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Vorbemerkung des Herausgebers

Das Jahrbuch des Wissenschaftskollegs ist Rechenschaft und Chronik zugleich. Es enthält die *Arbeitsberichte* seiner wissenschaftlichen Mitglieder, die Resümees der im Kolleg veranstalteten *Seminare* sowie einige *Aufsätze*, die jahrestypische Forschungsthemen vorstellen.

Der Langzeitbewohner des Wissenschaftskollegs kann die Arbeitsberichte eines Jahrgangs selten ohne ein Gefühl der Nostalgie lesen. Dem gelegentlichen Besucher der Institution oder dem Außenstehenden muß ein solches Gefühl fremd sein. Aber auch ihnen mag bei der Lektüre der Arbeitsberichte deutlich werden, wie spannend stets der Prozeß ist, in dem sich zwischen den Theorien und den Temperamenten, den Traditionen und den Projekten eines Jahrgangs von Wissenschaftlern (sowie Künstlern, Schriftstellern und Politikern) Querverbindungen herstellen und Netzwerke knüpfen, die schließlich seine unverwechselbare Identität ausmachen. Die Summe kleiner und großer, unerhörter und trivialer Begebenheiten, zu denen sich die Arbeitsberichte der Fellows addieren, machen den Roman eines Jahres aus: *Letztes Jahr im Wissenschaftskolleg...* Die Arbeitsberichte sind ein Stück jüngster Wissenschaftsgeschichte, geschrieben in Berlin.

Die Arbeits- und die Seminarberichte der Fellows sprechen für sich selbst. An manchen Stellen lassen sie das Ausmaß der „externen" Engagements immerhin ahnen, die ebenso unverwechselbar zur Arbeit des Wissenschaftskollegs gehören. Die Fortschritte aber des Collegium Budapest oder des New Europe College in Bukarest im einzelnen aufzuzählen, würde den Rahmen dieses Jahrbuches sprengen. Auch kann über das Abenteuer, die Graduate School of Social Science der Polnischen Akademie der Wissenschaften in einem deutsch-französischen Verbund zu fördern, ebensowenig berichtet werden wie über die Befriedigung, die die Unterstützung der „Bibliotheca Classica" in Sankt Petersburg mit sich bringt oder die Faszination, die vom durch das Wissenschaftskolleg und das Van Leer Jerusalem Institute gemeinsam initiierten Projekt „Europe in the Near East" ausgeht, in dem in Jerusalem junge Israelis, Palästinenser und Deutsche zusammenarbeiten. All diese Projekte zeigen, wie sehr sich das Wissenschaftskolleg zu Berlin als eine Institution begreift, der die Probleme der *polis* wichtig sind und die mit dazu beitragen will, von Berlin aus kulturelle und wissenschaftliche Fernwirkungen zu erzielen, die einer entstehenden Hauptstadt angemessen sind. Neue Verbindungen werden sichtbar auch darin, daß durch ein Stipendienprogramm der Andrew W. Mellon Foundation für ungarische, polnische, tschechische und slowaki-

sche Geisteswissenschaftler der Aufenthalt von drei Fellows am Wissenschaftskolleg erstmals von amerikanischer Seite finanziell unterstützt wurde.

1994 war das Jahr, in dem durch György Kurtäg deutlich wurde, was der *composer in residence,* den das Wissenschaftskolleg zusammen mit den Berliner Philharmonikern beruft, bewirken kann. Die Uraufführung des Auftragswerks „Stele" unter Leitung von Claudio Abbado am 14. Dezember 1994 in der Berliner Philharmonie war der offizielle Höhepunkt einer Kooperation, von der wir uns zum Nutzen Berlins noch viel versprechen.

1993/94 war auch das Akademische Jahr, in dem die erste Ernst Reuter-Vorlesung gehalten wurde. Wir hätten uns am Vorabend der Gremiensitzungen am 23. Juni 1994 keinen besseren Auftakt für diese Vorlesungen denken können als den Vortrag, den Jean André François-Poncet, der ehemalige französische Außenminister, zum Thema „Globale Herausforderungen — Europäische Antworten" hielt. Die Ernst Reuter-Vorlesungen werden von nun an in jedem Jahr sich einem Thema von weitreichendem öffentlichen Interesse widmen.

Dieses Jahrbuch geht zu einem Zeitpunkt in Druck, da in Berlin der Weltklimagipfel stattfindet. Im Akademischen Jahr 1993/94 hatte das Wissenschaftskolleg zwei Schwerpunktgruppen im Bereich der Biologie eingeladen: mit den im Hause üblichen Kürzeln hießen sie die „Kollektive Intelligenz"- und die „Biodiversitäts"-Gruppe. Über die Absichten und die ersten Ergebnisse dieser Schwerpunktgruppen wird im Aufsatzteil dieses Jahrbuchs berichtet. Dies war nur ein Auftakt: In den kommenden Jahren wird sich das Wissenschaftskolleg, in enger Abstimmung nicht zuletzt mit dem Innovationskolleg gleichen Namens an der Humboldt-Universität, verstärkt dem Bereich der Theoretischen Biologie zuwenden.

Wolf Lepenies

Arbeitsberichte

Andras Bozôki

Intellectuals and Democratization

Born 1959 in Budapest, Hungary. Degrees in law and political science (1983) and sociology (1985) at the Eötvös University Budapest. PhD in political science. Dissertation: *The Theory and History of Anarchism.* From 1988 associate professor at the Department of Sociology, Eötvös University. Main fields of research: political ideas, democratic transitions in comparative perspective, the main actors (intellectuals and other groups) in the process of democratization in East Central Europe. Fellowships for research: University of California at L.A. (1988-89), Institut für die Wissenschaft vom Menschen, Wien (1990 — 91). Activity in politics: participant at the political negotiations and spokesman of the Federation of Young Democrats (1989). From 1994 — present beside lecturing at the Eötvös University — professor at the Department of Political Science, Central European University, Budapest. From 1992 on editor of the Hungarian Political Science Review. Books: *Tdrsadalomismeret* (Social Knowledge, textbook, co-author, Budapest, 1991); *Anarchizmus* (Anarchism, co-editor, Budapest, 1991); *Liberty and Socialism* (co-editor, Savage, MD. 1991); *Post-Communist Transition* (co-author, London-New York, 1992); *Az anarchizmus elmélete és magyarorszdgi története* (The Theory and History of Anarchism in Hungary, co-author, Budapest, 1994); *Democratic Legitimacy in Post-Communist Societies* (editor, Budapest-Tübingen, 1994). — Address: School of Law and Government, Department of Sociology, Eötvös Lorand Universität, 1364 Egyetem tér 1, H —1112 Budapest.

I came to Berlin with the hope of writing a book on the role of intellectuals in the process of transition to democracy in East Central Europe, and to complete some of my half-finished research on 1. populism, 2. democratic legitimacy and 3. comparative democratic transitions.

Concerning the latter three, I was able to do what I had planned. In the subject of populist movements ideas and discourses, I wrote and published

a long comparative study published both in Hungarian *(Politikatudomâ-nyi Szemle,* vol. 3. no. 3, 1994) and English (Central European University, Working Paper 5, 1994) and I regard this as a first step toward a future book comparing the American, Russian, Argentine and Hungarian populisms. (Working on this article, I learned a lot from my conversations with Eduardo Rabossi.) I also finished a study in the problems of democratic legitimacy which became a chapter of my edited book, entitled *Democratic Legitimacy in Post-Communist Societies.* This book is a result of an international cooperation of scholars, since the contributors came from different countries of the region, giving accounts of the chances of and obstacles to democratic legitimacy in almost each country in East Central Europe. As the editor, I am grateful to Christian Schumacher and Hans-Georg Lindenberg of the staff of the Wissenschaftskolleg for their indispensable technical assistance. Finally, I completed a co-authored article comparing Portuguese, Spanish and Hungarian democratization, which is due to come out in a book in the United Kingdom in 1995 (G. Pridham & P. Lewis, eds., *Rooting Fragile Democracy).*

On the other hand, my project on intellectuals turned out to be much more complex than I had expected. After a couple of months I had to realize that I had overestimated my capacity to carry out this ambitious project in such a short period of time. Moreover, discussing the topic with historians and sociologists in the Kolleg, crucial parts of my original hypothesis were shaken, and I had to continuously re-examine my views. One may regard this as an embarrassing phenomenon, but I rather believe it was my luck. Participating at the meetings of the reading group called *Mittwochsgesellschaft,* I am especially grateful to my colleagues and friends (Gianfranco Poggi, Robert Darnton, Jürgen Kocka, François Hartog, Mark Lehmstedt and others) who showed me some new aspects of the problem. Taking different examples from history (from the "men of letters" of the 18th century France to the ex-GDR opposition), I benefited a lot from their methodological approaches and observations. With a little exaggeration I would say: finally I saw these meetings as being "dedicated" to helping my work. Studying systematically the literature on the political role of intellectuals in different historical upheavals, I now understand better the motivations and behaviour of the intellectual members of the East Central European "transitional elites" of the 1989 "velvet revolutions". I think this could not have happened without the communicative intellectual atmosphere of the Wissenschaftskolleg.

I wrote some chapters of the book in Berlin (an introductory chapter on possible interpretations; an analytical framework for the typology of creative-technical, political-nonpolitical intellectuals and their roles in the different fields of public life; and a case study on the informal power of intel-

lectuals and their movements in Hungary), but the book in its final shape can only be the result of further research in the coming years. A preliminary study was already published (in: C. Rootes & H. Davis, eds., *Social Change and Political Transformation.* London: UCL Press, 1994) which study, I hope, will be followed by others.

During my stay at the Wissenschaftskolleg, I was offered a job by the Budapest College of the Central European University. This offer presented new challenges to me in teaching the history of political ideas and East European politics in English. With the extensive use of the library, I also took the opportunity to prepare for the new courses I am to teach. That is why I have to express my thanks to Gesine Bottomley and all the librarians for their kindness and patience concerning my never-ending requests.

All in all, I regard my past year in Berlin as one of the most productive (and most pleasant) periods in my life. When I first visited Berlin in 1979, I saw only a dark city in a hopeless situation and desperate mood on the East side. Later I also visited West-Berlin, in the autumn of 1984: it was a strange little island of freedom closed around by walls. On my third time here, in May 1990, Berliners and tourists were all working hard and enthusiastically to destroy the still-existing wall with special home-made instruments. In the last ten months, I again saw a different city, with characteristic districts, operas, museums, and vivid cultural and intellectual life. It is still struggling with its divided past, just a few years after the historic change, but, no question, Berlin is going to be one real, cosmopolitan city again. It was an especially rewarding experience for me to be a witness of this unprecedented process, together with my new friends from all over the world.

Scott Camazine

(Self-) Organized Activity

Born 1952 in Tucson, Arizona (USA). 1974 A.B. in Biology, Harvard College, Harvard, MA. 1978 M.D., Harvard Medical School, Harvard, MA. 1993 Ph.D. in Biology, Section of Neurobiology and Behavior, Cornell University, Ithaca, NY. Since 1989 Medical Staff, Tompkins Community Hospital, Ithaca, NY. 1980— 83 Research Associate, Section of Neurobiology and Behavior, Cornell University. Summer 1987 Food and Agriculture Organization of the United Nations, Consultant to South Korea (Honey bee queen rearing and artificial insemination). 1986-87 Research Associate, Department of Entomology, Cornell University. 1992-93 NSF Predoctoral Fellow, Cornell University. — Address: Division of Biological Sciences, Section of Neurobiology & Behavior, Cornell University, Seeley G. Mudd Hall, Ithaca, NY 14853, USA.

At the suggestion of Professor Rüdiger Wehner, a Permanent Fellow of the Wissenschaftskolleg, four of us — Jean Louis Deneubourg, Nigel Franks, Tom Seeley and myself — were invited here in 1993 for a *Schwerpunkt.* Our group originally had the ungainly name "Self-Organizing Superorganism Systems (Collective Intelligence)." However, after our arrival we tried to shorten the title to "Self-Organizing Systems" which was both more appropriate for our topic of study and also had the advantage of avoiding the highly-charged term "intelligence" among the intellectuals of the Wissenschaftskolleg. Unfortunately, we did not succeed, and our fellow Fellows persisted in referring to us either as the "Collective Intelligence" group or the "Collective Unintelligence" group, depending upon their interpretation of our progress and their personal sympathies for us as individuals !

Our primary goal for the year was to work on a book' about self-organization in biological systems. I also hoped to develop a series of computer simulations to accompany the book. The environment of the Wissen-

[1] Camazine, S., Deneubourg, J. L., Franks, N., Seeley, T. *Building Biological Superstructures: Models of Self-Organization* (in preparation). See also pp. 255 ff. in this volume.

schaftskolleg has been an ideal setting for this work. The library facilities gave us ready access to the necessary literature, and the daily interaction with other members in the group provided a unique opportunity to develop ideas about the emerging field of self-organization.

What is self-organization? Self-organization is not a simple term to define. In general, it refers to the various mechanisms by which order and structure spontaneously emerge in complex systems. Examples of such structures and patterns include the stripes of zebras, the pattern of sand ripples in a dune, the coordinated movements of flocks of birds or schools of fish, the intricate earthen nests of termites, the whorls of our fingerprints and even the spatial pattern of stars in a spiral galaxy.

Our group at the Wissenschaftskolleg chose to focus the attention on a small subset of this universe of self-organized structures. We were interested primarily in biological systems involving groups of multicellular organisms, in particular the complex and intriguing insect societies of bees, ants and termites. In this context, self-organization is a process in which pattern emerges at the global (collective) level by means of interactions among components of the system at the individual level. What makes a system self-organized is that the collective patterns and structures arise without the guidance of well-informed leaders and without any set of predetermined blueprints, recipes or templates to explicitly specify the pattern. Instead, structure is an emergent property of the dynamic interactions among components in the system.

My own research may help clarify what we mean by self-organization. My work has been devoted to the study of honey bee societies. My goals have been to explore the remarkable social organization of insect societies and to unravel the mechanisms produced by natural selection that enable these societies to function as coherent, well-organized entities. A colony of insects faces the same challenges confronting an individual organism — defense, the need for nourishment, and reproduction. But though a colony may be a well-integrated unit, it nonetheless consists of many separate individuals, each relatively autonomous, and each relatively simple. Whereas the cells that constitute an individual organism are interconnected by extensive neural and circulatory networks, the members of a colony are only loosely connected. This raises the question of how the colony accomplishes its remarkable collective feats. How are colony-level decisions made without the colony equivalent of a brain or an organizing committee? How is colony homeostasis maintained without a sophisticated information network to relay data to a central control unit? Towards this end, my approach has been to view the insect society as a complex system of interacting units, and to examine how collective phenomena emerge from simple interactions among individual colony members. This ap-

proach couples extensive experimental observations with mathematical modelling and computer simulation. I have recently been working on the following topics: (1) the regulation of pollen foraging, (2) the formation of the concentric pattern of brood, pollen and honey on the comb, (3) the thermoregulation of colonies in winter, and (4) the social organization of the defensive behavior in European and Africanized honey bees.

How do the foragers of a honey bee colony know how much pollen to collect in order to feed the colony members? This is an example of collective activity that is precisely organized and carefully regulated. However, we know very little about the actual mechanisms that regulate pollen collection. Working with marked bees in observation hives, I have begun to unravel some of the mysteries of pollen foraging. I have shown that the colony experiences wide, rapid fluctuations in pollen stores and that the individual bees quickly respond to these changes by varying their rate of pollen collection. My current focus is to determine the precise cues by which the individual foragers assess their colony's need for pollen. Although we are still far from a complete understanding of the process, some valuable progress has been made.

When the colony is subjected to low pollen reserves, more bees forage for pollen, and each individual forages more rapidly. Conversely, if the colonies are given excess pollen, the foragers slow down their foraging rate or cease to forage. The homeostatic response by the foragers develops within a few hours of changes in the colony's pollen reserves, yet the forager does not require any direct contact with pollen to make her assessment. My experiments reveal that a forager does not assess the colony's pollen stores herself, *directly*, but rather receives information *indirectly* through other bees in the colony. It appears that the young nurse bees, whose task it is to consume pollen and feed the proteinaceous food to the developing larvae, gather information concerning the colony's pollen stores and pollen demand and then pass this information on to the foragers. In collaboration with Dr. Karl Crailsheim at the *Institut far Zoologie der Universität Graz* (Austria), we are testing this hypothesis of information acquisition. Using radioactive labelling techniques, we have obtained evidence that pollen foragers do indeed receive information about the colony's pollen reserves through the transfer of protein from nurse bees to foragers. We plan to continue this work in the summer of 1994 after my stay at the Wissenschaftskolleg.

A project I recently completed was an examination of how the characteristic concentric pattern of brood, pollen and honey develops on the combs of the honey bee colony. This colony-level pattern somehow emerges from the activities of thousands of individual bees. One possibility for how the pattern forms is that the queen and the foragers each know

where on the comb to place eggs, pollen and nectar, as if each had some genetically-encoded blueprint for the pattern. In contrast to this hypothesis, I have shown that the pattern develops spontaneously through a self-organized process based on the dynamic interactions of the colony members. Though each bee acts independently, using only a few, simple behavioral rules based upon limited knowledge, an orderly pattern emerges on the comb. To explain the process of pattern formation, I first conducted a series of observations and experiments in the field and in the laboratory to determine the behavioral rules of the bees. Then, through computer simulation, I explored the implications of these rules in a model. The predictions of the model match the comb pattern seen in nature, supporting the self-organization hypothesis.

Another project involves thermoregulation in honey bee clusters. In temperate climates, a honey bee colony must survive a long, cold winter. How the winter cluster of bees regulates its temperature is another mystery of honey bee social organization. This question has attracted the attention of several investigators, especially Bernd Heinrich, who has conducted extensive experimental work on the thermoregulation of a winter cluster of honey bees. Although several models of the thermoregulatory process have been put forward, no one has yet been able to describe in detail the mechanisms involved. In collaboration with two mathematical biologists, Leah Edelstein-Keshet and James Watmough, both at the University of British Columbia in Vancouver, Canada, we have developed a model of swarm thermoregulation based upon a self-organization mechanism that links the behavior of the individual bees to the colony-level thermoregulatory response. During my stay at the Wissenschaftskolleg, we have completed this model and have written a paper summarizing our work[2].

The different defensive responses shown by Africanized and European bees also challenges our understanding of colony-level social organization. Why are Africanized (so-called "killer") bees so much more aggressive than European bees? What are the precise physiological and behavioral differences between the two races of honey bees, and how do they explain the differences in colony-level defensive behavior? If we hope to minimize the impact of Africanized bees in the United States, it is essential to understand the factors at the level of the individual that contribute to the collective defense response. In collaboration with Jean Louis Deneubourg, I am trying to develop a model which describes the

[2] Watmough, J., Camazine, S., »Self-Organized Thermoregulation of Honeybee Clusters«. *Journal of Theoretical Biology* (submitted).

factors leading to differences between the aggression of European and Africanized honey bee colonies.

Through these and other studies, a fascinating picture of social organization and colony-level structures and patterns begins to emerge. In place of centralized systems of communication and information transmission, insect societies frequently employ mechanisms based upon self-organization. I feel strongly (and I think the other members of our *Schwerpunkt* would agree) that self-organization promises to be a mechanism that will emerge as an important unifying principle in physical, chemical and biological systems. In biological systems, self-organization reveals itself as a powerful mechanism used by natural selection for the creation of diverse regulatory and morphogenetic processes.

In sum, my fellowship at the Wissenschaftskolleg has been personally very rewarding. I have been able to continue my research on self-organization, to develop new ideas, to collaborate with my colleagues, to begin writing a book devoted to self-organization in biological systems, and to lecture on this topic both here at the Wissenschaftskolleg and elsewhere in Europe[3]. But I am sure that what I will remember most in the years to come are the many interpersonal relationships that developed: the thrills and frustrations of a collaborative venture and the joys of interacting with a unique group of fellows and staff at the Wissenschaftskolleg.

[3] »Mechanisms for building organization in insect societies.« Lecture at the *Zoologisches Institut der Universität Zürich.* Zürich, Switzerland, November 1993.
»Self-organizing pattern formation in honey bee colonies.« Lecture at the *Theodor-Boveri-Institut für Biowissenschaften.* Würzburg, Germany, May 1994.

Robert Darnton

Deviations from Monographism

1939 born in New York; 1957-60 Harvard College; 1960-64 Oxford University. 1964 PhD. in history. Since 1968 he has been a Professor at Princeton University. In 1987 he became the Director of the *Program in European Cultural Studies,* Princeton University. He held fellowships and guest professorships in Paris, Stanford, Wassenaar, Oxford, and Berlin. He is a fellow of several academies and a member on various editorial boards. His research interests are focused on the period of the Enlightenment, particularly in France, on printing history and on cultural history in general. Among his recent publications: *The Kiss of Lamourette: Reflections in Cultural History,* New York, 1989; *Edition et sédition. L'univers de la littérature clandestine au XVIlle siècle,* Paris, 1991; *Berlin Journal, 1989-1990,* New York, 1991; *Gens de lettres, gens du livre,* Paris, 1992; *The Forbidden Best-Sellers of Prerevolutionary France,* New York, 1995. — Address: Department of History, Princeton University, 129 Dickinson Hall, Princeton, NJ 08544, USA.

Sure enough, I did not do this year what I planned to do. I did what I planned to do five years ago, when I arrived for my first stay in the Wissenschaftskolleg. Few Fellows have the opportunity to re-enter paradise after it has been lost; so I should explain.

When I arrived here in August 1989, I planned to write a monograph on the forbidden books of pre-revolutionary France. To my surprise, however, I found myself in a pre-revolutionary Germany By October the revolution had become visible in the streets of Leipzig. By November it could be visited in East Berlin. Unable to resist the temptation of visiting, I spent most of the year chasing after events, interviewing participants, and trying to do some modest, street-level ethnography of everyday life in the GDR. The result was a book, *Berlin Journal* (I prefer its German title, *Der letzte Tanz auf der Mauer),* which captures something of the flavor of Berlin in 1989-90 but hardly qualifies as "Wissenschaft".

Generously, the Wissenschaftskolleg let me have a second try. Berlin remained as attractive as ever but relatively quiet in 1993; and when I

arrived for my second term as a fellow, I came with drafts of chapters and piles of documentation. So by January 1994 I was able to complete the book that I should have finished in 1990: *The Forbidden Best-Sellers of Pre-Revolutionary France.* It will be published by W. W. Norton in January 1995 along with a companion volume of statistics and bibliography, *The Corpus of Clandestine Literature in France, 1769-1789.*

The work on those two volumes actually goes back to 1965, when I first discovered the papers of the *Société typographique de Neuchâtel:* 50,000 letters and a mass of account books from the only eighteenth-century publisher-bookseller whose entire archives have survived. The Swiss canton of Neuchâtel was a Prussian principality in the age of Enlightenment, and Prussia, in the person of Frederick II, seemed to epitomize the ideal of enlightened absolutism. Frederick was delighted to see his Swiss subjects enrich themselves by printing the works of the *philosophes* and smuggling them across the border into France, where the demand for "philosophical books" had been stifled by the censorship and the police.

The term "philosophical", however, turned out to have had a particular meaning to the professionals of the book trade. It covered everything illicit — bawdy, cheeky, scandalous, and seditious works as well as Enlightenment treatises. The publishers in Neuchâtel simply tried to satisfy demand. So by following their activities, one can see precisely how the illegal literary market operated and what books reached readers in France during the twenty years before the Revolution. Of course a single source, however rich, may give a biased view of the general phenomenon. In order to correct for the biases built into the archives of Neuchâtel, I have studied various sources in Paris: records of books confiscated by customs agents, accounts of police raids on bookshops, clandestine catalogues circulated by other publishers, and the mountain of information produced by the authorities responsible for the book trade *(Direction de la librairie)* under the Old Regime. The result, in the second of the two volumes I completed this year, is a survey of the entire corpus of forbidden literature: 720 works, which are identified and listed in a bibliography along with information about the demand for them in thirty French cities. This volume is essentially a reference work. I hope it will prove useful for students interested in the literature that was actually read in the eighteenth century ("la littérature vécue") as opposed to the literature that appears in conventional histories.

The first volume provides a general account of publishing and the illegal book trade. It takes up themes that I had developed in lectures at the *Collège de France* (published in 1991 as *Edition et sédition. L'univers de la littérature clandestine au XVIIIe siècle),* but it also addresses questions about the implications of this kind of "histoire du livre": How were books read?

How did they affect their readers? How can one pass from a diffusion study to the study of public opinion and ultimately to an understanding of events — notably the events of 1787-1789? I have not come close to resolving those questions, but I think I have refocused them in a way that will redirect my research over the next decade. By giving me time to think those questions through without worrying about the everyday emergencies of university life, the Wissenschaftskolleg gave me a new lease on life in general.

It also exposed me to the influence of my fellow Fellows — that is, to the temptation of deviating from the straight and narrow path of monographism (my term for the professional concerns that often overcome professors). Of course, I tried to resist. But a deftly-placed question from one of the more Mephistophelian Fellows, Hans Magnus Enzensberger, has gnawed at me all year: "Why don't you write less professionally?" This wicked idea received support from our Rector, who announced to all of us in September, "You are here to take risks." And while that suggestion rattled around in my head, I found myself cornered in conversations about the human condition with Ashok Desai while he was waiting for open-heart surgery and after he recovered from it. We all rejoice in the knowledge that Ashok emerged from his year stronger than ever. I am not sure I did. After leaving the *Martin Luther-Krankenhaus*, I sought shelter in the archives — this time archives located in the former quarters of the *Stasi* and of the Central Committee of the SED of the GDR. I want to write a different sort of book, a book of case studies and reflections on censorship as a phenomenon in authoritarian political cultures: France in the eighteenth century, East Germany in the twentieth century, and, in between, British India in the nineteenth century.

Finally, I should report that the year 1993 —1994 gave me the opportunity to organize an informal seminar for Fellows interested in the subject of intellectuals and revolutionary change. We called ourselves the "Mittwochsgesellschaft" and met on Wednesday afternoons every two or three weeks throughout the year. Several Fellows had been observers, participants, or participant-observers in the revolutionary changes of Eastern Europe. They discussed general issues — the nature of cultural revolutions, the process of delegitimation, the sociology of the intelligentsia — and also offered a running commentary on what they had experienced — in Hungary (Andras Bozôki, political science), Poland (Remy Forycki, French literature, and Karol Sauerland, German literature), Russia (Marina Litavrina, the history of theatre), and East Germany (Mark Lehmstedt, German literature and the history of books).

At first, the seminar took the form of an East-West dialogue. But the Fellows did not want to be type-cast as representatives of East or West. So

the discussions turned into intellectual jam sessions, initiated each time by one of the *Mittwochsgesellschaftsgenossen* and organized around some reading that we did in advance. The other participants were: François Hartog (France, history and classics), Anthony Grafton (USA, history), Gianfranco Poggi (Italy and the USA, sociology), David Gugerli (Switzerland and Mexico, history and ethnology), Kurt Wölfel (Germany, German literature), and Paul Zanker (Germany, archeology and art history).

We did not reach any firm conclusions, but we enjoyed some fascinating tours of the intellectual landscape. At the end of the year, we discussed our work at a meeting of the old and new Fellows. Four of us began the meeting by brief talks, in which we defended a set of provocative theses. The theses give some idea of the issues discussed throughout the year. They are as follows:

Robert Darnton:

1. Although one can find "intellectuals" among the ancients and the humanists of the Renaissance, the intellectual as a social type was born in Paris between 1740 and 1750.
2. From 1750 to 1789, the population of "la France littéraire" more than doubled, and it included at least 1,000 Grub Street hacks, or "Rousseaux du ruisseau".
3. Demographic pressure compounded literary rivalries in a way that made intellectuals a powerful ingredient of the explosions of 1789 - 1794 and that challenges us to reconsider the role of literature in revolutions.

Gianfranco Poggi:

Intellectuals always confront two overlapping problems:
whether to think of themselves as a distinctive, relatively autonomous social group or as an aspect *or* component of a larger social entity;
whether to relate to institutionalized political power chiefly as the seekers of its protection and support *or* as its critics.
In the face of the current *kultureller Umbruch,* intellectuals have the opportunity to act as interpreters of the new circumstances. But they will probably miss the opportunity, among other reasons because of the competition from other, better equipped purveyors of meaning, chiefly the media.

Andrâs Bozôki:

Three roles of intellectuals in the disintegration process of state socialism:

1. "Totalitarian" period: independent activity is not allowed, the system does not tolerate neutrality. The "thaw" comes from party-members who try to explain the dogma in a different way (e.g. reformist communist intellectuals around Gomulka, Imre Nagy and Alexander Dubcek); "loyalty" period (Hirschman).
2. "Post-totalitarian" period: a compromise between the bureaucracy and the technocracy. Political neutrality accepted, professional career-building is possible. Indirect censorship as a "velvet prison" (M. Hraszti). The pluralization of the public sphere: first and second public, and an emerging "grey zone" between them. Opposition comes from inside and outside; "loyalty" and "voice".
3. "Revolutionary" period: the elimination of the duality of the public sphere. The delegitimation process is over; intellectuals become involved in the political mobilization through movements and the press (writers, journalists, social scientists, actors, lawyers etc.). The culture of critical discourse is triumphant over the old discourses; "voice" and "exit".

   Happy end? The consolidation of new democracy as hard times for the "movement-intellectuals". Building new loyalties and expressing new voices: intellectuals as masters, defining the new name of the game. The pluralization of intellectuals along ideologies and different attitudes toward politics. Between the state and market, between the state and civil society: intellectuals as inconsistent medium. "Exit", "voice" and "loyalty".

Karol Sauerland:

Die Intelligenz und die Revolution. Zum Beispiel Polen.

1. Eine Revolution waren die sechzehn Monate der Solidarnosé-Bewegung in Polen vom August 1980 bis zum 13. Dezember 1981. 1989 hat es in Polen nur Ansätze zu einer Revolution gegeben.
2. In einer Revolution geht es, wie es einmal Hannah Arendt formulierte, im Unterschied zu Revolten und Staatsstreichen um einen Neuanfang, die Gründung eines neuen politischen Körpers, die Konstituierung einer neuen Staatsform. Eine Revolution ist dann ausgebrochen, wenn sich Polizei und Armee weigern, die Waffen gegen die Bevölkerung zu gebrauchen.
3. Hannah Arendt sagte auch, es sei nicht Aufgabe der Revolutionäre (die ja zumeist Vertreter der Intelligenz sind), die Revolution vorzubereiten, sondern sie zu studieren und am Tage der Revolution, die immer unvorhergesehen ausbricht, von ihren Aufenthaltsorten — den Gefängnissen, Cafés oder Bibliotheken — zu den Akteuren zu eilen und sich an

der Schaffung des neuen politischen Gebildes aktiv zu beteiligen. In Polen eilten sie in großer Zahl auf die Werft in Danzig.

4. In dieser Revolution versuchten Vertreter der oppositionellen Intelligenz mit Rat und Tat der basisdemokratischen Bewegung zur Seite zu stehen. Sie arbeiteten das Gewerkschaftsstatut, Gesetzesentwürfe und Programme aus, die dann zum überwiegenden Teil öffentlich diskutiert wurden. Diese sechzehn Monate waren eine echte Schule der Demokratie.
5. 1989 gab sich der überwiegende Teil der Solidarnosé-Intelligenz mit der am Runden Tisch unter Ausschluß der Öffentlichkeit ausgehandelten "halben Demokratie" zufrieden. Den Elan im Volke versuchte sie zu stoppen, was ihr am Ende auch gelang.
6. Von politischer Macht hatte sie kaum Ahnung. So glaubten die meisten Vertreter der Solidarnosé-Intelligenz, den *homo politicus* Walesa in ein Denkmal verwandeln zu können. Der Konflikt war vorprogrammiert und damit auch die Enttäuschung der Öffentlichkeit.
7. Der erfolgreichste Intellektuelle nach 1989 war Balcerowicz. Ohne sein radikales Programm der ersten Stunde wäre Polen heute ein zum Osten und nicht zum Westen tendierendes Land.

B. Bemerkungen über die Siege der Postkommunisten im ehemaligen Osten.

Jean Louis Deneubourg

Actions locales, structures globales

Lieu et date de naissance: Ath, le 17 mars 1951. 1973 Licencié en Sciences Chimiques avec la plus grande distinction. 1979 Docteur en Sciences (Chimiques) avec la plus grande distinction et les félicitations du jury; thèse: *Essais de modélisation en sociobiologie et sociologie.* Chercheur qualifié au FNRS. Chargé de cours à l'Université Libre de Bruxelles (Eco-éthologie théorique), Directeur de l'Unite d'Eco-éthologie (Centre d'Etude des Phénomènes Non-Linéaires et des Systèmes Complexes). Thème de recherche: phénomène collectif dans les sociétés animales et dans les systèmes artificiels. — Adresse: Service de Chimie Physique, Université Libre de Bruxelles, Campus Plaine — CP 231, Boulevard du Triomphe, B — 1050 Bruxelles.

Le séjour au Wissenschaftskolleg avait comme but la rédaction d'un ouvrage consacré aux phénomènes collectifs et à l'auto-organisation dans les sociétés animales et ce en collaboration avec Scott Camazine, Nigel Franks et Tom Seeley. Ce projet était le résultat direct d'une initiative de Rüdiger Wehner.*

La production de structures (nids, réseaux de pistes, ...) par les animaux est souvent perçue comme la preuve d'une «intelligence animale». Cependant, un rapide parcours du règne animal où ces activités sont largement répandues, montre qu'il n'y a pas de corrélation entre la complexité des structures produites par une espèce et ses «capacités psychiques». Au contraire, les structures les plus impressionnantes par leur taille, leur diversité ou leur régularité sont produites — exception faite de l'homme — par des animaux aux capacités psychiques limitées: les arthropodes, le sommet étant atteint par les insectes sociaux.

L'étude de ces structures, comme pour toutes les structures biologiques, doit être abordée sous deux points de vue complémentaires. Le premier porte sur leur valeur adaptative et leur fonctionnalité: c'est le POURQUOI? Le second, le COMMENT?, porte sur l'étude des causes proximales, c'est-à-dire des mécanismes impliqués dans la formation de ces structures.

* Voir les articles dans la troisième section de ce livre, pages 255 s.

Notre travail accorde la priorité aux mécanismes et cherche à établir le lien entre les algorithmes contrôlant les comportements individuels des insectes et les structures et les décisions collectives qui émergent à l'échelle de la société.

Au vu de la complexité de ces structures, il n'est pas surprenant que les premières hypothèses formulées (et qui le sont encore parfois) pour expliquer ces performances aient été teintées d'anthropomorphisme. Ces hypothèses font appel à une connaissance par les individus de la structure globale à produire et en fait postulent que la complexité des réalisations de la société trouve son origine dans celle des individus et notamment dans leurs capacités à centraliser l'information, à agir et à décider au de travers la représentation de leur univers.

Les sociétés d'insectes fournissent un modèle de fonctionnement bien différent. Il s'agit d'une logique décentralisée basée sur la coopération d'unités disjointes, distribuées dans l'environnement et ne disposant que d'informations locales. Chaque insecte a un équipement sensoriel et communicatif relativement simple qui lui permet de répondre aux signaux provenant de l'environnement non social et à ceux émis par ses congénères.

Si ces notions ne sont guère neuves pour les spécialistes, ce n'est que très récemment que l'on a réalisé la capacité «organisatrice» de ces mécanismes qui avait été franchement sous-estimée.

Nous nous sommes intéressés, sans pour autant négliger les modes de fonctionnement alternatifs, en particulier aux phénomènes de type auto-organisation où les structures collectives résultent d'une multitude d'interactions locales sans qu'il n'y ait ce qu'on peut appeler un codage explicite des structures globales (plan, patron, ...).

A ce jour, différents exemples ont été étudiés expérimentalement et théoriquement. Il s'agit de choix collectifs, d'activités de construction, de la formation de réseaux, ... Nous avons essayé, à l'aide de ces exemples, de mettre en évidence les principes généraux de fonctionnement qui se dégagent.

Afin de mieux illustrer ces questions, prenons le recrutement alimentaire largement utilisé par les insectes sociaux. Les abeilles le pratiquent via des danses dans le nid (voir les travaux de S. Camazine et T. Seeley). La plupart des fourmis utilisent une piste chimique. Schématiquement, un «éclaireur» qui découvre une source de nourriture, rentre au nid en traçant une piste chimique qui va, d'une part, stimuler ses congénères à sortir du nid et qui, d'autre part, les guidera jusqu'à la source. Après s'y être alimentées, les fourmis recrutées rentreront au nid en renforçant la piste. En fait ce mécanisme relativement simple, qui à première vue, ne permet que de concentrer rapidement une population d'ouvrières autour d'une source de nourriture est, au travers de la compétition entre découvertes, à la base de

décisions collectives et de la formation de réseaux de pistes. Par exemple, si plusieurs sources de qualité différente sont découvertes, la colonie sélectionne la source la plus «intéressante», c'est-à-dire que l'énorme majorité des fourmis participant au recrutement exploitent cette source. Une telle sélection est possible, bien qu'aucun individu ne soit informé des différentes alternatives, simplement par le fait que l'intensité de la piste émise par une fourmi est directement liée aux caractéristiques de la source. La compétition entre les différentes informations dont une se reproduit plus rapidement (la source la plus riche) conduit à sa sélection. Nous avons ici un exemple de comportement relativement simple au niveau individuel conduisant à une «sélection naturelle» de l'information et permettant ainsi à la colonie d'effectuer le meilleur choix. Cet exemple met en évidence comment, notamment grâce aux communications à caractère amplifiant, la colonie est capable de générer une réponse collective efficace qui dépasse de loin l'échelle et les capacités des individus. Les sociétés d'insectes décideraient comme fonctionnent les écosystèmes: les espèces étant remplacées par des informations. La robustesse de cette procédure justifierait sa fréquence d'utilisation.

En parallèle avec ce projet d'ouvrage, nous avons collaboré avec Scott Camazine sur différents problèmes spécifiques, notamment celui de la défense des sociétés d'abeilles, et avec Nigel Franks sur certains aspects de la construction chez les fourmis.

Ce séjour au Wissenschaftskolleg a été une occasion unique pour nous de confronter nos idées, de nous forcer à les exprimer et ainsi, nécessairement, de les clarifier. Mais ce projet a été rendu possible aussi et surtout par l'accueil et la soutien que nous a réservé «l'Equipe du Wissenschaftskolleg» et l'ambiance des plus stimulantes qu'elle y fait régner.

Erhard Denninger

Individualrechte und Staatsziele

Geboren 1932 in Kortrijk (Belgien). Studium der Rechtswissenschaften und der Philosophie in Tübingen, Lausanne und Mainz. 1958/1966 Promotion (Dr. jur.) bzw. Habilitation. Seit 1967 Professor für Öffentliches Recht und Rechtsphilosophie an der Universität Frankfurt/Main. Dort 1970/71 beauftragter Rektor, 1973/74 Leiter der Abteilung für Wissenschaft und Kunst im Kultusministerium des Landes Hessen. Gastprofessuren in Chicago 1983 und Rom 1985, 1989, 1990. — Adresse: Institut für Öffentliches Recht, Universität Frankfurt, Senckenberganlage 31, D-61462 Frankfurt am Main.

Dem schmerzlichen Erstaunen darüber, wie schnell zehn Monate „Forschungsfreizeit" verfliegen können, folgt die selbstkritische Frage nach dem Geleisteten oder Erreichten. Sicher ist zunächst nur, daß Bert Brecht wieder einmal Recht behält: Die Pläne, die man gemacht hat, gingen (so) nicht auf. Der ursprüngliche Gedanke war, die Funktion und Funktionsgrenzen wichtiger Kommunikationsgrundrechte wie der Glaubens- und Gewissensfreiheit, des Rechtes auf informationelle Selbstbestimmung, der allgemeinen Meinungsfreiheit, der Pressefreiheit, der Kunst- und Wissenschaftsfreiheit im Hinblick auf die Konfliktbewältigung in einer kulturell, religiös, ideologisch, zunehmend auch ethnisch plural zerklüfteten Gesellschaft zu untersuchen. Dahinter standen die Beobachtungen und die Überzeugung, daß die genannten Grundrechte in besonderem Maße dazu geschaffen seien, auch scharf ausgeprägten, nicht „konformen" Subjektivitäten, sei es eines Einzelnen, sei es einer Gruppe, zum Ausdruck zu verhelfen und deren Erscheinungsformen zu schützen. Auf der anderen Seite steht das für alle (gleich) verbindliche, von allen gleichen Gehorsam heischende allgemeine Gesetz als Ausdruck der demokratischen *volonté générale :* Über den notwendigen und wünschenswerten Grenzverlauf herrscht in Literatur und Rechtsprechung seit langem hohe Unsicherheit. Die neuesten Beispiele — Teilnahmepflicht für türkische Mädchen am koedukativen Sportunterricht? — belegen dies und erweisen sich zugleich als winzige Spitze eines riesigen Problemeisbergs in einer modernen „multikulturellen" Gesellschaft. Bei der Darstellung wollte ich mit Einzelfragen zu Themen der Gewissensfreiheit beginnen und hatte dementsprechend wäh-

rend des ersten Drittels meines Aufenthaltes die Arbeit auf jene konzentriert. (Im Unterschied zu anderen Fellows hatte ich nicht schon ein halb oder zu drei Vierteln fertiges Buch im Gepäck, als ich nach Berlin kam, sondern stand am Beginn dieses Projekts.)

Es kam anders. Unter dem Datum vom 5. November 1993 legte die Gemeinsame Verfassungskommission von Bundestag und Bundesrat ihren Abschlußbericht mit den Empfehlungen zur Reform des Grundgesetzes vor (BT-Drcks. 12/6000). Ich war seit langem und zuletzt als Mitglied des Hessischen Verfassungsbeirates mit den Fragen der Grundgesetzreform beschäftigt gewesen, insbesondere mit den Problemkomplexen der Staatszielbestimmungen, auch der „sozialen Grundrechte" (betreffend Arbeit, Bildung, Kultur, Wohnung, Soziale Sicherheit) sowie mit der Frage des Einbaues direktdemokratischer Elemente in die bislang fast rein repräsentativdemokatische Verfassung. So lag meine Zusage nahe, als ich gebeten wurde, auf der Jahrestagung der Deutsch-Brasilianischen Juristenvereinigung in Münster (Westfalen) Ende November über Probleme der Verfassungsreform zu sprechen, zumal die Brasilianer derzeit vor ähnlichen Reformproblemen stehen. Die ausgearbeitete Fassung dieses Vortrages erscheint demnächst unter dem Titel „Verfassungsreform — ein kritischer Bericht" in den Jahrbüchern der genannten Juristenvereinigung.

Der damit angestoßene vertiefte Vergleich der Verfassungsgebungs-Arbeit in den fünf neuen Bundesländern mit dem, was die Gemeinsame Verfassungskommission auf Bundesebene zuwege gebracht hatte und mehrheitlich auch nur zuwege bringen wollte, lenkte meine Aufmerksamkeit in immer stärkerem Maße auf die grundlegenden, politisch primär wirksamen Motive zur Verfassungsgebung in Ostdeutschland einerseits, in Bonn — für Gesamtdeutschland — andererseits. (Das feinere Gespür für die dabei leitenden Erfahrungen und Bestrebungen hätte ich an keinem anderen Orte als in Berlin und speziell im Wissenschaftskolleg in ausreichendem Maße entwickeln können). Jetzt ging es nicht mehr um die grundrechtsdogmatische und verfassungssystematische Frage der Leistungsfähigkeit und Legitimation einer Figur wie der „Staatszielbestimmung" im Unterschied zum „echten" subjektiven Grundrecht, auch nicht mehr primär um die Frage, ob und wie direktdemokratische Elemente (Volksinitiativen usw.) mit dem repräsentativ-parlamentarischen System kompatibel gemacht werden können, vielmehr rückten die zugrundeliegenden Ideale einer sich verfassenden Gesellschaft und die Leitvorstellungen von den Funktionen einer Verfassung mehr und mehr in den Mittelpunkt. Auf das engere Problem der Staatszielbestimmungen bezogen mußte die Frage also dahin umformuliert werden, ob sich aus den aufzuzeigenden Verfassungsfunktionen Kriterien für die Aufnahme oder Ablehnung von Staatszielbestimmungen — und im positiven Falle: welcher? — herleiten ließen?

Unabhängig davon war zu der Frage Stellung zu beziehen, ob die allenthalben — und neuerdings sogar in Bonn — zu beobachtende Tendenz zu einer Aufladung des Verfassungstextes mit im wesentlich nur sozialethisch definierbaren und umsetzbaren, insofern also „unjuristischen" Gehalten und Geboten verfassungsrechtlich zuträglich und vernünftig oder aber von Übel sei. Meine im wesentlichen aus dem Material der neuen Landesverfassungen destillierte Hypothese zielte auf die Konfrontation zweier unterschiedlicher Komplexe von Verfassungsidealen (Leitvorstellungen), welche sich in Ost und West mit unterschiedlicher Stärke zur Geltung bringen. Während die Reformdebatte zum Grundgesetz letztlich bei den Idealen der Französischen Revolution und des liberalen Konstitutionalismus, nämlich von Freiheit, Gleichheit und Brüderlichkeit stehen bleibt, was sich juristisch u. a. in der Entgegensetzung von subjektivem Individualrecht und Allgemeinheit des Gesetzes ausdrückt, wurde und wird in den Diskussionen um die neuen Landesverfassungen eine neue Dreiheit von Verfassungsidealen sichtbar, welche jene andere ergänzt, modifiziert und teilweise aufhebt. Sie läßt sich mit den Stichworten der Sicherheit, der Vielfalt und der Solidarität umreißen. Die Allgemeinheit des Gesetzes bleibt formal bestehen, muß aber allerlei Einschränkungen hinnehmen; das subjektive Individualrecht verliert an Schärfe der Kontur; es wird häufiger nun von Pflichtmomenten begleitet, und die allgemeine Tendenz zur „Ethisierung" der Verfassung ist unübersehbar. Die Auseinandersetzung zwischen diesen beiden Grundpositionen, die sich auch in ihrer Antwort auf die Frage nach dem Sinn von Verfassung unterscheiden, beherrscht derzeit die Verfassungsreformdebatte, sofern sie überhaupt stattfindet, in Deutschland insgesamt. Weil ich in den Diskussionen mehrfach Mißverständnissen ausgesetzt war, darf ich in diesem Zusammenhang betonen, daß sich meine Hypothese zunächst auf die Beobachtung und Feststellung des Widerspiels dieser beiden Verfassungs-Ideal-Triaden beschränkt; ich würde mißverstanden, unterstellte man mir ein unkritisches Eintreten und Werben für die rechtlichen Konsequenzen der zweitgenannten Ideentrias. Ich sehe im Gegenteil eine Menge an juristischen Problemen mit ihr heranwachsen. Aber ich bin andererseits doch davon überzeugt, daß die Ideen der Sicherheit, Vielfalt und Solidarität auf längere Sicht eine solche Dynamik entfalten werden, daß man sich, jedenfalls verfassungsrechtlich, ernsthaft mit ihnen wird auseinandersetzen müssen. Das „Argument", das Grundgesetz sei ohnehin die beste aller Verfassungen und deshalb einer Verbesserung weder bedürftig noch zugänglich, ist jedenfalls unzulänglich.

Die verfassungsrechtspolitische Untersuchung habe ich durch eine gesellschaftstheoretische Parallele zu vertiefen versucht, indem ich den aus den USA nach Deutschland herüberschwappenden Kommunitarismus-

Liberalismus-Streit auf die Verfassungs-Diskussion projizierte. Mein vorläufiges Ergebnis zur sozialphilosophischen Grundsatzfrage, wie sie etwa von K. O. Apel und J. Habermas immer wieder gestellt wurde, geht dahin, daß es verfassungstheoretisch nicht sinnvoll ist, eine scharfe Dichotomie zwischen universalistischen moralischen Sätzen einerseits und nur ethischen, gruppengebundenen Sätzen andererseits durchzuhalten. Vielmehr besteht zwischen beiden ein Ergänzungsverhältnis, welches durch eine vorsichtige Konkretisierung des Menschenwürde-Satzes inhaltlich angereichert werden kann. Diese Überlegungen, die zum Teil noch sehr skizzenhaften Charakter trugen, konnte ich im Dienstags-Kolloquium unter dem Titel „Sicherheit / Vielfalt / Solidarität: Ethisierung der Verfassung?" zur Diskussion stellen. Von der wertvollen Kritik, die meine Thesen erfuhren, erschien mir diejenige (auch schriftlich formulierte) von Catharine A. MacKinnon als besonders wichtig. Sie machte mich anhand der Problematik der Gleichstellung der Geschlechter darauf aufmerksam, daß es bei der Vielfalt doch in Wahrheit um die Frage der Gerechtigkeit — in einem materiellen Sinne verstanden — und damit eigentlich doch um Gleichheit und nicht um Ungleichheit gehe.

Ende Mai 1994 hatte ich dann Gelegenheit, meine Ideen in einem Abendvortrag im Wissenschaftskolleg mit einer etwas veränderten Thematik unter dem Titel „Vielfalt, Sicherheit und Solidarität: Ein neues Paradigma für Verfassungsgebung und Menschenrechtsentwicklung?" der Kritik eines teilweise auch juristisch fachkundigen Publikums vorzustellen. Wie im Titel angedeutet, bezog ich nunmehr die Beobachtung der „Verschiebung" der Verfassungsideale auch auf die internationale Ebene der Menschenrechte. Schon vor einigen Jahren hatte ich die Spannung zwischen dem universellen Geltungsanspruch der Menschenrechte und ihrer Angewiesenheit auf staatliche Souveränität zu ihrer Durchsetzung analysiert. Mir scheint heute, daß die Entwicklung der Menschenrechte eine Art Vorreiterrolle auch für die Entwicklung nationalstaatlich positivierter Grundrechte spielt, daß aber die Abstützung in einer staatlichen Rechtsordnung für die Wirksamkeit beider, der Menschen- wie der Grundrechte, notwendig bleibt. So habe ich jenen älteren Text überarbeitet und mit dem neuen Text zu einer Studie verklammert, die im Herbst unter dem Titel „Menschenrechte und Grundgesetz" erscheinen soll.

Es scheint mir durchaus im Sinne einer Einrichtung wie der des Wissenschaftskollegs zu liegen, daß ihre Mitglieder Kontakte zu anderen akademischen oder universitären Institutionen Berlins pflegen. So nahm ich gern eine Einladung des Fachbereichs Politische Wissenschaft / Otto-Suhr-Institut der Freien Universität an, an einem Symposium aus AnlaB der Verabschiedung J. Fijalkowskis aktiv mitzuwirken. Mein kurzer Vortrag stand unter der Frage: „Was erwartet die Staatsrechtswissenschaft

von der Politikwissenschaft?" Die Antwort: nichts ! fiel für einige Kollegen enttäuschend aus, löste jedoch eine gehaltvolle Diskussion über das Verhältnis der Nachbardisziplinen aus.

Einer aktuellen und sehr konkreten Fragestellung war Mitte April eine von der Friedrich-Ebert-Stiftung veranstaltete Internationale Fachtagung gewidmet: „Nachrichtendienste, Polizei und Verbrechensbekämpfung im demokratischen Rechtsstaat". Im Mittelpunkt standen die derzeitigen Bestrebungen, die Verfassungsschutzämter und den Bundesnachrichtendienst zu Strafverfolgungsorganen gegen die organisierte Kriminalität umzubilden. In einem Vortrag „Verfassungsschutz, Polizei und die Bekämpfung der organisierten Kriminalität" (erster Abdruck in *Frankfurter Rundschau* Nr. 125 vom 1./2. Juni 1994, S. 12) habe ich dazu kritisch Stellung genommen.

Themen der Inneren Sicherheit nahmen und nehmen auch in anderen Zusammenhängen meine Kraft und Aufmerksamkeit in Anspruch: Das betrifft zum einen die Vorbereitung der zweiten Auflage des von H. F. Lisken und mir herausgegebenen und bearbeiteten *Handbuchs des Polizeirechts*, zum anderen die Ausarbeitung einer Antragsschrift für ein Verfahren der abstrakten Normenkontrolle gegen das neue Sächsische Polizeigesetz vor dem Verfassungsgerichtshof des Landes Sachsen in Leipzig. Als „Polizeirechtler", der ich auch bin, darf man Fehlentwicklungen, unter denen später alle Bürger zu leiden haben, nicht immer nur gänzlich tatenlos zuschauen.

Die isolierte Aufzählung dieser Aktivitäten ergibt ein einseitiges und falsches Bild von der Bedeutung, die das knappe Jahr im Wissenschaftskolleg für mich hatte und hat. Wichtig waren mir die Gespräche mit den in- und ausländischen Fellows, die mit Ausnahme von MacKinnon, deren Kolloquium ich moderierte, allesamt Nichtjuristen waren: Historiker, Soziologen, Philosophen, aber auch Biologen. Die Arbeit der Biodiversity-Gruppe begleitete ich mit Interesse und konnte dabei auch mit rechtlichen Hinweisen vielleicht ein wenig nützlich sein. Diskussionen über die deutsche Vergangenheit, über den Nationalsozialismus und den Holocaust nahmen nicht wenig Raum ein; ganz überwiegend waren es gute, ja notwendige Gespräche. Und Berlin ist der richtige Ort, sie zu führen. Ich habe bewußt mit anderen zusammen die Plätze in und um Berlin herum aufgesucht, wo einem die Geschichte sozusagen ins Gesicht und ins „Gemüt" springt.

Gute Freunde habe ich im Kolleg gewonnen. Daß sie so weit in der Welt zerstreut beheimatet sind, wirkt auch als Ansporn. Den „Berlinern" aber, deren Arbeit das Wissenschaftskolleg in Gang hält, gilt mein bleibender Dank.

Ashok Desai

Episodes of Growth and Decline

Born 1936. Studied accountancy in Bombay, economics in Cambridge: Ph D (Cambridge) 1963. Worked at the National Council of Applied Economic Research, Delhi from 1968 till 1991 on industry, technology and trade. Lectured in between at Universities of Oxford, Bombay, Delhi, South Pacific and Sussex. Coordinated an international Energy Research Group of experts 1983-86, published a 15-volume survey of energy research and technology. Worked as Chief Consultant in the Indian Ministry of Finance 1991-93 and helped design the reforms, reviewed in his latest book, *My Economic Affair* (Wiley Eastern, Delhi 1993). Currently Consulting Editor, *Business Standard,* India's second most important financial daily. — Address: *Business Standard,* Pratap Bhavan, Bahadurshah Zafar Marg, New Delhi 110 002, India.

I had expected to work in the Kolleg on ultra-long-term forces shaping economic growth. But my perspective changed between the time when I formulated the proposal in 1990 and when I arrived in Berlin in 1993. In the interval, I worked in the government and was engaged exclusively with extremely short-term problems: the government which came to power in 1991 inherited an economic emergency; every day began with a crisis and one had to think on one's feet. I realized that economic policy is always made in the short term and became interested in the short-term steering of economies — which is the core of policy-making today in many developing countries and East European economies.

What one is trying to do in these countries is to initiate a process of growth in production and employment which can be accelerated and sustained as long as possible. Spectacular bursts of growth have occurred in a number of economies in recent decades — in western Europe in the 1950s and 1960s, in Japan from the 1950s into the 1980s, in Brazil in the 1960s and 1970s, in Korea and Taiwan in the 1970s and 1980s, and the boom which began in China in the 1980s and which still continues. Conversely, there are long periods of stagnation or decline in some countries. Argentina and Philippines are the prime examples, but episodes of poor growth are also marked in the USA, UK, and India.

In studying these processes, one inherits from conventional economics a bias towards efficiency which is useful but insufficient. Economic policy is not written on a clean slate: one starts with an economy that is already there, and its characteristics have to be taken into account. There is considerable empirical literature that is supposed to guide one in the circumstances, based on the successes and failures of various countries. While working in the government I had developed a distrust of this approach. All too often, a bureaucrat who wanted to advocate a particularly stupid policy would argue that it was the policy in some heroic country such as Japan or Korea or Germany. Such arguments were so wrong so often that I formulated a law: every folly has a precedent in a model country.

On arrival in Berlin, I began by reading the literature on comparative economic policy in various countries, and finally decided that lessons of policy were not transferable across economies: a more analytical approach was needed. So I turned to the question: what causes economies to grow faster or more slowly? This is a very old question in economic literature: it goes back a hundred and fifty years at least. In the first half of this century, this line of research crystallized into trade cycle literature. After World War II, it died out as trade cycles in the traditional sense ceased. But more recently, the lasting slow-down in the growth especially of the European economies has led to a revival of interest in one form of trade cycle, namely long waves or Kondratieffs. I went through this literature and decided to work out postwar cycles for various countries.

I spent considerable time trying to derive cycles for a sample of countries. But after much experimentation I was forced to the conclusion that cycles are an artifice of economists, and that no objective method gives cycles of even remotely comparable length for different countries. Very simply, a cycle can be defined as a pair of turning points in an economic series, for instance a series of total output or of prices. I worked out many frequency distributions of the interval between two turning points. They showed that the longer the period between a pair of turning points, the lower the frequency of occurrence of the period. In other words, variations in growth processes can be legitimately viewed as purely random: I could find no robust support for the notion, well established in economics for the last 75 years, that there are Kitchins lasting 3-4 years, Juglars lasting 8 — 9 years, and Kondratieffs lasting about half a century.

Here there is a problem of deriving comparable figures of growth; what is regarded as growth in one period or country may not be so regarded in another period or country. For output is aggregated at a certain set of prices. The prices change so radically over time that countries rebase their output series roughly once every ten years. Relative prices vary even more across countries. At current prices and exchange rates, the output per head

of two countries may differ by 40 to 1; when valued by the same set of prices — prices of either country — the difference often shrinks to 10 to 1 or less. Besides, it is not 10 to 1 in respect to all the goods produced and consumed. The pattern of consumption changes as countries grow richer. So people in a poor country may have only a hundredth as many cars as those in a rich country, but may actually eat more bread or rice per head. These facts are well known thanks to the inter-country comparisons of gross domestic product pioneered by Gilbert and Kravis in the 1950s, and expanded to cover the majority of non-communist countries by Summers and Heston, who carried on their work. I obtained their time series, stretching from 1950 till 1992, for about 56 countries, and carried out a number of experiments on them.

These experiments established that there is a positive association between the long-term growth rates of output in different countries and the stability of growth, as measured by the reciprocal of the coefficient of variation of growth. The result is quite robust for 15 countries that were my primary sample (USA, Brazil, Argentina, Mexico, UK, France, Germany, Spain, Italy, Japan, Korea, Thailand, Philippines, India and Pakistan), less so for the larger sample of 56 countries which included a number whose statistics were suspect or whose openness made them highly susceptible to outside disturbances. I further found that the serial correlation of growth rates was generally positive in fast-growing countries and negative in slowly growing countries: in other words, that a year of high growth was more likely to be followed by another year of high growth in fast-growing countries, and the reverse in slowly growing countries. This result explained the failure to find standard cycles across countries: the cycles were likely to be shorter, and growth was likely to be interrupted sooner in more slowly growing countries. The highest growth rates recorded were often higher in slowly growing countries, but growth episodes collapsed faster in them.

From this point one could proceed in two directions: either one could look for inherent features in the growth processes of slowly growing countries that would lead to their quicker reversal and did not do so in fast-growing countries, or one could look at the operation of constraints in both types of economies. The second approach is easier to follow, since the constraints are well known and have been intensively studied. In particular, the concepts of the internal savings constraint and the external balance-of-payments constraint are standard. So I started in this direction, and established that the interruptions of growth processes were not caused by variations in investment and savings; savings and investment ratios were in general much more stable than output. Some other intriguing results emerged at this stage. For instance, investment-output ratios

over standard periods of 10 or 20 years were distinctly lower in the fast-growing Asian economies — Japan, Korea and Thailand — and higher in the European economies. The latter were so high in periods of low growth that the investment could not have been driven by the rate of return.

Putting these results together, it seemed to me that the variations in growth were caused by variations in demand rather than in supply, and yet, that they were not classically Keynesian in the sense of being driven by instability of investment. After this, the next step would have been to investigate the behaviour of the external constraint in different economies, and also the role of macroeconomic policy. But at this rivetting juncture in the investigation, my work was interrupted by a medical emergency, and self-repair had to take precedence over the repair of economies.

Robert Elegant

But What a School

Born in New York City on March 7, 1928. A.B., University of Pennsylvania, 1946. M.A., Chinese and Japanese Studies, 1950 and M.S., Journalism, 1951 Columbia University, New York. 1994 visiting professor, Boston University. — Books (non-fiction): *China's Red Masters,* 1951. *The Dragon's Seed,* 1959. *The Center of the World,* 1964, rev. ed. 1968. *Mao's Great Revolution,* 1971. *Mao vs. Chiang: The Battle for China,* 1972. *The Great Cities: Hong Kong,* 1977. *Pacific Destiny: Inside Asia today,* 1990. (Fiction:) *A Kind of Treason,* 1966. *The Seeking,* 1969. *Dynasty,* 1977. *Manchu,* 1980. *Mandarin,* 1983. *White Sune, Red Star,* 1986 (published in the US in 1987 as *From a Far Land). Bianca,* 1992. — Address: 7 Brown St., Cambridge, MA 01238, USA.

Besides the opportunity to live again in Germany for a time, I was drawn to the Wissenschaftskolleg by the lure of a more leisurely and more contemplative way of life. I further hoped that the noble steel of better minds would strike sparks from the base flint of my own.

All those expectations were to be realized, but not necessarily in the manner I had anticipated.

Yes, life was somewhat more leisurely and somewhat more contemplative for one accustomed to the rapacious demands of incessantly working on a new novel as well as a little journalism. Yet I found myself doing much the same thing while also attending colloquia and making my small contribution to them.

As for the lunches and the dinners, where almost as much intellectualizing as masticating took place, I am daily admonished by my scales. I have, as the Rector, himself not sylphlike, warned, found myself enlarged at least as much corporeally as intellectually. The Wissenschaftskolleg sticks to your ribs, as well as to your cerebellum.

Having been away from academia for some time, I had forgotten the intense concentration that distinguished scholars bring to bear on their specialities. I had also forgotten the ruthless elimination from their minds of subjects not germane to their own fields. Gustav Ranis and I found ourselves virtually forming a *mini-Schwerpunkt,* being virtually the only fellows with a keen interest in East Asia and some knowledge thereof.

I nonetheless found my colleagues always ready to assist me with views and facts to which I would not otherwise have had access. Since my additional project was a novel on the environmental movement, Dick Vane-Wright, Chris Humphries, and their associates were most directly helpful.

Coming to Berlin was a deliberate effort on the part of my wife and myself to shake ourselves out of the perhaps too comfortable life into which we had settled. We are this year at Boston University, where I am a visiting professor in Far Eastern Studies and Journalism.

In its own way, Boston is as stimulating as was Berlin, particularly because there exists at its seven or eight major universities a concentration of Far Eastern specialists, many of whom are old friends. Curiously, Boston is to me as alien as Berlin. Perhaps even more alien, since I had spent some five years in Germany over the last few decades, but no more than a few months in the United States.

Along with my perennial study of the emerging new balance of economic, military, and political power in East Asia, I was able to look first hand at the emerging new balance of power in Europe. I was, however, disturbed by the lack of knowledge in Europe, particularly Germany, regarding East Asia, as well as a somewhat timorous recoiling from learning more about the most dynamic part of the world. Nothing like the Kaiser's *Yellow Peril,* but, it appears, a wish to shield oneself from the harsh reality of powerful competition with Europe, particularly Germany.

The sojourn was also good for my self-discipline. Having spent decades as a foreign correspondent reporting to a head office ten thousand miles away, I initially found even the light obligations of the Kolleg a little onerous. Although I may not have adjusted completely to those demands, I learned again to live with men and women who are, to say the least, my intellectual equals.

Also the insights, the exchanges, the repartee, and the mental strife. In a way, it was like going back to school for one so long untrammelled. But what a school !

Hans Magnus Enzensberger

Hausaufgabe

Hans Magnus Enzensberger, 1929 in Kaufbeuren im bayerischen Allgäu geboren, hat in Freiburg, Hamburg, Erlangen und Paris à *la carte* studiert und sich seiner akademischen Pflichten mit einer Arbeit über Brentanos Poetik entledigt. Abgesehen von kurzen Intermezzi beim Rundfunk und im Lektorat versucht er sich seitdem als freier Schriftsteller: Er hat Gedichte, Essays, Stücke, Reportagen, Übersetzungen verfaßt, Zeitschriften und Anthologien herausgegeben und, als Verleger zur linken Hand, *Die Andere Bibliothek* gegründet. Nach allerhand Wanderjahren lebt er seit 1980 in München.

There is no free lunch. An diese schlichte Lebensregel, die auch dem Forschungsbetrieb keineswegs fremd ist, scheint das Wissenschaftskolleg zu Berlin sich nicht zu halten.

Es ist nämlich nicht so, daß der prospektive Fellow dort mit einem präzise formulierten Projektantrag vorstellig zu werden hat, samt Fachgutachten und Referenzen in dreifacher Ausfertigung. Was mich betrifft, so hätte ich eine solche Arbeitsbeschreibung auch kaum beibringen können, ein literarischer Autor kann nämlich schlechterdings nicht garantieren, ob und wann er das, was er sich vorgenommen hat, zu Ende bringen wird.

Offenbar vertraut das Kolleg darauf, daß der Eingeladene die Chance, die ihm geboten wird, schon nicht vertrödeln wird. Wahrscheinlich liegt darin sogar der Sinn dieser ungewöhnlichen Institution. Die Vorstellung, jeder Fellow müßte vor seiner Abreise aus Berlin ein gut verschnürtes Ergebnis vorweisen, wäre, glaube ich, konterproduktiv. Die Freiheit der Gäste, vom eingeschlagenen Kurs abzuweichen, gehört zur *raison d'être* des Kollegs.

Für einen Schriftsteller kann das bedeuten, daß er die gewöhnliche Brotarbeit ein Jahr lang beiseitelegt. Ich habe in diesen zehn Monaten keine Essays, keine Kritiken, keine Kommentare, sondern Gedichte geschrieben, eine Tätigkeit, die ebensoviel Zerstreuung wie Konzentration verlangt. Der Luxus, Zeit zu haben, ist dafür unentbehrlich. Das Ergebnis ist ein Buch, das vermutlich, unter dem Titel *Kiosk. Neue Gedichte,* im kommenden Jahr erscheinen wird.

Schwer zu sagen, welchen Einfluß die Umgebung auf ein solches Unternehmen nimmt. Zwar im einzelnen ließe sich das ohne weiteres zeigen: so, wenn in einer Überschrift von „privilegierten Tatbeständen" die Rede ist; ein derart präziser Terminus läßt sich nur mit Hilfe eines Juristen finden. Hier hat eine kurze Unterhaltung mit Erhard Denninger genügt, um das Problem zu lösen. In einem anderen Text tauchen gewisse entomologische Details auf:

Zur Frage der Reinkarnation

Ich betrachte die Fliege,
beschreibe sie,
wie sie ihre Taster rührt,
ihre dreigliedrigen,
dicht gefiederten Fühler,
wie sie sucht, saugt, schöpft
mit den fleischigen Endlippen
ihres Rüssels. Die Flügel,
aschgrau geädert,
glänzend geschuppt,
flimmern im Licht.
Tarsen, Klauen, Borsten
zittern vor Energie.
Mit den zweimal viertausend Linsen
ihrer riesigen Augen
betrachtet sie mich ...

Hier war Scott Camazine aus Cornell der richtige Gewährsmann. Aber solche Auskünfte, so sehr sie mir die Arbeit erleichtert haben, sind nicht das Entscheidende.

Ein weitverbreitetes Vorurteil will glauben machen, Dichter kämen ohne Kenntnisse aus. Ich würde das nicht behaupten. Es kann nicht schaden, wenn die Poesie ihre Ignoranz ablegt und sich, wie einst Lukrez, mit der Wissenschaft einläßt. Dabei verfährt sie wie die Elster, die für ihre diebischen Neigungen bekannt ist. So spielen auch in meinen Versuchen Fundstücke aus der Kunst- und Technikgeschichte, der Sprachphilosophie und der Physiologie eine Rolle. Eine Umgebung, wie das Wissenschaftskolleg sie bietet, war mir dabei von großem Nutzen.

Da aber kein Mensch monatelang nur mit dem Verfertigen von Versen beschäftigt sein kann, wären vielleicht noch ein paar Kleinigkeiten zu erwähnen, mit denen ich mir die übrige Zeit vertrieben habe: die Fertigstellung eines Bandes, der im Herbst 1994 erscheinen soll *(Diderots Schat-*

ten. Unterhaltungen, Szenen, Essays), und einer Anthologie von Reportagen mit dem Arbeitstitel *Die schlimmsten Reisen der Welt;* ein Opern-Libretto (in Zusammenarbeit mit Irene Dische); und die Materialsammlung zu einem Bühnen-Spektakel für den Weimarer Sommer 1995 — *Nieder mit Goethe!* Schließlich, als Fingerübung, eine Übersetzung aus dem Amerikanischen: das berühmte lange Gedicht *The Man with the Blue Guitar* von Wallace Stevens.

Im übrigen habe ich meine verlegerische Nebentätigkeit fortgesetzt. Auch dabei waren die vielfältigen Kontakte des Kollegs hilfreich. So bereite ich die Edition eines Bandes mit Amos Elons Berichten aus Jerusalem (1968-1994) vor, die erstmals einem deutschen Publikum vorgestellt werden; auch an eine Publikation von erotischen Romanen aus dem 18. Jahrhundert ist gedacht, für die Robert Darnton das Patronat übernehmen will.

Vielleicht ist es nicht überflüssig zu erwähnen, daß mir die Arbeit, im Klima der Wallotstraße, leicht von der Hand ging; auch für den, der seit vierzig Jahren kein Gymnasium und keine Universität von innen gesehen hat, ist es eben immer noch ein angenehmes Gefühl, schulfrei zu haben.

Der Renaissanceforscher

Ein Astrolog erloschener Sterne.
Unter dem grünen Lampenschirm
in Princeton, New Jersey,
schreibt er verschollene Codices fort,
umsichtig wie ein Spitzel,
wach wie ein Fälscher.

Wie ein erregter Liebhaber
fühlt er sich ein, von innen,
hautnah touchierend,
in den warmen Handschuh
der Überlieferung,
in die Rinde toter Gehirne.

Sein Latein hält mühelos mit
bei den nachtlangen Gastmählern
in Florenz und Bologna
unter Malern, Mathematikern,
Kardinälen, versunken
in saturnische Konversation.

Dann löscht er das Licht,
öffnet die magnetische Schranke
und fährt über den Freeway nach Haus
im glühenden Natriumdampf
der Peitschenlampen.

Remigiusz Forycki

Récits de voyage des explorateurs français en Russie (1761-1859)

Né en 1950 à Olsztyn (Pologne); ancien élève de l'Université de Poznan (1968 —1973), doctorat de troisième cycle (1977) et doctorat d'Etat (1987). Assistant, puis Maître de conférences à l'Institut de Philologie Romane à Varsovie (1977-1979), Professeur de Faculté (1989-1990), puis, à partir de 1990, Professeur titulaire à l'Université de Varsovie. Professeur associé à l'Université Blaise Pascal — Clermont II (1990-1991), Directeur adjoint du Centre Scientifique de l'Académie Polonaise à Paris (1991-1993). Publications: *Recepcja dramaturgü Wiktora Hugo w latach 1828-1863 w krytyce i na scenach polskich,* Warszawa 1977; *Stendhal. Génie du soupçon,* Varsovie 1987; une cinquantaine d'articles sur les relations franco-polono-russes. Marié, cinq enfants. — Adresse: Instytut Romanistyki UW, ul. Obona 8, PL-00 332 Warszawa.

