
Contents

K. Lorenz/K. Okawa/K. Kotrschal	108	Non-anonymous, Collective Territoriality in a Fish, the Moorish Idol (<i>Zanclus cornutus</i>): Agonistic and Appeasement Behaviours. Unpublished manuscript by Konrad Lorenz from February 1979
Anthony Chemero	136	Teleosemantics and the Critique of Adaptationism
Helena Knyazeva	145	The Synergetic View of Human Creativity
Armando Aranda-Anzaldo	156	On Natural Selection and Hume's Second Problem
Marie-France Chevron	173	Man's Special Position in Nature. The Relationship between Biological and Cultural Development
Simon Winter	185	Evolution, Categorization and Values
	197	Zusammenfassungen der Artikel

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Non-anonymous, Collective Territoriality in a Fish, the Moorish Idol (*Zanclus cornutus*): Agonistic and Appeasement behaviors

Unpublished manuscript by Konrad Lorenz from February 1979

About this text

This is an unusual paper, based on an unpublished manuscript by late Konrad LORENZ (1979) on the relationships between behavioral and social organization in *Zanclus cornutus* (Zanclinae, Perciformes), the Moorish idol or, in Hawaiian, the Kihikihi. This fragment describes the results of observations during 1976 and 1977 in his large (4 × 4 × 2m) reef tank. At the time of LORENZ' death in 1989, these observations were left in the drawer as were his plans to condense his lifelong observations into a book on "The Biology, notably Ethology of Perciform Fishes". It seemed appropriate to introduce, discuss, and summarize this long text. The approach and results are genuine LORENZ, with some additions and comments by OKAWA, whereas the interpretation is by KOTRSCHAL, who is also to blame for potential errors.

Publication seems worthwhile, because the text is both, scientifically and historically remarkable. If a fragment by late Ludwig VAN BEETHOVEN would be discovered, it's publication would be unquestion-

Abstract

*Observations in aquarium and field indicated that the Indo-Pacific coral reef fish *Zanclus cornutus* (Z), the Moorish idol (Zanclidae), is territorial after recruitment, but later roams a common territory together with conspecifics. The behaviors associated with this ontogenetic change were observed between April 1976 and July 1978 in a 4 × 4 × 2 m reef tank at Konrad LORENZ' home in Altenberg, Austria. A detailed description of these events in German by LORENZ from February 1979 was condensed into an extended English summary by K. KOTRSCHAL, who, with the help of late LORENZ' masters student K. OKAWA, also added the English framework of this paper. After introduction into the aquarium in February 1977, 5 postlarval fish first established territories at the bottom and walls of the tank, but during the following months, dyads started fusing their territories. Finally, fish roamed the tank together as an individualized group. Not reported so far from perciform fishes was a spectrum of "appeasement" behaviors, which were dyadically performed during and after breakdown of territorial borders. A few of these behaviors, such as "benthic pseudospawning" and "eel-like-swimming" may have been ritualized from sexual behaviors. "Appeasement" behaviors are usually attributed to species with considerable social cognitive abilities. Therefore, their presence in a fish is most remarkable.*

Key words

Aquarium observations, coral reef fish, dyadic interactions, ethology, social behavior, territoriality, ontogeny.

able. Konrad LORENZ was, in a way, the Beethoven of early ethology. The text shows, that he carried his workstyle, his passion for keen and long-term observations well into an advanced age (he was 74 when he started this project in 1976).

Second, and even more important, these observations by LORENZ are scientifically relevant. Biologists and ecologists interested in community organization and social structures of tropical reef fishes, generally do not report behavioral mechanisms (but comp. references in REESE, 1991), mainly because these are extremely difficult to observe in the field. This is particularly true for fish such as *Zanclus*, which may roam large areas and where infrequent behaviors may still be of crucial importance. As usual for LORENZ, results are primarily observational and qualitative, but in the

present case, he quantified interactions and performed a few experimental confrontations as well. His accurate descriptions allow a number of conclusions to be drawn. Specific and persistent dyadic relationships, for example, leave hardly an alternative explanation to individual recognition in these fish.

LORENZ' passion for fish can be traced back to childhood. His obsession with reef fish began in the late 1950ies at the MPI-Seewiesen where he had the resources to keep these splendid, colorful fishes in relatively large tanks. His interest in aggression provided the major scientific motive. In his book "On aggression" (1963), fish indeed play major roles. LORENZ quickly learned, that most agonistic interactions between reef fishes occurred among members of the same species (ZUMPE 1965). On that basis he developed a hypothesis that the ultimate function of what he called "poster colours" in the highly visual tropical reef fishes was to signal the occupation of a territory. On the proximate level, he interpreted these colours as sign stimuli triggering aggression (LORENZ 1962). Even though much research was subsequently provoked by this controversial paper, the question of the function(s) of poster colors remained a topic of research interest (Brockmann 1973; NEUDECKER 1989) and evidence was found for, as well as against LORENZ' hypothesis (FRICKE 1966; NEUDECKER 1989).

LORENZ interest for the Moorish idol began during this period. This beautiful fish is partly territorial after recruitment, but may change to schooling during certain phases of its life history in response to ecological conditions. This pattern is not unusual among reef fishes (BARLOW 1984; EHRLICH 1975; LOWE-MCCONNELL 1987; REESE 1991; ROBERTSON/GAINES 1986). From preliminary aquarium observations in Seewiesen, the idea emerged, that these fish may recognize each others individually. LORENZ went to Hawaii in 1967, where he performed the observations and field experiments mentioned in his text. Following his retirement from Seewiesen, in 1973, LORENZ turned his share of the money of the Nobel Price into a huge reef tank, which he stocked with different tropical reef fishes, including *Zanclus*.

LORENZ's original Text is in German and is presented here as it was found, except for corrected typing errors. Replacement of missing words as well as a few comments by KK were marked by []. Judged by the usual style and language standards of LORENZ's publications, this is probably an early draft from his original protocols. To preserve authenticity, no attempt was made to alter style, remove redundancies, etc. Instead of a literal translation of this text, an extended English summary (by KK) is provided. According to LORENZ's correspondence with friends and colleagues, this manuscript was probably intended for publication in the "MPG-Spiegel" or in the "Naturwissenschaften", but, for unknown reasons, was never finished.

The original manuscript by LORENZ covers some preliminary observations from Seewiesen and from fieldwork in Hawaii, but it mainly reports in detail the complicated social dynamics among a number of *Zanclus* in his huge Altenberg reef tank, between April 1976 and August 1977. Observations and experiments were continued by LORENZ and coworkers, notably by his masters student Keiko OKAWA into the early 1980s. Parts of these results are included into the extended English summary (below).

Life history changes in social organization of tropical reef fishes

Virtually all marine reef fishes face the problem of a major change in lifestyle when metamorphosing from planktonic larvae into substrate-orientated juveniles (JONES 1991; SMITH 1982). Small and therefore, highly vulnerable to predators, recruits must squeeze into whatever spaces are available at the reef, between or even within adult territories or they recruit to lagoon habitat nurseries and change to the forereef later (SALE 1969). Striking differences in poster coloration between juveniles and conspecific adults (Pomacentridae, Pomacanthidae) are common in the tropical reef fishes and may ease settlement.

Among the discussed functions of poster coloration, crypsis (LONGLEY 1917), territoriality (LORENZ 1962), species recognition (ZUMPE 1965), signalling to predators is certainly among the better-supported hypotheses (GOSLINE 1965; NEUDECKER 1989), even though it certainly does not exclude the others. Avoidance of predation in these conspicuous fishes seems to be crucially dependent on their knowledge of the local topography, which may even be advertised to potential predators by their aposematic (poster) colours (EHRLICH et al. 1977; GOSLINE 1965; KOTRSCHAL 1987). Field observations indeed suggest that Chaetodontids, for example, may form cognitive maps of their territories (REESE 1989). One may well assume that this is an ability common in fish with similar lifestyles. This crucial learning of topography can only be achieved by post-recruits by expanding their radius of action from their initial shelters.

Whether fish stay territorial, solitary, or as monogamous pair, or switch to temporary or permanent schooling, seems to depend mainly on food densities or the need to overcome the defence of competing territory holders (BARLOW 1984; FOSTER 1985; ROBERTSON et al. 1976, 1979; TRICAS 1989). Some species, such as the algivores among the relatively small damselfish (Pomacentridae) stay on their moderately

sized and vigorously defended territories throughout life. Others, such as butterflyfish and angelfish (Chaetodontidae and Pomacanthidae) occupy more extended territories, probably because they feed on low-energy items, such as coral tissue, sponges and even calcareous algae (RANDALL 1967, HOBSON 1974). Long-term monogamy is common in fish, which defend feeding territories, such as many surgeonfish and butterflyfish (BARLOW 1984, REESE 1991, ROBERTSON/GAINES 1986). In surgeonfish it seems, that post-recruits are first territorial, but later school (EHRlich 1975). Also LOWE-McCONNEL (1987) mention that, for example, some surgeonfish and damselfish, begin as territorial post-recruits, but tend to school later on, when they roam comparatively large "...home ranges..." which are "...defended against intruding conspecifics and heterospecifics..." (defended home range are, by definition, territories).

Thus, poster-coloured reef fishes may be able to switch from territoriality to schooling. In terms of LORENZ' ethological theory (1978), this means that the same sign stimulus (i.e. poster colors) may trigger different behaviors, depending on ecological and motivational context. Even when LORENZ's poster colouration hypothesis is basically correct (LORENZ 1962: poster colours are sign stimuli, triggering aggression in territorial fish), this adaptive flexibility of individuals may provide one explanation, why field work led to contradicting conclusions (EHRlich et al. 1977, EHRlich/EHRlich 1982, FRICKE 1966). Whereas the ecology of territoriality and schooling received broad attention by scientists (for example, JONES 1968, SALE 1980, 1991), reports on behavioral mechanisms remained scarce, (EHRlich 1977; EHRlich/EHRlich 1982; ERLACH 1986; MOTTA 1983; MOYERS et al. 1983; REESE 1975, 1991; ROBERTSON et al. 1976, 1979; SALE 1969; SCHOBBER 1988; ZUMPE 1965).

LORENZ intended to describe the behavioral mechanisms involved in this switch from territoriality to group swimming in Z and how they form individualized schools which roam common territories. Predictions are, that small or unfamiliar individuals should be more aggressive and territorial than large or familiar ones. Also, within equally sized cohorts more aggressive interactions should be found than between such cohorts, because competition potentially increases with similarity in size. LORENZ' aquarium observations reveal a potential of Z to form a surprisingly complex social organization, reminiscent to what primatologists would call a fission-fusion system (e.g. DE WAAL 1989). Clearly, field investigations are needed to test the working hypotheses obtained from these aquarium observations.

Materials and methods

Observations on *Zanclus cornutus* were performed by LORENZ in his big reef tank in Altenberg (North-west of Vienna) on 760 days, mainly in the afternoons, from April 1976 to September 1980. LORENZ's manuscript covers April 1976 to August 1977. The 1978 experimental introductions of new fish were reported by OKAWA (unpubl.) and are included in the summary. A conservative estimate of total observation time during the period relevant for this paper (April 1976 to July 1978) is 1000 hours. Fish were kept in a 32 000 l reef-tank (4 × 4 × 2m), with artificial coral skeletons. A stone reef at the bottom and styrofoam decorations disguising corners and walls provided abundant shelters. The entire bottom of the tank was covered with layers of coral rubble and sand, serving as a filter through which water was forced from the top to a clear-water-chamber at the bottom at a rate of 14 000 l per hour. The direction of flow was occasionally reversed and the suspended ooze resulting from this procedure was removed by a fast filtering system. Water from the bottom clear-water-chamber was also drawn into a foamer (35 cm in diameter, 115 cm height) at a rate of 12 000 l per hour to remove peptides from the water, it's discharge of which was returned to the tank via an oblique and illuminated plastic panel, from where the quickly growing algae were occasionally removed. Every month, 1000 l of the tank's content were replaced by freshly mixed seawater. The tank was illuminated via the roof of the aquarium building, which was tilted southwards and covered with a transparent plastic panel allowing also UV to pass. Additional lightning was provided at least 12 hours by 4 1000-Watt halogen bulbs, arranged in a way that they did not shade the skylight. Fish were fed the "LORENZIAN" way, frequently, richly and diverse.

