex relations to other organic beings and to external nature, will tend to the preservation of that individual, and will generally be inherited by its offspring." -- All evolution occurs in an ecological context, yet the workings of that context are "infinitely complex." Most evolutionary theory, however, avoids unravelling ecological complexity. Instead the organism-environment relationship is compressed into the concept of fitness conferred on an organism by its characters. The center stage in theory can then be occupied by the genetic basis and differential representation of characters within single species. This paper, being prepared for a Festschrift to Richard Lewontin, analyzes different approaches to integrating the structure and dynamics of evolution's ecological context into evolutionary theory.

Counterfactuals and *Ceteris Paribus* Assumptions in Evolutionary Biology and Food-Web Ecology.

Greg Mikkelson (University of Chicago)

Causal statements are often taken to imply counterfactuals. The correlations between cause and effect entailed by these counterfactuals need not hold across all possible situations, however. They are required only where other relevant factors are either equal or randomly distributed, i.e. where something like a "ceteris paribus" assumption holds.

Unfortunately, it is often not clear exactly how to interpret the ceteris paribus assumption. I will illustrate this problem with two examples: a thought experiment in evolutionary biology and a theoretical result from food-web ecology. In both cases, diametrically opposed conclusions follow, depending upon which of two initially plausible interpretations of the ceteris paribus clause is chosen.

These examples are of interest for other reasons, as well. The first, evolutionary example sheds light on functional explanation and the question of whether natural selection is a creative force. The second suggests that while food-web theory may have successfully refuted one version

of the complexity-stability hypothesis, it leaves the closely related diversity-stability hypothesis alive and well, despite rumors to the contrary.

Habitat Templates and the Changing World View of Ecology.

Kostas Korfiatis and G. P. Stamou (Aristotle University, Thessaloniki)

The habitat templates are graphical-qualitative models that describe the development of life-history strategies in specific environmental conditions. Within the framework of life-history studies, ecologists focused on the density-dependent factors as the factors determining life-history strategies. With the use of habitat templates the focus is centralized on the environmental causal factors, considering density- dependent phenomena as a byproduct of the environmental impact. This implies an important shift in the worldview of population ecology (especially life-history theory): Population is not considered as a closed system isolated from the environment. The object of study is the organism-in-its-environment, as a complex multilevel system. Life- history theory worldview combines holistic and reductionist insights. This shift also has methodological consequences: The use of a variety of heuristic models is demanded and the methodology of ecology becomes more pluralistic.

SUNDAY, JULY 23 10:30 - 12:30

Phylogeny

Organized by Peter J. Bowler (Queen's University of Belfast)
Chair: Peter J. Bowler (Queen's University of Belfast)

Historians of evolutionism have tended to concentrate on the development of ideas about the mechanism of change, but the first generation of biologists to be affected by the theory were often more concerned with reconstructing the history of life on earth. Many areas of reseach were affected, ranging from morphology to paleontology and taxonomy. In many cases, ideas about the evolutionary mechanism shaped– and were shaped by– efforts to reconstruct phylogenies. This session brings together papers dealing with a wide range of phylogenetic problems in the hope if stimulating a greater awareness of the extent to which late 19th-century biology was affected by this aspect of evolutionism.

The Evolutionary Morphology of Carl Gegenbaur and Ernst Haeckel.

Mario Di Gregorio (Universita' degli Studi, L'Aquila)

The main aim of this paper is to study the scientific partnership between Carl Gegenbaur and Ernst Haeckel in order to evaluate the impact of Darwinism on nineteenth-century biology. A major theme of the paper is that Gegenbaur and Haeckel were attempting to integrate Darwin into an older, morphologically-inspired framework, with results - perhaps surprisingly - closer to the focal preoccupations of Richard Owen, the scourge of "Darwinism." Rather than focussing on individual variation on which natural selection operates, Gegenbaur's method focuses on homology -- already echoing Owen - and therefore on the unity of nature rather than its diversity. His "Darwinism" thus helped to perpetuate the apparently contradictory idealist concept of typology.

Darwin's Barnacle Phylogeny as Interpreted by Other Carcinologists.