Parmi les mythes des Lumières, un des plus forts et des plus controversés a été celui qui faisait de l'Europe *le centre* philosophique et pensant du monde. Ce nouveau sentiment de supériorité face à l'autre partie du globe, barbare et non-civilisée, inspirait le besoin missionnaire de porter la lumière aux «aveugles de naissance» (pour Foucault l'une des plus grandes expériences mythiques du XVIII siècle était de «rendre la vue aux aveugles de naissance»). Ainsi, d'une part l'Europe passait pour une métropole intellectuelle du monde et en même temps elle devait sa mission historique à la conquête, à la victoire et à la soumission du monde «barbare», immature. *Eclairer* pouvait signifier une légitimation de l'assujettissement culturel et un droit à la présence sur les territoires non inclus par la pensée, selon les valeurs européennes. Le missionnaire philosophe prend la place du missionnaire jésuite; dès lors l'européanisation sera dirigée comme une croisade intellectuelle au «coeur des ténèbres» pour remplir «la page blanche» *(tabula rasa)* des temps primitifs. «La page blanche» remplie par l'homme blanc de l'Occident deviendra — à côté du mythe de l'Europe, centre pensant du monde — un autre mythe du XVIII siècle, figure du prosélytisme et de l'odyssée civilisatrice aux portes des ténèbres.

Dans cette Europe ayant pour mission de civiliser le monde y avait-il une place pour la Russie? L'apport de l'Empire des tsars au développement de la pensée, de la culture et de la civilisation de l'Occident était-il suffisant pour que la Russie soit reconnue, de plein droit, comme partie autonome de l'Europe? Sur cette question les avis étaient très partagés.

Jusqu'au XVIII siècle une ambiguïté fondamentale demeurait dans l'opinion française au sujet de la Russie: pour les uns, les Russes n'étaient que des Tatars à demi civilisés, pour les autres la Russie était un pays qui semblait annoncer l'avènement de la gloire et de la sagesse sur terre. Lorsque la Russie s'est approchée geôgraphiquement de l'Europe à la suite de la modification de ses frontières occidentales, il fallait modifier aussi l'orientation idéologique et la stratégie culturelle. La réelle présence intellectuelle des Occidentaux en Russie commence à l'époque où les encyclopédistes saluent avec enthousiasme l'avènement de Catherine II, avec laquelle il nouent immédiatement des relations en divers domaines. Les maîtres penseurs français sont comme hypnotisés par le pouvoir illimité dont disposent les tsars — pouvoir capable de changer «l'irraisonnable» cours de l'histoire. Du coup, la Russie se révèle comme un espace vierge, comparable au Paraguay des jésuites. Pour les «intellectuels» français, les Russes n'ont d'histoire que par rapport à la corruption de la civilisation occidentale; ils constatent la barbarie du Nord, puis son aggravation par le christianisme européen. Par rapport à la barbarie des «Scythes» (Russes), la barbarie chrétienne des «Welches» (Français) est totale. Dans leur conception du progrès, les penseurs français attribuent même à la Russie une importante mission civilisatrice. Ils semblent nier l'héritage barbare et asiatique de l'ancienne Russie (l'esclavage) pour assigner à l'occidentalisation voulue par Pierre I et Catherine II la valeur fondatrice d'une grande puissance européenne. Cependant, penser n'est pas faire, et le discours historique des philosophes français est marqué par une rupture profonde par rapport à la réalité sociale. Quoi qu'il en soit, il semble que les services qu'ils ont rendu à la Russie dans son expansion idéologique et politique en Europe ne cèdent en rien aux mérites militaries du feld-maréchal Souvorov.

Catherine II a su réunir autour d'elle un groupe important de savants, de philosophes et d'artistes qui constituaient une sorte de «république intellectuelle». Des initiatives intelligentes et de grande portée rendaient crédible à l'étranger la mission culturelle russe: le nouveau code de lois – *Nakaz,* le fait de se déclarer prêt à publier *l'Encyclopédie à* Riga, l'érection de la statue de Pierre I «farcie» des symboles de la réforme, l'invitation de Diderot, l'abolition de la peine de mort, la tolérance pour les loges francmaçonniques, les sociétés protestantes et catholiques, le droit d'asile accordé aux Jésuites — tout cela était accueilli en Occident avec enthou-

siasme et admiration. Les membres de l'establishment sur le Vieux Continent observaient avec angoisse les agissements des «nouveaux barbares», alors que l'opinion publique libérale regardait à l'Est avec espoir.

Contrairement à cette attente, au lieu de *«Ex oriente lux»*, c'était l'an 1789 qui avait illuminé le ciel de l'Europe ! Si F. Furet a raison en disant que quiconque parle de la Révolution française «doit montrer ses couleurs politiques», alors dans le cas de Catherine II sur ses «couleurs» se portait une ombre noire, réactionnaire, antirévolutionnaire. L'abolition de la monarchie en France a permis de poser les fondements pour une Europe moderne, tandis que dans l'Etat des tsars et de l'autocratie ce même événement a renforcé l'attachement à l'Ancien régime et a fait de la Russie un porte-parole de la réaction et un bourreau pour toute tentative de libéralisme. Avec la *Déclaration des droits de l'homme* la Révolution faisait entrer en vigueur les principes auxquels Voltaire et Rousseau, Diderot et Mably auraient souscrit et dont, avec une extrême habileté, la Minerve du Nord s'était servi. Son «libéralisme» ressemblait, hélas, aux villages de Potemkine, villages en carton ayant peu de chose en commun avec la réalité.

Pourquoi la Russie résiste-t-elle tant aux idées progressistes lancées par la Révolution française? Pourquoi, face à «liberté, égalité, fraternité» passe-t-elle aussitôt de l'autre côté de la barricade, révélant ainsi son caractère étranger, «asiatique»? On s'étonne beaucoup en Europe devant cet étrange refus total des principes de la démocratie. Au seuil du XIX siècle, les différences énormes se sont manifestées dans la manière *de faire de l'histoire russe*. Nous avons affaire ici à deux mentalités totalement opposées: la mentalité politique et la mentalité paternaliste voir familiale. La théâtralisation et l'anecdotisation des événements sont devenues, à la fin du XVIII siècle, un anachronisme insupportable. La Révolution a, à cet égard, profondément changé la mentalité des Européens. Elle a fait prendre conscience aux gens de leur vocation de *sujets;* elle a fait sortir du palais royal le théâtre historique pour le faire entrer sur l'arène politique du monde. La tristesse majestueuse racinienne a cédé la place à l'enthousiasme républicain davidien, la tragédie d'élite a été remplacée par la fête révolutionnaire. Néanmoins, la politisation de l'art, si manifeste sous la Révolution, n'a pas réussi aux muses. Un maigre programme esthétique ne faisait pas le poids à côté du programme politique, très puissant, très dynamique et de grande portée sociale. Il semble que ce «paradoxe littéraire» de la Révolution a ouvert à l'Occident une nouvelle perspective pour la réorientation et un renouvellement efficace de la *forme du discours* sur la Russie. Au lieu d'imiter les Grecs et les Romains, en chantant des hymnes et des odes à la Minerve du Nord, on pouvait à l'époque de Paul I se servir du drame bourgeois et de la comédie sentimentale pour décrire la

réalité russe. Le sentimentalisme éclectique de la fin du XVIII a révelé des possibilités d'inclure les sentiments et la littérature dans l'histoire. La transposition des réalités du système autocratique dans le langage du mélodrame donnait un curieux semblant de familiarité et suggérait l'existence en Russie de relations sociales fondées sur des rapports idylliques au sein de la famille. La forme littéraire libre créait l'illusion de détente politique; elle neutralisait ce qui tourmentait l'esprit — on pensait moins au système inhumain de l'esclavage. L'idéologie de «l'ordre familiale», si excessive, si intenable après 1815, est le produit et la conséquence d'une telle attitude apparemment apolitique. L'idée de la «Sainte Alliance des Pères de l'Europe» a pu naître de l'extrême familiarisation de la vie sociale dans la Russie de la fin du XVIII siècle.

La rencontre à Saint-Pétersbourg des penseurs qui menaient une action anti-napoléonienne (Joseph de Maistre, Mme de Staël, Jean Potocki, Ernst Moritz Arndt, Alexandre N. Galitzine, Adam Czartoryski) constitue une occasion de saisir les divergences qui existaient au sujet de la Russie. Pour les conservateurs, la pacification de l'Europe que Napoléon avait réussie, allait entraîner la liquidation des acquis de la Révolution et la remplacer par la «révolution bottée». Cette situation sera propice à la longue au retour des Bourbons. En même temps le déséquilibre européen permettra à la Russie de jouer un rôle d'arbitre et à Alexandre I de devenir le libérateur de l'Europe et son véritable bienfaiteur. En revanche, l'image libérale de la Russie est plus équivoque. Elle n'est plus le pays des grands espoirs, la «tabula rasa» glorifiée par Voltaire, Diderot, d'Alembert, Grimm, Falconet. Ce n'est point la Russie incarnée par la Mal, démasquée par Chappe d'Auteroche, Masson, Rulhière. Des penseurs comme Mme de Staël ou Jean Potocki sont fascinés par la Russie où se mêlaient nationalités, langues, coutumes, rites, religions, cultures, inégalités socio-politiques, passions criminelles et hospitalité sans aucune borne. Ils sont intrigués par la mystérieuse «âme russe». B. Baczko observe qu'une civilisation se définit par ses barbares (par ses fous — selon Foucault — ou par ses marginaux — d'après B. Geremek) qui «n'existent que parce que d'autres hommes les définissent comme tels». Cette remarque n'est juste qu'à la condition que le mot «barbare» possède un sens péjoratif. Or, «le barbare Russe» n'est plus synonyme de non-civilisé, cruel, inhumain. Les nouveaux barbares jouent désormais un rôle capital dans la propagation et dans la circulation du progrès. Ils accélèrent la diffusion de la civilisation. Leur apparition est accompagnée d'une grande explosion de l'énergie primitive et d'un déchaînement des passions. Il est vrai que les Russes sont *sauvages* par leur manque de Lumières, mais «heureusement» ils sont barbares par leur vigueur militaire, et l'avenir de l'Europe appartient à eux. Les

Français seront écrasés par les barbares du Nord, car leur civilisation est déjà rongée par la servilité.

La vision romantique de la Russie est, elle aussi, pleine d'ambiguïtés. Le premier penseur occidental ayant connu, compris et décrit le système russe dans sa totalité, était Astolphe de Custine. Sa *Russie en 1839* constitue un aperçu *synthétique* de l'autocratie. Il a démonté et décrit «la machine publique» russe, en dévoilant les moindres rouages qui faisaient marcher ce mécanisme sans âme. Dans son livre il dénonce: esclavage, claustrophilie, militarisation, soldatomanie, vol, alcoolisme, tyrannie, hypocrysie, bas niveau intellectuel, réformes perpétuelles «décrétées d'en haut», manque de société civile, etc. Il est plus qu'étonnant, que cette interprétation a été vigoureusement réfutée par Balzac dans sa *Lettre sur Kiew.* Pour décrire la Russie il faut, selon Balzac, se débarasser de l'esprit critique et scientifique, renoncer à la longue tradition culturelle et au développement organique de la civilisation européenne. Il faut donc se faire Russe pour pouvoir parler de la Russie. Ainsi, face à l'Occident «bavard» et «pourri» se dresse la Russie «sainte» et impénétrable où l'unité de l'espace est fondée sur l'unité idéologique.

Ici apparaît la notion chère à Albert Lortholary de «mirage russe». La valeur sémantique de cette expression implique au moins deux concrétisations: *premièrement* le mirage russe en Occident et pour le Européens, donc le fantasme de la «page blanche» et de la conviction qu'on peut réaliser en Russie le Pays de Cocagne, l'Eden terrestre; *deuxièmement* le mirage russe en Russie et pour les Russes eux-mêmes, donc la conviction de la supériorité de la Russie sur les autres pays d'Europe. Maintes fois dans les moments historiques décisifs ces deux aspects du mirage russe se sont combinés pour donner un mélange détonnant qui fraie la voie aux despotismes ou aux totalitarismes les plus cruels. Hélas, ce processus s'effectuait avec l'acceptation et même le concours de ce que nous appelons aujourd'hui *intellectuels* qui trop souvent devenaient les hommes à tout faire de la Russie.

Je tiens à remercier ici le «Kolleg» pour le service en cristal qu'il m'a offert. Sans son aide généreuse, il m'aurait été difficile de mener à bien mes travaux. Soyez assurés, chers Amis, de ma sincère reconnaissance et gratitude.

Nigel Franks

The Genesis of the (Super-)Organism in Evolution and Ontogeny

Born 1956 in Oldham, Lancashire. 1974-76 University of Leeds, Department of Pure and Applied Zoology, B.Sc.; 1978-80 Smithsonian Tropical Research Institute, Panama; PhD. Thesis: *The Evolutionary Ecology of the Army Ant* Eciton burchelli *on Barro Colorado Island, Panama.* 1980-82 Research Fellow, Harvard University. 1982-90 Lecturer, since 1990 Senior Lecturer, in Ecology, School of Biological Sciences, University of Bath. — Research interests in ecology (predator-prey interactions and community structure), behavioural ecology (kin selection, life history strategies, division of labour in social insects), evolution (self-organization and collective problem-solving in social insects). — Address: School of Biology and Biochemistry, University of Bath, Bath BA2 7AY, GB.

The central enigma of biology is that its awe-inspiring diversity and complexity (at all levels from bio-macromolecules to bio-diversity) has arisen from a process, evolution by natural selection, that involves, at least in part, randomness and chance. Self-organization theory begins to suggest how biological complexity can spring from the partly haphazard process of natural selection. Self-organization theory shows how complex and sophisticated patterns and processes can arise through large numbers of surprisingly simple interactions among the sub-components of a system. In other words, how relatively simple micro-processes can generate novel designs at the macroscopic level.

The greatest unsolved problems in the life sciences lie in the domain of developmental biology. Self-organization theory suggests an explanation of how a simple code (i.e. the genetic code) can give rise to the organism, in all its glory, be it *Eschericha coli* or *Elephas maximus*. This theory suggests how biological pattern (phenotypes) can arise without explicit encoding of all its details in the genome: it explains some of the emergent properties in biological organization. However, in science, theory alone does not suffice. Biology is both an empirical and a theoretical subject. One of the major problems in developmental biology is that not only is there, as yet,

no potentially adequate theory but that experimentation is also extremely difficult. It is all too easy to take organisms apart but very difficult to put them back together again. This is, in part, why I have chosen to investigate superorganisms rather than organisms. The supra-organismic ant colony, because it is assembled from individual organisms, can be quickly taken apart and put together again, without subjecting individual workers to invasive treatment. Furthermore, we can directly observe and quantify the critical interactions among its subcomponents: The worker in an ant colony is the functional equivalent of a cell in an organism. But unlike cells in an organism, we can directly observe the interactions among worker ants. For these reasons, social insects offer the possibility of unravelling the key enigma in biology, the genesis of the organism (and the superorganism) in evolution and ontogeny.

Ant colonies embody all of the most important aspects of biological organization. In simple terms, they are more than the sum of their parts and they are robust, flexible, fault-tolerant systems capable of self-repair. Our elucidation of these properties makes these studies of interest not only to the field of biology but also to both the academic and industrial computer-science communities.

My studies at the Wissenschaftskolleg have been dedicated to furthering our understanding of the combined role of self-organization and natural selection in biology. I have contributed to the focal project of my stay in Berlin, a book on "Building Biological Superstructures: Models of Self-Organization"*. In addition, I have completed a monograph on *Social Evolution in Ants* and written or completed a number of primary papers in this field. (These publications are listed below.)

I wish to take this opportunity to thank all the staff of the Wissenschaftskolleg zu Berlin for facilitating every aspect of my studies during the 1993/1994 academic year. I have never before encountered an institution entirely populated by such professional, helpful, cheerful and charming people. The Wissenschaftskolleg zu Berlin is indeed a superstructure which is more than the sum of its parts.

Books in Preparation:

Camazine, S., Deneubourg, J. L., Franks, N. R., Seeley, T. D. (1995) *Building Biological Superstructures: Models of Self-Organization*

* cf. pp. 255 ff. in this volume.

Bourke, A. F. G., and Franks, N. R. (1995) *Social Evolution in Ants.* Princeton University Press (in press)

Papers in Primary Journals:

Sendova-Franks, A. B., and Franks, N. R. (1994) "Social resilience in individual worker ants and its role in division of labour." *Proceedings of the Royal Society.* London (B) 256: 305 — 309

Stickland, T., Franks, N. R. (1994) "Computer image analysis provides new observations of ant behaviour patterns." *Proceedings of the Royal Society.* London (B)

Franks, N. R., and Tofts, C. (1994) "Foraging for work: how tasks allocate workers." *Animal Behaviour* 48: 470-472

(In addition, I have submitted 6 other papers to primary journals during my time in Berlin.)

Chapters in Edited Books:

Franks, N. R. and Partridge, L. W. (1994) "Lanchester's Theory of Combat, Self-Organization and the Evolution of Army Ants and Cellular Societies." In: *Behavioral Mechanisms in Evolutionary Ecology.* (ed. L. Real) Chicago University Press, pp. 390-408

Seminars:

1) "Self-Organization in Sociobiology", Wissenschaftskolleg zu Berlin, 16th November 1993

2) "The Mathematics of Warfare and the Evolution of Combat in Ants." Zoologisches Institut der Universität Zürich, 2nd December 1993

3) "Self-Organization and Collective Intelligence in Ants: Questions of Priority." Schwerpunkt "Emergence of pre-rational intelligence in biology: From sensorimotor intelligence to collective behavior", Zentrum für Interdisziplinäre Forschung Universität Bielefeld, 26th January 1994

4) "Social Organization and Self-Organization Mathematics and Biology", Joint Lecture Programme, Zoologisches Institut und Mathematisches Institut der Universität Würzburg, 11th February 1994

Thomas Gelzer

Klassisches Altertum _ Lebendiges Erbe

Geboren am 29. 6. 1926 in Basel. Dr. phil. I Universität Basel 1953 in den Fächern Griechisch, Latein und Allgemeine Sprachwissenschaft; Gymnasiallehrerpatent in den Fächern Griechisch, Latein und Deutsch. Habilitation: Universität Zürich 1964. Ordentlicher Professor für Klassische Philologie mit besonderer Berücksichtigung des Griechischen: Universität Bern 1970, emeritiert 1991. — Adresse: Institut für klassische Philologie und antike Philosophie, Universität Bern, CH-3000 Bern.

Wenn ich zum Abschluß die Ereignisse und die Ergebnisse dieses Jahres nochmals Revue passieren lasse, so erscheint es mir als eine außerordentlich fruchtbare und gewinnbringende Zeit. Den für meine Einladung an das Wissenschaftskolleg Verantwortlichen sowie dem Rektor und dem Stab seiner Mitarbeiterinnen und Mitarbeiter, die uns alles, was wir brauchten, wohlvorbereitet und mit unermüdlicher Hilfsbereitschaft zur Verfügung stellten, möchte ich vor allem meinen herzlichen Dank sagen. Aus der Fülle der uns hier angebotenen Möglichkeiten und der unter diesen Voraussetzungen gewonnenen Erfahrungen und Eindrücke können hier nur einige wenige Punkte herausgegriffen werden.

Mein Aufenthalt in Berlin galt in erster Linie der zusammenhängenden Begründung und Neugestaltung eines seit langem vorbereiteten Arbeitsprojekts zum Thema „Antikes im Faust II", aus dessen Umkreis einzelne Ergebnisse in verstreuten Teilpublikationen vorläufig vorgelegt worden waren. Hier konnte es entscheidend gefördert, jedoch noch nicht im geplanten Umfang abgeschlossen werden. Es soll möglichst bald noch durch die Einsicht in die von Goethe selber benutzen Bücher und Sammlungen in Weimar ergänzt werden. Dazu trug auch bei, daß ich durch unvorhergesehene Umstände veranlaßt wurde, meinen Arbeitsplan teilweise abzuändern und für mich selber allerdings auch sehr interessante, aber etwas disparate Gegenstände zu behandeln. Für den Abschluß des Projekts, das zur Beantwortung der ausgewählten Fragen einer gewissen Breite der dokumentarischen Nachweisungen zu Goethes Rezeption seiner antiken Quellen und zu ihrer schöpferischen Umwandlung im Hin-

blick auf die Verwendung im Faust bedarf, zeichnen sich jetzt die folgenden Leitlinien ab. Als Goethe von 1825 an die neue Bearbeitung des Faust II in die Hand nahm, hatte er sich von dem exklusiven Klassizismus der Zeit um 1800 weitgehend gelöst. In der „Helena", dem Zentrum des ganzen Faust II, die er zuerst allein publizierte, ging es ihm gerade auch darum, „daß der leidenschaftliche Zwiespalt zwischen Klassikern und Romantikern sich endlich versöhne." Das Altertum erhielt damit im Faust II keine geringere, aber eine veränderte Bedeutung, und es war zu untersuchen, wie Goethe damit nun verfährt. Anregungen aus dem Altertum spielen eine Rolle in allen Abschnitten des Handlungszusammenhangs, der mit Fausts Gang zu den Müttern einsetzt, über die Klassische Walpurgisnacht zur Vereinigung mit Helena im dritten und dann über ihre Ersetzung durch ein verwandeltes Gretchen am Anfang des vierten Aktes nach den Verstrickungen im vierten und zu Beginn des fünften Aktes zu seiner Erlösung in den letzten Szenen führt. Dahinter läßt sich ein vom Neuplatonismus geprägtes Weltbild erkennen, das Goethe am deutlichsten in den letzten Szenen zur Schau bringt. Querverbindungen zu den anderen Teilen dieses Handlungsstrangs weisen auf den inneren Zusammenhang hin. Die Chronologie der Entstehung und der Ausarbeitung ist von Bedeutung für die Erfassung von Goethes Intentionen. Er hat sich wie von allen anderen Anregungen, die er aus verschiedenen Quellen übernahm, auch vom Neuplatonismus ein eigenes Bild geschaffen. Wichtig ist dabei, daß er ausging von Plotin in der Übersetzung und mit dem Kommentar des Ficino. Dort fand er eine Konzeption der Schönheit, der Liebe und der Kunst, die ihn besonders ansprach. Dazu hat er auch Bilder und Handlungsmotive aus Dantes *Divina Commedia* übernommen Es war zu untersuchen, wie er sich dabei mit Dantes christlicher Dogmatik auseinandersetzt. Anläßlich meines Colloquiums im Wissenschaftskolleg und in Vorträgen, zu denen ich an die Freie Universität und die Humboldt-Universität in Berlin sowie an die Universität in St. Petersburg eingeladen wurde, hatte ich Gelegenheit, das Problem des Neuplatonismus im Faust II zur Diskussion zu stellen und erhielt dabei mancherlei förderliche Hinweise.

Ein besonders vielschichtiges und rätselhaftes Stück ist die Klassische Walpurgisnacht. Goethe stellte sich damit die Aufgabe, zur alten nordischen Walpurgisnacht ein „antikisches" Gegenstück zu schaffen, das in die Richtung der freieren Kunstregion und der höheren Ansichten weist, die sich Faust in seinem Verhältnis zu Helena eröffnen. Er setzte beide durch eine Reihe teilweise fast wörtlicher Anspielungen in Beziehung oder, besser gesagt, in sichtbaren Gegensatz zueinander. Die Repräsentation des Altertums sieht da ganz anders aus als in der „Helena", die sie vorbereitet. Zu ihrer Gestaltung hat er Anregungen von verschiedenen Seiten her mit-

einander verflochten. Für den ersten Teil, das „Schauderfest", lehnte er sich an die thessalischen *Peloria* an. Zur dramatischen Gestaltung übernahm er Anregungen von Aristophanes. Hinter den in der Klassischen Walpurgisnacht erscheinenden mythologischen und anderen Figuren steht die an Plato orientierte Seelen- und Dämonenlehre, die Plutarch in einigen Traktaten seiner *Moralia* darstellt. Sie lieferte auch Anregungen für die Gestaltung des Mysterienfestes in den Felsbuchten des ägäischen Meeres. Dazu übernahm er auch Motive der Handlung und Personen von Dante. Um seinem Leser den verborgenen Sinn der Bilder und der Handlung im Zusammenhang des Ganzen verständlich zu machen, bediente er sich einer Technik der Hindeutungen und der Spiegelungen, deren Anwendung gerade hier beispielhaft beobachtet werden kann. Davon habe ich einiges dargestellt in einem Artikel „Mythologie, Geister und Dämonen: Zu ihrer Inszenierung in der Klassischen Walpurgisnacht", der in der Festschrift zu Ehren von Hellmut Flashar erscheint.

Für ihre neugegründete Zeitschrift *Hyperboreus* baten mich meine Petersburger Freunde um einen Beitrag. Dafür eignete sich die im 18. Jahrhundert von Christian Friedrich Matthaei in Rußland, genauer gesagt in Moskau, gefundene Handschrift M der Homerischen Hymnen, in der allein der große *Demeter-Hymnus* (Hom. Hy. 3) überliefert ist und die auch in der Geschichte der Philologie eine nicht unbedeutende Rolle spielt. In einem Artikel „Zum *Codex Mosquensis* und zur Sammlung der *Homerischen Hymnen"* wird der Versuch unternommen, den Weg der Homerischen Hymnen von der Entstehung der erhaltenen Sammlung an bis zur ersten Ausgabe des *Demeter-Hymnus* durch David Ruhnken nachzuzeichnen. Die Sammlung der Homerischen Hymnen ist auf zwei Wegen überliefert, 1.) in der Handschrift M, 2.) in einem erweiterten sogenannten „Hymnencorpus" zusammen mit den *Orphischen Argonautika,* den *Orphischen Hymnen* und denen des Proklos und des Kallimachos. Außer den wenigen Hymnen, die schon die Alexandriner kannten (zu ihnen gehört auch der *Demeter-Hymnus),* aber nicht als Homerisch anerkannten, ist die Großzahl der anderen erstmals zitiert in den *Orphischen Argonautika,* d. h. wohl etwa im 5. Jh. n. Chr. Die Sammlung dürfte also nicht lange vorher entstanden sein. Eine Reihe anderer Faktoren machen es wahrscheinlich, daß sie in Alexandria in neuplatonischer Umgebung zusammengestellt wurde, und dann in einer Variante in das „Hymnencorpus" übernommen wurde. Handschriften beider Zweige kamen wohl zu Anfang des 7. Jahrhunderts, zur selben Zeit wie die neuplatonischen Schriften der Alexandriner Schule, nach Konstantinopel. Sie blieben dort wie diese und andere heidnische Schriften lange im Verborgenen und sind deshalb wie diese in verstümmeltem Zustand erhalten. Dort wurden das Hymnencorpus im 14. und die Vorlage von M in der Mitte des 15. Jahr-

hunderts wiedergefunden. M ist von Ioannes Eugenikos geschrieben. Die Handschrift wurde im 17. Jahrhundert wohl in Konstantinopel erworben von einem griechischen Hieromonachos Dionysios, der sie nach Nezin in der heutigen Ukraine brachte, bevor er sie 1680, als er Lehrer an der akademischen Schule der Brüder Lichudes in Moskau geworden war, mit seiner Bibliothek nach Moskau transportierte. Dort gelangte sie schließlich in die Hände des Mannes, von dem Matthaei sie kaufte und in Abschriften sowohl Ruhnken zur Publikation wie dem Grafen Christian Stolberg zur Übersetzung zur Verfügung stellte. Der Wettbewerb zwischen diesen beiden um die erste Bekanntmachung und gewisse unvorhergesehene Verwicklungen führten dazu, daß Ruhnken genötigt war, den *Ceres-Hymnus* in drei „Erstpublikationen" vorzulegen. Später konnte er die Handschrift für die Leidener Bibliothek erwerben, wo sie jetzt liegt. Einiges davon, was schon den Zeitgenossen der ersten Publikationen (z. B. durch die *Göttinger Nachrichten)* oder doch seit dem 19. Jahrhundert bekannt war, ist in den gängigen Ausgaben der *Homerischen Hymnen* wieder vergessen worden. Dazu wurde ich eingeladen, meine Ergebnisse in St. Petersburg in einem Vortrag an der neuen Bibliotheca Classica vorzustellen, aus dessen Diskussion sich noch einige neue Aufschlüsse über das russische Wegstück dieser Geschichte ergaben.

Für die zusammen mit zwei Berner Kollegen vorbereitete Publikation eines unpublizierten Goldamuletts, der *Lamella Bernensis,* mit der zusammen zwei andere Amulette (die *Tablette magique de Beyrouth,* erste Publikation 1909, dann nochmals 1991, und PSI 29, 1912, = PGM XXXV, 1931) neu herausgegeben und kommentiert werden, hatte ich die Beschreibung der drei Amulette zu übernehmen. Die neue Lesung (zusammen mit Chr. Schäublin) der schwer lesbaren Texte und die genauere Beobachtung ihrer Herstellung führte zu einigen interessanten Ergebnissen betreffend die Datierung (die Schriften wurden von Peter Parsons und Mitarbeitern ins 5./6. Jh. n. Chr. datiert), den Text, die Komposition und die Beschriftung dieser frühbyzantinischen, aber in einer langen Tradition stehenden, auf Gold, Silber und Papyrus geschriebenen Amulette. Sie brachte auch einige neue Aufschlüsse zum Text, zur Sprache sowie zur Verbindung von Zeichnungen, Zauberzeichen und Text gegenüber Preisendanz' Ausgabe und Interpretation der PGM XXXV.

In Berlin fand ich ganz ausgezeichnete Arbeitsbedingungen. Der liebenswürdige Empfang und die Einweisung in das Wissenschaftskolleg durch Herrn Meyer-Kalkus machten sie sozusagen vom ersten Tag an zugänglich. Besonders wertvoll war mir für meine Arbeit die unkomplizierte und effiziente Beschaffung und die Lokalisierung und Bereitstellung nicht ausleihbarer, nur in den Bibliotheken benutzbarer Bücher durch Frau Bottomley und ihre Mitarbeiterinnen, die auch in schwierigeren Fäl-

len bibliographische Recherchen besorgten. Das war um so wichtiger, als einstweilen kein vollständiger Zentralkatalog der Berliner Bestände in einer öffentlichen Bibliothek vorhanden ist. Herr Lindenberg und seine Mitarbeiter leisteten mir bereitwillig Hilfe und Anleitung bei der Einrichtung und dem Gebrauch des Computers. Ebenfalls schon bei meiner Ankunft fand ich eine Einladung von den Kollegen des Seminars für Klassische Philologie der Freien Universität vor, ihre reiche und ausgezeichnet organisierte Bibliothek zu benützen. Sie stellten mir dort auch einen Arbeitsplatz (neben demjenigen von Anthony Grafton) zur Verfügung. Vielerlei Anregungen erhielt ich im freundschaftlichen Verkehr und in Diskussionen mit den Herren Ehlers, Harlfinger, Krischer, Seidensticker und Frau Karin Alt. Durch sie lernte ich auch den Germanisten Joachim Wohlleben kennen, der mir ebenfalls großzügig Rat und Hilfe gewährte. Die Hilfsbereitschaft auch auf allen Stufen ihrer Mitarbeiter und studentischen Hilfskräfte war mir sehr wertvoll. Herr Christian Brockmann verschaffte mir den Zugang zu den überlieferungsgeschichtlichen Ressourcen des Aristoteles-Archivs. Das FU-Seminar ist auch ein Treffpunkt anderer in Berlin arbeitender Philologen und auswärtiger Gäste, von denen ich einige dort kennenlernte. Dank Frau Bottomley konnte ich mich auch nach und nach in der Benützung anderer Bibliotheken zurechtfinden. Wir erhielten eine nützliche Einführung durch einen leitenden Bibliothekar in die Staatsbibliothek (jetzt Haus 2) an der Potsdamer Straße, wo ich dann auch die Handschriftenabteilung benützte. Ältere Bücher sind, so weit die Bestände erhalten sind, in der Staatsbibiliothek Unter den Linden (jetzt Haus 1) zu finden. Von dort wurden mir auch sehr großzügig ältere Bücher zur Ausleihe überlassen. Dort sind in einem der Lesesäle auch alte Handbücher und Enzyklopädien bequem zur Hand. Die Schwierigkeiten der Koordination und Zusammenführung der beiden Staatsbibliotheken, die sich auch für die Benützer einstweilen noch nachteilig auswirken, ließen von dieser Seite her etwas von den Problemen der Vereinigung der beiden Teile Berlins erkennen. Weiteres stand auf der Universitätsbibliothek der Freien Universität zur Einsicht bereit, wo aber wiederum ältere Hilfsmittel (z. B. die Handschriftenkataloge der italienischen Bibliotheken) nur lückenhaft vorhanden sind.

Im Zuge der unter der Leitung seines Rektors Wolf Lepenies auf Öffnung, Schaffung von Kontakten, Kommunikation und Präsenz des Wissenschaftskollegs in Berlin und im internationalen Austausch ausgerichteten Politk des Hauses ist uns von Anfang an eine Vielzahl von Anregungen und Erfahrungen übermittelt worden. Gleich zu Beginn wurden wir bei einem Empfang mit Berliner Kollegen aus verschiedenen Disziplinen und mit Persönlichkeiten des wissenschaftlichen Lebens bekanntgemacht und damit wurden erste Kontakte hergestellt, die sich im Verlauf des Jahres als

wertvolle Hilfen zur Einführung in das Leben der Stadt bewährten. Dort lernte ich auch den Leiter der Außenstelle der Schweizerischen Botschaft in Berlin, Minister Paul Widmer, kennen, der umsichtig und mit großem Erfolg im Dienste des kulturellen und wissenschaftlichen Austauschs zwischen der Schweiz und Deutschland arbeitet. Durch ihn und in seinem gastfreundlichen Hause wurde ich mit Schweizer Kollegen an den Berliner Hochschulen bekanntgemacht, begegnete andern, die auf der Durchreise in Berlin waren, und wurde zu wissenschaftlichen und künstlerischen Veranstaltungen eingeladen, an denen Deutsche und Schweizer zusammenwirkten.

Als kurzzeitiger Fellow des Wissenschaftskollegs und nachher als Gastdozent am Deutschen Seminar der FU war der Germanist Karol Sauerland in Berlin, den ich früher schon in Wolfenbüttel und in Bern getroffen hatte. Er interessierte sich für meine Arbeit am Faust II und stellte sich als Moderator meines Colloquiums zur Verfügung. Seine Frau leitet eine Arbeitsgruppe über zeitgenössische Schweizer Literatur in Warschau. Beide sind von Minister Paul Widmer empfangen worden, der Herrn Sauerland für eine öffentliche Diskussionsrunde mit unserer Nationalratspräsidentin Lang in Berlin im August eingeladen hat. Wir sind seither in lebhaftem Austausch geblieben und werden ihn auch weiter pflegen.

Das Zusammenleben und Arbeiten mit den Fellows aus nahen und entfernten Ländern, aus dem Westen und aus dem Osten, und das Gespräch mit Vertretern verschiedenster Disziplinen erwies sich als äußerst anregend. Im geselligen Verkehr, bei den gemeinsamen Mahlzeiten, beim gemeinsamen Besuch von Theatern, Konzerten und Vorträgen entwickelten sich freundschaftliche Beziehungen, die uns weiterhin verbinden werden.

Im Rahmen der Veranstaltungen des Wissenschaftskollegs dienen auch die regelmäßigen Dienstagscolloquien, an denen die Fellows über ihre Arbeit berichten und unter der Leitung eines Moderators aus ihrem Kreis diskutieren, dem gegenseitigen Kennenlernen und vermitteln auch Einblicke in die verschiedensten Sach- und Forschungsgebiete und ihre Probleme.

Da wir auch zur Kritik aufgefordert sind, erlaube ich mir immerhin die Bemerkung, daß die Verpflichtung zur Vorbereitung und zur Teilnahme an den Colloquien und die große Zahl der Vorträge und Darbietungen, an denen die Präsenz der Fellows erwartet wird, gelegentlich eine kritische Grenze erreichen, wo sie zur Belastung zu werden drohen, die auf Kosten der Konzentration auf die wissenschaftliche Arbeit geht. Es wäre zu überlegen, ob solche Veranstaltungen nicht mit Vorteil eher auf die Einführungsphase und auf einige zentrale Anlässe des Hauses konzentriert werden könnten. Am Anfang wäre auch der Nutzen formeller Diskussionen

größer, während man sich mit der Zeit so gut kennenlernt, daß Gespräche unter Interessierten leichter ohne Anleitung zustandekommen.

Aus der Vielfalt des Gebotenen möchte ich aber auch dankbar auf die musischen Veranstaltungen hinweisen, die mir besondere Freude bereiteten: eine kommentierte Vorstellung eigener Kompositionen von György Kurtâ.g zusammen mit seiner Frau, einen exquisiten Klavierabend von Martâ Kurtag, den Zyklus von Alan Marks mit Schuberts sämtlichen Klaviersonaten, Konzerte des Vogler-Quartetts und des Ensembles Divertimento Berlin, bei dem wir Musiker der großen Berliner Orchester in heiteren Darbietungen kennenlernten. Aus dem Bereich der zeitgenössischen Literatur gewährte uns Hans Magnus Enzensberger mit einer brillanten Lesung Einsicht in seine neuesten Gedichte. Einmal besuchte uns Günther Grass und gab uns die Gelegenheit, uns mit ihm zu unterhalten. Außerhalb des Kollegs führte uns Dorothea Zanker durch die von ihr gestaltete Ausstellung „Idea und Natur" mit Aquarellen und Zeichnungen romantischer Maler aus dem Lenbachhaus in München.

Ich hatte Berlin das erste Mal noch vor dem Bau der Mauer und seither mehrmals bei kurzen Besuchen gesehen. Die Stadt in ihrem jetzigen Zustand, ihr großes Angebot an wissenschaftlichen und kulturellen Veranstaltungen, Museen, Theatern, Oper, Musik, Ausstellungen auf der einen und ihre Probleme und Schwierigkeiten auf der anderen Seite näher kennenzulernen, erwies sich dennoch als eine anspruchsvolle Aufgabe, zu der uns von Seiten des Kollegs jede mögliche Hilfe geboten wurde. Frau Sanders hielt uns auf dem laufenden über Programme und günstige Sonderangebote und besorgte mit unermüdlicher Geduld und Liebenswürdigkeit Karten und Eintritte für Veranstaltungen aller Art. Für mich traf es sich besonders gut, daß in diesem Jahr das Berliner Philharmonische Orchester einen Faust-Zyklus durchführte, von dem mir die großartigen Aufführungen der „Damnation de Faust" von Berlioz und der „Szenen aus Goethes ‚Faust` " von Schumann in eindrücklicher Erinnerung bleiben.

Eine ausgezeichnete Einführung in die aktuellen Probleme der vereinigten Stadt bot die Einladung zu einem Besuch des Berliner Abgeordnetenhauses, des ehemaligen Preußischen Landtags, bei der uns Frau Hanna-Renate Laurien temperamentvoll in die politischen Probleme einführte und sehr freizügig Fragen beantwortete. Von besonderem Interesse im Hinblick auf politische Fragen waren auch die Vorträge des damaligen Präsidentschaftskandidaten, Jens Reich, anläßlich der Verleihung des Anna-Krüger-Preises und die erste Ernst-Reuter-Vorlesung von Jean-André François-Poncet über „Globale Herausforderungen und Europäische Antworten" aus französischer Sicht. Interessante Perspektiven eröffnete eine Vorstellung der Arbeit und der Mitarbeiter des Forschungs-

schwerpunkts „Zeithistorische Studien" in Potsdam durch Herrn Kocka, wo Historiker westlicher und östlicher deutscher und ausländischer Herkunft zusammenarbeiten an der Erforschung der Geschichte der beiden Teile Deutschlands.

Vermittelt durch die Beziehungen von Mitgliedern des Wissenschaftskollegs und von Fellows erhielt ich auch die willkommene Gelegenheit zum Kennenlernen einer Anzahl sonst nicht leicht zugänglicher Orte und Institutionen in und um Berlin. Davon möchte ich besonders erwähnen: eine Führung durch die „Villenkolonie Grunewald"; einen Besuch in Wilhelm v. Humboldts durch Schinkel erbautem Schloß Tegel mit seiner klassizistischen Sammlung historischer Kunstwerke und den Grabstätten im Park; eine Führung durch einen Konservator des Schlosses Sanssouci, bei der uns auch die Bibliothek, andere sonst dem Publikum verschlossene Räume und die in Restauration befindliche Gemäldegalerie gezeigt wurden; eine Führung durch Professor Michael Brocke über den Jüdischen Friedhof in Berlin-Weißensee; und speziell das Altertum betreffend: eine Einführung in die Räumlichkeiten, die lange gehorteten Schätze der Abklatsche und in die erneuerte Arbeit durch den Leiter des „Corpus Inscriptionum Graecarum" in der ehemaligen Akademie, und eine Führung durch die zu neuer Aufstellung vorbereiteten Funde des Telephos-Frieses und des Altarraums des Pergamonaltars im Pergamonmuseum.

Von besonderer Bedeutung war für mich die Wiederaufnahme und die neue Anknüpfung von Kontakten mit Wissenschaftlern aus dem uns lange Zeit weitgehend verschlossenen Osten. Unter den Preisträgern des am 11. November 1993 ausgerichteten „New Europe Prize for Higher Education and Research" begegnete ich unerwartet meinem hochgeschätzen Freund Alexander K. Gavrilov aus St. Petersburg wieder, mit dem zusammen ich ein Jahr in Princeton verbracht hatte, und der für die Vorarbeiten zu seiner neuen Ausgabe des Marc Aurel einen Monat in der Schweiz an der Fondation Hardt gearbeitet hatte. Der Preis war bestimmt zum Aufbau einer neuen „Bibliotheca Classica" in St. Petersburg. Neue Kontakte ergaben sich anläßlich der von Bernd Funck organisierten Konferenz über „Akkulturation und politische Ordnung im Hellenismus" (10.-14. März 1994), die ungefähr ebensoviele Forscher aus dem Osten und aus dem deutschsprachigen Westen zusammenführte. Daran nahmen unter anderen fünf Kollegen aus St. Petersburg, zehn aus Moskau, weitere aus Rumänien, Ungarn und Polen teil. Daraufhin wurde ich durch Vermittlung von A. K. Gavrilov und A. A. Zaicev vom Petersburger Kulturfonds vom 15.— 22. April 1994 zu Vorträgen an der Universität und an der Bibliotheca Classica eingeladen. Dank der ausgezeichneten Vorbereitung und der überwältigenden Gastfreundschaft der Petersburger Kollegen wurde dieser erste Aufenthalt in Rußland für mich sehr ertragreich und zu

einem überwältigenden Erlebnis. Ich erhielt dabei auch Gelegenheit, das neue Klassische Gymnasium zu besuchen, an dem im laufenden Schuljahr der erste Jahrgang von Abiturienten ausgebildet wird. Mit großer Bewunderung konnte ich feststellen — und das galt schon für die Konferenz in Berlin —, wie gut meine russischen Kollegen wie auch die Lehrer am Klassischen Gymnasium und sogar russische Studenten die deutsche Sprache beherrschen als lebendiges Erbe der St. Petersburger wissenschaftlichen und kulturellen Tradition. Die großen Bibliotheken der Stadt enthalten reiche und alte Bestände an Büchern, die aber seit den 20er Jahren und vermehrt seit dem Zweiten Weltkrieg vernachlässigt wurden, und was dort noch vorhanden ist, ist vor allem den Studierenden nur schwer zugänglich. Es zeigte sich, daß die Bibliotheca Classica einem dringenden Bedürfnis zur Ausstattung mit allgemein zugänglicher neuer Literatur und mit technischen Arbeitsinstrumenten zur Neubelebung der großen alten Tradition der russischen Altertumswissenschaft und zur Ausbildung von Nachwuchskräften als Lehrer und Forscher in höchst erfolgreicher Weise entspricht und derart sofort zu einem Nucleus lebendiger wissenschaftlicher Aktivität geworden ist. Sie erfreut sich auch bereits des Interesses und der aktiven Unterstützung durch die internationale Altertumswissenschaft. Ich habe darüber einen kleinen Bericht zu Händen der Stifter des „New Europe Prize" und anderer Interessenten verfaßt.

Anthony Grafton

Twelve Months in Oz

Anthony Grafton was born in New Haven (CT) on May 21, 1950. He studied history, classics and history of science at the University of Chicago and University College London, where he worked with Arnaldo Momigliano. After receiving his doctorate in 1975, he became an assistant professor of teaching history at Princeton University, where he has been ever since. He became Dodge Professor of History at Princeton in 1993. Guest professorships: Columbia University, California Institute of Technology, Collège de France, Ecole des Hautes Etudes en Sciences Sociales. Publications: *Joseph Scaliger* (Oxford, 1983-93); (with L. Jardine) *From Humanism to the Humanities* (London and Cambridge [MA], 1986); *Forgers and Critics* (Princeton, 1990; German 1991; French 1993); *Defenders of the Text* (Cambridge [MA] and London, 1991). — Address: Department of History, 129 Dickinson Hall, Princeton University, Princeton, NJ 08544, USA.

On August 2, 1993 we arrived, hot, dusty, and exhausted at Tempelhof. The cheapest one-way flights we could find (on Sabena) had brought us from JFK, by way of a few hours of sightseeing in Brussels and an oddly long flight on an oddly small prop-plane, to Berlin. A European academic family, arriving at New York, as we did in Berlin, dishevelled, overwhelmed with luggage, and unequipped with visas, would have been put immediately in the holding pen and left there to starve. We, by contrast, were greeted with civility. And as soon as I had explained, in German that had all the charm and color of a trickle of brown water from a rusty pipe, that I was a visiting professor, all further formalities disappeared and the way to the two taxis we needed was made straight. We could see that we were no longer in Kansas.

Our first sight of the Wissenschaftskolleg brought the further realization that we had in fact reached Oz. This impression only grew in strength with the days — for example, at the first assembly my son and I attended at the Rathenau Gymnasium, where an all-girl steel band serenaded new children and their parents (we have no steel bands in central

New Jersey; also no Gymnasien). Clearly we would have to keep our eyes open and learn to duck.

In fact, our Berlin year had even more surprises in store for us than these first strange weeks led us to expect. My work began more or less as planned. I spent the first part of August proof-reading and indexing the big book on the sixteenth-century scholar Joseph Scaliger, which I had finished a year before: miraculously, the volume appeared on schedule, before the end of the year. September and October went on making a book out of some lectures about four Renaissance intellectuals — Leon Battista Alberti, Pico della Mirandola, Guillaume Budé and Johannes Kepler. I had tried to work out from a variety of different kinds of evidence — ranging from the skills they had learned in school to their own manuscript notes to their own literary works — the different ways in which these men had read and used Greek and Latin texts. I had held the lectures in Rome, Paris and Ann Arbor, in languages as varied, and I needed to do more research as well. In September, before the Kolleg library was open, the seminar libraries of the *Freie Universität* provided a wealth of material on their blissfully open shelves. In October, when I leapt on the newly available loan service like hunger on a loaf, a flood of material from the *Staatsbibliothek*, the FU and Berlin's other remarkable libraries began to course across my desk. The manuscript expanded: articles published in a vast range of French historical journals in the last century, for example, revealed in vivid color and detail the Parisian library and bookselling scene that Budé, the founder of the Collège de France, knew and worked in. In early November I finished a draft and sent it off to my publisher. I assumed that my substantial work on the book was done: that it would soon be accepted, and after a couple of weeks' polishing go into production; and that I could now start work on my real object for the year, the Renaissance astrologer, mathematician, dream-interpreter and mad savant Girolamo Cardano — whose books were readily available to me (at least on Thursdays and, stretching a point, on Mondays) *in Haus I* of the Stabi.

It was not to be. The wonderful Cardano collection at the Stabi, all of it duly listed in the standard finding guide for 16th century books, turned out to be a set of ghosts, and not in the standard bibliographical sense. Almost all of the books had disappeared during the war, leaving behind only their cards in the old catalogue. The two experts consulted by the publisher of my book on reading, following American custom, disappeared into black holes or brown studies; one took months to produce a report, the other never did so at all. Meanwhile the material grew. A visit to Tübingen enabled me to meet a local expert on Kepler, Friedrich Seck, who led me to a large amount of unstudied material on teaching at the

University of Tübingen in Kepler's time as a student. Suddenly I could reconstruct, in intricate detail, how one of the greatest scientists in the history of the west had learned the techniques of classical philology that he applied — it was an age of encyclopedists — throughout his life, alongside his mathematical techniques. Even more exciting news came from a colleague at the Wissenschaftskolleg, Thomas Gelzer, to whom I described my work one day while waiting for the gong to sound. I mentioned that Budé's family had converted to Protestantism and moved to Geneva, and that one of them, at the end of the last century, had still had seven of Budé's notebooks, bulging with extracts from classical texts. Within a week after this short conversation in the *Clubraum* I had been invited to come to the *Fondation Hardt,* a classical research institute high on a hill outside Geneva, and spend a week studying the notebooks at my leisure. They posed new questions and offered new insights. By the end of the year at the Kolleg, when my publisher finally offered me a contract for the book I had submitted in the fall, the ironic witchcraft of the Kolleg's *genius loci* had given me enough new material to require another rewriting of the text, which will take place in New Jersey and reach completion, I hope, before Halloween.

I did work on Cardano — quite a bit of it. An almost complete edition of his works appeared in the mid-17th century. The modern reprint of this offered plenty of undisturbed plots to excavate. A few books of importance turned up in the new Stabi; microfilms of other texts came from Göttingen and elsewhere; a visit to Paris in May, where I served as *Directeur d'Etudes Associé* at the *Ecole des Hautes Etudes en Sciences Sociales,* gave me time in the matchless collections of the *Bibliothèque Nationale* as well as the chance to present and discuss my work with some very lively Parisian scholars. Especially rewarding was the discovery that a rich vein of complementary evidence about an astrologer contemporary with Cardano, Erasmus Reinhold, lay unpublished in the *Geheimes Staatsarchiv* in Dahlem. Above all, the year of conversations with Thomas Gelzer, Francois Hartog, Ora Limor, Richard Trexler and Paul Zanker gave me a range of new questions to think about and new scholarship to read. I presented my tentative results in April at the fellows' colloquium and ended the year with a bit over half the research for Cardano done — as well as a new outline for the whole enterprise, which hardly resembles the one I arrived with a year ago.

I failed to finish with Cardano, quite simply, because Berlin — and the Wissenschaftskolleg — came between me and my Renaissance man. Wilhelm Schmidt-Biggemann, historian of Renaissance philosophy at the FU, invited me to join his seminar during the fall semester and to codirect a *Forschungskolloquium* in the spring, on "Spekulative Philologie in der

frühen Neuzeit". This naturally took place in German (even philosophical German). Frustrated by my feeble active command of the language, inspired by the teaching of Eva Hund, and supported by my wonderful colleagues in the advanced learners' group, I plunged in and tried to learn, not only to speak but also to write this most Begriff-stricken of languages. Several texts I wrote in German words (other will have to judge the sentences) will appear in the fairly close future — one in a volume of talks from this July's conference on "Formen der Wissensvermittlung" at the Potsdam *Einstein Forum,* another in a volume of essays stemming from the *Forschungskolloquium* at the FU. More important, as my German ceased constantly to creak and sputter, I realized that I had landed in the midst of a very talented group of younger scholars and advanced students, whose nets of contacts extended to their contemporaries elsewhere in Germany. Many plans for future discussions, and more than one collaborative project, emerged from my Monday evenings at the Philosophical faculty and the *Silberlaube.*

If this were not distraction enough, there was also the very bad example of François Hartog, a historian who thinks (our normal professional rule is, of course, "only digest"). Some eager discussions about the development of history and the social sciences — and our joint discovery of the combative and fascinating recent German literature on this field — inspired us to organize a small *Treffen* about the use of examples in early modern history and other fields, which took place in May. The funding was arranged, with miraculous ease, by Dr. Nettelbeck, and the arrangements were conducted, with miraculous deftness, by Andrea Friedrich. A day and a half of lively, polyglot sessions seemed to please, and certainly engaged, all participants. The quality of both lectures and discussions leads us to think that a further and more formal conference, aiming at a publication, would also prove rewarding. We hope to arrange this in due course.

Finally, the tragic origins of the German footnote. An article on the history of documentation in historical writing, which I had written before my arrival, proved fascinating to some of the journalists I met in the Kolleg (another bit of evidence for the thesis that this is Oz). Their response — and the warm interest of an editor — fanned my interest in the subject: and this time the Berlin libraries lent themselves to my quest. The *Nachlaß* of Leopold von Ranke, who played a principal role in my story, was heaped up in disorderly boxes, each one an Mi Babi's cave, at *Haus II* of the Stabi. The printed literature of early modern and 19th century historiography lay in piles on the shelves of the libraries of the FU (notably the *Meinecke-Institut),* the Stabi and elsewhere. Again and again, the staff of the library threw out their nets and hauled in a heavy catch, reeking with historical

interest. Miraculously, overconsumption didn't cause indigestion. By the end of July, I had completed a short book on the footnote from the Renaissance to Ranke, to be published, in the first instance, in a German translation by the new *Berlin Verlag.* This essay tries both to tell a story and to point several morals about the ironies inherent in the development of positivism and objectivity. For good or ill, it is a Wissenschaftskolleg project (though, like others of its kind, it stems ultimately from questions posed in a Princeton seminar). It owes much to corridor and printer-room conversation as well as to the Kolleg's research facilities.

A steady crackle of small-scale projects accompanied the slower-paced large-scale ones through the year. To keep my general education going, I wrote a number of long book reviews. In addition to those already mentioned I gave lectures in Princeton, Tübingen, Evanston, Cambridge (MA), London, Paris, Athens, Göttingen, and at the Institutes for Classical and Romance Philology of the FU and the chair for Ancient History at the Humboldt University in Berlin. Last but not least, as Germans say, my family and I had the chance to travel from this strategically-placed base, to Italy, to Greece, to a number of cities in the former East and to as many corners of the former East Berlin as we could manage in a year. No part of this year's work meant more to my family or to me, than the chance to know and come to love the strange, half-ruinous charm of this haunted city, which will now haunt us — and, we hope, draw us back.

Our warmest gratitude then, to the whole staff of the Wissenschaftskolleg — for luring us here; for arranging for our children to become native informants on local mores as they attended German schools; and for making our stay in this wonderland outside normal academic space and time so enjoyable, so productive and so revealing. We're especially grateful to have been housed in the Heydenstraße (an accident caused by the existence of a place for my daughter in what turned out to be a wonderful class at the Carl-Orff-Schule). This distant posting didn't make us feel excluded from the warmth and hospitality of the other fellows. But it did give us the chance to learn about such features of local culture as the *Imbiß,* on which my son became especially expert, and to make any number of non-academic friends. In addition, it gave me the wonderful opportunity of a half-hour's walk to and from work, much of it on the yellow brick road by the Hubertussee, whose ever-changing beauties and irrepressible ducks cheered me on the sleepiest mornings and chilliest afternoons, down to the last hot days of our second summer in Berlin. A final word of thanks belongs to those who made the implausible seem easy: Barbara Sanders, *sine qua non,* who welcomed us (and more than once saved our sanity) when we arrived last summer at the low point of the Kolleg year; Eva Hund, who gave endlessly generous instruction in the mysteries of the

German language and much else; and Frau Bottomley and her staff of women of valor, whose inexhaustible energy and patience brought the hidden treasure of Berlin's libraries to an ignorant, greedy stranger.

David Gugerli

Rede / Ströme

Ich wurde 1961 in Männedorf in der Schweiz geboren. Von 1980 bis 1986 habe ich an der Universität Zürich Geschichte, Neuere Deutsche Literaturgeschichte und Literaturkritik studiert und war anschließend bis 1989 Assistent an der Forschungsstelle für Sozial- und Wirtschaftsgeschichte der Universität Zürich, wo ich 1987 auch promovierte. Von 1989 bis 1993 hielt ich mich als Forschungsstipendiat des Schweizerischen Nationalfonds an der Maison des Sciences de l'Homme in Paris, am Colegio de México in Mexiko Stadt und an der Stanford University auf und arbeitete an einer kultur- und technikgeschichtlichen Studie zur Elektrifizierung der Schweiz. Zur Zeit bin ich Athena-Stipendiat des Schweizerischen Nationalfonds. — Wichtigste Veröffentlichungen: *Zwischen Pfrund und Predigt. Die protestantische Pfarrfamilie auf der Zürcher Landschaft im ausgehenden 18. Jahrhundert,* Zürich 1988. Hg. mit Sebastian Brändli u. a.: *Die Schweiz im Wandel. Studien zur neueren Gesellschaftsgeschichte,* Basel / Frankfurt 1990. Zusammen mit Rudolf Braun: *Macht des Tanzes — Tanz der Mächtigen. Hoffeste und Herrschaftszeremoniell 1550-1914,* München 1993. Hg.: *Allmächtige Zauberin unserer Zeit. Zur Geschichte der elektrischen Energie in der Schweiz,* Zürich 1994. In Vorbereitung: *REDE/STRÖME. Zur Elektrifizierung der Schweiz 1880 –1914.* – Adresse: Camino Real al Ajusco 161, 16020 Tepepan-Xochimilco, México D.F., bzw. Huebacherweg 16, CH-8335 Hittnau.

Wo die traditionelle Technikgeschichte bislang ihre Ursprungsmythen gepflegt hat und wo die Ökonomen seit Schumpeter den atemberaubenden Dreisprung von der Invention über die Innovation zur Diffusion technischer Artefakte nie richtig zu Ende führen mochten (weil ihnen der sichere Blick von den breiten Schultern des mächtigen Innovators hinab auf die Unübersichtlichkeit technischen Wandels genügte), da setzt heute eine postmoderne Analyse technischer Entwicklung überraschend apodiktisch ein: „There is no off switch to the technological" (Avital Ronell,

The Telephonebook). Diese elektrotechnische Metapher schärft, nolens volens, auch den Blick für einen neuen technikgeschichtlichen Ansatz. Denn: Wenn es tatsächlich keine (einfache) Möglichkeit gibt, das Technische auszuschalten, dann ist vielleicht auch die Vorstellung von einem gewöhnlichen „Ein"-Schalter auf einen — allerdings folgenreichen — Kurzschluß zurückzuführen. Wie aber eine Geschichte der Elektrifizierung schreiben, ohne immer wieder auf große Anfänge zurückkommen zu müssen? Wie sähe eine Geschichte der Technik aus, die sich für einmal dem Problem der Diffusion technischer Systeme und Artefakte widmete, um sie in ihrem sozioökonomischen Zusammenhang zu erklären und zu deuten?

Mit dieser Frage (und einem knappen Zentner Papier) bin ich vor rund dreihundert Tagen ans Wissenschaftskolleg zu Berlin gekommen. Weder habe ich dort, dies sei gleich vorweggenommen, bei der Konfrontation von Frage und Material einen Urknall miterlebt, noch fand, durch die schiere Präsenz von Interdisziplinarität im Berliner Grunewald, eine klar datierbare methodische Initialzündung statt, die mir das Schreiben erleichtert hätte. Im Gegenteil. Was sich im Bereich der schweizerischen Elektrizitätswirtschaft seit dem ausgehenden 19. Jahrhundert im Hin- und Herlaufen zwischen Begriffen, Artefakten, Argumentationsfiguren und Redeweisen verknüpft, gekreuzt, verschoben, überworfen und verstärkt hatte, war in mühsamer Kleinarbeit zu rekonstruieren, Schritt für Schritt. Und gleichzeitig war es den neuen Eindrücken, die das Kolleg mir bescherte, gegenüber zu stellen. Gewiß keine einfache Sache. Von der Bedeutung des elektrotechnischen Diskurses wollte ich im Dienstagskolloquium meine Mitfellows überzeugen, wollte ihnen zeigen, daß die Diskursanalyse es erlauben würde, auf den vertrauten Anfangskitsch der Technikgeschichte zu verzichten. Den einen war dies zu trivial, während andere übelriechende Dämpfe aus den Grüften Foucaultscher Theorien zu wittern vermeinten. Ihre Kritik hat mir schließlich nicht weniger geholfen als die zahlreichen bestätigenden Hinweise und Anregungen Dritter. Manche Mißverständnisse ließen sich glücklich beseitigen, in der Mittagspause, beim Kaffee, oder spät in der Nacht, vor einem das Tageswerk oft genug als Makulatur ausspeienden Drucker. Im Lauf der Zeit, eben während der uns zugestandenen dreihundert Tage, entstand so ein mir neues Arsenal von Einsichten und Vermutungen, Annahmen und Gewißheiten, mit dessen Hilfe sich jenes andere Geflecht von Rede / Strömen besser entwirren und rekombinieren ließ. Hans-Jörg Rheinberger, Yehuda Elkana, Jürgen Kocka, Kurt Wölfel, Joachim Nettelbeck, Anthony Grafton, Wolfgang Kemp und Paul Zanker bin ich dafür zu besonderem Dank verpflichtet. Vielleicht gerade darum, weil auch ihre Redeströme keines-

wegs von einem bedeutenden Anfang gekennzeichnet waren, sondern sich vielmehr auf ungezwungene Art ergaben.

Wichtiger noch als die Absenz eines großen Anfanges (beim Strom in der Schweiz und bei den Gesprächen in Berlin) scheint mir jedoch im Rückblick das Gefühl, daß im Wissenschaftskolleg etwas entstanden ist, das sich durchaus weiterentwickeln läßt. So habe ich aus meinen (zu) zahlreichen Referaten in Berlin, in Salzburg und in Göttingen von einer ebenso geduldigen wie interessierten Zuhörerschaft viel lernen können, und das Buch zur Elektrifizierung der Schweiz, welches mir ganz besonders am Herzen lag, ist in wesentlichen Teilen fertiggeschrieben worden. Darüber hinaus fand ich Zeit, zwei neue Projekte zu entwerfen, das eine zum Thema Telefonie und sozialer Wandel in Mexiko Stadt (1948 —1992), das andere zur politischen Dimension der schweizerischen Kartographie im 18. und 19. Jahrhundert. Schließlich habe ich, mit großer Unterstützung des Fellowsekretariates, einen der „allmächtigen Zauberin unserer Zeit" gewidmeten Tagungsband herausgegeben. Er dokumentiert die in den letzten fünf Jahren unternommenen Anstrengungen auf dem Weg zu einer lebendigeren Technikgeschichte — auch in der Schweiz.

François Hartog

Les Anciens et les Modernes et le passé imprévisible

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Mon projet de travail consacré au couple formé par les Anciens et les Modernes, à différents moments de crise de leur longue histoire, s'est développé dans trois directions. Parti d'une réflexion sur les rapports de la Révolution française avec l'antiquité, avec centrale et disputée la notion d'imitation, il me paraissait nécessaire de passer de la seule politique à l'esthétique (et justement à leurs échanges ou interférences), en profitant de mon séjour au Wissenschaftskolleg pour prendre en compte J. J. Winckelmann et sa réception en France. Lues et immédiatement traduites, les *Gedanken,* comme ensuite la *Geschichte der Kunst* contribuèrent en effet à focaliser l'attention sur la Grèce, terre de la Beauté, de la belle Nature et de la Liberté et à relancer le débat sur l'imitation. En deuxième lieu, il convenait de cerner différentes querelles depuis le Moyen Age ou différents moments de cette Querelle, reprise, continuée, renouvelée, en m'arrêtant sur celle qui lui a donné son nom, la dispute inaugurée par Ch. Perrault. Enfin, le couple anciens/modernes, vu comme un cadre de pensée et un instrument permettant à une société, à divers moments de son histoire, de discriminer dans son passé, conduisait de plus en plus vers une interrogation sur les rapports au temps et leurs transformations. Les avatars du couple Anciens / Modernes sont alors à mettre en rapport avec ce que j'appelle régimes d'historicité.

La Révolution française peut aussi être vue comme la dernière grande querelle des anciens et des modernes: querelle politique focalisée sur la

question de la liberté. Mais où, au fond, chacun se revendiquait comme moderne. Les Jacobins se voulaient résolument modernes et, s'ils faisaient appel à l'exemple des anciennes républiques, c'était justement aux fins de devenir pleinement modernes. Les Thermidoriens se présentaient comme les vrais modernes et, contre la confusion des lieux et des temps opérée par leurs adversaires, ils mettaient en avant la distance séparant des anciens: la liberté des modernes n'est pas celle des anciens, qui, par conséquent, ne doivent plus servir de modèle politique.

Quels étaient en fait la portée et le sens de la référence antique? Elle a d'abord été une arme contre la monarchie, contre les ennemis de la République ensuite. Elle a surtout offert déjà dans la période pré-révolutionnaire un cadre et un langage pour concevoir et dessiner un espace politique encore inédit, pour désigner et s'approprier le politique comme tel. Ensuite, quand tous les repères ont vacillé, elle a, dans l'urgence et l'angoisse de l'action, fourni aux acteurs des mots, des tournures, des *exempla* pour tenter de dire ce qu'ils étaient en train de faire, pour dire le présent, lui donner forme et agir sur lui. C'est en ce point que s'est trouvé réactivé le quiproquo de l'imitation: copie ou inspiration, *imitatio* ou *aemulatio*. Il ne s'agissait pas, pour les Jacobins, d'imiter les anciens pour directement reproduire, mais de faire appel à eux, de les invoquer, de les faire venir dans le présent, tout en proclamant, avec Saint-Just, le refus de l'imitation: «N'imitez rien de ce qui est passé avant vous; l'héroïsme n'a point de modèles». Alors qu'ils furent cependant accusés par les Thermidoriens d'avoir voulu «régénérer» la France, en la transformant en une nouvelle Sparte. De cette «illusion» découlèrent des «maux infinis»: la Terreur. Ainsi, par le recours au concept d'illusion (promis à un bel avenir), ce mauvais usage de l'antiquité fournissait une «origine intellectuelle» à l'échec sanglant de la révolution.

Avec le fameux appel de Winckelmann («La seule façon pour *nous* de devenir grands et, si c'est possible, inimitables, c'est d'imiter les anciens»), lancé en 1755, on semble se situer aux antipodes de la phrase de Saint-Just. Pourtant, dans les deux cas, il s'agit de commencements. Imiter les Grecs veut dire cesser d'imiter les Français, qui, eux-mêmes, ont imité les Romains, qui sont eux-mêmes les élèves des Grecs. En brisant cette chaîne de l'imitation, on ira puiser directement aux «sources» de l'art. L'imitation est alors choix de l'originalité et promesse d'une *Bildung* authentiquement allemande. Pour Saint-Just, l'appel à l'antiquité et le refus de l'imitation ne sont pas contradictoires, dans la mesure où la révolution se veut commencement absolu et voit l'antiquité aussi comme rupture inaugurale: le monde est «vide» depuis les Romains. L'un propose l'imitation pour que *nous* devenions inimitables, l'autre la récuse parce que *nous* sommes (déjà) grands et inimitables. Imiter, ne pas imiter, seraient, en somme,

deux stratégies opposées en apparence et, pourtant, inséparables, pour avoir l'audace de commencer ou pour faire face à l'angoisse des commencements.

Ces remarques sur l'usage de l'antiquité rejoignaient les propositions plus générales de Robert Darnton sur la Révolution française comme «révolution littéraire». Aux intellectuels qui, tel Fabre, avaient commencé par récrire Molière, revint la tâche de créer du sens, en reconstruisant la réalité sociale. Ce fut là le point de départ du séminaire informel, proposé par Darnton, qui, partant de la Révolution française, discuta ensuite tout au long de l'année le rôle des intellectuels dans les anciens pays de l'Est avant et depuis 1989.

La seconde direction, celle de la Querelle et de Perrault, me conduisit vers une réflexion sur le parallèle. Variété de *l'exemplum*, exemple dédoublé, il a été l'instrument par excellence de *l'historia magistra vitae*. L'idée et l'occasion m'en furent données par la proposition — aussitôt acceptée par le Kolleg — que nous avions faite Anthony Grafton et moi d'organiser une rencontre informelle autour de «l'exemple» *(l'exemplum*, l'exemplaire, le paradigme, le cas, l'exemple comme point de rencontre entre la tradition classique et les modernes sciences sociales; voir rapport dans ce même volume, p. 190 s.). Avec Perrault, le parallèle, comme forme, semble triompher. Pourtant, qu'est-ce qu'un parallèle qui conclut systématiquement à la supériorité du second terme, les modernes? Entre eux et nous, il n'y a en fait plus de *comparaison* possible. *Le Parallèle des anciens et des modernes* entérine un renversement dans la perception du temps: nous sommes les vrais anciens et eux sont des enfants. A travers la problématique de la perfection, il est prêt à historiciser les aniens (ils ont atteint la perfection qu'ils pouvaient atteindre en leur temps), mais pas les modernes (la perfection est là ou, pour ainsi dire, là où nous sommes). Toute courtisanerie mise à part, Perrault ne peut penser au-delà du présent, qui se donne comme point de vue d'où jauger le passé. De même que Louis XIV est vu comme le modèle des rois présents et passés, de même, par un retournement de *l'historia magistra*, c'est le présent qui fait désormais la leçon au passé.

La dissolution de *l'historia magistra* (et donc du parallèle comme forme opératoire de l'histoire) intervient en France, au moment de la Révolution (plus tôt en Allemagne avec l'élaboration du concept de *die Geschichte*, au singulier). On entre alors dans ce que je nomme «régime moderne d'historicité», c'est-à-dire dans un type de rapport au temps où c'est le futur qui domine. L'histoire s'écrit désormais du point de vue du futur. Ainsi les révolutionnaires sont pour ainsi dire pris entre deux régimes d'historicité: l'ancien, d'où leur appel à l'antiquité et à Plutarque, le nouveau, d'où le vertige de la page blanche où doit s'écrire le futur et la rupture avec les anciens.

J'ai consacré la dernière partie de mon travail à la mise à l'épreuve de cette notion de régime d'historicité. Ce fut l'objet de mon *Kolloquium* au Wissenschaftskolleg. Partant de l'hypothèse que 1989 marquait la fin du régime moderne d'historicité, je proposais d'examiner différents moments de crise du temps: les années 1560-1580, la fin du XVIII° siècle, autour de 1914, les vingt ou trente dernières années caractérisées par une omniprésence du présent; mais aussi en remontant beaucoup plus loin dans le temps, au début du second millénaire avant Jésus-Christ, les «oracles historiques» mésopotamiens, où s'opèrent d'intéressants échanges entre divination et histoire, ou encore, dans *l'Odyssée*, la douloureuse découverte par Ulysse de l'historicité. D'où la question terminale: comment pour nos sociétés trouver une forme de circulation entre présent, passé et futur, telle qu'ils soient tous les trois coprésents sans que ne vienne à s'instaurer la tyrannie d'aucun d'entre eux? Ne pas écrire l'histoire du point de vue du futur veut dire pour l'historien que le passé redevient, en un sens, «imprévisible» et qu'il lui faut le rouvrir, en y retrouvant les passés qui ont été un jour, un moment, des futurs possibles. Il suffit de se demander comment écrire aujourd'hui une histoire nationale pour voir que l'imprévisibilité du passé n'est pas une question purement académique.

Christopher John Humphries

Systematics, biogeography and conservation

Born in Derby, England, 1947. 1966-69 student at the University of Kingston-upon-Hull; 1969-72 Ph.D. student at the University of Reading; From 1972 — present held various jobs at the Natural History Museum, London; 1972-74 Assistant Curator, European Herbarium; 1974-80 Head Curator of European Herbarium; 1980-90 Principal Scientific Officer, General Herbarium; 1990 — present Senior Research Scientist. Also held two research fellowships at the University of Melbourne (1979 — 80, 1986). Author of more than 90 publications in the fields of general systematics, plant systematics, cladistic biogeography and art curation. Monographs include: *The Hamlyn guide to trees* (1981, in English, Spanish, German, French and Dutch); *Cladistic Biogeography* (1986); *A generic monograph of the Compositae: Anthemideae* (1993); *Cladistics: a practical course in systematics* (1991); *Catalogue of the Natural History drawings by Joseph Banks on the Endeavour voyage (1768-1771) held in the British Museum* (Natural History) (2 vols. 1984, 1987). Edited volumes include *Ontogeny and Systematics* (1988); *Austral Biogeography* (1991); *Systematics and Conservation Evaluation* (1994); *Banks' Florilegium* (in 22 parts 1980-89). — Address: Department of Botany and Biogeography & Conservation Laboratory, The Natural History Museum, Cromwell Road, London SW7 5BD.