Preceding observations were performed 1954–1973 in reef tanks at the Max PLANCK für Verhaltensphysiologie in Seewiesen, Germany and 1967 on the coral reefs of Kaneehoe Bay, Hawaii, where LORENZ spent 6 weeks at the Hawaii Institute of Marine Biology of the University of Hawaii, Oahu, Hawaii.

Shortly after filling the tank with seawater, the first two *Zanclus*, one small, one large, were introduced in April 1976. In June 1976, 8 medium-sized (6 cm) individuals followed. Fish did not seem to be healthy from the onset and there were problems with feeding (in nature, *Zanclus* are relatively specialized sponge and algae feeders, HOBSON 1974, RUSS 1984), so all initial 10 fish were dead by December

1976. A new group of 5 small, post-larval *Zanclus*, accustomed to take aquarium food, was introduced February 1977, another medium-sized fish May 1977 and another 2, one small, one medium-sized in June 1977, The smallest of the newcomers was quickly killed by the others, so the tank contained 8 *Zanclus* in August 1977, when LORENZ's report ends. Five of those (4 from the post-larval batch and the individual from May 1977) grew to an adult size (approx. 13 cm body length) and roamed the tank as a group, when 1978, 5 new *Zanclus* of approx. 5 cm body length were introduced (4 in May, one in June). Another medium-sized fish (7 cm) was introduced in July 1978. Thus the tank contained one cohort of 5 *Zanclus* older than 2 years and another cohort of 6 smaller *Zanclus* by the middle of 1978.

Despite its size, LORENZ's observation tank was still a tiny fraction of a reef, or even of a territory of some reef fishes. Therefore, caution is appropriate to relate

observations in captivity with behavior the wild. LORENZ used to argue that captivity may cause behavioral deficits, but never additions of behaviors not shown in the wild. Limited space in an aquarium, regular food supply and a lack of predation undoubtedly increases frequencies and, possibly, intensities of interactions, but certainly could not produce behaviors which were not part of the species inventory of action patterns. This is the case for all captive animals, including monkeys and apes (DE WAAL 1982). He excluded, of course, behavioral stereotypies caused by severely limited space, as described by HOLZAPFEL (1939). In the present case, this increased intensity is a benefit rather than a drawback. One reason for the lack of field data on this topic may be that interactions of this kind are hard to observe. Thus, there is at present no alternative to Aquarium observations, which provide a source of hypotheses on processes in the wild rather than being their exact representation.

Results

Original text by Konrad Lorenz from 1979

WÄHREND DES JAHRES 1963 HIELT ICH AM MAX-PLANCK-Institut für Verhaltensphysiologie Seewiesen 7 Jungfische von *Zanclus canescens* [now *Zanclus cornutus*] (Acanthuridae, Percomorpha) in einem Becken von 1 mal 1,8 m Grundfläche und 1,5 m Wassertiefe. Die Fische verteidigten zunächst Einzelreviere, schwammen aber später im Schwarm. Der Übergang von dem Einen zum Anderen wurde damals nicht näher beobachtet. Bei den bereits gemeinsam im ganzen Becken umherschwimmenden Fischen war jeweils derjenige dominant, in dessen früherem Revier sich der Schwarm gerade aufhielt. Bemerkenswerterweise besagt dies, daß nicht nur jeder der 7 Fische jeden der übrigen 6 individuell kannte, sondern auch, daß er wußte, an welcher Stelle dieses Individuum besonders zu fürchten war.

In diesem Becken waren die Reviere der Fische begreiflicherweise unnatürlich klein und lagen zum Teil in der Lotrechten übereinander. Bei Grenzstreitigkeiten zeigen *Zanclus* wie viele andere territoriale Fische Parallelimponieren, bei dem jeder der beiden seinen Körper räumlich so einstellt, daß die auf seine Seitenfläche errichtete senkrecht nach dem Reviermittelpunkt zeigt, mit anderen Worten, die Ebene des abgeflachten Körpers liegt genau in derjenigen

der Reviergrenze. Besonders deutlich wurde dieser Effekt, wenn Territorien zweier Fische übereinander lagen, sodaß ihre Besitzer beim Breitseitsimponieren flach wie Flundern im Wasser standen. Kommt es zum Kampf, so flieht der Besiegte. Unter natürlichen Umständen liegen die Reviergrenzen ziemlich genau lotrecht, wie auch bei den zur Zeit beobachteten *Zanclus* in einem Becken von 4 mal 4 m Grundfläche.

Die erwähnten Fische lebten nur einige Monate und es war daher nicht zu erwarten, daß ihr Verhalten genau dem des Freilebens entsprach. Nach dem Prinzip, daß die Gefangenhaltung nur Ausfälle, niemals aber ein das natürliches Verhalten an Differenziertheit übertreffendes hervorrufen kann, schien mir das individuelle Sich-Erkennen und der kollektive, nicht-anonyme Revierbesitz untersuchenswert.

Als ich im Frühjahr 1964 einige Wochen als Gast des [Ernst REESE, Marine Biology Laboratory der University of Hawaii at Manoa], Oahu verbringen durfte, galten meine Beobachtungen in erster Linie *Zanclus canescens*. Die Fische erwiesen sich als schwer zu fangen und ich verdanke Mr. L. ZUKARAN 5 Stück etwa ... mm langer und gleichgroßer *Zanclus*. Freiwasserbeobachtungen auf Coconut Island, ergaben, daß in Kanehoe Bay, wo die meeresbiologische

Station der Universität Honolulu gelegen ist, *Zanclus* dieser Größe in Scharen von etwa 20 bis 30 Individuen herumstreichen. Im gleichen Areal begegnete ich immer Scharen von etwa gleicher Stückzahl und hatte den Eindruck, daß sie immer aus den gleichen Individuen bestanden, was allerdings unbewiesen bleibt. In Riffnähe sah ich immer nur einzelne, sehr viel kleinere, etwa 6 cm lange Tiere. Ich versuchte zunächst, meine an Aquarien gewohnten und nicht gegen die Scheiben tobenden Exemplare in allseits geschlossenen Plastikbecken am Riff zu versenken, um die Reaktion der dort lebenden kleineren *Zanclus* zu beobachten. Ich selbst hielt mich mit Atemgerät etwa 6 m entfernt auf. Die freien, ortsbeständigen Fische scheuten aber so sehr vor den Becken, daß sie zwar aufmerksam wurden, aber an die eingeschlossenen Fische nie näher als 2 bis 3 m herankamen, wobei sie bei den Gefangenen keine Reaktionen auslösten. Ich ging nun dazu über, an den Becken einen aufklappbaren Deckel anzubringen, den ich, wenn ein freilebender *Zanclus* in die Nähe kam, den Gefangenen durch Zug an einer Nylonschnur freisetzen konnte. Wenn ich dies tat, schwamm der Freigelassene sofort auf den Artgenossen zu. Fische, die man in eine ihnen fremde Umgebung setzt, schließen sich immer mit Artgenossen zu einer geschlossenen Schar zusammen, das tun auch Arten, die normalerweise nie in Scharen schwimmen, sondern Reviere bilden und solitär leben. Der Fremde [schwamm auf den freien *Zanclus*] zu. Dieser wich vor dem viel größeren Artgenossen und schwamm regelmäßig landeinwärts, d.h. also dem Riff und seinem Revier zu. Dort angekommen, machte er plötzlich kehrt und griff den Eindringling heftig an und verjagte ihn auch. Diese Folge von Verhaltensweisen wiederholte sich bei jeder Wiederholung meines Versuches, nur wurde der zweite und der dritte freigelassene *Zanclus* nicht nur von einem, sondern gleichzeitig von zwei kleinen *Zanclus* angegriffen, der 4. gar von dreien. Das mir erstaunliche war, daß die kleinen, offensichtlich revierbesitzenden Fische bei höchster Kampferregung nicht *untereinander* in Streit gerieten, sondern im Getümmel immer nur den fremden, größeren Artgenossen rammten, und nach seiner [Vertreibung] kampflös auseinandergingen. Dies scheint meine Annahme einer individuellen Bekanntschaft und eines gemeinsamen Revierbesitzes zu bestätigen. Das 5. Versuchstier ließ ich [hier bricht der Text ab].

Als ich in Altenberg ein Seaquarium von 4.4 m² Bodenfläche und 1.5 m Wassertiefe zur Verfügung hatte, versuchte ich, diese Annahme weiter zu prüfen. Ich erwarb im April 1976 kurz intereinander

einen großen und einen kleinen *Zanclus* (Gra und Kla), die einander zunächst nicht beachteten. Beide waren gut an Kunstfutter gewöhnt, als ich am 7. VI. [?] 8 frisch importierte kleine 6 cm lange *Zanclus* kaufte und sofort in das Becken setzte, in der Annahme sie fänden im reichen Algenwuchs genügend natürliche Nahrung. Gra beachtete die neuen kleinen *Zanclus* so wenig wie Kla. Dieser griff aber [dann] die Neuen pausenlos an und drängte sie in der rechten vorderen Ecke des Beckens zusammen, wo sie verschüchtert und z.T. nahe an der Oberfläche standen. Am 8. VI. begannen zwei der Neuen ihrerseits den Kla anzugreifen, ohne indessen je die anderen Neuen, mit denen sie gezwungenermaßen eng beisammen schwammen, je zu bekämpfen. Sie verteidigten also ein gemeinsames Revier gegen einen einzigen Gegner. Dieses Revier breitete sich ganz langsam von der rechten lotrechten Kante des Beckens entlang der unteren Vorder- und rechten Seitenkante des Beckens aus. Obwohl gerade zu dieser Zeit ein Copepode [Spaltfußkrebs] der Gattung *Thisbe* im Becken auftrat, wurden die neuen 8 *Zanclus* schnell magerer, einer starb am 13. VI., ein weiterer am 17. VI. Am gleichen Tag kam es zum höchst intensiven Kampf zwischen dem schneidigsten Neuen (Zef) und Kla, Zef floh danach zwar in die rechte vordere Kante, Kla wagte es zum erstenmal nicht, ihm dorthin zu folgen. Das Revier der 6 Neuen wurde von jetzt ab "anerkannt". Gleichzeitig aber begannen die gegeneinander zu kämpfen. Am 26. VI. hatten sie feste Reviergrenzen gegeneinander festgelegt, diejenigen, die am schlechtesten dabei wegkamen, starben bald Hungers, da sie immer noch kein schwebendes Ersatzfutter annahmen. Ihre Reviere funktionierten also als echte Nahrungsterritorien. Am 27. VI. griff einer der Neuen den großen Kla an, der vorher von den kleineren so wenig beachtet wurde, wie diese von ihm. Von den Territorien der Neuen, von denen am 28. VI. noch 3, darunter ein Todeskandidat am Leben waren, breitete sich das von Zef weiter und weiter aus, gleichzeitig mehrten sich seine Kämpfe mit den wenigen überlebenden zugleich mit ihm angekommenen *Zanclus*. Einem am 29. VI. hinzugesetzter weiterer *Zanclus* (Neu) gelang es, wohl weil die meisten seiner Gegner mager und schwach waren, ein Revier in der rechten vorderen Ecke zu besetzen. Er wurde also in dieselbe Richtung gedrängt, in die seinerzeit der ansässige Kla die Neuankömmlinge verdrängt hatte. Kla, der ältestansässige Fisch war gegen Neu deutlich aggressiver [als gegen andere]. Am gleichen Tag wurde erstmalig gesehen, daß Kla und Zef friedlich miteinander schwammen. "Schooling" ist am besten mit Scharschwimmen zu übersetzen und

danach zu definieren, daß mindestens 2 Individuen durch Rektionen *aufeinander* zusammengehalten werden. Die Aggressivität zwischen Kla und Zef nahm in den folgenden Tagen rasch ab.