Michael T. Ghiselin (California Academy of Sciences)

Darwin's monograph on the Cirripedia was based upon his evolutionary thinking, but it was published before he could make his phylogenetic ideas explicit. Therefore his contribution has not received as much attention as it might. Much of the discussion has focused upon technical details of anatomy rather than his truly original contribution. However, Darwin's work was profoundly influential on the work of Fritz Müller and Anton Dohrn, both of whom worked on the phylogenetics of Crustacea. To Müller we owe the application of the principle of recapitulation to phylogenetics; to Dohrn the development of the principle of succession of functions. The

controversies surrounding the interpretation of the sexual relations of barnacles provide one example of Darwin's reasoning not being fully appreciated by later workers.

To Tree or Not to Tree: John Henry Comstock and Evolutionary Phylogeny.

Pamela M. Henson (Smithsonian Institution)

John Henry Comstock (1845-1930), professor of entomology at Cornell University, pioneered the application of Darwin's ideas to taxonomy in the late 19th century. Comstock accepted Darwin's argument that the natural classification would reflect evolutionary history. He sought to apply Darwin's ideas about specialization, generalization, analogous characters, embryological characters, among others, to the practice of analysing and correlating taxonomic characters. Comstock did not draw evolutionary trees, arguing that not enough was known about evolutionary relationships to construct them, but he intended his classificaction to reflect phylogeny. This paper will address his views on trees versus classifications and contrast his work with the neo-Lamarckian tradition found among American vertebrate paleontologists.

Neo-Lamarckian Taxonomy and its Relationship to Neo-Lamarckian Evolution.

David Polly (University of Michigan)

The neo-Lamarckians were an almost exclusively American group of evolutionists who postulated changes in developmental timing as the primary cause of evolution. This paper will explore the influence of their ideas on classification in shaping their evolutionary theory. Neo-Lamarckian ideas about taxomony were taken almost directly from the work of Louis Agassiz. They believed that during the course of evolution, characteristics associated with one taxonomic level could change independently of those at other levels; e.g. genera could evolve from family to family without losing generic identity. In embryological development, an organism could change from one major group to another: this was because taxonomic groups were defined by characteristics pertaining to the group's level in the Linnean hierarchy, not because the neo-Lamarckians were simple recapitulationists. Evolution could thus be portayed as saltatory, and as moving in predictable directions.

The Boundary Between Bioethics and Science Studies

Organized by Tomoko Y. Steen (Cornell University)
Chair: Tomoko Y. Steen (Cornell University)

In the early 1980s, the field of science studies began to analyze the sciences through its new methodologies, such as network study and a social constructivist analysis. Today, scholars in traditional fields, such as the history of science and philosophy of science, are interested in

integrating these new methodologies into their own studies, while others in these fields criticize the new methodologies for their inadequacy.

There is not, however, very much interaction between science studies and bioethics. Our panel is an inquiry into how science studies and bioethics can interact with each other through their methodologies. The panel consists of two scholars from bioethics and two from science studies. It will discuss this issue through four case studies, namely, animal euthanasia, international attitudes to biotechnologies, a social constructivist analysis of Japanese human genetic technology, and over-population issues and laws.

Shaping Future Generations– The Social Constructivist Analysis of Human Genetic Technology in Japan.

Tomoko Y. Steen (Cornell University)

The establishment of human genetic technology began in the 1910s as a means for nation states to enforce eugenics. Until after World War II, when the tragedy of racist eugenics was uncovered in Germany, eugenics was considered to be an essential strategy for each nation state to maintain its quality. Because of the negative impact of the Holocaust, however, eugenics became "taboo." Human genetic technology, on the other hand, has separated from eugenics and continued to develop as if this technology had nothing to do with eugenics policy in Germany. Today, human genetic technology is uncovering secrets of the human body entirely through the Human Genome Projects, and the ethical implications of the technology have finally begun to be raised.

As in other nation states, human genetic technology was developed in Japan as a tool for eugenics. Yet, because of differences in social and historical background, this technology evolved differently from that in the U.S. and Europe. My paper will open the black-box of Japanese huma genetic technology and its ethical implications through the analytical methodology of "SCOT" (Social Construction of Technology).