My work at the Wissenschaftskolleg was primarily on two topics: conservation principles and conservation applications. I was one of the team of nine researchers of the "Schwerpunkt Biodiversität", and indeed one of the gang of four who will edit our final joint publication, the book entitled: *Priority Areas Analysis; Systematic Methods for Conserving Biodiversity*[*]. The aim of the book is to produce a slim, punchy guide to the principles

* See section III *(Aufsätze)* of this volume, pages 205 ff.

and practise of systematic methods of reserve selection and their applications to the real world problems. Indeed, the original plan was to evaluate the contributions by ecologists and systematists using conservation constituency.

Since UNCED and the signing of the *Convention on Biodiversity* in Rio in 1992, for many nations the present theme and major challenge in conservation biology is to stem the unprecedented loss of biodiversity. A wide number of approaches will be required and presently there are a number of different debates. The goal of conserving as many species as is possible is one such debate that pervades much of literature, and the associated reserve design discussion for *in situ* protection has centred around interpretation of empirical studies on how best to "capture" biodiversity in space and time. A whole set of essential questions has revolved around where to place protected areas in relation to the features or species we wish to retain, and it was in this arena that I worked for much of my time at the Wissenschaftskolleg.

My first assignment was to write a chapter about *complementarity,* a subject we virtually unanimously concluded comprises one of the unifying principles of our various UK, Australian and South African approaches. Analysis of published and unpublished papers led to the conclusion that complementarity had independently been recognised in the early 1980s and had been applied to area selections at both global and regional scales. One generality is that, with limited options and resources for reservation, it is a sensible approach to select the most complementarity areas for any given units of biodiversity whether they be characters, species or higher ranking surrogates, such as land systems or other ecological and geological features. Crucially, quantitative methods have been devised which optimise the number of features for every area to be included in a reserve network by selecting complementary sites. Quantitative methods then tell us what is the absolute minimum required to satisfy the goal of representation, and act as the baseline for reserve design. Thus for any given region, if every site was unique to that area, then it would be necessary to include every site in the reserve network. Conversely if every attribute being measured occurred in just one area, then only one site would be required to fulfil the criterion of representation of at least one each of every attribute. In practice there are various degrees of overlap between one site and another, and the task of determining a practical reserve network often demands detailed analysis of many different combinations of choices. Associated with complementary are the principles of *irreplaceability* (sites essential to a reserve network) *and flexibility* (alternative choices of essential site combinations), which were written up by colleagues Tony Rebelo and Bob Pressey in adjacent chapters of the book. For me it was particu-

larly gratifying to see these and other aspects of the project come together and become unified into an agreed point of view. Also, to turn some of the ideas into working computer software routines showed that being together had a useful synergy, and produced some concepts which now have so much value in our work back in the Natural History Museum.

Naturally, there is a huge gulf between the theoretical ideals of systematic methods and the actual practice of designing a practical scheme that will satisfy the many different criteria of reserve selection. For my second task I started to research and write up one of the later chapters about the application of systematic principles to real problems, on different global, regional and local scales. Using data compiled earlier by two Spanish colleagues, Isabel Castro Parga and Juan Carlos Moreno Saiz, I worked with Paul Williams to analyse one regional problem — the effectiveness of the existing "national" and "natural" park system within the Iberian peninsula. This led me into using measures of efficiency to determine the differences between idealised reserve networks and the existing reserve system to determine which new areas would be essential to include in a new scheme designed to preserve as much biodiversity as possible. The results showed that existing schemes selecting reserves for widely different and often opportunistic criteria were not very efficient and showed considerable redundancy by comparison to the optimised schemes. I was also fortunate to have access to similar data sets for South Africa and various parts of Australia, which will eventually form the core of this chapter.

Being away from home gave me a lot more free time than usual, and so at weekends I tended to use at least a few hours re-writing a new edition of *Cladistic Biogeography* (first edition, 1986, Clarendon Press, Oxford), with Lynne Parenti, an ichthyologist from the Smithsonian Institution. During five months we managed to write about half of the book using e-mail. Since the first edition, the subject of cladistic biogeography has moved in fits and starts with various new developments that need to be integrated into a new work. Cladistic biogeography is a method of historical analysis that combines modern systematics with vicariance interpretations, and has as its basic premise that Life and Earth evolved together through time. Therefore, the promise is that we should be able to search for patterns of relationships among areas of endemism throughout the whole world. Our aim is to present a comprehensive review of theory and methods, summarising the academic history leading to its formulation, and presenting practical examples for the systematist who may wish to incorporate biogeography into taxonomic revisions. We also postulate that the least understood biogeographic problem is the timing of the opening of the Pacific Ocean in the Jurassic / Triassic and the relationships of the terrestrial areas around the Pacific rim. We have

examined many of the different theories in an effort to determine one coherent explanation.

I came to the Wissenschaftskolleg with many high hopes, with specific goals and a great desire to make the conservation project work. Although we did not achieve all that we set out to do, we got on with each other reasonably well as we travelled down a fairly long road. The interactions within the group were endless and invariably stimulating. I learned a lot. I also enjoyed the company of the other fellows immensely, particularly the "collective intelligence" biologists and the other participants who joined in with our seminar series. I am particularly grateful to Elena Lazos, Erhard Denninger, Ashok Desai, Sandra Mitchell, Gustav Ranis, and Wolfgang Streeck for their thoughtful contributions. I thoroughly enjoyed the Thursday night dinners, with everything from intense debate on Bosnian war crimes to the history of comparative religions and even Swiss jokes. The Wissenschaftskolleg is in a beautiful setting, although living and working in the same room becomes less attractive as the days grew longer and warmer ! However, it was a wonderful place in which to think and I basically learned that not everything has to be so frenetic as life in Blighty. Indeed, towards the end of July the weekends became most precious as I raced to meet my self-inflicted deadlines. I am not the first to say it, but life for the solo fellow really was like being in a monastery with college high table. I was pleased to make acquaintance with colleagues in other Berlin institutions and it was a privilege to get to know the Berlin Botanic Gardens almost as well as I know Kew Gardens. My thanks to the staff but particularly Brigitte Zimmer, Werner Greuter, Walter Lack, Beat Leuenberger and Sylvia Arroyo. Travelling to the former East Germany was irresistible. The transport system was superb. Potsdam became a fond favourite for me, as well as for my wife, son and daughter during their brief holiday visits.

I made many friends during my 5-month stay amongst the fellows and staff alike. I wish to thank everyone at the Wissenschaftskolleg for making it a fruitful time.

Wolfgang Kemp

Topophilie

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Dank dem Sofa, ihm zuerst. Ich habe einmal ein Jahr in einem Forschungszentrum verbracht, wo mir nur ein Schreibtisch und ein Schreibtischstuhl zur Verfügung standen. Da habe ich keine Zeile geschrieben. Auf dem Sofa in meinem Arbeitszimmer, mit dem Blick hin und wieder auf die Bäume und die dahinter leise brummende Koenigsallee habe ich schreiben können. Ich habe mein Buch über *Christliche Kunst. Ihre Anfänge, ihre Strukturen* am Wissenschaftskolleg fertiggestellt. Es sollte abgeschlossen sein, als ich kam, war es natürlich nicht und wurde in Berlin umfangreicher, sicherer und hoffentlich besser. Bestimmt hat es in Berlin Qualitäten gewonnen, die unter den normalen Bedingungen unseres zerissenen Arbeitens kaum zu haben sind: Zusammenhang, Abstand, Mut zum Ganzen. Es ist natürlich ein großes Geschenk, ein Jahr lang aus der deutschen Universität (fast ganz) entlassen zu sein. Es ist ein großer Luxus, wenn einem alle Literaturwünsche fast vom Munde, auf jeden Fall aber vom Zettel abgelesen und erfüllt werden. Und es hilft enorm, in Gesprächen bei Tisch mal eben Fragen zu klären, die reichen von „Ab wann gibt es Schlösser?" bis zu „War das allegorische Schriftverständnis nicht ein Jahrtausendirrtum?". Und daß der Kollege von der Archäologie, daß Paul Zanker meine Ausflüge in die spätantike Kunst wohlwollend begleitete, war auch sehr ermutigend.

Mein Buch greift die Frage nach dem Christlichen an der christlichen Kunst wieder auf, die das 19. Jahrhundert für beantwortet hielt und die das 20. Jahrhundert entweder verdrängte oder an Spezialdisziplinen wie die christliche Ikonographie delegierte. Hier dagegen wird der Anspruch erneuert, daß sich allgemeine Aussagen über den Zeitraum treffen lassen, in dem die christliche Kunst ganz „bei sich" war, das ist grob gesprochen die Zeit von 400 bis 1400. Die Hauptaufmerksamkeit gilt dabei der Kunst nach Konstantin, welche in ausführlichen Werkinterpretationen und anhand der Hauptmedien der christlichen Bildverkündigung vorgestellt wird. Gleichwohl ist diese Kunstepoche nur der Ausgangspunkt, um die großen Fragen zu stellen und nach den spezifischen Strukturprinzipien zu forschen.

Ich begreife christliche Kunst von ihrem Kompositcharakter her, von der Tatsache, daß sie sich in vielen Bildern, in mehreren Aussagemodi und — zusammengenommen — in komplexen Bildsummen äußert. Zu ihrem Motto könnte man die ersten Worte des Hebräer-Briefs machen, die davon sprechen, daß Gott sich „vielfach und auf vielerlei Weise" *(polymeros kai polytropos)* offenbart hat. Ich sehe in christlicher Kunst das Bedürfnis am Werk, bestimmte Elemente auf bestimmte Weise zu kombinieren, mithin konstante Relationsmuster auszubilden. Ich halte mich nicht an den „Hintersinn", sondern an den „Beziehungssinn" der christlichen Kunst.

Weiterfragend und den „Beziehungsgrund" dieses Sinns zur Debatte stellend, eröffne ich zwei Perspektiven, die in letzter Instanz zusammenfallen: Ich analysiere das in diesen Synthesen anschaulich und verpflichtend werdende Strukturwissen einer Religion, und ich stelle dar, was einer Kunst aufgegeben ist, wenn eine Weltreligion eine große Erzählung wie die beiden Testamente zur heiligen Schrift erklärt. Mein Buch ist auch eine umfassend angelegte Betrachtung über das Verhältnis Bibel und bildende Kunst. Meine These: Weil die Bibel so ist, wie sie ist, war die Kunst gehalten, den engeren Aufgabenbereich einer Bibelillustration zu überschreiten. Nicht nur die Kunst ist auf die Bibel angewiesen, es gilt auch die Umkehrung: Die Bibel bzw. die christliche Religion, welche sie zu ihrem zentralen Text erhebt, ist auf die Kunst angewiesen. Damit ihre spezifischen Defizite kompensiert werden und damit in einem Medium bewahrt bleibe, was die Theologie des „Hintersinns" schon früh geopfert hatte: die „historische Evidenz" (Auerbach) der beiden Testamente.

Eigentlich wollte ich am Kolleg ein kleines Buch über Erzählen in Bildern schreiben, ein Thema, das mich schon lange umtreibt und das ich in Einzelpublikationen oft gestreift habe. Ich konnte mir am Kolleg die nötige Klarheit verschaffen, wie ich das Projekt angehe, was vor allem heißt, wie ich es einschränke und dennoch konstruktiv genug anlege. Das ist geschafft; die ersten 50 bis 60 Seiten stehen; ich bin guten Mutes, das

kleine Werk im Jahr 1994 abschließen zu können, bevor die nächsten sieben mageren Jahre beginnen. Weil Raum, erzählter Raum in diesem Buch eine große Rolle spielt, war ich so vermessen, mir durch die Bibliothek so ziemlich alles besorgen lassen, was zum Thema Raum in ästhetischer, architekturgeschichtlicher, geographischer, sozialhistorischer und psychologischer Sicht erschienen ist. Vielleicht mache ich daraus eine Anthologie, vielleicht nenne ich sie „Topophilie". Und damit bin ich schon beim nächsten Thema. Vermutlich wäre ich mit meinem Hauptprojekt weitergekommen, wenn da nicht Berlin, Potsdam und die Mark Brandenburg gewesen wären. Das Neue, Unerwartete, Ablenkende kam von den Orten, von den *topoi*. Nur ein wenig konnten Catharina Berents und ich dem Kolleg davon mitteilen, als wir die Stadtgestalt Berlins zum Gegenstand einer Exkursion für die Fellows machten. Anderes wird folgen. Der Raum und die Orte, sie ließen sich idealerweise erschließen, wenn man von einem so idealen und amönen Ort wie dem Kolleg aus aufbrechen und dorthin zurückkehren konnte.

György Kurtâg

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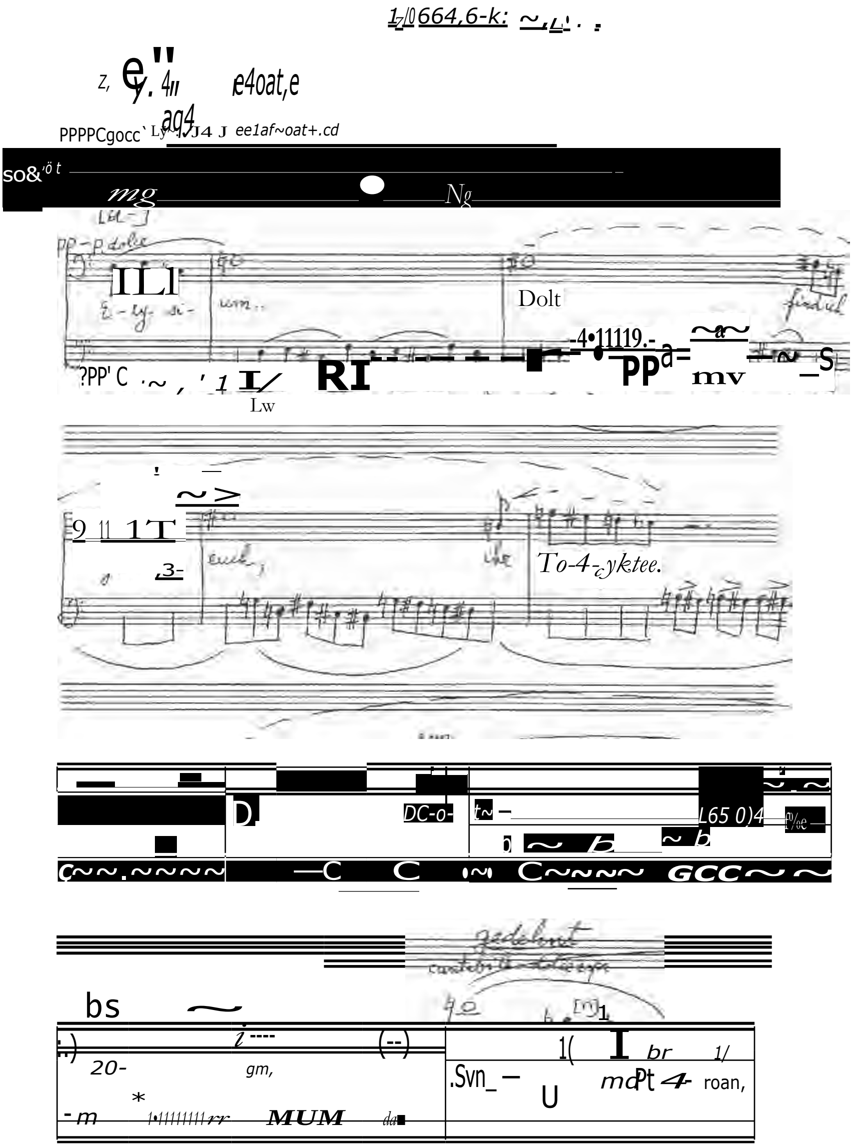
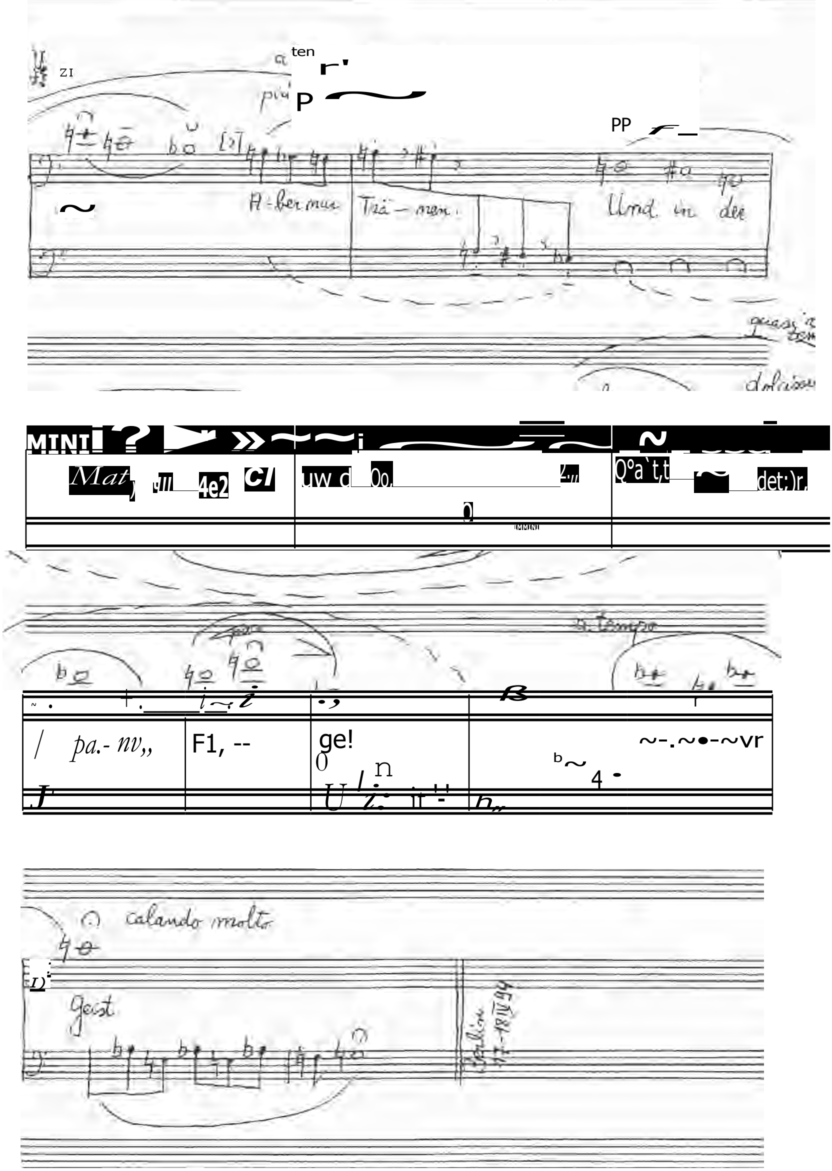
Werke (Auswahl): *Viola Concerto* (1954); *String Quartet Op 1* (1956); *Quintet for Wind Instruments Op 2* (1959); *Eight Piano Pieces Op* 3 (1960); *Eight Duos Op* 4 for Violin and Cimbalom (1961); *Jelek / Signs Op* 5 for Viola; (1961); *The Sayings of Péter Bornemisza Op* 7 (concerto for Soprano and Piano, 1963-68); *Games / Jatekok* (Piano, 1973-76); *Hommage à Andras Mihaly Op 13* (12 microludes for String Quartet, 1977); *Messages of the Late Miss R. V. Trussova Op* 17 (Soprano and Chamber Ensemble, 1976 –81); *Omaggio a Luigi Nono Op 16* (Chorus, 1979/81); *Kafka-Fragments Op 24* (Soprano, Violin, 1985-86); *Three Old Inscriptions Op 25* (Soprano, Piano, 1986); *Requiem po drugu* („Requiem für einen Freund", Sopran und Klavier, 1986-87); *Officium breve* (String Quartet, 1988-89); *What is the Word* (Beckett-Vertonung für Ildiko Monyok, 1990 –91); *Rückblick. Altes und Neues für vier Spieler* (Hommage à Stockhausen) im Rahmen der Berliner Festwochen 1993; *Grabstein für Stephan; Quasi una fantasia ... Op 27 No 1; Op 27 No 2* (Doppelkonzert für Cello und Klavier und 2 Kammerensembles). – Adresse: Liszt Ferenc tér 9, H-1061 Budapest VI.

Während der Zeit am Wissenschaftskolleg arbeitete György Kurtâg an einem 1981 begonnenen Liedzyklus „Lieder der Schwermut und der Trauer" (IIecros yHbIHHM H ne'iaaH), den er im März 1994 abschloß, und an ETHAH („Stele", Op 33) für groBes Orchester, vollendet im Oktober und uraufgeführt durch das Berliner Philharmonische Orchester am 14. Dezember 1994 unter der Leitung von Claudio Abbado.




Und in der
a tempo
calando molto
Geist

Mark Lehmstedt

Zur Regulation öffentlicher Kommunikation

Jahrgang 1961; 1983-87 Studium der germanistischen Sprach- und Literaturwissenschaft an den Universitäten Leipzig und Berlin. 1987-1991 wissenschaftlicher Assistent an der Universität Leipzig, 1990 Promotion. 1992-1993 Stipendiat der Fritz-Thyssen-Stiftung in München, 1993 Gastwissenschaftler am Forschungsschwerpunkt Europäische Aufklärung Berlin. Arbeiten zur Sozialgeschichte der Literatur und zur Geschichte des Buchwesens. — Adresse: Schliemannstraße 12, D-10437 Berlin.

Die Kisten, mit denen ich im Oktober 1993 in die Wallotstraße einzog, enthielten das Rohmaterial für zwei Monographien, die ich in den folgenden zehn Monaten schreiben wollte. Die Unterlagen für die zweite Studie — über Buchmarkt, Zensur und Aufklärung in Bayern am Modell des Münchner Verlegers Johann Baptist Strobl — habe ich in all den Monaten nicht einmal ausgepackt, und auch das erste Buch — über Produktion und Rezeption von Comics in der DDR — gedieh nur bis *zu* Seite 300, womit aber wenigstens der historisch-narrative Teil abgeschlossen war. Dort angekommen, wurde deutlich, daß den hochkomplexen Verhältnissen in der DDR mit dem gängigen Verständnis von Zensur nicht beizukommen war. Schlimmer noch: Auch für die bayerischen Verhältnisse im späten 18. Jahrhundert (die Quellen zur geplanten Strobl-Untersuchung entstammen vorrangig den minutiös überlieferten Münchner Zensurakten) erwies sich dieser allgemein anerkannte Zensurbegriff als durchaus untauglich. So verbrachte ich denn einen Gutteil der zum Schreiben reservierten Zeit mit der Suche nach einem in sich logischen und zugleich dem historischen Material angemessenen begrifflichen Instrumentarium und gelangte allmählich zu ersten Ansätzen einer Theorie der Regulation öffentlicher Kommunikation. Als ich meine Überlegungen im Kolleg vorstellte, entstand eine höchst kontroverse Debatte, die sich nicht zuletzt am Begriff der „produktiven" Zensur (im Gegensatz zur „destruktiven" Zensur) stieß. Für Zu- und Widerspruch, vor allem aber zahlreiche Denkanstöße bin ich insbesondere Jürgen Kocka, Robert Darnton, Yehiel Limor, Klaus Schreiner und Karol Sauerland zu Dank verpflichtet — ohne sie

wäre ich in noch mehr Sackgassen geraten. Eine extrem erscheinende Nagelprobe — bayerische Zensurpolitik im 16. Jahrhundert — schien dann jedoch nicht nur die prinzipielle Brauchbarkeit des Ansatzes zu zeigen, sondern führte zu der Einsicht, daß Zensur nur funktional, im Kontext einer umfassenden Kulturpolitik zu fassen ist, die ihrerseits integraler Bestandteil der Politik aller europäischen Staaten zumindest seit der frühen Neuzeit ist. Nun stand der Weg zur Fortführung der Comic-Studie (und der Untersuchung zu Strobl außerdem) wieder offen, aber da war die Unendlichkeit von zehn Monaten schon vergangen.

Von der nicht immer erbaulichen Beschäftigung mit der Zensur hoffte ich zwischenzeitlich Erholung zu finden in einer kleineren Studie über den Königsberger Buchhändler Johann Jakob Kanter, dessen Buchladen in der zweiten Hälfte des 18. Jahrhunderts zu einem faszinierenden Mittelpunkt der dortigen, äußerst lebendigen literarischen Öffentlichkeit geworden war. Zwar wollte ich es ursprünglich bei diesem einen Vorstoß in nordöstliche Regionen bewenden lassen, doch dann entwickelte sich eine Art von Sogwirkung, die mich immer weiter nach Osten lenkte, bis ich schließlich bei den von der vertrauten mittel- und westeuropäischen Situation völlig abweichenden Buchmarktverhältnissen Rußlands und den fast gänzlich vergessenen deutschen Buchhändlern in St. Petersburg und Moskau im 18. Jahrhundert angelangt war. Nach fünfzehnjähriger Abstinenz versuchte ich, wieder Russisch zu lesen (: zu entziffern), um mich in die verblüffend reichhaltige buchgeschichtliche Forschungsliteratur vor allem der Leningrader Akademie-Institute einarbeiten zu können. Auf diese Weise entstand schließlich ein großer Aufsatz über die Bedeutung des deutschen Buchhandels in Osteuropa für den interkulturellen Transfer im Zeitalter der Aufklärung, ein Thema, das ich gemeinsam mit einigen Slawisten auch künftig weiterverfolgen will.

Natürlich ist das Wissenschaftskolleg alles andere als ein Ort, an dem man konzentriert und ungestört arbeiten kann; das sagt einem nur niemand vorher. Und geschähe es, würde es keiner glauben. Ganz im Gegenteil scheint das Haus systematisch auf Ablenkung zu zielen — in Gestalt der täglichen Mittagsrunde, der Dienstags-Kolloquien, der von Robert Darnton versammelten Mittwochs-Gesellschaft, der Donnerstags-Abende, der Hauskonzerte und Filmvorführungen, der Exkursionen und diversen Kollegfeste (vermutlich nicht nur meiner Tochter haben Robert Darnton und Antje Vollmer die durch nichts mehr zu erschütternde, aber auch durch niemanden mehr zu übertreffende Vorstellung von Weihnachtsmann und Knecht Ruprecht vermittelt). Das alles war noch zu „ertragen", doch die gefahrvollsten Versuchungen gingen, zumindest für mich, von der Bibliothek aus, und ich muß gestehen, ich bin ihnen erlegen. Noch nie habe ich so entsetzlich viel gelesen wie in diesem knappen Jahr, kreuz und

quer durch die deutsche, europäische und amerikanische Buchgeschichtsforschung, immer auf den Spuren der Fußnoten und Literaturhinweise – es genügt ja, sie auf einen kleinen Bestellschein zu übertragen, und schon hatte man nach ein, zwei Tagen das Gewünschte auf dem Tisch. Nur wer die verworrene Berliner Bibliotheks- und Katalogsituation kennt, weiß die Bibliothek des Wissenschaftskollegs wirklich zu schätzen, sieht aber auch mit Schrecken jenem immer näher rückenden Tag entgegen, an dem er wieder ohne sie zurechtkommen muß. Aber das gilt eigentlich fürs ganze Haus.

Ora Limor

Space and Polemics

Born 1942 in Ra'anana, Israel. Studied History at the Hebrew University, Jerusalem. Ph.D. in Medieval History, The Hebrew University, 1985. Senior lecturer of Medieval History at the Open University of Israel. Books: *Between Jews and Christians. Encounter between Cultures in the Middle Ages and the Renaissance* (Tel Aviv, 1993); *Die Disputation zu Ceuta 1179 und zu Mallorca 1286. Zwei antijüdische Schriften aus dem mittelalterlichen Genua* (Munich, 1994); *Jerusalem Pilgrims* (Jerusalem, forthcoming). — Address: The Open University, 16 Klausner Street, Ramath Aviv, Tel Aviv.

I came to Berlin to write a book about Jerusalem. It was my innermost hope that the geographical and cultural distance would provide me with a more suitable angle of vision, an insight perhaps difficult to obtain when one is within touching distance of things themselves. That, of course, was not the main reason I came to Berlin, but it was an appropriate answer to the vexing question which people asked me, and I asked myself, what is it about Berlin which makes it a suitable place for research on the Holy Places of Jerusalem. As expected, the difference in landscape, colour and atmosphere between Berlin — a green city immersed in lakes (at least for those who had the good fortune to spend their time in Grunewald) — and Jerusalem — a city of white stone on the edge of the desert — was quite extraordinary. Beyond that, however, the two cities had a surprising similarity: Both are cities overwhelmed by the weight of an old-new history, both are full of whole or ruined buildings and monuments, each of which tells a story. They are loaded cities. Beyond the sense of strangeness known to the tourist, I immediately felt a strange familiarity with the burden of history and emotion. Like Jerusalem, Berlin is a good city for contemplation, a particularly suitable city to spend an unusual year in, to be a stranger in, or in which to indulge in introspection.

The title of the book I began to write is *Space and Polemics,* and it deals with the evolution of the Holy Places, particularly in Jerusalem, against the background of the perpetual dialogue between Christianity and Judaism, with their rival polemics and interpretations. The Christians inherited from the Jews the Holy Land, the Bible and the idea of election, giving

all these their own interpretation — an interpretation which engaged in a perpetual dialogue with the Jewish presence (physical or spiritual, actual or historical). When the Jews were able to return to Jerusalem and to practice their religion there, they had to respond to this Christian interpretation, and to develop their own conceptions against the background of this interpretation or in reaction to it. The book has six chapters, three of which are general, and three which illustrate the general themes through an examination of specific Holy Places. The first chapter deals with the *Judaica Veritas* (the "Jewish truth"), that is, the Christian attempt to provide their Holy Places and objects with legitimation through the authority of the Jews. Underlying this attempt is the belief that the Jew preserves for the Christian an ancient memory — a memory older than the historical existence of Christianity and which justifies its claims. The task of the Christian is to retrieve this memory from the Jew. The second chapter looks at the map of the Holy Sites and the processes of remembering and forgetting which influenced its development. The third chapter deals with the liturgy — the organization of time around sites through the use of texts common to the two religions, transforming the city in general and the various sites in particular into a stage of a didactic theatre, and recreating each year at fixed periods the founding narrative of the faith. The fourth chapter compares the Jewish Temple and the Church of the Holy Sepulchre, as two sites competing for the illustrious title of "navel of the earth" with its various significances. The fifth chapter deals with Mount Zion and the foundation-myth associated with it: the founding of the Christian Church ("The Mother of the Churches"), and the founding of the Messianic genealogy. The last chapter deals with the Mount of Olives, and the organisation of its eschatological geography in both religions (and in Islam as well). Most of the material for the six chapters has now been collected, but so far the writing of only three chapters has been completed.

The time which was meant to be set aside mainly for writing the book had to be allocated to other projects, old and new, which took its place. Two books I wrote in recent years had now reached various stages of completion and suddenly appeared on my table. The first, *Die Disputation zu Ceuta 1179 and zu Mallorca 1286. Zwei antijüdische Schriften aus dem mittelalterlichen Genua,* which will be published in the coming months in the *Monumenta Germaniae Historica,* was sent to me for the preparation of the indexes — one of quotations, another of names, places and subjects, and a third — a *Wortregister.* This was an exhausting technical task which took up a great deal of time and excluded any other activities. The other book was *Jerusalem Pilgrims,* about to appear in Jerusalem, which came back from the editors, and I had to prepare its final ver-

sion. And, in addition, I had to produce a few articles which I had committed myself to writing:

The article "Christian Sacred Space and the Jew", which constitutes the first chapter of the book mentioned above, examines various traditions - stories and legends — all concerned with a single theme: "The Knowing Jew", the Christian image of the Jew who willingly or unwillingly identifies and preserves the holy things of Christianity, thus granting it legitimacy.

"Egeria and Paula Read the Holy Places" is about the way in which two women-pilgrims at the end of the fourth century related to the Holy Sites, the way in which they understood the relationship between the Scriptures and the places mentioned in them, and the evidence they provide concerning the beginnings of Christian pilgrimage. This article deals with the reading and the writing of two fascinating women, and I thought about it for some time. The thought took flesh in Berlin, partly because of the preoccupation of some of us with matters of gender. This preoccupation became intense, not only because of the inspiring presence of Catharine MacKinnon amongst us, but especially because we were only five women out of a group of forty fellows, a minority whose small number proved to be a problem from the beginning, even if there was no real reason for it to be a problem.

"Religionsgespräche. Jud. Chr." is an article on the public religious disputations between Jews and Christians in the Middle Ages which I wrote for the *Theologische Realenzyclopaedie.*

And, in addition to these, there were a few book-reviews, especially of books dealing with inter-religious polemics.

Berlin was a good place to reflect on polemics and dialogues, and a good place to carry on polemics and dialogues. Although there was no *Schwerpunkt* dealing with mediaeval matters, it was easy to find an attentive ear and to obtain assistance from other historians, and especially from Klaus Schreiner, whose deep interest in tolerance in the middle ages and in inter-religious relations made him a faithful collaborator and partner in dialogue. And, in general, our class, as Tony Grafton put it, was perhaps not the most successful one and perhaps not the brightest one, but it was undoubtedly the nicest one. The wonderful harmony which reigned amongst us right from the beginning became an established fact by the second half of the year and turned our leave-taking into an unforgettable sentimental scene. As a result, if the seminars were somewhat tepid, the parties were wonderfully warm. It seemed that even the staff of the Kolleg was washed by the tides of mutual affection, and the congenial personalities of Barbara Sanders, Hans-Georg Lindenberg and their colleagues will not quickly be forgotten. The German lessons became an occasion for learning German history and culture, and a few of us made great efforts to

prattle a little in the local tongue, even if we stammered. If only the most outstanding amongst us had outstanding achievement to their credit, we all derived some benefit and enjoyment, especially because of Eva Hund, who for some of us embodied all the attractiveness of our Berlin hosts.

German history and culture, for those who looked for them and wanted to penetrate their secrets, were elsewhere, and each of us discovered his own Berlin. One cannot mention everything, and it seems almost superfluous to say that the *Philharmonie*, the *Komische Oper*, the concerts of Alan Marks, the museums, and of course the many remains of the glorious past of the German Jewish community, which a few of us sought out enthusiastically — all these became a central and influential part of our lives and work.

The book *Space and Polemics* will hopefully be concluded in Jerusalem. Berlin will remain another space and another time, which will have its influence on this book, and probably on many other things I shall do in the future.

Marina Litavrina

Russian Emigrant Theatre in Europe

Born on November 20, 1955 in Russia. Associated Professor at the Russian Academy of Theatre Art in Moscow. Graduated from the same Institution in 1980, Ph.D. (1985), History and Theory of Theatre. Lectured on: History of Russian Theatre XVIII-XX centuries. Special courses: Chekhov and the World Theatre, Russian Emigrant Theatre 1918-1940. Main publications in the last 5 years: "Moscow Art Theatre: 1930 and other years", in literary magazine *MOSKVA*, 1988, n. 10 (Moscow Art Theatre in the epoch of Stalin). "Capital Punishment, or Drama because of an Island", in *Teatralnaya zhizn* (Theatre Life magazine) 1989, nn. 16, 17 (Theatre and Soviet censorship). "Love under Party Control" (gender problems and Russian stage of the 1920s), in: *Teatralnaya zhizn* 1989, n. 23. "American Gardens of Alla Nazimova" (Russian star in Hollywood), in: *Slavyanovedenye* (Revue des Etudes Slaves) 1993, n. 4. "The Mythologeme of Theatre in Russian History" (dramatic chronicle "The Path of Nemesis" by N. N. Evreinov), in: *UNESCO Bulletin*, International Association for Slavonic Cultural Studies, 1994, n. 27 (Theatricality in Life and Art). — Address: Profsoyouznaya ul. 8-2, App. 460, 117292 Moscow, Russia.

I arrived in Berlin on the 4th of October 1993, the day the White House in Moscow was stormed and nobody was sure how events would end. I was not sure whether I would reach the promised land of Wissenschaftskolleg. Besides, I was invited here as a researcher working on a project "Russian emigrant theatre in Europe 1919-1939". These two facts perhaps, put together, created a specific mood — not so-called Russian melancholy, but something more modern, not familiar yet, and that's why the acclimatization period took a longer time.

First of all, I understood that my subject is rather new even for colleagues and librarians. Catalogues of theatre and art libraries were silent and at first it looked like there had never been the phenomena of Russian artistic Berlin, as described by Nabokov, Bely and Ehrenburg. There were no traces of the five Russian theatres on Ku'damm and Professor Micha-

ela Böhmig's book *Das Russische Theater in Berlin* was a fairy tale. What was proven and where to go for it?

Then I referred to the help of "Great Silent" — Russian emigrant actors of the 1920s played in the films of Murnau, Wien and other German directors. So I started with the archives of the *Stiftung Deutsche Kinemathek,* where I watched nine films starring Russian actors and worked with the film reviews. But even this main film archive in Berlin lacked some German films of the 20s and 30s, and I found them in the *Bundesarchiv* (Filmarchive). I should mention that the staffs of both institutions were highly professional and helpful. Some of the films were theatre productions put on film with the participation of former Moscow Art Theatre actors. This fact is not widely known in Russia.

At the same time, I started working in the *Walter-Unruh-Archiv* of the *Institut fiir Theaterwissenschaft* (Free University), where I found materials on Russian cabaret theatres in Berlin, reminiscences of directors, rare photographs. Some other materials of the emigrant branch of Moscow Art Theatre (the so-called Prague group) were found in the archive of the *Akademie der Künste.*

At the same time I was preparing my colloquium on Russian-Soviet theatre of the 1920s, which was held on the 25th of January. I also showed the documentary film I brought with me to some colleagues working on Russian theatre (Frau Bauermeister, Herr Maß, Frau Dalugge).

Meanwhile I continued my work on a book about the Russian-American film star Alla Nazimova, managed to find colleagues working in this theme in America and got many articles, references, and reviews through interlibrary loan. At the same time, getting some other materials was more difficult than I expected.

I have to give at this point a short summary of my work in Paris libraries (April 15 through May 30, 1994). I had already asked about the possibility of making a trip to Paris for my research work on Russian emigrant theatre before coming to Berlin (because I already had a proven that many materials can be found only there) and was very glad to receive support for my aspirations from the administration of the Wissenschaftskolleg. I was happy to get the invitation of the *Maison des Sciences de l'Homme,* which provided me with accommodation at *Maison Suger* for fifteen days. The staff of Maison Suger was helpful and provided residents with all necessary facilities. I also profitably consulted with Mme Sonya Colpart, Maison des Sciences de l'Homme, who works with Russian researchers.

The main place for my work was the *Bibliothèque d'Arsenal, Département des Arts du Spectacle* of the National Library. The famous Collection Rondell there contains archives of Russian émigré metteurs en scene, such as N. N. Evreinov (I studied mostly his unpublished works written in

exile), a big collection of references, and articles from the French, German, English, Italian, Spanish, and Czech press on Russian emigrant companies of 1920-1940.

Another library — that of the *Institut des Langues Orientales* — contains materials in the Russian language and emigrant periodicals, such as *Volya Rossii* and *Perezvony,* which cannot be found in Russian archives due to the Soviet system of censorship that existed before 1989. Of great help for my project was a visit to the head of the faculty of Russian Language, Literature and Culture, M. Gérard Abensour, whose works I already knew very well (they are mainly devoted to Russian 20th century theatre and N. N. Evreinov). Also I had a meeting very fruitful for communication and the exchange in research work with Professor Béatrice Picon-Vallin (CNRS), who is a well-known theatre historian working on Meyerhold, Russian Jewish Theatre, and the modern Russian stage.

Another library of the IRENISE (former *Institut des Études Slaves)* possesses complete collections of Russian magazines issued in exile for many years, such as *Annales contemporaines (Sovremennye Zapisky)* and the final numbers of 1935-1936 of the magazine *Le Théâtre et la Vie (Teatr i zhizn),* which was the main periodical of the Russian theatre milieu in Berlin and Paris (Berlin's issues, which can be found in the *Bibliothek der Kilnste,* cover only 1922 — 25 and may not be photocopied). But I could find the complete collection of this periodical only in the *Bibliothèque Russe Tourgenev.* As the library founded by the famous Russian writer and considered to be one of the best holdings of Russian emigré periodicals (some of them are unique), it was worth spending hours simply rewriting the materials (photocopying is not allowed). I also made a trip to *Saint Geneviève du Bois,* the Russian Cemetery, where many actors, directors and writers from the first wave of emigration are buried, and I took pictures of the tombs of these outstanding people. I did as well some topography research in order to create a map of Russian theatre in Paris and to identify the buildings (theatres, cafés, assembly halls) where performances by Russian emigrant actors were given and took pictures of these places. Back in Berlin, I wrote an article about Russian theatre in Paris which has now been adopted for publication by the Department of the History of Russian Culture, Russian Academy of Sciences, in a special issue devoted to the exile Russian culture between the two World Wars. My research on Russian theatre in Paris also drew the interest of the *Institut fiir Theaterwissenschaft* in Bern, and I was invited to give a talk at the congress of theatre historians in Switzerland, November 1994.

In May, after my visit to Paris, I prepared an article for a conference "Barocco and Avant-garde" at the Institute of Slavonic Studies, Russian Academy of Sciences. Though the conference was held in my absence, I

was also asked to prepare material for a collective monography on the subject (to be issued in 1995).

At the beginning of June, I attended the conference "Russian emigrants in Germany" in the city of Constance. There I was able to meet many colleagues whose works I knew before, among them Professor Michaela Böhmig, the main specialist in the field of "Russian Theatre in Berlin", with whom I had very fruitful contacts. In June I also managed to visit several private archives and to get acquainted with the people who knew Russian Berlin very well. They are rare, nowadays there are few remaining eye-witnesses of the events of the 1920s. First of all, Vera Lourier, a Russian emigrant poetess, who personally knew the main figures of Russian literary and artistic emigration. I also became acquainted with Dr. Waltraud Werner, who curates the archive of Russian emigrant painter Massutin, a close friend of the famous actor Michael Chekhov. These letters of Michael Chekhov to and from Berlin are hardly known, only partly published and of course were of great interest. The archive also contains photographs of Russian emigrant actors, which are also of great importance for my research.

At last, I managed to have an interview with the German film star Vera Tschechowa, a granddaughter of the Russian emigrant actress and film star of German cinema of the 20s and 30s, Olga Tschechowa.

At the end of June, I had the opportunity to work at the *Archiv der Theaterpolizei, Brandenburgische Landesarchive* in Potsdam. Needless to say, access to such an archive was impossible before 1989, so few people knew about the files on Russian emigrant theatre to be found there. The Potsdam archive proved to be an important source of exact information (dates of birth of theatre people, addresses in Berlin, the location of Russian emigrant colonies and theatres, repertoire, etc.) for anyone trying to write a chronicle of theatre exile in Berlin. The correspondence between the Russian actors and directors and police authorities could also become a subject for separate study. In contrast to some other places, in the Potsdam archive it was possible to order photocopies of materials, and I hope to have them as evidence for my meditations on the experience of Russian artistic bohème in Berlin in the 1920s.

Throughout the whole year I was preparing an anthology of plays written by Russian dramatists in exile which are still unpublished in Russia. I hope it will be of great use to my students, whom I promised to prepare such a publication. This could be done only due to the great help of the library of the Wissenschaftskolleg and the opportunity to copy the materials for free.

The *Staatsbibliothek* was an important place of work for me as for everybody, mainly with the Russian language emigrant press of the 1920s

and the German press on the theatre of the same period. The *Osteuropa-Lesesaal* also has directories, catalogues of the Russian emigrant press throughout the world, which our Moscow libraries lack. Of great help was the opportunity to read current magazines of Slavonic studies issued in France, the USA and other countries. I also worked in the department of rare books and manuscripts.

Of course, one cannot manage the preparation of a book or any publications on theatre or cinema without visual materials. With the help of the photography archive of the *Stiftung Deutsche Kinemathek,* I have managed to prepare a "gallery" of actors' portraits (laser copies). Having identified the exact places of former Russian theatres in Berlin, I took photos of the buildings that survived through the war and still exist. In Paris I also managed to buy some original programmes of the performances of Russian emigrant theatres, and here in Berlin, with the help of Frau Dr. Helga Haas from the *Institut für Theaterwissenschaft* (Free University), I copied the ads and programmes of Russian theatres in Berlin.

Of course, I tried at the same time to follow the theatre life in Berlin and also got to know with the researchers who are preparing the exhibition "Moscow-Berlin", which is to be held in 1995. I suppose that I have managed to see the best performances of the traditional *Theatertreffen 94* in Berlin and also visited some seminars on modern theatre in the Free University and the seminars on Russian exile cinema in "Arsenal".

I should mention that I received constant help from many members of the staff, first of all Reinhart Meyer-Kalkus, Katharina Biegger, Christine von Klitzing, Theresa Köbele, Elissa Linke and many others. I cordially thank Professor Yehuda Elkana for his interest in my studies.

Throughout the academic year, I attended the seminar of Professor Robert Darnton on the history of ideas and the intellectual sphere. In this academic year I was the only Russian fellow and I had a specific subject of research. Studies on emigrant culture are a relatively new thing for Russian academic studies and are of acute interest in my country. As I could feel at the international conference on Russian emigrants in Germany held in Constance, they find a solid audience here in Germany too — a special programme has been launched. But many fellows who deal with the "genre humain" and general cultural studies helped me in my work here: Robert Darnton, François Hartog, Wolfgang Kemp, Thomas Gelzer, and others.

I also want to thank the Rector personally for his moral and material support of my studies and my journey to France. Long live Wissenschaftskolleg, with its family-like structure: the "parents" — the staff each year has to take care of and to look after 44 "children", new-born fellows.

Meanwhile, the names of "Altfellows" look down from the bookshelves like ancestors in family portraits.

Catharine MacKinnon

... and the Invisibility of Women

I was born on Oktober 7, 1946. I hold a J. D. from Yale Law School (1977) and a Ph.D. in Political Science from Yale Graduate School (1987). My major interest is the law and politics of sex equality. I have taught law for 12 years and am currently a tenured full Professor at the University of Michigan Law School, where my teaching focuses on equality and freedom of expression. I designed legal claim for sexual harassment as a form of sex discrimination and co-authored ordinances recognizing pornography as a violation in its essentials by the Supreme Courts of Canada. Major Publications: *Sexual Harassment of Working Women: A Case of Sex Discrimination* (Yale University Press, 1979). *Feminism unmodified: Discourses on Life and Law* (Harvard University Press, 1987). *Pornography and Civil Rights: A New Day for Women's Equality* (with Andrea Dworkin, Organising Against Pornography, 1988). *Toward a Feminist Theory of the State* (Harvard University Press, 1989). "Reflections on Sex Equality Under Law," *Yale Law Journal* N° 1281, 1991). *Only Words* (Harvard University Press, 1993. — Address: Law School, University of Michigan, 625 State St., Ann Arbor, MI 48109-1215, USA.

My year at the Wissenschaftskolleg was productive and gratifying. Coming from the American Academy, it was a singular relief to find a place on earth where scholarship is taken seriously as a life pursuit. The environment was conducive to accomplishing a great deal.

My major project was my legal casebook, *Sex Equality*, centering on American case law with extensive commentary, notes, and comparative law. I wrote the first three chapters (225 pages) and outlined the entire 17 chapters in detail. Working with my German colleague, Susanne Baer, comparative materials were selected, translated, and integrated. Being in Berlin facilitated this collaboration immeasurably. Foundation Press has recently expressed its interest in publishing this book.

The following scholarly projects were also completed:

1.. Introduction to the British Edition of *Only Words* written and prepared for publication;
2. Third Lecture of *Only Words* revised to incorporate European legal materials, and the entire manuscript prepared for the British edition;
3. Article, "Equality and Speech", based on third lecture of *Only Words,* finished and prepared for publication in *Collected Courses of the Academy of European Law;*
4. Article, "Prostitution and Civil Rights", written and prepared for publication in *Michigan's Journal of Gender and Law;*
5. Extended Book Review, "Pornography Left and Right", of Richard Posner's *Sex and Reason* and Edward deGrazia's *Women Lean Back Everywhere,* written;
6. Essay, "Speech, Equality, and Harm", written and prepared for publication in a collection by Laura Lederer and Richard Delgado (eds), *The Price We Pay* (Farrar, Strauss);
7. Book proposal for *Death's Whore: The Pornography of Murder,* written;
8. Collection of previously unpublished or uncollected writings and speeches, *Men's Laws, Women's Lives,* designed and submitted; Harvard Press has expressed its interest in this book.

In addition to freeing time, the "headroom" provided by the Wissenschaftskolleg made possible the conception of longer-range scholarly projects, such as the new book proposal and the new collection design. I was also freed to edit and revise Jeffrey Masson's *When Elephants Weep,* on the question of animal emotions, and to assist with his translation of Feuerbach's Kaspar Hauser manuscript on soul murder, both of which are also relevant to my scholarly interest.

I restricted my lecturing but, in addition to a seminar on *Only Words* (March) and my Colloquium (July) at the Kolleg, made the following major presentations:

1. "Rape as Nationbuilding", World Structures Convocation, Washington, DC (November 1993);
2. Training for the Expert Commission on crimes against women in ex-Yugoslavia (Zagreb, Croatia, March 1994);
3. "From Auschwitz to Bosnia", Yale Law School conference sponsored by Orville Schell Human Rights Center (March 1994);
4. "Pornography, Civil Rights, and Speech", Oxford University (June 1994)
5. and 6. "Toward a New Theory of Equality" (two different versions), Humboldt-Universität Berlin and Institut für Sozialforschung, Frankfurt, (July 1994).

Being in Berlin provided welcome respite from the American press. I did, however, give a number of interviews and went to London for extensive publicity for the UK edition of *Only Words* in early June. In winter, Susanne Baer translated it with my participation; *Nur Worte* came out from Fischer Verlag in July, 1994.

Considerable research was conducted for *Sex Equality* and the other publications and lectures listed above, particularly on European law. The Kolleg's computer assistance was particularly crucial in this work. I also engaged in empirical and archival investigations for *Death's Whore* throughout Europe.

The Berlin location supported my international human rights work as well, as particularly facilitating contact with Muslim and Croatian women survivors of Serbian genocide. I assisted clients and the Expert Commission on war crimes in the former Yugoslavia in investigating breaches of international law. I also conducted and supervised extensive investigation for our civil suit against Radovan Karadzic.

In addition, valuable connections were made with German women lawyers, judges, scholars, politicians, and journalists. (I particularly appreciated the efforts of the Rektor to introduce me to German legal counterparts.) Friendships and collaborations with German colleagues could grow. My fluency in German improved to the point of understanding lectures and answering questions, as well as reading in my field, passably conversing, and assisting with translations. Germans speak wonderful English, but access to the language opened a whole world — social, cultural, professional, and personal. If I had had time to take classes, it would have been even better. The importance of learning German cannot be overstated. The structured social expectations also required learning some social skills I had previously been able to avoid. I look forward to a lifetime of intellectual and personal contact with colleagues and friends from this year.

My major criticism concerns not only the massively disproportionate absence of women fellows (a defect partially remedied next year), but also the intellectual environment of gender illiteracy. Almost 25 years of scholarship on woman and of study from the viewpoint of women in virtually all fields was not only little represented, it was treated as disruptive. Typical reactions ranged from pride in ignorance to arrogant dismissal, giving the sense that whatever the topics of gender or women might raise for a subject — as angle of vision or as object of study — is either beneath knowing or already known. Perhaps two male fellows brought depth of knowledge of feminist scholarship in their own fields.

About halfway through the year, it occurred to me that most of the lectures could accurately have had "and the invisibility of women" as a subti-

tle. Examples included: a philosophical discussion of Enlightenment values without noticing their exclusion of women or the extensive literature on this subject, together with a development of the thesis of "local realism" without recognition that feminism has been practicing precisely that for 25 years; a presentation on equity in South Asian economic development without discussing its impact or lack of impact on sex equity (this lecturer did, however, distinguish himself by knowing a lot about the subject and being able to answer questions); a presentation on human rights and the state without mention of the work in the women's human seminar on "cultural upheavals" and social transformation, in which the women's movement was not, apparently, mentioned even once; a presentation on the notion of "the barbarian" in which a good many of the archaeological artifacts studied depicted females, but neither women nor gender was noticed; a lecture on concepts of time in history without considering whether the historical constructs apply to women's experience of time; and a consideration of a change from one philosophical approach to another in German history, largely traced through two different treatments of "the uterus", without considering the implications of its location in women's bodies.

Then there was the passing reference to the East German state as "weiblich", without discussion of what women had to do with that regime or what it had to do with women. And the biodiversity project could have used some sense of the historical uses of difference in deciding who survives. This is not at all to single out these presentations but to characterize a reigning atmosphere. Only one lecturer, upon questioning, expressed humility at his ignorance and acknowledged the crucial role of relations between the sexes for his subject. Other than my own colloquium, only one I attended (I did miss a few), on German constitutional law, systematically took issues of gender into account, as if ignoring 53 % of the population would be intellectually inadequate and gender literacy were normal.

Whether raised by a man or woman, identifying gender issues in public always produced snickers, coughing, body shifting, eye averting and embarrassment, as if the real discussion were being returned to a discussion in which gender was unmarked and the voices were almost unrelievedly male. This institutional mood persisted in spite of the support of the Rektor and others in authority for women's concerns and in spite of the many individuals who, approached one at a time, proved receptive to considering the impact of these issues on their work. Shifting the demography of the fellow population will improve this environment only if scholarship on women is better represented and respected.

As a further feature of this setting, it is inexplicable that so many stunningly able and gifted women populate the staff, yet all the top positions are occupied by men. As to arrangements and accommodations, child care

must be provided. Apart from improving the lives of staff, this is essential if women are to be fellows, as well as to permit something beyond a "Hausfrau" existence for partners who accompany fellows and children. Also, our year was overwhelmingly and outrageously white and apparently exclusively heterosexual, both gaps which need to be permanently filled to produce the stimulating intellectual environment of the high quality to which the Kolleg aspires.

Although the Wissenschaftskolleg is hierarchical, I have every confidence that it will prove open to these changes, and will continue to grow intellectually and socially, becoming an even more receptive, supportive, and engaged place than the one I was privileged to benefit from this year.

Christopher Margules

Effects of Human Action on the Persistence or Extinction of Species

Born on October 25, 1951 in Canberra, Australia. I attended secondary school in Davis, California and Canberra. I obtained a B.app.Sc. from the Canberra College of Advanced Education in 1974 and a Graduate Diploma in Recreation Planning (part time) from the same institution in 1978. I worked as technical assistant, then as experimental officer in CSIRO until 1978. From October 1978 to October 1981, I was a D.Phil. candidate at the University of York (UK). The degree was awarded in 1982. Since then I have conducted ecological research in the CSIRO Division of Water and Land Resources, moving in 1986 to the Division of Wildlife and Ecology, where I am now a Principal Research Scientist. I am author or co-author of 22 papers in refereed journals, 18 refereed book chapters or conference papers, and editor of three volumes. I was married in 1974 and divorced in 1987. There are three children, Nick, Elisabeth and Tom. I married Janice in 1990 and she died in her sleep, too young, on September 25, 1994. — Address: CSIRO, Division of Wildlife and Ecology, P.O.Box 184, Lyneham, ACT 2602 — Australia.

My five-month stay at the Wissenschaftskolleg was spent primarily on my commitment to the biodiversity group *(Schwerpunkt "Methoden zur Erfassung der Artenvielfalt bei der Auswahl von Reservaten")* project. This was a book manuscript entitled *Priority Areas Analysis: Systematic Methods for Conserving Biodiversity.* The mechanism used most commonly for the protection of biodiversity at the present time is to alienate areas of land or water from utilisation or exploitation by providing legal protection as reserves or similar classes of land tenure, usually called protected areas. We chose to use the generic term, biodiversity priority areas, recognising that it may not be possible to protect all areas which intrinsically deserve to be protected, and that of those that do become protected, not all will be protected to the same degree. In the past, the identification of such areas has largely been *ad hoc* and opportunistic, leading to a biased

representation of biodiversity within them. Our aim was to devise more systematic methods for locating priority areas so that a less biased representation can be achieved in the future. This is an immensely practical problem, but one which required scientific progress before it could be solved. Governments and natural resource management agencies throughout the world would like an answer to it, especially now that the biodiversity convention requires the many governments which have signed it to actively pursue biodiversity protection. However, before proceeding to develop those methods, it was necessary to pin down the specific role priority areas should play in the overall goal of protecting biodiversity. The biological world is fundamentally different from the physical world. The material physicists deal with displays regularity within homogeneous infinite classes, the members of which cannot be distinguished from one another (e. g., electrons, hydrogen atoms). In contrast, biological systems are organised hierarchically from the molecular, to the ecosystem level. They are characterised by complexity and variability at every level above the molecular and processes operating at one level cannot necessarily be assumed to operate at any other. Logical classes such as genotypes, species communities and ecosystems are heterogeneous. Each member of each class is an individual, which can be distinguished from every other individual. The variety of biological configurations at all hierarchical levels is unknown but clearly extremely large and probably unmeasurable. Yet protecting this complexity is the goal of biodiversity protection. It means protecting hierarchical systems that consist of finite classes of heterogeneous entities that are self-organising (maintain themselves at the expense of the environment) and display emergent properties (different processes at different levels). Priority areas alone cannot achieve this goal. Sympathetic management outside of priority areas will also be needed. The role of priority areas is that, collectively, they should encompass examples of all, or as much as possible, of the biodiversity of the region, country or biome they are situated in. Thus, priority areas make a necessary contribution, but not a sufficient one, to the overall protection of biodiversity.

With this role in mind it was possible for the group to devise explicit efficient procedures for identifying networks of priority areas that more comprehensively represent biological diversity[*]. Recognising that legitimate alternative land uses severely constrain practical options, the methods provide the greatest possible flexibility in the location of priority areas to facilitate negotiation and expedite the planning process. However, the

[*] See the relevant seminar reports in this volume, pages 193 ff., 198 ff.

question of the adequacy of representation — how much is enough — is still open. Perhaps another project at the Wissenschaftskolleg might help solve this problem as well and provide an even more powerful tool for policy-makers and planners?

There was some time for other work-related activities. I helped run a five-day workshop in Pori, Finland, along with other Australian colleagues, on the design and sampling strategies of biological surveys, the analysis of survey data and the use of those data in the identification of biodiversity priority areas. This workshop was attended mainly by officers of natural resource management agencies from Norway, Sweden, Finland, Estonia, Latvia, and Russia. The aim was to show how methods developed in Australia for collecting and analysing data on the distribution patterns of species and communities could be applied to the Taiga, the northern boreal forests. The Taiga in Scandinavia has been logged systematically for many years, but large areas of the Russian Taiga are only now being opened up for timber production. The opportunity may exist for biodiversity priority areas to be identified before exploitation gets into full swing and therefore to be taken into account at the start of the planning and development process.

I also visited the University of Halle and the *Umweltforschungszentrum* in Leipzig to give seminars on the identification of biodiversity priority areas and my other main research interest, the effects of habitat fragmentation on the persistence or extinction of species. Some collaborative research projects were discussed and are planned to get underway once my commitment to the Wissenschaftskolleg project is complete. Semi-natural habitat in the Leipzig-Halle region is extremely fragmented. The present patterns of distribution and abundance of species in this fragmented landscape will help me interpret the results of a field experiment underway in Australia, where I have deliberately fragmented forest habitat with bulldozers to track the persistence or extinction of species. The results of this experiment could, in turn, inform the management of landscapes that have been fragmented for centuries, such as that of the Leipzig-Halle region, by identifying the kinds of species that have disappeared, and the ecological requirements for their reinstatement. Back at the Wissenschaftskolleg, I completed "Deutsch eins für Ausländer" thanks to the excellent, patient tuition of Eva Hund.

There is one more thing. Two of my children, Nick and Elisabeth, had the best and most educational experience of their lives and my wife, Janice, the most peaceful time she could remember.

The working environment of the Kolleg stands in stark contrast to the one I, along with most other scientists, find myself in these days, in which time and effort is mostly taken up with grant applications, external con-

sultancies to earn funds for staff salaries and the juggling of ever scarcer resources to support demands for higher levels of productivity. I very much appreciated the opportunity provided by my fellowship at the Wissenschaftskolleg to think carefully about, discuss with colleagues and write down a solution to one of the many practical problems of protecting biodiversity, which I feel sure will influence planners and policy-makers in many parts of the world, should they read it.

Sandra Mitchell

Metaphysics in Historical, Social, and Scientific Context: The Case of Superorganism Theory

B. A. in Philosophy (1973), Pitzer College, Claremont, California; M.Sc. in Logic, Philosophy and Scientific Method (1975), London School of Economics and Political Science; Ph.D. in History and Philosophy of Science (1987), University of Pittsburgh. Instructor and Assistant Professor of Philosophy, 1985-89, The Ohio State University. Assistant Professor of Philosophy and Science Studies 1989-1992, University of California, San Diego. Fellow of *Zentrum far Interdisziplinäre Forschung,* Universität Bielefeld, 1991-92, Associate Professor of Philosophy and Science Studies 1993-present, University of California, San Diego. Major areas of interest include the structure of explanation in biology and cultural anthropology, metaphysical issues in evolutionary and developmental biology, the nature of the unity or disunity of science. — Address: Department of Philosophy, University of California, San Diego, 9500 Gilman Drive, La Jolla, CA 92093 —0302, USA.

My research during the year I was a resident at the Wissenschaftskolleg addressed general philosophical questions concerning the structure and presuppositions of scientific theories and explanations. Especially in the face of apparently competing alternatives, understanding what is at stake for the various claims put forward can help to resolve or dissolve the conflict. Scientific theories make at least two sorts of ontological commitments in explaining observations — what basic entities exist and what forces act upon those entities. I have focused my philosophical investigations of these matters on explanations of the complex behaviors of social insects. Recently, there has been a "revival" of superorganism theory, which purports to defend a new entity as real, and thus to challenge what is taken to be an ontologically meager reductionist perspective in biology. Invoking a "superorganism" suggests that the features of prototypical individual organisms (like human beings) can be usefully applied in identi-

fying other entities, including associations of individual organisms into societies. One aspect of my study involved the comparison of the multiple contexts in which superorganism theories were promoted in the United States in the early 20th century with that of the current revival. While at the Kolleg I completed a manuscript, *Superorganism: Then and Now*, which suggests that the assumptions of modern defenses of the superorganism differ substantially from earlier ones, although the role these views play as challenges to neo-Darwinism is similar. This paper will be published in 1995 in the *Yearbook in the Sociology of Science.*

My work then turned to more general metaphysics and more contemporary biology. Lurking behind particular efforts to explain a specific phenomenon are more fundamental commitments to the unity or disunity of science itself. What are the relationships between the various disciplines that make up the current practices of science and how are they involved in explanation? Do they compete, are they compatible, or even in the end reducible? Even within the sub-disciplines of biology these questions can be raised. I had previously developed a model of self-organization with Robert E. Page, to explain some aspects of the division of labor in honey bee colonies. We presumed the presence of genetic diversity among individual bees, and argued that some division of labor would emerge from interaction among the individuals, and without invoking adaptation at the colony level. I took the problem of explaining this complex feature of insect colonies as a location for questioning the relationship between allegedly competing accounts. Here I benefited from discussions with members of the "Schwerpunkt" *Social Intelligence.* In developing a manuscript on *Complexity and Pluralism,* Nigel Franks and Jean Louis Deneubourg were especially helpful for refining my arguments detailing when and where conflicts occur. I compared the structure and application of three different self-organization explanations of division of labor — the genetic account proposed by Page and myself, an architectural one by Franks, and a learning algorithm by Deneubourg. I tried out versions of this manuscript in lectures at the Kolleg, the *Fors chungsschwerpunkt Wissenschaftsgeschichte und -theorie,* the *Einstein Forum,* Hamburg University, and the Universities of Oslo and Trondheim in Norway. The response from those audiences along with discussions with fellows of the Kolleg have had an important influence on my thinking. The interaction I had with the social insect biologists was no accident, as the Kolleg recognized our shared interests in designing the constituency of our year. However, in addition there were unanticipated discussions and collaborations which have moved my research in ways I could not have predicted beforehand. This, I think, is a unique and the most valuable opportunity provided by the Wissenschaftskolleg. I was exceptionally lucky in that two new and

quite different projects emerged from discussions with members of the Biodiversity"-Schwerpunkt" and with Wolfgang Streeck.

After their Tuesday colloquium and many individual discussions, I found myself more and more intrigued by the issues addressed by the focal group working on "Biodiversity Reserve Selection Methods". Debates between Paul Williams and Dan Faith on alternative phylogenetic measures for diversity highlighted deep questions about what we know about the natural world, and the policy implications of this >pure< science suggested by Dick Vane-Wright and Chris Humphries fueled my already burning interests in interdisciplinary interactions. I was graciously invited to participate in a series of seminars with their group, other fellows of the Kolleg, and outside visitors. Gustav Ranis continued to raise the question of the value of biodiversity itself, and argued that it must be addressed prior to any defense of a particular strategy for conservation. I believe it was, in part, in response to his persistence that at the end of the year the Biodiversity group asked me to contribute to the first chapter of their book. My task is to explicate how the various economic, aesthetic, and moral values people have and can assign to biological diversity influence the ways in which diversity is measured. I have been working on this since returning to California and will present my results in a workshop I have organized at the London School of Economics in March, 1995. Dick Vane-Wright will present the work of their group at this occasion, and a social scientist from the L. S. E. will also make a presentation. While I think of my work as both preliminary and provisional, I am planning to continue to do research in this area. I have the Kolleg and the Biodiversity group to thank for opening up a new and vital arena for this philosopher of biology.

The second new direction was again spawned by the Tuesday colloquium. Wolfgang Streeck presented a critical account of the recent history of the European Community, detailing the predicament of maintaining authority at the super-national level in the face of the autonomy of the constituent nations. He outlined new ways to think about these political structures. In my colloquium I discussed the notions of the unity and disunity of science that are plausible given the fact of the pluralism exhibited by scientific disciplines and sub-disciplines. Fellows in the audience pointed to the similarities in the types of questions we were addressing, even though the contexts are so different. While in Berlin, Streeck and I met on several occasions to explore the similarities further and are now collaborating on a project entitled *Global Order and Pluralism in Science and Politics.* This project is a >pilot< comparative study whose goal is to contrast theories of global governance and models of scientific structure in order to better understand each of these domains. Developments in each

field are being considered. On the side of political theory, in building an international order, hopes for universalistic world government have been widely abandoned, and new forms of order are being sought that recognize the apparently irrevocable diversity of national systems or cultures. In the European Community, for example, the quest for "harmonization" of rules and regulations, pertaining to products, health and safety regulations, etc., has given way to a search for new, "softer" regimes that leave national sovereignty intact while at the same time allowing integration to proceed. The tools that have been developed for this purpose are new, untested, and poorly understood instruments of international law, including mutual recognition of national standards, standardization of interfaces of systems rather than systems themselves, limitation of regulation to common minima, regulation of outcomes rather than methods to achieve them, harmonization of measurement procedures rather than substantive goals, etc. What these have in common is that they try to respect the different logics of national systems, thus endorsing "pluralism". This entails avoiding subsumption under universal, general principles, while at the same time trying to assure mutual compatibility and respect. On the side of natural science, the last decade has witnessed an increase in the defense of the "disunity" of science, from both philosophical and sociological perspectives. The fact of multiple research strategies and languages both across divergent disciplines, e.g., psychology and neurobiology, and within a given discipline between, for example, theoreticians and experimentalists or developmental and evolutionary biologists, suggests the need for something more complex than a naive "unity of science" doctrine. We expect that close scrutiny of pluralism in the sciences may shed light on the logic and the possibilities and limitations of a non-statist, non-universalist, pluralist form of international governance in a world of sovereign nation-states. In addition, the theoretical and practical advances already developed in the study of political structures may allow a more thorough understanding of the type of pluralism existing in the scientific domain.

These two new projects owe their inspiration and initial formulation to the Wissenschaftskolleg, to the institution of the Tuesday colloquium, to the lunches that forced us out of our offices and out of our individual pursuits to discussions across tables and disciplines. The intellectual inspirations born there will undoubtedly grow and develop in the coming years.

But while the professional side of life at the Kolleg was more than rewarding enough in itself, it was combined with the opportunity for real friendships to emerge. As one-half of the *Herr and Frau Sprecher* couple, I worked with Robert Darnton on supporting the `lighter' side of life, including the dances, the movies, and the "Abschiedsfest". The community of fellows, their families, and Kolleg staff is what I shall miss most.

Berlin's music and theater and life will draw me back, I am sure, but the city will not be the same organism without the heart of the people who made up the 1993-1994 year of the Wissenschaftskolleg.

Reinhard Mocek

Übergänge. Aufregende Wissenschaftsgeschichte oder wie ein nüchterner Arzt zum Romantiker wurde

1936 geboren in Chemnitz. 1954-59 Studium der Philosophie und Biologie an der Karl-Marx-Universität Leipzig, da auch 1965 Promotion zum Dr. phil. Nach der Habilitation an der Philosophischen Fakultät der Martin-Luther-Universität Halle-Wittenberg 1969 bis 1991 Professor für Dialektischen Materialismus. 1976 Einrichtung eines Fachbereichs „Wissenschaftsgeschichte und -theorie"; Leitung des universitären Arbeitskreises für Wissenschaftsgeschichte. 1988 Gründer und Leiter des Interdisziplinären Zentrums für Wissenschaftstheorie und -geschichte (IZW) an der Martin-Luther-Universität Halle-Wittenberg. 1990 Gastprofessor an der Universität Konstanz. — Adresse: VoßstraBe I, D-10117 Berlin.

Mit der Idee, meine Monate im Wissenschaftskolleg auf die Anfertigung eines Buchmanuskriptes zu konzentrieren, kam ich zum Oktoberanfang in den Grunewald — und wurde bald schon vom Ratschlag von Wolf Lepenies, um alles in der Welt nicht schon mit einem ausgereiften Vorhaben anzureisen, verunsichert. Etwas gänzlich Neues beginnen? Nun muß ich den gestandenen Kollegprofis nicht erst beweisen, daß man jenseits des fünfzigsten Lebensjahres nichts wirklich Neues mehr bewirken kann; die Gipfel des wissenschaftlichen Lebenswerkes sind bereits erklommen Doch manch einer wird von der Anstrengung, sich an den scharfen Kanten des Berges dann auch festhalten zu müssen, so gebunden, daß er den Blick in die Ferne, das genußvolle Entdecken dieser Welt aus der einmal gewonnenen Perspektive verabsäumt. Wissenschaftliches Leben ist an unseren Universitäten wenn schon nicht gerade ein Daseinskampf, so aber doch ein zeitbindendes Geschäft. Nachdenken ist leider keine moderne Tugend in der Wissenschaft mehr; und dies ist vor allem bedingt durch äußere Umstände. So habe ich den wohlgemeinten Ratschlag leicht modifiziert.

Ich blieb beim Thema, aber ich ging es noch einmal durch. Nachdenklich eben.

Von Wolfram Kaiser, dem souveränen Kenner der Medizingeschichte – der halleschen zumal —, wurde ich vor etwa fünfzehn Jahren zur Mitarbeit an einem kleinen biographischen Bändchen über den halleschen Stadtphysikus Johann Christian Reil eingeladen. Für den Bürger der Stadt Halle ist dieser Mann auch heutzutage kein Unbekannter; Reilstraße und Reilsberg sind geographisch allgegenwärtig; daß es sich hier um einen Arzt handelt, weiß man eben. Gänzlich anders sieht das im allgemeinen Wissenschaftsbewußtsein unserer Tage aus — da spricht kaum noch jemand von Reil; und ich bin jetzt so boshaft und zitiere die Frage von einem der vielen berühmten amerikanischen Weltenbummler, die im Wissenschaftskolleg zu rasten pflegen, während des Kolloquiums zu Reil, das ich pflichtgemäß und gern durchführte. Ja, er fragte mich schlicht und einfach, ob dieser Reil überhaupt eine bekannte Größe in der Wissenschaft sei, sich mithin soviel forscherischer Aufwand um seine Person lohnen würde. Das war die einzige durchgreifende Verunsicherung im Kolleg, an der ich zu knabbern hatte; ich überwand sie, als ich mir selbst die Frage stellte, wie viele Namen berühmter amerikanischer Ärzte mir geläufig sind. Da stand es eins zu eins.