Drei Karten der Reviere vom 3., 8., 15. und 22. VII. 1976. [nicht abgebildet]

Am 23. VII. starb einer der 3 noch überlebenden Neuen, einer verlor sein Revier aus dem Grund und nur der erst am 22. hinzugesetzte Fisch konnte in der rechten unteren Raumecke liegendes Revier gegen das Andrängen von Kla und Zef halten. Diese beiden hielten nun immer mehr zusammen, jeder drang immer öfter ungestraft ins Revier des anderen ein. Wenn sie aneinandergerieten fand dies immer an der Grenze statt und nahm mehr und mehr die Form ritualisierter Angriffe an: Der anschwimmende Fisch drehte in runder Kurve aus dem "Kollisionskurs" ab, schon mehrere cm ehe er an den anderen herangekommen war. Das betonte Abwenden aus der auf den Partner gerichteten Geraden ist eine Befriedungsgeste, die analog bei vielen Tieren vorkommt, auch bei Säugern, zum Beispiel Rindern (Zeeb[?]). Am 1. IX. verschwand Neu, der letzte, der am 4. VIII. sein Territorium verloren hatte und nur durch gezielte Fütterung aus einer in Oberflächennähe aufgehängten Schale am Leben erhalten worden war. [Er] verschwand spurlos (umgebracht, von Krabben aufgefressen). Zef und Kla wurden gegeneinander immer friedlicher, agonistisches Verhalten milder Art wurde vom Partner meist durch die Befriedungsgeste des Flachlegens beantwortet.

Bis dahin hatte keiner der kleineren *Zanclus* dem Großen (Gra) irgendwelche Beachtung geschenkt, nicht mehr als irgend einem andersartigen Fisch. Am 25. VII. griff Zef den Gra mitten im freien Wasser [an], dieser fluderte und floh widerstandslos in Deckung. Ab 4. VIII. kam es zwischen Zef und Kla nie mehr zu Revierkämpfen, kleine Angriffe, die mitten im Revier des einen oder anderen vorkamen, endeten immer mit Demutstellung des Angegriffenen friedlich. In deutlicher Korrelation zur Hemmung agonistischen Verhaltens zwischen Zef und Kla wurden beide gegen den größeren *Zanclus* Gra aggressiv, und zwar am 11. und 12. VIII., als einige andere *Zanclus* noch lebten. In der Interaktion zwischen den beiden wurden Befriedungsgesten (betontes Abwenden, fludern) immer häufiger, agonistisches Verhalten seltener und weniger intensiv. Nur a, 14. VIII. sah ich noch einmal einen bis zum Rammstoßwechsel und Kreiseln intensiven Kampf zwischen den beiden, nach welchem sich Zef, dann aber beide auf Gra stürzten. Bei den folgenden Angriffen auf den größeren Fisch war immer noch

deutlich, daß Zef in der linken Beckenhälfte und Kla in der rechten, also in ihren früheren Territorien aggressiver waren. In der folgenden Woche wurde Gra von den beiden dauernd umhergejagt, ohne sich je zum Kampf zu stellen. Am 20. [VIII.] hatte er Risse in der weichen Rücken- und Afterflosse, Abends war er völlig zerfetzt. Am nächsten Tag stellte er sich, in die Ecke getrieben zum Kampfe. Bisher war nur von Säugetieren bekannt, daß sich jemals der Unterdrückte dem artgleichen Unterdrücker zu einem Verzweilungskampf stellt, und zwar von Wölfen (Zimen) und Hyänenhunden (van Lavik). Am gleichen Tag sah ich zum ersten mal eine besondere Form des ritualisierten Kampfes, die allein *Zanclus canescens* zueigen ist, das sogenannte Wettschwimmen. Die beiden Gegner schwimmen, so schnell sie können, dicht nebeneinander her, im freien Wasser wahrscheinlich sehr weit, in meinem Becken in Kurven. W. Beebe beschrieb dieses Verhalten, indem er sagte, die Fische schwenken so gleichzeitig, als ob sie nur ein *Zanclus* wären. Auch beim Schnellschwimmen ist die Brustflosse das fast einzige Lokomotionsorgan, die Caudale bleibt wie alle anderen Flossen gefaltet. Nur in scharfen Kurven, in denen der äußere Fisch den weiteren Weg zu drchmessen hat, benutzt [er] manchmal sekundenlang die Schwanzflosse. Der Wettstreit wird dadurch beendet, daß der Unterlegene, d.h. früher Ermüdete [aufgibt]. An diesem Wettschwimmen waren 3 *Zanclus* beteiligt, da Kla ebenso wie Zef mitschwammen. Da Gra kein von den beiden anderen respektiertes Revier besaß—bis dahin hatte es zwischen ihm und den Kleineren keine territorialen Auseinandersetzungen gegeben—wurde er mitleidslos weiter verfolgt und war am Abend völlig zerzaust, alle Flossenhäute der weichen Rücken- und Afterflossen waren zerrissen. Da wendete sich plötzlich Gra zum Gegenangriff, um 18 20 sah ich ihn erstmals Zef ernstlich und mit Erfolg rammen. Am 21. [VII.] besiegte er Zef und schon am Abend dieses Tages waren seine zerkämmten Flossen teilweise geheilt. Am 23. VII. wurde er zwar noch gelegentlich von den beiden Kleinen gejagt, kehrte aber regelmäßig plötzlich um und griff seinerseits an. Am 24. [VII.] floh er nicht mehr, sondern stellte sich dem Angriff Zefs durch Breitseits-Imponieren. Von da ab verfolgte Gra die beiden Kleineren fast pausenlos, im Laufe des VIII. war Kla verschwunden, am 17. IX. fehlte auch Zef, der zwar von den dauernden Verfolgungen etwas mager, aber sonst gesund war, offenbar von Gra getötet (Das Töten eines Artgenossen geht bei *Zanclus* sehr schnell, nachdem einmal die Flossen eines Unterlegenen bis

zur Behinderung der Flucht zerschlossen sind. Regelmäßig reißt der Sieger dann die Branchiostegalgallhaut auf, sodaß das Atmen des Opfers behindert wird). Gra starb am 9. XII. 1976 unter zentralnervösen Symptomen, er zeigte am Tage Nachtfärbung und wurde blind.

Die obigen Beobachtungen regen zwar interessante Vermutungen an, verlieren jedoch dadurch an Wert, daß alle beteiligten Fische nicht ganz gesund waren.

Erst am 18. I. 1977 konnte ich 5 sehr kleine, offensichtlich eben erst aus dem *Acronurus*[Larven]-Stadium metamorphosierte *Zanclus* kaufen. Alle trugen noch den larvalen Präopercularorn. Ich setzte sie einzeln, mit anderen Fischen vergesellschaftet, in große, alteingereichtete, veralgte Becken und bot ihnen an weißen Porzellanschalen angetrocknetes Mischfutter (Muschel-, Tintenfisch- und Krebsfleisch, vermischt mit Tetramin Trockenfutter) an. Alle 5 sind heute, 10. II. 1979 gesund und am Leben. Sie waren an Verschiedenheiten der Rückenflosse gut kenntlich [und werden als] Gl, Ba, Kn, Ku und Fr [bezeichnet]. Als sie verlässlich Ersatzfutter, auch freischwebendes annahmen, setzte ich sie alle zugleich am 6. II. 1977 [ein]. Gleich nach dem Einsetzen schammen alle 5 in dicht geschlossener Schar umher. Um 18 00 kam es zum ersten Wettschwimmduell zwischen Gl und Fr. Gl ist bei weitem am aggressivsten. Danach Ba gegen Fr. Am 7. II. Vormittags [ist es] friedlich, nachmittags mehrmals regellose Kämpfe, Gl ist der Stärkste, Ku der am wenigsten Aggressive. Um 17 35 langes Wettschwimmn zwischen Gl und Fr, kurz darauf zwischen Ba und Fr, dann auswegsuchendes Auf- und Abschwimmen an der Vorderscheibe. Am 8. II. sind Gl und Fr oft zugleich im Hintergrund des Beckens, kommen manchmal nach vorn und greifen dann die vorn schwimmenden heftig an. Um 18 00 nimmt Ku in der rechten Vorderecke bodennahe Stellung, vertreibt Fr. Um 18 10 [findet sich] Gl an derselben Stelle. Er zeigt dabei den grauen Schläfenstrich, der defensive Kampfbereitschaft bedeutet. Etwas später schwimmt Fr aus der rechten hinteren Ecke kommend, gerade auf Gl, der frei mitten im Becken steht zu, wendet dicht vor ihm und geht zurück nach rechts hinten. Aus dieser betonten Abwendung ist bei *Zanclus* die wichtigste Befriedungs-Gebärde geworden. Die Ritualisierung beteht nur in der Bindung dreier Bewegungselemente zu einer Einheit, in einer Intensivierung des Zuschwimmens auf den Partner, in der Plötzlichkeit des Abbremsens und dem sofortigen Übergang in eine elegante, bogenförmige Abwendung. Beim "unritualisierten Vorbild"

wüde auf ein so heftiges Hinschwimmen zu einem Artgenossen unbedingt ein Rammstoß folgen. Das noch nicht ritualisierte Abwenden vom Gegner geschieht fast nur an der Reviergrenze. Befriedungsgeste und unritualisiertes Vorbild sind durch alle denkbaren Übergänge miteinander verbunden.

Am 8. II. [sind] Gl und Fr oft allein im Hintergrund, beide bedrängen die drei anderen an die Vorderscheibe gedrängten. Im Laufe des Tages [ver]teilen sich nach einigen Grenzkämpfen Gl und Ba in die linke und rechte Hälte des Beckens. Um 18 00 stellt sich Kuna erstmalig mit grauer Schläfe (Defensiv-Färbung) dem Angriff Gls, es kommt zum Kreiseln (gegenseitiges Rammen), wonach Gl weicht, kurz darauf dasselbe [wieder]. Gegen Abend kommen Gl und Fr nicht mehr an die Vorderscheibe, [um] 18 45 [ein] echter Grenzkampf zwischen Kuna und Fris. [Um] 18 55 Wettschwimmen [zwischen] Gl und Fr, wonach sich Fr ganz in die rechte hintere Ecke, Gl an die linke Wand begibt. Fr steht meist im rechten hinteren Winkel, obwohl ihn niemand dorthin gedrängt hat. Die Wahl eines Platzes, der als Zentrum des Reviers fungiert und den wir mit G. Allen als Focus bezeichnen, ist für *Zanclus* typisch. Abb.[1] zeigt die Reviere abends [am] 8. II. Kn hatte sich bis dahin nicht verteidigt. Während der nächsten Tage waren die 3 nach vorne gedrängten *Zanclus* auffallend tolerant gegen [?], in dem Maße aber, in dem Gl und Fr an einer bestimmten Grenze halt machten, begannen sie untereinander zu kämpfen. Ba drängte zunächst Kuna in die rechte Ecke und dehnte sein Revier nach hinten gegen Fr aus, Kn kam an schlechtesten weg und verteidigte ein Revier mit 3 Fronten (siehe Abb. [1]). Bajo drängte Knopf von links her gegen die Wand, diese aber wird von Kuna verteidigt. Am 11. [II.] wird Kn aus diesem ungünstigen Revier vertrieben und im ganzen Becken umhergehetzt. Dabei gehen die einzelnen *Zanclus* nicht aus ihrem Revier heraus, achten aber an der Grenze darauf, daß der Flüchtling aus dem Revier des Nachbarn nicht in das ihre getrieben wird. Schließlich geht Kn in Dekkung hinter einer die linke hintere Kante abschirmende Styroporkulisse. Zwischen deren Rand und der Hinterwand ist nur ein schmaler Spalt, den Kn hinfort gegen Gl verteidigt. Dieser "patrouilliert" fortan wachsam vor diesem Ausgang und erlaubt nicht, daß Kn hervorkommt. Dieser wird hinter der Kulisse gefüttert, am 14. III. wagt er sich in extremer Defensiv-Färbung zur Fütterung kurz aus der Spalte, in der er sofort wieder verschwindet. Am 19. III. sah ich Gl am unteren Vorderrand der Kulisse drohen und schloß daraus, daß dort ein Loch entstanden

sei, aus dem Knopf seinerseits heraus drohte. Im nächsten Augenblick schoß dieser aus dem Loch heraus, griff Gl intensiv an, nach heftigem Kreiseln floh er, aber nicht ins Loch zurück, sondern um den Hinterrand der Kulisse herum in die gewohnte Spalte. Gl patrouilliert fortan abwechselnd vor dem Loch und der Spalte. [Am] 28. III. greift Kn hinter der Kulisse heraus Gl an. Von da an wurde Kn nicht mehr hinter der linken Eck-Kulisse gefüttert, [am] 30. III. greift Kn den Gl aktiv an.