Global Attitudes to Biotechnology and Universal Bioethical Reasoning.

Darryl Macer (University of Tsukuba)

In 1993 the International Bioethics Survey was conducted in Australia, Hong Kong, India, Israel, Japan, New Zealand, The Philippines, Russia, Singapore and Thailand. The survey included 150 questions with 35 open ones, on subjects in biotechnology and genetics. (D. Macer, Bioethics for the People by the People, Eubios Ethics Institute, 1994). Comparisons to North America and Europe will be used to suggest to what extent people around the world have universal bioethics. Bioethics is the study of ethical issues associated with decision-making in biology or life. The open comments will be discussed, especially concerning views on the ethical limits of the use of biotechnology and genetics to change living organisms, including humans. The open comments reveal few fears of eugenics or playing God in the use of gene therapy or genetic screening, and more comments support improving genes or economic benefits.

Animal Euthanasia.

Lawrence Carbone (Cornell University)

A controversy exists in America over the acceptability of physical (i.e. non-pharmacological) methods of killing rats and mice for research. The two main methods under scrutiny are decapitation and 'cervical dislocation'. My paper examines the history of this on-going controversy, by examining modifications in the American animal welfare laws, and in the American Veterinary Medical Association's panel recommendations on euthanasia of animals. A central issue is the question of who best decides research animal practices: scientists, regulators or veterinarians. AS this issue is contested, revisions of the AVMA panel report over a twenty year span reflect a shift from a clinical practice model of veterinary medicine to a more scientized approach.

Over-Populating the World: The Construction of the Global Population Crisis in the U.S. and Internationally.

Saul Halfon (Cornell University)

Concerns about global overpopulation have received renewed attention in recent years, in both U.S. domestic policy under the Clinton Administration and in international policy circles. This recent interest has culminated in the events at the 1994 Cairo Conference on Population and Development. What drives such concerns is not readily obvious. Demography, the scientific field most closely associated with population issues, has by no means achieved consensus on the importance of rapid population growth. Theories from this field have supported policies that both support and militate against such population growth, thus providing more of a resource to political actors than a necessarily compelling epistemology. In this paper I will explore the content of recent policy constructions of population both domestically and abroad. I will then attempt to explain the relative stabilization of these policies in relation to the web of discursive, institutional, and disciplinary resources that keep it in place. These resources are fluid and ever changing, thus providing multiple and sometimes contradictory cultural sites for negotiating population.

Mutualism, Symbiosis, and Cooperation: Conceptual Interaction of Ideas

Organized by Surindar Paracer (Worcester State College), David Blitz (Central Connecticut State University) and Joel Schwartz (College of Staten Island, NY)
Chair: TBA

The purpose of this symposium is to present papers and invite scholars to explore the early debates over symbiosis and. cooperation, in order to examine the confusion that soon

develops between the two concepts, and to analyze the role of social categories in the study of interrganism associations. The session will focus in particular on the contributions of P. J. van Beneden, Anton de Bary, T. H. Huxley, and Peter Kropotkin.

P. S. van Beneden (l809-1894) in 1876 was the first to discuss two forms of animal association in addition to parasitism: commensalism in which there is no harm to the host, and mutualism, in which the host, as well as the "guest" benefit. Van Beneden formulated his theory within an anthropomorphic and natural theology framework.

Anton De Bary (1831-1888), a German botanist, coined the term "symbiosis" in 1879 to describe the phenomenon of two different organisms which live together, under which he included van Beneden's three forms of animal associations (parasitism, commensalism, and mutualism). Moreover, de Bary extended the concept to the realm of plants and fungi, where conscious intent could not be attributed to the organisms, dropped the natural theology frameworks and identified symbiosis as a possible factor in evolution. One theme of the session will be to trace the role of the social categories of cooperation and competition in this early work on symbiosis, and to identify ideological aspects in these biologists' analysis of the interface between individual organisms and social interactions. Questions to be examined include: what role did anthropomorphic play in the development of the concepts of mutualism and symbiosis? Why did symbiosis and cooperation come to be considered as identical?