Nun, „mein" Reil wurde im Jahre 1759 in dem ostfriesischen Dorf Rhaude geboren, kam über Göttingen zum Medizinstudium nach Halle und fand — nach einem Intermezzo als Landarzt in seiner friesischen Heimat — bald den Weg zurück an die Friedrichs-Universität, wo er zu einem herausragenden Arzt und Gelehrten wurde, dessen Ruf selbst Goethe in die Saalestadt führte. Als Physiologe und Anatom leistete er zu seiner Zeit Hervorragendes auf dem Gebiet der Hirnforschung und begründete mit dem *Archiv für die Physiologie* im Jahre 1796 die erste einschlägige Fachzeitschrift in der Welt. Humboldt hat ihn hochgeschätzt und in die Vorbereitungsarbeiten zur Gründung der Berliner Universität einbezogen. 1810 ging Reil nach Berlin und wurde neben Hufeland zu einer der großen Gründergestalten der Medizinischen Fakultät der Friedrich-Wilhelms-Universität. Tief berührt hat ihn die Demütigung Preußens durch Napoleon; und der Kampf der vereinigten Armeen gegen das französische Heer markiert zugleich den letzten Lebensabschnitt Reils. Als Leiter der ostelbischen Lazarette starb Reil am 22. November 1813 an einer Typhusinfektion.

Eine der vielen heroischen Biographien — was aber ist das Problem? Das kleine Bändchen zu Reil, das Kaiser und ich damals fabrizierten, läßt es noch nicht erkennen. Reil gilt als ein Umbruchsdenker. Aufgewachsen im Geiste aufklärerischer Philosophie, hat er sich nach der Wende zum neunzehnten Jahrhundert allmählich — so heißt es in der biographischen Litera-

tur zu Reil — zur Philosophie der Romantik bekannt. Das ist zunächst nicht weiter aufregend, gilt es doch für etliche Mediziner dieser Zeit, die der sterilen und dogmatisch gewordenen Aufklärung den Rücken kehrten und glühende Anhänger der insbesondere von Friedrich Wilhelm Joseph Schelling ausgearbeiteten romantischen Naturphilosophie wurden. Andreas Röschlaub wäre da zu nennen, Ignaz Troxler, Karl August Eschenmayer, und viele andere. Doch bei der Lektüre der Schriften Reils – die ich entgegen meiner sonstigen Gewohnheit streng chronologisch aufbaute — fiel mir bald auf, daß er doch eigentlich stets ein und denselben naturphilosophischen Standpunkt vorzutragen pflegte, gewiß mit einigen Ergänzungen und Erweiterungen, aber immer wieder auf dieselben Prämissen zurückkommend. Für ihn war das Leben die Eigenschaft einer spezifisch organisierten Materie, wobei die Lebensprozesse selbst durch einen bestimmten Chemismus geprägt waren, für den er die Formel der steten Veränderung von Form und Mischung der Materie ersonnen hatte. Den Vitalismus lehnte er ab; doch, als Beispiel eines der vielen sich scheinbar selbst fortschreibenden Fehlurteile in der Medizingeschichte, gilt Reil bei nicht wenigen Medizinhistorikern als Vitalist, in kurioser Fehlinterpretation einer seiner bekanntesten Schriften, betitelt „Von der Lebenskraft", aus dem Jahre 1796. Kurios, weil man eigentlich annehmen möchte, daß die Interpreten das Buch, das sie klassifizieren, auch wirklich gelesen haben. Und da würden sie sofort bemerkt haben, daß dieses Werk von Reil gegen die Annahme einer Lebenskraft vitalistischen Typs gerichtet ist. Nun, das sei hier lediglich angemerkt.

Doch zurück zu meiner Leseüberraschung in Reils Schriften. Denn ich fand, daß Reil bis zum Jahre 1807 mit wenigen Modifikationen nur seinen chemistischen Lebensbegriff vertreten hat — um dann plötzlich, wie aus heiterem Himmel, in Tonfall und Interpretation zu einer romantisch-philosophischen Erörterung zu finden, die in etlichen grundlegenden Positionen das gerade Gegenteil von dem aussagte, was er noch bis an die Schwelle dieses Jahres geschrieben hatte ! Das war mir unerklärlich; zugleich aber war ich auch irritiert von der Sekundärliteratur, die sämtlich dieses Phänomen übersehen hatte und einhellig von einem allmählichen, sukzessiven Übergang Reils zur romantischen Naturphilosophie sprach, wobei man sich nur darum stritt, wann der entscheidende Schritt erfolgt sei. Nun begann ich zu grübeln, nach dem Anlaß zu fahnden, der einen solchen plötzlichen philosophischen und naturtheoretischen Standortwechsel motiviert haben könnte. Ich vermutete zunächst, daß es die besondere Natur des Forschungsgegenstandes war, an der Reil dieses Bekenntnis zur Romantik exemplifiziert hat — es geschah nämlich in einer Studie zu den Entwicklungsstadien der Gebärmutter während der Zeit der Schwangerschaft. Es lag nahe, daß ein solches Organ zu einer Reflexion über die

metaphysischen Hintergründe des biotischen Seins angeregt hat. Der Uterus ist ein lange Jahre brachliegendes Organ, ohne Übung und funktionale Erprobung (immerhin lebte man in der Zeit, da sich erstmals Vermutungen über den Zusammenhang zwischen Organgebrauch und Organleistung regten), das plötzlich für eine überaus aufwendige, intensive, anstrengende Leistung aufzukommen hat. Und diese Leistung ist nicht mehr und nicht weniger als die Reproduktion eines neuen Ganzen ! Ein Teil eines Ganzen reproduziert ein neues Ganzes — ein typisch romantisch-naturphilosophisches Motiv ! Doch bei genauerem Hinsehen zeigt sich, daß die einhundertseitige Studie Reils zur Gebärmutter einen eigenartig heterogenen Eindruck macht. Denn Reil legt zunächst in sieben Paragraphen eine durch und durch mechanistische Betrachtung der Veränderungen des Uterus während der Gravitations- und danach der Austreibungsphase vor. Aber zwei längere naturphilosophische Fußnoten schon zu den ersten Paragraphen und zwei angehängte Paragraphen mit naturphilosophischer Diktion erhärten den Verdacht, daß Reil eine bereits fertige Schrift im Nachhinein romantisch-naturphilosophisch „erweitert" hat. Nun wurde es spannend; bloße Spielerei konnte das nicht sein. Hier steckte ein prinzipielles Problem dahinter. Ich kam diesem Problem auf die Spur beim Lesen der Abschiedsrede Reils 1810, als er nach Berlin wechselte. Denn dort resümiert er einen gewaltigen Umbruch in der Wissenschaft der bis zu diesem Zeitpunkt abgelaufenen Ära, die durch eine gänzlich neue Sicht auf die Natur geprägt sei: „Selbst das Tote ist zum Leben auferstanden, die Mechanik der Himmelskörper vergeistet und die Wissenschaft bis in die Tiefen der Erde eingedrungen, das Naturhandeln auf Gesetze zurückführend, die einerlei sind mit den Gesetzen denkender Geister." Nun gut, auch das würde noch auf eine kognitive Grundlage für den Umbruch im naturtheoretischen Denken weisen, doch jetzt kommt die entscheidende Wendung: „Niemand anders als den deutschen Gelehrten gebührt diese Wiedergeburt der Wissenschaften ...".![1] Reil stellt diese Aussage in den politischen Kontext seiner Zeit. 1810 war eines der sieben Jahre napoleonischer Herrschaft über Preußen, eine Zeit, die mit der Niederlage des preußischen Heeres bei Jena und Auerstedt im Oktober 1806 ihren Anfang nahm. Halle wurde kurz darauf besetzt, die Universität über ein Jahr lang geschlossen. Studenten und Gelehrte wurden vom antinapoleonischen, propreußischen Geiste ergriffen. Reil, der bis dahin voller Hochachtung der französischen Wissenschaft gegenübergestanden und den zweiten Band seiner Fieberlehre noch 1802 Napoleon als Dank für

[1] Reil, J. C., Abschiedsrede bei Niederlegung seiner Professur in Halle, gehalten am B. September 1810. In: *J. C. Reil's kleine Schriften wissenschaftlichen und gemeinnützigen Inhalts* (hg. von Chr. F. Nasse, Halle 1817, S. 319).

dessen Verdienste um die Wissenschaft gewidmet hatte, wurde von diesem Bekenntnis zu seinem preußischen Vaterlande zutiefst ergriffen. Ganz im Sinne des Losungswortes von Friedrich Wilhelm III. anläßlich der Gründungsvorbereitungen für die Berliner Universität, wonach der preußische Staat „durch geistige Kräfte ersetzen (muß), was er an physischen verloren hat"[2], widmete sich Reil der Zuarbeit zu dieser nationalen Idee aus der Wissenschaft heraus. Die romantische Naturkonzeption wird von Reil als der deutsche Geist in der Wissenschaft betrachtet. Und so, wie dieser deutsche Geist die Wissenschaft vorangebracht habe, mit derselben Sicherheit und Zuversicht werde dieser Geist, wenn er das Volk beseelt, dieses von seinen französischen Ketten befreien.

Nationale Aspekte der Wissenschaft — das ist ein besonderes Thema, das ich hier nicht extra anschneiden will. Für meine Problemstellung aber ist dieses Bekenntnis Reils hochinteressant, weil es darauf verweist, warum der im aufgeklärten Geiste großgewordene und in seiner Wissenschaft einen biologischen Chemismus vertretende Reil sich zur Naturanschauung der Romantik bekennt — nicht aus kognitiven wissenschaftlichen, sondern aus politischen Gründen ! So erklärt sich auch der hastige, unausgegorene Stil der bereits knapp vorgestellten Gebärmutterschrift, die somit den Charakter einer Übergangsschrift erhält. Wahrscheinlich ist dieses Werk schon 1806 verfaßt worden, denn im gleichen Jahr wurde in Halle eine gynäkologische Klinikabteilung eröffnet. Reil hat in seinem *Archiv für die Physiologie* fast einen ganzen Jahrgang den Problemen von Schwangerschaft und Geburt gewidmet, auch andere Autoren kommen zu Wort. Die schon fertig vorliegende Abhandlung hat Reil dann — eben diesem Bekenntnis zur nationalen Wissenschaft folgend — durch romantisch-naturphilosophische Passagen erweitert. Das Fragmentarische und Uneinheitliche dieser Schrift war Reil selbst offenkundig; er nutzte die darauffolgenden Jahre, um seine gesamte heilkundliche Theorie und Praxis unter dem Leitgedanken der romantischen Naturanschauung — insbesondere Schelling folgend — neu aufzuarbeiten. Der quantitativ gewaltige Ertrag konnte von ihm selbst nicht mehr publiziert werden; erst 1815/1816 erschien die dreibändige *Einführung in die allgemeine Pathologie,* 1816 dann auch die *Einführung in die allgemeine Therapie,* aus Reils Nachlaß herausgegeben von P. D. Krukenberg und Chr. F. Nasse. Beide Werke aber blieben relativ unbeachtet. Reil selbst hatte keine eigene naturphilosophische Schule gebildet; die anderen Ärzte der Ära der Romantik aber folgten eigenen Systemen, nicht selten uneins miteinander. Und allmählich verblaßte auch schon der Stern der unter dem Einfluß der Romantik

[2] Wagner, A., *Die Entwicklung der Universität Berlin 1810-1896.* Berlin 1896, S. 3.

theoretisierenden Mediziner, bis sich dann in den zwanziger Jahren des 19. Jahrhunderts die Kritik am spekulativen Charakter der romantischen Medizin rasch zu formieren begann - eine Vorbereitung der wenig später einsetzenden, vom Geiste des Positivismus geprägten Totalkritik an der romantischen Naturanschauung, die kein gutes Haar mehr an ihr ließ und dazu führte, daß selbst die großen Gestalten dieser Periode der deutschen Wissenschaft der Vergessenheit anheimfielen. Vor allem Reil war von diesem Schicksal betroffen.

Reils Übergang von der Spätaufklärung zur romantischen Naturanschauung erweist sich als ein extern ausgelöster, politisch motivierter Wechsel. Gewiß gab es vorher schon Signale, die zumindest die Bekanntschaft Reils mit dem neuen Paradigma bezeugen. Wer das ganze Für und Wider auch in der Sekundärliteratur zu dieser Frage nachlesen möchte, der schaue in das Büchlein, das als Frucht meiner Fellowship bald erscheinen wird.[3]

Nun bleibt aber noch die Pflicht zur Verallgemeinerung. Oder sollte man aus der Wissenschaftsgeschichte nicht zu lernen versuchen? Was die Motive des Übergangs betrifft, so steht Reil innerhalb der Ärzte der Romantik wohl allein da. Wer romantisch lebte und webte, der tat das schon vor 1806, zumal ja Schelling selbst zu dieser Zeit seine philosophischen Interessen anderen Themen zu öffnen begann. Aber das Beispiel „Reil" ist für die Vielfalt und für die Art und Weise, wie sich in der Naturwissenschaft Paradigmenwechsel vollzogen haben, recht aufschlußreich. Nicht immer ist ein neues Paradigma bereits mit dem ausgeschöpft, was es im engen Sinne naturtheoretisch zu sagen hat. Das trifft natürlich vor allem für diejenigen Paradigmen zu, die über das Naturwissenschaftliche im engeren Sinne hinausweisen. Solche Paradigmen aber sind heutzutage durchaus nicht selten. Alle systemtheoretischen und selbstorganisatorischen Ansätze zur Reformulierung traditioneller wissenschaftlicher Probleme sind von einer solchen Prägung. Wie bald aber könnte es zu einem verbindlichen Paradigma für alle Wissenschaft werden, nur solche Forschungen zu leisten, die einem gesamtmenschheitlich gemeinten Wohl der Menschheit dienen?

[3] Mocek, R.: Johann Christian Reil (1759—1813). Das Problem des Übergangs von der Spätaufklärung zur Romantik in Biologie und Medizin in Deutschland. Frankfurt a. M. 1995.

Gianfranco Poggi

Social Power in its Diversity

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For a long time, most of my teaching and writing has oscillated between two broad themes: the interpretation of the writings of the Sociology Greats — I have written on Tocqueville, Marx, Durkheim, Max Weber, and Simmel — and the analysis of the nature and development of modern political institutions — I have written two books on the state. The last book I published, shortly before coming to Berlin, dealt with *Georg Simmel's Philosophy of Money;* accordingly, my research and writing at the Wissenschaftskolleg has oscillated toward the other thematic pole, and once more has the state as its protagonist. But the scope of the book I have been drafting while spending ten most enjoyable months at the Wallotstraße 21 (I was one of the fellows who did not just have a study in the *Neubau,* but resided there) is rather wider, for it considers the state itself chiefly as the major institutional embodiment of *one* form of social power, onto which it juxtaposes *other* forms.

The concept of "power", with or without the adjective "social", has long been the object of much debate on the part of social scientists, philosophers, and other scholars; and this debate has been particularly lively and productive over the last three decades or so. But much of it has been focused on epistemological issues in which I personally have little interest, or on relatively generic conceptual aspects of the power phenomenon. Many years ago, my mentor Juan Linz (a Fellow at the Kolleg in 1990/91) suggested to me instead, as a particularly worthwhile theme for sociological and politological investigation, the variety of institutional forms that social power assumes, and in particular the interplay, rivalry, and mutual accommodation between political, economic, ideological,

and military power. Beginning in the late sixties, I occasionally explored this very broad theme in my courses on political sociology or political theory; and the stay at the Kolleg offered me a unique opportunity to analyze it more closely and to draft what I hope will become my next book.

I had deluded myself that I could complete an advanced draft of a book on the topic by the simple device of writing out *in extenso* the copious notes I had accumulated for the purpose of teaching. Things did not quite turn out that way, among other reasons because the actual process of writing confirmed time and again somebody's dictum to the effect that there are no things, no matter how complicated, that a bit of reflection will not reveal to be even more complicated. Another naive assumption I had made was that the writing of each chapter draft should be preceeded by a fair amount of new reading on its theme. After about a month of intensive and inspiring, but exceedingly inconclusive reading, I was reminded of a witticism, according to which the *carabinieri* (the members of the Italian military police) always operate in pairs because one of them can read, and the other can write. I realized, that is, that if over the remaining nine months of my fellowship I was to draft most of a book, *writing* was what I chiefly had to attend to, and reading had to be considered a dangerous distraction from that main task. This is why, impressed as I was by the excellent service run in the Weiße Villa by Frau Bottomley and her collaborators, I found myself (I believe) among the "low users" of the Kolleg's Library. Practically all of my work time at the Wallotstraße 21 went into the labor of drafting chapters of my book.

Before outlining the results of that labor as they now stand, let me make it clear that (as will be immediately apparent to the informed reader) the book I worked on does not aim at breaking new ground, but rather at systematizing a body of well-known (though sometimes controversial) sociological material. As far as the present draft goes, the book opens, predictably enough, with a general presentation of the phenomenon of social power. The second chapter introduces the book's central theme, and suggests that social power typically assumes a variety of major forms, each based on a given group's privileged control over a distinctive social resource. The three chief forms have long been identified as political, economic, and ideological power; they result from the possession, respectively (in the wording of a contemporary Italian political theorist) of means of destruction, means of production, and means of persuasion - sword, plough, and book, as Ernest Gellner might phrase it. I subscribe to this conceptual tripartition, which closely relates political power to organized coercion; but, as we shall see below, I consider it empirically justified to give semi-independent status to coercive power itself.

In the third chapter, I discuss political power, drawing widely on Ger-

man writings on the subject, and particularly on a recent book by the Freiburg sociologist Heinrich Popitz. From this point on, the other power forms are considered chiefly as partners or opponents of political power itself, which, as I said, I regard mainly as embodied in the state. The scheme of analysis I employ throughout consists in identifying, for each non-political power form, the claims it typically lays upon the state, as well as (vice-versa) the state's claims upon it.

Among the non-political forms of social power, I consider first what might be called ideological or normative power; its generic aspects are discussed in the fourth chapter, while the following two chapters discuss relations between, on the one hand, institutionalized religion and modern "creative" intellectuals, and on the other hand, the state. (Because of my interest in relations between state and intellectuals, I profited particularly from an informal seminar run at the Kolleg by Robert Darnton, as well as from the opportunity to interact with Tony Grafton, Andras Bozôki, Hans Magnus Enzensberger, and François Hartog).

In chapters seven and eight, my analysis of economic power simplifies matters somewhat by considering exclusively industrial property and the associated forms of business, and examining first the claims business forces typically lay upon the state, then the chief ways in which the latter draws resources from the economic system. I emphasize the explosive novelty of the contemporary variant of this relationship: while economic power is increasingly "global", political power remains territorially bounded. Hence the phenomenon (as a recent Italian book title phrases it) of "wealth without nations and nations without wealth".

As indicated above, although conceptually all forms of institutionalized coercive power belong within the political sphere, in certain circumstances and to a certain extent a state's military elites may conduct themselves toward a country's political leadership as if they constituted a semi-independent power center. While my ninth chapter discusses this well-known phenomenon, the final one will juxtapose with it a much less well-known one: the police, which also constitutes an aggregation of coercive power, may sometimes deploy it (or fail to deploy it) in ways that challenge the constitutional supremacy of political leaders.

In Berlin, I drafted all the chapters described except the last one. But those drafts, besides being just that, i. e. *relatively* early versions of chapters (I emphasize "relatively" because each of the drafts I now have is the product of quite a deal of drafting and redrafting), are also incomplete. As my summary above suggests, their content is chiefly conceptual and typological: they locate recurrent issues in inter-power relations and schematically indicate the range of the related arrangements. I would like most chapters in the book to add to this kind of sociological argument a few

short accounts of major episodes illustrating the historical significance of the various inter-power relations; and I would like most of those accounts to draw on the "story" of the Weimar republic, and thus to impart some historical concreteness to that argument. It had been my intention, during my stay at the Kolleg, to gather material for my discussion of such German episodes. But it occurred to me, once I had reached the Kolleg, that the best use to which I could put its unique atmosphere and facilities was to grapple with the sociological substance of the book by the sustained work of drafting on largely conceptual matters whose provisional results I have related. I assumed that the business of writing the narrative illustrations (beginning with those on the Weimar Germany) could be undertaken without much difficulty in the context of normal University teaching. So far (I write in mid-October 1994) I have been proven wrong; the draft of my chapter on the police has not been started yet, nor have I been able to undertake any of the several "narratives" I plan to introduce into my book. I wonder if this qualifies me for another year at the Kolleg?

Alexandre Popovic

Mes chers amis, dans le monde musulman ...

Né le 29 mai 1931 à Belgrade. Directeur de Recherche au Centre National de la Recherche Scientifique et Chargé de Conférences à l'Ecole des Hautes Etudes en Sciences Sociales et à l'Ecole Pratique des Hautes Etudes de Paris. Après s'être intéressé à l'histoire du monde musulman médiéval, il s'est tourné vers l'histoire moderne et contemporaine des musulmans des Balkans. Il dirige le groupe de recherche du CNRS intitulé «La transmission du savoir dans le monde musulman périphérique». Il a publié: *La révolte des esclaves en Irak au IXe siècle* (Paris, Geuthner, 1976); *Les ordres mystiques dans l'Islam, cheminements et situation actuelle* (en collaboration avec Gilles Veinstein, Paris, Editions de l'EHESS, 1986); *L'Islam balkanique* (Berlin-Wiesbaden, Otto Harrassowitz, 1986); *Naqshbandis. Cheminements et situation actuelle d'un ordre mystique musulman* (en collaboration avec Marc Gaborieau et Thierry Zarcone, Istanbul-Paris, Institut Français d'Etudes Anatoliennes — Editions Isis, 1990); *Les musulmans yougoslaves (1945-1989). Médiateurs et métaphores* (Lausanne, l'Age d'Homme, 1990); *Presse turque et presse de Turquie* (en collaboration avec Nathalie Clayer et Thierry Zarcone, Istanbul-Paris, Editions Isis, 1992); *Un ordre de derviches en terre d'Europe* (Lausanne, l'Age d'Homme, 1993); *Les derviches balkaniques hier et aujourd'hui* (Istanbul, Editions Isis, 1994); *Les musulmans des Balkans à l'époque post-ottomane. Histoire et politique* (Istanbul, Editions Isis, 1994); *Cultures musulmanes balkaniques* (Istanbul, Editions Isis, 1994). — Adresse: Ecole des Hautes Etudes en Sciences Sociales — Centre d'Etudes sur la Russie, l'Europe orientale et le Domaine Turc, 54, bd. Raspail, F-75006 Paris.

Tout Fellow normalement constitué arrivant à Berlin se trouve devant un dilemme insoluble: soit profiter des conditions de travail qui lui sont offertes au Wissenschaftskolleg pour avancer au maximum ses projets et ses

travaux professionnels; soit faire table-rase de tout cela pendant les dix mois dont il dispose, pour s'immerger dans l'histoire et la culture allemande d'hier et d'aujourd'hui, afin d'essayer de comprendre un certain nombre de choses ...

C'est naturellement une solution «moyenne» (donc batarde !) qui s'impose d'elle-même à tout le monde, d'où les signes d'énervement chez les uns et chez les autres, que l'on décèle vers la fin de notre séjour du côté de la Wallotstraße .. .

Cependant, un Fellow travaillant dans un domaine «excentrique», doit s'attendre lui, de surcroît, à quelque chose de plus. Il doit en effet être prêt à écouter patiemment les élucubrations multiples (et parfois à répétition) sur des sujets «humanistes» plus généraux, ou bien, franchement surréalistes, mais qui lui donneront en revanche l'occasion de pouvoir réfléchir longuement pendant ce temps-là à ses propres travaux. (J'ai disserté quelque peu à ce sujet lors de mon intervention intitulée «Les fourmis musulmanes» au cours d'un colloque du mardi matin, il y a quelque mois).

Mais revenons sur terre.

En relisant mon programme de travail d'octobre 1993, j'ai eu la confirmation que je suis décidément un Fellow absolument parfait (ce en quoi je n'ai jamais douté d'ailleurs), car tout ce que je comptais faire au cours de cette année est soit terminé pour de bon, soit «en cours d'achèvement», alors que beaucoup d'autres projets, et de travaux en tout genre, sont venus s'ajouter entre-temps

Faisons les comptes d'apothicaire en suivant les six points de mon programme d'octobre 1993: la préparation du volume sur la presse des musulmans des Balkans avance lentement mais sûrement, ainsi que la préparation de celui sur la confrérie des derviches *sa'dis,* et il en est de même (mais avec des lenteurs «indépendantes de ma volonté») des deux volumes collectifs annoncés, dont je suis le co-éditeur. Deux des trois articles qui étaient «en cours d'achèvement» sont maintenant sous presse («Le culte des saints chez les musulmans des Balkans», et «Les *medrese* dans les Balkans: des premières innovations du milieu du XIXe siècle à nos jours»), alors que le troisième se fera, «dans les transes», au cours de cet été inchallah ! Le numéro 14 de la *Lettre d'Information* de «La transmission du savoir dans le monde musulman périférique» est paru en juin 1994, alors que la parution du fascicule numéro 2 du *Dictionnaire biographique des savants et grandes figures du monde musulman périphérique* subira quelque retard. Ajoutons enfin l'essentiel, à savoir le point numéro 4 du programme en question, que je garde sciemment pour la fin, afin de faire ressortir au maximum l'exploit extraordinaire (et quelque peu déraisonnable) de mon éditeur stambouliote qui aura réussi à publier en 1994 non pas *un, mais trois* volumes de mes «oeuvres choisies», regroupant ainsi une soi-

xantaine de mes textes anciens, sous les titres suivants: (1) *Les derviches balkaniques hier et aujourd'hui;* (2) *Les musulmans des Balkans à l'époque post-ottomane. Histoire et politique;* et (3) *Cultures musulmanes balkaniques.* Il est inutile d'ajouter que ce marathon scripturaire (de onze cents pages en tout) comportait non seulement la préparation des textes en question pour la publication, ainsi que les interminables séances de correction d'épreuves, mais aussi la confection des *addenda* (un pour chaque texte), des *Avant-propos* et des *Postfaces* ... Il m'est tout particulièrement agréable de rappeler ici à ce sujet, que ce travail de bénédictin fut grandement facilité par les compétences (et la bonne humeur constante !) de ma secrétaire, Marianne Karbe, dont l'aide m'a été extrêmement précieuse, et que je remercie de tout coeur.

Disons aussi, que pour meubler mes loisirs, j'ai fait également quelques communications et conférences, dont voici la liste: «The Image of Islam as an Instrument of Ethnie Radicalization in the Balkans», communication présentée à la Conférence «Ethno-Radicalism and the New World Order» (an Israeli-German Conference), organisée par le Berlin Institute for Comparative Social Research, Berlin, November 12-14, 1993; une «discussion informelle» à propos de mon texte «Sur quelques mythologies journalistiques» *(Liber,* n° 14, Paris, juin 1993) aus Wissenschaftskolleg même, 13 décembre 1993; «Approches de l'Islam périphérique», communication présentée au Séminaire «Les sciences sociales et le fait religieux», organisé par le Centre franco-allemand de recherches en sciences sociales, Berlin, 24 janvier 1994; «La transmission du savoir dans le monde musulman périphérique», conférence présentée à *l'Institut für Islamwissenschaft der Freien Universität Berlin,* 8 février 1994; «Le monde musulman *périphérique* et le monde musulman *transplanté»,* conférence présentée au *Forschungsschwerpunkt Moderner Orient,* Berlin, 7 avril 1994; «Les pratiques de mortification dans les ordres mystiques musulmans», communication présentée à la table-ronde «Les confréries dans le monde méditerranéen (XVème-XXème siècle). Confréries chrétiennes. Ordres mystiques musulmans», Centre Thomas More à la Tourette, l'Arbresle, près de Lyon, 9-10 avril 1994; «Conflits ethniques dans les Balkans», conférence présentée au *Forschungskolloquium zur Entwicklungssoziologie «Transformationsprozesse», Fakultät für Soziologie, Universität Bielefeld,* 11 mai 1994; «La transmission du savoir dans le monde musulman périphérique», colloque au Wissenschaftskolleg, 28 juin 1994; «Mémoire et mémorisation chez les derviches, ou de l'usage de la poésie dans les *tarikat»,* communication présentée au Colloque International «Memoria. Kulturelle Erinnerung und Formen ihrer Bewahrung im Islam und seinem Umfeld», *Seminar für Semitistik und Arabistik, Freie Universität Berlin,* 5-9 juillet 1994; «A propos de la presse musulmane balkanique des origi-

nes à 1918», communication présentée au Colloque «Letters to the editor: Newspapers and Public Opinion in the Islamic World», *Seminar fûr Sprachen und Kulturen des Vorderen Orients, Ruprecht-Karls-Universität Heidelberg,* 8-9 juillet 1994.

Mais mon séjour à Berlin a été aussi une occasion exceptionnelle pour resserrer les liens (ou en créer de nouveaux) avec divers collègues berlinois, voire plus largement avec ceux d'Allemagne, et leurs institutions respectives, avec lesquels je collabore depuis de longues années, sans parler des liens d'amitié qui me lient dorénavant avec les Fellows 1993/94 et le personnel du Wissenschaftskolleg, dont certains sont devenus tout simplement des amis très proches.

Une très belle année universitaire en somme, fertile et agréable à tous les points de vue, mais comme nous le répétait si justement notre *Rektor,* tout au long de ces dix mois, *mes chers amis, dans le monde musulman ...*

Bob Pressey

Save the World, then Study Medieval History

Bob Pressey is a Senior Research Scientist with the National Parks and Wildlife Service in New South Wales. Those readers who do not know the location of New South Wales have probably had a Euro-centric education which has left them with only a sketchy geography of the Southern Hemisphere. Here is a clue, though: New South Wales was once used by Britain as a penal colony. This could explain the accompanying photograph. Bob has a BSc (Hons) from the University of Sydney, a Master of Environmental Studies from Macquarie University and a PhD from the University of New South Wales, somewhere in the Southern Hemisphere. The motivation for those qualifications has been a need to understand the natural world and a desire to use that knowledge to change the way human beings use the natural world. For the last eight years, he has been engaged in research on systematic approaches to establishing protected areas for nature conservation.

I came to Berlin because I went to Caracas. I went to Caracas because my parents gave me a sense of fairness that I directed to the situation of all the species that share the planet with *Homo sapiens*, the wise ape. When I was at school, in the western suburbs of Sydney, I began to wonder why beautiful parts of Australia were being cleared for profit, not for need, and why irreplaceable topsoil was being blown and washed out to sea. I also wondered why some species had been exploited to the point of extinction, an oblivion much more profound than death. I was indignant that the French government persisted with nuclear testing in the South Pacific, despite the weight of evidence that the outcome was long-term poisoning of the surrounding land and sea and that fallout would find its way into the milk that I drank.

Those questions and reactions began a long random walk through the education system, into seven or so years as a freelance environmental consultant and then into my present job as a researcher. The walk was a quest for knowledge and ways to change the things that were wrong about the

way our species manages the `environment' — that term often given to all the other things in the world apart from *Homo sapiens*. The word 'environment' rolls off the tongue fairly easily, especially in these days when it is fashionable in some circles to express concern for whales and kangaroos. But the environment is much more complex and wonderful than any of us know.

Why be concerned about the conservation of biodiversity? Well, first of all, it is fading before our eyes as more people try to extract more things from the natural environment. Why should this be a bad thing? There are several reasons. Many conservationists argue that there are sound anthropocentric reasons for nature conservation and appeal to enlightened self-interest. They list all the benefits that come from a healthy and diverse natural environment and list the consequences of losing these services and opportunity values. Others, like myself, simply believe that nature conservation is a good thing. How do we justify that? In the same way that we justify voting rights for women or legislation against the abuse of children and animals. The rights of animals, women, workers, blacks and children are ethical stances that we now take for granted. But many people had to battle and make sacrifices for them, just as they are doing now for the rights of nature (Nash, 1990). An ethic is a self-imposed constraint on behaviour. An important emerging ethic in global society is that nature conservation is good for its own sake. This ethic will become increasingly influential in the coming decades.

Part way through my random walk, which still has about forty years to go, barring accidents, I flew to Caracas, the capital of Venezuela. I was to attend the IV World Congress on National Parks and Protected Areas. One of the other Australians to attend was Chris Margules, with whom I had worked for some years. Early in the two week congress, we met Dick Vane-Wright and Chris Humphries from the Natural History Museum in London. I had seen their work and we had much to discuss. I also found them congenial company, and we were all committed to the exploration of Venezuelan music, food and beer. We also shared a healthy cynicism about the slow pace of change in environmental management due largely to political pragmatism but also partly to the cumbersome bureaucracies that have been put in place to further the environmental cause.

Soon after our time in Caracas, Vane-Wright and Humphries were contacted by the Wissenschaftskolleg zu Berlin about possible fellowships. The idea of a *Schwerpunkt* on biodiversity conservation emerged which would culminate in a multi-authored book on systematic approaches to selecting protected areas. I was contacted about being one of the participants. The editors of the book were to be the `Caracas Four', veterans of many restaurants and music spots in Venezuela. So from the western sub-

urbs of Sydney, my random walk in the area of environmental protection led to Wallotstrasse 19, and what an impressive place it was.

My time in the Kolleg was limited by other commitments to one month in October 1993 and two months in June-July 1994. In October, Vane-Wright and myself worked on a detailed outline of the book that would come out of the academic year. Authors were then allocated in (hopefully) appropriate combinations for the topics to be covered. All that was left was to begin work on the book but I returned to Australia concerned about the feasibility of producing an integrated product with so diverse an authorship. The diversity was not only in background and expertise but in perspectives on biodiversity and how to protect it. By the time I came back in June, much progress had been made, although there were vigorous debates about some issues and the structure of the book continued to evolve. More importantly, it became clear that the book would work — we had managed to use our diversity to strengthen, not weaken, the project.

The Kolleg itself was a marvellous experience. Memories of the October visit are dominated by the fine family apartment in Koenigsallee, bicycle rides in the Grunewald, stern Berliners on Kurfürstendamm, and a weekend trip to Prague. Life in June and July seemed so full and intense that my e-mails and faxes to colleagues in Australia waxed lyrical about the beauty and richness of Berlin in summer, without being able to explain my euphoria. There are so many things to remember that I could fill pages here. Some that come to mind now are runs in the Grunewald with Scott Camazine most mornings, our regular attempts to better our times over a set course, finishing at the Grunewald gate, bending over panting and hurting with me asking what time we had done, Scott saying something like `that was not fun !', walking back into the Kolleg sweating in an unrefined way before showering, coffee and several breakfasts on the terrace on balmy mornings, hammering on the keyboard afterwards in my office above the entrance to the Kolleg, looking out the window onto gardens and a quiet treelined street, lots of lunches with some of the best minds in Europe benignly amused at this bunch of scientists working on nature conservation or something, most of them preferring to talk about issues more important that this thing called biodiversity (perhaps some new scientific fashion that will fade and be replaced), afternoons on the computer, evening dinners on the Kolleg terrace and exchanging Australian observations on life in Europe with Janice Margules, who died much too young on returning to Australia, all the other evenings in Berlin with fragments of recollection now mixed, the "Quasimodo" basement with the basse so loud that it vibrated in the chest cavity, watching and dancing a little at "Abraxas", countless cafes and restaurants with tables on the sidewalks, conversation and wonderful company over wine and borrowed cigarettes,

nights on the top floor terrace with the sounds of cars and conversation drifting up from the cafe, the old lady in the opposite apartment tidying her room, the full moon rising over the rooftops, the Piranha cocktail bar and ordering drinks by the half litre (or was it by the litre?), an evening beside the lake, a long moonlit walk in the forest, a late picnic deep in the Grunewald and a rainstorm to make it seem wild and distant from cities, wanting to stay in a fairy tale forest far from the rest of the world, racing the buses down Kudamm on the Kolleg bicycle, playing pool at the "New York" cafe, and countless other impressions very difficult to disentangle and explain. When people asked me about my time in Berlin, I could only say that it was wonderful. I couldn't tell them exactly why.

If all these fragments seem to emphasise aspects of my stay other than research, the nominal reason for my presence in Berlin, I should reassure you that we did work hard. The Kolleg gave us an experience that is rare in a professional career — the best minds in a field of science gathered together to agree, argue, reconsider and write about a structured compilation of ideas and techniques. The book will be a major scientific advance. It will pull together many existing ideas in new ways and will describe new ideas. It will be called something like "Priority Areas Analysis: Systematic Approaches for Conserving Biodiversity". It will mainly concern the selection of new protected areas. This is important because protected areas will continue to be the most important means of carrying other species into the twenty-first century, along with *Homo sapiens*. The importance of systematic approaches is that most protected areas have been selected in an *ad hoc* way and are much less effective than if they had been located systematically in response to an explicit goal. The application of systematic approaches will maximise the effectiveness of any new protected areas.

So the Wissenschaftskolleg has facilitated the production of a book on state-of-the-art approaches to locating protected areas. This will be very influential in scientific circles and will alert many managers and policy-makers to problems and solutions. What remains to be done? Unfortunately, a great deal. The availability of systematic approaches will not, in itself, save the world, or even the world's remaining natural areas. The main impediments to saving nature are political pragmatism, lack of perspective by conservationists in promoting areas for protection, and the conflicting agendas of agencies and lobby groups. Overcoming these will need substantial changes to current policy agendas. These changes will be accelerated if more scientists learn the rules of the policy game and play more effectively.

What else is needed? The combination of improved science and enlightened policy must produce cultural and social organisations that foster, not just tolerate, nature conservation. At the moment we have a type of ecol-

ogical apartheid — reserved land and the rest. What we need is a spectrum of protection measures from outright nature conservation through various mixes of conservation and extractive uses, to outright extraction at the other end. Moreover, these protection measures must be applied to the right places in the right ways. More broadly, we need a way of designing human activities so that the compatibility of human activities with the maintenance of natural diversity is a much more important criterion than at present. This is the approach promoted by the Wildlands Project, a grand vision for nature conservation that is continent-wide and will be implemented over decades and centuries (Noss and Cooperrider 1994). It is highly, but not hopelessly, ambitious. The first step in making a grand vision become a reality is to articulate it.

As part of this grand vision will be an emphasis on biophilia, acknowledgment of the human bond with other species and encouragement of respect for other life forms (Wilson 1984). Another crucial part will be the prudent location of protected areas of several types. This is where our team will contribute, thanks to the sponsorship of the Wissenschaftskolleg zu Berlin. When the whole process of the integration of human activities with the natural world is further underway, it will be time to devote more effort to such luxuries as medieval history—areas of intellectual endeavour that are interesting but do not make *Homo sapiens* civilised. `Civilised' means brought out of a state of barbarism, instructed in the arts of life, or enlightened and refined. We can claim to be all these things when we learn to live without destroying the natural world of which we are part.

Much of the pleasure and effectiveness of working at the Wissenschaftskolleg was due to its staff who were invariably helpful and pleasant. I would like to express my thanks to all the staff of the Kolleg, its kitchen, its administrative support, and its library, with whom I dealt during my stay.

References that any civilised person should read:

Nash, R. (1989) *The Rights of Nature: a History of Environmental Ethics.* University of Wisconsin Press

Noss, R. F. and Cooperrider, A. Y. (1994) *Saving Nature's Legacy: Protecting and Restoring Biodiversity.* Island Press, Washington DC

Wilson, E. 0. (1984) *Biophilia.* Harvard University Press, Cambridge, Massachusetts

Eduardo Rabossi

Naturalizing Human Rights

Born in 1930. Degrees in Law (Buenos Aires) and Philosophy (Duke). Post-graduate studies (Oxford). Full Professor of Metaphysics and Philosophy of Language and Professor of Human Rights at the University of Buenos Aires. Director of the Argentine Society for Philosophical Analysis. Former President of the Interamerican Philosophical Society. Former Fellow of the National Humanities Centre, Member of the National Commission for the Disappeared and State Undersecretary for Human Rights (1984-89). Book publications: *The Moral Justification of Punishment. Ethics and Analysis. Ethical Studies. Philosophical Analysis. Language and Metaphysics. The Universal Charter of Human Rights. Nunca Mas* (co-author). — Address: Anchorena 1342, 3er piso, 1425 Buenos Aires, Argentina.

Some contemporary philosophers have paid attention to human rights and have produced, typically, a number of justificatory/foundationalist theories concerning their nature and contents. They disagree among themselves about the scope and details of their justifications, but they agree on the three basic tenets: [a] human rights are a species of moral rights (`moral' does not refer here to a positive morality but to a critical, philosophical one), [b] human rights are justified/founded when deducible from a moral principle (or set of moral principles), and [c] the proper philosophical approach is an aprioristic one.

As I did not share this approach to philosophizing about human rights I found myself involved in discussions with some foundationalist colleagues. But such discussions were of a "sporting" type: the sort of conceptual discussions philosophers are prone to be engaged in. Actually, the real motivation to develop a different theory of human rights came, not from philosophical controversy, but from human rights practice. During the 70's Argentina was under the spell of terrorism, both "lay" and state, and human rights violations were an everyday affair. The claim for protecting basic civil and political rights was then urged by NGOs and some political parties, and the activation of the regional and universal human rights agencies was attempted with some success. It was clear that human

rights were an important reality in contemporary culture. It was also clear that an operative theoretical framework that would ground actual practise was badly needed: foundationalist doctrines were irrelevant to that effect. The adequacy criteria for such a framework were rather obvious: a theory of human rights should be free from aprioristic assumptions, has to be based on a careful description and evaluation of the cultural, political and legal features of the human rights phenomenon, and should be useful for practical purposes.

I arrived in Berlin with the general plan of a book in mind, several manuscripts and the evaluation of some basic bibliography. I knew that I needed to think hard about the project, to check up some essential bibliographical items that I was unable to find out in my country, and to discuss extensively my views with some colleagues. I was sure that the Wissenschaftskolleg would satisfy the "material" needs, but how about the bibliography and, particularly, the intellectual atmosphere that is so essential in this sort of work? The list of scholars was impressive but variegated. It ranged from historians to literary critics, from musicians to biologists, from political scientists to legal experts. Would that group of people manage to constitute an intellectual community?

It did, indeed, and in a very impressive way. The Tuesday Colloquia became a forum to present our ideas and a place were public discussions took place. Lunchtime was also an opportunity to exchange ideas. And the quiet atmosphere of the studios was a proper place for fruitful dialogues. I soon became involved in long talks, for example, on human rights violations in Argentina and Serbia, on democratic transitions, on modern German constitutional law, on Nazism and neo-Nazism, etc. But the unexpected twist came from the biologists. To be frank, I never expected that my topic would have relevant connections with biology. To my surprise the Collective Intelligence Group was working on problems that pose interesting questions concerning social groupings and rules, communitary existence, and social solidarity. The same applies to the Biodiversity Group. I went to great lengths discussing the nature of collective intelligence, the features of global problems, and the intricacies of the Biodiversity Convention. In short, the intellectual experience was unsurpassed.

But living in the Wissenschaftskolleg was also a most attractive social experience. The Thursday dinners, concerts, conferences, and informal meetings in different apartments helped to build up a friendly atmosphere. That was also a remarkable success.

I arrived back in Argentina enriched by a period of intensive and productive work, with the manuscript of a book that — I hope — will soon see the light, with a host of new friends, with a face-to-face knowledge of Berlin and its people, and with the feeling that creating an ideal community

like that of Wissenschaftskolleg is not an impossible deed. It is not, of course, an easy one. You need to put together an attractive big city, some handsome surroundings, an impressive headquarters, comfortable lodgings, a kind but firm rectorial leadership, an able administrative staff, some extraordinary librarians and secretaries (my *Sekretärin* was the ideal one), and a group of nice scholars. If you mix these ingredients carefully you will get the Wissenschaftskolleg.

Gustav Ranis

Development: Theory, Policy and Political Economy

Born 1929, Darmstadt, Germany. B.A. Brandeis University, 1952. M.A. Yale University, 1953. Ph.D. Yale University, 1956. Director, Pakistan Institute of Development Economics, (1959-61). Professor of Economics, Yale University (1964). Frank Altschul Professor of International Economics, Yale University (since 1982). Assistant Administrator for Program and Policy, U.S. AID/Department of State (1965-67). Director, Economic Growth Center, Yale University, (1967-75). Ford Foundation Visiting Professor in Mexico (1971-72) and in Colombia (1976-77). Chief of ILO Comprehensive Employment Strategy Mission to the Philippines (1972-73). Chief of World Bank / Caricom Mission on Production and Investment Incentives in the Caribbean (1981). Main books: *Development of the Labor Surplus Economy: Theory and Policy,* 1964 (with John Fei). *Growth with Equity. The Taiwan Case,* 1979 (with John Fei and Shirley Kuo); *The State of Development Economics: Progress and Perspectives,* 1988 (with Paul Schultz); *Taiwan: From Developing to Mature Economy,* 1992; *The Political Economy of Development Policy Change,* 1992 (with S.A. Mahmood). Also more than 120 journal articles and other contributions on theoretical and policy related issues of development. — Address: Economic Growth Center, Department of Economics, Yale University, Box 8269 Yale Station, 27 Hillhouse Ave, New Haven, CT 06520-8269, USA.

During the 1993 — 94 academic year at the Wissenschaftskolleg, I was able to accomplish three main tasks. The first was to finish a book on development theory and policy, together with my Yale colleague, Professor John Fei, a project which had been under way for several years prior to my arrival in Berlin. The excellent library facilities made available by the Institute, combined with the enhanced ability to focus and concentrate, enabled me to break the back of this project. The volume started out as a revision of our joint earlier work, *Development of the Labor Surplus*

Economy: Theory and Policy, but ended up as a very different intellectual activity, reflecting both changes in the real world and in our conceptual apparatus for dealing with it. The volume begins with a view of the intellectual history of development theory, starting with the physiocrats and ending with the so-called "New Growth Theory". It then proceeds to present our own view of the transformation process in the open dualistic economy context. The second half of the volume is devoted to an analysis of the comparative development experience of the typologically distinct economic systems of Asia, Latin America and Africa in the light of the preceding framework. Professor Fei spent ten days in Berlin in November, which proved very helpful in ironing out differences and moving the manuscript forward. The volume will be published by Blackwell.

A second task I set for myself this year was a comparative investigation of the political economy of development policy change in Mexico, the Philippines and Taiwan. This constituted an effort to understand why societies differ in their ability to respond to the developmental challenge, i. e. moving beyond the IMF / World Bank type of description of alternative growth paths and their very different outcomes in Asia, Latin America and elsewhere. I was essentially able to complete the task of linking the policy changes made, or not made, to such dimensions of the landscape as the initial cohesiveness of a society, the size and location of various natural resource-related rents and the related ability of various interest groups to either frustrate reform efforts or accommodate themselves to the inevitability of change.

I was also able during the year to carry forward work on decentralization and development at both the theoretical and empirical levels. Having had some prior involvement in the generation of the *Human Development Report* of the United Nations Development Program, I have become increasingly aware of the importance of institutional and governance-related dimensions of transition growth efforts. A two-week trip to the Philippines in early 1994 moreover permitted me to achieve some empirical verification of a number of theoretical propositions dealing with the comparative efficiency and equity of both social and economic overhead allocations by central as opposed to local government authorities.

The possibility of interacting with other fellows engaged in very different intellectual pursuits constituted one of the real benefits of the year's activity at the Institute. In particular, at their request, I spent some time with the Biodiversity "Schwerpunkt" group, commenting especially on the economic and political dimensions of their project and at the same time learning a good deal about the biological/technical issues involved.

During the year I was invited to lecture at the Kiel Institute for World Economy, at the Technische Hochschule, Darmstadt, and at the OECD

Development Center in Paris. I also attended conferences at the North-South Center in Ottawa, Canada, at Stanford University and at UNCTAD in Geneva.

Anthony Rebelo

Algorithms for Biodiversity Reserve Configurations

Born in Bellville, South Africa in 1959 and matriculated at the Settlers High School. Received a B.Sc. degree from University of Cape Town in 1980; honours in 1981 and Ph.D. in 1992 on "The preservation of the Cape Flora: status, causes of rarity, ideals and priorities". Most of my career has been spent in the fields of pollination and conservation biology, first at the University of Cape Town and later at the National Botanical Institute. I have published some 40 scientific articles in these fields, including "A preliminary synthesis of pollination biology in the Cape Flora." At present I am involved in the "Protea Atlas Project", an attempt to involve laymen in amateur botany so as to map and conserve the Cape Flora, for which I edit a quarterly *Newsletter,* manage the data base and produce annual distribution maps and conservation reports. I have been proposed as co-chairman of the IUCN Species Survival Commission's "Cape Flora Specialist Group", but the establishment of the group will have to await my return to the new South Africa. — Address: Conservation Biology Research Unit, National Botanical Institute, Kirstenbosch, Private Bag X7, Claremont 7735, South Africa.

The invitation to spend some months at the Wissenschaftskolleg came at an interesting time. Locally, scientists and conservationists were in the doldrums concerning the future of their fields, and the general populace in South Africa was gearing up for a new South Africa amid predictions which raged from total anarchy to a golden era. Careerwise, I had just finished writing up most of my Ph.D. papers and had to decide whether to continue in the field of prescribing local and regional conservation priorities or change back to a new field more in line with the urgent needs of third world aspirations.

Arriving in January half way through the development of the book on Conserving Biodiversity, it became apparent that, apart from my chapt-

ers, several key issues needed to be addressed, and I devoted my time to them. Briefly these included:

1. There are a variety of algorithms for finding the "optimal reserve configuration." But these do not yield the same results. This has two causes — inefficient algorithms, which should be abandoned, and a "neglect" of the implications of the answer yielded by the algorithm. There is currently too much emphasis on a single answer, when in fact, there is a whole constellation of equally acceptable configurations which allow all taxa to be preserved in the same minimal area. This flexibility in conservation systems is the only hope conservationists have of attaining the goal of preserving biodiversity in a world where economic development requires habitat destruction. With colleagues in South Africa, I explored ways of determining this flexibility, which turned out to be far more complex than I had imagined. Instead of discrete "islands" of solutions, a maze of archipelagos appears to be the rule, defying extrapolation from simple algorithms.
2. Just before leaving for Berlin, I was invited to present a configuration for preserving all the plant species in the province of Natal, South Africa. It became obvious that Natal could not hope to achieve this goal, but could readily achieve a less ambitious goal of preserving all the plant species which occurred predominantly in the province. This was unacceptable to the conservation authorities. At Berlin this dilemma — marrying locally perceived goals with "international requirement" — proved to be one of the more exciting debating points among fellows. It will be a while before the full extent of this conflict is realized — until now there has been little attempt to prescribe specific local conservation requirements in international terms, or to ascertain what national and local authorities see as their conservation goals. The ratification of the Rio Convention requires that countries take steps to document and conserve their biodiversity — little thought has been given to the implications of this requirement.
3. The availability of the large number of data bases accumulated by the Natural History Museum London, which were available for manipulation, revealed another unexpected problem. Analyses of different groups (hawkmoths, tigerbeetles, birds, plants) were resulting in different reserve configurations and thus establishing conflicting conservation goals depending upon the groups investigated. This is an unacceptable state of affairs: we cannot have proponents of butterflies proposing a different set of conservation priorities than that proposed by other interest groups. It turns out that this too is a bogus: provided flexibility is taken into account, these alternative data bases can be accommodated into a single, ranked set of flexible priority sites.

Adding new data (in the form of distributions for additional taxa) will not much influence the irreplaceable component of the ideal nature reserve configuration, although it may narrow down options among the flexible component and require additional sites. Details will vary from region to region, but we need many more comparative data bases before principles and guidelines can be determined.

A major problem with my brief stay at the college was that I was sandwiched in the middle of the evolution of the book. The structure, rationale and philosophy of the book changed dramatically as each co-author came and went. This was inevitable as each contributor had tackled different problems, at different scales, in different political arenas and using different types of data. As the book develops into its final stages, it will be instructive to look back at the first drafts and marvel at the extent to which the interaction at Berlin has matured our outlooks.

My stay in Berlin bridged the gap between the old and the new South Africa. From afar I was able to keep track of developments around the election. As I was continually reminded, Berlin offered a counterpoint to these changes — its "apartheid" had ended a few years previously, integration was proceeding and the new unity was growing apace. Eva Hund, in her German classes, introduced us to some of the emerging racial, political and economic problems. Coupled with extensive sightseeing around Berlin, I am still intrigued by which features have been completely eradicated, which structures remain unchanged and the grandiose future plans for the new capital. What will happen in South Africa?

I would like to thank the staff at the Wissenschaftskolleg for their efficient and friendly assistance, especially Dr. Hans Georg Lindenberg for the electronic services, Andrea Friedrich for so efficiently sorting out my visa problems, and Gerhard Riedel for providing services for keeping the family entertained. Our stay was greatly lubricated by our anchor-family, Dick, Hazel, Naomi and Coral Vane-Wright — their efforts resulted in our stay being thoroughly normal — anticipated cultural, language, travelling, shopping and child-minding difficulties were easily overcome. Both my daughters took to *Vorschule* and *Kindergarten* with an ease that still leaves me astonished. I should like to thank the families of the fellows for the excellent support which allowed my wife to take advantage of the opportunity to explore Berlin and surroundings with our two-year-old son.

Hans-Jörg Rheinberger

Wi s senschaftsgeschichten

Geboren 1946, 1966-1973 Studium der Philosophie, Soziologie und Linguistik in Tübingen und Berlin, M.A. in Philosophie 1973, 1973-1979 Studium der Biologie in Berlin, Diplom in Biologie 1979, Promotion zum Dr. rer. nat. 1982, 1982-1990 wissenschaftlicher Mitarbeiter und Arbeitsgruppenleiter am Max-Planck-Institut für Molekulare Genetik in Berlin Dahlem, 1987 Habilitation an der FU Berlin im Fach Molekularbiologie, 1989-1990 Forschungsaufenthalt an der Stanford University, Program in History of Science, 1990-1994 Universitätsdozent am Institut für Medizin- und Wissenschaftsgeschichte an der Universität zu Lübeck, 1992-1993 Lehrstuhlvertretung am Institut für Wissenschaftsgeschichte an der Universität Göttingen, seit Oktober 1994 a. o. Prof. an der Naturwissenschaftlichen Fakultät der Universität Salzburg mit den Lehrgebieten Molekularbiologie und Wissenschaftsgeschichte. — Adresse: Universität Salzburg, Naturwissenschaftliche Fakultät, Institut für Genetik und allgemeine Biologie, Hellbrunnerstr. 34, A-5020 Salzburg.

Der Juli war ausgefüllt mit zwei Verpflichtungen, die ich mir für den letzten Monat am Wissenschaftskolleg zugezogen hatte: ein Vortrag — *„All that Gives Rise to an Inscription in General"* — im Rahmen der Tagung des Potsdamer Einstein-Forums über „Formen der Wissensvermittlung" und die Durchführung der Sommerakademie über *„Communicating Nature* — Die Semiotisierung der Natur im 19. und 20. Jahrhundert" mit den Walter-Rathenau-Stipendiaten im Berliner Verbund für Wissenschaftsgeschichte. Bei beiden Anlässen ging es um die historischen Voraussetzungen sowie die epistemischen und praktischen Folgen der Durchsetzung des Informationsdenkens in der Molekularbiologie. Damit ist zugleich einer der Schwerpunkte meiner Arbeit am Wissenschaftskolleg im vergangenen Jahr angedeutet.

Als ich am 31. Juli ans Kolleg zurückkam, waren die meisten Fellows abgereist. Das Abschied-nehmen-Müssen wurde mir damit weitgehend erspart, was blieb, war, die Bücherkisten zu packen und den Mitarbeitern

des Kollegs für das wunderbare Jahr zu danken, das sie uns allen im Grunewald ermöglicht hatten.

Zu den schönsten Erfahrungen der Zeit am Wissenschaftskolleg zählt eine Arbeit, die noch nicht geschrieben ist, und die vielleicht nie geschrieben wird. Mit großer Regelmäßigkeit begab ich mich jeden Montagmorgen — eine Strebe im Gerüst der dahinfließenden Wochen, wie die gemeinsamen Mittagessen Streben im Gerüst der dahinfließenden Tage waren – in das Archiv zur Geschichte der Max-Planck-Gesellschaft an der Boltzmannstraße in Dahlem. Ich hatte unter anderem vor, mir den Nachlaß von Carl Correns etwas näher anzusehen. Emmy Stein, eine ehemalige Mitarbeiterin des Kaiser-Wilhelm-Instituts für Biologie, hat in den fünfziger Jahren begonnen, Material zu einer Geschichte dieses Instituts zu sammeln und den vom Krieg verschonten Teil der Papiere von Carl Correns zu ordnen. Ihr Vorhaben blieb in den Anfängen stecken. Der Gründungsrektor des KWI für Biologie promovierte bei Carl von Naegli in München, welcher seinerseits zu den Korrespondenten Gregor Mendels gehörte. Das posthume Schicksal Naeglis — zweifellos einer der bedeutendsten Botaniker des 19. Jahrhunderts — wurde es, die Bedeutung von Mendels Arbeit unterschätzt zu haben. Man könnte es als einen Akt historischer Wiedergutmachung betrachten, daß sein Schüler Correns als Privatdozent in Tübingen zwischen 1894 und 1900 jene Kreuzungsexperimente mit Erbsensorten und Maisrassen durchführte, die ihm — neben Hugo de Vries in Amsterdam und Erich von Tschermak-Seysenegg bei Wien — den Namen eines „Wiederentdeckers" der Mendelschen Regeln eintrug, ein Ereignis, das gemeinhin als die Geburtsstunde der klassischen formalen Genetik angesehen wird. Correns' diesbezügliche Versuchsprotokolle befinden sich bei den Archivmaterialien. Auf welchem Weg gelangte er zu den Mendelschen Vererbungsregeln? Es schien mir bemerkenswert, daß eine der Schlüsselepisoden der neueren Biologiegeschichte, soweit es Correns angeht und soweit für mich ersichtlich, noch keinen Wissenschaftshistoriker hinter diese Protokolle und Notizen gelockt, jedenfalls nicht zu einer eingehenden Analyse veranlaßt hat.

Das Verführerische an dieser Geschichte war, daß sie unmittelbar nichts mit meinen Projekten für die Zeit am Kolleg zu tun hatte. Ich ging Blatt für Blatt die ganzen Protokollkonvolute durch, die sich aus der Zeit von 1890 bis 1900 im Nachlaß fanden. Wer auch immer sie geordnet haben mag, ob Emmy Stein oder Correns selbst, sie sind nach Pflanzennamen geordnet und diese wiederum nach dem Alphabet. Wir werden den Fluch des Alphabets nicht los bis zum Ende der Zeiten, diese irreversible Linearisierung allen Kontextes. Die Kreuzungsversuche mit Pisum, Zea Mais, Levkojen, Phaseolus, Lilium, Matthiola gehen einher mit einer Vielzahl von Untersuchungen über den Bau und das Wachstum der Zellmembran,

Reizphysiologie, Algen, die vegetative Vermehrung von Moosen, Floristisches. Die Beobachtungen, Zeichnungen und Berechnungen stehen oft in Zusammenhang mit den Lehrveranstaltungen, die Correns in Tübingen durchgeführt hat. Bei Mais waren es die Xenien, das heißt bevorzugt an den Samen und Früchten der Mutterpflanze sichtbar werdende Merkmale des Pollengebers, die Correns im Anschluß an Charles Darwins Beobachtungen interessierten. Die Versuchsaufzeichnungen zu Pisum und Mais scheinen ziemlich vollständig zu sein; jedenfalls gehören sie zu den umfangreichsten Bündeln. Es sind Ergebnisprotokolle fast ohne erläuternde Notizen, schwer, wenn überhaupt interpretierbar für jemanden, der solche Versuche nicht selbst durchgeführt hat — vielleicht ein Grund für ihre bisherige Vernachlässigung. Viele der Blätter sind nachträglich und oft mehrmals bearbeitet: Die Kreuzungsserien des jeweils nächsten Jahres veranlaßten Correns zu Einfügungen in den Protokollen des Vorjahres. Geringfügige Unterschiede in Tintenfarbe und Handschrift wären zu identifizieren und den verschiedenen Bearbeitungsgängen zuzuordnen. Im Laufe dieser Überarbeitungen tauchen schließlich die Mendelschen Zahlenverhältnisse auf. Die Kreuzungsversuche hatten anfänglich, jedenfalls gemessen an ihrem Umfang, einen eher beiläufigen Stellenwert. Sie begannen unscheinbar, als Nebensache, als *Allotria,* wie Correns später sagte, im Kontext der anderen Tübinger Vorhaben. Sie gewannen im Verlauf einiger Jahre an Umfang, bis sie schließlich um die Jahrhundertwende Correns' Arbeit dominierten.

Stieß Correns auf die Mendelschen Vererbungsregeln, bevor er dessen Arbeit las? Oder verhielt es sich umgekehrt? Der Schlüssel zur Beantwortung dieser Frage ist vielleicht ein einziges, datiertes Blatt in einem der Konvolute über Pisum-Kreuzungen mit einem Hinweis auf den Augustiner. Der Eintrag lautet:

16.IV.'96.

Mendel (66) unterscheidet:
dominirende u. recessive Merkmale. Für unsere

Faelle ist	dominirend:	recessiv:
—Samenform	rund	kantig
— Samenschale: *(Albumen)*	grau bis braun	weiss
—Kotyledonen:	gelb	blassgelb, grün
— Frucht:	einfach gewölbt runzlig	
	grün (unreif)	gelb (unreif)

Die dominirenden und recessiven Merkmale treten gleich bei der ersten Generation so hervor, dass die ersteren je 3, den letzteren je 1 Individuum aufweist.

Die Hybridform von Samengestalt und Kotyledonen entwickelt sich unmittelbar direct dch die Befruchtg

Cot. (also) gelbY + grüne = gelbe + grünt = $^3/4$ gelb + $^1/4$ grün
Form. rundY + kantige = kantigY + runde = $^3/4$ rund + $^1/4$ kantig

Nicht verändert wird die Samenschale, die Fruchtform u. die Fruchtfarbe.

Spaeter aber giebt Mendel z. B. an, dass A (Samen rund, Cot. (S. 19) gelb) mit B (Samen kantig Cot. grün) bestäubt, lauter gelbe Samen gab, die rund waren.

Aufgrund vorläufiger Vergleiche kann die Lesart 16. IV. '00 als Datum wohl ausgeschlossen werden. Bleibt es beim 16. IV. '96, so muß das Kapitel Correns in der Geschichte der „Wiederentdeckung" der Mendelschen Regeln neu geschrieben werden. Seinen publizierten Äußerungen zufolge stieß Correns nämlich erst Ende 1899 auf Mendels Arbeit, kurz nachdem ihm am Ende einer schlaflosen Novembernacht die Erklärung für die bei Pisum und Mais beobachteten Zahlenverhältnisse aufgegangen war. — Es wird kein Weg an einer Mikroanalyse der Protokolle vorbeiführen. In den vielen Stunden, die ich bisher über Correns' Aufzeichnungen zugebracht habe, bin ich noch auf keinen gangbaren Pfad durch das Labyrinth dieses verästelten Experimentalprozesses gestoßen.

Es wäre auch zu schön gewesen, wenn es mir gelungen wäre — nach meinem ersten intellektuellen Lebensversuch als ‚dekonstruktivistischer` Philosoph Auf Dem Grat und meinem zweiten als ‚empiristischer` und ‚reduktionistischer` Molekularbiologe an der Ihnestraße in Dahlem —, in diesem akademischen Jahr zu einem erfolgreichen Wissenschaftshistori-

ker zu werden, der zu den Quellen geht, um aus ihnen wohlfeile historische Klarheit zu gewinnen. Es ist also wieder nichts geworden mit einer ordentlichen Verankerung im Katalog der akademischen Fächer. So hat mich die Losung des Wissenschaftskollegs von der produktiven Verunsicherung der Disziplinen eingeholt, bevor sie ausgesprochen war.

Vielleicht bin ich nicht konsequent genug im Verfolgen meines Zufallsfundes gewesen. Vielleicht ist aber auch das Bestandteil eines Kalküls, der seinerseits stochastische Züge hat. Hätte ich zugunsten dieses einen ‚Nicht-Projektes` alle meine anderen Vorhaben zurückstellen sollen? Ich habe es nicht getan, und so seien von diesen die folgenden wenigstens genannt: „Geschichte der Molekularbiologie", erscheint als Kapitel in Ilse Jahn et al., *Geschichte der Biologie,* Fischer Verlag, Jena 1995; der Sammelband *Objekte, Differenzen, Konjunkturen. Biologisch-medizinische Experimentalsysteme im historischen Kontext,* herausgegeben zusammen mit Michael Hagner und Bettina Wahrig-Schmidt, Akademie Verlag, Berlin 1994; das Buchmanuskript *Toward a History of Epistemic Things,* abgeschlossen im Juni 1994. Das Manuskript endet mit folgenden Worten:

"I have tried to follow, as closely as possible, the history of an experimental system — the *in vitro* system of protein biosynthesis based on rat liver cell extracts —, its establishment and reproductive dynamics, and the conjunctures, filiations, and substitutions it underwent during the course of 15 years, from 1947 to 1962. I have tried to show that such a system generates and follows its intrinsic time requirements, and that it constitutes a historical trajectory, much like objects and forms of art do constitute such trajectories. But I have also tried to show that such a trajectory, at one and the same time, creates and inserts itself into a particular technical, instrumental, representational, and institutional context. If we take the notion of context literally, an experimental system itself can be seen as having the characteristics of a text. It is the essence of a text that it remains a text only through being continually reread and rewritten. A text in the Derridean sense: «Cette possibilité structurelle d'être sevrée du référent ou du signifié (donc de la communication et de son contexte) me paraît faire de toute marque, fut-elle orale, un graphème en général, c'est-à-dire, comme nous l'avons vu, la restance non-présente d'une marque différentielle coupée de sa prétendue ‚production` ou origine. Et j'étendrai même cette loi à toute <expérience> en général s'il est acquis qu'il n'y a pas d'expérience de pure présence mais seulement des chaînes de marques différentielles.» (Derrida, *Marges de la Philosophie,* p. 378). This is exactly what holds for a productive experimental system in the realm of a space of representation. The enterprise called modern science derives its power from its peculiar spaces of representation. The forces they enact, and the rules by which they are governed, are no longer those of a Cartesian Subject.

Accordingly, our methodologies for assessing what happens where the generation of unprecedented events has taken over, must change. And so must change our views of what it means for a scientist, as a subject, to be engaged in the production of scientific novelty. During the last 150 years, our view of the world has been profoundly altered. We have witnessed "economies" that are no longer centered around an ego: a Darwinian economy of nature, a Marxian economy of production, a Nietzschean economy of moral, a Freudian economy of the unconscious, a Saussurean economy of the sign, a Foucaultian economy of discourse. We need an economy of scientific change, an economy of epistemic things.

Throughout this book, I have referred to the different epistemic stages and events of a particular research system: the protein synthesis system of Paul Zamecnik and his group at the Massachusetts General Hospital in Boston. I have localized it within the history of cancer research, of biochemistry, and finally of molecular biology. The reorientation at work in such a localization will naturally not come to a halt before my own narrative. In fact, if this narrative will give rise to recurrent assessments at all, it will have done its service. One objection will be that the institutional setting, the societal interests, the political power game involved in making science, and the actors have not enough been given voice. To which I answer that my purpose has been different: I have tried to convey a sense of what it means for an experimental system to become articulated in space and time so as to shape a whole laboratory culture, and what it means to become, to remain, and finally to cease being a generator of epistemic novelty. My concern has been with traces and things rather than with theories and interests. For those who have followed to this point, that will be an unnecessary statement. For those who read books by looking for conclusions, it stands as a necessary *caveat."*

„Theoretische Biologie" heißt das neue Zauberwort im Zauberberg an der Wallotstraße. Das gibt zum Rätseln Anlaß, and es wird für einen gewesenen Fellow spannend sein zu verfolgen, was daraus wird. Es ist wohl so, daß ich aus der Philosophie, in die ich einmal hineingeriet, bis heute nicht wieder herausgekommen bin. Aber je tiefer ich mich darin verstricke, desto weniger weiß ich, was Theorie ist. Meine Devise ist — Erzählen.

Karol Sauerland

Die Intelligenz und die Denunziation

Karol Sauerland, geboren in Moskau als Kind deutscher Emigranten. 1955-57 Studium der Philosophie an der Humboldt-Universität, Abbruch aus politischen Gründen. 1958 Übersiedlung nach Polen und 1959 Annahme der polnischen Staatsbürgerschaft, Studium der Mathematik sowie der Germanistik an der Warschauer Universität. 1963-65 Assistent für mathematische Logik und Modelltheorie bei Prof. Dr. Andrzej Mostowski. Heute Professor für deutsche Literaturwissenschaft und Ästhetik an den Universitäten in Warschau und Thorn, Vorsitzender der Philosophischen Gesellschaft in Warschau. Bücher: *Diltheys Erlebnisbegriff* und *Einführung in die Ästhetik Adornos* (Walter de Gruyter) und zur Geschichte der deutschen Ästhetik (polnisch), Artikel und Essays in internationalen Zeitschriften und Sammelbänden über deutsche Literatur und Philosophie von der Aufklärung bis zur Gegenwart, politische Beiträge in der *Neuen Zürcher Zeitung,* im *Kursbuch* und anderswo, Herausgeber der Zeitschrift *Europa* im polnischen „zweiten Umlauf" (Samisdat), Gastprofessuren an der ETH Zürich, in Mainz und an der FU Berlin. — Adresse: ul. Nowogrodsko 23/6, PL-00 511 Warszawa.

Seit Jahren sitze ich an einem Buch, das den vorläufigen Titel *Dichter, Denker und die Macht. Von Franz Kafka bis Heiner Müller, von Friedrich Nietzsche bis Hannah Arendt* trägt. Den größten Teil dieses Buches hatte ich vor der sogenannten Wende konzipiert bzw. geschrieben. Anfang 1988 hatte ich an der ETH, als ich dort Adolf Muschg, der sich gerade am Wissenschaftskolleg in Berlin aufhielt, vertreten durfte, eine Vorlesung *Der Dichter und die Macht* gehalten, die ich dann im Wintersemester 88/89 als Gastprofessor der Universität Mainz in erweiterter Form wiederholen konnte. Ich kann rückblickend sagen, daß ich in den Kapiteln des Buches auf Probleme reagierte, mit denen ich mich seit 1980/81, d. h. seit der Entstehung der Solidarnosc, besonders konfrontiert sah. Obwohl ich in die Solidarnosc-Bewegung involviert war und später unter dem Druck der Vertreter des Kriegszustandes zu leiden hatte, wollte ich intellektuellen

Abstand wahren, den ich durch die Suche nach historischen Ähnlichkeiten und womöglichen Gesetzmäßigkeiten sowie durch die Freude an phantastischen Vorstellungen (was wäre wenn ...) zu erreichen glaubte. Letztere sind das Thema des ersten Teils meines Buches. Dort entwerfe ich Machtmodelle, die ich aus literarischen Werken destilliere. Ich unterscheide vor allem den idealen von dem perfekten Machtapparat. Den idealen treffen wir im Werk von Kafka an, den perfekten schildern uns Samjatin, Huxley und Orwell. Der erste braucht Unordnung und Schlamperei, der zweite perfektioniert alles, er muß sogar in die sexuelle Sphäre, die Quelle dauernder Unordnung, eingreifen. Er schreckt vor Eingriffen am menschlichen Körper nicht zurück. In einem weiteren Kapitel wende ich mich Schriftstellern zu, die sich vorzustellen suchen, was passiert, wenn bestimmte generelle Veränderungen im Zusammenleben eingeführt oder von anderen als allgemeinen Willen empfunden werden. So fragt Canetti in der *Komödie der Eitelkeit,* was geschieht, wenn das Sich-Spiegeln verboten wird. Und Ionesco jagt der Idee nach, wie die Menschen auf plötzliche Verwandlungen in Nashörner reagieren würden. Im letzten Kapitel dieses ersten Teils zeige ich anhand des Werkes des bis 1985 in der DDR gelebt habenden Schriftstellers Wolfgang Hilbig, wie die Macht ein Nichts sein kann, wenn nämlich niemand bereit ist, von ihr zu erzählen, sie zu mythisieren. Der zweite Teil des Buches ist konkreten Fällen der Verwicklung von Dichtern und Denkern in die Macht gewidmet. Ich beginne mit Lukács und Bloch, deren plötzlicher Übergang zum Kommunismus überraschend war; ich behandle ferner den Stalinkult westlicher Intellektueller in den dreißiger Jahren und gehe in einem gesonderten Kapitel auf jene parteilosen sich zum Kommunismus bekennenden deutschen Schriftsteller oder Denker ein, die sich ideologisch engagierten. Ich nenne sie — wie etwa Brecht, Bloch, Feuchtwanger oder Arnold Zweig— parteilose Parteimitglieder. Sie spielten vor allem in Krisenzeiten eine führende Rolle, indem sie wesentlich dazu beitrugen, die angeknackste Reputation der Partei zu retten. Am besten läßt sich dies an Feuchtwangers Reisebericht *Moskau 1936* zeigen. Ein besonderes Problem erscheint mir nach der erneuten Lektüre von Stalinschen Schriften, etwa die *Über die Grundlagen des Leninismus* von 1924 und *Der Marxismus und die Fragen der Sprachwissenschaft* von 1950, wie gebildete Menschen von diesem Mann hingerissen sein konnten. Ich nenne dieses erst skizzierte Kapitel „Das Faszinosum Stalin, von seiner Sprache aus betrachtet".