Die vom 10. III. wiedergegebene Geographie der Territorien bleibt in groben Zügen bis 22. III. erhalten, nur Kuna wird allmählich an an die rechte Wand gedrängt, sodaß Bajos Revier nunmehr unmittelbar an das von Fris grenzt. Später wird Kuna, der auch im Wachstum zurückbleibt, langsam der Wand entlang in die rechte hintere Ecke und von da hinter einen Felsaufbau gedrängt, der sich vor der Mitte der Hinterwand erhebt. Fortan hält er sich mit knapper Not, wie aus der nunmehr dauernd getragenen Defensiv-Färbung mit grauen Schläfen hervor geht. Am 22. III. wird die Reviergrenze zwischen Glubsch und Fris aufgehoben. Schon seit einigen Tagen hatten die Begegnungen an der Reviergrenze mehr und mehr die schon beschriebene Form der Befriedungsgeste mit bogenförmiger, betonter Abwendung angenommen, doch hatte es zwischen ihnen noch ein Duell mit Kreiseln und ernsten Rammstößen gegeben, danach aber *nie mehr* (??? Protokolle genau durchschauen [Anm. LORENZ]). Wenn einer schnell auf den anderen zuschwimmt, stellt sich dieser quer und imponiert breitseits, worauf der Ankommende mit betont scharfer Kurve abbiegt. Diese Zeremonie, früher nur für die Begegnungen an der Grenze angewendet, wurde allmählich ort-unabhängig und die Fische begrüßten einander in dieser Weise wann und wo immer sie zufällig aufeinander trafen.

Im Lichte diese Nichtangriffspaktes erscheinen einige frühere Beobachtungen erwähnenswert. Wie die Revierdiagramme zeigen, hatten Gl und Fr schon durch lange Zeit die längste gemeinsame Grenze. Dementsprechend sind in der Zeit vom 9. II.—dem Tag nach dem Einsetzen der Jungfische—bis zum 22. III. mehr Kämpfe zwischen Gl und Fr verzeichnet, als zwischen irgendwelchen anderen Individuen. Schon am 13. II. hatten Ku, Kn und Ba allerseits “anerkannte” Reviere und damit schwand auch die verhältnismäßige Verträglichkeit von Gl und Fr. Schon am 11. II. weideten Gl und Fr manchmal friedlich nebeneinander an ihrer späteren Reviergrenze. Ihre Kampf-Schwelle. [diese] Distanz ab der sie einander angriffen, war deutlich kleiner, als die allen

anderen *Zanclus* gegenüber. Andererseits waren die 3 anderen, solange sie gemeinsam ins rechte vordere Eck gedrängt waren, gegeneinander toleranter als gegen die beiden Besitzer der großen hinteren Territorien. Im Diagramm vom 13. II. geben die Pfeile die Richtung auf den jeweils passiv angegriffenen an. Ihre Zahl entspricht (mit geringen Fehlern) derjenigen der von 18 bis 19 Uhr protokollierten Angriffe, mit Ausnahme derjenigen zwischen Gl und Fr, die zu schnell aufeinanderfolgten, um zählbar zu sein. In der größeren Zahl der Fälle war Gl der Angreifer. Man beachte auch die Zahl seiner Angriffe auf den meist unsichtbar hinter der Kulisse in der rechten hinteren Ecke stehenden Knopf, ebenso die geringe Zahl der Angriffe von Gl und Fr gegen Ba und die verhältnismäßig große Zahl der Auseinandersetzungen zwischen Ba und Ku. (Vom Standpunkt des aufgeschobenen Handelns (“delayed response”). Ku wird von Bajo mehr und mehr nach hinten gedrängt, Fris ist weniger aggressiv gegen Ku und Ba. m 19. II. schien es, als ob Fris und Kuna friedlich miteinander die rechte hintere Ecke bewohnten, sie schwammen oft reaktionslos dicht nebeneinander, doch schon am 20. griff der größere Fris den Kuna wütend an und er verschwand hinter der rechten hinteren Kulisse. Knopf und Kuna sind von nun ab kaum mehr zu sehen, man entnimmt ihr Vorhandensein nur daraus, daß Fris von Zeit zu Zeit hinter die rechten hinteren Kulisse droht, Glu dagegen hinter die linke. Beide bleiben mehr und mehr im Wachstum zurück. Knopf kann hinter der linken hinteren Kulisse gefüttert werden, das Revier Kunas ist schwer zugänglich. Er wird von Fris allmählich aus der rechten hinteren Ecke verdrängt und wohnt nun in der Deckung hinter einigen großen Felsblöcken in der Mitte der Hinterwand—was nur aus den Angriffen und dem Drohen von Fris geschlossen werden kann. Von Friedlichkeit zwischen Glu und Fris kann schon lange keine Rede sein, besonders nahe der Hinterwand geraten sie wiederholt in heftigen Kreiselnämpfen aneinander, während bei ihrem Zusammentreffen nahe der Vorderscheibe immer nur die Reviergrenz-Zeremonie verzeichnet wird. Die Reviergrenzen bleiben bis 1. III. unverändert.

Am 2. III. geht um 17 Uhr zufällig das Licht aus und bald darauf schwimmen alle 5 *Zanclus* in dicht geschlossener “anonymer” Schar, es wird deutlich, wie sehr Knopf und Kuna im Wachstum zurückgeblieben sind. Als das Licht wieder angeht, beginnen beide gierig Algen zu weiden, sie haben ja, in ihre Höhlen gebannt, nur Kunstfutter bekommen. Nach 5 Minuten beginnt Bajo erst den Kuna, dann den

Knopf und dann den Glu anzugreifen. Kuna flieht anstatt in sein Versteck, unorientiert gegen die Vorderscheibe und bekommt ein paar Rammstöße von Ba, ehe er sich hinter die Felsen Mitte hinten zurückzieht. Die drei dominanten Fische schwimmen danach noch einige Zeit im Schwarm, gehen zwischendurch in ihr Revier zurück, um sich dann nochmals zusammenzufinden. Das Scharschwimmen (schooling) im Dämmerlicht ist eine regelmäßige Erscheinung. Es kann ebenso durch grobe Veränderungen im Becken, wie Einführen einer neuen Dekoration oder Hineinspringen eines Menschen verursacht werden. Um 1910 ist das normale Revierverhalten wiederhergestellt.

8. III. Sowohl Banjo wie Fris haben ihr Revier auf Kosten von Glu merklich erweitert. Kuna ist etwas mutiger geworden und wird manchmal im freien Wasser oberhalb seines Verstecks futtersuchend gesehen, meist aber sofort von Glu oder Fris oder beiden zurückgejagt. In diesem Fall geraten die beiden oft lotrecht über dem kleinen Revier von Kuna aneinander.

13. III. Glu ist deutlich aggressiver gegen Kuna, der jetzt fast dauernd oberhalb seines Verstecks frei im Wasser steht. Meist ist er defensiv gefärbt. Einmal, als Fris ihm in sein Versteck zu folgen wagt, kommt es doch zum Kreiseln. Fris und Glu wechseln darin ab, Ku zum Verharren in seinem Versteck zu zwingen, wobei Glu aber mehr Zeit damit verbringt, den Knopf am Hervorkommen hinter der linken Eckkulissee zu verhindern. Dabei hat er zwei Stellen zu bewachen, nämlich den die ganze Beckenhöhe einnehmenden Spalt zwischen Kulissee und Hinterwand, und zweitens das schon erwähnte Loch.

Am 20. III. 17 30 stößt Glu 3 mal hintereinander in das Loch hinein, auf den dem Beobachter unsichtbaren Kn. [Um] 18 00 stößt dieser im wilden Angriff aus seinem Loch hervor auf Glu und flieht erst nach intensivem Kreiseln, nicht ins Loch zurück, sondern nach außen um die Kulissee herum in den Spalt an der Hinterwand. Dies wiederholt sich, Knopf schießt in Defensivfärbung aus dem Loch und flieht nach kurzem Angriff auf Glu in die Spalte. [Um] 18 35 schalte ich wieder das Licht aus und nach einigen Minuten wieder an. Alle 5 *Zanclus* schwimmen in dichter Schar. Als es wieder hell wird, begibt sich Knopf ohne angegriffen worden zu sein in sein Revier, Bajo greift als erster die anderen an, als die Schar nahe der Vorderscheibe in seinem Revier ist. Dann kommt Knopf noch einmal hervor und es setzt einen intensiven Kreiselkampf zwischen ihm und Glu. Am nächsten Tag ist Knopf unsichtbar, Glu droht mehrmals ins Loch.

Dagegen ist Kuna oft sichtbar, kämpft einmal mit Fris.

22. III. 1977 haben Glu und Fris die Grenze zwischen ihren Revieren aufgelöst. Noch Tage vorher gab es einen Kreiselkampf, also immerhin eine hochintensive agonistische Auseinandersetzung zwischen ihnen, wiewohl im Allgemeinen ihre Begegnungen mehr und mehr zur Befriedungsgeste wurden, die allerdings bisher nur an ihrer Gebietsgrenze ausgeführt wurde. Nun aber ist diese Zeremonie vom Ort unabhängig geworden, wo immer sich beide begegnen, schießt einer—meist Glu—scharf auf den anderen zu, der sich dann querstellt und breitseitig imponiert, worauf der Heranschwimmende in überbetontem scharfem Bogen abwendet. Keinerlei Unterwürfigkeitsgesten, wie sie bei nahezu allen anderen mir bekannten Percomorphen (Cichliden, Grammiden, Pomacentriden, Pomacanthiden u.a.) in homologer Weise vorkommen. Nach einem solchen Zusammentreffen bleiben beide dicht beisammen und weiden häufig. Weiden als ritualisierte Übersprungbewegung wirkt bei grasfressenden Säugetieren nicht selten als Befriedungsgeste, so nach Walter bei...

Nach der Fusion der Reviere wurden Glu und Fris paradoxerweise weniger aggressiv als die drei anderen. Kuna kommt nun viel weiter aus seinem Versteck hervor und weidet ruhig an der Vorderseite seiner bisherigen Deckung. Bajo hat sein Revier weiter nach links ausgedehnt. Das kam wahrscheinlich dadurch zustande, daß Bajo regelmäßig den Fris angriff, wenn er ihn links vorne an einer Stelle sah, an der er nur den Glu zu sehen gewohnt war. Für diese Interpretation spricht, daß sowohl Glu als auch Fris Bajo gegenüber deutlich ängstlicher waren, wenn sie sich just in dem Ihnen neuen, bisher nur dem Partner gehörigen Revier befanden. An den Grenzen, an denen sie schon seit langem dem Bajo gegenüberstanden waren sie viel mutiger ihm gegenüber.