A second focus of the session will be the debate over competition and cooperation between T. H. Huxley and Peter Kropotkin. T. H. Huxley (1825-1895) was not only a noted biologist, but also a popularizer of science. He examined how science influences ethics, education, and government policy, and was especially active in this regard as science correspondent for the monthly review Nineteenth Century. In his writings criticizing evolutionary ethics, Huxley stressed the contradiction between the competitive nature of struggle for life at the biological level, and the cooperative or altruistic implications of ethics.

Peter Kropotkin (1842-1921), a Russian prince who defended anarchism and studied science, argued that the key factor of biological evolution was not individual competition in the struggle for life, but rather group cooperation in the struggle of species against hostile nature. He provided the counterpoint to Huxley, and ironically, succeeded him as science correspondent for Nineteenth Century.

The Huxley/Kropotkin debate focused on the interpretation and social implications of Darwinism, bringing up once more the relationship between the biological and the social that will be a focus of this session.

Symbiosis, Mutualism and Cooperation: A Conceptual Enigma.

Surindar Paracer (Worcester State College)

The phenomenon of symbiosis was first described in an essay entitled "Die Erscheinung der Symbiose" by Anton de Bary in 1879 and confusion soon developed between the concepts of symbiosis and mutualism. Reacting to P. J. van Beneden's ideas of commensalism, parasitism, and mutualism in the animal kingdom, de Bary was confronted with the unique nature of lichens, along with a wide spectrum of associations among fungi and their hosts. I would argue that de Bary coined the term, symbiosis, to include a variety of parasitisms, mutualism, and lichenism in order to

explain the associations that exit among members of the plant kingdom. But as some lichens came to be understood as mutualistic associations, various authors employed the term symbiosis to only describe those associations in which the both partners are mutually beneficial. In this paper, I will discuss why the concepts of symbiosis and cooperation came to be considered as identical and explain the role that anthropomorphism played in the development of these concepts.

Symbiosis and 19th Century concepts of the Social-Biological Interface in theWork of Alfred Espinas and Paul Topinard

David Blitz (Central Connecticut State University)

The concept of symbiosis was initially developed by du Bary (1831-1888) not only to generalize the work of van Beneden (1809-1894) to plants, but also to provide a more neutral vocabulary to replace van Beneden's anthropomorphic terminology. In using stereotypes of human interaction (such as thief, beggar, and altruist) in his colorful description of animal behavior, van Beneden had viewed the biological world from a narrow human perspective. Purged of this prejudice, the concepts of parasitism, commensalism, and mutualism were re-applied to animal species by a number of workers in related fields. This paper will examine the issues involved in the use of the biological concept of symbiosis and its various forms (parasitism, commensalism, and mutualism) in the work of two French theoreticians: the comparative psychologist Alfred Espinas (1844-1922) in his work Animal Societies (1877) and the physical anthropologist Paul Topinard (1830-1911) in his work Anthropology and Social Science (1898). The paper will also examine the influence these two works have had on 20th century discussions of cooperation as a factor in human society, with special reference to Warder Allee (1885-1955) and Ashley Montagu (b. 1905).

Thomas Henry Huxley as Science Correspondent: The Debate over Competition and Cooperation in Nature.

Joel Schwartz (College of Staten Island)

The professionalization of science in 19th century Europe caused members of the scientific establishment to explain their activities and work to the educated public. Early in the century, in Scotland, an amateur scientist, Robert Chambers, tried to impart a love of science to his readership with his articles in his *Chambers Journal*, a publication he founded with his brother, William. This encyclopedic periodical attempted to describe all facets of Scottish history and culture, and was the vehicle by which Chambers wrote about the wonders of the emerging profession of science.