Im dritten Teil meines Buches halte ich nach Persönlichkeiten Ausschau, die versucht haben, ihre Unabhängigkeit zu bewahren, wie Ernst Toller und Gustav Landauer in der Münchener Revolution, wie Gottfried Benn seit Ende der dreißiger Jahre oder wie Karl Kraus, der es verstand, über dreißig Jahre lang eine wirklich unabhängige Zeitschrift, nämlich *Die*

Fackel erscheinen zu lassen_ Dieses Unternehmen wurde allerdings 1933 nach Hitlers Kanzlerschaft sinnlos. Kurz darauf prägte Karl Kraus, den tiefen, wie ich zu begründen suche, Satz: *Mir fällt zu Hitler nichts ein.* Es gibt natürlich auch ein Kapitel, in dem ich über das Phänomen des Dissidenten reflektiere. Im letzten Teil des Buches stelle ich schließlich drei für meinen Zusammenhang relevante Machtauffassungen dar: die von Nietzsche und deren Auslegung durch die Sozialisten Trotzki, Lunatscharski, Sorel und den Polen Brzozowski, die von Witfogel, wie er sie im *Orientalischen Despotismus* entwickelt hat, und schließlich die von Hannah Arendt. Das Buch hätte ich wahrscheinlich in brutaler Weise, unvollkommen, 1989 erscheinen lassen sollen. Unnötigerweise hatte ich mich mit dem Aufbau-Verlag eingelassen, aber vielleicht hat das polnische Sprichwort „Nie ma tego zlego, co by na dobre nie wyszlo" („Es gibt nichts Schlechtes, was sich zum Guten nicht wendet") seine Berechtigung, denn vielleicht hat es sein Gutes, daß vieles von dem, was ich geschrieben hatte, heute seine Aktualität bzw. große Anziehungskraft verloren hat. So doktere ich weiter an dem Buch herum, was u. a. bedeutet, daß ich es nicht lassen kann, die jetzt immer wieder erscheinenden neuen Enthüllungen mit meinen Erkenntnissen zu konfrontieren. Eine solche neue Enthüllung war das 1991 erschienene Stenogramm einer geschlossenen dreitägigen Parteiversammlung der deutschen Kommission des Sowjet-Schriftstellerverbandes, d. h. der deutschen und ungarischen kommunistischen Schriftsteller im russischen Exil. Diese Parteiversammlung fand vom 4. — 9. September 1936 in Moskau statt. Jemand, der die Geschichte der kommunistischen Bewegung nicht näher kennt und dieses Stenogramm liest, wird meinen, es nicht mit sensiblen Intellektuellen, sondern mit Menschen zu tun zu haben, deren Leidenschaft es ist zu gehorchen, zu schnüffeln und zu denunzieren. Man kennt so etwas aus Dörfern, in denen der Geistliche um moralische Reinheit kämpft, sowie aus Klosterschulen und Internaten älteren Typs.

Ich begann, dem Phänomen der „Lust an der Denunziation" im Sowjetimperium und im Dritten Reich nachzugehen. Dank meines von der *Andrew W. Mellon Foundation* finanzierten Aufenthaltes im Wissenschaftskolleg zu Berlin vom 19. 1. bis zum 17. 4. 1994 kam ich zu Einsichten, die mich selber überraschten. Bei meinen Studien wurde mir klar, daß es wesentliche Unterschiede zwischen den Denunziationssystemen im Dritten Reich und im Sowjetimperium gibt. Auf eine kurze Formel gebracht: Die Nazis stützten sich auf eine massenhafte freiwillige Denunziation aus den Kreisen der Bevölkerung, während die Sowjets ihr Denunziationssystem selber organisierten. Sie rechneten nicht mit einer spontanen Unterstützung „breiter Massen", sondern entschlossen sich lieber zum Aufbau eines gigantischen Sicherheitsdienstes mit Tausenden und

Abertausenden von offiziellen und inoffiziellen Mitarbeitern. Die Gestapo arbeitete demgegenüber mit relativ wenig Personal, wie die neueste Forschung, insbesondere der Brite Robert Gelletaly in seinem Buch *The Gestapo and German Society* sowie die beiden Deutschen Klaus-Michael Malimann und Gerhard Paul gezeigt haben. Ein Glücksfall wollte, daß ich Paul Gelletaly in Berlin persönlich kennenlernen durfte. Gleichzeitig war mir die Möglichkeit gegeben, mit Mitarbeitern der Gauckbehörde und Herrn Gauck zu sprechen. Ohne ihre Erkenntnisse über die Zusammensetzung und Arbeitsweise der Stasi hätte ich meinen Vergleich nicht so gut durch Fakten absichern können, obwohl ich die Stasi nur als einen Sonderfall des Tscheka- (NKWD-, KGB-) Systems behandle, das wiederum nur ein Teil dessen ist, was man am besten mit Bolschewismus bezeichnet.

Ich habe mich natürlich gefragt, wie es im Sowjetimperium zu einem so umfassenden Denunziationssystem hat kommen können. Eine wichtige Rolle haben sicher die „Tschistkas", wie die Parteireinigungen im Russischen heißen, gespielt. Sie setzten gleich nach dem Sieg der Sowjetmacht über ihre Gegner ein. Bei der ersten Tschistka von 1921 wurden etwa 175 000 Mitglieder aus der KPdSU ausgeschlossen. Bei der zweiten, 1929, waren es bereits 300 000. Hinzukommt, daß die Kontrollkommissionen zwischen beiden Reinigungen 260 000 Mitglieder aus den Reihen der Partei entfernten. 1933 leitete die Partei die dritte Reinigung ein. Die Tschistka bedeutete die Institutionalisierung der Denunziation. Jedes Parteimitglied mußte von Zeit zu Zeit bereit sein, über alles und alle, d. h. sich und jeden anderen, zu sprechen. Dieses immer wieder praktizierte Melde- und Überwachungssystem schlug am Ende in eine epidemische Denunziation um.

Die Ergebnisse meiner Studien zu diesem Thema konnte ich am 29. März 1994, einem Dienstag, im Wissenschaftskolleg vortragen. Es entspann sich eine interessante, sehr sachliche Diskussion, die mir half, einige meiner Urteile zu präzisieren. Die Diskussion veranlaßte mich auch, ein weiteres Kapitel für mein Buch auszuarbeiten. Ich nenne es vorläufig „Das Phänomen der Bolschewisierung in der europäischen Linken". Diese Bolschewisierung setzte bereits zu Beginn des Jahrhunderts als Gegenbewegung zum sogenannten Bernsteinschen Revisionismus ein und trat um 1920 ihren Siegeszug an. Eine besondere Rolle spielte hier die III. Internationale und der Beschluß, daß sie in Moskau ihren Sitz haben solle. Gegen diesen Beschluß wehrte sich die deutsche Delegation, die Vertreterin der stärksten kommunistischen Partei in Westeuropa. Sie setzte sich aber nicht durch. Damit war die Voraussetzung für die fortschreitende Abhängigkeit der deutschen Kommunisten von den russischen geschaffen.

Bei meinen Studien kam ich zu dem Schluß, daß der Terminus Stalinismus irreführend ist, denn er suggeriert, daß das Sowjetsystem vorher eine andere, quasi humane Qualität aufwies, was keineswegs der Fall war. Von Anfang an wurde im Sowjetimperium ein mörderisches System der Gleichschaltung errichtet. Leider habe ich dieses Kapitel nicht mehr in Berlin abschließen können. Dazu war die Zeit zu kurz, wenn man bedenkt, daß ich gleichzeitig die Anmerkungen zu den bereits existierenden Kapiteln (etwa 280 Seiten) zu verifizieren und zu ergänzen hatte. Hierbei ergaben sich auch allerlei Veränderungen im Text selber.

Am Ende meines Aufenthaltes referierte ich in der sogenannten Mittwochsgesellschaft Grundbegriffe der politischen Theorie Hannah Arendts, die, wie ich finde, helfen können, die revolutionären Veränderungen im Jahre 1989 besser zu begreifen. Es entfaltete sich eine lebhafte Diskussion. Einige Teilnehmer fanden, ich solle mein Manuskript über Hannah Arendts Theorie zu Ende führen und einem Verlag zum Druck anbieten. Ich nahm an den Sitzungen der Mittwochsgesellschaft, deren Sprecher Robert Darnton war, gern teil, denn es ging immer wieder um Fragen, die einen Schwerpunkt meiner Forschungen darstellen: die Rolle der Intelligenz in den letzten zwei bis drei Jahrhunderten.

Ich habe den Eindruck, daß ich bisher noch nie so intensiv die Zeit für meine Forschungsarbeit habe nutzen können. Sicher liegt das an dem glänzenden Bibliothekssystem und daran, daß man keine überflüssigen Wege zu machen braucht. Und durch den täglichen Umgang mit so vielen Gelehrten fühlt man sich tagtäglich aufgefordert, seine Studien voranzubringen. Mit Bedauern verließ ich kurz nach Ostern die Wallotstraße, aber mit dem Gefühl der intellektuellen Bereicherung dank der vielen Diskussionen mit den Fellows, Gästen und ständigen Mitarbeitern des Wissenschaftskollegs.

Klaus Schreiner

Mit Maria und Toleranz

Geboren 1931 in Jagstfeld (Baden-Württemberg); Studium der Geschichte, der mittellateinischen und klassischen Philologie sowie der Theologie an den Universitäten Tübingen und München. 1961 Promotion zum Dr. phil. an der Universität Tübingen. Danach Wissenschaftlicher Assistent am Institut für geschichtliche Landeskunde und historische Hilfswissenschaften der Universität Tübingen; Habilitation im Jahre 1968; Dozent und außerordentlicher Professor an der Universität Tübingen. Seit 1976 Professor für allgemeine Geschichte unter besonderer Berücksichtigung des Mittelalters an der Universität Bielefeld. — Publikationen in Buch- und Zeitschriftenform über Themen aus folgenden Bereichen der mittleren und neueren Geschichte: Verfassungs-, Sozial- und Mentalitätsgeschichte; Ordens- und Kirchengeschichte; Bildungs- und Frömmigkeitsgeschichte; Universitäts- und Wissenschaftsgeschichte; Geschichtsschreibung und Hagiographie; Begriffs-, Theorie- und Rezeptionsgeschichte. Neueste Buchveröffentlichung: *Maria. Jungfrau – Mutter – Herrscherin*, München 1994. — Adresse: Fakultät für Geschichtswissenschaft und Philosophie, Universität Bielefeld, PF 10 01 31, D-33501 Bielefeld.

Es war am 4. Oktober 1993, als ich mein Auto mit Büchern und Klamotten vollstopfte und mich auf den Weg nach Berlin machte. Der Gedanke an fröhliche Wissenschaft in der Berliner Gelehrtenvilla, Wallotstraße 19, ließ guten Muts und guter Dinge sein. Die Frage, ob das Leben in einem siebenundvierzigköpfigen Rudel wissenschaftlich ambitionierter Geistesmenschen eine Quelle ungetrübter Freude sei, weckte keine hochfliegenden Erwartungen, sondern verunsicherte. Als am 31. Juli 1994 die Zeit des Fellowships abgelaufen war, bestand kein Anlaß mehr, sich durch gemischte Gefühle irritieren zu lassen. Der Abschied stimmte dankbar und glücklich.

Der Ortswechsel von Bielefeld nach Berlin versetzte nicht in die Lage eines unbeschwerten Neubeginns. Altlasten mußten abgetragen, eingegangene Verpflichtungen abgearbeitet werden. Kaum hatte ich im Wissen-

schaftskolleg Kisten und Koffer ausgepackt, begab ich mich nach Paris, wo ich auf einer Tagung des Deutschen Historischen Instituts einen Vortrag zu halten hatte. Die Tagung befaßte sich mit „Regionalen und nationalen Identitäten im späten Mittelalter und in der frühen Neuzeit"; das Thema meines Vortrages lautete: „Maria Patrona. Die christliche Gottesmutter als Herrin und Beschützerin von Städten, Ländern und Königreichen". Am Beispiel eines vornehmlich aus politischen und gesellschaftlichen Interessen gepflegten Marienpatronats suchte ich die identitätsbildende Funktion von Religion kenntlich zu machen. Die schriftlich ausgearbeitete Fassung des Vortrages soll im nächsten Beiheft der *Francia* erscheinen.

Zu einem druckfähigen Manuskript ausgearbeitet habe ich in den Wochen danach einen Vortrag über „Frömmigkeit in sozialgeschichtlichen Wirkungszusammenhängen des Mittelalters", den ich im Februar 1994 an der Humboldt-Universität aus Anlaß einer Tagung über „Mediävistik nach der Wende" gehalten habe. Erscheinen soll das Manuskript demnächst in einem Beiheft der *Historischen Zeitschrift.* In einen lesbaren, mit Anmerkungen ausgestatteten Aufsatz umgeschrieben habe ich außerdem einen Vortrag über „Gerechtigkeit und Frieden haben sich geküßt. Friedensstiftung durch symbolisches Handeln in der Welt des Mittelalters", den ich bei einer Tagung des „Konstanzer Arbeitskreises für mittelalterliche Geschichte" auf der Reichenau gehalten habe. (Demnächst in: *Vorträge und Forschungen.)* Für die *Zeitschrift für Historische Forschung* mußte das von Peter A. Dykema und Heiko A. Oberman herausgegebene Buch über *Anticlericalism in Late Medieval and Early Modern Europe,* ein Wälzer von 704 Seiten, rezensiert werden. Der Bericht, der Begriff und Sache des mittelalterlichen und frühneuzeitlichen Antiklerikalismus kritisch zu bilanzieren sucht, wuchs sich zu einer eigenständigen Abhandlung aus. Unter dem Titel „Gab es im Mittelalter und in der frühen Neuzeit Antiklerikalismus? Von der Schwierigkeit, aus einem modernen Kampfbegriff eine Kategorie historischer Erkenntnis zu machen" wird sie im nächsten Heft der *Zeitschrift für Historische Forschung* erscheinen. Auf einer Tagung, die Peter Blickle am Historischen Kolleg in München über Grundfragen des Kommunalismus konzipiert hatte, sprach ich über „Autonomie, Teilhabe, Konsens. Leitbegriffe kommunalen Handelns in Stadtgesellschaften des hohen und späten Mittelalters". Der Band mit den in München gehaltenen Vorträgen befindet sich im Druck. Dem Gedankenaustausch mit Berliner Kollegen und Studenten dienten zwei Vorträge, die ich an der Freien Universität hielt: den einen über die „Dormitio Mariae. Marias Grabeskirche in Jerusalem und die Bedeutung des Marientodes für die Frömmigkeit des abendländischen Mittelalters", der als Beitrag zum „Studium exemplare" über „Jerusalem und das Heilige

Land" gedacht war; den anderen als öffentlichen Fakultätsvortrag über „Glaubenszwang und Toleranz im Mittelalter". Vorträge, die etwas taugen sollen, erfordern Zeit und Kraft, Rezensionen im übrigen auch, sofern das, worüber man schreibt und urteilt, zuvor auch gelesen wurde.

Einen Offenbarungseid, wegen solchen Pflichtübungen mein eigentliches Arbeitsvorhaben vernachlässigt zu haben, brauche ich dennoch nicht zu leisten. Mitte Januar stellte ich im allwöchentlichen Dienstagskolloquium „Widersprüchliche Einstellungen und Verhaltensweisen der mittelalterlichen Christenheit gegenüber Juden" zur Debatte. Gefragt und gesucht wurde nach Antworten, die den Widerspruch zwischen theologisch begründetem Toleranzangebot und praktischer Diskriminierung verständlich und erklärbar machen. Um die theoretischen Grundlagen und begriffsgeschichtlichen Ausprägungen der Thematik in den Blick zu bringen, konnte ich auf meinen Artikel „Toleranz" (in: *Geschichtliche Grundbegriffe. Historisches Lexikon zur politisch-sozialen Sprache in Deutschland,* Stuttgart 1990) zurückgreifen. Die Wintermonate im Wissenschaftskolleg hatte ich vornehmlich dazu benutzt, Metaphern und Begriffe, in denen sich Bereitschaft zu religiöser Duldsamkeit und Wille zu kirchlichem Zwang sprachlich artikulieren, mit zeitgenössischen Ereignissen, Strukturen und Prozessen zu verknüpfen. Was bei meinen begriffs-, struktur- und kommunikationsgeschichtlichen Untersuchungen herauskam, hat nunmehr in einem Manuskript literarische Gestalt angenommen, das die *Historische Zeitschrift* in einem ihrer nächsten Hefte veröffentlichen will.

Zu meinem wissenschaftlichen Gepäck, das ich von Bielefeld nach Berlin transportiert hatte, zählte insbesondere ein überarbeitungs- und ergänzungsbedürftiger Rohentwurf für eine Kultur- und Sozialgeschichte mittelalterlicher Marienverehrung. Untersucht und dargestellt wird in dieser Abhandlung die religiöse, kulturelle und politische Symbolik einer Frau, an deren Wirkungs- und Verehrungsgeschichte abgelesen werden kann, was Menschen des Mittelalters von der Mutter des christlichen Messias erbaten, erwarteten und erhofften — an religiöser Sinngebung, an konkreten Hilfen in Not und Bedrängnis, an Fürsprache bei Gott. Die Auswahl der behandelten Themen sucht der Tatsache Rechnung zu tragen, daß Religion im Mittelalter eine ausnehmend soziale Angelegenheit war. Marienverehrung gab Gruppen, Städten und Regionen ein Gefühl der Zusammengehörigkeit; sie rechtfertigte den Herrschaftsanspruch der Reichen und unterstützte den Protest der Armen. Rituelles Handeln brachte gleichermaßen Probleme individueller Lebensführung und Angelegenheiten des gesellschaftlichen Lebens zur Anschauung und zur Sprache. Dem Buch liegt die Absicht zugrunde, an einem aussagekräftigen Paradigma mittelalterlicher Frömmigkeits- und Theologiegeschichte mit Hilfe

erzählerischer und bildlicher Mittel Religion, Kultur und Gesellschaft miteinander zu verbinden. Der Verleger drängte. Knapper werdende Zeit verkürzte den nächtlichen Schlaf und machte mich bisweilen zu einem müden, ungeselligen, unaufgeräumten Zeit- und Tischgenossen. Anfang September ist das Buch termingerecht erschienen. Es zählt 592 Seiten und enthält zahlreiche Abbildungen (und zugegebenermaßen auch Druckfehler).

Mit dem Rückblick auf die Zeit im Wissenschaftskolleg verbindet sich das Bewußtsein, auf dem Feld der Wissenschaft nicht nur gearbeitet, sondern auch geerntet zu haben. Gut, daß es Erinnerung gibt, die fest- und wachhält, was Zeit vergehen läßt: die abendlichen Konzerte im Haus des Wissenschaftskollegs, die schönen Künste, die ich in Museen und Konzertsälen, in Opern- und Schauspielhäusern der Stadt gesehen, gehört und genossen habe, die Debatte mit Kolleginnen und Kollegen über Probleme des Fachs, die freundschaftliche Geselligkeit im Kreis der Fellows, die selbst- und lautlose Hilfsbereitschaft jener, die in Bibliothek und Büro, in Küche und Werkstatt den lebens- und überlebensnotwendigen Alltag organisierten. Dank und nochmals Dank für eine ungewöhnliche, wunderbare Zeit !

Thomas D. Seeley

The Building of Biological Organization

Born 1952 in Pennsylvania. Studied Chemistry and Mathematics at Dartmouth College. Ph.D. in Biology, Harvard University, 1978. Junior Fellow in the Harvard Society of Fellows (1978/80), Assistant and Associate Professor at Yale University (1980/86). Since 1986, Professor of Biology, Cornell University. Major areas of scientific interest: the evolution and organization of insect societies, animal communication, and operations research. Books: *Honeybee Ecology: A Study of Adaptation in Social Life* (1985, Princeton University Press), and *The Wisdom of the Hive* (1995, Harvard University Press). — Address: Section of Neurobiology and Behavior, Mudd Hall, Cornell University, Ithaca, NY 14853, USA.

I came to the Wissenschaftskolleg with the hope of achieving two goals: to finish writing one book on the organization of honey bee colonies, and to begin writing a second book (with three other fellows — Scott Camazine, Jean Louis Deneubourg, and Nigel Franks) on the phenomenon of self-organization in animal groups. Despite being in Berlin only four months, complete success was achieved in the first project, and good progress was made in the second endeavour.

The book on the bees, titled *The Wisdom of the Hive*, summarizes what is known about how the thousands of individuals within a honey bee colony work together as a harmonious whole in gathering their food. This book, however, is not just about honey bees. Bees are simply aesthetically pleasing and easily studied animals whose sophisticated colonies vividly express an important puzzle in biology: What are the devices of social coordination, built by natural selection, that have enabled certain species to make the transition from independent organism to integrated society? The study of the honey bee colony, especially its food collection, has yielded what is probably the best understood example of cooperative group functioning outside the realm of human society. This example deepens our understanding of the mechanisms of cooperation in one species in particular and, by providing a solid baseline for comparative studies, helps us

understand the means of cooperation within animal societies in general. I feel the book will serve its purpose if readers can gain from it a sense of how a honey bee colony functions as a unit of biological organization.

I feel most grateful to the Wissenschaftskolleg for providing a haven for undisturbed concentration and long periods of searching thought, without which I could not have written the final, synthetic chapter of this book. In it I identify the main features of honey bee colony organization and place them in a larger context, by drawing comparisons between the inner workings of a bee colony and those of other kinds of functionally organized groups. These include multicellular organisms (groups of cells), colonies of marine invertebrates (groups of zooids), certain human organizations (groups of people), and multiprocessor computers (groups of electronic processors). All such highly cooperative groups share the basic problem of rationally allocating their members among various activities so that the more urgent needs of the ensemble are satisfied before the less urgent ones. They also share the problem of coordinating the actions of their members to achieve coherent patterns of activity. The solutions to these problems, however, vary greatly among the different kinds of integrated group. By comparing these solutions and reflecting on the functional significance of their similarities and differences, we deepen our understanding of the mechanisms that make close cooperation a reality.

With respect to the second project, the joint writing of a book on self-organization in animal groups, I enjoyed an incredibly fruitful exchange of ideas with my colleagues on the "collective intelligence team" as we strove to identify the fundamental concepts of self-organization as they apply to biological (living), as opposed to physical (non-living), systems. In both types of systems, one finds that groups of interacting subunits can build patterns (definite arrangements of the subunits in time or space) without any intervening external influence, such as a pre-existing pattern in the environment or a plan imposed by a leader. The puzzle is how patterns nevertheless arise. This mystery is especially strong in the case of certain insect societies, consisting of many thousands of individuals, which build extremely sophisticated nests and other structures in homogeneous environments and without anyone possessing synoptic knowledge of the building process. Somehow each individual contributes in the proper way to the collective enterprise despite the absence of supervision. Our book, titled *Building Biological Superstructures: Models of Self-Organization*, will provide the first coherent statement of the puzzle and will review what is currently known about its answer. These are issues that have never been clearly discussed in the biological literature, hence our group was faced with the challenge of delineating an essentially new avenue of scientific investigation. Without the cross-fertilization that is possible at the Wis-

senschaftskolleg, where one may engage in numerous, long, and hard discussions, our research group never would have achieved the progress that it did. With the basic conceptual framework assembled, our group is well on the way to completing its project.

As a final point, I wish to thank all those who made the stay so enjoyable for me and my family We deeply appreciate the cheerful and efficient assistance of the entire staff at the Wissenschaftskolleg. I feel especially grateful to Dr. Hans-Georg Lindenberg for his assistance with computers, Frau von Klitzing for her help in arranging schooling for one of my daughters, Frau Sanders for help in purchasing theatre tickets and many other kindnesses, and Frau Hund for her enthusiastic language classes. My family and I look back most fondly on our four months in Berlin. To all, we give thanks.

Wolfgang Streeck

Die soziale Dimension des europäischen Einigungsprozesses

Professor für Soziologie und industrielle Arbeitsbeziehungen, University of Wisconsin at Madison, seit 1988. Vorher Gastprofessuren in Florenz, Warwick, Mailand und Madrid. Zuletzt erschienene Bücher: *Governing Capitalist Economies: Performance and Control of Economic Sectors* (Hg. mit J. Rogers Hollingsworth und Philippe C. Schmitter), New York und Oxford: Oxford University Press, 1993; *Social Institutions and Economic Performance: Studies in Industrial Relations in Advanced Capitalist Economies,* London and Beverly Hills: Sage, 1992; *Public Interest and Market Pressures: Problems Posed By Europe 1992 (* mit G. Mayes und Wolfgang Hager), London: MacMillan, 1992; *Beyond Keynesianism: The Socio-Economics of Production and Employment* (Hg. mit Egon Matzner), London: Edward Elgar, 1991 (Taschenbuch 1994); *New Technology and Industrial Relations* (Hg. mit Richard Hyman), Oxford: Basil Blackwell, 1988. — Adresse: University of Wisconsin-Madison, Department of Sociology, 8116B Social Science Building, 1180, Observatory Drive, Madison, WI 53706, USA.

Im Rückblick wundere ich mich, warum ich mein Jahr am Wissenschaftskolleg nicht von vornherein als für mich ideale Gelegenheit begriffen habe, mich mit den seit meinem Weggang aus Deutschland im Jahre 1988 so gründlich veränderten Verhältnissen in Europa durch unmittelbare Anschauung vertraut zu machen. Ich kam mit einer Reihe alter Verpflichtungen an das Kolleg, die ich so schnell wie möglich erledigen wollte, um dann ein bereits angefangenes Buchmanuskript über die „soziale Dimension" des europäischen Einigungsprozesses abzuschließen und mich danach, so etwa im Sommer 1994, an die Vorbereitung einer vergleichenden Untersuchung über den Ursprung der Berufsbildungssysteme in verschiedenen westlichen Gesellschaften zu machen.

In Wirklichkeit kam alles ganz anders. Nach ein paar Monaten, während des dunklen Berliner Winters, hatte ich den bedrückenden Gedan-

ken, daß mein Leben vielleicht nie wieder in etwas anderem bestehen würde als im hastigen Abschluß alter Projekte, unterbrochen allein von gelegentlichem, ergebnislosem Grübeln über den Ursprung des endlosen Zustroms immer neuer alter Projekte. Jetzt, da das Jahr zu Ende ist, sehe ich das nicht mehr ganz so. Fast alle alten Projekte sind nun wirklich abgeschlossen, und eine Anzahl von Manuskripten, die längst beim Verlag hätten sein sollen, sind es jetzt tatsächlich. Man kann das auf zweierlei Weise sehen: daß ich heute nicht viel weiter bin, als ich vor einem Jahr hätte sein sollen — oder daß ich jetzt, nach meinem Jahr am Kolleg, wirklich tun kann, was ich tun sollte, wollte und will.

Im übrigen ist auch das noch zu pessimistisch. Zwar ist das Europa-Buch nicht fertiggeworden, aber es ist doch konzeptionell ein gutes Stück vorangekommen. Im Prinzip könnte ich jetzt mit dem Schreiben der Endfassung anfangen — und glaube eigentlich, dies auch zu Hause in Madison tun zu können, nun da der Schreibtisch endlich leer ist, oder doch so gut wie leer. (Vielleicht sollte man sich von vornherein bewußt darauf einstellen, ein Jahr wie das am Kolleg zum „Aufräumen" und „Saubermachen" zu nutzen — keine geringe Sache, wie ich nun weiß.) Mit dem Berufsbildungsprojekt, meinem liebsten, wird es allerdings wohl nie etwas werden.

Dafür gab es anderes. Durch intensive Beobachtung vor allem meiner europäischen Kollegen aus den geisteswissenschaftlichen Disziplinen habe ich mir Techniken zur Verteidigung meiner schöpferischen Muße gegen die Zumutungen schnell alt werdender neuer Verpflichtungen angeeignet, die hoffentlich auch in den USA funktionieren. Und trotz all der überfälligen Dinge, die schon lange keinen Aufschub mehr duldeten, habe ich ein bißchen bei den Kollegen aus der Biologie und Philosophie zugehört und, hoffentlich, etwas gelernt. In Diskussionen vor allem mit Sandra Mitchell ist mir klargeworden, wie viele wichtige Parallelen zwischen den Problemen internationaler Organisation und den Beziehungen zwischen verschiedenen wissenschaftlichen Disziplinen bestehen; hieraus soll irgendwann ein gemeinsamer Aufsatz werden.

Vor allem aber stellte sich während meines Jahres am Kolleg allmählich heraus, daß sein eigentliches Thema meine Wiederbegegnung mit Deutschland und Europa war. Als „Europeanist" an einer amerikanischen Universität hatte ich natürlich die europäischen Ereignisse verfolgt, und die Kontakte zu den hiesigen Kollegen waren nie abgerissen. (So konnte ich das Jahr am Wissenschaftskolleg auch dazu nutzen, einen Sonderband der *Politischen Vierteljahresschrift* zum Thema „Staat und Verbände" herauszugeben.) Aber das Atmosphärische kann man von außen nicht mitbekommen. Vielleicht fehlten mir auch meine Studenten. Jedenfalls ließ ich mich im Laufe des Jahres zunehmend darauf ein, Vorträge zu halten, die es mir erlaubten, Kontakte in Wissenschaft und Praxis wieder-

aufleben zu lassen und bekannte Orte neu zu sehen. Jetzt, beim Zusammenzählen, stelle ich fest, daß ich außerhalb des Kollegs in zehn Monaten nicht weniger als zwanzig Vorträge gehalten habe, davon sechs in Berlin, fünf im übrigen Deutschland, sieben in anderen europäischen Ländern und zwei in Nordamerika. Einige davon waren die üblichen „alten Verpflichtungen". Die meisten aber haben es mir ermöglicht, nicht nur meine Gedanken zu verschiedenen Themen weiterzuentwickeln, sondern auch an vielen Orten neue Eindrücke zu sammeln. Wozu das alles letztendlich gut sein wird, werde ich erst später wissen. Wer aber wie ich immer wieder auf die Funktionalität des Nicht-Funktionalen besteht, darf diese getrost auch einmal für sich selber reklamieren.

Richard C. Trexler

The Three Kings — Triumphal Image of Christian Rulers

Born in 1932 in Philadelphia PA, USA. Doctoral degree from Johann Wolfgang Goethe-Universität, Frankfurt am Main, 1964. Historian of Renaissance and Late Medieval History. 1964-66 Texas Western College, El Paso. 1966-68 Occidental College, Los Angeles. 1970-78 University of Illinois, Urbana. 1978 to the present: Professor at the State University of New York at Binghamton. Several times: Directeur Associé, EHESS, Paris. Summer Semester 1990: Guest Professor, University of Bielefeld. Important Publications: *Public Life in Renaissance Florence (Studies in Social Discontinuity.* Academic Press: New York, 1980, new edition: Cornell University Press, 1991); *The Christian at Prayer. An Illustrated Prayer Manual Attributed to Peter the Chanter (d.1197) (Medieval and Renaissance Texts and Studies:* Binghamton, 1987); *Naked Before the Father. The Renunciation of Francis of Assisi (Humana Civilitas,* vol. 9; Peter Lang: New York, 1989). —Address: Department of History, State University of New York at Binghamton, Binghamton, NY 13901, USA.

The past academic year 1993 —1994 has been most productive for me personally, and I am much indebted to the Kolleg for making it so. In what follows, I will sketch what I have done, then give some impressions about the makeup of this "class of '94," and finally some suggestions regarding the future of the Kolleg.

I arrived here having stated my intention to write a book on the evangelical magi, or the *heilige Drei Könige,* but in fact I first planned to complete another book: on homosexual behavior in the Americas at the time of the Iberian conquests. Both these books have indeed been finished and are now being considered by various presses. The first is provisionally entitled *The Journey of the Magi: a World History of a Western Story,* the second *Like a Woman: Gender Construction and Political Order at the Time of the European Counquest of the Americas.* Indeed, I was so successful in completing these books that, in the last two months at the Kolleg, I found

myself working on quite other projects, two small articles and a small book that I came here with no real hope of working on. These articles are relatively complete, the one re-editing and commenting on a Roman treatise *(incunabulum)* on homosexual behavior, dated ca. 1480, the other examining one aspect of the famous Pazzi Conspiracy in Florence, dated 1478: the so-called Florentine Synod said to have met to defend Lorenzo de' Medici against the attacks of Pope Sixtus IV. I have also made substantial progress on an English translation with apparatus of the *Hodoeporicon* of Ambrogio Traversari.

The book on the evangelical magi argues that for 1500 years whether in past theatre or in the fine arts, the image of the magi was the main triumphal image of Christian rulers, and until the sixteenth century was used by these rulers as a means of legitimating their own authority. European rulers played the magi in art and festival, and, essentially through *sacre rappresentazioni*, participated in the legitimating aura of the "Three Kings". The book begins with the Roman triumph, and continues until the present day: The last picture in this book rich in images is of a destroyed Cologne parading the relics of the Kings through the streets in 1948, attempting through this Journey of the Magi to reorganize the city and its political geography.

The book on native homosexual behavior as observed by Iberians in the early days of the Iberian Conquest is essentially about the characteristic American institution of the "berdache", biological males who, for their entire lives, dressed and acted like women in every imaginable way. The argument of this book is that the berdaches were an important part of the construction of what we would today call the state, which, I argue, consisted in the ability of big men to convert, through social construction, lesser men into "women". Introduced by two long chapters on the European sexual universe in 1492, the book continues with chapters on the berdache in military and diplomatic, religious, and daily life in what we would today call Latin America.

These labors have been much aided by my chance to collaborate with the fine group of scholars who have been my colleagues. Near the end of 1993, I presented one slice of my magi project to them in a Tuesday colloquium. Not only were the comments challenging. The subject of the Three Kings easily captures the visual imagination, with the result that, especially over the Holidays, several of these colleagues clipped out various articles and pictures from newspapers regarding the quasi-dramatic appearance of children and adults playing the magi across much of Europe on 5 or 6 January and sent me postcards of paintings or other representations of the magi. They were enough taken with my hypothesis that the third magus has strong feminine qualities in late medieval art that

they tested that hypothesis on all the images of the magi they subsequently encountered. Finally, my colleague Kurt Wölfel actually turned up a painting in Soest (Westphalia) in which, for all intents and purposes, the third king is dressed like a woman, with a woman's veil and crown, a picture I have now brought into the book itself. Thus the collaboration with colleagues has been invaluable to me. As a practical matter, this collaboration ended with several of these colleagues reading various chapters of the penultimate draft of the magi manuscript.

Collegiality is obviously a desideratum among such a relatively small group of scholars, and I feel that the rector generally maintained the right tone in encouraging the same: gentle and often subtle nudging for the fellows to participate in Kolleg events. This was facilitated by excellent lessons in the German language, so that, by the new calendar year, foreigners could participate in the general discourse if they wanted to. In general, the hard scientists chose not to learn the language of their hosts, leading to a colloquium at the end of the academic year in which a talk on the treatment of epileptic patients delivered in the host language was not attended by any of the hard scientists, including a medical doctor, because they did not know the language. I do not know how to deal with this problem other than through a genial polyglot, chosen annually for the purpose, who would take it upon him or herself to force these bridges.

Still on the matter of collegiality, it must be said that there were simply not enough women among the colleagues this year. Still, one must then go on to praise all those responsible for the amazing makeup for the coming year 1994— 95, in which one third of the fellows will be women. The hearts, and the heads, of the selectors are in the right places. Not only is this statistic unparalleled in the remainder of academic life in Germany, it exceeds the usual achievement in the United States and other countries as well. It seems to me that this is one area in which the Kolleg, if it repeats these numbers in subsequent years, can be a model for the rest of the country's academic life. The chance to serve as a model for academic advances in Germany is one the Kolleg obviously wants to cultivate in every way. I cannot congratulate it enough on this achievement.

Nor could I be more gratified by my associations with new friends here at the Kolleg. Like at the Institute for Advanced Study in Princeton when I was a member, I was again brought into everyday contact with people concerned with a broad range of different subjects, each of them challenging and inherently fascinating. The breath of fresh air, the enrichment these mixtures brings to the demands of specialization, have furnished some of the best moments of my life, and this year at the Kolleg is no exception. Suddenly, all sorts of things outside of one's purview matter, and we step out of our footnotes long enough to enrich and recharge our-

selves for the coming years without that stimulation. I will remember all of my colleagues vividly and with a warm smile.

Regarding the future of the Kolleg, the most pressing need for those in charge is to change Berlin's weather. If that indeed cannot be negotiated, the depression that can accompany this weather must simply be negotiated. On balance, this is all well and good, because the access to music and museums and theatre here has been phenomenal for me, and that is a strong calling card. Next, let me compliment the Kolleg for the various tours and other vehicles providing us access to the city. Indeed, I would say that the single most important accoutrement of my stay here were the piano and camera recitals that brightened many of our evenings in the Kolleg itself. So as for the future, do keep these institutions and access to them !

In considering this question of the future of the Kolleg, I would insist again on the precedence to be given to equalizing access to the Kolleg for women before turning to another area I feel strongly about. This is the library. The bottom line is this: The Kolleg is not presently a place where one can start or develop a research project, but only where one can finish something that has already been bibliographied. The reason for this is computer inadequacy at the national and regional levels. The talented library staff, and certainly its dedicated director Frau Bottomley, waste a great deal of time on the telephones determining whether book X is to be found in a local library, when all of this would be easily available to the scholar if a decent computer system was in place.

I would identify the retarded state of library computer facilities in the Republic, and specifically in and around Berlin, as the single most pressing retardant to scientific research nation-wide. In my view, the turtle-like movement of information is a central threat to the economic well-being of the country, not to mention to its scientific apparatus. The country needs a crisis conversion of its information resources to those of a modern state and a high-powered national commission to make information accessible to the public through computers. My unspecialized but strong conviction is that other countries are simply leaving Germany in the dust in regard to information retrieval.

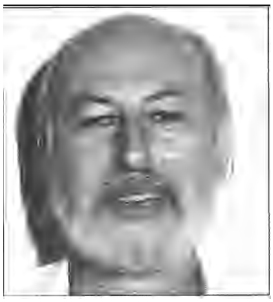
The central question for the Kolleg is whether there are ways in which — similar to the leadership role it can take regarding the presence of women in German academic life — it can serve as a model for efficient library utilization. Thus the Library's proposal to make library catalogues available to all fellows through Internet, besides its obvious utility for scholars, can also have a key public relations value for the Kolleg once it is in operation. Doubtless, no decisive breakthroughs can be made at the Kolleg level until the government has decided to modernize information services for the

general public in its libraries. But it should be possible for the Kolleg to show other, larger institutions the way by key innovations that visiting fellows would then carry back to their universities. Optimally, the visiting fellow should one day be able to sit at his or her computer terminal, generate bibliographies, determine the location of books, and order them.

I do not want to conclude this report without praising the quality of the permanent faculty and staff, from top to bottom and with only a rare exception. I have rarely seen a group of people who seem to associate with each other so warmly, and while I do hope that in the future the same modification of the gender order can be carried through in the administration as among the fellows, I do praise it lavishly as it is. Perhaps Frau Sanders and the library staff deserve individual praise, but the fact is that person after person is special, capable, and friendly. I have thoroughly enjoyed being here.

Richard I. Vane-Wright

Assessing Priorities for the Conservation of Biological Diversity



Born in Kent in 1942, Dick Vane-Wright's first real interest started seven years later to the day — a fascination for butterflies. After later childhood flirtations with model engineering, astronomy, chemistry and jazz trumpet, he entered the British Museum (Natural History) as a scientific assistant working on *nematocerous Diptera* (midges, gnats, mosquitoes, crane-flies). After graduating from University College London with first class honours in zoology in 1967, he returned to the Museum and his first love, to become head of the newly-formed Butterfly Section, with a staff of five and responsibility for curating the world's most important collection of these insects (2.6 million specimens). From 1967-1984, he worked on the classification and comparative biology of tropical butterflies, culminating in two major collaborative works, *Milkweed Butterflies* and *The Biology of Butterflies* (both with P. R. Ackery). From 1984-1990, he was Deputy Keeper of the Department of Entomology. In 1990 he turned to full time research, with increasing emphasis on conservation and biodiversity, but also revitalising previous work on the systematics and evolution of milkweed butterflies and their mimics (mainly in collaboration with Professor Michael Boppré, Freiburg). — Address: Biogeography and Conservation Laboratory & Department of Entomology, Natural History Museum, Cromwell Road, London SW7 5BD, GB.

A fellow biologist once told me of an experience he had on the remote Indian Ocean island of Diego Garcia. He had been studying the island's wild life and the surrounding coral reefs for several months before the arrival of three thousand marines. These military men had the task of building a five-kilometre runway for nuclear bombers. It took them just three months, at the end of which one proud bulldozer driver said to my friend, »Say Tony, what do you think of that — when we came here three months ago, there was nothing. Now look at it !«

As biologists, our reaction to such stupefying (even if unintentional) philistinism is one of horror — we laugh to hide our anguish. But to be effective, our emotions must be channelled. To help place the conservation of biological diversity on an equal and valid footing with other uses of the land and sea, we need a rational approach, especially to the problem of setting priorities. Thus the basic objective of my year at the Wissenschaftskolleg was simple enough: to be part of the "Schwerpunkt" entrusted with writing a book about methods for identifying priority areas for the conservation of biodiversity.

Two things I have learned in life are that I am a poor judge of character (I once gave £ 3800 to a double-glazing salesman) and to be wary of expectations. Despite this, my expectations for Berlin were high, dangerously high. The first month passed in a haze, battling with supermarket trolleys and a child who hid under a chair rather than be examined by a friendly school doctor; with a language I proved incapable of mastering; and trying to entertain the kids from time to time while my wife struggled with her M.A. In between I dreamed (with Bob Pressey) about new ideas we wanted to explore and discovered some of the peculiarities of *Windows, Word,* e-mail, FTP and the electronic network in the office building.

Having worked together on an initial rewrite of the book outline, Bob Pressey returned to Australia at the end of October, whereupon Chris Margules and Rüdiger Wehner arrived for a couple of days. Dreams suddenly turned to sterner reality. Had we started writing yet? No, not really. Who was in charge of the project? Nobody, really. What was to be done? It was agreed that, as I was the only member of the nine-strong »Schwerpunkt« to be at the Kolleg for the entire year, the only logical step was to make me, in effect, project manager.

With some dread I accepted, having hoped until then that we could proceed, like the original genesis of the whole thing, on a more or less anarchic basis. To be a natural leader is one thing; to be a leader among equals from whom you expect to gain more than give, is another. A few days later I issued a circular to all members of the group pointing out that new research was not the primary objective — we had to pull together, largely to harmonise what we already knew, and write a book. A bleak but realistic message in relation to my original (unrealistic) dreams of academic rebirth in a German seminary. Two weeks later my wife's father fell seriously ill, rallied, and then died. Around the beginning of December, the first of two (well hidden) disputes within the group erupted. By Christmas I felt rather desperate, with little personal achievement on the main project to show, grief and adjustment problems afflicting me and my family, wondering how I would cope with another seven months. So much for expectations.

After returning from a hectic Christmas and New Year visit to England, personal and professional fortunes improved. The coming and goings of »Schwerpunkt« contributors continued, and I began to live vicariously through their achievements as much as through my own. Although they all encountered difficulties of one sort or another, most made substantial progress on the basic work and also achieved growth in understanding through direct contact among members of the group and with other Fellows. Even so, in hindsight, to have quite so many contributors, with a complex pattern of arrival and departure, was not ideal (one effect of bringing the original project proposal forward a year). This was because, with each new departure and key arrival, ideas had to be re-rehearsed. This inevitably led to changes — a form of progress in itself — but it was obvious that some of the changes would have been different if the whole team had been together all the time, or if the sequence of arrivals and departures had been different. Even with our last arrival (the re-appearance of Bob Pressey at the beginning of June for his second session), further substantial changes were called for (and largely implemented or even re-introduced).

Another consequence of being `elected' co-ordinator for the group was the need to organise seminars and colloquia. It was clearly desirable to take advantage of the Kolleg facilities to run a major colloquium to provide peer group review of what we were proposing, ideally before it was too late to respond to any feedback or criticism generated. Early on we also realised that it would be helpful to get input, perhaps at the same colloquium, to our attempts to consider policy and implementation issues – the original fourth section of the book. All of our team were biologists, but this section of the book promised to stray into economics, sociology, politics and law. At the meeting with Rüdiger Weimer and Chris Margules at the end of the first month, it became apparent that we were really talking about two quite separate meetings. The first should be small, comprising mainly professional conservationists, to concentrate on ideas about policy and implementation. This was set for March, when six of the nine members of the "Schwerpunkt" were scheduled to be present, and by which time it was hoped we would be far enough ahead with the first three parts of the book to really benefit from this stimulus for the final section. The second meeting, a larger peer-group review exercise, was set for June.

Organising these two meetings, in the sense of trying to get suitable people to come to Berlin at appropriate times, became an almost inexplicably major factor in my life, right until June. Without the help of Andrea Friedrich and others, perhaps nothing would have been achieved. In the end, the March meeting, after seemingly endless uncertainty, took place on two days in April, with only two of the five people originally invited in attendance (although we did have an excellent, literally last minute

replacement for a third). However, it still had a major impact on our work*. The June meeting, for which I foolishly delayed final arrangements because of endless uncertainties over the first meeting, never reached the critical mass originally anticipated. It too, however, brought useful ideas (including some salutary suggestions from a German colleague), and introduced a number of valuable contacts. In contrast, a small series of `Biodiversity Forum' seminars, organised with input from other fellows (notably Erhard Denninger, Ashok Desai, Sandy Mitchell, Eduardo Rabossi, Gustav Ranis and Wolfgang Streeck), not surprisingly proved much easier to organise and probably had equally significant effects on our thinking as the two set pieces. The latter, given the hospitality of the Kolleg that was on offer, proved extraordinarily difficult to get together, more so than many far larger meetings that I have organised in the past. Biodiversity is a very trendy subject, and it showed in the number of people who turned down a free trip because of `Full-diary Syndrome'.

Over the last few weeks I tried to work as a more `regular' member of the group, preparing some examples for the chapters on taxonomic measures and applications, assembling a unique database on Danish butterflies (see below), and trying to get on with my own textual contributions to a number of chapters. Finally time ran out all too soon.

My own contribution to the project during the year has largely been to provide continuity and stability, through the colloquia, meetings, discussion, and commenting on the work of others. My own attempts at writing the single chapter on which it was agreed I would take the lead proved erratic, and at the end of July Chapter 1 still remains incomplete. Despite this difficulty, I am totally confident that my contributions will be finished in time, and that we will all continue in effective collaboration by electronic mail over the next seven months, up to the submission date to Oxford University Press on 28th February 1995. My overwhelming impression is that, without the coming together and commitment that working at the Kolleg has engendered, and without the differences of interpretation and understanding exposed amongst the various contributors, this work would never have been undertaken, let alone completed. The book is `in the bag', even though much still remains to be done.

During the year I also pursued a number of other activities, some directly, other only indirectly related to the main objective. Most were either pressing ghosts from the past, or unique opportunities presented during my time in Berlin.

* See reports, pp. 193 ff.

Outside the Kolleg, I gave only two lectures — perhaps an all-time low for me. Both took place in Bonn, at Museum Koenig. The first was a seminar for students, the second was a plenary lecture ("The shrinking ark: can systematics contribute to the preservation of biodiversity?") for the Museum's International Symposium on Biodiversity and Systematics in Tropical Ecosystems (2 —7 May). These meetings gave a useful opportunity to try out some ideas for a lecture I will give at the Missouri Botanical Garden, St Louis, at the end of September. I also made a number of valuable contacts — not least with environmental law specialist Lyle Glowka, who is working on an IUCN Explanatory Guide to the Convention on Biological Diversity. In the communication arena, I made interviews with two reporters, one together with Paul Williams for *Die Berliner Wochenpost,* the other for *Der Tagesspiegel,* both of which resulted in articles about the Wissenschaftskolleg Biodiversity Project.

Back in London, where Chris Humphries and I run the recently-founded Biogeography & Conservation Laboratory, grant applications seem an ever-present fact of life. During the year, I prepared one large and two small grant applications — the large one, made to the UK `Darwin Initiative for the Survival of Species', was successful. In addition, I received good news of an earlier EU-Human Capital and Mobility programme application, which will bring one of the researchers from Museum Koenig to London for two years, and this also required attention while still in Berlin.

An unexpected event was a request to be a member of a four-person advisory group to review the work of the Zoologist Museum, Copenhagen. An increasing number of museums now appreciate the value of such peer-group reviews, intended to look at the quality and relevance of their research, teaching and exhibition work, the quality, maintenance, accession policy and accessibility of their collections, and the efficiency of their administrative organisation. Having been on the receiving end of such visits for two decades in London, it was a challenge to see the problem from `the other side', especially as I was scheduled to return to London briefly at the end of June to make a presentation to yet another "visiting group" to my home institution. After wading through 10 ems of CVs and research plans for all the senior scientists at the Museum, I visited Copenhagen from 24 — 27th May. It proved to be a fascinating, wholly absorbing and quite exhausting visit, in which the gang of four interviewed over 30 senior staff, met with the Director on several occasions, and talked to groups of students, technicians and visiting scientists. On the last day we raced to produce a draft report, since finalised and presented to the Museum at the beginning of July.

This event, although apparently remote from my main objective, was meat and drink for a professional museum worker. It affected my own

presentation in London on 27/28 June, was a help in considering the needs of the *Museum für Naturkunde* (see next section), and quite incidentally produced one of the most interesting data sets suitable for assessing methods of priority areas analysis that I have yet seen — a doctoral thesis by Michael Stoltze completed only weeks before, which assembles historical data for Danish butterflies over two different time-spans, together with a purpose-designed survey attempting to record appropriate data for the whole of Denmark during 1990-1993. After returning from Copenhagen, Paul Williams and I made a 10 km grid for Denmark within WORLDMAP, and I entered 31,797 data points for 72 native Danish species from Stoltze's maps. I expect to see some of the results included in the book.

The *Museum für Naturkunde*, about 400 metres east of where the Wall used to cross Invalidenstrasse, holds one of the greatest zoological collections in the world. In my own field, I rank it second only to my home institution. I had hoped to make fairly regular visits during my stay, to work with the Lepidoptera collections, but apart from two brief occasions, this never materialised. However, after meeting Professor Hubert Markl (Konstanz) in June, I got drawn into discussions about the vacant position of director of zoology at the Museum. Professor Markl, together with Rüdiger Wehner and Professor Clas Naumann (Director of Museum Koenig), were all members of the search committee. I sent an open letter to Professor Markl indicating how important I thought the post was, and what a unique opportunity it represented not only to re-invigorate systematic zoology within Germany, but perhaps more widely in the rest of Europe. On 11th July I had a meeting with the Director of the Museum, the mineralogist Dieter Stöffler, a number of his staff and two members of the Humboldt University administration. Since then we have had further correspondence about the issues involved. I hope that these exchanges will prove helpful to the Museum and the search committee. Certainly, the brief behind-the-scenes tour given to me on 11th July proved intensely interesting, and gave me a much better understanding of the wide range of problems faced in trying to bring such a venerable institution up to date.

Another of the problems created by coming to Berlin a year earlier than originally planned was the number of projects in progress that I had been unable to finalise before arrival. Some of these came to haunt me, it seemed, and occupied much time at the computer or going through proofs. The most important involved reworking a long chapter on the classification of butterflies for a forthcoming double volume on Lepidoptera, to appear in the famous *Handbuch der Zoologie* series (published, incidentally, in Berlin). In the weeks following arrival I re-drafted, edited, faxed and e-mailed my two co-authors, as we struggled to respond both to

referees' comments and the need to reduce our over-long contribution to an acceptable size. The latter was achieved by excising a major portion, preparing that for a journal, and then re-organising the remainder, all in a race to meet the `final' deadline. As the day approached during November, we were still one critical review short. On the day I was poised to print the final version, the missing review arrived — 12 single-spaced pages of criticisms and suggestions ! We could not ignore this, so we missed the deadline and returned wearily to e-mails and faxes. Eventually the main text went to our long-suffering editor (Niels Peder Kristensen, Copenhagen) in February, and the journal article to *Entomologica Scandinavica* only in May. At the time of writing, we still do not know if the latter has been finally accepted !

The classification of butterflies and our ideas about the biology of these creatures that depend on a natural classification have formed the backbone of my career in zoology. World-wide, there are about twice as many species of butterflies as there are birds, and globally they are the best-known insect group of any real magnitude. For me and my colleagues in the Biogeography and Conservation Laboratory, the first steps in our ideas about priority areas analysis were set down in *Milkweed Butterflies* (P. R. Ackery & R. I. Vane-Wright, 1984). Currently I am applying the technology refined in Berlin to the two newly-funded projects, the Darwin Initiative work on the Western Ghats and the EU-HCM project, the first with strong, the latter with total emphasis on butterflies as biodiversity surrogates. In future I hope to continue further collaborative research on the classification and biology of butterflies, to resolve some of the many problems identified while preparing the *Handbuch* chapter. Thus the time taken up in reworking this material, although unwelcome and largely unexpected, was very much in the mainstream of my work.

Other publications worked on extensively while in Berlin include two papers on Moluccan butterflies, both in press and both redrafted to take account of referees' comments and new information. A final version of a paper by Dr Paul Eggleton and myself on principles of phylogenetics and their implications for comparative biology was agreed, and the proofs for the edited volume to which it belongs *(Phylogenetics and Ecology*, Eggleton and Vane-Wright, in press) were read at great speed — as were proofs for Forey, Humphries and Vane-Wright *(Systematics and Conservation Evaluation,* OUP, 1994). First proofs for a third book, edited by Phillip Ackery, Campbell Smith and myself *(Carcasson's African Butterflies,* CSIRO, in press) were also handled, together with re-writes of part of the introductory material for this annotated catalogue for all of the butterflies of Africa — 3600 species, one fifth of the world total. This last work was the greatest dinosaur of the lot, having been started literally two

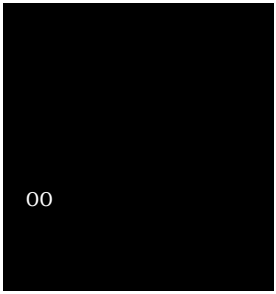
decades ago. We hope to see publication of this *magnum opus,* the first modern catalogue of all the butterflies of an entire zoogeographical realm, later this year.

The one and only personal butterfly project that I originally *planned* to pursue in Berlin, an Action Plan on milkweed butterflies for IUCN Species Survival Commission, did not progress by a single jot. Many years ago when struggling to understand Piaget and theories of cognitive development in childhood so I could design and write text for an exhibition on human biology, I was once told that my indecision was holding up production at Brazilian bauxite mines necessary to make Aluminium required for the exhibition cases. During my year at the Kolleg, I learned that South American economies were not to be counted alongside those of the miracle economies of Asia. A feeling of *déjà vu,* of personal failure, and of sympathy for the metal workers of Brazil sweeps over me again.

Despite this and other problems alluded to earlier, the year has been a mind-altering experience. Is there life after Berlin? I believe our project will make a significant impact on efforts to save more of life's diversity for the enrichment and benefit of future generations. A request to produce a working paper based on our methods for the German Department of Economic Co-operation and Development, in relation to their needs for assessing biodiversity projects to be funded by the World Bank's Global Environment Facility, gives me positive encouragement in this. In terms of my own existence, my life has undoubtedly been enriched — not only through contact with colleagues, other Fellows, visitors and many other new acquaintances, but also by the remarkable staff of the Kolleg, the remarkable institution that they run, and the curious and equally wonderful city of Berlin.

Antje Vollmer

Ein Jahr Abstinenz und Distanz



Antje Vollmer, geb. 1943, Politikerin und Publizistin. 1962-68 Studium der Evangelischen Theologie und Diplompädagogik in Berlin, Heidelberg, Paris, Tübingen. 1969-75 Assistentin an der Kirchlichen Hochschule, Dissertation in Kirchengeschichte, Gemeindepraxis in Berlin-Wedding. 1976-82 Tätigkeit in der ländlichen Bildungsarbeit. 1983 —90 Mitglied der Fraktion *Die Grünen* im Deutschen Bundestag, teilweise als Fraktionssprecherin. 1991— 94 überwiegend publizistische Tätigkeit für *TAZ, Spiegel, FAZ, Die Zeit, Stern* etc.; außerdem Tätigkeit in einer Epilepsieklinik in Bethel. — Adresse: Auf dem Kley 46, D-33619 Bielefeld

Hätte Wolf Lepenies mich nicht eines Tages angesprochen, ich hätte nicht einmal gewußt, daß es das gibt. Den Namen kannte ich wohl, allein mir fehlte die Vorstellung vom Wissenschaftskolleg. Und wenn ich mehr gewußt hätte, ich hätte nicht im Traum daran gedacht, daß ich eine Kandidatin dafür sein könnte. „Wir zwei sind in diesem Jahrgang die Paradiesvögel, die es immer geben muß", sagte Hans Magnus Enzensberger — freundlicherweise im freundschaftlichen Plural. „Gehen Sie dahin, es gibt einfach gar nichts Unangenehmes bei einem solchen Aufenthalt, wirklich nichts", sagten Gertrud Höhler und Adolf Muschg, die sich dann auch als ehemalige Fellows vorstellten.

So ermuntert und ermutigt war ich — neben den Darntons, die das Kolleg schon kannten — gleich die Erste des Jahrgangs 1993/94, die in der weitläufigen Villa Walther eintraf. Ich habe es nicht bereut. Ich habe das Jahr rundum genossen, auch wenn ich erst heute genau weiß, was so ein Out-of-Area-Aufenthalt sein kann — und was er noch sein könnte, wenn man vorher schon wüßte, was man erst vom Ende her weiß. Das ist so wie beim Hausbau, sagen die Leute.

Der größte Gewinn für mich war die Distanz. Ich war bisher gewohnt, Politik zu machen. Ich konnte jederzeit jede politische Situation in Sekundenschnelle analysieren und fand nichts dabei, alle Tage über Politik nachzudenken. Um von dieser Art von Besetzt-Sein durch die Tagespolitik freizukommen, war das Jahr im Kolleg Medizin, Askese (zu Luxusbedingungen, versteht sich) und eine nachdrücklich angebotene Schutzhaut. Am Anfang meiner Zeit in Berlin habe ich noch regelmäßig Artikel und

Kommentare für die *TAZ* geschrieben, auch zwei größere *Spiegel-Essays* und diverse Beiträge zu Büchern und Publikationen. Bücher zu lesen war ich zu dieser Zeit kaum noch gewohnt. Ich las sie hastig, um in Eile ihre Essenz zu erfassen, oder unter dem Druck, sie sofort wieder in schriftlicher Form zu „verarbeiten". Nach drei bis vier Monaten änderte sich das vollständig: Ich hörte auf, für die *TAZ* regelmäßig die Tagespolitik zu kommentieren. Ich nahm mir ein immer umfangreicher werdendes eigenes Buchprojekt vor und las dafür Grundsatzwerke. Ich schaffte es wieder, stundenlang, Seite für Seite, an einem Text zu bleiben.

Mein waghalsigstes Experiment: Ich bestellte alle Tageszeitungen ab. Selbst nach dem Mittagessen im Kolleg ging ich nach einiger Zeit der Entwöhnung oft achtlos an dem Zeitungsstapel vorbei. Und ich stellte fest: Es fehlte mir fast nichts ! In dem Sieb der Wirklichkeitserfassung, das mir verblieb, in den Tischgesprächen, Nachrichtensendungen, Fetzen von Alltagswahrnehmungen blieb alles Wesentliche doch hängen, das in diesen Monaten passiert ist. Wieviel Zeit und Konzentrationsfähigkeit aber hatte ich gewonnen !

Das Jahr war schon halb vorbei, da hatte ich einen gravierenden „Rückfall": Ich mußte mich um meine Zukunft kümmern, will sagen: um meine Kandidatur für den Deutschen Bundestag. Nicht nur ich kümmerte mich darum, sondern auch einige andere. Wie ich aus meinem Buchprojekt inzwischen genauer weiß, gibt es Sphären des Öffentlichen, in denen sich die Gewaltpotentiale besonders bündeln. Politik ist so ein Bereich verdichteter Rivalität. Ich hatte also im Umkreis dieser Kandidatur einige Schwierigkeiten, die mich alles in allem gut einen Monat Zeit und Kraft gekostet haben. Dabei war die Nicht-Erreichbarkeit durch die konsequente Schutzfunktion der Mitarbeiterinnen und Mitarbeiter des Kollegs für mich eine große Hilfe. Am Ende trat wieder Ruhe ein, ich war auf der hessischen Landesliste, ich war wieder einmal um eine Erfahrung reicher — ich konnte mich wieder meinen Büchern und meinem Thema zuwenden.

Mein Buchprojekt trägt den Arbeitstitel: *Europa und das Chaos.* Angefangen hatte ich mit der Frage, was sich eigentlich geändert hat, seit der Kalte Krieg und die große Blockkonfrontation nicht mehr die europäische Welt ordnete. Inzwischen ist daraus eine kurzgefaßte europäische Zivilisationsgeschichte über den Umgang mit der innergesellschaftlichen Aggressivität und den ständig vermehrten Chaospotentialen geworden. Die Stoffsammlung ist mir dabei etwas unmäßig geraten. Etwa die Hälfte habe ich bisher ausformuliert. Wann ich das Buch abschließen kann, ist derzeit noch nicht genau absehbar. Ich werde das Buch aber zu Ende schreiben.

Nicht zuletzt aus persönlichen Gründen habe ich meist zu Hause gearbeitet. Das war nicht immer einfach und gelegentlich habe ich die beneidet,

die ein vom Privatbereich und Kinderspiel getrenntes Arbeitszimmer hatten. Die Bibliothek hat mir in allem geholfen, so etwas habe ich bis dahin nicht gekannt. Von unschätzbarem Vorteil wurden für mich mehr und mehr die Mittagessen. Nicht nur, weil diese Form der Eßkultur ein mir bisher unbekannter Luxus war, sondern auch, weil ich fast zu jedem Thema, das ich behandeln wollte, dort einen Fachexperten traf: mit Herrn Denninger konnte ich über das römische Recht reden, mit Paul Zanker über die Gewalt in der römischen Arena, mit Herrn Desai über Mahatma Gandhi, mit Herrn Popovic über die Frage, welche Opferpraktikten der Islam kennt, mit Bob Darnton über die Rolle der Intellektuellen bei dem Prozeß der innergesellschaftlichen Dämonisierungen, mit Herrn Kocka über Historiker und ihre Parteilichkeit — und mit Hans Magnus Enzensberger über alles zusammen. Von diesen Gesprächen habe ich außerordentlich profitiert und geklaut, soviel ich nur konnte.

Zuletzt will ich die Vergnügungen nicht vergessen, die mir ein Zusammensein mit soviel kreativen Menschen verschafft hat: die Leidenschaft und Eleganz von Catharine MacKinnon, die Begeisterungsfähigkeit der Kämpfernatur von Jeffrey Masson, die feine poetische Ironie von Kurt Wölfel, die skurrile Genialität von Tony Grafton, die Noblesse und Musikalität der so liebenswerten Marta und György Kurtag und das Lächeln, mit dem sie federleicht an ihren Tisch huschten, um ihn als erste wieder zu verlassen. Das unermüdliche, kundige Informiertsein von Herrn Meyer-Kalkus über alle Theaterereignisse der Republik. Zu den Kunstwerken des Hauses zähle ich auch die Kochkunst und die gepflegte höfliche Zivilität aller Mitarbeiterinnen und Mitarbeiter, die bald den Stil aller Fellows untereinander prägte, was ihnen beiläufig und wie absichtslos gelang. Und nicht zuletzt: die rhetorischen Glitzerwerke des Rektors Wolf Lepenies — immer kurz, pointenreich und voller Finessen, belesen und selbstironisch — unnachahmlich.

Paul Williams

Character Diversity

Born 1959 in London. M.A., Ph.D. in Natural Sciences from the University of Cambridge (1985). Postdoctoral research at The Natural History Museum, London, on behaviour, distribution and systematics of bumble bees, working particularly in Kashmir. Since 1990 as part of the NHM Biogeography and Conservation group, working on measures of biodiversity, design of computer tools for biodiversity analysis, and in joint work at the Royal Botanic Gardens, Kew, on applying methods for choosing conservation areas for rainforest trees in the Amazon basin. — Address: Biogeography and Conservation Laboratory, The Natural History Museum, Cromwell Road, London SW7 5BD.

My part during five months at the Kolleg was primarily to write or co-ordinate two of the chapters for the "Schwerpunkt Biodiversität" book and to develop implementation of methods in the form of computer software. These chapters cover how to measure biodiversity (in the absence of an agreed definition) and how to make the most of existing data (in the absence of appropriate data).

Biodiversity has been seen as including all of the complexity of life on Earth. Consequently, no single measure can capture all its aspects, and conservationists must choose which aspect they most value for conservation. In the past, different interest groups have placed value at the level of either genes, species or ecosystems. Discussions with other Fellows — Sandra Mitchell and Gustav Ranis — and with visitors — Katrina Brown and Norman Myers — helped to clarify economic arguments for the value of biodiversity. This led to a focus on *option value* (analogous to a form of insurance) as a justification for conserving the variety of underlying characters (homologous attributes) of organisms. These characters can also be seen as depending on expressed genes and as governing interactions within ecosystems.

Unfortunately, character diversity cannot simply be added up in the field. Work in Berlin has helped to clarify how character diversity may be estimated from taxonomy for small groups of well-known organisms. Patterns of ownership of characters by species can be predicted from the

pattern of genealogical relationships among species using process models for how characters evolve.

Another problem for biodiversity accounting is that the total number of different organisms in even quite small patches of land cannot be counted directly. The narrowly circumscribed problem of estimating diversity for small, well-known groups of organisms may be far removed from considering the diversity of all life, but its solution can be used to justify other very different approaches, such as the use of land classifications, as less direct surrogates for character diversity. These more remote surrogates may be less precise, but should not be viewed as necessarily inferior. They may be both practical to measure as well as providing functional links among species that are important for the future viability or persistence of biodiversity.

During the year, I continued to develop computer software (WORLDMAP) as a test platform for ideas from the biodiversity group, enabling the consequences to be explored with real data. This involved changes in the algorithms for diversity measures. In addition, heuristic procedures to identify those near-minimum sets of areas required to account efficiently for all species (or other attributes) among areas were introduced and refined. These setwise procedures have to be able to cope with multiple as well as single representations of species, and to convey information on the irreplaceability or flexibility of area choices, in order to aid negotiations in land-use planning.

Inevitably, prior commitments to other papers and reports intruded and several new projects were initiated. I took the opportunity to spend time at the entomology department of the Humboldt museum, where collections include important type material of bees described by H. Friese and others. This contributed to long-term projects on a catalogue of bumble bees of the world (c. 3000 names), a revision of species concepts (c. 250 species), and an atlas of species distributions world-wide, all necessary foundation work for investigating questions in historical biogeography.

Predictably, the greatest difficulty at the Wissenschaftskolleg was in allocating time among Berlin, life at the Kolleg, and the biodiversity project. The year was a unique opportunity and has definitely been successful in harmonising the diversity of views within this group project.

Kurt Wölfel

Topographie eines „Weiten Feldes"

1927 in Würzburg geboren, dort in den Nachkriegsjahren Studium der Germanistik, Geschichte und Philosophie; Promotion 1951. Von 1955 bis 1958 als Lektor, dann *assistant lecturer* an den Universitäten Leicester und Birmingham. Habilitation für das Fach Neuere deutsche Literaturgeschichte an der Universität Würzburg 1963, nach Umhabilitation Diätendozentur in Göttingen. Von 1964 bis 1982 o. Professor an der Universität Erlangen-Nürnberg, seit 1982 an der Rheinischen Friedrich-Wilhelms-Universität Bonn. Emeritierung 1992. Seit 1966 Präsident der Jean Paul-Gesellschaft. Gastprofessuren: 1969/70 and der University of California, Riverside; 1982 an der University of Michigan, Ann Arbor. Publikationen über Themen zur Geschichte der Poesie und Poetik der Neuzeit. Forschungsschwerpunkt: 18. Jahrhundert. – Adresse: Germanistisches Seminar, Rheinische Friedrich-Wilhelms-Universität, Am Hof ld, D-53113 Bonn.

Ich kam am 7. Oktober 1993 in das Wissenschaftskolleg mit munteren Erwartungen und einigen guten Vorsätzen. Diese betrafen die Arbeiten, denen ich in den nächsten zehn Monaten nachgehen wollte, jene verdankten ihre Munterkeit dem Umstand, daß sie auf lauter Vergnüglichkeiten gerichtet waren. Ich wollte die Stadt Berlin, die mir flüchtig, und ihr brandenburgisches Umland, das mir gar nicht bekannt war, kennenlernen, alle Museen besuchen und einige davon mehrmals, wenigsten einmal in der Woche in ein Theater gehen und so oft es sich tun ließ in die Philharmonie. Auch hatte ich (denn ich bin in Bonn zu Hause) Vorstellungen im Kopf, wie man sie aus der Provinz in die Metropole mitbringt, nämlich von intellektuellen Eliten, die dort scharenweise anzutreffen sind, und einer abendlichen Geselligkeit, nach welcher man zu später Stunde dem Tagebuch anvertrauen würde: „Heute bei W. X und Y kennengelernt"; eventuell, als Flügelschlag des Selbstbehauptungswillens, mit dem Zusatz: „Y ein Flop." Und um auch noch den letzten Pflasterstein dieses hedonistisch angelegten Weges zur Hölle zu nennen: Ich freute mich darauf, ausgiebig Tischtennis zu spielen, von dessen eifriger Pflege im Kolleg man mir berichtet hatte.