Am 23. III. schwamm nach einem kurzen Grenzgeplänkel mit den beiden anderen Bajo in eigentümlich schlängelnder Bahn ins Revier der beiden anderen, die ihm nichts tun und alle 3 schwimmen in dichter Schar bis an die Hinterwand, wo sie dann plötzlich doch zu raufen beginnen. Dieses Schlängelschwimmen wurde später oft gesehen, es steckt an und führt dazu, daß plötzlich alle anwesenden *Zanclus* in einem dicht gedrängten Haufen durcheinanderwimmeln. Die Teleonomie [die Funktion] dieser sehr eindrucksvollen Verhaltensweise ist unklar.

Am 25. III. vollführten Glu und Fri zusammen eine Folge von Bewegungsweisen, die bei Cichliden [Buntbarschen] die des Ablai-chens sind. Es wird zuerst ein Stein mit schabenden Bissen gereinigt, dann streichen die Fische mit angehobenem Schwanz und nach unten gedrückter Genitalregion über die geputzte Stelle, wobei das Weibchen Eier ablegt, das Männchen diese befruchtet. Häufig voll-führen *Zanclus* die Bewegungsfolge vor dem Ablai-chen "leer" = Scheinablaichen. J. und G. BAERENDS, die diese Bewegungsweise als erste beschrieben, nannten sie "skimming". Laich wurde von den *Zan-clus* dabei nicht produziert, oft vollführte nach dem Steinputzen nur einer der beiden Bewegungen. Am 27. III. sah ich Glu und Fris noch einmal scheinab-laichen, und zwar an genau derselben Stelle. Diese Beobachtung bestärkte mich in dem irrigen Glau-ben, daß die Zeremonie zur Paarbildung gehöre, doch sah ich sie am 31. III. von Glu mit Bajo, und das war an dem Tag, an dem Glu und Fri die Revier-grenze gegen Bajo auflösten und diesen fortan im gemeinsamen Revier duldeten.

Am 14. IV. sah ich noch einmal Glu und Fri die-selben wenigen Quadratzentimeter Stein putzen, während Bajo aufmerksam danebenstand, worauf dann dieser die Scheinablaichbewegung vollführte. 15. III. Bajo mit Fris, nachdem sie gleichen Tags [?]. 25, 26. V. noamoi [noch einmal] Fris mit Bajo.

Die 3 großen *Zanclus* waren ab 31. III. untereinan-der recht friedlich, bei gelegentlichen kleinen Reibe-reien dominierte vorraussagemäßig immer derjenige, in dessen früheren Revier sie stattfand. Sehr unter-schiedlich aber war das Verhalten der 3 zu den bei-den "Außenseitern", die inzwischen im Wachstum noch mehr hinter den [anderen] dreien zurückge-blieben waren. Knopf, der in dem dreieckigen Raum hinter seiner Kulisse (s. Abb. [1]) ein abgesondertes Leben führt, wurde nach wie vor von allen dreien angegriffen, sowie er sich hervorwagte. Ich sehe ihn immer nur sekundenlang, wenn er seinerseits auf einen der 3 Großen vorstößt, um sofort wieder in seiner Spalte zu verschwinden. Mit Kuna hat Knopf zu dieser Zeit keine gemeinsame Grenze!

Schon am 26. III. floh Kuna vor Glu auf viel grö-ßerem Abstand als vor Fris, am 27. kam es zwar noch zu einem kurzen Kreselkampf, als Fris ganz in Kunas Gebiet engedrungen war, am gleichen Tag etwas spä-ter auch zu den ersten deutlichen Befriedungsgebär-den zwischen diesen Beiden an der Reviergrenze. Kunas Revier dehnt sich unter dem verminderten Außendruck allmählich aus. Die Grenzen werden jetzt auch von Glu respektiert, mit Fris tauscht Kuna wiederholt an der Grenze Befriedungsgebärden aus,

beide auch dicht nebeneinander und Fris geht oft unangefochten tief in Kunas Revier. Auch Glu ist etwas duldsamer gegen Kuna, nicht aber gegen Knopf.

Bajo, der oft unangefochten in das bisher nur Glu und Fris gehörige Revier einschwimmt, ist dennoch gegen die beiden anderen etwas aggressiv, wenn sie ihrerseits in sein Revier kommen, immerhin tauscht er im eigenen Gebiet mit Fris Befriedungsgebärden aus, wonach sie um 17 15 an derselben Stelle schein-ablaichen wie Glu und Fris am 25. III. und 27. III., also tief im früheren Revier von Glubsch. Am 5. IV. und 6. IV., 14. IV. Bajo mit Fris, 26. IV. nochmals Scheinablaichen Fris mit Glu an gleicher Stelle.

30. III. Knopf wird mutiger und greift, in Defen-sivfärbung mit grauen Schläfen, aber intensivst den Glubsch an. Am gleichen Tag um 18 25 greifen Fris und Glu rechts hinter Deckung an, gleich darauf in gleicher Richtung Kuna und aus der Deckung in der in der rechten hinteren Ecke, kommt Knopf heraus-geschossen, den ich in dieser Gegend nie vorher sah. Knopf verteidigt etwas später seinen neuen Revier-besitz, hat also jetzt zwei Reviere, zwischen denen etwa 3 m Feindesland liegen. Glu vertreibt Kno, der flieht in sein früheres Revier links hinten, Kuna greift ihn an und er flieht nach rechts hinten oben. Kuna trachtet, sich friedlich an die 3 Dominanten anzu-schließen, wird aber stets von Glu vertrieben, Bajo und Fris würden ihn dulden. Seinerseits war Kuna ab 31. III. sehr aggressiv gegen Knopf, sowie dieser sein neues Revier rechts hinten oben bezog. Ab 1. IV. patrouilliert Kuna aber auch an der linken Kulisse, manchmal sogar mit Glu, der ihm dann merkwürdi-gerweise nichts tut. Am 4. IV. fällt mir erstmalig auf, daß nicht nur Kuna den Fris in seinem Revier duldet, sondern auch umgekehrt dieser ihn. Am 5. IV. hat Glu das Loch in der linken Kulisse dauernd im Auge und kommt von weither angebraust, wenn Knopf auch nur den Kopf herausstreckt. Das Paroullieren an der Grenze eines nur in langen Intervallen sicht-baren Gegners ist ein bemerkenswertes Beispiel eines sehr langdauernden "aufgeschobenen Handelns" (delayed reaction).

10. IV. Kuna weist wiederholt Glubsch zurück, der in sein Revier einzudringen versucht. Befindet sich Kuna aber weiter vorne, d.h. im Revier von Fris so verschwindet er sofort, wenn Glu von weitem her-ankommt.

13. IV. wird ein Kreiselduell zwischen Kuna und Knopf protokolliert, mit der Erweiterung ihrer Reviere kommen diese beiden Außenseiter erst jetzt miteinander in Berührung. Am 14. langes intensives Wettschwimmen zwischen Kuna und Knopf, das vor

Knopfs Versteck endet, wo ihn Glu angreift und in die Deckung treibt. 15. IV. Knopf kommt, von Glu ungesehen aus seinem Loch und wird sofort von Kuna angegriffen und zurückgetrieben. Glu "respektiert" Kunas Revier, greift ihn auch selten an, wenn er im Gebiet von Fris ist, wiewohl nie gesehen wurde, daß Fris ihn etwa gegen Glu verteidigt hätte. Als am 16. IV. alle 4 friedlich in Fris Revier schwimmen, droht Glu ins Loch, in dem er den Knopf sieht.

Am 18. IV. wird in der rechten vorderen Ecke des Beckens eine Kiste aus PVC versenkt, die nach der Mitte des Beckens zu eine Sichtscheibe hat und einen kleinen *Zanclus* von der ungefähren Größe Knopfs und Kunas enthält. Zu meinem Erstaunen reagiert zunächst keiner der *Zanclus* auf den Neuen. Erst um 17 05 steht Kuna vor der Kiste, also mitten im Revier Bajos. Dieser erlaubt Kuna, bis an die Vorderscheibe zu kommen. Erst später bekommt Bajo graue Schläfen (Defensiv-Färbung!) und greift Kuna an, der Widerstand leistet und sich hinter der Kiste in der rechten vorderen Ecke verschanzt. Bajo wird darauf allmählich immer aggressiver gegen Kuna, treibt ihn nach hinten und greift ihn schließlich in der rechten hinteren Ecke, im Revier von Fris an, wo er ihn seit vielen Tagen geduldet hatte. Bajo patrouilliert nun fast dauernd die rechte Wand, um zu verhindern, daß Kuna nach vorne kommt. Dennoch kommt er abends bis an die Vorderscheibe der Kiste und tauscht mit dem Eingeschlossenen Drohgebärden aus. Die drei großen *Zanclus* sind immer noch völlig reaktionslos gegen die Kiste und ihren Inhalt. Erst am 19. IV. wird Glu aggressiv gegen Kuna, den er an ungewohnter Stelle sieht, noch dazu in Drohstellung. Er vertreibt ihn immer wieder und reagiert nun plötzlich stark auf den Eingeschlossenen, den er ja in der Stellung des Breitseitsimponierens durch die Klarscheibe zu sehen bekommt, und versucht diesen zu rammen.

Kleindruck: Das lange Nicht-Reagieren auf einen Artgenossen, der zwar in fester Umrahmung, aber doch klar sichtbar im Revier eines sonst höchst aggressiven Fisches steht, bedarf einer besonderen Erklärung. Schon bei meinen Freilandversuchen (S.) wunderte ich mich darüber. Der große individuelle Unterschied ist merkwürdig. Bajo, in dessen Focus die Kiste steht, reagiert überhaupt nicht, Kuna kommt von weitem herbei und Glubsch reagiert erst, als er diesseits der Scheibe Drohgebärden Kunas sieht.

Im Laufe des 19. [IV.] wird Glubsch immer böser gegen Kuna und trachtet ihn aus seinem neueroberten Gebiet längs der rechten Wand zurück nach hinten zu treiben. Dabei gerät er zunächst nicht mit Bajo

aneinander, der gleiches tut. Trotz der doppelten Gegnerschaft versucht Kuna immer wieder an die Sichtscheibe der Kiste zu gelangen, liefert sogar dort ein Kreiselduell mit Bajo, in das Glu eingreift und Kuna gründlich verjagt, Glu ist von nun an fast ununterbrochen an der Kiste. Bajo, in dessen Revier-focus sich das alles abspielt, wird allmählich aggressiver gegen Gl, der auf seinen ziemlich milden Vorstoß widerstandslos weicht. Dieser [Glu] ist aber weit aggressiver geworden und liefert Bajo am 20. IV. 19 15 ein Kreiselduell, in dem Bajo schließlich Defensiv-Färbung (graue Schläfen) bekommt und weicht. In der Folge greift Bajo nur Kuna an, der immer wieder längs der Wand von hinten heranschleicht. Nur Glubsch reagiert intensiv auf den Eingespernten und richtet am 20. IV. Rammstöße gegen die Sichtscheibe. Kuna kommt immer wieder vor die Kiste und rammt auch gegen die Sichtscheibe und als Glubsch ihn angreift, wehr er sich und es kommt mehrmals zu intensivsten Kreiselduellen, einmal gelingt es Kuna den viel größeren Glub zu besiegen, wohl nur deshalb, weil dieser dem Eingespernten mehr Beachtung [schenkt]. Glub "klebt" auch von nun an an der Kiste, ist kaum je woanders als vor der Sichtscheibe. Der Eingeschlossene ist nun genügend eingewöhnt, um das Agonistische Verhalten der aneren durch die Sichtscheibe hindurch zu erwidern.