Toward the latter part of the 19th century, Huxley also attempted to reach the same audience as Chambers; i.e., the literate British public who had some interest in scientific developments. He addressed many gatherings, gave public lectures and participated in various forums and wrote numerous essays in his desire to educate the British middle and upper classes on the role of science in society, e.g., how science influenced ethics, education, even governmental policy decisions. A highlight of his efforts was his position as science correspondent

for the journal, *Nineteenth Century*, a publication published by the Metaphysical Society, a group to which Huxley belonged. A fellow member and friend of Huxley, James Knowles, founded the magazine in 1877, and its goal was to explore all aspects of human endeavor and knowledge. It was an ideal forum for Huxley because its audience was precisely the same stratum of society he had been previously trying to reach. He utilized his position to advance his ideas in the many debates he found himself embroiled in, such as the question of agnosticism, spiritualism, and particularly noteworthy, the question of cooperation vs. competition in nature. He also used his position to continue to educate his audience; his signed articles reported on the recent advances in all aspects of the sciences, and the implications of such developments. His successor as science correspondent at *Nineteenth Century* was Petr Kropotkin, who ironically supported cooperation over competition, completely contrary to Huxley's point of view.

From Biology to Economics: A Search for Cooperation in a Darwinian Age.

Robert Hartwig (Worcester State College)

In the late 19th century, socialists, anarchists and some Christians found the harshness of a society based on Darwinian competition unacceptable and consequently sought scientific support for alternative economic theories of production. Opposition to popular Darwinism found its most influential expression in the writings of Petr Kropotkin (1842-1921), especially *Mutual Aid: A Factor In Evolution* (1902) which appeared as a series of essays in *Nineteenth Century* beginning in 1890. This paper will explore the origin of Kropotkin's ideas on intraspecies cooperation and mutual aid as an important contributing factor to the survival, flourishing and progress of species. Kropotkin's personal observation on animal behavior along with broad reading of contemporaries such as Espinas' *Les Societes Animales* (1877) as well as classics such as Buffon's *Histoire Naturelle* (1707-1788) provided the necessary insight to challenge the darwinian notion of natural selection. It is interesting to note that Kropotkin seemed quite unaware of the work of P. J. van Beneden (1809-1894) or Anton de Bary (1831-1888). The influence of socialists like Fourier (1772-1837), Saint Simon (1760-1825) and Proudhon (1809-1865) whose ideas Kropotkin surely came to know during his stay in France, and his association with a secret society of mutualists will be examined.

Biology Education in Russia: The Past and the Future

Organized by Valery N. Soyfer (George Mason University and International Soros Science Education Program)
Chair: Valery N. Soyfer (George Mason University and International Soros Science Education Program)

Russian biology has had excellent achievements dating back to the end of the XIX century. Due to the efforts of such outstanding biologists as N. Koltsov, A. Menzbir, S. Chetverikov, A. Serebrovsky, N. Vavilov and others, Russian biology not only produced pioneer works in such

fields as physiology, genetics, and embryology, but also helped to educate thousands of biologists at a high level. After the 1917 Revolution, several fields in biology were developed (physiology of the higher nervous system under the supervision of Nobelist Ivan Pavlov, or cell biology under N. Koltsov, or biophysics under A. Lazarev), whereas several others were hampered (primarily genetics). The terrible heritage of Lysenko played and is still playing a negative role in scientific research and biology education.

A new stage of biology development and the emergence of the new fields such as genetic analysis, genetic engineering, and biotechnology raise new problems which are very difficult for Russian scientists and educators: lack of textbooks, shortage of equipment and lack of funds. These new difficulties have appeared due to the economic turmoil which is ongoing in Russia. How to find a way out of the present situation, how to help support outstanding results, how to choose the best educators and students, how to promote the development of new trends in biology education? The establishment of the new International Soros Science Education Program (ISSEP), with almost $100 million support of the education system, and an additional $125 million for the support of the International Science Foundation created a new experience in the world community. The selection of almost 5,000 awardees for Soros educational grants in Russia in 1994 plays a serious role in healing the wounds of the education system caused by the economic turmoil. During this session, the participants will discuss specific problems of biology education and, more general subjects, such as the role of timely support of the preservation of excellence in education. The session will be supported partially by the ISSEP.

Input of Russian Biology to World Science in the End of the XIX Through the Mid-XX Centuries, Crushing of Genetics Under Stalin and Khrushchev Regime, and New Demands for Education

Valery N. Soyfer (George Mason University and International Soros Science Education Program)

(Abstract not supplied)

Contemporary Molecular Biology as a Part of the Biology Core Curriculum in High School

Vera E. Zaitseva (1567th Moscow High School)

The main drawback in teaching classical biology in Russian schools is that students acquire knowledge on biological variety before the principal laws of living nature. Molecular biology is studied deeply enough, but classical biology has already slipped out of the minds of most students. The structure of biological education should be reconstructed so that basic theories such as cell structure and function, molecular biology, genetics and evolution theory serve as the "nucleus" which is surrounded by concrete data about biological variety.