Die guten Vorsätze hatte ich materialisiert bei mir in Gestalt von Büchern, Leitzordnern mit geordneten und Mappen mit ungeordneten Exzerpten, Notizen und Konzepten, mit denen ich in meinem Zimmer die heischend aufgesperrten Rachen der leeren Regale vorläufig in bescheidenem Umfang stopfen und dem Schreibtisch den Anschein von Bewohntheit geben konnte. Es war das Eingangskapital, mit dem ich wissenschaftlich zu spekulieren beabsichtigte, um es aufs ansehnlichste zu vermehren. Fürs erste wollte ich eine erweiterte, durchaus verbesserte Fassung einer Studie herstellen, die ich einige Jahre zuvor als Studienbuch für die Fernuniversität Hagen geschrieben hatte. Ich war darin von der hypothetischen Konstruktion einer „wirklich aufgeklärten" Tragödie ausgegangen und hatte am Ende festgestellt, daß sich innerhalb der Epoche der Aufklärung selbst eine solche Tragödie nur erst rudimentär realisierte. Ich wollte das weiter verfolgen: die Wirksamkeit einen „Trägheitsprinzips" in der Geschichte literarischer Formen, derart, daß sich epochentypische Formkonzepte nur mühsam, mit halben Erfolgs- und ganzen Mißerfolgserfahrungen, in die poetische Praxis umsetzen, und daß, was eine Epoche im Sinn hat, sich erst dort erfüllt, wo die Historiographie bereits deren Ende und Überholtheit feststellt.

Aus diesem Vorsatz ist, außer einigen Notizen, nichts geworden. Schuld daran trug ein zweites Arbeitsprojekt — und mit der Rede von ihm komme ich, über „Vorsätze" hinaus, zu dem, was ich während des Aufenthalts im Kolleg wirklich ge- und betrieben habe. Die Beschäftigung mit Kleists Aufsatz „Über das Marionettentheater" hatte mich zum Begriff der Grazie geführt, der in Deutschland nach der Mitte des 18. Jahrhunderts plötzlich und strahlend wie ein großer Komet am Begriffshimmel der ästhetischen Theorie und Praxis erschien. Grazie ist ein gemeineuropäischer Begriff, abendländisch im umfassenden Verstand, d. h. aus der griechischen (charis) und römischen (gratia) Antike herkommend, vom Christentum in der Bedeutung „Gnade" als Zentralbegriff übernommen, und als ästhetischer Terminus nicht nur in der Theorie der Künste, sondern auch in den Bildungsprogrammen seit der Renaissance von Bedeutung. Ich hatte also ein weites Feld vor mir. Wie weit es sich erstreckt und welche verschiedenen Regionen sich in ihm abgrenzen und zusammenschließen durch einen Ideenverkehr, der offen und untergründig, als Frei- und Schleichhandel, betrieben wird, kam mir allmählich in den Blick.

Die Perspektive meiner Arbeit war anfänglich von der Frage bestimmt, wie sich Kleists befremdlich provozierender, ins Zeichen der Paradoxie gestellter Text darstelle, wenn ich ihn als einen Endpunkt einer Begriffsgeschichte betrachte, die sich zum einen weit in die Vergangenheit zurückverfolgen läßt, zum anderen aber mit besonderer Intensität im letzten Halbjahrhundert vor Kleist sich abspielt. Kleists Aufsatz war eine Art Wider-

ruf, die Destruktion einer ästhetischen und darüber hinaus überhaupt menschlichen Positivität. Er zerriß die Verbindung von Mensch und Grazie und deklarierte, was bisher als — von den Göttern, Gott oder Natur verliehenes, von Kultur und Bildung erworbenes — Prärogativ des Menschen galt, als dessen Defizienz. Meine Hypothese war, eine Verkehrung von solcher Radikalität entspringe nicht als herkunftsloser Einfall dem Kopf des Autors wie Pallas Athene dem Zeus. So begann ich die Suche nach der Vorgeschichte des kleistschen Gedankens, d. h. nach Symptomen einer pathologischen An-, wenn nicht Hinfälligkeit der Grazie in den entsprechenden Diskursen des 18. Jahrhunderts. Diese Suche war erfolgreich, und sie hatte — angeregt vor allem durch eine Abhandlung meines Mitfellows Wolfgang Kemp („Die Beredsamkeit des Leibes") — eine Erweiterung meines Blickfeldes im Gefolge. Die Weise, wie sich im 18. Jahrhundert Grazie zu einer problematischen und labilen Größe entwickelte, ließ sich als Vorgang verstehen, der in sozialgeschichtlicher Perspektive besondere Plausibilität gewann. Soweit die Rede von Grazie seit der Renaissance die menschliche Person in ihrer Lebendigkeit betraf, war sie bezogen auf die aristokratisch-höfische Lebensform und Gesellschaftsstruktur. So hatte Grazie einen konkreten Ort in der Lebenswirklichkeit, ihre Theorie hatte eine gesellschaftliche Praxis vor Augen, berief sich auf ein Sein, nicht nur auf ein (ideales) Sollen: Castiglione konnte bestimmte Individuen als exemplarische Eigner von Grazie nennen. Das 18. Jahrhundert betreibt die Destruktion dieser sozialen Welt und der in ihr geltenden Lebensformen; doch es bewahrt ein Gutteil der mit diesen verbundenen ästhetischen Werte und nimmt sie in das neue Paradigma humaner Bildung bürgerlicher Observanz auf. Aber diese Werte verlieren dabei den lebenswirklichen Boden unter den Füßen. Statt in einer Gesellschaftsstruktur und ihrer Lebenspraxis zu gründen, wird Grazie nun eingeschlossen in die Idealität von Kunst, wird verinnerlicht zu einer Seelenbeschaffenheit, findet ihren Ort in geschichtsphilosophischer Utopie, oder wechselt aus der Bezogenheit auf Kultur über in die auf jenen gesellschaftlich ortlosen Naturbegriff, mit welchem Rousseaus Zivilisationskritik das Jahrhundert zu spekulieren gelehrt hat. Winckelmann, Wieland, Herder und Schiller sind vornehmlich die Autoren, an deren Schriften dieser Prozeß demonstrabel wird. Ich habe das Wissenschaftskolleg verlassen im Bewußtsein, die Topographie des „weiten Feldes" mit nach Hause zu tragen, dessen Erkundung ich während meiner Fellowschaft betrieben habe.

Unterbrochen wurde meine Beschäftigung mit dem Thema Grazie eine Weile durch eine extensive Herder-Lektüre: Ich war eingeladen worden, für den Katalog der Weimarer Ausstellung zum 250. Geburtstag Herders die Einleitung zu schreiben und dann auch den Vortrag zur Eröffnung der Ausstellung zu halten. Endlich gehört in diesen Bericht auch noch, daß ich

für ein großes Wörterbuch ästhetischer Grundbegriffe die Bearbeitung des Artikels „Interesse"/„Interessant"/ „interesselos" übernahm und begann, Material für diese begriffsgeschichtliche Darstellung zu sammeln.

Als Winckelmann seine *Geschichte der Kunst des Altertums* beendet hatte, schrieb er einem Freund: „Ich schlage das Buch zuweilen nur auf, um fröhlich zu sein, denn ich bin völlig mit mir zufrieden." Es verriete ein allzu starkes MaB an Selbstvertrauen, wollte ich die Hoffnung aussprechen, nach der Beendigung meiner Arbeit über die Grazie einen solchen Satz gleichfalls schreiben zu dürfen. Aber „um fröhlich zu sein" werde ich das Buch gewiß auch aufschlagen — fröhlich in der Erinnerung an das Jahr im Wissenschaftskolleg.

Paul Zanker

Antike Philosophen, Barbaren und ein bedrängendes Stadtbild

Geboren 1937 und aufgewachsen in Konstanz am Bodensee. Studium der Fächer Klassische Archäologie, Geschichte, Latein, Germanistik in München, Freiburg und Rom. Promotion 1962 in Freiburg. Reisestipendium des Deutschen Archäologischen Instituts, Habilitation 1967, Privatdozent in Freiburg. Von 1972-1976 o. Professor in Göttingen, seither in München. Gastprofessuren und Studienaufenthalte in Princeton (1982), Oxford (1985), New York (1986), American Academy in Rom (1989), Berkeley (1991). Hauptarbeitsgebiete: Kulturgeschichte des Hellenismus und der römischen Kaiserzeit. Für einen Nichtfachmann lesbare Bücher: *Augustus und die Macht der Bilder* (München 1987); *Die Trunkene Alte* (Frankfurt 1989); *Pompeji* (Mainz 1994). — Adresse: Archäologisches Institut der Universität München, Meiserstr. 10, D-80333 München.

In der publizierten und privaten Panegyrik über das Wissenschaftskolleg werden oft Metaphern wie Insel, Hügel / Berg und Garten benutzt. In der Tat ist etwas Unwirkliches an diesem wunderbaren Ort, an dem einem vieles von der täglichen Mühsal abgenommen und das Forschen in jeder Weise erleichtert wird und wo man von einer selbstverständlichen Atmosphäre der Freundlichkeit und Zugewandtheit umgeben ist.

Aber der Frieden täuscht, denn überall in dieser harmonischen Welt lauern „Anregungen", oft in solcher Qualität und Fülle, daß mir meine archäologischen Karten ständig durcheinander geraten sind. Es gab Fellows, denen ich fast zwanghaft immer wieder von meiner Arbeit erzählt habe, obwohl ich doch schon bald wußte, daß z. B. jede Unterhaltung mit Anthony Grafton nicht nur neue Bücherbestellungen zur Folge haben würde. Daß ich unter diesen Umständen wenigstens mein Buch über *Die Maske des Sokrates. Das Bild des Intellektuellen in der antiken Kunst* abgeschlossen habe, mit dem ich eigentlich schon nach vier Monaten hatte fertig sein wollen, grenzt ans Wunderbare.

Dem Manuskript lagen sechs Vorlesungen zugrunde, die ich 1991 in

Berkeley gehalten habe (Sather-Lectures). Ich versuchte darin, aus Bildnissen der antiken Dichter, Philosophen, Redner, Lehrer, Ärzte eine Geschichte des sozialen Image der „Intellektuellen" zu konstruieren. Der Weg führt vom 5. Jahrhundert v. Chr. bis in die Spätantike durch sehr unterschiedliche Gesellschaften, in denen die „Intellektuellen" ganz verschiedene Rollen spielten. In jedem Kapitel mußte also aufs Neue und auf knappstem Raum eine soziale Bühne rekonstruiert werden, auf der ich meine Philosophen und Dichter auftreten lassen und die Körper und Mienen ihrer Ehrenstatuen in ein Verhältnis zur „normalen" Selbstdarstellung der Zeitgenossen setzen konnte. Da die meisten einschlägigen Bildnisse nicht im Original, sondern nur in römischen Kopien überliefert sind, waren in den Fußnoten komplizierte quellenkritische Voraussetzungen zu klären.

Während meiner Zeit am Wissenschaftskolleg habe ich mich vor allem mit den beiden letzten Kapiteln des Buches beschäftigt. Leider sind sie dank der so günstigen Umstände am Wissenschaftskolleg auch fast doppelt so umfangreich wie die übrigen geworden ! In dem einen geht es um „Hadrians Bart und die Haare des Apuleius". Ich versuche darin zu zeigen, wie sich im Laufe der antoninischen Zeit in den führenden Schichten überall im Imperium Romanum eine neue Form der Selbststilisierung und der Bartmode ausbreiteten. Vorbild waren die Porträts der berühmten Griechen, deren Büsten seit langem zum festen Dekor vornehmer Häuser und Villen gehörte. Das neue „Intellektuellen-Gesicht" der Männer war nur eines der zahlreichen Erinnerungsrituale, mit deren Hilfe sich eine ganze Gesellschaft gegenseitig versicherte, im Besitz einer vollkommenen Kultur zu sein. Die damals entstehende „Kulturreligion" hatte eine klassische Tradition zum Gegenstand, die es so zuvor nie gegeben hatte. Vielmehr war in der Kaiserzeit entsprechend den Bedürfnissen eines multikulturellen Vielvölkerstaates etwas Neues entstanden, das dem einzelnen über die politische Loyalität hinaus Zugehörigkeit und Identität vermitteln konnte.

Das letzte Kapitel des Buches beschäftigt sich u. a. mit den Auswirkungen dieser, wie ich anhand archäologischer Quellen zeigen konnte, breite Schichten der Gesellschaft erfassenden „Kulturreligion" auf die Konzeption der frühen Christus-Ikonographie. Dem schon von den Apologeten des 3. Jahrhunderts verbreiteten Schlagwort vom Christentum als der wahren Philosophie entsprechen die Bilder von Christus als dem philosophischen Lehrer. Die Zusammenhänge gehen bis in Einzelheiten. So „erbt" der bärtige Christus z. B. die langen Haare der charismatischen Wanderphilosophen *(theios aner)*, und der strahlende jugendliche Heiland tritt in einer Maske auf, die sich aus den Heroen-Stilisierungen *der jeunesse dorée* der antoninischen und severischen Zeit ableiten läßt.

Ziemlich viel Zeit habe ich damit verbracht, die Eigenarten des antiken Intellektuellen-Bildes mit dem schwer zu fassenden Image des modernen Intellektuellen zu vergleichen, ohne daß am Ende mehr als ein paar Randbemerkungen in mein Manuskript eingegangen wären. Anlaß dazu war nicht zuletzt der Umstand, daß es am Wissenschaftskolleg gleich mehrere Fellows gab, die sich Gedanken über Situation und Aufgabe des modernen Intellektuellen machten — wie könnte es auch anders sein an einer solchen Institution. Die, die aus einem der ehemaligen sozialistischen Länder kamen, untersuchten dabei ihre eigene Rolle, fragten vor allem, warum die beim Umsturz aktiven Intellektuellen ihren Einfluß so schnell verloren hatten, und warum gerade in ihren Reihen das Denunziantentum so geblüht hatte. Es gab sogar eine Sitzung der sogenannten Mittwochsgesellschaft von Robert Darnton, in der wir einen Fellow unseres Jahrgangs, nachdem wir ihn zum typischen „freischwebenden" Intellektuellen erklärt hatten, stellvertretend für alle verhörten.

Eine zweite Arbeit hatte die Bildstereotypen des Barbaren in der griechischen und römischen Kunst zum Gegenstand. Darüber hielt ich einen der Abendvorträge im Kolleg. Ich hatte das Thema zunächst unter dem Aspekt „Feindbild" angegangen und als Vergleich zwischen den konträren Konstruktionen in Griechenland und Rom konzipiert. Gespräche im Kolleg, vor allem mit Antje Vollmer über die römische Arena, brachten mich dann dazu, den Blickwinkel zu erweitern und nach Entfaltung und Bewältigung von Gewalt unter verschiedenen politischen und gesellschaftlichen Bedingungen zu fragen. Wieviel Gewalt braucht eine streng organisierte Gesellschaft? Welcher Zusammenhang besteht zwischen den Stereotypen vom minderwertigen Barbaren — in dem man nur noch eine „materia vicendi" sah, wie es ein konstantinischer Hofdichter so treffend in der Sprache des Unmenschen formulierte — und den kontinuierlichen Hinrichtungen von Gefangenen in der Arena? Kann man aufgrund des Gebrauchs bestimmter Bildstereotypen etwas über die mentale Einstellung der Zeitgenossen aussagen?

Natürlich habe ich wie die meisten anderen Terminarbeiten und leider auch viele Gutachten schreiben müssen. Daneben sind aber wenigstens zwei Kolloquiumsbeiträge druckfertig abgegeben worden. Auch konnte und wollte ich mich den Einladungen Berliner Kollegen und Freunde nicht ganz entziehen; so habe ich sowohl an der Humboldt- wie an der Freien Universität Vorträge gehalten und bin mehrfach als „Festredner" aufgetreten. Im Alten Museum habe ich anläßlich der Neuaufstellung des Sarkophag Caffarelli über „Girlande und Fest" und anläßlich einer Ausstellung der Basler Bildhauerin Bettina Eichin über eine aus neun Bronzestatuen bestehende neue Musengruppe gesprochen.

Wer sich der Arbeit und den ständigen „Anregungen" auf dem Musen-

hügel entziehen und erholen will, steht vor der Wahl, in welche der beiden Städte Berlin er fahren soll. Das nach wie vor verhärtete Nebeneinander der beiden Gesellschaften, das bis in das Publikum der einzelnen Theater, Konzertsäle, Kneipen, Geschäfte hinein zu spüren ist, hat mir auf die Dauer ziemlich zugesetzt. An jeder Ecke überfällt einen hier die gegenwärtige deutsche Befindlichkeit in einer Ballung, die nach Entladung drängt, die zumindest den reizbaren Bewohner des selbstzufriedenen Südens der Republik nicht zur Ruhe kommen ließ. Mit wem man auch ins Gespräch kommt, sofort verhakt man sich im deutsch-deutschen Gestrüpp. Scheinbar harmlose Themen münden unweigerlich in Bekenntnissen grundsätzlicher Positionen, meist ohne Perspektiven.

Es gibt wahrscheinlich derzeit keine Stadt, die in vergleichbarer Weise von ihrer Geschichte gezeichnet wäre wie Berlin. Die Baukräne und die täglichen Debatten um die künftige Gestaltung der „Hauptstadt" halten einen in Atem, spielen sie sich doch vor dem Hintergrund der deutschen Katastrophe ab, deren Zeugen oder Spuren in den Architekturen überall gegenwärtig sind. Wo sonst gibt es solche Kontraste und Spannungen in den äußeren Erscheinungsbildern? Wenn man im Stadtbild einen besonders verdichteten Ausdruck von Wert- und Zielvorstellungen einer Gesellschaft sieht — ich habe unter diesem Aspekt antike Städte analysiert —, was bedeutet dann der hoffentlich noch zu verhindernde Wiederaufbau des Schlosses der Hohenzollern, um nur das spektakulärste Beispiel zu nennen? Was für Werte und Wunschvorstellungen werden das künftige Gesicht der Stadt prägen? Wer will welches Berlin und welche Republik? Überall wird man mit Umbruch und gigantischen Zukunftsaufgaben konfrontiert, aber wo sind die Köpfe, die die Diskussion mit weitem Horizont führen könnten? Überforderung der Strukturen und des Personals, die in beiden Teilen der Stadt ja oft noch von gestern stammen. Ein einzigartiges Schauspiel, mittendrin ein Archäologe als Zuschauer, der die Ruinen und Narben eher in Kassandrastimmung verschwinden sieht. Die Gründerzeit Wilhelms II. scheint näher als der Klassizismus Schinkels.

Auf visuelle Wahrnehmung trainiert, nahm ich manches vielleicht intensiver wahr als andere Bücherschreiber, so z. B. die unbeschreiblich scheußlichen Skulpturen, mit denen die „Villa Walther", in der die meisten Fellows wohnen, so überreich geschmückt ist. Wir haben viel über den abstrusen Geschmack des wilhelminischen Baumeisters und seines Bildhauers gelacht. Aber wenn man sich das Bildprogramm etwas näher ansieht und weiß, daß es kurz vor dem ersten Weltkrieg entstanden ist, kann einem das Lachen vergehen angesichts der mit Kinderliebe und Mutterfreuden verbundenen Kriegslüsternheit, die einen da in humanistischer Verbrämung und flankiert von den unsäglichsten lateinischen Zitaten entgegenschlägt. Mich haben die apotropäischen Masken, mit denen die Fas-

saden bestückt sind, buchstäblich bis ins Bett verfolgt, denn es gab in unserer Wohnung kein Fenster, aus dem man nicht auf diese Skulpturen schaute. Auf dem Balkon standen zwei klassische Grabaltäre und selbst, wenn man in den Garten ging, begrüßten einen zwei römische Panzerstatuen. Weniger als hundert Jahre früher hat Schinkel in Berlin und Potsdam seine wunderbar humanen, bescheidenen Villen für die königliche Familie gebaut. Die Stadt ist voll von diesen in ihrer Symbolhaltigkeit bedrängenden Gegensätzen.

Nie habe ich gleichzeitig in so vielen verschiedenartigen Welten gelebt und sie in ihrer Widersprüchlichkeit so unmittelbar empfunden wie in diesen Monaten. Als ich im Oktober nach Berlin kam, erwartete ich ein ruhiges Jahr ungestörter Arbeit und anregender Muße. Am Ende finde ich mich eher in einem Zustand der Unruhe und Verunsicherung wieder, der weit über die Einstellung zur eigener Arbeit, ihren Zielen und Standards hinausreicht. Aber nach den Worten unseres Rektors ist das ja ein Anzeichen dafür, daß die Kur im Wissenschaftskolleg angeschlagen hat und eine Besserung des intellektuellen Gesamtzustandes bevorsteht.

Seminarberichte

Examples

Symposium organized by Anthony Grafton and François Hartog 20-21 May 1994*

Participants: Ann Blair (University of California at Irvine), Anthony Grafton (Princeton University — Wissenschaftskolleg), François Hartog (Ecole des Hautes Etudes en Sçiences Sociales, Paris — Wissenschaftskolleg), Gérard Lenclud (Centre Nationale de la Recherche Scientifique, Paris), Paolo Mancosu (Technische Universität Berlin), Glenn W. Most (Universität Heidelberg), Wilfried Nippel (Humboldt-Universität zu Berlin), Wilhelm Schmidt-Biggemann (Freie Universität Berlin), Michael Werner (Ecole des Hautes Etudes en Sciences Sociales, Paris), Paul Zanker (Universität München — Wissenschaftskolleg).

Papers: Glenn Most: *The School of Athens;* Anthony Grafton: *Lorenzo Valla and Exemplar History;* François Hartog: *Exemple, Parallèle, Perfection;* Ann Blair: *The Example in Early Modern Europe: Between Natural History and History;* Paolo Mancosu: *The Use of Examples in Early Modern Mathematics: Descartes' Case;* Paul Zanker: *Venus als Vorbild;* Michael Werner: *La littérature nationale comme exemple (Gervinus);* Wilfried Nippel: *Historische Beispiele bei Max Weber;* Gérard Lenclud: *Exemple, Catégorisation: Le Potlatch Kwakiutl*

We report with pleasure on the results of the informal discussion of examples in (and outside) historiography, which the generous help of the Wissenschaftskolleg enabled us to hold. We intended from the start to launch an exploration rather than to arrive at fixed results. We invited ten speakers (ourselves included) representing several disciplines; nine gave papers, and one other guest (the Berlin philosopher W. Schmidt-Biggemann) served as a learned and competent discussant. The papers ranged widely in subject matter and style: some speakers accepted our invitation to be entirely informal, others presented written texts. Happily (and against our expectations) language did not prove a problem, although at least four were spoken at various times. Several fellows of the Wissenschaftskolleg also attended some talks and took part in the debates. The discussions were simultaneously engaged and serious: the general atmosphere was that of a small working group rather than a normal conference, and all the better for that.

The first three papers addressed themselves most directly to the question we began from: was there really a single classic doctrine or method for

* The seminar was sponsored by the *Otto-und-Martha-Fischbeck-Stiftung.*

using examples in historical writing? The very way the question is posed suggests the common answer: no. Though the rhetorical tradition did establish conventions for citing examples of heroic action or comparing exemplary cultures, it also contained many tensions and offered considerable room for criticism, innovation, and fruitful self-contradiction. In each case, analysis of primary sources (Raphael's *School of Athens* and the relevant passages from *Thucydides;* Valla's *Gesta Ferdinandi regis Aragonum;* Leroy's *Vicissitude* and Perrault's *Parallèle)* made the general arguments vivid and helped to keep the discussion focused on the central theme of *historia magistra vitae.*

The three papers next presented addressed the use of examples outside history: in natural philosophy and encyclopaedias, in mathematics, and in classical sculpture. We had hoped that these cases might offer instructive parallels or contrasts, and in fact they did so: particularly striking, and perhaps most unexpected, was the fact that Descartes' use of mathematical examples showed a strong resemblance to the phenomena observed in very different disciplines. The discussion of encyclopaedias showed with great clarity how enormous an amount of intellectual energy was expended in early modern Europe on collecting skyscraper-sized mounds of examples in every imaginable field of study, whether scientific or humanistic. And the last presentation of the day reminded us that the monumental use of examples continues to the present and showed most elegantly how apparently identical gestures and forms (in particular, those of the Cnidian Venus) take on radically new meanings (in funeral statues for Roman matrons — not to mention post-modern installations).

Finally, the last three papers raised the second question from which we began: what happened after historical thought ceased to pivot on reflection about examples? Did they, for example, simply change allegiance and become mere instances used to demonstrate theories in cultural history or social science? Investigation of three complex cases (the literary history of Gervinus; the comparative sociology of Weber; and the anthropological investigation of the Potlach) yielded no simple results, though it did produce three elegant and instructive lectures. Speakers and discussants agreed that we need to know more about how instances work in the social thought of the nineteenth and twentieth centuries — in particular, about how a single act, person or event comes to be seen as typical of an epoch or a whole civilisation. The final lecture, on the potlatch system, used Gallic wit to show exactly how a society can degenerate into an instance of a theory originally unconnected with it — to the point where the very society in question begins to imitate the version of itself provided by social scientists.

All participants seemed to enjoy both the formal discussions and the informal ones that went on over meals in and outside the Wissenschaftskolleg. All seemed to think that it would indeed be a good idea to stage a later and more formal version of this discussion, perhaps at the Potsdam Einstein-Forum.

Special thanks are owed to Frau A. Friedrich, whose intelligence and solicitude relieved us and the speakers of all practical worries, and to all the other members of the Kolleg's staff who collaborated to make the occasion a memorably pleasant one.

Priority Areas Analysis: Systematic Methods for the Conservation of Biodiversity

Biodiversity Group colloquium, 26-27 April 1994*

Participants: Erhard Denninger (University of Frankfurt / Main — Wissenschaftskolleg), Ashok Desai (Ministry of Finance, India — Wissenschaftskolleg), Gudrun Henne (Free University Berlin), Chris Humphries (Natural History Museum London — Wissenschaftskolleg), Chris Margules (CSIRO Wildlife & Ecology Canberra — Wissenschaftskolleg), Kathy MacKinnon (World Bank), Jeff McNeely (International Union for the Conservation of Nature and Natural Resources), Sandra Mitchell (University of California at San Diego — Wissenschaftskolleg), Beatrice Murray (United Nations Environment Programme HEMS Munich), Gustav Ranis (Yale University — Wissenschaftskolleg), Tony Rebelo (National Botanical Institute Claremont South Africa — Wissenschaftskolleg), Dick Vane-Wright (Natural History Museum London — Wissenschaftskolleg).

Objective

The Biodiversity Group had, as its main task during the academic year, formulation of a book on systematic approaches to the conservation of biological diversity (see `Representing Biodiversity' — pp. 226). Although based on the biological disciplines of ecology and systematics, the book is intended to be applied; if it is not influential with policy makers and planners, it will fail to achieve its goal.

The book will consist of four main sections: the challenge of representing biodiversity, data and measurement, designing networks of conservation areas, and implementation. The first three sections comprise the main scientific basis of the work, while the last section is intended to explore how these ideas can be put into practice. Involving ideas on implementation and management, together with political, economic, legal, sociological and educational issues. Preparation of this final section posed special problems for a team of biologists.

Although the multidisciplinary forum offered by the Kolleg undoubtedly helped the group to address these issues, it was felt there was need for input from leading specialists used to dealing with both theoretical and practical aspects of biological conservation. With this in mind, and

` The seminar was sponsored by the *Ot to-und-Martha-Fischbeck-Stiftung*.

following discussion with Rüdiger Wehner, it was agreed that a small colloquium be organised, involving a minimum of three specialists from leading non-government conservation organisations, together with available members of the Group. In addition, other Fellows of the Kolleg who had contributed to an informal series of seminars under the title `Biodiversity Forum' were also invited, together with a student from the Free University of Berlin studying legal aspects of the Convention on Biological Diversity.

The colloquium

Following informal discussions between members of the Biodiversity Group and the three NGO specialists on the morning of Tuesday 26th April, the afternoon of the first day was devoted to talks on data bases, biodiversity measurement and reserve network design, given by Chris Humphries, Chris Margules, Tony Rebelo and Dick Vane-Wright. An overview of the book and its proposed contents was thus presented, to raise questions about general objectives and, in particular, the function of the fourth section.

The second day commenced with Jeff McNeely, as spokesman for the three NGO specialists, providing a wide-ranging critique of the issues. In particular, concern was raised about the audience of the book — who was it aimed at? It had become apparent that the Biodiversity Group was trying to address two quite separate audiences. On the one hand it was necessary to marshal the scientific arguments in the most exacting way possible, to persuade academics and other conservation biologists that the systematic approach being advocated represented a scientifically sound advance on what had gone before. For this audience an academic treatise was both necessary and appropriate.

With respect to implementation, the view was strongly expressed that, however compelling the arguments and however well-structured the last part of the book might be, politicians and policy makers would simply ignore it. A different solution was needed. It was proposed that the Biodiversity Group prepare a second, short and accessible document, to outline and draw attention to the main treatise. This would in part represent an executive summary of the most important factors, but should also address the political and implementation issues as a set of policy guidelines. If this was accepted, it was felt that the fourth part of the main book should not deal with these matters explicitly, but present instead examples of real applications at different spatial scales, including analyses of sectoral interests, and finish with a section reviewing limitations and identifying needs for further research.

Dr McNeely went on to provide a detailed review of the Convention on

Biological Diversity, pointing out where the work of the Group was most relevant. Articles 6 (on the preparation of national plans and strategies), 7 (on the identification and monitoring of biodiversity) and 8 (on *in situ* conservation) were seen as particularly important. This offered additional insight to an analysis of the Convention made earlier by Eduardo Rabossi, at a `biodiversity forum' meeting, where he made a comparison of the biodiversity convention with the Convention on Human Rights. McNeely proposed that, following a succinct account of the values of biodiversity, the relationship of relevant convention articles to the subject matter of the book should be identified at the outset, using the Convention as a framework wherever possible.

A major feature of the biodiversity convention is its national basis - responsibility for the conservation and sustainable use of biological diversity is recognised as a sovereign right. From this it follows that the principal target audience for *Priority Areas Analysis* must be governmental. Some countries or states have already prepared comprehensive system plans (e.g. Indonesia, British Columbia), and it was suggested that it might be valuable to comment on these. However, in making any such comments, it was also stressed that it was very important to be wary of applying criticisms based on hindsight; new methods should be seen as a way of helping to do things better in future, not eroding or undermining existing decisions (it was readily agreed at this point that the book would not criticise previous plans — the main point was to raise the level of debate, in particular by moving into a new phase of quantitative analysis).

Thus motivation of governments was also identified as a crucial issue. To persuade governments that action on biodiversity is important, cost-benefit analyses are likely to carry most weight. Gustav Ranis pointed out that, because of this, convincing arguments about the value of biodiversity are vital. The book should try to link the advantages of biodiversity conservation to economic applications — and implications. It was observed that the Organisation for Economic and Cooperative Development (OECD), for example, invested in projects often adverse to biodiversity at a rate in excess of 300 billion dollars per year; at present the World Bank Global Environment Facility (acting as the provisional Financial Mechanism of the Convention) was intending to fund biodiversity projects at a significant, but nonetheless minute fraction of this rate (about 2-300 million dollars per year).

Ashok Desai commented that values ascribed to biodiversity were either akin to religious beliefs, or were utilitarian. If a definition of biodiversity could be agreed, then an efficient approach to maximising biodiversity protected by conservation measures would constitute a rational approach to utilitarian values. If by adopting systematic principles gov-

ernments could increase their chances of successful application to the GEF, this by itself would represent a good economic argument.

Despite the necessary focus at national level, one of the primary systematic ideas underpinning the work of the Biodiversity Group has always been to develop methods that are independent of scale. Thus the book could also be promoted as a set of tools of value to a wide variety of potential users (notably governments, but also local governments, NGOs and sectoral interests). Sandy Mitchell agreed that a "tool-box" metaphor was good, and proposed that it be linked, in addition to the hierarchy of goals and levels, to the different problems involved in the conservation of biological diversity in temperate and tropical regions, and terrestrial, marine and freshwater ecosystems. In terms of the balance of material to be included, more examples of tropical systems were needed, together with examples of freshwater and marine systems. All this would have to be linked to priority issues, the utilisation of limited resources, and how to make enlightened choices between multiple options. At a previous meeting of the Biodiversity Forum, Wolfgang Streeck had characterised some of these problems as the need to establish both system and subsytem goals, to permit some resolution of the conflicts which almost inevitably arise between different levels of responsibility.

People are thus another issue of great importance — they represent both the problem and the solution. People are crucial to conservation issues because they will determine what will be done. It was felt that some emphasis on local needs would make the book more interesting as well as more directly relevant. Related problems involving conflict resolution include planning and management in relation to science, and patterns of land ownership. Erhard Denninger and Gudrun Henne discussed links to existing and developing legislation, both at national (e.g. German) and international levels. The concept of genetic resources, enshrined within the Convention, poses particular problems for developing new legislative mechanisms to deal with the obligations entailed. Linking proposals in the book to legal issues was seen as desirable.

Beatrice Murray raised the question of `politically correct' phrasing. For example, even the word monitoring, so widely used in the biological and conservation literature, could have sinister or even subversive implications in some countries. Worse, the suggestion that certain areas might be "useless" for achieving specific goals, even when limited objectives were clearly spelled out, could have disastrous consequences in the hands of the biologically naive but politically devious. All land has some conservation value, or should be managed as sympathetically as possible, both with respect to ecosystem services, the intrinsic value of nature, and the rights and needs of people living on that land. In this context, within the EU for

example, the set-aside programme for `surplus' agricultural land could be seen as a golden opportunity for biodiversity — or a missed opportunity. Methods such as those under development by the Biodiversity Group could be applied to set-aside options with, potentially, considerable gains for biodiversity conservation.

Results

The two-day colloquium ranged over many ideas, possibilities, problems and examples, and led to a number of significant changes in the direction and style of the project. The most striking impact concerned the key issue of the meeting: the content of the concluding section ("part 4"). After the formal meeting closed, Dr MacKinnon stayed for a further day and entered detailed discussions about this final part. In the process, it was agreed that she would join the Group and become a co-author on two of the four chapters planned for this section of the book. Some basic ideas regarding content of the second, short document, and possibilities for its publication were also raised — but no decision could be made without negotiation with Oxford University Press during final stages of preparing the main text.

In conclusion, the colloquium achieved its goal, with a distinctive impact on subsequent development of the project. Members of the Biodiversity Group were very grateful for the input received, from both distinguished guests and other Fellows alike.

Biology, Economics and Politics in Biodiversity Conservation

Biodiversity Group colloquium
7— 8 June 1994*

Participants: Katrina Brown (Centre for Social & Economic Research on the Global Environment, University of East Anglia), Dan Faith (CSIRO Wildlife & Ecology Canberra — Wissenschaftskolleg), Werner Greuter (Berlin Botanical Garden), Gudrun Henne (Free University Berlin), Chris Humphries (Natural History Museum London — Wissenschaftskolleg), Ian Kitching (Natural History Museum London), Chris Margules (CSIRO Wildlife & Ecology Canberra — Wissenschaftskolleg), Norman Myers (Oxford), Bob Pressey (New South Wales National Parks & Wildlife Service — Wissenschaftskolleg), Tony Rebelo (National Botanical Institute South Africa — Wissenschaftskolleg), Josef Settele (UFZ Umweltforschungszentrum Leipzig-Halle), Campbell Smith (Natural History Museum London), Dick Vane-Wright (Natural History Museum London — Wissenschaftskolleg), Paul Williams (Natural History Museum London — Wissenschaftskolleg).

How can we determine where limited resources for *in situ* nature conservation be spent so as to preserve the maximum amount of biological diversity? This represented a basic question faced by the Biodiversity Group in preparing their book, *Priority Areas Analysis: Systematic Methods for Conserving Biodiversity*. In addition to biological criteria and analytical procedures, economic, political and other factors affecting implementation had to be considered. Having wrestled with these problems for eight months, the function of this colloquium was to provide peer group comment on the approaches and methods being advocated, in time for reconsideration or revision before submission of the final text.

Following arrivals on Monday 6th June, the meeting commenced with a special session of the regular Tuesday colloquium, entitled *The value of biodiversity — anthropocentrism to biocentrism? With* Erhard Denninger as moderator, four short presentations were given by Robert Darnton, Gustav Ranis, Elena Lazos Chavero (Mexico) and Norman Myers, followed by general discussion.

A wide range of views were expressed. Robert Darnton suggested a pressing need to put biodiversity problems into a wider context regarding the many supposed threats to the survival of humanity. Both macro- and

* The seminar was sponsored by the *Otto-und-Martha-Fischbeck-Stiftung*.

micro-economic issues were discussed. Gustav Ranis made the point that, unless a clear economic argument is made for the conservation of biological diversity, governments are not likely to be strongly supportive. Elena Lazos Chavero was particularly compelling regarding the link between respect for local human culture, human rights, self- determination and use of biological diversity. The sheer urgency of the issues involved was emphasised by Norman Myers: in terms of human lifetimes and expectations for succeeding generations, biodepletion is irreversible. Dr Myers was also emphatic that it was essential to approach the problem by design rather than by default — but if design was to be effective, we had at most only two or three decades left to achieve anything worthwhile.

Commencing in the afternoon and occupying most of the second day, members of the Biodiversity Group delivered a series of papers outlining the content and thinking behind the 14 chapters planned for the book:

- R. I. Vane-Wright: "An Overview of the Wissenschaftskolleg Biodiversity Project - Origins and Goals"
- C. R. Margules: "The Challenge"
- R. L. Pressey: "Progress to Date"
- P. H. Williams: "Choosing Data and using Existing Sources of Data
- C. R. Margules: "Collecting New Data"
- P. H. Williams: "Measuring Character Diversity Using Taxonomic Surrogates"
- D. P. Faith: "Measuring Character Diversity Using Environmental Surrogates"
- A. G. Rebelo: "Selection Procedures"
- C. J. Humphries: "Complementarity and Efficiency"
- A. G. Rebelo: "Irreplaceability and Flexibility"
- R. L. Pressey: "Setting Priorities within the Network"
- C. J. Humphries: "Applications"
- C. R. Margules & D. P. Faith: "Development"
- R. I. Vane-Wright: "Communicating to Decision- and Policy-Makers"

In addition, Paul Williams demonstrated *Worldmap* software, and Dan Faith demonstrated the *Diversity* package.

The concluding session, on the role of priority-areas analysis, was held as a round-table discussion, with Bob Pressey as moderator. The invited guests, notably Katrina Brown, Werner Greuter, Norman Myers and Josef Settele, were asked to comment on what they had heard. The following reflects some of the many points raised.

The technology described, while in some cases not sophisticated enough, seems in other cases needlessly complex. In the Philippines, where land conversion has approached or even reached 100 % on many islands, the only sensible decision would be to safeguard all of the remaining natural and semi-natural areas. There was also a risk of the approach being too static. For example, in Germany there are some 5000 protected areas so that, with this degree of fragmentation, selected or target species are continually under threat; a dynamic approach to management is needed to ensure their survival. (With regard to this particular point, the procedures advocated in *Priority Areas Analysis* include the need for adequate monitoring, not only to keep track of what is happening and to measure success or failure, but also precisely because planning is seen as a dynamic, cyclical process, which requires continual updating of the information base and iteration and reiteration of the analyses.)

A cultural divide was noted — the Biodiversity Group was firmly rooted in an anglophone tradition regarding conservation problems, and perhaps needed to be more aware of ideas generated by non-anglophone workers. Equally, the methods advocated by the Group ought to be made more widely available to non-English speaking scientists, amongst whom many of the ideas were perhaps little known or poorly understood. The possibility of a summary version of the book made available in other languages was briefly discussed.

Concern was also voiced that, by advocating the use of environmental variables *in lieu* of biotic data, there was a risk of seeing this as a panacea — when what was really required was a quantum leap in the quality and quantity of data on species and their natural occurrence. While it was reasonable to emphasise the responsibility that conservation biologists, including ecologists and systematists, had to the rest of society, society should also accept responsibilities towards such professional groups. More research was needed on organismic biology, autecology and land management for conservation, and these needs should be communicated to decision-makers. Appropriate institution building was needed in developed nations as much as in developing nations, especially if scientists in developed countries were to have any real opportunity to assemble the data needed for effective action, and help transfer technological skills in conservation evaluation and management to scientists in developing countries.

Another element considered missing from the presentations was a general sense of urgency and the recognition of the highest priorities. Options are closing fast, and it could be argued that there is only about a decade left for effective action. Where was the sense of this? Wrong priorities are still being pursued by conservationists, such as spending $5 million on the

conservation of white-tailed deer (essentially a pernicious pest in biodiversity terms). On the other hand, real high priorities recognised long ago, such as Madagascar, or the peat bogs of Scotland, are often forgotten, with more or less disastrous results. A suggestion was made that, to avoid losing sight of the main priorities, the Group should try to identify no more than 100 priorities at the global level — perhaps 50 top rank and 50 second rank. These should integrate degrees of threat with biological criteria, perhaps focusing on forests and coral reefs, where most diversity is to be found. Such priorities should also take into account political realities affecting the possibilities for success. (With respect to these last three points, the objective of the Group has always been to develop basic tools or methods for assessing priorities, not actual priorities *per se;* within the terms of the Convention, setting actual priorities for action is primarily a function and prerogative of national governments.)

Insufficient connection with other disciplines was noted. The approach of the Group appeared linear, and this should be stated at the outset. The need to integrate ideas behind priority areas analysis with other disciplines could be highlighted in the concluding chapter on research and development. Although option value is important for the future, the book should be clear that biodiversity has multiple values, including the current functional values of existing ecosystems. Policy needs wider attention. The general role of environmental economics for achieving success needs to be stressed. Issues such as opportunity and incremental costs in relation to conservation priorities ought to be spelled out. With respect to providing aid or similar forms of investment, as already noted, the likelihood of achieving success, as demonstrated for example by previous commitment to conservation initiatives, is also very important. In this context, evidence of local action is a critical factor, as is the degree to which initiatives are integrated into different sectors of the economy.

Regarding analysis at different levels, attempts to select priorities globally through the World Bank's GEF have met and will continue to meet with opposition by national governments. Operation at the national level, perhaps through biodiversity institutes, seems more manageable. But increasing emphasis should be placed at the local level, where the mobilisation of different sectors of society to take account of and plan for biodiversity conservation would represent the most potent route to success. If the priority areas approach could be linked to geographical information systems operating at the district level, this would be particularly helpful.

In conclusion, the wide range of opinions and comments underlined the Biodiversity Group's own view that the methods they are developing are necessary but by no means sufficient for the effective conservation of biological diversity. One of the objectives has been to develop the methods of

priority areas analysis, based on biological principles, to the point where biological factors can be integrated on an equal basis with other elements of the socio-economic matrix. Many of the comments received represented no more than mild frustration that this integration has not proceeded further. The views expressed will undoubtedly help set the agenda for future work.

Aufsätze

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Teil I

Research Group
"Biodiversity Reserve Selection Methods"
Daniel P. Faith, Kevin J. Gaston, Chris J. Humphries,
Chris R. Margules, Anthony O. Nicholls,
Robert L. Pressey, Anthony G. Rebelo,
Richard I. Vane-Wright, Paul H. Williams

The Wissenschaftskolleg Biodiversity Project: Priority Areas Analysis

The objective of this *Schwerpunkt,* originally set up as a team to explore reserve selection methodology, was to produce a book on methods for assessing priorities for the conservation of biological diversity. This was primarily in response to needs identified in the Convention on Biological Diversity, signed by 142 countries represented at the UNCED meeting held at Rio de Janeiro, June 1992.

Over the academic year, nine biologists attended the Kolleg to work on this project (six Fellows: Chris Humphries, London; Chris Margules, Canberra; Tony Rebelo, Cape Town; Bob Pressey, Sydney; Dick Vane-Wright, London; and Paul Williams, London; and three Guests of the Rektor: Dan Faith, Canberra; Kevin Gaston, London; and Nick Nicholls, Canberra). All will be contributors to the eventual volume, *Priority Areas Analysis,* scheduled to go to Oxford University Press in early 1995. Following earlier discussions and her brief visit to the Kolleg in April, Dr Kathy MacKinnon, biodiversity specialist at the World Bank, will also be a co-author, as will another of the Fellows, Dr Sandra Mitchell, San Diego.

Following the April colloquium`, when it became apparent that the book would inevitably be heavy on theoretical analysis and technical procedures, it was agreed to produce a slim additional booklet which would review the main conclusions and address policy and implementation issues, in a style accessible to politicians and environmental managers. This was recognised as necessary because, from the outset, the project was not conceived simply as an academic exercise, but also as an attempt to affect policy and decision-making at national and international levels.

The genesis of the biodiversity project can be dated with unusual precision. During the 4th World Congress on National Parks and Protected Areas, held in Caracas in February 1992, the four Fellows who will also act as editors of the book (Humphries, Margules, Pressey and Vane-Wright) first met as a group. There, with the prospect of the UNCED `Earth Summit' to be held at Rio only four months away, they discussed the desirability of a book about methods for the systematic assessment of

* See pp. 193 ff. in this volume

priorities for the conservation of biodiversity, on global, national and local scales. They felt strongly that, before worrying more about *how* to manage areas for biodiversity protection, it would be wise to devote greater time and effort to deciding *where* such areas should (or could most effectively or economically) be located. However, given that all four worked for institutes which had no sabbatical or long-term study-leave system, the idea was promptly abandoned as a mere pipe-dream. How would it be possible to get together for six months or a year to undertake such a task?

By some process akin to ESP (which remains to be explained), Professor Rüdiger Wehner contacted Humphries and Vane-Wright in London during March to enquire if they had any major book projects they wished to undertake, involving team work with colleagues from overseas. Amazed, they replied yes, they did have exactly such a project in mind, but it had been shelved for lack of opportunity. The draft outline was prepared, CVs collated, and bundles of paper flowed between London, Canberra, Sydney, Cape Town, Zürich and Berlin.

Two months later, as the managers and politicians struggled and (with the exception of President Bush) eventually agreed to sign the biodiversity convention, Chris Humphries, together with the palaeontologist Peter Forey, ran a symposium in London entitled "Systematics & Conservation Evaluation". Seven members of the proposed project team were there to give papers, and Professor Wehner flew to London to meet and talk with them. Following this, Bob Pressey stayed on for another four weeks to take the lead in writing up some initial ideas, first discussed in Caracas, about basic principles for priority areas analysis.

During the autumn, when the Kolleg and team members had more or less reached agreement, a formal proposal was sent to Oxford University Press to undertake eventual publication of the book. At about this time the decision was also made to bring the project forward a year, to 1993-94, rather than 1994-95 as originally planned. The urgency of the need for such a book had been recognised. However, being at the Kolleg a year earlier, although desirable in terms of timeliness, created difficulties with regard to availability, completion of previous work, and lack of readiness with new data and software. Nevertheless, these problems were largely overcome and by the end of July 1994 all the major features of the book were in place, leaving the authors and editors confident of finalising the manuscript on the agreed date in early 1995.

Two papers are presented separately below, one historical, one current. The first, which appeared in *TREE (Trends in Ecology & Evolution)* in 1993, represents the article on which Bob Pressey took the lead in London in June 1992. The second paper is adapted from a draft of Chapter 2 of the

book, and gives an overview of the project with additional emphases and clarification beyond the *TREE* paper. To give these two papers some context, a brief introduction to the concepts and values of biological diversity follows, together with a list of chapter titles and contributing authors for the book.

Concepts and Values of Biological Diversity

The biological conservation movement has steadily shifted focus from saving certain charismatic animals from extinction, or special wilderness areas from destruction, to concern for an ever wider range of rare and threatened species or habitats. Over the last two decades an apparently final shift has occurred, to concern for biological diversity in general, including the whole of genetic, phenotypic, taxonomic and ecosystem variety. Biodiversity is thus a complex concept covering the whole range of life patterns and processes, including composition, structure, and function. No simple definition or single measure of biological diversity can be all encompassing. In practice, the number of species found in an area (biome or ecosystem) has been the most widely used first approximation. This is reasonable in that the main boundaries of most genetic recombination systems (`gene pools') are set by species limits, and populations of different species are widely viewed as the fundamental units of plant and animal communities, or even of the evolutionary process.

The values attributed to biological diversity can be divided into two general classes: material values and existence values. The former include all sorts of biological products of direct use to us, such as timber, fibres, genes for biotechnology, drugs and food, or biological processes of indirect use, such as natural control agents to counteract pests, fertile soils, and ecosystem services (e.g. clean air, fresh water, nutrient cycling). Existence values concern ethical, spiritual and cultural factors such as human rights (including rights of local people, and of future generations), knowledge, aesthetics, respect for creation, respect for nature, and respect for cultural diversity. Activities like horticulture and ecotourism span the two classes, providing financial gain for those able to satisfy the desires of other humans for aesthetically pleasing living objects, or the excitement of seeing wildlife in natural surroundings.

To attempt to put monetary values on biological elements or processes *in isolation* is largely to miss the point, or even strip them of their true significance, which is essentially context-dependent. However, the annual incomes, profits and other revenues derived from the harvesting of native timber or wild fish stocks for example, or the trade value of life saving

drugs derived from natural products, or the importance of fertile soils for agriculture, can hardly be denied or ignored. A major problem occurs once we are forced to atomise nature, and consider trade-offs between separate elements and other interests. How do we calculate the true cost of damming a single river for hydro-electric power, which we estimate will increase gross national product by 1 % over the next 25 years, if we believe it will cause the extinction of one or more unique forms of life? This economic conundrum is exacerbated by the uncomfortable fact that, after more than 200 years of research, taxonomists have yet to come up with a comprehensive catalogue of all the living things found at any one place, let alone across the entire planet, while ecologists have almost no idea about how much, or which elements of biodiversity are crucial for sustaining human life, ensuring spiritual well-being, or underpinning sustainable economic growth.

Included within human value systems are sectoral interests which, although dependent on the maintenance or even strengthening of certain elements of biodiversity, may nonetheless come into conflict. Others are general, indicating potential value in all elements, or the totality of biological diversity. Of particular interest in this context, for the development of a general, cross-sectoral approach to biodiversity and its conservation, is the notion of *option value.* This signifies the largely unknown but potential future value of each gene, species or ecosystem. The ultimate value of biological diversity may lie in its importance for the ability of future human generations to adapt to ever changing conditions for life on Earth. By saving as many different species' `blueprints' as we can now, we may best anticipate the needs of future generations to undertake all manner and kinds of ecological engineering, including the restoration of degraded lands and reconstruction of lost ecosystems, which may yet prove to be necessary for our own survival.

The total option value of a set of species depends in part on the disparity of the characteristics found amongst its members. In making choices for *in situ* conservation (the only practical approach for protecting most of biological diversity, and the only approach that addresses current quality of life and ecosystem service needs), it would be rational to maximise the number of species characteristics supported by a set of conservation areas. In so doing we seek to guard, as much as possible, against the alarming trend of biological impoverishment. This is the central issue which the Biodiversity Group, faced with current levels of taxonomic and ecological ignorance, has seen as a rational and immensely worthwhile challenge.

Priority Areas Analysis: Systematic Methods for Conserving Biodiversity*

Edited by C. J. Humphries, C. R. Margules, R. L. Pressey & R. I. Vane-Wright; with contributions from D. P. Faith, K. J. Gaston, C. J. Humphries, K. MacKinnon, C. R. Margules, S. D. Mitchell, A. O. Nicholls, A. G. Rebelo, R. L. Pressey, R. I. Vane-Wright & P.H. Williams Oxford University Press 1995.

* Table of Contents of the book that resulted from our stay at the Wissenschaftskolleg

R. L. Pressey, C. J. Humphries, C. R. Margules,
R. I. Vane-Wright and P. H. Williams

Beyond Opportunism: Key Principles For Systematic Reserve Selection*

The intention and practice of conservation reserve selection are different. A major reason for establishing systems of conservation reserves is to sustain biological diversity. This involves protecting examples of as many natural features, e.g. species, communities or environments, as possible. In reality, however, new reserves have rarely been dedicated for their representation of features. Furthermore, the opportunism that has characterized the development of reserve systems can actually jeopardize the representation of all features in reserves through the inefficient allocation of limited resources. More systematic approaches are essential if reserves are to play their role in protecting biodiversity. Some basic principles for conservation planning are emerging from recent systematic procedures for reserve selection. These principles will help to link intention and practice.

The conservation of biodiversity is a major challenge for the forthcoming decades and a wide array of approaches will be necessary to address it. These include research, education, *ex situ* collections, economic incentives and the establishment of protected areasl. Of this variety of approaches, *in situ* protection will play the most crucial role2,3.

The term `reserves' is used here to describe areas under a range of *in situ* protection measures, from wilderness to managed extraction of resources for commerce or subsistence. Determining the boundaries of reserves has two aspects: design and location. Design criteria such as size, shape and connectedness have been widely discussed. The location of reserves, or reserve selection, is currently receiving increased attention and is the concern of this article.

Reserves are most likely to fulfil their critical role in conserving biodiversity if reserve systems become as representative as possible; that is, if they contain examples of as many elements of biodiversity as possible.

* Slightly revised version of a paper first published in *Trends in Ecology and Evolution,* 8 (1993) 124-128

This will partly depend on how well biodiversity can be measured for the purposes of conservation planning (see box at the end). It also depends heavily on how available data are used to make decisions on reservation. Representative reserve systems must be achieved with economic resources which are only a tiny fraction of those devoted to activities destructive of nature[3]. Moreover, the options for reservation are diminishing steadily as intact habitats are reduced and competition with alternative land uses increases.

New reserves must therefore be selected carefully so that limited resources are deployed most effectively. More importantly, reserve selection must do more than react to threats: land must be allocated to conservation at least as assertively as to competing uses, and conflicts must be resolved explicitly. These needs will not be met by the widely-used opportunistic approaches that have led to very uneven representation of natural features in reserve systems[15] and do not stand up against well-argued cases for other land uses.

Recognition of these limitations has led to the development of systematic, explicit procedures for assigning conservation value and selecting reserves. The more recent of these have concentrated not on individual sites but on the problems of combining sites into representative networks. From this new perspective have emerged some fundamental principles for selecting reserves, principles that reflect the ways in which individual sites relate to one another as members of networks.

Three principles for selecting priority regions and regional reserves

The three principles discussed below concern reserves as a means of conserving biodiversity, although reserves also have other purposes in protecting scenic, recreational or inspirational values. Application of the principles requires an explicit statement of the representation target of a reserve network, which can be all or a subset of the features in a geographical area and any proportion of population size or geographical extent.

Complementarity

With limited options and resources for reservation, an intelligent strategy is to assess the content of any existing reserves and then to proceed in a stepwise fashion, selecting at each step the site that is most complementary in the features it contains. Iterative procedures emphasizing complementarity in selecting reserves have been devised independently at least four

times: twice in Australia[16,17], in Britain'$ and in South Africa[19], although the term was not coined until 1991[13]. The algorithms and selection of specific sites differ among the procedures described, but all demonstrate that the features of a region can be represented in a minimal or near-minimal set of sites if they are highly complementary.

Complementarity is closely related to the notion of efficiency, expressed by 1-(x/t), where xis the number or area of sites needed to achieve a representation target and t is the total number or area of sites[20]. High efficiency is therefore achieved with procedures that emphasize complementarity. Scoring procedures that neglect the complementarity of sites[21] can be highly inefficient in delineating reserve networks[20], as can opportunistic reserve selection22-24.

Efficiency, and therefore complementarity, are very important because in most regions there is a limit, however ill-defined, to the land or water area which will be devoted to conservation. Efficient solutions to the problem of representing all features in an area are not only more defensible but also minimize the risk of reaching a ceiling of acceptable reserve area before all features are protected (Fig. 1).

Efficiency is highly dependent on the size of the geographical units used in the selection exercise and the indices of biodiversity used as targets for reserves. Large selection units requiring little or no amalgamation to produce viable reserves inevitably overrepresent some features and result in lower efficiencies to achieve the same target than very small units[27]. Efficiency will also vary with identical selection units according to whether the target features are, for example, a few widespread and co-occurring vegetation types or many plant species including narrow endemics which rarely co-occur. In addition, if the selection units vary in size, numerical and areal efficiencies are likely to differ[20].

With these factors considered, efficiency can be used to demonstrate the relative effectiveness of alternative approaches to reserve selection. Thus, the numerical value of an iterative selection of all landscape types in semiarid Australia was 0.969 compared to 0.786-0.078 for scoring procedures[20]. In the Cape Floristic Region of South Africa, two iterative algorithms represented all Proteaceae species with efficiencies of 0.904 and 0.898 compared to 0.822 for a random selection, the high values being due to the clumped distribution of the species[24].

Flexibility

In most regions, there are many ways of combining sites to form representative networks of reserves. An example for a small data set (Table 1) shows that there is a minimum combination size (size = number of component sites) below which no combinations are representative. This is the

`minimum set' of sites which can be identified with procedures using the principle of complementarity.

An analysis of site combinations shows, -however, that there can be more than one minimum set — seven in the case of Table 1— and hundreds or thousands of potential reserve networks larger than the minimum. In large data sets, there will be many more representative combinations, although in practice the number of these alternative reserve networks will be strongly constrained by considering only those combinations which contain essential sites for conservation, defined in various ways, and exclude sites which are unsuitable as reserves.

The principle of flexibility refers to this diversity of potential networks and has implications for selection procedures. Procedures giving only one answer to the problem of representing all features in an area can be indispensable in indicating the requirements of representation targets and in comparative analyses. Nevertheless, for the design of a real network, the exploration of the flexibility inherent in reserve networks is important. The more alternative networks that can be appraised, the more likely the planner is to find one which is not only representative but also maximizes values of design and land suitability and/or minimizes costs. Achievement of these other objectives will probably lower the efficiency of representation. The assessment of alternative networks should therefore allow the planner to determine the extent to which this occurs.

The range of alternative networks can be partly explored by constraining iterative analyses (excluding or forcing the inclusion of sites) and comparing the results to those from other starting conditions. This is a useful adaptation to changes in the availability of sites as, for example, some are destroyed or offered for reservation. It can also quickly demonstrate the implications of acquiring particular areas of interest or building on the existing reserve system rather than exchanging present reserves for new areas. If an iterative analysis is constrained repeatedly and systematically and/or the alternative sites considered at certain steps of the algorithm are recorded, a larger subset of the representative combinations of sites can be generated[24]. The CODA procedure (Conservation Options and Decisions Analysis)[27] is a more explicit way of exploring the flexibility of networks. It displays alternatives to selected sites and allows initial selections to be changed while calculating the degree to which targets for each feature have been achieved.

Flexibility arises from the non-unique occurrences of many indices of biodiversity. A critical implication of flexibility is that most reserve plans are fluid to some extent. Some component sites are fixed but others can be replaced. This variety of possible configurations gives scope for sensible resolutions of land use conflicts.

Irreplaceability

All sites occur with equal frequency when all possible combinations of a given size (number of component sites) are considered. This frequency is nC/t, where t is the total number of sites in the region, n is the size of combination and C is the total number of possible combinations of a given size (see Table 1). An important question for conservation planning is whether each site occurs with equal frequency in the set of representative combinations or alternative reserve networks (right hand column in Table 1). Sites actually occur with widely different frequencies in representative combinations. For example, at a combination size of seven in Table 1, there are 1150 representative combinations. In this set of potential reserve networks, sites are distributed in ten levels of frequency, from 11-100 %, with most sites occurring in relatively few combinations.

Frequencies of occurrence in representative combinations can be called levels of irreplaceability[28] with irreplaceability defined in two ways: (i) the potential contribution of a site to a reservation goal; and (ii) the extent to which the options for reservation are lost if the site is lost. Irreplaceability therefore provides a fundamental way of measuring the conservation value of any site.

For regional data sets with many sites, direct measurement of irreplaceability is impossible because of the vast numbers of possible combinations that would have to be generated and analysed. However, an accurate predictor of irreplaceability has been developed for small data sets[28] which is probably also accurate for whole regions. The present predictor can be used to approximate levels of irreplaceability and display a `landscape' of conservation value (Fig. 2).

Irreplaceability has also been approximated by displaying alternative results of iterative algorithms[24]. This approach will certainly identify the unique sites with irreplaceabilities of 100 %, although several issues remain unresolved: its effectiveness in distinguishing lower levels of irreplaceability; the influence of the particular rules and measures employed; and the correspondence of the results with a combinatorial analysis (e.g. for Table 1), which is the most fundamental and accurate way of identifying the frequency of occurrence of sites in the optional networks.

Levels of irreplaceability such as those in Figure 2 can guide the design of whole reserve networks, with choices proceeding from the most to the least irreplaceable. They also provide a way of anticipating conservation value when decisions are necessary on individual sites or when real-world constraints force departures from an original network design. In conservation planning, the notion of a gradient of irreplaceability is also compatible with the more traditional concepts of sites which have outstand-

ing value for a variety of reasons — these sites can be regarded as equivalent to those with maximum quantitative irreplaceability.

Principles in practice

These three principles can be applied at different scales. On a broad scale, they can identify priority regions for conservation efforts in a global or national context. On a finer scale, within regions, they can determine the locations of sites comprising representative networks.

The big picture -- identifyingpriority regions

The WORLDMAP computer program[13] identifies key regions for conserving the biodiversity of one or more taxonomic groups at global or national scales. Biodiversity is measured as a combination of the number of species or higher taxa in a region and the taxonomic differences between them[7], although endemism measures are also supported. A critical aspect of the system is the implementation of the principle of complementarity. This is used to find a priority sequence of regions to represent all taxa by identifying the maximum increment of unrepresented biodiversity possible at each step (Fig. 3).

An important consideration is the degree to which areas prioritized for one taxonomic group are congruent with those for another. In general, they only partially coincide, so different distributional patterns of groups influence the priority order of regions and the number of complementary regions necessary to represent all taxa. Three direct solutions to the problem of assessing total diversity are being investigated: summation techniques for combining data from several, but ideally many, different groups; reliance on indicator groups, chosen either because they are well known[29] or because of demonstrated correlations[30]; and the use of more inclusive taxa. Another major influence on the results of WORLDMAP is that, as regions are defined at broader scales, a greater proportion will be totally irreplaceable because more will have endemics.

Recent applications of WORLDMAP include analyses of Old World fruit bats and African antelopes for the IUCN Species Survival Commission and South American trees in collaboration with the Royal Botanic Gardens, Kew, UK. Future applications will also investigate the irreplaceability of regions for representing biotas.

A previous method for identifying global conservation priorities is the `hotspots' approach using data on plant species. The IUCN study[31] proposes two main criteria for choosing important regions — species richness and number of endemics — with several subsidiary criteria. Myers[32] uses

two criteria — degree of endemism and severity of threat. There is no doubt that both studies have identified globally important areas for conservation, but the arbitrary selection of sites raises questions about which other critical areas would emerge from a more exhaustive compilation of data and more systematic analyses.

Another global approach has identified centres of endemism for birds[29]. This improves on the `hotspots' approach by systematically using data on about one quarter of bird species — those with total geographical ranges smaller than an arbitrary threshold. Other systematic assessments are underway at global or continental scales. Some are likely to be more effective than others, just as regional selection procedures differ in efficiencies (see above). Given the significance and urgency of global assessments of conservation priority, there is a pressing need to compare the alternative evaluation methods so that their implications for conserving biodiversity can be clearly understood.

The regional picture — selecting reserve networks

Once a priority region has been identified, the problem remains to delineate a network of reserves capable of representing all the features requiring protection. The principle of complementarity has been applied with several iterative analyses of regions in south-eastern Australia, for example in South Australia[33] and on the south coast of New South Wales[34] Another recent application has been in South Africa[24]. Such analyses provide rapid indicative pictures of the minimum requirements for representative reserve networks and are valuable for comparing scenarios with different representation targets or with certain sites included or excluded.

The CODA procedure has applied both complementarity and flexibility to the South-East Forests of New South Wales to find a network of sites which represent a minimum percentage area of all environments, as well as occurrences of rare species and other important features[27]. Irreplaceability has only recently been tested on data from the Western Division of New South Wales[28], but its potential value in planning applications is established. Extensive trials are underway to validate the present predictions of irreplaceability levels for large data sets and to develop models for a wider variety of reservation goals.

Because regional methodologies differ not only in the configuration and content of the resultant reserve networks but also in the likelihood of achieving reservation goals[20], comparative trials are also desirable at this scale.

The way ahead

Application of the principles of complementarity, flexibility and irreplaceability will maximize the chances of achieving representative networks of reserves — something that many scientists and conservationists regard as an important, if difficult, goal. This benefit will come from increased effectiveness of planning that employs these principles and an improved ability to lobby and negotiate using systematic, defensible and flexible reserve plans.

In actual planning exercises, these principles will have to be applied with due consideration of other factors, such as design criteria which might determine the viability of reserved populations. At the selection stage, as much information as possible is needed on the areas and combinations of habitats required by certain species and on the existing and future impacts of natural disturbances and extractive land uses on the proposed reserves. The definition of `representative' needs to be extended to cover not only examples of land types or populations of species, but also the spatial and temporal dynamics addressed by landscape ecology and metapopulation studies.

Priorities will also have to be adjusted on a global scale according to the apparent threats to regions[14] and, on regional scales, in response to threats and differences in the most appropriate form of protection. The success of reserve location, design and management will have to be measured by monitoring the features and qualities for which the reserves are initially dedicated.

Improved procedures are being developed to apply the three key principles outlined here, and trials are underway on a wide variety of data sets. While ideas on each of the principles are being refined, work is also proceeding to integrate their application at a range of scales in Australia, Britain, South Africa, Uganda, South East Asia and South America.

Definitions of biodiversity and their use in reserve selection

There are several ways of defining biodiversity, which is itself a rubric to cover all of nature's variety. For any measure of diversity to be useful for conservation evaluation, it should be able to represent both alpha, or local, diversity and beta diversity — variety among areas (gamma diversity at larger spatial scales) .

One of the simplest measures of diversity is species richness, a count of the number of species[4]. Ecologists interested in community structure have also included the relative abundances of species in composite measures. More appropriate measures of ecological diversity would represent the functional diversity of what the species do[3,54] although sufficiently detailed information is likely to be very difficult to obtain for large numbers of species.

Another approach to diversity is to measure the variety or pattern of taxonomic differences among the organisms themselves. Biological classifications, of differing quality, are available for all groups of organisms to summarize these differences and new taxonomic diversity measures are able to cater for a broad range of data qualities? 1a

Regardless of definition, conservation planning requires indices of biodiversity to be distinguished, labelled and related to specific areas. These tasks therefore become central problems in the conservation of features such as species[1]l and communities[12].

Different indices vary in utility at different scales. For example, taxonomic richness on a global scale can focus attention on priority regions for individual groups of organisms[13] but, on the local scale, the density of this information is generally insufficient for decisions on actual reserve boundaries. Some indices, such as DNA variation within species, will remain largely inaccessible. Similarly, the sheer number of living organisms will preclude complete enumeration and geographic referencing before crucial conservation decisions have been implemented[14].

The practice of conservation therefore uses various approximations of biodiversity depending on the available data. Aspects of biodiversity which are not considered directly will be protected only incidentally, if at all.

Table 1. Possible and representative combinations of sites for a range of combination sizes' in an environmental province in western New South Wales, Australia[b]

Combination size (no. sites)	Possible combinations	Representative combinations'
1	24	
2	276	
3	2 024	
4	10 626	
5	42 504	7
6	134 596	134
7	346 104	1 150
8	735 471	5 980
9	1 307 504	21 457
10	1 961 256	57 043

[a] Combination size is the number of component sites.
[b] The province (4CB; Ref. 25) has a total area of 3820 square km, comprises 24 pastoral holdings and contains 17 land systems'.
Representative combinations are those which represent every land system at least once.

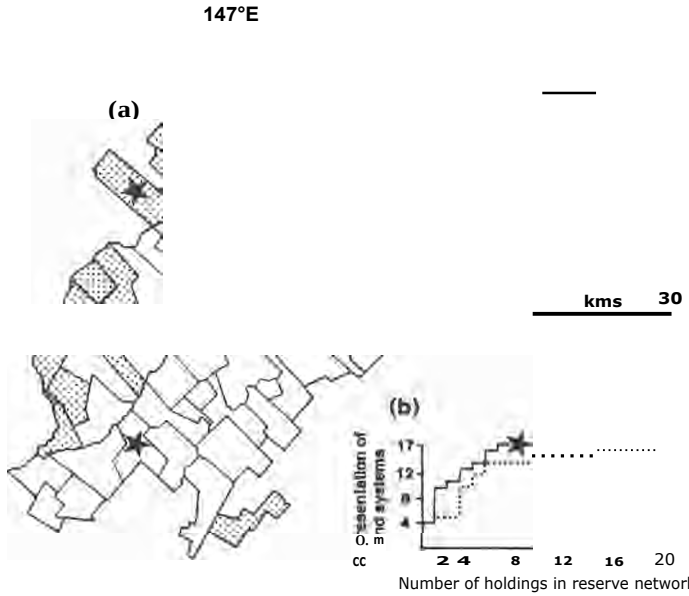

Figure 1
(a) An environmental province in the Western Division of New South Wales (Province lAC)[25] consisting of 95 pastoral holdings with a total area of 6625 square km (the northern edge is the Queensland border); the map shows two indicative sets of holdings needed to represent all land systems[26] in the region at least once: (i) stars indicate a minimum set identified by a simple algorithm which selects sites with unique land systems and then proceeds stepwise to select the site with the rarest unrepresented land system; if there is a choice, it selects the site which adds the most unrepresented land systems; (ii) shading indicates the set required if all holdings are scored according to the average rarity of the land systems they contain (in terms of frequency within the region) and reserved in order of score; note that reservation in order of scores for richness (number of land systems) requires 87 sites (92 % of total) to represent all land systems.
(b) Cumulative representation of land systems if holdings are added to a reserve network according to minimum set selection (stars) and scoring (shading).

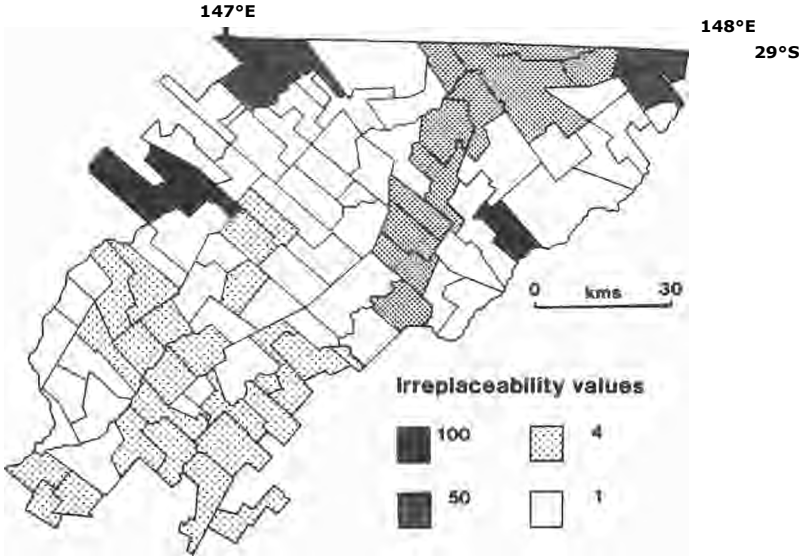

Figure 2
A `landscape' of conservation value for the environmental region in Fig. 1 derived from predicted irreplaceability levels28.

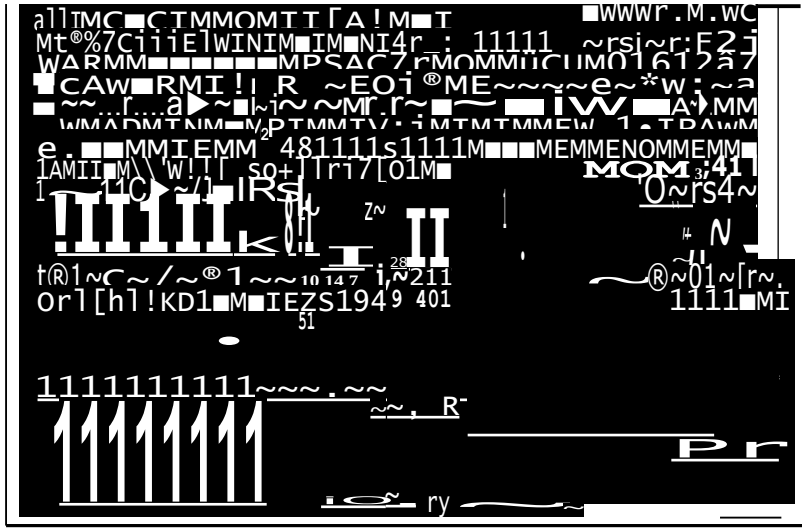

Figure 3
WORLDMAP[13] output showing the sequence of 58 priority regions needed to represent efficiently all the owl *(Strigiformes)* species of the world.