Durch diese Geschehnisse bekommt Knopf genügend Ruhe, um sich von seiner Einschüchterung zu erholen. Am 20. IV. 18 35 stoßen Knopf und Kuna an der Mitte der Hinterwand, also weit von der bisherigen Reviergrenze Knopfs aufeinander und es setzt ein wildes Kreiseln, das in Wettschwimmen ausklingt. Kuna reagiert von nun ab überhaupt nicht mehr auf den in der Kiste Eingeschlossenen. Er jagt dauernd Knopf, doch gelingt es ihm nicht, diesen in seinem früheres Versteck hinter der Kulisse im linken hinteren Eck zurückzudrängen. Knopf bleibt dauernd sichtbar und das Jagen geht immer öfters in echtes Wettschwimmen über. Am 21. IV. sehe ich zum ersten Mal die höchst intensive Form des Kampfes bei *Zanclus*, das auf S. beschriebene [fehlt] Nasenkringeln und gegenseitige Sich-Verbeißen. Am 22. IV. gibt es Vormittags noch mehrere Kreisel- und einige Nasenkringelkämpfe zwischen Knopf und Kuna, Knopf geht nie mehr hinter die Kulisse, sondern weidet mit Bajo und Fris vor dieser im Freien, wobei diese ihm gegenüber zwar etwas gespannt wirken, aber deutlich Friedensgesten andeuten. [Um] 18 40 (nach Akademiesitzung) schwimmt Knopf völlig frei im gemeinsamen Revier der 3 Großen, Kuna aber ist verschwunden, wie sich

später herausstellt, schwer verletzt rechts hinten unter Felsen versteckt.

Glu bekämpft in den nächsten Tagen buchstäblich pausenlos den Eingeschlossenen. Knopf gewinnt rasch an Mut und bewegt sich, nachdem er geringe Widerstände seitens Bajos und Fris überwunden hat, frei im ganzen Becken. Knopf also ab 30. IV. 1977 Mitglied des Revierbesitzerkollektivs. Glu ignoriert ihn völlig, bis am 23. IV. 1977, 11 20 Knopf seinerseits den vor der Kiste patroullierenden Glu angreift. Dann setzt es Kreiselduelle, in denen Glu den Knopf zwar besiegt, aber nie weit vertreibt, da er völlig auf den Eingeschlossenen konzentriert ist. Bajo, der sonst erfahrungsgemäß viel weniger aggressiv ist als Glu, verjagt den Knopf intensiver als dieser, wohl weil die Kiste genau im Focus seines Reviers steht. Knopf wird daraufhin äußerst angriffslustig und es setzt zwischen 10 30 und 11 40 nicht weniger als 17 ernste Kämpfe zwischen Knopf und Glu. Bajo, in dessen Focus sich Glu bei Belagerung des Eingesperrten ja befindet, greift Glu mehrmals an. Dieser Zustand bleibt bis 30. IV. unverändert, Bajo greift von Zeit zu Zeit Glu an, einmal tut das auch Fris, da Glu auf den Eingesperrten konzentriert ist, und die anderen auf ihn, der an ungewohnter Stelle steht, kann Knopf völlig frei im ganzen Becken umherstreifen. Am 1. IV. kämpft er nochmals heftig mit Glu, der schließlich flieht und von Kno durchs ganze Becken gejagt wird. Interessanterweise betrachtet Glu jetzt den Platz vor seiner Kiste als seinen Focus, zu dem er zurückflieht, wenn er geschlagen ist.

Am 3. V. ist Kuna plötzlich wieder sichtbar, mit zerfetzten Rücken- After- und Schwanzflosse und nur einem Auge. Er verbirgt sich alsbald wieder hinter einem Gestrüpp veralgter Gorgonien skelette an der rechten Beckenwand, später hinter der Styroporleiste rechts oben. Daß der sich dauernd dort aufhält, geht aus dem dorthin gerichteten Drohen Knopfs hervor, der sich jetzt frei an der rechten Wand, nahe dem Focus Bajos und auch nahe dem dauernd von Glubsch eingenommenen Platz vor der Kiste. Kuna gewinnt in den ersten Maitagen langsam an Raum, Knopf bekommt graue Schläfen, wenn der hinter die Deckung Kunas vordringt, d.h. er "erkennt das Revier an, mit anderen Worten, er verhält sich ängstlich, als Fisch im fremden Revier. Einmal geraten Glu und Knopf aneinander, als sie beide zugleich Kuna angreifen wollen.

Am 7. V. wird der *Zanclus* aus der Kiste entlassen. Wegen eines Knicks in der Dorsalis erhält er den Namen Knick. Längere Zeit geschieht gar nichts, dann Kreiseln Glu und Knick. Fris wird hellgrau und

geht wütend auf Knick los, dann Knopf, dann wieder Glubsch, dann Bajo. Alle 4 jagen den Neuen, zunächst ohne je aneinanderzugeraten. Der Ort, an dem jeder angreift, steht in deutlicher Beziehung zu seinem Focus. Die Angriffe auf Knick erreichen die Intensität des Nasenkringelns, d.h. des ungehemmten Zubeißens und Festhaltens. Um 16 30 verschwindet Knick. Glu, Bajo, Fris und Knopf patroullieren vor der Deckung in der er verschwand, zwischendurch aber auch vor der Stelle, an der Kuna verborgen sitzt, d.h. rechts oben an der Wand. Um 16 50 wurde Knick nochmals sichtbar, mit total zerfetzten, kurz abgebissenen Flossen. Er sah so hoffnungslos aus, daß ich versuchte, ihn herauszufangen, dazu war er aber noch zu flink und zu gut orientiert.

8. V. Es gelingt, Kuna an der rechten Wand zu füttern, indem ich Futter an der algenbewachsenen Wand verreihe. Kuna gewinnt so weit an Mut, daß er sich unterhalb seiner nicht ganz bis zum Beckenboden reichenden Deckung zeigt und sich sogar dort in einem Kreiselduell gegen Glubsch behauptet, später greift er von oben herab aus seiner Deckung vorstoßend, einmal den Glubsch, einmal den Bajo an. Gegen Abend paroulliert er offen die Grenze seines neuen Kleinreviers, macht Ausfälle bis zu 50 cm gegen Glubsch. Einmal stößt er gegen Fris vor, stoppt dicht vor ihm und wendet in Befriedungsgeste von ihm ab, gleich darauf saust er in "Schwalbenstellung" (?) dicht an Fris vorbei gegen Knopf vor, liefert mit diesem ein Kreiselduell und kehrt dann nicht von Knopf verfolgt, in sein Revier in der oberen rechten Ecke zurück. Die zahlreichen Ausfälle, die Kuna in nächster Zeit aus seiner eigenen Deckung heraus macht, richten sich sämtlich gegen Glubsch und noch intensiver gegen Knopf, kein einziger gegen Fris. Er besiegt in Kreiselduellen mehrmals Glu, der zur Zeit mindestens doppelt so schwer ist und er vergrößert dabei sein Revier allmählich. Dieses wird von Glu und Knopf gemieden, Fris darf hinein.

9. V. 1977. Hier Diskontinuum im Revierbesitz. 19 10 kommt Fris diagonal durchs ganze Becken gerade auf Kuna los, der ruhig im Freien grast, hält dicht vor ihm ruckartig an, wendet um 90° und schwimmt ganz langsam weg. Kuna, der kurz vorher einen bösen Ausfall gegen Knopf gemacht hatte, reagiert überhaupt nicht und grast weiter. Kuna hat also am 9. V. abends ein Revier, das rechts vorne vom Styroporwulst bis zum Boden reicht und das von Glu und Knopf respektiert wird, während Fris es unbestraft betreten darf. Beobachtungsunterbrechung (Grünau).

Bis 16. V. hat sich dieses Gebiet längs der Vorderseite nach links um etwa 50 cm erweitert, Kuna vertreibt aus dem neuerworbenen Gelände den Bajo, obwohl dieser dort seinem Focus nahe ist. Knick ist wieder sichtbar, er steht im Schatten des Überhangs der rechten hinteren Kulisse, die die Ecke abschrägt. Er ist dort nicht sehr auffällig, da er im Schatten steht, er ist mutig genug, um sich durch den herankommenden Knopf nicht in die Deckung (hinter der Kulisse) treiben zu lassen, sondern liefert ihm ein Kreiselduell. Er beherrscht während der nächsten Zeit die Vorderseite der rechten Eckenkulisse, aber nicht ganz bis zum Boden.

Es muß klar gemacht werden, daß zu dieser Zeit Glubsch, Fris, Bajo und Knopf das ganze Becken bis auf die Kleinreviere der Außenseiter Knick und Kuna beherrschen, alle Kämpfe zwischen diesen 4 Dominanten sind mehr oder weniger Focus-bezogen und gehen ohne Folgen aus. Keiner von ihnen verteidigt eine *Grenze* gegen einen anderen, dies tut nur Kuna gegen Glubsch und Knopf, schon weniger gegen Bajo und gar nicht mehr gegen den "befreundeten" Fris. Es fällt auf, daß es die ganze Zeit nicht *eine* agonistische Auseinandersetzung zwischen Glubsch und Fris gegeben hat!

Am 18. V. von 18 14 bis 18 50 (Schluß der Beobachtung) nicht weniger als 18 Angriffe Knopfs auf den in seinem Kleinrevier stehenden Kuna, arg genug, um zum Kreiseln zu führen. Kuna wird von dem deutlich stärkeren Knopf allmählich weiter und weiter in sein Revier rechts oben im Eck zurückgedrängt, kommt allerdings, sowie Knopf sich anschickt, wegzuschwimmen, wieder hervorgeschossen und verfolgt ihn. Knopf greift dauernd mit delayed response den unsichtbaren Kuna an, nicht aber den vor der rechten hinteren Kulisse sichtbaren Knick.

Im Laufe der nächsten Tage kommt Kuna immer weiter aus seiner Ecke heraus. Auch Knick wird allmählich weniger bekämpft, anstelle von Rammstößen und Kreiseln sieht man mehr und mehr Breitseitsimponieren und Schwanzschlag. 26. V. 19 10 weidet Kuna friedlich dicht bei den 4 dominanten *Zanclus* am Boden, macht hintereinander eine betont friedliche Abwendung vor Knopf, anschließend eine etwas ängstliche vor Bajo. Etwas später schwimmt Fris ungehemmt ins Revier von Knick, der kommt ihm entgegen und wendet betont friedlich ab (ev. Kleindruck). Knick ist aber noch nicht ganz Mitglied der revierbesitzenden Gruppe, denn am 19. V. um 17 50 bekämpft er, wenn auch wenig intensiv den Knopf an dessen früherer Grenze und treibt etwas später auch die 3 Großen von seinem

Focus Mitte der Hinterwand. Später sind alle 4 bei Kuna in dessen Revier. Kuna macht qualitativ und quantitativ verschiedene Bewegungen gegen Knopf und gegen Fris. Gegen ersteren vollführt er eine Superposition von Scheinrammstoß und Abwenden, gegen Fris typisch befriedende Abwendungen. Noch am 22. V. verteidigt Knick sein Gebiet gegen die 4 anderen, richtet seine Angriffe besonders gegen Knopf, doch werden bald auch halb-freundliche Abwendungen Kuna vor Knopf verzeichnet. Am gleichen Tag geraten Kuna und Knick in der Mitte der rechten Wand zum erstenmal aneinander und bekämpfen einander wütend, Kreiseln. Da beide ihre Reviere allmählich ausdehnen, wird die Grenze zwischen beiden länger. Knicks Revier reicht nun vom oberen Teil der rechten hinteren Ecke bis zum Boden. Zwischen diesen beiden Fischen kommt es zu einer Form der Auseinandersetzung, die ich bisher nicht gesehen hatte. Sie stehen fast 1 m voneinander entfernt und vollführen abwechselnd scharfe Vorstöße gegeneinander, ohne sich dabei je näher als 20 cm zu kommen. Offensichtlich ein Konflikt aus hoher aggressiver Motivation und ebenso hoher Furcht [=Fluchtbeitschaft]. Ein solches Hin-und-Her wiederholen die beiden in der Folge oft, am 26. V. kommt es zwischen ihnen zu einem sehr intensiven Kreiselduell, an das sich beidseitiger Rückzug mit Hin-Her anschließt. Von den Kollektivbesitzern darf nur Fris in das Revier Kunas eindringen und wird dort mit Abwendung "begrüßt". Gegen Glu und Bajo ist er zurückhaltender, doch kann es vorkommen, daß Kuna dicht an einem der neben ihm stehenden Großen vorüberschwimmend, den Knopf angreift. Die Grenzausinandersetzungen Kuna—Knick dauern an. Knick dehnt sein Revier an die Hinterwand aus, 18 45 vollführt er ein Vorstoß-Wende-Duell aus dem "Tunnel" heraus mit dem von vorne angreifenden Knopf. Fris schwimmt ungehindert in Knicks Revier rechts hinten, dieser kommt ihm entgegen und wendet ab, stößt aber unmittelbar darauf wütend auf den von ferne links sich nähernden Knopf.