Some approaches for implementing these ideas will be presented.

Conflict Between Educational Demands of Ordinary and Gifted Children and the Strategy of Biology Education on the Eve of the XXI Century.

Galina A. Sokolova and Yevgeniya G. Petrash (520th Moscow High School)

Traditionally, children gifted in biology were considered as "young naturalists" with a marked interest in observing living nature. The accumulation of such experiences over time creates a growing motivation to study biology. In recent years, a new motivation has manifested: an interest in the ecological problems of man in terms of his living environment. Here a basis has arisen for the organization of research work.

For children without an evident inclination to naturalistic observation, a teacher should create conditions to form such a motivation. A good approach is to involve these children in activities which appear to be very attractive for their "naturalistically" inclined schoolmates.

Gradually, motivation to biological knowledge diversifies to subjects including exotic forms of life, cellular and molecular biology, genetics and evolution. This reinforces in children interest in new knowledge in other sciences. Thus, a teacher may discover more gifted children in his/her class.

Excellence in Education: How to Create New Approaches and Mechanisms for the Recognition of the Excellence

Vsevolod Borissov (International Soros Science Education Program, Moscow)

Many aspects make it desirable to recognize those teachers who constitute the elite as measured by outstanding ability to provide fertile ground for the simultaneous appearance of many talented students. Such an elite deserves very careful attention from society: additional financial support for these teachers is most efficient, and their particular approaches and methods are most valuable for spreading among other teachers.

An interesting procedure has been applied recently in Russia to reveal excellence in high schools in order to support them under conditions of the current economic crisis. A polling of many students in different Russian universities was performed with questionnaires asking students to name their best high school teachers in mathematics, physics, chemistry, and biology. A small number of teachers has proved to be mentioned by students much more frequently than others.

It can be suggested that polling students specifically in particular biological fields (for example, biodiversity or molecular biology) may help analyze the most effective methods in teaching the different aspects of biology.

The Social Organization of Research

Contributed papers
Chair: TBA

Arc of a paradigm: The Cold Spring Harbor Symposium on Quantitative Biology, 1933-1953.

Nathaniel Comfort (SUNY Stony Brook)

The Cold Spring Harbor Symposium on Quantitative Biology is one of the most-cited professional meetings in the history of molecular biology. Established in 1933 by physiologist Reginald Harris, the Symposium was the reification of "quantitative biology," the lifelong passion of Harris's father-in-law, Charles Davenport. Harris redefined quantitative biology in terms of physiology rather than genetics, partly to step out of Davenport's formidable shadow, and the early Symposia reflected this bias. Under director Milislav Demerec, in the 1940s the Symposium turned toward the nature of the gene. Demerec's ability to identify the sexy topics and important players in biology helped the Symposium define the fashionable areas of research. The Symposium's acme was the 1953 meeting, co-organized by Max Delbruck, in which James D. Watson presented his and Francis Crick's model for the structure of DNA. By the mid-1950s, Demerec's judgment seemed to slip. The important new areas in biology-biochemical genetics, the genetic code- were not represented at Cold Spring Harbor, neither in the meetings nor in the research. The Symposia focused instead on topics that were either premature or overripe. The next important Symposia took place in the 1960s, after Demerec's retirement.

Technological Change in Biology: Case Studies in Plant Genetics and Plant Breeding Research.

Patricia Nevers, Raimund Hasse, Rainer Hohlfeld and W.-Ch. Zimmerli (Universitat Hamburg)

The interrelationships between science and technology in the biological sciences were investigated in case studies involving interviews with 54 researchers at nine different institutes working in the fields of plant genetics and plant breeding research. Three dimensions, encompassing the organization, practice and logics of the research under scrutiny, were analyzed in an attempt to identify composite types of research and to examine their distribution in various different institutional backgrounds. A major result was the identification of a type of research referred to as "methods oriented" research in addition to "explanation oriented" and "product oriented" research. While the latter two research types are respectively oriented towards expanding biological theory and developing new plant varieties, the main focus of methods oriented research is the development and propagation of experimental techniques. In this capacity methods oriented research was found to play a key role in the convergence of science and technology.