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Chris R. Margules and Paul H. Williams

Representing Biodiversity

The specific problem the biodiversity project sought to address is how to identify and map a set of areas within a defined region which together represent all, or as much as possible, of the biological diversity of that region. We call such areas biodiversity priority areas because they should have priority for protection and/or appropriate management. Such areas alone will not be sufficient for the long-term maintenance of biological diversity and should not, therefore, be seen as a solution to the greater problem of general biodiversity protection. Overall protection will require much more than the identification and management of priority areas. Priority areas will never encompass all of biodiversity nor will they sustain the biodiversity they do encompass over time if they are managed in isolation from the surrounding matrix of other natural, semi-natural and production lands. However, priority areas should form the core of conservation plans for biodiversity protection.

The book and its contents were conceived in the light of three realities affecting biodiversity conservation: biological resources will continue to be used for human necessities and comforts, so land use change will continue apace (at least in the short to medium term) regardless of calls for it not to; there is some as yet undefined upper limit on the total area that will be managed or maintained primarily for the protection of nature; thorough inventories of all organisms (taxa) at all places is not an achievable goal in the short to medium term, if at all.

To attain the goal of identifying and mapping priority areas there must be an acceptable way of measuring biological diversity, a way of determining an acceptable level of representation and, having determined that level, a cost-effective way of allocating limited resources to secure it. Various partial yet practical and realistic measurements for biodiversity were devised, together with methods for deciding where in the landscape priority areas should be located so that a given level of representation can be achieved. There is no proposed single best measure or single sufficient level of representation, and no perfect means of deciding where priority areas should be located. As knowledge accumulates and scientific methods are refined, different measures and levels will seem to be appropriate. However, current biological theory and up-to-date scientific practice have been brought to bear now because the need is pressing. Decisions are being made now to designate areas for protection or exploitation, and it is

desirable that these decisions be informed by all available knowledge. Because the need is urgent in the face of continuing land use change and because biodiversity protection competes with legitimate alternative uses of biological resources, the methods for identifying priority areas have to be explicit, efficient, cost- effective and flexible. In addition, because data are incomplete and knowledge is limited, they have to make the most effective use of available data and it will always be necessary to re-examine priorities as knowledge accumulates.

Biological diversity protection has tended to be *ad hoc* in the past, favouring the biodiversity of areas with least potential for extractive uses. Increasingly, protection is being advocated in response to threatened land use change or biological resource utilisation, but without clear measures of the contribution different areas could make to an overall representation goal. Our objective here is to describe suitable methods for deriving explicit statements about the relative contribution different areas, both alone and in combination, can make to the protection of biological diversity. Armed with such statements the return on any given conservation investment can be measured and enlightened trade-offs can be made. Negotiation can be entered into in the early stages of land use planning and policy-making, and initiatives can be taken to protect, or manage sympathetically, areas which make an appropriate, significant or unique contribution to the overall goal.

Recognising that competing land uses are a severe constraint on Modiversity protection, to facilitate negotiation the methods provide maximum flexibility in the location of priority areas. In accepting this need it must also be acknowledged that some areas in any region, country or locality are indispensable if full protection is to be achieved. No other areas can be substituted for them because, for example, they contain unique components of biodiversity. Such places are also identified by the methods.

Biodiversity Priority Areas

The basic components of biological diversity are interpreted here as the characters that confer variety on or amongst taxa; that is, the differences among organisms which govern their functional interactions. The number of characters is phenomenally large and in practice unknown. Yet sustaining this variety, unknown and unmeasured, the variety of life on earth, is the goal of biodiversity protection. To achieve this it will be necessary to maintain the complex hierarchical biological organisation that sustains characters within taxa, taxa within communities or assemblages, and such

communities within ecosystems. It is not reasonable to expect networks of biodiversity priority areas alone to maintain such complexity. What, then, should their role be? Ideally, they should encompass a sample of biodiversity. In practice, they will only contribute, within the limitations of current knowledge, to encompassing that sample.

Theoretically, phylogenetic pattern can be used to predict the distribution of characters among taxa. Figure 1 depicts the phylogenetic pattern among 10 African milkweed butterflies (Nymphalidae) as an example. This pattern can be interpreted as an estimate of the overall character difference between terminal taxa (usually called species) and can therefore be used to compare the overall biological diversity contained within sub-sets of those taxa. In practice — in the field — it is the persistence, hybridisation or extinction of populations that will determine the fate of characters. It is at the population level that biodiversity protection will succeed or fail. If populations of all species persist, or are allowed to pursue an unimpeded course of events to extinction, wider dispersal, hybridisation or some other form of evolution, then biodiversity will have been successfully protected. Thus, an ideal goal for biodiversity priority areas might be that, together, they should sample and maintain populations of all (known or extant) terminal taxa. Such a sample would have the same mean and variance as the wider populations; the same genetic, morphological and demographic means and variances (Margules et al., 1994). A sample does not imply sufficiency and a sample alone will not sustain biodiversity but a sample of populations of all terminal taxa is a rational goal for biodiversity priority areas.

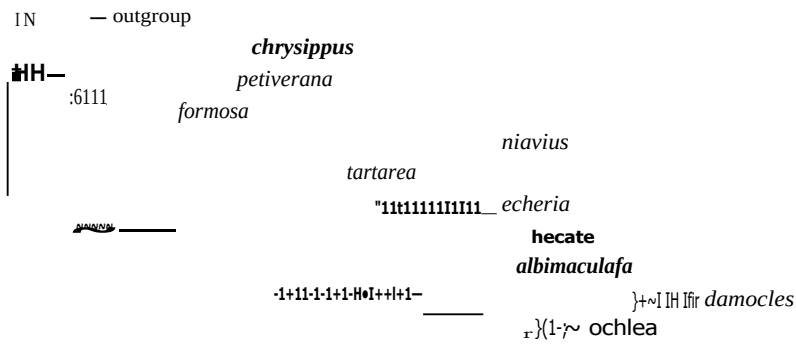

Figure 1. — Hierarchical classification (cladogram) for the 10 (out of 15) species of African milkweed butterflies for which data on both adult morphology *and* male pheromone volatiles are available. Each cross-hatch signifies one character (autapomorphy, synapomorphy, reversal, homoplasy) unique to individual species (terminal branches) or shared by two or more species at the internodes (subterminal branches). Data from Ackery & Vane-Wright (1984) and Schulz et al. (1993), for *Danaus chrysippus, Tirumala petiverana, T. formosa, Amauris niavius, A. tartarea, A. echeria, A. hecate, A. albimaculata, A damocles* and *A. ochlea.* Character state changes per branch vary within *Amauris* from a maximum of 39 *(A. echeria)* to a minimum of 5 (internode for *damocles + ochlea).*

Implementation of this goal is hampered by a lack of knowledge, both theoretical and empirical. With adequate knowledge, it should be possible to identify a set of areas throughout the world which together contain all the millions of terminal taxa, as exemplified by Figure 1, and hence all of the underlying characters. It should then be possible, with adequate knowledge, to locate areas within those regions which sampled populations of those taxa and were capable of maintaining such sub-populations over time. However, knowledge is limited. Figure 1 is the best estimate, based on current knowledge, of the phylogenetic pattern amongst the given subset of 10 African milkweed butterflies, but it is not necessarily the true one. Not only the interrelations of the terminal taxa, but even the number of terminal taxa recognised within such a subset, are all subject to re-assessment and revision. In addition, current records of geographical locations are likely to be biased, at least on spatial scales useful for conservation planning. Most field records are collected in a haphazard manner from locations where the species of interest are likely to be found, or are conveniently accessible. Locations with butterfly records are probably only a sub-set of the locations where butterflies actually occur and there are probably few records of where they were looked for but not found. Recorded absences are necessary to establish geographical ranges and recorded absences are rare in biological collections (Margules & Austin, 1994). At the coarse global scale this may not be much of a problem. We can be almost certain that there are no wild lions (or pseudopontiine butterflies) in Australia, no koalas (or trapezitine butterflies) in China, and no pandas (or calinagine butterflies) in Africa, but at finer scales, e. g. the distribution of koalas within south-eastern Australia or the distribution of purple hairstreak butterflies in the U. K., recorded absences are needed to establish the limits of species' ranges. The milkweed butterfly phylogenetic pattern is only one of millions and the same comments apply to all of them. Similarly, the descriptive knowledge needed to identify a sample of a population and the ecological knowledge needed to manage populations so that they remain viable and their evolutionary options are kept open is lacking for all but a very few species.

In summary, the goal of conserving characters by protecting samples of populations of terminal taxa (species) has to be revised because of: imperfect knowledge of terminal taxa and their phylogenetic relationships; biased data on the geographical ranges of occurrences of taxa; and inadequate management prescriptions for ensuring the persistence of populations.

From this it is apparent that some form of compromise is necessary. Hence the term `represent'. An appropriate revised goal for biodiversity priority areas is to represent the known biodiversity of a region, country or

biome. At this time, only a sub-set of taxa are sufficiently well known and well mapped to be represented with confidence in priority areas. Indeed, based on the inadequacy of distributional data at the taxon level in many parts of the world, strong cases can be made for working with assemblages of taxa or environmental classes, in which case the aim becomes to represent each assemblage or each environmental class in the priority area network. These sub-sets of taxa, assemblages, types of environments, etc. are the currently known and therefore measurable components of biodiversity.

Most research in conservation biology is aimed at managing populations in the wild. A widely used product of this research is population viability analysis, which tries to predict likely time to extinction under different management regimes. This is the `how' of conservation biology. Our purpose is to ask the question, `where should we locate priority areas in the first place and, therefore, where should this management take place?' The tools available for doing this fall into two distinct but interdependent classes. One encompasses the methods for acquiring suitable data sets, and the other comprises the methods for using those data sets to identify priority areas. Considerable effort has gone into improving the first class of methods in recent years, particularly in the field of computer technology. Much of the software and associated activities now familiar to conservation biologists and planners consist essentially of tools for compiling better data sets. Some well known examples include MASS (MacKinnon, 1994), BIOCLIM (Nix, 1986; Busby, 1991) and Conservation International's RAP (Rapid Assessment of Biodiversity Priority Areas). Improvements have been made in the display and manipulation of data using sophisticated Geographic Information Systems (GIS) and more are on the way, e.g. ERMS (Pressey et al., 1995). Investment in improving the second class of methods has been small in comparison, but is now picking up (e. g. Kirkpatrick, 1983; Ackery & Vane-Wright, 1984; Margules et al., 1988; Margules, 1989; Rebelo & Siegfried, 1990; Vane-Wright et al., 1991; Rebelo & Siegfried, 1992; Scott et al., 1993; Williams et al., 1993; Pressey et. al., 1993; Faith & Walker, 1994). Both sets of tools are necessary but quite different. They are considered briefly in turn below and in detail in the forthcoming book.

(a)

Attributes	Sites														
	AB		C	D	EF		GHI			J	K	L	M	N	O	P
1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		
2			1	1	1	1	1	1	1	1	1	1	1			
3						1	1	1	1	1	1					
4						1	1	1	1	1						
5							1	1								
6							1									
7							1									
8								1	1	1	1					
9								1	1						1	
10								1	1						1	
11										1						
12										1						
13												1	1	1		
14															1	1
15															1	1

(b)

Attributes	Sites															
	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P
1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	0	0
2	0	0	1	1	1	1	1	1	1	1	1	1	1	0	0	0
3	0	0	0	0	0	1	1	1	1	1	1	0	0	0	0	0
4	0	0	0	0	0	1	1	1	1	1	0	0	0	0	0	0
5	0	0	0	0	0	0	1	1	0	0	0	0	0	0	0	0
6	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0
7	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0
8	0	0	0	0	0	0	0	1	1	1	1	0	0	0	0	0
9	0	0	0	0	0	0	0	1	1	0	0	0	0	0	1	0
10	0	0	0	0	0	0	0	1	1	0	0	0	0	0	1	0
11	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0
12	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0
13	0	0	0	0	0	0	0	0	0	0	0	1	1	1	0	0
14	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
15	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1

(c)

Attributes	Sites															
	AB		C	D	EF		GH		I	J	KL		MN		O	P
1	1	10	25	12	18	21	31	11	17	12	10	6	5	4	0	0
2	0	0	100	19	15	17	13	31	23	34	15	9	71	0	0	0
3	0	0	0	0	0	1	197	31	1821	114	45	0	0	0	0	0
4	0	0	0	0	0	12	14	16	15	14	0	0	0	0	0	0
5	0	0	0	0	0	0	176	230	0	0	0	0	0	0	0	0
6	0	0	0	0	0	0	16	0	0	0	0	0	0	0	0	0
7	0	0	0	0	0	0	1785	0	0	0	0	0	0	0	0	0
8	0	0	0	0	0	0	0	14	780	71	179	0	0	0	0	0
9	0	0	0	0	0	0	0	19	100	0	0	0	0	0	1	0
10	0	0	0	0	0	0	0	4	11	0	0	0	0	0	1	0
11	0	0	0	0	0	0	0	0	0	140	0	0	0	0	0	0
12	0	0	0	0	0	0	0	0	0	845	0	0	0	0	0	0
13	0	0	0	0	0	0	0	0	0	0	0	98	71	169	0	0
14	0	0	0	0	0	0	0	0	0	0	0	0	0	0	981	19
15	0	0	0	0	0	0	0	0	0	0	0	0	0	0	67	183

Figure 2. – Three areas x attributes matrices showing (a) presence only data; (b) presence/absence data; and (c) abundance data.

Data Sets

Data sets consist of objects and attributes. Objects for the present purpose are areas; geographically (or biogeographically) defined units of land or water. They may be large or small and regular or irregular. Grid cells, catchments, tenure classes and habitat patches are some of the different kinds of areas used in later examples. Attributes are the properties of areas. They may be taxa or the characters they represent, or they may be more heterogeneous entities such as assemblages or environmental classes. Data sets should convey a consistent level of detail across the localities, biomes or countries they cover because the identification of biodiversity priority areas requires a comparison across all such regions. This means that they will have to be derived from some set or sets of raw data, involving some form of raw data analysis. The analysis can include one or more of the following: classification of environmental variables; classification of biological records to derive, for example, species assemblages; and the estimation of wider spatial distribution patterns of species or assemblages with statistical or empirical models relating records of occurrence to environmental variables.

Information about areas and attributes is most conveniently recorded and stored in an `areas by attributes' matrix. Attributes themselves can have different states. Figure 2 depicts three areas by attribute matrices. One (Fig. 2a) contains attributes of the `presence only' kind. Attributes such as species have been recorded as present in some areas, but there is no indication of abundance or extent, and the lack of a recorded presence within other areas does not imply absence. Rather, it means that it is not known whether the attribute occurs there or not. The second matrix (Fig. 2b) contains `presence/absence' attributes. In this case the absences are real within the limits of sampling intensity, meaning that the attributes were looked for, recorded as present where they were found and recorded as absent where they were not. The third matrix (Fig. 2c) contains estimates of abundance or extent, as well as absences. The methods for identifying priority areas (section "Identifying Biodiversity Priority Areas" below) can be applied to data sets with all three kinds of attributes with, successively, more confidence in the results. Unfortunately, almost all records of taxa are of the presence only kind. Most field records have been collected opportunistically, and the species collected are often the ones of interest to the collector. Figure 3 is a map of koala records from part of northern coastal New South Wales, Australia, showing how those records closely map the road network (from Margules & Austin, 1994). From this set of records, it is not possible to define the range of this species in this

area because there are few, if any, records of where it was looked for but not found. More systematic data collection methods have to be devised and implemented (see also Gillison & Brewer, 1985; Austin & Heyligers, 1989; 1991; Margules & Austin, 1994). In the meantime, the best use has to be made of existing data, even if field records are geographically biased and incomplete. There are techniques available to estimate spatial distribution patterns from presence only data and there are techniques for estimating underlying character difference among taxa. Both add value to existing data sets.

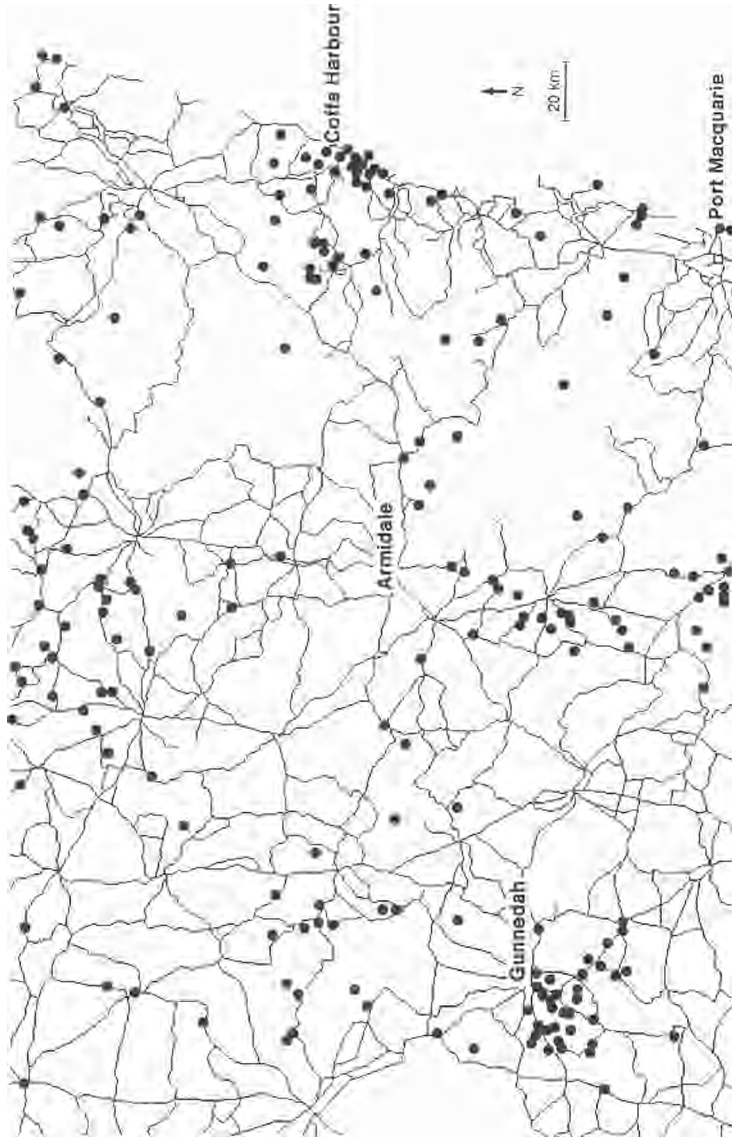

Figure 3. — A map of Koala records from part of northern coastal New South Wales, Australia, showing how the records closely follow roads. — Reproduced from Margules & Austin (1994a), with permission of the Royal Society of London.

Biodiversity Surrogates

The task of discovering, naming and then determining the systematic affinities of all species is daunting. Scientific names have been given to approximately 1.4 million species of plants, animals and micro-organisms (Wilson & Peters, 1988; Ehrlich & Wilson, 1991), but this is only a fraction of all species. Estimates of arthropod diversity in tropical forests alone range from about 7-80 million (Erwin, 1982; 1983; Stork, 1988), and knowledge of other invertebrate phyla is even poorer. Estimates have been made that if the collection and description of new species was to continue at the current rate, using traditional methods, it would take several thousand years to catalogue the world's biodiversity (e. g. Soulé, 1990) and in fact the rate is slowing because funding for taxonomy has declined (e. g. Stork & Gaston, 1990; Whitehead, 1990).

Urgency has led to the development of methods for rapid biodiversity assessment. The idea is that semi-professionals, variously referred to as apprentice curators (e. g. Sandlund, 1991) or biodiversity technicians (Oliver & Beattie, 1993), receive only basic training in field biology and systematics. They make field collections which are then sorted into broad taxonomic groups and morpho-species or recognisable taxonomic units (Oliver & Beattie, 1993). In this way rapid estimates of the total biological diversity at particular sites or over particular areas can be made.

The best developed programme is in Costa Rica, where the goal is to obtain an inventory of Costa Rica's biodiversity by the year 2000 (Janzen, 1991). The energy and commitment of those involved in the Costa Rican enterprise (see Gamez, 1991; Sandlund, 1991; Hovore, 1991; Janzen, 1991; and Wille, 1993, for comprehensive accounts) may well see it through and it may, in time, prove to be a model for other countries. Oliver & Beattie (1993) offer support with their finding that with only basic training biodiversity technicians estimated to within 13 % the actual number of spider species, to within 6 % the actual number of ants, to within 38 % the actual number of polychaetes, and to within 1 % the actual number of mosses, in samples from Australia. However, such approaches raise a variety of problems and their adequacy is questionable (Cranston & Hillman, 1992).

Even if Costa Rica reaches its goal by the year 2000 and even if biodiversity technicians can be enlisted and trained at a fast rate, the likelihood that approximate inventories will be available for most parts of the world in the short term, say, 20 to 50 years, seems remote. Even when inventories do become available they will be catalogues or lists of species or, far less helpful, merely estimates of numbers of taxa from particular locations. There is a lack of identity across samples from different areas so they cannot be compared validly. Wider spatial distribution patterns will still have to be estimated (i.e. those raw data would still have to be analysed using

spatial modelling techniques) before they could form an adequate data set for identifying biodiversity priority areas. In the short to medium term, only groups of taxa representing a very small proportion of total biodiversity will be available for priority area identification.

Since complete inventories are not a practical option, yet land use change is proceeding apace, some measurable biodiversity surrogate is required. Realistically, there are three kinds available: sub-sets of taxa or higher taxa, assemblages, and environmental variables or classes. It is not possible at this stage to nominate one as better than the others because there are valid arguments, summarised below, for and against each of them. In reality it seems likely that some combination will be used because the data available will normally come from a variety of sources.

Taxa

Although there is disagreement among biologists about the definition of species, most people recognise the term and think they understand it. Species are usually the units with which diversity has been measured (Vane-Wright, 1992). Higher taxa may also be used if a relationship between the distribution patterns of higher taxa and the distribution patterns of species can be demonstrated (Williams et al., 1994). It would be cheaper and easier to identify samples at higher taxon levels (Williams & Gaston, 1994). In the short to medium term, there may be little choice as to the sub-set of taxa, because it depends on available data and available experts. However, if there is the opportunity to choose, then consideration should be given to both focal taxa (those we have good information about and which are taxonomically tractable such as birds and vascular plants), and target taxa *(sensu* Kremen, 1992 — those that can be demonstrated to be better than average indicators of a wider range of biodiversity).

Most taxa remain undescribed and even of the taxa that are known, only a small sub-set is sufficiently well studied, both in terms of taxonomic status and geographic distribution, to be used to identify biodiversity priority areas. Unfortunately, there is no evidence that sub-sets of taxa represent biological diversity as a whole. Vane-Wright (1978) pointed out that, despite the coevolution theory for butterflies and plants, overall plant diversity was a poor predictor of butterfly diversity on a global scale. Majer (1983) showed that variation in plant diversity accounted for only 24 % of the variation in ant diversity in part of Western Australia. Yen (1987) found no correlation between the number of vertebrate species and the number of beetle species and further, that neither beetle nor vertebrate communities corresponded to plant communities in south-eastern Australia. Prendergast et al. (1994) showed only partial correspondence between areas rich in butterflies, dragonflies, liverworts, aquatic plants and breed-

ing birds in the U.K. It seems unlikely that priority areas identified using one or a few taxonomic groups as surrogates will adequately represent biodiversity as a whole, even though such analyses are valuable for action plans centred on particular groups, such as birds, for example.

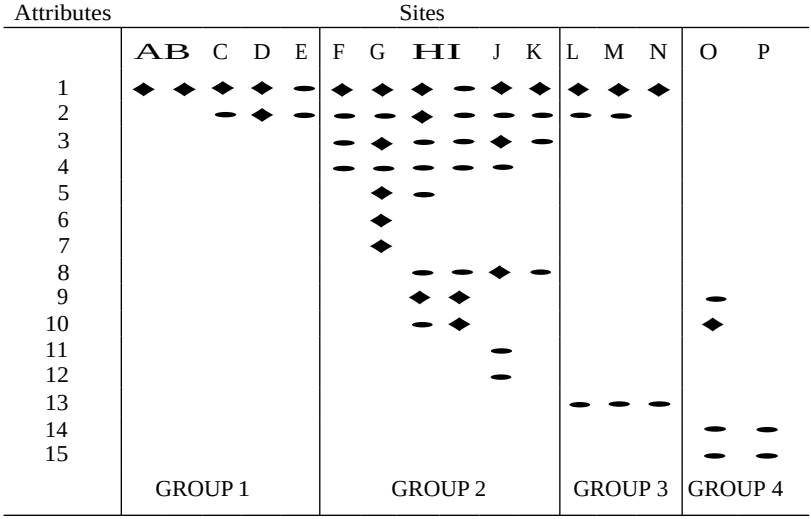

Figure 4. — Four groups of areas that might be identified using numerical clustering, based on shared attributes.

Assemblages

The term assemblages is used here generically to cover a range of ill-defined classifications such as community, association, habitat type, etc. They are generalised entities more heterogeneous than taxa. Taxa are distributed patchily within them and may only be present at some places and at some times. Assemblages can be subjectively derived using a small number of dominant or prominent species or they can be produced with numerical pattern analysis techniques.

Figure 4 is one classification that might result from a numerical pattern analysis of the data set illustrated in Fig. 2. Four classes have been recognised based on the attributes they have in common. Areas O and P have two attributes in common not shared by any other areas. Areas L, M and N have one attribute in common not shared by any other areas. Areas F, G, H, I, J and K all have at least three attributes in common and, although two of those are shared by some other areas, they also share a number of other attributes in various combinations. Areas A, B, C, D and E share one attribute also shared by most other sites, but what they have in common is the absence of most other attributes. This class illustrates one of the dangers of using classes as surrogates for biodiversity. It contributes nothing to the representation of the data set in Fig. 4 and by representing this particular class in a priority area, the opportunity to represent more of another class with more attributes might be foregone. In real data sets, areas and attributes can number in the order of thousands and classification (or ordination) can help simplify such complex multi-variate data and help reveal underlying trends. Basic texts on these kinds of analyses with a biological emphasis include Clifford & Stephenson (1975), Williams (1976) and Gauch (1982).

Assemblages represent various alternative combinations of species and the interactions between them and, therefore, more ecological complexity than individual taxa. Larger organisms such as vascular plants and vertebrates, most often used to delineate assemblages, are nested ecologically within interactions between smaller organisms such as nematodes, arthropods, fungi, protozoa and bacteria, and they have diminutive radiations in comparison (McKenzie et al., 1989). On the other hand, protecting a single area as a representation of an assemblage is likely to miss some species (Presley, 1994a) and some ecological complexity because it may be impossible to judge whether a given part of an assemblage is an adequate representation of the whole.

Environments

Environment is also a generic term covering land classifications based primarily on physical and climatic variables, numerical or intuitive, which

may or may not incorporate some biotic variables such as vegetation. Land systems (Christian & Stewart, 1968) are examples of intuitive classifications, while environmental domains (Richards et al., 1990; Belbin, 1993) are examples of numerical classifications. Environmental variables may also be used, unclassified, to estimate unmapped species distribution patterns (Walker & Faith, 1993). Different kinds of environments are assumed to support different sets of species (with some overlap) and have been used at broad scales as biodiversity surrogates (e.g. Mackey et al., 1989; Richards et al., 1990; Belbin, 1993).

There is strong theoretical support for the use of environments as biodiversity surrogates, which can be summarised as follows: each species has a unique distribution within environmental space determined by its genetic make-up and its physiological requirements. This distribution is, in turn, constrained by ecological interactions with other species. This is the concept of the niche (Hutchinson, 1958). Plant ecologists use the term "individualistic continuum" for essentially the same concept (Austin 1985). Species respond individualistically in, say, abundance or frequency, to resource gradients, and that response is constrained by interactions with other species. The implications are threefold: each species occupies a unique niche not readily predicted from that of other species (i.e. there is little overlap in environmental space); therefore, species distribution patterns are most accurately measured in multi-dimensional environmental space and only then translated to geographic space; the resultant spatial pattern shows high or dense populations in scattered locations representing most favourable habitat (or mix of environmental variables), and lower, more sparse populations in areas of more marginal habitat.

Thus, geographic distribution patterns can be linked to variation in the environment. Whittaker (e.g. 1956; 1960), Perring (e.g. 1958; 1959), Austin et al. (1984) and Austin et al. (1990), among others, provide empirical support. Nix (1982; 1986) has argued that for many purposes, including estimating the spatial distribution patterns of taxa, complete niche specification is unnecessary and that in most cases, five regimes, namely solar radiation, temperature, moisture, mineral nutrients and other components of the biota, are sufficient.

A network of priority areas representing the range of environmental combinations in a region is likely to encompass unknown species and known species with unmapped distribution patterns. Furthermore, the data needed to delineate environments (e. g. climatic data, geology maps, etc.) are more widely available than unbiased biological data and are often available at a consistent level of detail across wide geographic areas. On the other hand, as is the case with assemblages, protecting a single area as a representation of an environment is likely to miss some species because it

is not clear what an adequate representation might be. Similarly, the relationships between environmental classes and the distribution and abundance patterns of taxa are difficult to quantify, and some species may require a combination of environmental variables not recognised by a classification (Pressey, 1994a).

Combinations of surrogates

Taking into account the limitations on current knowledge, the limited resources for acquiring new data and the goal of adequately representing each surrogate, it seems likely that in practice some combination of these surrogates will be most widely applicable. In many localities, some data on the distributions of taxa are available but at an inconsistent level of detail and geographically biased. More usually, at least some environmental data are available at a consistent level of detail and it may be that assemblages have been mapped as, for example, vegetation or habitat types. A rational way to proceed in such cases would be to represent each environment or assemblage, overlay available distribution maps of taxa to see which, if any, were still not represented, and add areas to complete the representation (e.g. Nicholls & Margules, 1993). Alternatively, the geographic locations of selected taxa, such as rare or vulnerable species, could be used as seed points around which to build up a representation of each environment.

Collecting New Data

It may be necessary or desirable to collect new biological data. If the resources are available for doing so, then the most effective use should be made of those resources and any new collecting activities should provide data that can be analysed to maximise the information gained. In particular, new data should be collected in a way that facilitates accurate estimation of the spatial distribution patterns of species. This can be achieved with the following protocol: a conceptual framework, based on current ecological theory; field survey design principles based explicitly on the conceptual framework for locating field sample sites; a rationale for deciding which measurements should be made at field sample sites in addition to records of the target species; and appropriate analytical methods for estimating wider spatial distribution patterns from the point records that field sample sites represent (Margules & Austin, 1994). One chapter in the book is devoted to collecting new data and will consider each of these needs, covering theoretical, statistical and practical issues in describing some recent advances in the design of biological surveys and the analysis of survey data (see also Austin & Heyligers, 1989; 1991; Nicholls, 1989).

Summary of Data Requirements

Biodiversity surrogates may be taxa (e.g. species), species assemblages or environmental classes or variables, or they may be combinations of these. Compiling a data set is a process that includes both acquiring relevant raw data and, in most cases, analysing those data (classification, ordination and/or mapping) so that they are in a form suitable for identifying biodiversity priority areas. Raw biological data are records of the geographic locations of taxa, which may be available from previous collecting expeditions or surveys, or collected during new surveys. Raw environmental data may be extracted, for example, from meteorological records, or perhaps from existing thematic maps of geology, soils etc. Although thematic maps depict classes, they can be treated as raw data for the purpose at hand.

For the analysis of biological data there are two general options. One is to estimate the geographic distribution patterns of taxa, either intuitively, or by relating actual records of locations to environmental variables using predictive empirical models such as BIOCLIM (Nix, 1986; Busby, 1991) or statistical models such as GLMs (e.g. Nicholls; 1989). Empirical models are for data of the presence only kind. That is, there are records of where a taxon occurs but it is not known whether a non-occurrence is a true absence or simply a result of the taxon not having been looked for there. Almost all museum and herbarium data sets, the most common and widely available biological data source, are of this kind. Statistical models are appropriate for data of the presence/absence kind. That is, the absence of a species is the result of it having been looked for but not found.

The other option for analysis of biological data is to classify the data into assemblages and map the boundaries of assemblages. Classification can be done intuitively or with numerical methods, and mapping can be intuitive or it can utilise computer based empirical or statistical methods. Similarly, if the intention is to use environmental classes, classification can be intuitive or numerical and mapping can be manual or computer based.

Whether it is biological, environmental or a combination of both, the end result is a data set, either in raw form or derived from raw data records, that contains maps, on paper or in electronic form, of the chosen biodiversity surrogates. This data set can then be used, with the methods summarised below and detailed in the forthcoming book, to identify biodiversity priority areas.

Identifying Biodiversity Priority Areas

Biodiversity priority areas should, collectively, represent the biodiversity of the locality or region they are situated in; that is, they should encompass all of the attributes in the data set. Effective protection need not necessarily require formal reservation, because in some cases it may be possible to treat areas as protected if current land use is compatible and likely to continue. Methods for identifying priority areas are only one aspect of overall biological conservation planning and management.

In the past, parks and reserves, areas currently protecting components of biodiversity, have been set aside primarily for reasons other than the representation of biological diversity. The earliest National Parks, e.g. Yellowstone in the USA and Royal, near Sydney, Australia, were chosen for their outstanding natural features and beauty. Many areas throughout the world continue to be set aside for similar reasons or, for example, because they protect particular rare species or wilderness areas. The most common reason, however, appears to be that the land concerned was of little use for commercial exploitation or human habitation (Runte, 1979; Pressey, 1994b). Thus, in general, reserve selection has tended to be opportunistic, *ad hoc*, or in response to some perceived external threat. This has had three unfortunate results. Firstly, the biodiversity (taxa, assemblages, environments) most in need of strict reservation is not protected. Consequently, limited conservation resources have been used inefficiently, in that current protected areas protect relatively few attributes. Thirdly, therefore, there is now a very uneven representation of biodiversity in existing reserves (Pressey et al., 1993).

Scoring and ranking procedures were developed in an attempt to make priority setting more systematic. In these procedures, multiple criteria, e. g. diversity, rarity, naturalness and size, among others, are given scores, the scores are combined and candidate areas are ranked with highest priority going to the area with the highest score. Many studies have reviewed these procedures or the criteria used (e. g. Margules & Usher, 1981; Margules 1986; Usher, 1986) but it was not until 1989 that Pressey & Nicholls (1989) examined their efficiency in achieving the goal of full representation of natural features. In summary, they found that selecting areas from the top ranked down, based on a variety of different multi-criteria scores, require at least a fifth, but in most cases more than half, of all areas if full representation was to be achieved. Application of scoring and ranking procedures does not improve efficiency greatly over *ad hoc* representation.

Four principles

The selection methods to be described in the book are designed to help remedy current uneven representation and to promote efficiency. They are based on four principles: vulnerability, complementarity, flexibility and irreplaceability, summarised below. The methods are efficient and explicit. Efficiency is needed because the amount of land or water realistically available primarily for biodiversity protection is limited. The real prospect exists that the upper limit will be reached well before biodiversity is adequately represented. It is important that the methods be explicit for two reasons. First, for the results to be independently verifiable they must be repeatable. Second, the priority area network so identified can be justified more easily and defended more readily (Margules et al., 1994).

Vulnerability

Some ecosystems and the biota they contain are more vulnerable to threatening processes than others. Fertile soils and good rainfall are conducive to agricultural production, for example. Similarly, some species cope with impacts such as habitat fragmentation, grazing, etc. well, being favoured by the changed conditions, while others suffer a reduction in abundance and a contraction of range. In many cases it is possible to predict which kinds of habitats or ecosystems are most likely to be exploited and in some cases it is possible to predict which species will cope well with exploitation of their habitat and which species won't.

This kind of information on vulnerability or threat can be used to help set the goals of a priority area selection procedure. Because resources for conservation planning are limited, not all biomes, parts of countries, countries or regions can be dealt with equally or at the same time. Priority for identifying priority area networks should be assigned to places that are vulnerable to threatening process such as land clearing for agriculture, and the goal might be to represent all species that are vulnerable to threatening processes, rather than all species willy-nilly, in which case the data set being used would contain only those species. One chapter of the forthcoming book is devoted to vulnerability and goal setting.

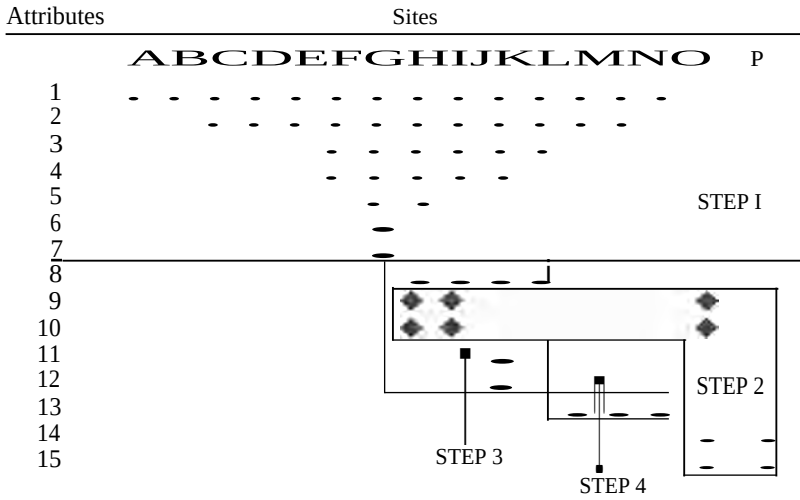

Figure 5. — The species represented at each step in an application of complementarity.
Area G, selected at the first step (see text) represents species 1-7.
Area O, selected at the second step, adds species 9, 10,14 and 15.
Area J, selected next, adds species 8, 11 and 12.
At the fourth step, any one of areas L, M or N would add species 13.

Complementarity

The selection of biodiversity priority areas has to proceed from the goal of representing all attributes in the data base and not be side-tracked by other equally legitimate but different goals, such as the preservation of natural scenery or wilderness areas. It follows from this goal that any new areas added to an existing protected area network should contribute new attributes (unless all of them are already represented). This common sense observation reflects the principle of complementarity. Priority areas should complement one another in terms of the attributes they contain; the characters, taxa, assemblages, environments, etc. It follows that an area contributing most to completing a full complement will not necessarily be the richest area (which is, in fact, often the case). This is why ranking procedures, such as selecting a sub-set of richest areas, are often so inefficient. They fail to take account of spatial heterogeneity and the turnover in species (or other attributes) from area to area.

Complementarity is a property of the areas x attributes matrix. It is a measure of the extent to which an area, or set of areas, contributes unrepresented attributes to an existing set of areas (Faith, 1994). Figure 5 is another representation of the data matrix shown in Figs 2 and 4. There is a total of 15 species in the matrix in Figure 5. Area G has two unique species and five others shared by one or more other areas. Imagine area G has been identified as a biodiversity priority area, which seems likely as it has two unique species. Seven of the 15 species are represented in area G. The remainder is the residual complement, comprising species 8 to 15. Area O contains four species from that unrepresented complement, more than any other area. If area O was added to area G as a second biodiversity priority area, 11 species would be represented, leaving a residual complement of just four. Area J contains three of those four. If area J was added to the network it would leave a residual complement of just one species, which could be represented by any one of areas L, M or N.

This illustrates an iterative heuristic procedure which measures the unrepresented complement in each area at each step and adds the area with the largest unrepresented complement until the total complement is represented (e.g. Kirkpatrick, 1983). A similar heuristic algorithm, which is explained in detail in the book, takes areas with the rarest species first and then adds areas with the rarest remaining unrepresented species until all are represented. Thus, area G would still be chosen first but J would be chosen second. They both have seven species and both have two unique species, but G contains one species (species 5) represented in only two areas, whereas the next rarest species in area J is represented in four areas altogether (species 8). The next choice would be between areas O and P which both have the rarest remaining species (species 14 and 15) and 0

would be chosen because it contributes those two next rarest species plus two others not yet represented (species 9 and 10). Once again, the last unrepresented species (species 13) could be represented by any one of areas L, M or N. In this example the list of sites chosen is the same, but the order is different. Using other data sets, the lists might differ between the two algorithms.

Complementarity is very important because it leads to an efficient representation of biodiversity surrogates and, therefore, to an efficient use of limited conservation resources such as land (or water) and funds. Algorithms which incorporate complementarity procedures are much more efficient than *ad hoc* approaches and scoring and ranking approaches (Pressey & Nicholls, 1989; Pressey, 1994b).

Replaceability, Irreplaceability and Flexibility

Figure 5 also illustrates the principles of flexibility and irreplaceability. Areas G and J are irreplaceable because they contain unique species (species 6 and 7 and species 11 and 12 respectively). Areas L, M and N all contribute the same species to the full complement so they can be substituted for one another. They are replaceable. Area O is also replaceable, though less so than L, M or N. Area O contributes four species to the full complement. Those same four could not be contributed by any other single area, but area P plus either H or I could be substituted for O. These have been described as different levels of irreplaceability (e.g. Pressey et al., 1993). Areas G and J are globally irreplaceable. Area O is goal irreplaceable because the goal is to find the fewest areas that represent all species. It would cost an extra area to replace O. Areas L, N and M are all replaceable (in this case, each one with one of the other two).

The existence of replaceable areas, which may be rare in some cases but seem to be common in many data sets, facilitates negotiation with alternative competing land uses. All replaceable areas are negotiable, though some have associated costs, such as the extra area needed if O was not available. Irreplaceable areas are not negotiable because without them it would be impossible to achieve the goal of representing all attributes. The fate of irreplaceable areas is a matter of policy, not planning. They should form the core around which the rest of a priority area network is built up. Flexibility is a property of the network of areas. It arises because many areas can be substituted with one or more others. Flexibility refers to the different spatial arrangements of areas available to achieve the overall goal.

Priorities within Networks

Fully complementary priority area networks can be identified using heuristic iterative procedures. While the results may be close to optimal in the sense that a minimum or near minimum set of areas is identified that together represent all attributes, implementation in the real world may be difficult. Some areas may be unsuitable because, for example, they are degraded, or simply unavailable for compelling social, economic or political reasons. Flexibility in spatial configuration can help towards practical solutions, but even when pragmatic compromises have been made, not all preferred areas will have equal status. It is likely that priorities within the identified network will have to be set. The impacts of any threatening processes will affect both the timing of protection and the type of protection measure.

Areas that are threatened or more vulnerable to threatening processes in general would have a high priority, but it may be necessary to examine them closely before coming to any final decision. For example, consider a habitat remnant in cropland and a more remote area in rugged terrain. They occupy different environments and contribute different sets of taxa to the full complement but the habitat remnant is more vulnerable simply because of its location. If it was determined that the habitat remnant was seriously threatened, or that populations of taxa there had low probabilities of persistence unaided, then it might be accorded priority and scarce management resources might be allocated in an attempt to protect it. On the other hand, if the area occupied by the remnant was not irreplaceable, alternatives could be sought. Even if the remnant was irreplaceable, it is conceivable that a greater contribution to the overall conservation goal might be obtained by abandoning it and diverting scarce management resources elsewhere, particularly if it was assessed to have low viability even if aided on a large scale. However, it is better to have known in the first place which set of species the remnant contributed and whether it was flexible or irreplaceable, so that the trade-off between allocating management resources and sacrificing the goal of representing all attributes could be made on the basis of all possible information.

Summary

The problem addressed by the biodiversity `Schwerpunkt' and developed in the forthcoming book is how to identify a sub-set of areas within a specified region which together represent as much as possible of the biological diversity of that region. Such areas are called biodiversity priority areas. This is an immensely practical problem soluble only through both theoret-

ical and empirical investigation. Because total biodiversity as we have interpreted it is not directly measurable, it will only ever be possible to represent surrogates; sub-sets of taxa, assemblages, and/or environments. Two classes of methods are required. The first represents methods for deriving suitable data sets; the second, methods for using those data to identify priority areas. Raw data such as field records of the locations of taxa can be treated in various ways to improve their quality, such as modelling wider spatial distribution patterns and estimating character diversity from phylogenetic patterns. New data should be collected in a more systematic and cost-effective way than they have been in the past, enabling more accurate statistical models of spatial distribution patterns. The methods described for identifying priority areas are efficient and explicit heuristic algorithms. They employ the principles of vulnerability, complementarity, flexibility and irreplaceability. Their goal is to represent all of the attributes in the available data set in a minimum or near minimum set of areas, which should be the starting point for biodiversity conservation planning. Preferred biodiversity priority areas can then be determined via an exploration of flexibility and possible alternative configurations, and an assessment of the costs associated with controlled departures from the most efficient representation, to facilitate negotiation with competing land uses.

Biodiversity priority areas are necessary but not sufficient for the long-term maintenance of biological diversity. The methods and procedures to be described in the book are some of the tools needed by conservation biologists to help them identify priority areas. Other tools, such as planning procedures incorporating competing land uses, and management prescriptions to minimise the risk of extinction of local populations, can then be focused more sharply on the places and populations in greatest need.

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Aufsätze
Teil II

Research Group
"Collective Intelligence:
Self-Organizing
Superorganism Systems"
Scott Camazine, Jean-Louis Deneubourg,
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Building Biological Superstructures: Models of Self-Organization

Introduction

Managing a large group of units (e.g. engineering systems) often involves a centralized and hierarchical chain of command. In this case, there is a "chief" who knows the positions, goals, etc. of each unit at a given moment. On the basis of this information, the centre issues commands. This reflects the idea that to achieve optimality, a single central unit should have access to all the available data as well as the algorithms required to analyse them. These algorithms are necessarily complex and highly specific; they cannot tolerate internal errors and imprecise or incomplete information, nor cope with changes. The consequence is that each solution must be constantly monitored and overhauled to deal with unforeseen events. This leads to a spiral of increasing complexity and instability.

There is, however, scope for alternative and complementary approaches.

Looking at insect societies, we can see that the organisational system they have "selected" is in many respects diametrically opposed to the above schema. Instead of one single central control unit, society is organised through interaction between units and with the environment. Our research has focused on understanding how patterns emerge at the level of society by means of mechanisms involving self-organization. Self-organization is a process by which a pattern at the global level of a system emerges solely from various interactions among the lower-level components of the system. Moreover, the rules specifying the interactions among the system's components are followed only using local information, i.e., without reference to the global pattern.

Self-organization refers to a broad range of pattern formation processes in both physical and biological systems. The mechanisms of self-organisation in biological systems have many features in common with those in physical systems. However, there is one fundamental difference between self-organization in biological and in physical systems, relating to the nature of the rules governing the interactions of each system's components. This means there is an extra dimension to self-organization in bio-

logical systems, because in these systems the rules of interaction can be finely molded by natural selection. By tuning the rules, selection shapes the pattern formation. Hence group activity can give rise to adaptive superstructures.

Using a wide range of examples, our primary goal has been to demonstrate the link between the relatively simple behavioral programs of the individuals in a group and their collective behavior, which appears complex and at the same time adaptive. We have also investigated to what extent these mechanisms are general. In this type of research, most of the examples are provided by social insects and are based on their foraging activities or building behavior. However, we have chosen to extend our study to other groups of organisms which produce self-organized structures such as the coordinated movements of fish in a school, or the synchronized flashing of fireflies.

The goal raises a number of questions we have to address throughout the work:

(1) To what extent can mechanisms of pattern formation based on self-organization account for biological superstructures? (2) What alternative mechanisms of biological pattern formation can be found? (3) Under what circumstances do organisms use self-organization as opposed to alternative mechanisms? (4) What degree of complexity at the individual level is required to generate the observed complexity at group level? (5) How much of the observed complexity at group level is a reflection of the complex environment rather than of complexity at the level of the individual? (6) To what extent do widely differing organisms adopt similar, convergent strategies of pattern formation?

We have endeavoured to develop these concepts during our stay at the Wissenschaftskolleg and have left a written record of our work. We would like to thank the initiator of this project, Professor Rüdiger Wehner, for his consistent help and encouragement, and the staff of the Wissenschaftskolleg for making this project possible.

Prologue — Aims and Scope

The book we were working on during our stay at the Wissenschaftskolleg is about the building of biological superstructures. We use the term "superstructure" to mean *"that which is built upon something else as a foundation: a structure raised upon something"* (Oxford English Dictionary). The superstructures of interest are those built upon a foundation of interactions among organisms, hence we focus on objects built by or of groups of organisms. Moreover, we focus our attention on those products of group activity which are group-level adaptations, not merely incidental by-products of the behaviors of a group's members (Williams 1966). In other words, the superstructures of primary interest represent part of a shared extended phenotype of the replicators — the genes and memes inside the group's members — whose information ultimately steers the building process (Dawkins 1982).

A prime example of an adaptive superstructure is a nest built by a colony of the fungus-growing termite *Macrotermes.* With its thick protective walls and labyrinth of ventilation ducts, this air-conditioned castle of clay confers substantial positive fitness effects on the genes of its termite builders, by providing them with a safe and stable environment. The book is concerned with understanding how such superstructures develop by mechanisms involving self-organization.

"Building Biological Superstructures: Models of Self-Organization" is in part an introduction to self-organization in the biological systems that are its subject matter. The first part of the book provides the conceptual basis and tools for understanding the particular examples of self-organization that constitute the remainder of the book. We also present examples of biological phenomena that show how self-organized superstructures have emerged in groups of organisms that are not the highly cooperative societies seen in humans or social insects, such as bees, ants and termites. In the absence of allied genetic interests among group members, these superstructures are generally less sophisticated than the strongly adaptive group-level superstructures Finally, we summarize the lessons learned from self-organization and speculate how this approach will increase our understanding of the development of biological structures and how it will provide new avenues of research.

Even though biological self-organization is a relatively new field, there is already a large literature on specific topics. Therefore, we have been selective in the examples we present in this book. We have chosen to focus our attention primarily on groups of *multicellular* organisms which pro-

duce self-organized structures and processes. These include the coordinated movements of fish in a school, the synchronized flashing of fireflies, and the diverse activities which maintain the social organization of insect societies.

What is of special interest to us are the *mechanisms* by which superstructures are built. Recent research has begun to reveal that even the most sophisticated superstructures that we will consider, such as the nests of termite colonies, may be self-organized structures built through the iteration of surprisingly simple behavior patterns. Our primary goal in writing this book is to forge a link between the behavioral programs of the individuals and their emergent superstructures and properties. This goal raises a number of questions to be addressed throughout the book: (1) To what extent can mechanisms based upon self-organization account for biological superstructures? (2) What level of complexity at the individual level is required to generate the observed complexity at the collective level? (3) How much of the observed complexity at the collective level is a reflection of complexity of the environment rather than complexity at the level of the individual?

We believe that much of the complexity of self-organized structures seen in biology arises because the rules governing the interactions among the components of these systems have evolved through natural selection. This has generated an enormous diversity of interactions, far surpassing the diversity of interactions possible in chemical and physical systems. This makes the study of biological self-organization particularly exciting and challenging. Furthermore, it guarantees that the study of biological self-organization will not simply be a reworking of chemical and physical models of self-organization using the same equations with variables of different names.

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Jean Louis Deneubourg and S. Goss

Collective Patterns and Decision-Making*

In recent years, the study of group behaviour has been dominated by two questions, namely "What is the evolutionary process by which social life has developed?" (e. g. Wilson 1975, Barash 1977), and "Is behaviour optimal?" (e.g. Krebs & Davies 1984). Preoccupied with these ideas, sociobiology has more or less forgotten to ask *how* groups or societies forage, move, defend themselves, and generally do what they do. As a result, while the adaptive value *(why)* of belonging to a group is well documented (anti-predator, reproduction, etc; e. g. Morse 1980, Broom 1981), we only poorly understand the mechanisms *(how)* by which these groups are formed and modified as a function of their activity. While there are detailed descriptions of collective behaviour, on the one hand, and equally detailed descriptions of individual behaviour on the other, the causal links between the two and their often surprising difference in complexity are usually neglected.

The key to bridging these gaps lies in remembering that at each moment the members of an animal group decide, act and interact, both amongst themselves and with the environment, permanently changing the state of the group. Just as sociobiology, with its population genetics and games theory, shows the importance of dynamics and individual interactions in the evolution of social behaviour, we propose the analysis of these interactions as the straightest path to understanding the short term collective behaviour of animal groups.

The most widely-observed social interaction concerns allelomimesis in its many forms (roughly speaking, do what my neighbour is doing; see e. g. Sudd 1963, Scott 1972, Altmann 1985). For example, one bird takes off, those near it also take off, and very quickly the whole flock has taken off. Recruitment in social insects is another classical example, in which one forager discovers an important food source, recruits inactive foragers in the nest to go to it, which in turn recruit still more foragers. Allelomimesis is by definition autocatalytic, in that if I do as others, then others do as I,

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and we all shall end up doing the same thing. Another term for this is positive feed-back, and we shall use the three terms rather indiscriminantly. In the context of this article, they all refer to the idea that the probability of an individual adopting a particular behaviour or state is an increasing function of the number of individuals already exhibiting that behaviour or state. Most often allelomimesis is considered as little more than a mechanism for aggregation, cooperation and reciprocal altruism (e. g. Milinski 1987) or synchronisation. We would like to emphasize three of its less intuitive properties or consequences:

— Allelomimesis is important in structuring a group's activities.
— Even very simple allelomimetic behaviour can be the source of complex and often surprising group behaviour.
— Different group behaviour can be based on identical allelomimetic behaviour.

Such parsimonious ideas strongly contrast with the traditional biological approach, in which individual complexity is necessarily at the root of collective complexity, and in which the observation of a different collective behaviour automatically elicits the search for a different individual behaviour.

To show these properties — better known in physical and chemical systems as self-organisation (Nicolis & Prigogine 1977) — as clearly as possible, we deliberately minimise both individual behavioural complexity and long-term differences between the members of a group, whether due to age, experience, or any other factor. In our wish to concentrate on the mechanisms behind collective behaviour, we shall more or less ignore its fitness or benefit, either collective or individual.

While we illustrate our ideas mainly with examples of social insect foraging behaviour, they apply to a wide range of animal species' group behaviour. Mathematical models play an essential role in linking the individual and the collective behavioural levels. Quantitative individual observations define the model's kinetics. Its dynamics and stationary states correspond to the collective pattern or decision observed (and can also be used to calculate a benefit), and are compared with experimental results to test the model's validity.

Patterns and decision-making via trail pheromone in social insects

Of the different animal groups, social insects are those in which these properties are the most easily and clearly shown. To varying degrees, their societies are composed of a large number of individuals, characterised by the simplicity of their behavioural repertoire, their limited individuality

and capacity for learning, and the inherent randomness of their behaviour (this last characteristic is by no means limited to social insects). On the other hand, communication between individuals, notably by pheromone, is usually highly developed.

In spite of this individual simplicity, and perhaps, as we shall see, because of it, the twenty thousand or so social insect species exhibit a bewildering panoply of social behaviour, fully illustrating the contrast between their individual and collective levels of complexity (e. g. Wilson 1971). Furthermore, we can experiment on these societies in a way impossible in any other kind of collective decision-making organisation. Unlike molecules or cells, workers are easily visible, and we can manipulate insect societies and place them in experimentally controllable situations with relative ease.

An important benefit of sociality is that it provides an opportunity for the exchange of information, and foragers of most social insects can communicate the location of a food source or a favourable foraging zone to their nest-mates by one means or another. We shall consider the means used by the majority of ant and termite species (and some bees): trail pheromone.

The same autocatalytic scenario may be observed whenever trail pheromone is used. The direction chosen by a forager that passes a given point laying pheromone will influence the direction chosen by the next ant that passes, which also adds pheromone in the direction chosen. In this way, one specific direction is rapidly and collectively selected out of a number of initially equivalent possibilities. Repeated along a series of points, this process is the means by which the foragers from a well-defined trail between, for example, the nest and the source or foraging zone. We shall now examine some of the widely different patterns and decisions generated by this one autocatalytic mechanism.

Army ant swarm patterns:

(Raignier & Van Boven 1955, Schneirla 1971, Franks 1989). Army ants live in colonies of up to 20,000,000 individuals, roughly 1 cm long and practically blind. Their group behaviour is anything but simple. Every day, a colony will form a swarm raiding system made up of 200,000 foragers, covering 100 X 10 m, catching some 30,000 prey items which are all transported to the nest, and they do this in highly structured and species characteristic patterns, in the total absence of any central organisation (Fig. 1).

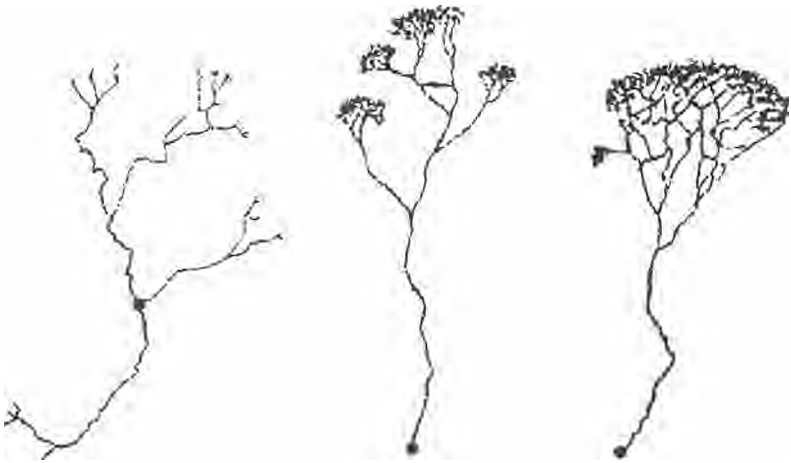

Figure 1 Foraging patterns of three army ant species (redrawn from Rettenmeyer 1963 and Burton & Franks 1985). A, *Eciton hamatum;* B, *E. rapax;* C, *E. hurchelli.*

The traditional approach to these different swarm patterns is to assume that each is optimal for the prey these species hunt, and corresponds to complex, species-specific individual behaviour. For example, *Eciton burchelli* has a more dispersed swarm and feeds more on scattered arthropods than *E. hamatum,* which feeds more on insect colonies and has a more concentrated pattern, *E. rapax* being intermediary in diet and pattern. However, Monte Carlo simulations of one simple trail laying/following behaviour can generate different characteristic swarm patterns (Deneubourg et al. 1989). With a homogeneous, low-density food distribution, the simulation generates a front and a central trail (Fig. 2A), very like those of the Argentine ant *Iridomyrmex humilis'* exploratory swarms (Deneubourg et al. 1990). With a higher food density, the central trail branches repeatedly, forming a river delta pattern very like that of *Eciton burchelli* (Figs 2B and 1C). With a heterogeneous food distribution, the delta splits into a number of more or less concentrated sub-deltas, forming a pattern intermediate between that of *E. hamatum* and that of *E. rapax* (Figs 2C and 1A, B).

While the individual ants in the simulations have exactly the same qualitative and quantitative behaviour, the different spatial distributions of the foragers returning with food (via the different food distributions) interact with the flow of ants heading away from the nest to produce the qualitatively different patterns.

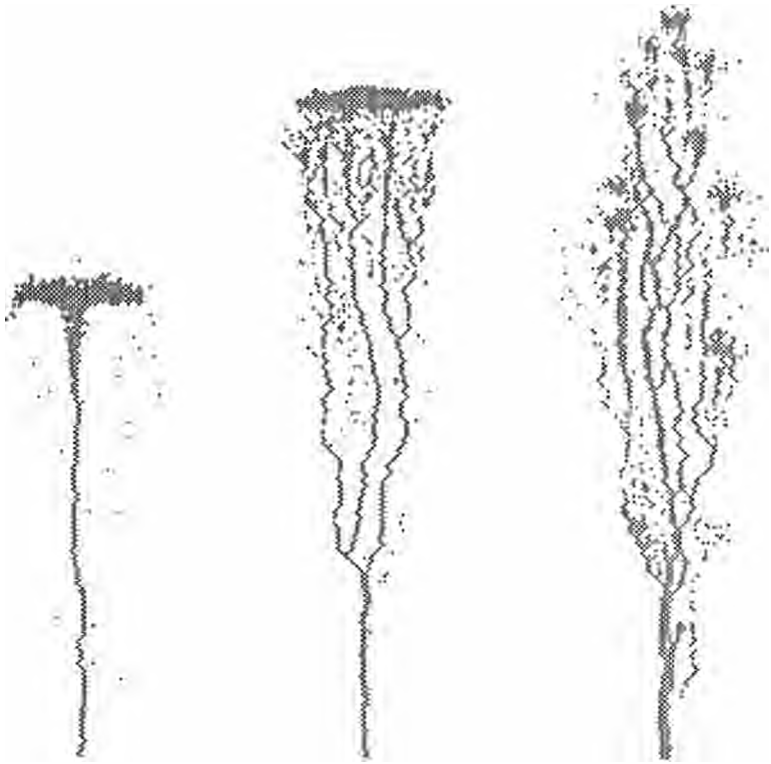

Figure 2 — Three distinct foraging patterns developed by Monte Carlo simulations of the same model with three different food distributions (reproduced from Deneubourg et al. 1989). Ten ants leave the nest per step. Ants advance into the foraging area. If they find food they return to the nest. At each point, the probability of moving per time step = 0.5 + 0.5 tanh [(L + R)/ 100 —1], where L and R are the quantities of pheromone ahead left and right. Those that move choose between ahead left and ahead right, with the probability of choosing ahead left = $(5 + L)^2/[(5 + L)^2 + (5 + R)^2]$. A maximum of 20 ants are allowed at each point. Having moved, the advancing ants lay 1 pheromone unit at the point chosen (with a saturation level of 300 units), and returning ants lay 10 pheromone units (with a saturation level of 1 000 units); 1 /30th of the pheromone at each point evaporates per time step. A, each point has a 1/10 probability if containing 1 food item; B, each point has a 1/2 probability of containing 1 food item; C, each point has a 1/100 probability of containing 400 food items.

The simplicity and autocatalytic nature of their trail laying/following is further illustrated by the occurrence of circular mills. One can sometimes observe in the field (or easily provoke in the laboratory) a group of army ants turning around and around a circular obstacle. The more they turn the more they lay trial around the obstacle, only stopping when they are totally exhausted. Removing the obstacle makes no difference once the circular trail has been formed (Fig. 3B), and one can justifiably describe the army ant foraging system as the blind leading the blind. (Note that army ant foragers lay trail pheromone both when returning to the bivouac with food and more or less continually as they move outwards from the nest.)

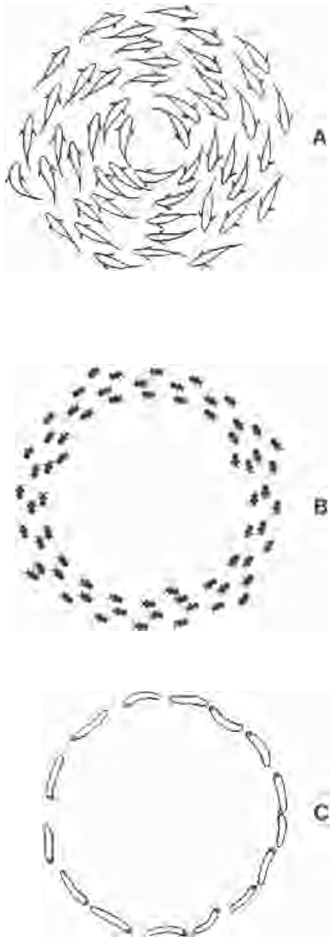

Figure 3 — Three circular mills generated by the same process in three widely different species, namely fish (A, redrawn from Parr 1927), army ants (B, redrawn from Schneirla 1971) and gregarious caterpillars (C, based on Fabre 1879). The circular mill based on *Eciton* illustrates how chemical signals dominate their movement. The workers are dropped into a small tray containing a large circular object. They start to move round the object, laying trail pheromone as they go. This causes them to move faster and faster as the trail gets stronger. The object is then removed, but the ants continue to turn until exhausted.

Messor pergandei's rotating foraging trail pattern:

Bernstein (1975) and Rissing & Wheeler (1976) described a spatial oscillation in *Messor pergandei*. A concentrated foraging column develops in a sector of the foraging area and rotates like the hand of a clock around the nest with a period of 1— 3 weeks and with a variable degree of irregularity. These columns change direction more slowly in years or regions when food is abundant. This complex behaviour can be modelled with the same autocatalytic scenario as above (the foragers laying pheromone only when returning with food) without needing to invoke spatial memory, complicated systems of coordination or any change of individual behaviour with food density (Goss & Deneubourg in press).

Briefly, the ant foragers choose a foraging sector as a function of the pheromone concentration associated with each sector. The choice is autocatalytic, as foragers that find food in a sector add to its pheromone. As a sector's food runs out, the trail is less reinforced and starts to diminish in strength. The foragers start to explore the two adjacent sectors, and in the same way concentrate on one of them. When that sector runs out of food, one of the two adjacent sectors is empty, having been exploited just before, and the other is full. The trail to this latter sector is thus more reinforced than the first. The colony thereafter spontaneously switches from sector to adjacent sector, and the column may rotate indefinitely (Fig. 4).

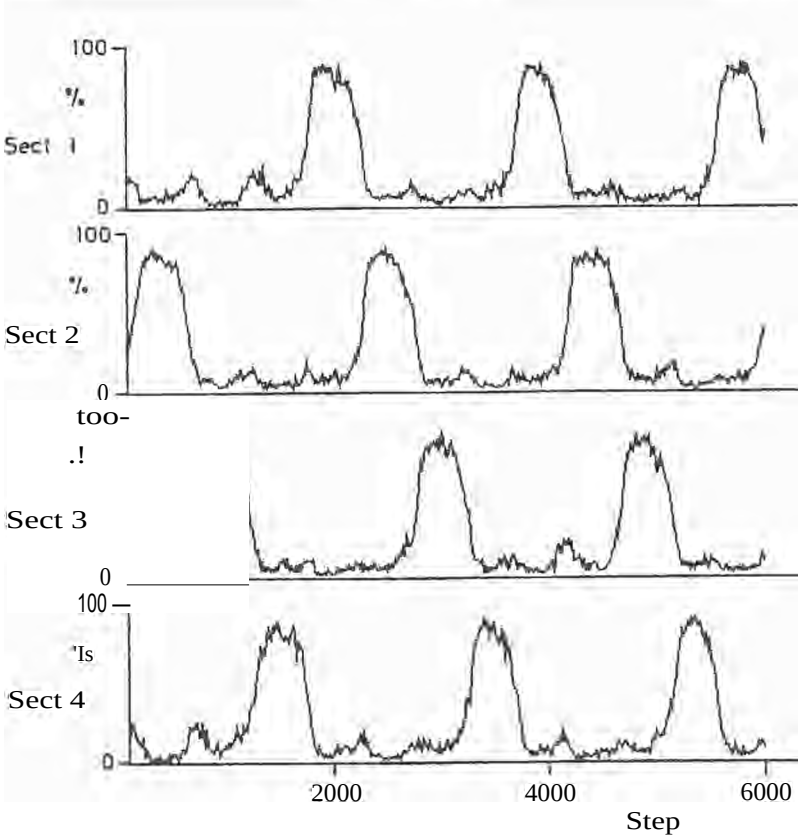

Figure 4 — Monte Carlo simulation showing the percentage of foragers in each of four sectors of a circular foraging area as a function of time (reproduced from Goss & Deneubourg 1989). The foragers clearly form a trail that starts in sector 2 and rotates clockwise to sectors 3, 4, 1, 2, 3, etc., with a regular period. Each sector contains initially 2000 seeds; 2 seeds arrive and 1/1000th of the total number disappear per unit time in each sector. A trail leads to each sector, characterised by C pheromone units, of which 1/30th evaporate per unit time; 100 foragers leave the nest per unit time. A fraction, $f = (20 + C)^2/E(20 + C_i)^2$, choose sector i. Of these, 1/20th diffuse into each of the two adjacent sectors. The number of ants that find seeds in a sector = 0.1 F,S,/(1000 + S), where F, and S, are the corresponding numbers of foragers and seeds. At the end of each step, all the foragers return to the nest. Those that have found a seed add one pheromone unit to the trail leading to the corresponding sector. Those that find no seeds return without marking.

With increasing food abundance, the model passes from random foraging to the formation of a trail that rotates about the nest. The greater the abundance the more slowly the trail rotates until it finally becomes fixed on one sector (a trunk trail), thus agreeing with the experimental observations. As with the army ant swarms, the same simple behaviour generates different complex patterns under different conditions. That *M. pergandei* develops rotating trails, while another harvester ant species forms trunk trails, (e. *g. Pheidole militicida:* Hölldobler 1974) and another forages randomly (e. g. *Pogonomyrmex maricopa:* Hölldobler 1974; *P. californicus*, even though this species can form recruitment trails: S. W. Rissing pers. comm.) need not necessarily correspond to a behaviour that is species specific, but could, at least in part, simply be due to different ecological conditions and/or food preferences.

Collective decisions:

The army ant swarm patterns and the *M. pergandei* clock pattern are spectacular examples of a more general problem concerning the spatial organisation of a group of foragers. The use of trail pheromone however is not only a system adapted to the exploitation of a patchy environment or of prey needing cooperative exploitation, but is also the touchstone of a collective decision-making system.

When new sources are discovered simultaneously, recruitments are started to them. Responding to the food sources' different quality, the foragers lay more or less pheromone when returning to the nest. The recruitments to them proceed at different rates, and they compete for inactive foragers, who are waiting in the nest to be recruited. This competition can generate complex social decisions well beyond the capacity of an individual.

Experiments combined with modelling have shown that trail-laying ants can use their trail recruitment to choose the richest food source (Pasteels et al. 1987, Beckers et al. 1990). For example, *Lasius niger* foragers, when offered simultaneously a 0.1 M and a 1 M sucrose solution, concentrate their activity on the 1 M source (Fig. 5A). When offered two identical 1 M sources simultaneously, they concentrate on one of them rather than exploiting both equally (Fig. 5B). However, they can become prisoners of their trail system in the sense that once one trail is well-established, a new trail is unable to compete with it and develop, even if it leads to a richer source. When offered just a 0.1 M source, *L. niger* foragers establish a trail to it and exploit it. If you then add a richer, 1 M source, they discover it but are incapable of switching their activity to it (Fig. 5C). This illustrates quite clearly that optimal foraging theory is not always the most appropriate model for understanding collective, autocatalytic behaviour.

Seeley (1985) analyses a similar collective decision-making process in honeybees that allows the hive to select the best of two sugar sources.

Ants that lay trail pheromone more or less continuously, such as army ants and *Iridomyrmex humilis*, can select the shortest route to a source (Goss et al. 1989), again via the competition between two rival recruitments. If you place a bridge that has a short and a long branch between an *I. humilis* nest and a food source, the interplay between those going to the food and those returning gives an initial advantage to the shorter branch. The autocatalytic trail system amplifies this initial difference, leading to the selection of the shortest branch of the bridge (Fig. 6). Again, they can be prisoners of their own history, and do not always choose the optimal solution. If you at first place a bridge with just one branch, the ants establish a trail on it. If you then add a second and shorter branch, they are incapable of switching to it.

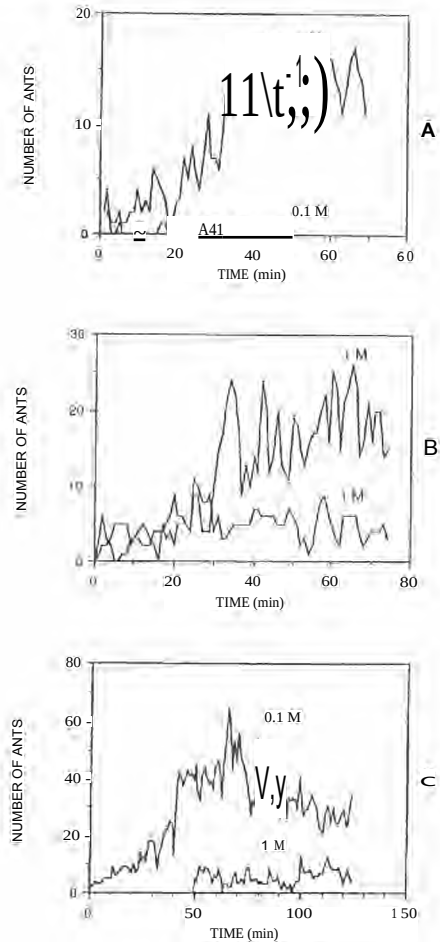

Figure 5 — Three examples of collective decision-making via food recruitment in ants, as shown by the time evolution of the number of foragers around two sucrose sources presented to a colony in a 0.8 m^2 arena (reproduced from Beckers et al. 1990). A, faced with a choice between two simultaneously presented sucrose sources of different quality, 1 M vs 0.1 M, the *Lasius niger* colonies always concentrate their activity on the richer source. B, faced with a choice between two 1 M sucrose sources, the *L. niger* colonies always concentrate their activity on one of them. C, presented with a 0.1 M sucrose source, the *L. niger* colony starts to exploit it; if you then introduce a 1 M sucrose source, they remain faithful to the first discovered, albeit weaker source, even though they have discovered the richer source.