27. V. Knick darf sich in der hinteren Beckenhälfte frei bewegen, wird also von Glubsch und Fris toleriert, nur als er ganz nach vorne, in den Focus von Bajo kam, trieb ihn dieser ziemlich milde fort. Mittags schwimmen alle 3 unangefochten in Knicks Revier rechts hinten, an der Grenze zu Kuna tritt ihm dieser entgegen, es bleibt aber bei friedlicher Abwendung und Rückzug beider in ihre Reviere. Etwas später Bogenschwimmen beider an der gleichen Grenze, beide mit grauen Schläfen (defensiv!). 12 00 Bajo und Fris Scheinablaichen an genau derselben

Stelle wie [...]. 12 45 Fries schwimmt im freien Wasser auf Knick zu, der spreizt breitseits [imponieren]. Fris wendet ab, gleich darauf ebenso friedlich endendes Breitseitsimponieren zwischen Knick und Kuna an der Reviergrenze. Abends, 18 Uhr schwimmen alle 6 *Zanclus* friedlich an der Vorderscheibe. Dies ist eine Scharbildung, die immer dann erfolgt, wenn die Fische *weg* wollen. Die Vorderscheibe mit dem dunklen Beobachtungsraum wirkt offenbar wie das tiefe Wasser des offenen Meeres. Nach einiger Zeit wieder Breitseitsimponieren, einmal stößt Knopf nach Knick, der ohne Erregung nach hinten (Richtung seines früheren Revieres) ausweicht.

Am 28. V. ist der kleinere *Zanclus* Knick wie ausgewechselt. Schon vormittags sehe ich einen wilden Kreiselkampf zwischen Kuna und Knick, mitten in Kunas Revier, in das sich Knick gestern noch nicht hineinwagte, Knick sitzt heute im früheren Revier Kunas. Dieser hat eine große, dreieckige Rißwunde über die Analis bis auf den Bauch. Knick patrouilliert pausenlos am Boden unter unter Kunas Styroporfalte und obwohl Kuna von oben her wiederholt auf ihn hinabstößt, wird er von Knick mehr und mehr zurückgedrängt. Dieser zeigt hochintensive Bewegungsweisen des Kämpfens, die ich bis dahin an *Zanclus* nie gesehen hatte. Er verbeißt sich einmal in Kunas Maul, sodaß ein Maulzerren wie bei Cichliden zustandekommt, ein anderes mal packt er den in seine Deckung fliehenden Kuna an der Schwanzflosse und zieht ihn wieder hervor. Knick greift Knopf fast ebenso heftig an wie Kuna und zwingt ihn, sich aus dem Kollektivrevier vorläufig in sein früheres Versteck hinter der linken hinteren Kulisse zurückzuziehen. Die 3 großen *Zanclus* sind von diesen Umwälzungen im Gebiet der Kleinen kaum betroffen. Bajo und Fris scheinlaichen einmal an der alten Stelle, Bajo vertreibt einmal milde Knick, als der an ungewohnter Stelle steht.

Am 29. V. ist die Fehde Knick-Kuna etwas abgeklungen, Kuna kann recht vorne am Boden weiden, geht aber doch nach oben in seine Deckung, als Knick naht. Dieser seinerseits muß es sich aber (in nicht beobachteter Zeit) mit dem Kollektiv Glubsch, Fris und Bajo verdorben haben, denn während er sich vorher frei im ganzen Becken bewegen durfte, wagt er sich jetzt nicht mehr aus seinem—allerdings recht groß gewordenem—Gebiet an der rechten Wand heraus, jedenfalls nie bis über die Beckenmitte nach links und wird auch dort nie von den 3 Großen besucht. Ich sagte morgens: "Wenn Kuna gescheit wäre, würde er ein Revier links vorne etablieren". Um 12 50 hat er genau dies getan und schwamm unangefochten unter den Dominanten, auch Knopf

durfte sich nun diesen anschließen, wenn sie sich links im Becken befanden, rechts durfte er und Kuna sich von Knick nicht sehen lassen, dieser seinerseits durfte nicht in die linke Beckenhälfte. Knopf und Kuna begegneten sich nun nach kurzer Pause wieder, Kuna ist erheblich kleiner als Knopf und bedeutend kleiner als die 3 Großen. Alle 5 weiden friedlich dicht nebeneinander. Als sie dabei weit nach rechts kommen, kommt es zum Kreiselduell Knopf-Knick, gleich darauf greift Kuna Knick an, Knick wird an der rechten Wand etwas zurückgedrängt und verliert das Gebiet, in dem er noch gestern Kuna so hart bedrängte. Knopf und Kuna sind nun viel friedlicher gegeneinander, mehrmals Abwendungen beobachtet. Knick wagt sich nicht einmal in die Mitte des Beckens, geschweige denn nach links. Einmal, als er ziemlich frei im Wasser steht, kommt Bajo von der Beckenmitte her auf ihn zu—ohne Angriffsintentionen—Knick verschwindet in seiner Deckung rechts an der Wand. Doch zeigt Knick "Grenzverhalten" in Bezug auf das in der Abb.[fehlt]. skizzierte Gebiet. Hin-Her rammstoßen um 18 55 am Eingang in den "Tunnel", den Knick verteidigt und in dessen Eingang er kurz darauf nach einem Kreiselduell gegen Knopf flieht (am 2. VI. 10 55 greifen Knopf und Kuna zugleich den Knick an derselben Stelle an).

1. VI. 1977 (zurück von Grünau) Kuna ist sehr viel freier geworden, er darf bis fast an die linke Wand, seine 2 getrennten Reviere sind nunmehr durch eine Brücke Verbunden. Knick dagegen ist wie vorher auf die rechte Beckenhälfte beschränkt. Von den 3 Großen kein Angriff, nur einmal wird Bajo andeutungsweise aggressiv gegen Knopf, als dieser ganz an die Vorderscheibe kommt. Knick seinerseits beginnt immer deutlicher, sein Revier abzugrenzen, es kommt zum Kreiseln mit Knopf, der ihn in Deckung zurücktreibt. In den nächsten Tagen, vom 2. bis 9. VI. ist Knick immer einer der Kampfespartner, sei es als Angreifer oder als Angegriffener. Die einzigen sonstigen agonistischen Auseinandersetzungen bestehen im Wettschwimmen von Fris und Bajo. Wettschwimmen ist die intensivste Form des Kampfes zwischen Kollektivrevierbesitzern und endet typischerweise darin, daß am Ende jeder der beiden seinen Focus aufsucht. Zwischen Knick und den 3 Großen entwickelt sich ein asymmetrisches Grenzverhalten: Sie dürfen in sein Gebiet hinein, aber er nicht heraus. Knopf und Kuna werden von Knick als vollwertige Reviernachbarn bekämpft und vertrieben. Im Kollektivrevier von Bajo, Glubsch und Fris werden sie geduldet. Besonders eindrucksvoll ist es, wenn Knopf und Kuna in nächster Nähe eines oder mehrerer der 3 Großen weiden und aus

tiefstem Frieden heraus den Knick anreifen, der es wagt, aus seinen Reviergrenzen herauszukommen. In der Woche vom 2. bis 9. VI. werden 15 agonistische Auseinandersetzungen zwischen einem der beiden Kleinen und Knick verzeichnet und nur 9 zwischen Knick und einem Großen. Zwischen 4. und 8. VI. wurde nicht beobachtet, in dieser Zeit muß einer der *Zanclus* heftigst gegen Kuna gekämpft haben, dieser ist bei meiner Rückkehr völlig zerfetzt und in seiner sowieso leeren Augenhöhle ist eine frische rote Wunde, es muß also zum Nasenkringeln gekommen sein. Knopf ist zunächst unsichtbar, wird aber anschließend in seinem alten Versteck hinter der linken hinteren Kulissee entdeckt, also vom Kollektiv-Mitglied zum "Außenseiter" degradiert. Kuna fürchtet sich nicht vor Fris, sondern weidet dicht neben ihm, verschwindet aber hinter seiner Deckung rechts vorne oben, als Knick von weitem herankommt. Er ist körperlich nicht merklich geschädigt und macht Vorstöße von oben er gegen Knick. Einmal kreiseln die beiden sogar, wonach Knopf wieder [Rest des Satzes fehlt, soll wahrscheinlich heißen: seine Deckung aufsucht].

Am 15. VI. 1977 setze ich 2 weitere *Zanclus canescens*, einen von der Größe von Bajo, Glu und Fris, sowie einen kleineren von der Größe Kunas ein. Sie werden von allen alten *Zanclus* wütend gejagt, auch von Kuna und Knopf, die zu diesem Zweck weit aus ihrer Deckung vorstoßen. Da die beiden Neuen zunächst nur fliehen und nach Deckung suchen, geraten sie besonders häufig an die vielversprechende Deckung der linken hinteren Dekoration, die aber von Knopf erbittert verteidigt wird. Schließlich finden sie hinter dem zylindrischen Topf der Turbelle Ruhe, und zwar beide, ohne zunächst aneinanderzugeraten. Sie sitzen also jetzt innerhalb des linken Teilreviers von Kuna. Dieses Gebiet wird durch sie ein wenig aus der [Ebene?] heraus nach rechts und nach hinten gedrängt, was dadurch begünstigt wird, daß Kuna vom "Revierdruck" der 3 großen entlastet ist, die durch die beiden neuen Fische abgelenkt werden. Als am nächsten Tag Bim sich allmählich längs der Vorderscheibe aus der Ecke hervorwagt, greift [er?] ihn an und jagt ihn zurück. Keine Anzeichen agonistischen Verhaltens zwischen Bim und Kleinneu. Bajo wird allmählich immer aggressiver und patrouilliert schließlich die linke vordere Ecke dauernd. In den nächsten 3 Tagen werden 35 agonistische Auseinandersetzungen zwischen alteingesetzten *Zanclus* und den beiden Neuen beobachtet, ein einziges Kreiseln zwischen Knopf und Kuna verdirbt die Statistik, zu dem gesagt werden muß, daß die beiden aneinander gerieten,

als sie von 2 Seiten her den kleinen Neuen jagten und dieser vor ihnen in einer Spalte verschwand. Befriedigungszeremonien und dichtes Beisammenstehen der beiden Neuen wurde am 16., 18. und 19. VI. gesehen, danach nicht mehr und am 19. griff Bim den Kleinen zum ersten Mal an. Der kleine Neue versucht längs der linken Wand nach hinten zu explorieren, wird aber immer wieder zurückgejagt, seine Versuche, das Gebiet Knopfs hinter der linken Kulissee zu erobern, scheitern immer wieder. Knopf wagt sich dabei immer weiter hinter der Deckung hervor. Wenn das geschieht, stößt Kleinneu weit aus der linken Ecke heraus vor und greift Knopf an, wird aber durch die Einmischung Knicks sofort zurückgeschleucht. Dieser Vorgang wird viermal beobachtet, bis es am 21. VI. 9 25 dem Kleinneu gelingt, bei Knopf einzudringen. In der Deckung der Kulissee muß sich ein wilder Kampf abgespielt haben, dann kam Klein herausgeschossen und wurde sofort von Knick weiter und in die linke Ecke gejagt. Am 22. VI. wiederholt sich dasselbe; als Kleinneu von Knopf hinter der Kulissee herausgetrieben wird, sind Knick und Kuna sofort hinter ihm her "wie Hunde hinter einem Hasen". Schließlich beteiligt sich auch Glubsch und Kuna. Die Kämpfe erreichen den höchsten Intensitätsgrad, das sogenannte Nasenkringeln, und irgendwann im Laufe dieser Auseinandersetzung hat Knopf sein linkes Auge eingebüßt.