Evolutionary Models of Scientific Traditions.

Sergio Martinez (UNAM, Mexico, D. F.)

Scientific change has been traditionally seen as change of theories or theory-related concepts. In the last decade this state of affairs has radically changed. There are many accounts of experimental traditions and their dynamics. However, some important assumptions of this theory-oriented perspective still remain, and have an important impact on many discussions and ways of formulating problems in history and philosophy of science. I maintain that there are different sorts of traditions in science, that these traditions have epistemic goals that are relatively independent and persistent through time, and that the recognition of the existence of these

traditions has important implications for formulating and answering important questions in the philosophy of science. In particular, in this paper, I want to show that scientific change in experimental traditions satisfies a criterion that I consider has to be satisfied by an evolutionary model in order to have explanatory value. However, scientific change in theoretical traditions does not satisfy this criterion. This criterion requires that the variation that is subject to selection is aggregative with respect to the function to which is adaptive. The fact that some traditions, but not others, might be amenable to evolutionary modelling in more than a mere analogical sense, sheds light on the discussion concerning the pertinence of evolutionary models for understandinng scientific change.

The Origins of the Neutral Theory of Molecular Evolution as the Result of Interactions Between Traditions.

Edna Suarez (UNAM, Mexico, D. F.)

Theory-oriented philosophy of science tends to identify disciplines with theories, even when they recognize the existence and importance of non-theoretical factors in the constitution of disciplines. In my PhD. dissertation I elaborate the view that scientific disciplines should be seen as articulations of different sorts of traditions. In this paper I want to show that this account of disciplines and scientific change sheds light on the role of theories in the structure of disciplines that go beyond the traditionally simplistic account. As a case study, I will focus on the realation between the discipline of molecular evolution and the Neutral Theoy developed by Motoo Kimura and Jack L. King and Thomas H. Jukes (1968-1969). This theory has been viewed as an extension of an important debate that took place since the fifties between two theoretical traditions, the "classical" and "balance" schools of population genetics. This interpretation follows the traditionally theory-oriented perspective of scientific change. It ignores important developments and interactions between experimental and descriptive traditions that were decisive for the possibility of articulating the different versions of the theory and more generally, for the formation and consolidation of the new discipline of molecular evolution.

Studies of Evolutionary Theory

Contributed papers
Chair: TBA

Economics and Evolution Theory.

Peter T. Saunders (King's College, London)

That there are links between evolution theory and economics is obvious, even if there is no consensus about how much each owes to the other. There are now intense and fundamental debates going on in both subjects, and these too are remarkably similar. In both, the orthodox theory seeks explanation in terms of external forces: natural selection in evolution, the market in economics. There is a strong reliance on on optimization arguments and on a crucial, if implicit,

assumption of linearity. The critics stress the importance of the existing structure of the system, and over the past few years have been drawing on recent advances in our understanding of complex nonlinear systems in general.

The Undefinability of Fitness and the Implication for Structure of the Theory of Natural Selection.

Sebastiaan A. Verschuren (Vrije Universiteit, Amsterdam)

Susan Mills and John Beatty proposed a propensity interpretation of fitness in order to reveal the empirical content implicit in evolutionary explanations. Their proposal has met with approval as well as criticism. I shall critically review the responses to their proposal, in order to vindicate my claim that the notion of fitness cannot be defined, and that it therefore fails to function in the explanation of the actual number of offspring. The vindication I will give rests on the view of Alexander Rosenberg that the concept of fitness is supervenient on the manifest properties of organisms. However, I will not join Alexander Rosenberg, who follows Mary Williams, in his conclusion that the impossibility of defining fitness does not undermine its role in the theory of natural selection. I discuss the implications of the undefinability of fitness for the structure of the theory of natural selection.

Please note: The index is organized by **first names.**