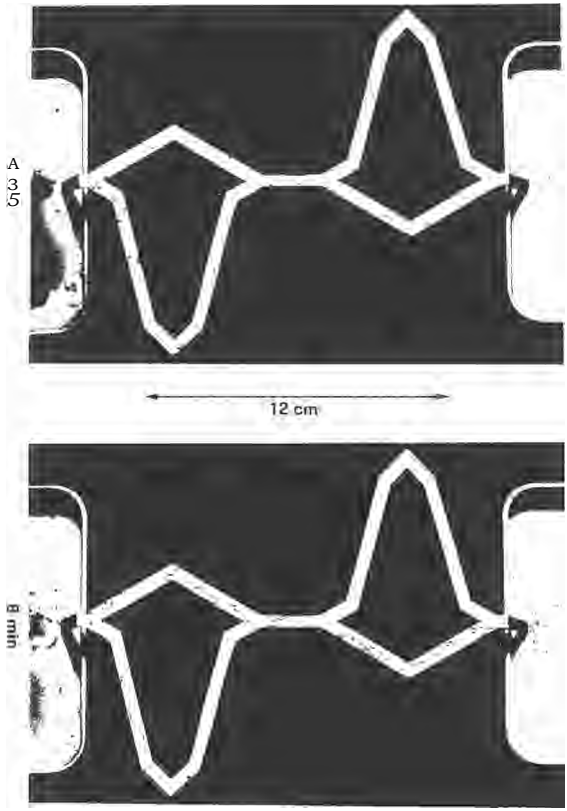

Figure 6— A colony of *Iridomyrmex humilis* selecting the short branches on a bridge between the nest and the foraging area (photos taken 4 and 8 min after the bridge was placed). (Reproduced from Goss et al. 1989).

It should be stressed that the choices described above are not the result of individual foragers comparing the quality of the two food sources, the lengths of two bridges, nor even the result of inactive recruits in the nest comparing the signals from different recruiters. Via the autocatalytic amplifying mechanism, exactly the same trail-laying and trail-following behaviour can generate different patterns and decisions if there is:
—different past activity;
—different environmental conditions.

Chemical communication organises widely different animal societies

The use of chemical signals to maintain group cohesion and guide group movement is by no means limited to social insects, and it is remarkable how widely different species have developed similar systems that generate similar, if not exactly the same spatial patterns and decisions, underlining the generality of the organisational principles we present.

For example, social bacteria use chemical trails to form army-ant-like "swarms" (reviews in Reichenbach 1986, Shapiro 1988; models in Pfistner 1989, Stevens 1989). The gregarious caterpillars *Malacosoma neustria* also use trails in a manner similar to army ants, enabling them both to diffuse information about richer foraging zones (more trail is laid to better food sources) and to find the same collective nest site at the end of a day's activity (Fitzgerald & Peterson 1988). Circular mills, similar to those observed in army ants, can be seen in the caterpillar of the European processionary moth *Thaumetopoeia pityocampa* (Fabre 1879) and also in the mud-snail *Nassarius obsoletus* (Crisp 1969), generated by a combination of chemical, tactile and perhaps visual cues.

Other intertidal molluscs use trails to help each other find protected rest sites (Focardi et al. 1985). Many different larvae, such as *Dendroctonus micans* (Grégoire 1988), use pheromone to recruit to favourable food sources. As a consequence, *D. micans* forms different group structures under different conditions, or even under the same conditions, with the same individual behaviour (Deneubourg et al. 1990).

Finally, Le Masne (1952) reviews an astonishing range of chemically and tactilely coordinated group behaviour in insects and other invertebrates, such as the rhythmically synchronised feeding in *Trichiocampus viminalis* larvae (which leave regular parallel perforations in leaves), or the massive group migrations of certain Nonctuidae larvae or army worms.

Visually-mediated collective patterns and decisions in widely different animal societies

The organisational properties of allelomimesis are by no means limited to groups using chemical communication, as in the examples described so

far. Visual communication is more widespread and can in the same way generate the same or similar patterns and decisions in animal groups such as swarms of locusts, schools of fish, flocks of birds, troops of antelopes, etc. All may take different forms in different situations (resting, feeding, moving, predator-avoiding, ...) (review in Wilson 1975) in a manner that appears highly coordinated but with no leaders. All may function with the same logic of simple individual behaviour, amplified and structured by autocatalytic communication.

Spatial patterns:

For example, in a number of fish species, the same school can move in an amoeba-like fashion, in a circular mill (Fig. 3A, C, as army ants, processionary caterpillars and mud-snails), in a rectilinear form, form defensive pods, or split in different manners to avoid predators or feed on different prey (e. g. Parr 1927; Breder 1959, 1976). Many of these forms can be generated by a single mathematical model which defines the movements of an individual as a function of the velocities, positions and orientations of its neighbours (Sakai 1973, Suzuki & Sakai 1973, Huth & Wissel 1989). Thus allelomimesis with identical individuals can structure the school. It could also be behind the observation that many fish, such as surgeonfish (Barlow 1975) and killifish (Fraser 1973), form schools under some conditions, but not under others, without having to invoke factors like genetically different populations.

Similarly, many mammal species such as musk-oxen (Teuer 1965) or sheep and cows live in herds that take different forms under different conditions, such as the presence of predators, or during different forms of activities such as resting, ruminating and grazing. Elephants, bison and quail form defensive circles, and predators often adopt complex attacking patterns to cope with defensive formations. As with fish schools, Jarman & Jarman (1979) propose that the tendency to take the same speed and direction is the major force that allows ungulate herds to be a stable and structured organisation. Focardi & Toso (1987) have modelled this.

Hoffman et al. (1981) describe large mixed flocks of seabirds that form foraging patterns very like those of army ants, via a recruitment mechanism. Double-crested cormorants form coordinated fishing flocks whose form depends on their size (Bartholomew 1942), and Rand (1954) gives a number of different species that form complex collective foraging patterns. It will be a challenge to show to what extent simplicity and allelomimesis could be behind this multitude of group forms.

Collective decisions:

The information centres described in relation to communal roosting or

colonial nesting bird species (Ward & Zahavi 1973) show a strong analogy with social insect colonies. Members of a roost, for example, are thought to be capable of recognising when other members of the colony return from a successful foraging flight and to choose their next flight direction accordingly, this being the equivalent of trail recruitment. Similar complex decisional or spatial patterns have been observed, and can be understood by similar and even identical analyses of the individual kinetics. For example, weaver birds have been shown to switch from a poorer food source to a richer one (De Groot 1980), and pigeons may concentrate on one source, neglecting nearby identical ones (Lefebvre 1983). Again, these are more than just simple aggregation mechanisms.

Another classic example concerns the dances used by honeybees to recruit workers to make a collective selection of the best site to move their nest to (Lindauer 1961, Seeley 1985). Again, non-linear positive feed-back is important, as only one nest must be chosen out of a number of alternatives.

Collective Construction

We have illustrated our article with examples taken from two highly visible collection behaviours, foraging and grouping. However, we would not like to finish without mentioning collective construction. Roads, tunnels, nests, warrens, dams, collective inhabitations, ... Who are the animal architects? In other words, do the principles described above also apply to this aspect of social activity?

At first sight these complicated structures appear highly deterministic, and again it is not surprising that the "plans" have been thought to be explicitly contained in the individual's genetic code. Grassé's (1939, 1959) classic study of the way termites build complex and regular nest structures (completed by Deneubourg 1977, Bruinsma & Leuthold 1978) shows that this is not necessarily the case, and that a large number of pheromone-mediated autocatalytic interactions between random builders (I tend to lay mud bricks where others have laid mud bricks) can lead to the formation of regular and complex physical structures in the complete absence of any planning, either centralised or in the heads of each individual. A similar process has been shown to be behind the construction of bee nests (Darchen 1959, Belic et al. 1986).

Discussion

In our wish to be clear in showing how a consideration of social dynamics can promote the understanding of group behaviour, we have inevitably made some rather provocative simplifications, not least of which being our treatment of individuals as simple identical "molecules". The degree

of individual complexity found in animal groups is of course extremely variable, not only when comparing very different species such as termites and primates, but also when comparing more closely related ones. While self-organisation can and does appear with complex individuals just as with simple individuals, it is evident that it is not the only mechanism active, and that individual complexity or division of labour can also be the source of collective complexity.

The question raised by the variability in individual complexity may be stated in the following terms: At what level does a society's complexity appear, that is to say does it lie within individuals or between individuals? What part of social behaviour must be explicitly coded into the individuals's behaviour, and what part is determined by the interactions between individuals? This problem is not only fundamental to the relationship between an individual and the society, but also underlies the relationship between an organism and its organs, an organ and its cells, and between a cell and its macro-molecules. If we have deliberately stressed here the role of collective complexity in group behaviour, it is because we both wished to show up its multiple and powerful possibilities and because it has hitherto been neglected (with the notable exceptions of Grassé 1939, 1959; Darchen 1959; Lindauer 1961, Wilson 1962; Breder 1976; Seeley 1985).

When feed-back is discussed in animal groups, it is nearly always negative feed-back that is considered and its role is limited to that of a regulatory mechanism, in which fluctuations are damped and equilibrium is the goal, and again social insects provide many examples. For example, Wilson & Hölldobler (1988) review a number of situations in which ants, with only local information, are capable of regulating which type of food the foragers are encouraged to bring back, or the number of individuals of different castes produced. Positive feed-back is only rarely considered.

Generally speaking, the role of positive feed-back is reduced to that of a mechanism that not surprisingly produces exponential growth and spatial/temporal coordination. For example, it allows groups to exploit *en masse* random discoveries (e. g. Sudd 1963, Deneubourg et al. 1983, Towne & Gould 1988), or to form clusters, but nothing more (e. g. Scott 1972, Altmann 1985). In this article we have stressed the fact that competing positive feed-backs very rapidly amplify external or internal fluctuations. The examples we have given show how the resulting patterns and decisions are surprisingly complex and "creative".

Such self-organising societies have a number of properties that compare favourably with a more deterministic organisation, such as sexual division of labour, social hierarchy, caste-regulation (Oster & Wilson 1978), or age-related ethology. Firstly, they can be based on simple individuals, requiring only simple programming and autocatalytic communication.

Large numbers of individuals can thereby be coordinated into collective structures. Moreover, these structures can interact with the environment, allowing different collective behaviour to appear from the same individual behaviour under different conditions. In this way they combine the advantages of simplicity, reliability and adaptability with a considerable economy of genetical coding. Only the limited number of simple rules describing the individual behaviour and interactions need be explicitly coded. It is not necessary to foresee every situation as complex and flexible collective structures are automatically generated from these simple rules.

Because of these advantages, such self-organising algorithms are also destined to play a more active role in what is the major characteristic of our industrial society, namely the use of machines to perform useful tasks. Specifically, the principles governing self-organisation in animal societies can be used in the design of fail-safe distributed control systems for robot teams, and are already being applied to the management of distributed data networks (Gallagher 1977, Merlin & Segall 1979).

In the light of the ideas presented here and the wide range of species to which we have seen they apply, simple causes should perhaps be considered more systematically when seeking to explain complex collective behaviour. It is our belief that any species adopting allelomimetic-type behaviour, for whatever reason, will "unwittingly" and automatically provide itself with a capacity for collective decision-making and pattern formation, and general structuring that far exceeds that of its isolated individuals. Along with the better-documented reproductive and defensive benefits, this is surely one of the major reasons why sociality has flourished independently and many times across all the major animal orders.

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Scott Camazine

Collective Intelligence in Insect Colonies by Means of Self-Organization

The issue: How does colony-level pattern emerge from the activities of individuals?

Colonies of insects are well-organized entities, displaying remarkable collective abilities. For example, the internal temperature of honeybee colonies is precisely regulated to within 0.5° of 35°C, despite wide fluctuations in ambient temperature (Heinrich 1985). Certain African termites build enormous, intricate nests which maintain an even internal temperature and high humidity while permitting adequate oxygen and carbon dioxide exchange with the environment (Löscher 1961). One usually associates these kind of precise regulatory capabilities with multicellular organisms such as mammals whose body temperature, glucose and electrolyte levels, and respiratory exchange are carefully maintained by intricate physiological mechanisms involving circulatory and neural pathways, as well as central processing by the brain.

In this regard, a colony of social insects presents a challenging paradox: The colony exhibits complex patterns of social behavior permitting exact homeostatic adjustments and the creation of complex nest structures. However, the colony appears to lack the requisite physiological machinery and collective intelligence to accomplish these tasks.

The self-organization approach to insect societies shows how higher-order, colony-level complexity arises not through the sophistication and complexity of the individual workers in the colony, but as an automatic consequence of large numbers of simple subunits interacting concurrently. Three examples of self-organizing processes in social insects are discussed: (1) Pattern formation on the combs of honey bee colonies, (2) Collective nectar source selection by honey bees, and (3) Brood sorting in ants. These examples show how simple subunits interacting in parallel and guided by simple probabilistic rules of interaction can generate complex collective behaviors and patterns.

Methods of analyzing the phenomenon

How does the colony efficiently apportion its work force among the var-

ious tasks required for colony survival? How are honey bee foragers able to exploit the best among an array of ephemeral food sources in the environment? How does a swarm of bees discover and agree upon an appropriate nest site from many potential sites in the field?

The colony consists of thousands of loosely assembled individuals each functioning rather autonomously. It is inconceivable that an individual colony member could acquire detailed information about the state of the entire colony and play a central role in monitoring and directing the activities of other colony members. In an insect colony, there are no foreman, consultants or supervisors who gather and process information and organize the colony's activities.

A useful approach to understanding colony organization is to view the insect colony as a complex system. Self-organizing higher-order phenomena may emerge spontaneously in such a system when (1) large numbers of individual subunits (2) using behavioral rules based upon local (versus global) information (3) act simultaneously and independently.

Steps in the analysis of a particular colony-level phenomenon in insect colonies are as follows: (1) Identify an important colony-level process whose mechanism is poorly understood. (2) Identify the individual subunits involved in the process. (3) Identify the informational input to the subunits. (4) Characterize the effect of the input on the subunits. (5) Simulate the hypothesized system for confirmation.

Consider the example of colony thermoregulation of a honey bee swarm. Here the subunits are all individual worker bees in the swarm. The informational input in this case may be the bee's monitoring of its own temperature and that of its immediate local surroundings. The effect of this input is the set of behavioral and physiological activities used by the individual bee.

Through simulation of the system of swarm thermoregulation, one can ask whether the proposed model generates the observed colony-level process. For example, does the simulation result in the observed temperature profile under a normal range of ambient temperatures? If not, this may suggest that further experiments and observations are indicated to verify the accuracy of steps (2)-(4). Computer simulations are crucial at this stage because when large numbers of subunits interact, even in quite simple ways, the outcome is difficult, if not impossible to predict. The output of complex systems is often non-intuitive and unexpected.

A model of a colony-level process is merely a hypothesis stated in a rigorous mathematical manner. But unlike a verbal statement of a hypothesis, it is dynamic in that it actually does something. By simulating the model on the computer, one generates a specific output which serves as a test of the model. If the output matches the colony-level process under

consideration, then one has increased confidence that the original observations and experiments correctly revealed the components of the process and their proper interactions.

Thus, models are tools for testing hypotheses. They are particularly useful since they require that the interactions among all the components be precisely specified. A facile verbal argument can easily hand-wave its way around a complete explanation of a process, but in a computer simulation each component of the process and its interactions must be explicitly defined. Lacking even the smallest detail, a simulation does nothing because a computer program is like a chain. Every link is critical to the integrity of the whole.

This approach to analyzing a colony-level process as a complex system can be characterized as bottom-up rather than top-down. First the components of the system and their interactions are characterized based upon field observations and experiments. Then these components are assembled in a computer model. Once you are confident that nothing crucial has been left out, the model then permits assessment of the significance of each of the system's components and their interactions. It may also provide information about aspects of the internal dynamics of the system that are not readily available through field observations or experiments.

One difficulty with the bottom-up approach is that it requires a very detailed understanding of the components of a process in order to achieve a real understanding of how the colony-level process emerges. For this reason, it is often tempting to use a top-down approach. One starts with the same colony-level phenomenon, but lacks a complete understanding of the components or their interactions either because one is at an early stage of examination, or because some components of the system resist analysis. Instead, one guesses what the components might be and how they might interact. One hopes to hit upon a model that works (reproduces the observed colony-level process). One can then conclude that the proposed model may have some relationship to the actual mechanism. Unfortunately, even if one does come up with a model that gives a good prediction of the colony-level phenomenon, one has little confidence that the proposed mechanism has any bearing on reality because many different hypothesized models may all be capable of mimicking properties of the system.

In practice, one often resorts to a combination bottom-up and top-down approach. Ideally one would study a system in as much detail as possible, extracting as many of the components and interactions based upon direct observation. Gaps in one's knowledge can be filled in with hypotheses. If the model simulation behaves as expected, further obser-

vations and experiments can be designed to determine whether the hypothesized components and interactions are valid.

Examples of Self-Organization in Insect Societies
I will now briefly present 3 examples of the application of this approach:

1) Self-organizing pattern formation on the combs of honey bee colonies;
2) Collective nectar foraging in honey bees;
3) Brood sorting in ants.

1) Self-organizing pattern formation on the combs of honey bee colonies

A honey bee colony comprises approximately 25,000 worker bees and a single queen. In addition to the adult bees there is immature brood, consisting of developing eggs, larvae and pupae, as well as a variable amount of accumulated food, namely honey and pollen. These are stored within the hive in a series of parallel wax combs subdivided into approximately 100,000 cells. A characteristic well-organized pattern develops on the combs, consisting of three distinct concentric regions — a central brood area, a surrounding rim of pollen, and a large peripheral region of honey.

The well-organized pattern suggests a possible adaptive function. A compact brood area may help to ensure a precisely regulated incubation temperature for the brood and may facilitate efficient egg-laying by the queen. The location of pollen in the rim adjacent to the brood area, where it is readily accessible to the nurse bees, may promote efficient feeding of the nearby larvae.

The pattern is not only well-organized, but also consistent throughout the season. This feature may also be adaptive. Each day honey and pollen from tens of thousands of foraging trips are deposited into the cells, stores of honey and pollen cells are continually consumed, hundreds of eggs are laid, and mature adult bees emerge from their cells. Despite the constant turnover, in which cells are often refilled with something different, a stable pattern persists.

The presumed adaptive significance of the pattern raises the important question of what mechanisms account for its origin and maintenance. How does this colony-level pattern emerge from the activities of thousands of bees? I present a model of pattern formation based upon self-organization.

This model is derived from experimentally determined behaviors of honey bees (Camazine 1991). The behavioral rules can be summarized as follows:

(1) The queen moves over the combs rather unsystematically searching for empty cells in which to lay eggs. Approximately 95 % of eggs are deposited within 3 cell lengths of another brood cell. The queen lays between 1000 and 2000 eggs per day.

(2) Once an egg is laid in a cell, it remains in place for the 21 days required for development to the adult stage.

(3) Honey and pollen are deposited in randomly selected cells, either empty or partially filled with the same substance.

(4) The ratio of honey removal to honey input is approximately 0.6. The ratio of pollen removal to pollen input is approximately 0.95. The average ratio of pollen input to honey input is approximately 0.25.

(5) Honey and pollen are removed from cells in an amount proportional to the number of surrounding cells containing brood.

Note that each of these simple rules is based entirely on local information, namely the content of the cell itself and that of its closely neighboring cells. This feature makes the system well suited to incorporate the behavioral rules into a probabilistic cellular automaton model. The model exhibits the characteristic concentric pattern of brood, pollen and honey under a wide range of parameter values. Figure 1 shows the process of pattern formation on a small section of comb from the center of the colony. In the initial stages of the simulation (Figure la) pollen (gray circles) and honey (white circles) are found throughout the comb as bees deposit their loads randomly on the empty frame. At the same time, the queen wanders over the frame from a central starting point laying eggs (black circles) in empty cells near other cells containing eggs. The result is a haphazard mix of honey and pollen, with a central region sparsely occupied with eggs. Many of the cells interspersed among the eggs contain honey and pollen. This is the early disorganized stage. As the simulation proceeds (Figure lb), pollen begins to disappear from the periphery, resulting in a solid region of honey. In addition, both pollen and honey disappear from the center of the comb leaving a compact, roughly circular brood area. A band of pollen develops between the brood and the honey. In this mature stage (Figure lc) the characteristic well-organized pattern appears. How does this transformation occur? Three processes contribute to the pattern formation. First, a compact brood area results from the preferential removal of honey and pollen near brood. This continually provides empty cells in the brood area into which the queen lays eggs. A second process explains the segregation of honey and pollen in the periphery. Since both are deposited randomly, initially both pollen and honey fill the comb. However, since

less pollen is collected than honey and since pollen has a greater turnover rate than honey, pollen cells are more likely to be emptied than honey cells, and these empty cells will be more likely refilled with honey. Gradually any pollen deposited in the periphery is removed, leaving this region almost entirely honey. So, where is the pollen stored? The only cells available for pollen are those with a high turnover rate. These are the cells at the periphery of the brood area. Once a cell is occupied by an egg, it is "reserved" as a brood cell for the next 21 days of honey bee development. But in the interface zone between the central brood and the peripheral region of honey, the preferential removal of honey and pollen continually provides a region where cells are being emptied at a relatively high rate. These cells are available for pollen.

In this self-organizing system, there is no need to specify particular locations for eggs, pollen or honey, nor do the bees require any global knowledge about the developing pattern to which they are contributing. Following a few simple rules based upon local information, the comb pattern emerges automatically through the dynamic interactions of the bees.

2) A model of collective nectar source selection in honey bees

A honey bee colony chooses among different nectar sources in the field, selectively foraging from those which are most profitable. This model, based on Camazine and Sneyd (1991), describes the colony's decision-making process, and consists of a system of non-linear differential equations describing the activities of the foraging bees. Parameter estimates are based on previously published data (Seeley et al. 1991). Numerical solutions of the equations agree closely with experimental observations. A model of the activities of the individual bees is also presented in the form of a one-dimensional probabilistic cellular automaton. This model illustrates how the individual activities of the foraging bees contribute to the overall pattern of colony-level foraging presented in the differential equation model.

We begin with a pool of foragers lacking knowledge of the potential nectar sources available in the field. Each of these bees is called a "follower bee" because she reaches a nectar source by following the dance of a nest mate who has already discovered a patch of flowers. Consider the behavior of one such bee as she begins her day of foraging (Figure 2). The dance floor area of the hive contains bees dancing for different nectar sources.

The follower bee selects a dancer for nectar source A. After following the bee's dances, she flies to that nectar source. Upon arrival, the forager gathers a load of nectar and returns to the hive.

After relinquishing her nectar to a food storer bee, the forager may do one of three things, as indicated by the branch points (diamonds) in Figure 2. First, she may abandon the food source and return to the pool of uncommitted followers. Alternatively, if she decides to continue to forage from the nectar source, she may either perform recruitment dances before returning to her patch of flowers or continue to forage at the food source without recruiting nest mates.

Many factors affect the probability that an individual bee dances for or abandons the food source (Seeley 1986, Seeley et al. 1991): nectar sweetness, distance to the food source, ease of nectar collection, colony intake rate. In this model, for simplicity, we consider nectar sources that differ in quality only with respect to sugar concentration, all other factors being equal.

These features of the colony-level decision-making process can be incorporated into a mathematical model of foragers choosing between two nectar sources. First, we assume that, at any moment, each foraging bee is in one of the seven places (compartments) shown in Figure 3. These compartments are:

H_a: unloading nectar from food source A,
Hb: unloading nectar from food source B,
D_a: dancing for food source A,
Db: dancing for food source B,
A: feeding at food source A
B: feeding at food source B,
F: following a dancer.

The dance floor (shaded area in Figure 3) contains three of the compartments: those bees dancing for A, those bees dancing for B, and those bees following a dancer. In contrast to Figure 2, note that Figure 3 consists of two separate cycles, one for each food source, with the follower compartment, F, the only one shared by the two cycles. Thus, bees from one feeder can switch over to the other feeder only by passing through the dance floor and following a dancer for the other food source. The figure suggests that the dance area plays the central role in the decision-making process. Whatever information is transferred among the bees is assumed to take place here.

Two factors affect the proportion of bees in each of these 7 compartments: (1) The rate at which a bee moves from one location to another, and

(2) The probability that a bee takes one or the other fork at the five branch points (diamonds) of Figure 3. For each of the 7 compartments we specify a rate constant p_{1_7}, in units sec^{-1}. Thus, the average time a bee stays in A is given by $1/p_3$, and similarly for the other compartments. The values for these rate constants are given in Seeley et al. 1991.

We next consider the movement of bees at the branch points. The first branch point occurs after a bee has unloaded her nectar in the hive. Here, a bee may desert the nectar source and return to the dance floor to follow another dancer. The fraction of the bees that abandon their food source (or the probability of any one bee doing so) is denoted by the function f_a, which we call the abandonment function. f_a is a function of the quality of the food source, and thus the fraction, $f_x a$, indicates the probability that a bee, upon leaving H_a, will abandon nectar source A to become a follower. Abandonment diminishes the number of bees committed to a food source and provides a pool of uncommitted bees which follow dancers for one nectar source or the other.

The second branch point determines the proportion of the committed bees that dance for the nectar source they have just visited. Although at the second branch point there is no filtering of bees away from the food source to which they are committed, this branch point affects the probability that an uncommitted forager will follow a dancer for one or the other food source, as described below. The probability that a bee will become a dancer for her food source is denoted by the function f_d, the dancing function. As for the abandonment function, its value depends on the quality of the food source, with $f_d a$ indicating the probability that a bee foraging at nectar source A performs recruitment dances.

The third branch point occurs on the dance floor when bees follow dancers for one or another nectar source. The fraction of the follower bees leaving the dance floor to go to food source A is denoted by the function O, the following function. A bee entering the dance area randomly encounters dancers, and follows the first dancer she encounters. In the situation of just two nectar sources, A and B, the probability of a follower encountering dancers for A can be roughly estimated by $D_a/(D_a + D_b)$ where D_a and D_b are the number of bees in each of the dance compartments A and B, respectively. However, since only a portion of a bee's time in the dance area is actually spent dancing, it is necessary to multiply D_a and D_b in the above expression by the proportion of time that the foragers actually dance. These fractions are denoted by t_o and t_b. Thus $f_a = D_a t_a/(D_a t_a + D_b t_b)$. This fraction takes into account the number of dancers for each food source as well as the time spent dancing, and thus indicates the proportion of the total dancing for each nectar source.

Using the parameter values in Seeley et al. 1991, we can determine how well the model's predictions correspond with the results of actual field experiments. Figure 4a shows the results of an actual experiment (Seeley et al. 1991) and Figure 4b shows the computed solutions of the model.

The differential equation model does not track the activities of individual foragers, making it difficult to appreciate the behavioral pattern of the foragers as they are recruited to nectar sources and abandon other sources. This can be visualized using a probabilistic cellular automaton model based upon the same probabilities of dancing, abandoning, and following used in the differential equation model. In Figure 5, the initial state of each individual forager bee is shown as a square cell in the top row of the figure. Each bee can be in one of 3 states: either committed to feeder A (white), committed to feeder B (black) or uncommitted (gray). Each iteration of the model represents one cycle of a bee's returning to the hive and making a decision to either remain committed to her feeder or to abandon the feeder and become a follower bee. A follower bee goes to the dance area and randomly selects a bee dancing for one or the other feeder. Moving down, each cell in a column displays the activity of an individual bee over time.

As shown in Figure 5, the cohort of foragers quickly "locks in" on the best food source (feeder A, shown as white). This occurs because bees for feeder B tend to abandon their food source and become uncommitted follower bees, and uncommitted bees tend to switch to feeder A. The model demonstrates how differential rates of dancing and abandonment based upon nectar source quality create a positive feedback system that rapidly filters the majority of uncommitted bees to the best food source. The model supports the view that selective exploitation of the most profitable nectar sources occurs through an autocatalytic, self-organizing process.

3) Brood sorting by ants

Leptothorax unifasciatus ant colonies occupy flat crevices in rocks. Their brood is kept in a single cluster arranged in a distinct pattern of concentric rings. Eggs and micro-larvae form the center of the cluster with successively larger larvae arranged in radial bands away from the center of the brood cluster. An exception is the band of prepupae and pupae which are placed in an intermediate position between the largest, most peripheral larvae and the more centrally-located medium-sized larvae. If the patterned arrangement of brood is experimentally disorganized, the ants will re-sort the brood over the course of several hours (Franks and Sendova-Franks 1992).

What is the mechanism of brood sorting used by the ants? The approach is to ask whether brood sorting can be explained by a process of self-organization. In other words, can brood sorting be explained through the behaviors of individual ants each acting autonomously following simple rules based upon local information?

An alternative hypothesis of pattern formation would posit a pattern template which the ants follow. In other words, the pattern forms as the result of ants placing each class of brood in particular locations based upon a pre-specified blueprint or plan. If the ants have some means of knowing where to place each type of brood, the concentric pattern could arise simply as a consequence of the ants following the orderly arrangement latent in the blueprint.

We believe that a self-organization mechanism is not only simpler but also more likely to be the actual mechanism used by the ants in sorting their brood. It does not require global knowledge of the large-scale structure of the brood pattern and requires less intelligence on the part of the individual ant as each employs only simple behavioral rules based upon local information gathered moment to moment during its brood sorting activity.

The following is a description of what is known about brood sorting in these ants with particular emphasis on identifying the "rules of thumb" used by each ant in the process. However, we emphasize that this study is still in its early stages, and thus will require some reasonable hypotheses to fill in the gaps of our knowledge. We hope to refine the model and to eliminate its top-down (speculative) aspects through further observations and experiments (Franks et al., in preparation).

Franks and Sendova-Franks (1992) have made the following observations concerning brood sorting in *Leptothorax unifasciatus*, under conditions in which the ants were encouraged to abandon their old nest and emigrate to a new empty nest consisting of two narrowly separated glass plates:

1) Starting with an empty nest, brood items are initially placed randomly within the new nest.
2) Soon after the onset of brood sorting, ants tend to place brood items near other items, especially if they are of the same type.
3) Even before all the brood items have been carried to a new nest, brood within the new nest may be picked up and relocated, often nearer to larger groups of items of the same type.
4) Items seem to be more frequently removed from small rather than large clusters.
5) Ants constantly re-sort their developing brood from day to day to maintain the characteristic concentric brood pattern.

These observations suggest that brood sorting is a dynamic process based upon large numbers of separate brood item movements occurring concurrently through the actions of many ants. The ants do not appear to utilize a pattern template imposed by environmental gradients such as temperature or humidity.

Based upon these preliminary observations of the behavior of individual ants, we propose a model which incorporates most of these details. It is similar to the previous model presented by Deneubourg et al. (1991) in that sorting is achieved without either hierarchical decision-making, communication between individuals, or any externally imposed template of the pattern. The two behavioral rules of the ants in the Deneubourg model are that (1) if a brood item is isolated, it is more likely to be picked up by an ant, and (2) when carrying an object, the probability that an ant will deposit the item is greater if there are more of the same brood type in the immediate neighborhood.

The rules proposed here are slightly different, but both models incorporate the ideas of parallel (concurrent) activities of autonomous ants following simple rules based upon local information. The two rules programmed into the simulated ants of the Deneubourg model are basically an implementation of observations 2, 3 and 4 above, without any top-down modeling assumptions about the behavior of the ants. Unfortunately, those two rules are insufficient to generate concentric bands of brood, although they do result in the segregation of different brood types.

The model presented here was developed for two reasons. First, it corresponds better with the observed pattern because it generates concentric regions of brood. The key behavioral rule in both models that generates pattern is a positive feedback mechanism in which brood objects are placed near items of the same type. This rule of "like sorts with like" can be thought of as analogous to an attraction or affinity of a brood item with neighboring brood items, as if brood of the same type sticks together with a certain cohesion. To achieve concentric regions of sorted brood, it is necessary to make one additional assumption. We assume that brood of the same type has an affinity for neighboring items of the same type as in the Deneubourg model, but in addition, there is a certain specified, but weaker affinity of a brood item for a neighboring brood item of a different type. This assumption is warranted on the basis of the observation that the ants maintain the brood in a coherent cluster rather than as a series of discrete islands of same type brood. This additional brood sorting rule is only a slight extension of the rule proposed in the Deneubourg model, and does not contradict any of the known behaviors of the ants. Although it was not specifically mentioned in Franks and Sendova-Franks (1992) as an aspect of the behavioral of the ants, it is unlikely to have been noticed

at the level of analysis undertaken in their study. This assumption was also made because it is the basis of a proposed model of cell sorting in developmental biology (Steinberg 1963) that readily yields a concentric structure.

Relating self-organizing brood sorting in ants to a more general process of morphogenesis in developmental biology is the second reason for presenting this model. It is becoming apparent that self-organization approaches are not only crucial for understanding how collective intelligence emerges from the activities of thousands of individual colony members, but also for understanding biological organization in general. Furthermore, developments in one field (such as morphogenesis) may provide insights into other fields.

I simulate the brood sorting process of ants on a 2-dimensional grid of uniform locations. Each grid location can either hold a single brood item or the location may be empty. For simplicity, as in the Deneubourg model, brood items are classified into only two types, eggs or larvae, rather than the 5 types distinguished by Franks and Sendova-Franks (1992). The simulation is initiated with a random array of a specified number of the two brood types. The remaining locations are empty. The simulation consists of a series of brood item movements on the grid. A brood item is chosen randomly but is picked up with a probability that is a function of the number of brood items immediately surrounding it. The brood item is then moved and has a greater probability of being set down the greater its affinity for its nearby neighbors. The measure of the affinity or goodness of fit of the selected brood item with its immediate neighbors is a weighted function of its immediate brood neighbors. In the Deneubourg model, the goodness of fit was assumed simply to be an unweighted function of the number of same type brood neighbors. That was the simplest and most logical assumption to make, lacking any more detailed information. In this model I assume that there is a "cohesion" of eggs for eggs and larvae for larvae as in the Deneubourg model, but in addition I assume that there is also a measurable "adhesion" between dissimilar brood items. Furthermore, the value (weight) of each type of attraction is not equal. The relative affinity values are as follows egg:egg > egg:larva > larva:larva. (In contrast, in the Deneubourg model gives equal weight to each type of interaction: egg:egg = larva:larva.)

Thus the simulation consists of selecting more isolated brood items (with poor fit) and moving them to a new locations where they have a greater affinity to their neighbors. By simply giving different weights to each type of brood interaction, the model readily generates a concentric pattern resembling brood sorting by *Leptothorax* ants (Franks and Sendova-Franks, 1992).

What support is there for this model? Trinkaus (1969) discusses various theories of cell segregation in which a concentric pattern of cells develops from an initially random arrangement of 2 cell types. One test of the validity of a particular cell sorting hypothesis is to compare not only the final result of the hypothesized model (the pattern of cell segregation) but also to examine how the pattern develops over time. As seen in Trinkaus, different hypotheses of sorting yield different intermediate patterns. A prediction of a differential adhesion model such as that proposed here for brood sorting is that, early in the sorting process, islands of brood form, separated by empty spaces. These brood clusters show a tendency for the eggs to cluster within them. As the simulation progresses to the final fully-sorted stage, the clusters coalesce into a single brood cluster with eggs in the center. This pattern of development does not occur with the other mechanisms discussed but does occur in the model of brood sorting by ants. Figures 6a,b show the early and final stages in a brood sorting process governed by differential affinity. (Eggs are represented by white circles, larvae by gray, and empty locations by black).

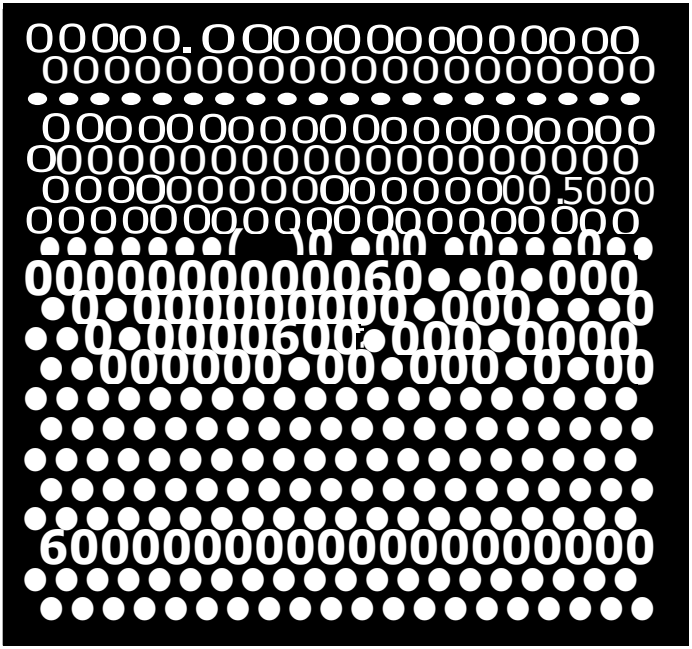

Figure la

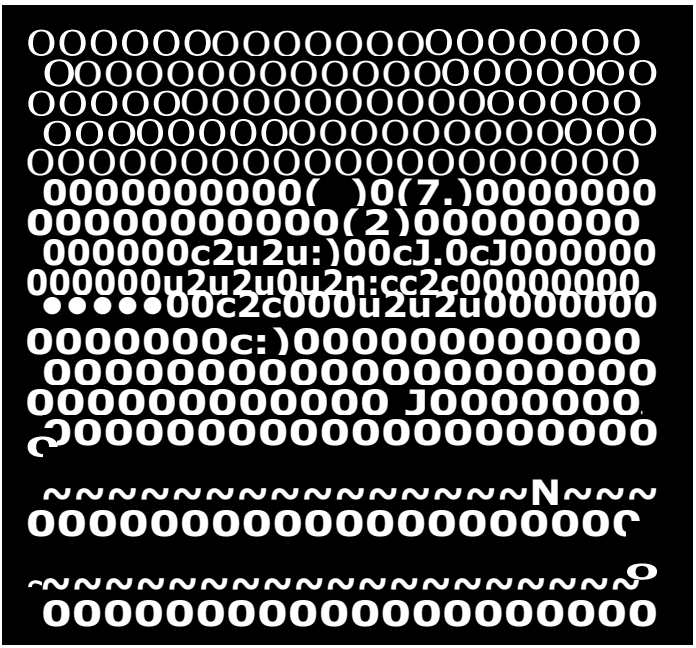

Figure 1 b

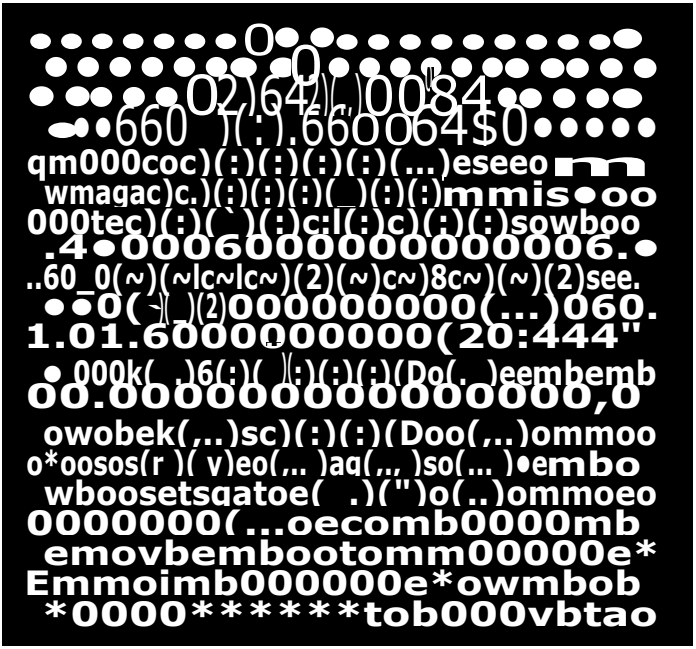
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Figure 1 c

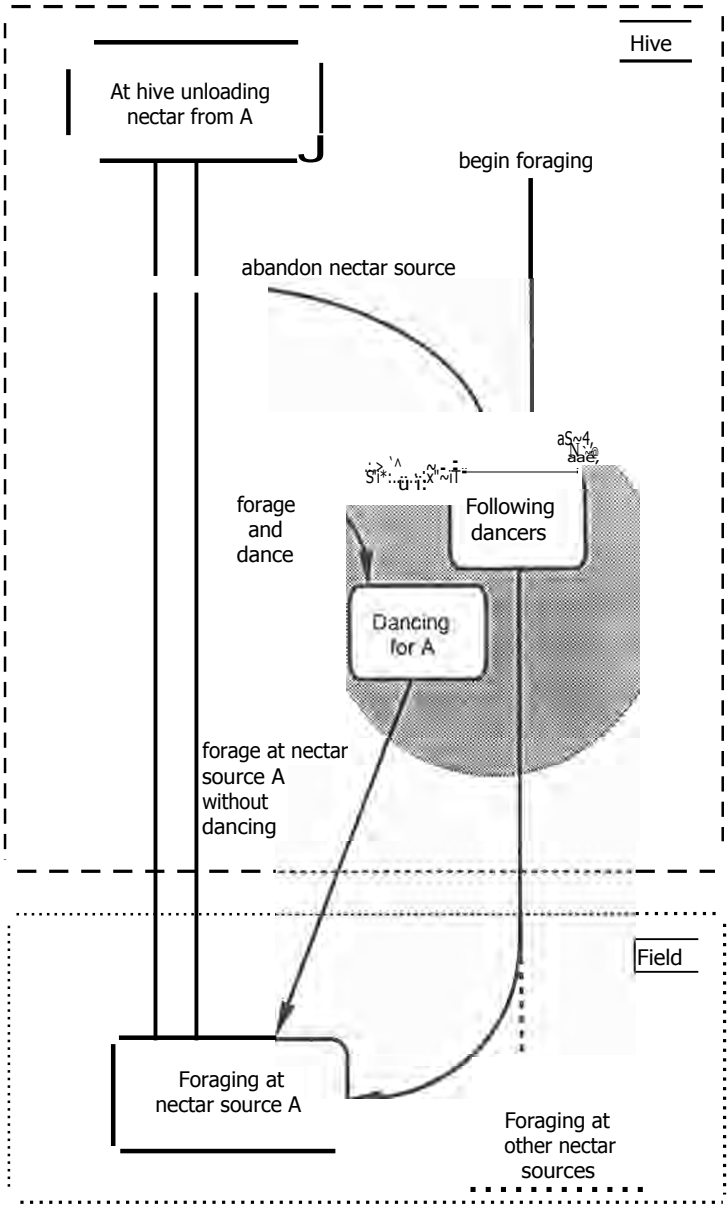
Hive
At hive unloading nectar from A
begin foraging
abandon nectar source
forage and dance
Following dancers
Dancing for A
forage at nectar source A without dancing
Field
Foraging at nectar source A
Foraging at other nectar sources

Figure 2

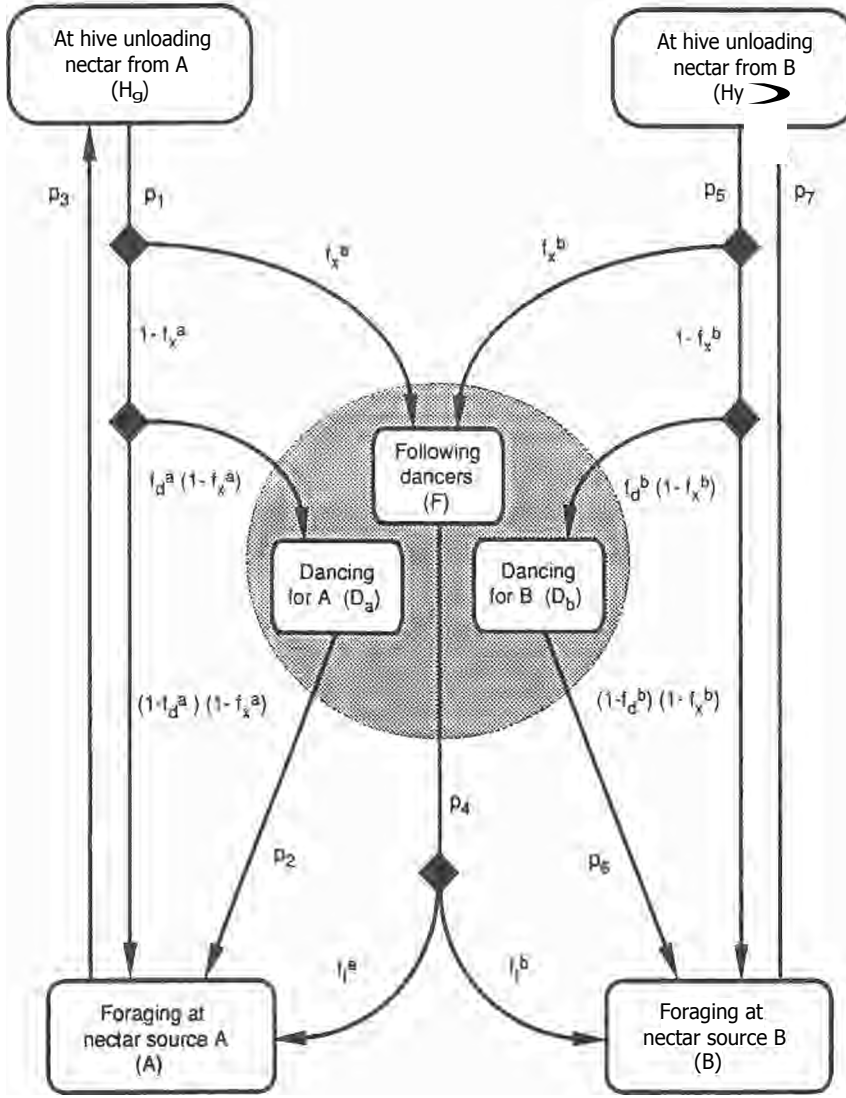
At hive unloading nectar from A (H_a)
At hive unloading nectar from B (H_b)
p_3
p_1
p_5
p_7
f_x^a
f_x^b
$1 - f_x^a$
$1 - f_x^b$
Following dancers (F)
$f_d^a (1 - f_x^a)$
$f_d^b (1 - f_x^b)$
Dancing for A (D_a)
Dancing for B (D_b)
$(1-f_d^a)(1 - f_x^a)$
$(1-f_d^b)(1 - f_x^b)$
p_4
p_2
p_6
l_l^a
l_l^b
Foraging at nectar source A (A)
Foraging at nectar source B (B)

Figure 3

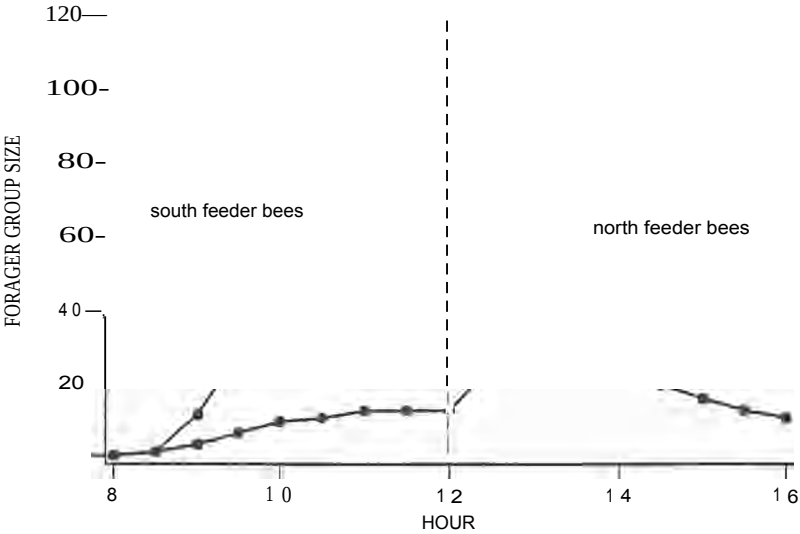
120
100
80
60
40
20
FORAGER GROUP SIZE
south feeder bees
north feeder bees
8
10
12
14
16
HOUR

Figure 4a

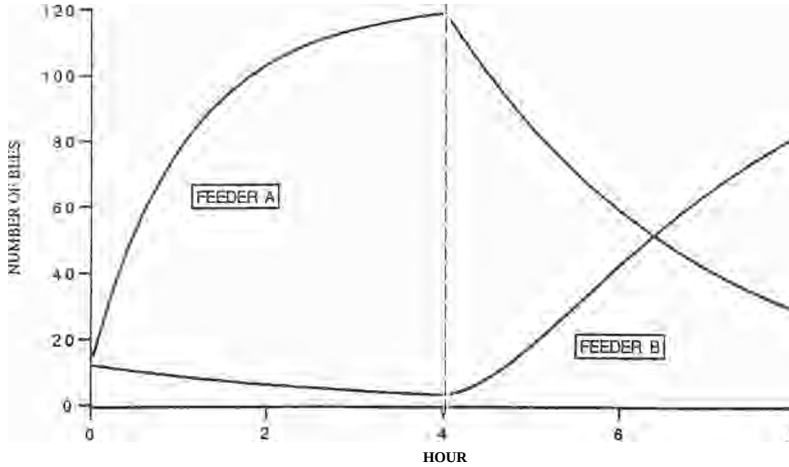
120
100
80
60
40
20
0
NUMBER OF BEES
FEEDER A
FEEDER B
0
2
4
6
8
HOUR

Figure 4b

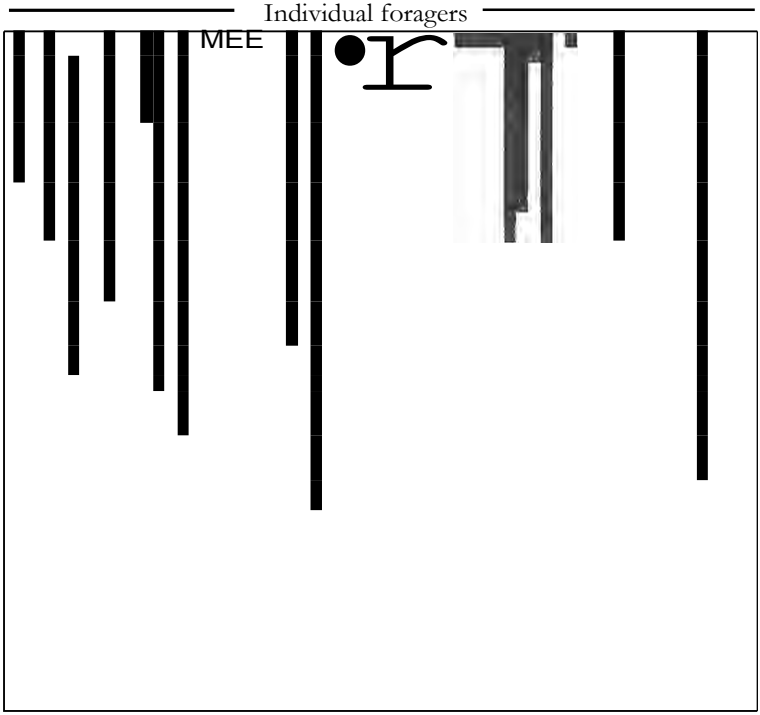

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Figure 6a

Figure fib

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Nigel R. Franks

Collective Intelligence and Limited Rationality in the Organization of Ant Colonies and Human Societies*

"Traditional economic theory postulates an `Economic Man' who in the course of being `economic' is also `rational'. This man is assumed to have knowledge of the relevant aspects of his environment which, if not absolutely complete, is at least impressively clear and voluminous. He is assumed also to have a well-organized and stable system of preference and a skill in computation that enables him to calculate, for the alternative courses of action that are available to him, which of these will permit him to reach the highest attainable point on his preference scale."

"Broadly stated, the task is to replace the global rationality of Economic man with a kind of rational behavior that is compatible with the access to information and the computational capacities that are actually possessed by organisms, including man, in the kinds of environments in which such organisms exist."

[From *A Behavioural model of Rational Choice,* by H. A. Simon, 1955]

New insights for understanding complex systems may be obtained by studying ant societies in the light of certain classical theories of organization. These theories suggest that (a) organisms and organizations are capable only of limited rationality, due to the complexity of the environment; (b) classifying such environments is important to understanding the scope of adaptive decision-making; and (c) societies operating in complex environments may usefully resort both to a redundancy of parts (individuals) and a redundancy of functions (behavioural flexibility) to

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promote their survival. These ideas are illustrated by a few diverse examples from ant societies.

I introduced the term Collective Intelligence (Franks 1989) to draw attention to the collective abilities of social insect colonies. Such societies have collective problem-solving and decision-making capabilities beyond the scope of their individual members. The study of adaptive decision-making by animals in their natural environment is known as behavioural ecology. In this article I look at collective intelligence in insect societies in relation to both classical behavioural ecology and general theories of organization and I consider how societies operate in environments where they can exhibit only limited rationality.

Behavioural ecology is concerned with the causes and consequences of decision-making (in its broadest sense) by organisms in their natural environment. It is the general theme of this paper that behavioural ecology should be able to provide insights for the development of artificial life technologies such as robots (or teams of robots) for work in complex and partly unpredictable environments (see for example Deneubourg et al. 1990; 1991).

However, I also suggest that mainstream behavioural ecology, especially as applied to social insects, whilst taking certain insights from human economic theory, has largely missed other equally important insights, namely from the general theory of organisations (see e.g. Morgan 1986), which should enrich both behavioural ecology and studies of artificial complexity.

Many, if not all, of the decision-making systems behavioural ecologists explore are concerned with the organism's choice of alternative patterns of investment of limited resources (including time and energy). Given this preoccupation with resource allocation, it was almost inevitable that behavioural ecologists should turn for inspiration to economic models of investment strategies developed in the humanities. The most obvious example is cost/benefit analyses involving optimization principles. For example, the marginal value theorem was developed as a guideline for investment by firms (see e.g. Intrilligator 1971) — but the same theory can also be applied to calculate the optimal time for organisms to give up foraging in one depletable patch and move on to another in patchy environments (Stephens & Krebs 1986).

The application of economic optimization theory in behavioural ecology has been both extraordinarily successful and rather controversial. It has been successful as a modelling methodology for showing quantitatively some of the huge diversity of sophisticated behavioural adaptations of organisms to their natural (including social and sexual) environments. It is controversial because it can be regarded as bordering on the naive —

appearing to invoke omniscient organisms (or at least an omniscient evolutionary process). This best of all possible worlds is hardly compatible with a long-term view of the evolution as a blind process in which 99 % of all organisms have become extinct (Wilson 1992, p. 192). The strengths and weaknesses of optimization models in behavioural ecology have been debated at considerable length (Oster Wilson 1978, Gould & Lewontin 1979, Stephens & Krebs 1986, Parker & Maynard Smith 1990, Stearns 1992). The heat of this debate, which shows little sign of abating, is only really useful if it causes the protagonists to clarify the issues and, more importantly, if it encourages the exploration of alternative approaches.

The alternative approach of this paper is to explore the ways in which complex environments limit rational decision-making and how societies of organisms are sometimes better at making certain kinds of adaptive decisions than solitary organisms, even though societies also have a limited rationality.

I will begin by briefly reviewing an important body of literature concerning decision-making and the structure and function of organizations that has had relatively little influence on the growth of ideas in behavioural ecology.

The importance of the work of such luminaries as Herbert Simon and Fred Emery for understanding the limitations of patterns of organization in human economic and social systems, which began in the 1940's and 1950's, has been recognized within the humanities (Herbert Simon was awarded the Nobel prize in economics in 1978). Indeed, it continues as a mainstay of many social scientists' approach to the development of management schemes and studies of administration (Morgan 1986). However, I have been able to find rather little evidence of the impact of their ideas in biology (for two brief citations of Simon by behavioural ecologists, see Stephens & Krebs [1986] and Seeley [1989]). This comparative neglect is unfortunate and surprising because Simon and Emery and their colleagues have been particularly concerned with decision-making by individuals or organizations who have access only to limited information in highly complex environments. As such, their work is highly pertinent to behavioural ecology, especially of social organisms whether or not they are natural or artificial life forms.

Limited Rationality as a Consequence of Environmental Heterogeneity

It is all the more surprising that Simon's work has been largely neglected by behavioural ecologists because one of his influential papers (Simon 1956) is centred on a detailed model of a hypothetical organism foraging for food upon which its survival depends. For this reason, I suggest that it is appropriate to recognize Simon as one of the first behavioural ecol-

ogists. However, the fundamental difference between Simon's approach and that of latter day behavioural ecologists is that Simon developed and used his model to show how the decision-making capabilities of organisms (including man) can be just as severely limited by environmental constraints as by behavioural constraints (Simon 1955). It was to emphasize that all economic organizations have limited rationality that Simon (1956) formulated his classic model. (Before detailing this model it is worth recalling that in the 1950's the term behavioural ecology had not yet been coined, indeed two of its founding fathers, Niko Tindbergen and David Lack, were just beginning to provide its foundations [see Krebs & Davies 1987]).

The mathematical model Simon (1956) proposed was simple and elegant, yet it still has some counter-intuitive lessons of value today. Simon imagined an animal inhabiting a flat environment consisting of a network of paths. The organism has a single requirement, food, and engages at any one time in only one of three activities: resting, exploring and eating. Packets of food are rare and they occur at random nodal points in the network of paths. Each food depot is sufficient for one meal. A meal gives the animal the capability to move around the network. It moves and metabolizes at fixed average rates and is able to store a certain amount of food energy, so that it needs to eat at certain average intervals. The organism can perceive a circular portion of its environment, this "vision" enables it to move towards a meal if one lies within its planning horizon. The problem for the organism is to choose its path in such a way that it will not starve.

The organism behaves as follows:

(1) it explores the network at random looking for a meal:
(2) when it sees one, it proceeds and eats:
(3) if the total consumption of energy during the average time required, per meal, for exploration and food-getting is less than the energy of the food consumed in the meal, it can spend the remainder of its time in resting.

Consider that the proportion of nodes in the network where there is food is p, and $0 < p < 1$. An average of d paths lead from each node. The organism can see v moves ahead, that is, it would move through a series of nodes in the network to reach a meal v moves away. H is the maximum number of moves the organisms can make between meals without starving. At any instant, the organism sees d nodes, one move away, d^2 nodes two moves away, and in general, dk nodes k moves away. In all, it can see $d + d^2 + \quad + d^\circ = (d/d\text{-}1)(dv\text{-}l)$ nodes.

When the organism makes one move it sees d' new nodes: m moves

reveal md^v new nodes. The probability, Q = 1-P that it will not survive will be equal to the probability that no feeding positions will be visible in (H-v) moves: this follows because it can make a maximum of H moves, and v of these will be required to reach food it has discovered on its horizon. Because p is small, the possibility that food will be visible inside the organism's planning horizon on the first move can be disregarded.

Let p be the probability that none of the d^v new points visible at the end of a particular move is a food point.

Then:

$$1\text{-}P = Q = p^{(H\ v)} = (1^{-p)(H\text{-}v)dv} \quad (2)$$

Hence, the probability of this simple organism's survival, from meal to meal, depends on four parameters, two that describe the organism and two the environment: p, the richness of the environment in food; d, the richness of the environment in paths; H, the storage capacity of the organism; and v, the range of vision of the organism.

Simple numerical substitution reveals the relative importance of these parameters. Assume that p is 1/10,000, (H -v) is 100, d is 10, and v = 3. Then the probability of seeing a new feeding node after a move is 1 — p = 0.095. (From equation (1) $p= (1^{-}1/10000)^{100^{c}}$). The probability of survival P from substituting into equation (2) = 0.9995. Hence there is in this case only one chance in 10,000 that the organism will fail to reach a food point before the end of the survival interval. Suppose now that the survival duration (H-v) is increased one-third, that is, from 100 to 133. The probability of starvation is now reduced to less than one chance in 100,000. A one-third increase in v (from 3 to 4) has an even greater effect, reducing the probability of starvation from one in 10^4 to one in 10^{40}

Using the same values, p = 0.0001 and (H-v) = 100, the probability of survival if the organism behaves completely randomly can be estimated to 0.009. In this case $P' = [1-(1-p)^{100}] = 0.009$. In other words, the organism's perceptual powers multiply by a factor of roughly 900 the average speed it takes to find food.

In summary, Simon's (1956) model shows beautifully the relation between the range over which an organism can perceive its environment, its energy storage capacity, and its ability to survive. Simon's (1956) concluding remarks are very revealing:

"Since the organism, like those of the real world, has neither the senses nor the wits to discover an `optimal' path— even assuming the concept of optimal to be clearly defined — we are concerned only with finding a choice mechan-

ism, that will lead it to pursue a `satisficing' path, a path that will permit satisfaction at some specified level of all its needs.«

Simon's (1956) suggestion that organisms should suffice rather than optimize has been criticised by Stephens & Krebs (1986). Indeed, given the competitive struggle for existence through evolutionary time, the efficient should replace the merely sufficient, but Simon's major point was, and surely remains true, that the environment is very likely to set many of the limits to rational decision-making and the scope for adaptive responses. For this reason, the technologist's dream of making machines (or teams of machines) with some attributes of social life is always likely to be constrained by environmental complexity. Simon's contemporary, Emery (1967) explored this issue in certain ways that might also be useful in behavioural ecology and artificial life.

The Issue of Environmental Complexity

The reason I have considered Simon's (1956) foraging model in detail is not just for its simplicity and clarity (or because its originality has been overlooked by most behavioural ecologists), but because it helped to support a useful appraisal of environmental complexity by F. Emery (1967). Even more usefully, Emery's analysis in turn leads to some suggestions that I believe are useful in studies of behavioural ecology, especially for students of social insects, and that may, therefore, yield some intriguing insights for artificial life.

F. E. Emery's (1967) Classification of 4 Levels of Environmental Complexity:

1) The Type 1 Environment

This is a placid environment in which there is a random distribution of resources and only one player. It is the environment of Simon's (1956) model organism. Since the resources are randomly distributed and are not dynamic, there is no pattern and nothing to learn. As Emery (1967) points out, in such environments there is no distinction between strategy and tactics.

2) The Type 2 Environment

These environments are more complex but still placid (as in type 1) and there is only one organism or player. But here resources are patterned in space. Hence the organism can have an "optimal" or at least a better than average location. In such environments learning can be useful.

3) The Type 3 Environment

This kind of environment is like that of type 2 except now there is more

than one organism. Such players may compete for limiting resources and best locations. Competitive behaviours and strategies, e. g. territoriality, may play a role in such environments.

"The distributed reactive environment ... A Type 2 environment in which there is more than one system of the same kind, or, to put it another way, where there is more than one system and the environment that is relevant to the survival of one is relevant to the survival of the other."

4) The Type 4 Environment

Emery's called his type 4 environments "Turbulent Fields". These are like type 3 but unlike type 1 and 2 because the dynamics come from the environment itself as well as resulting from the dynamics of the players. Good examples of a type 4 environment or turbulent field are mobile growing or declining prey populations, which are dynamic environments for predators. Such turbulent field environments are so complex and predictably unpredictable that management policy is extremely difficult; marine fisheries are a case in point (Allen and McGlade 1987).

The realization that real environments for human organisations and companies are type 4 turbulent fields might explain why the economist's understanding of microeconomics — the theory of the idealized firm — is so complete while understanding of macroeconomics — the overall economic environment — is so incomplete. Similarly, we might understand the foraging strategy of one organism quite well but understand rather poorly the dynamics of the ecological community of which it is a part.

Such a classification of environment types and their effects on limiting rationality, following Simon and Emery, might be useful in itself. Even more useful, however, are Emery's suggestions for ways to cope with turbulent field environments. These are outlined in detail in Emery, so I will consider only a subset of these ideas, those that I consider to be most useful and provocative for understanding ant colonies and potentially for organizing teams of robots. Chief amongst these is the positive role of redundancy in adaptable organizations.

Emery's (1967) first line of reasoning about how organisms may cope with complexity is what I would term "resorting to ignorance". That is, an organism simplifies its turbulent environment by either not sensing or by ignoring its complexity. This may seem trite and trivial — but ignorance is one of the key adaptations in all our lives (cf. Dante's free man *intra due cibi).* Recall the story of the silly ass (a donkey !) which starved to death because it found itself equidistant from two identical piles of equally attractive and nutritious hay (this sophism is often attributed to the 14th century French scholastic philosopher and logician Jean Buridan, but is not found in his works). Death by indecision would be almost infinitely

improbable for a real organism in a natural environment partly because it would be too ignorant to act so stupidly. Ignorance or superficiality are seen by Emery (1967) as the defence of an organization (or an organism). against overly complex environments. Referring to such defences in general he suggests *"they tend to fragment the spatial and temporal connectedness of the larger social systems and focus further adaptive efforts on the localized here and now"*. A good example of adaptive ignorance is the blind leading the blind in army ant raids (see later in this paper).

Emery's second line of reasoning concerns the utility of redundancy. The first clause of the following quote provides a good working definition of artificial life.

"In designing an adaptive self-regulating system one has to have built-in redundancy or else settle for a system with a fixed repertoire of responses that are adaptive only to a finite, strictly identified set of environmental conditions. This is an important property of any system, as an arithmetical increase in redundancy tends to produce a log-increase in reliability. The redundancy may be achieved by having redundant parts but then there must be special control mechanisms (specialized parts) that determine which parts are active or redundant for any particular response. If the control is to be reliable it must also have redundant parts and the question of a further control emerges. In this type of system, reliability is bought at the cost of providing or maintaining the redundant parts, hence the tendency is toward continual reduction of the functions and hence cost of the individual part. The social system of an ant colony relies more upon this principle than does a human system, and a computer more than does an ant colony. The alternative principle is to increase the redundancy of function of the individual parts. This does not entail a pressure toward higher and higher orders of special control mechanisms, but it does entail effective mechanisms within the part for setting and re-setting its functions — for human beings shared values are the most significant of these self-regulating devices. Installing these values of course increases the cost of the parts. The human body is the classic example of this type of system although it is becoming more certain that the brain operates by means of overlapping assemblies based on similar sharing of parts." (Emery 1967, page 230).

Emery above is considering hierarchical control, with a single control unit; our recent research on ants suggests that they have distributed self-organizing control (see Deneubourg et al. 1989, Franks 1989, Franks et al. 1990, 1991, 1992). This interpretation implies that all worker ants are, at least initially, remarkably similar to one another. The problem of having too many middle managers and not enough shop floor workers is over-

come because each ant is its own boss and its own workforce. In this way, the ant society combines the benefits of reliability and flexibility by having both redundant individuals (parts) and individuals with redundant abilities (functions). One natural consequence of examining the mechanisms of ant social cohesion is that we can begin to understand their limited rationality in decision making. The next section of this essay will consider some real-life and theoretical examples of the collective intelligence and limited rationality of ant societies.

Examples of limited rationality, responses to complex environments and solutions through redundancy in ant societies

a) The blind leading the blind

Deneubourg et al. (1989) developed a model, later tested in the field by Franks et al. (1991), which showed literally how the blind could lead the blind in army ant raids. Their model proposed that the huge and complex army ant foraging systems* could be constructed by identical individuals using very simple rules such as "Follow the strongest available pheromone trail and lay your own trail wherever you go". This positive feedback means that traffic attracts traffic. Equally important, ants returning with food from the raid front use the same rules, but lay even more trail on the homeward journey. Hence homebound workers alter the course of outward bound foragers and *vice versa.* In this way the discovery of food has a major influence on the pattern of raiding. Army ant raid patterns read and respond to the foraging environment. Such event-driven or, more correctly, circumstance-driven systems have interesting properties. For example, differing army ants that recognize particular prey with a characteristic spatial distribution will have differing foraging patterns, even though they may have identical communication systems.

But such collective systems have limited rationality. If an army ant colony discovers large packets of prey, such as other ant nests, that are few and far between, it will produce a distributed foraging system consisting of many small sub-swarms. Highly mobile arthropods are unlikely to fall prey to such small swarms because when they jump in a random direction they are unlikely to land back in a swarm and be captured, and hence such potential prey will not influence the raiding pattern. Thus when a colony starts to prey on other ants it is likely to continue to search with small distributed swarms and it will have little ability to switch to other prey. Such collective systems can become locked onto a particular search image. Compare this with a colony that finds many small and frequent prey and

* See Figure 1 on p. 263 in this volume.

so maintains a large, dense, cohesive swarm. Such a colony will continue to capture small mobile arthropods, but it may be able to switch to raiding ant nests. For some colonies, once they have started to read the environment they cannot continue to assess it without prejudice. It seems possible that army ants periodically stop raiding in part to re-initialise their foraging system, allowing them to reconsider the possibilities. Indeed is sleep in general a mechanism for re-booting software ?

b) Selecting the shortest path

In a series of pioneering experiments and models, Goss et al. (1989) and Beckers et al. (1992) have shown that certain ant colonies are able to select the shorter of two paths, even though no single individual has necessarily walked and compared the two routes. This system works like the blind leading the blind through the positive feedback of recruitment communication. Ants that walk the shorter path get there and back sooner, influence their nest mates sooner and so on. Traffic attracts traffic and the longer trail may be completely discarded.

To explore the scope of such collective decision-making, Stickland et al. (1992, 1993) modelled the abstract algorithms of this type of recruitment, including selection of the richer of two food sources either equidistant or asymmetrically located from the colony. They assumed, realistically, that the strength of recruitment can be proportional to resource quality. They were able to show that, depending on the recruitment system, a colony could make a very accurate decision, i.e. choosing with few errors the marginally richer of two food sources, or it could make a rapid decision to concentrate its efforts on one or the other food source. However, there is a trade-off between rapid decision-making and accurate decision making: a recruitment system that makes rapid decisions tends to make inaccurate ones and *vice versa.*

It is possible for a colony to make the wrong decision particularly if (a) it has a small population, (b) foragers leave the nest at infrequent intervals and (c) if each forager has a large influence on the behaviour of its nestmates. Here a worker may make the longer trip before another one has had an opportunity to make the shorter one. These findings imply that a colony's decision-making is likely to change purely as a function of its populations size, even though individuals do not change their communication system. Non-linear systems such as these change their collective behaviour as a function of the number of interacting units. A small group of interacting robots may behave very differently to a large group of interacting units. The problem becomes even more intriguing when different qualities of food are available at different distances. One very important result is that colonies may be completely unable to concentrate their foraging

efforts on a food source that is beyond a certain threshold distance, no matter how rich it is, because a weaker but closer food source will capture all the traffic. In other words, any decision-making system of this kind sets a limit on free exploration (Stickland et al. 1993). Other examples of non-linear decision-making systems in ants may be found in Franks et al. (1990), Hatcher et al. (1992) rhythms and mutual exclusion; Franks & Sendova-Franks (1992) sorting algorithms; Franks et al. (1992) building behaviour; and Tofts & Franks (1992) division of labour.

In conclusion, we should expect societies of people, robots and ants to have limited rationality. In the first attempts to co-ordinate distributed artificial life systems, it seems appropriate to relax the engineer's and certain economist's demands for optimized efficiency and be content with systems that initially suffice. As it is in evolution, optimization can be a distant ideal, but in the technology of artificial life, just as in the blind process of natural selection, the theoretical "best" should not be the enemy of the sufficiently good.

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