Bemerkenswert an den Beobachtungen jener Tage ist das Fehlen von Auseinandersetzungen zwischen den alteingesessenen Fischen, die in der Verfolgung der beiden Neuen, vor allem des Kleinen, oft in großer Aggression räumlich dicht zusammenkommen, ohne einander etwas zu tun. Ebenso bemerkenswert ist das immer wieder gezeigte aufgeschobene Handeln (delayed response), wenn sie, oft nach längerer Pause, den in Deckung befindlichen und ganz sicher keinem von ihnen sichtbaren Gegner suchen und schließlich ins Freie treiben.

Ab nun jagen alle alteingesessenen Fische den Kleinneu, auch Bim greift ihn nunmehr wütend an, wenn er in das bisher gemeinsame Gebiet eindringen will und kommt dabei weit heraus. Der Gejagte ermattet rasch, zu Mittag kämpft er dennoch mit Nasenkringeln gegen Kuna, 15 00 wird er tot in der Spalte des Filters gefunden. Kuna versucht, in sein früheres Teilgebiet in der linken vorderen Ecke einzudringen, wird aber von Bim daran gehindert. Am 23. und 25. VI. (am 24. nicht beobachtet) zählte ich 14 agonistische Auseinandersetzungen zwischen den alteingesessenen Fischen, 3 Kämpfe von Glubsch, 2 von Knick und einen von Kuna gegen Bim. Schon am 26. VI. wird Bim weitgehend toleriert

und schwimmt im ganzen Becken frei herum, weidet z.B. dicht neben Glubsch am freien Boden, wendet sich aber abwehrend gegen diesen, als er ihm in sein früheres Territorium in der linken vorderen Ecke folgt. Am 27. VI. wagt er sich so weit nach hinten, daß er in die Nähe von Knopfs Revier kommt, der sich inzwischen nicht mehr nr hinter, sondern auch an der Vorderseite der linken hinteren Kulisse aufhält. Es kommt zu ganz besonders wütenden Kämpfen, die beiden kennen sich ja bisher nicht und es kommt in den nächsten Tagen zu heftigen Kämpfen zwischen beiden. Am 30. [VI.] besteht zwischen Bim und den 3 dominanten *Zanclus* das schon einmal bestehende einseitige Verhältnis, d.h. sie tolerierten ihn in ihrem kollektiven Revier, während er sie seinerseits nicht in sein Gebiet eindringen läßt. Durch die teilweise erfolgreichen Angriffe auf Bim gewinnt Knopf an Mut und wagt sich immer weiter aus seiner Ecke. Am 1. VII. greift Knopf den Knick von Weitem an, später kommt es zum Kreiselkampf zwischen den beiden. In den nächsten Tagen wird Knick zunehmend aggressiver gegen Knopf, doch bleibt ihm der Einäugige nichts schuldig, besiegt ihn und verfolgt ihn am 2. VII. 19 00 bis weit nach rechts hinten. In den nächsten Tagen ändert sich im Revierverhalten der Fische wenig, zwischen den drei Dominanten gibt es die üblichen [ein unleserliches Wort] Reibereien, zwischen Fris und Bajo kommt es wiederholt zum Wettschwimmen, an dessen Ende aber beide friedlich nebeneinander stehen bleiben, zwischen ihnen sieht man auch wiederholt Befriedungsgesten und dichtes Zusammenschwimmen. Knopf greift manchmal heftig den Bim an, wagt sich weit vor und imponiert breitseits, es kommt zum Wettschwimmen, in das aber Knick eingreift, der beide auseinander und Knopf zurück in seine Deckung treibt. Knopf steht am Abend des 8. VII. im Eingang zu seiner Deckung, sieht den Knick von weitem und bekommt daraufhin, ohne sich sonst zu bewegen, graue Schläfen. Um Knopf zu schützen, stellen wir eine große Fächerkoralle vor den Eingang zu seiner Deckung. Knopf gewinnt durch diese Maßnahme tatsächlich an Mut und greift wiederholt in raschen Vorstößen andere Fische an. Besonders heftig aber Bim, obwohl er dabei entlang der linken Wand besonders weit aus seinem Versteck hervor muß. Er hat meßbar am meisten Angst vor Knick, bei dessen Herankommen er graue Schläfen bekommt, was er bei gleicher Annäherung der weniger aggressiven Bajo und Fris nicht tut. Eine vorübergehende Erkrankung von Knick und Glubsch hat wahrscheinlich mit einer Erweiterung von Knopfs Territorium zu tun. Glubsch magert merklich ab, frißt nicht und schwimmt

müde, ohne zu fixieren und ohne jemals aggressiv zu werden im Becken umher. Niemand greift ihn an, niemand wird durch die offensichtliche Veränderung seines Verhaltens im individuellen Erkennen gestört. Erst am 21. VII. greift er erstmalig wieder an und zwar den Knick. Fris und Bajo liefern sich manchmal Wettschwimmen, die friedlich enden. Der noch etwas kranke Glubsch greift am Abend des 21. in heftigem Ansturm den Fris an, stoppt jählings und wendet in Befriedungsgeste ab. Man kann sich der Interpretation nicht entziehen, daß der noch etwas kranke Fisch sich "geirrt" hat. Im nächsten Augenblick greift er Kuna an und vertreibt ihn. Knopf erweitert in der Folge sein Territorium, besonders auf die Vorderscheibe zu und die 3 alten Großen beginnen toleranter gegen ihn zu werden. Es kommt nun wiederholt folgendes Verhalten vor: alle vier großen Fische, die 3 Alten und Knick, weiden an der Grenze zu Knopfs Territorium, Knopf schießt hervor und greift Knick an, auch wenn dieser ihm am Fernsten ist und er dicht an Glubsch, Fris und Bajo vorüberschwimmen muß. Umgekehrt wird am 23. VII. beobachtet, wie alle 7 Fische, also einschließlich Knopf am Vorderrande von Knopfs gegenwärtigem Revier (Abb. [fehlt]) am Boden weiden, als Knick unvermittelt auf Knopf losfährt und ihn hinter die linke Kulisse zurücktreibt. Wie das Auszählen der agonistischen Auseinandersetzungen ergibt, besteht die größte Feindschaft [hier fehlt eine Zeile im Manuskript mit dem Ende dieses Satzes und den Beginn des nächsten... wahrscheinlich "zwischen Knick und Knopf"] ...sowie ein Ansteigen der Auseinandersetzungen zwischen Knopf und Kuna. Am 4. VIII. [1977] wird erstmalig Wettschwimmen zwischen den beiden beobachtet, am 5. VIII. plötzlich intensivste Kämpfe, die zu wiederholtem Nasenkringeln führten. Knick mischt sich nicht ein, das eine noch vorhandene Auge Kunas ist nach den Kämpfen stark getrübt, man hat den Eindruck, daß beim Nasenkringeln nach dem Auge des Gegners gebissen wird und daß die beiden Einäugigen es beim Nasenkringeln verloren haben. Am 6. VIII. gibt es immer noch heftige Kämpfe und Wettschwimmen zwischen Knopf und Kuna, diesmal mischt sich Knick ein, worauf Kuna "fludert" und sogar nach rückwärts schwimmt. Nach diesen Ereignissen steht Knopf meist an einem großen Gorgonien skelett rechts im Hintergrund des Beckens. Er toleriert Fris und Bajo, stößt jedoch wild gegen Bajo vor und jagt ihn bis in das linke vordere Eck, dort stellt sich Kuna und es kommt zum Wettschwimmen, nach welchem sich Knopf an die Mitte der Hinterwand begibt.

— Here, KL's original text ends —

Summarized results

An abbreviated translation of Lorenz' original German text by Kotrschal

Chronology of events

Konrad LORENZ reports, that in Seewiesen, in the early 1960ies, 7 juvenile *Zanclus* (Z) first defended individual territories after introduction into a big reef tank of $1.8 \times 1 \times 1.5$ m (l \times w \times h), but later schooled. However, the behavioral interactions during this transition were not observed. While schooling, individuals were still dominant near the centers of their former territories.

In 1967, LORENZ observed on Hawaiian reefs that adult Z (12–16 cm body length) schooled in groups of 20–30 individuals, whereas close to the reef substrate, the spaced-out juveniles were territorial. As a confrontation experiment, 5 Z were released, one at a time, at the reef close to juvenile, territorial Z from transparent containers. Initially, each resident Z fled from the new one, which attempted to join, but turned, probably at the centers of their territories, attacked and chased the intruder. Up to 3 neighbouring, territorial Z joined the chase without ever showing agonistic behaviors towards each others.

Observations and experiments in the big Altenberg reef tank started in April 1976 (see Materials and Methods). Only the smaller of the two first Z attacked the 8 Z, introduced shortly thereafter; these matched his own size (approx 6 cm). Within hours and days, the novel individuals established small territories, but starved to death within months, because they were not trained to take artificial food prior to introduction. In June 1976, the remaining 3 new Z had firmly established territories of fluctuating sizes.

By the end of June 1976, the small Z started schooling together with one of the later introduced Z. Agonistic interactions along territorial borders decreased and were replaced by increasingly ritualized "(pseudo-)attacks" (approach-ram to approach-and-turn, Tab. 1), which was later interpreted by LORENZ as a "greeting"- "appeasement"-ceremony within individual dyads. Both Z were increasingly aggressive against the original, large Z, which showed submission by "floundering" (Tab. 1). Fights with this large Z often assumed the ritualized form of tight and fast parallel swimming ("racing", Tab. 1). Till September 1976, the large Z gradually became dominant over the two smaller ones, which both disappeared by September 17., probably killed by the large Z. The latter died in December 1976, diseased and blind.

End of January 1976, 8 very small, postlarval Z were purchased and trained to take artificial food, before 5 of them were introduced into the large reef tank on February 10, 1976. Within the first few hours, they formed a dense school, started dyadic "racing" (Tab. 1) the same evening and engaged in territorial fights the other day; the first "pseudo-attacks" (Tab. 1) were observed between particular dyads.

In the following months, LORENZ observed a spectrum of social behaviors listed in Table 1. The five Z defended territories of different sizes, their borders fluctuating, along the bottom and walls of the tank. In March 1976, three dominant Z each defended approximately one third of the surface, the two others were subdominant, confined by attacks of the others, to shelters which formed the foci of small territories. March 22, two of the dominant Z ceased to be aggressive towards each others, showed "pseudo-attacks" (Tab. 1) and soon patrolled a common territory, formed by fusion of their original territories, which they defended against the three others. No submissive behaviors, such as raising of the head by the subdominant in many other percomorph fishes were observed, except for "floundering" (Tab. 1). Natural dawn and dusk, but also disturbances, such as somebody jumping into the tank or sudden, untimely dimming of the light, caused all 5 Z to form a dense school. Afterwards, individuals returned to their territories and resumed specific dyadic interactions.

After fusion of their territories, the two Z were less aggressive against the three others than before. Till the end of March 1977, the third Z with an initially large territory was accepted by the former two within the common territory, which covered now three quarters of the tank. Before that, the newly accepted individual approached the others in a conspicuous "eel-like" mode of swimming (Tab. 1). In occasional agonistic interactions between these three individuals, each was dominant close to the former center of its territory. These three Z were also largest now (approx. 6 cm) and the remaining two subdominants were growing more slowly. Following fusion of territories, dyads occasionally engaged in benthic "pseudospawning" (Tab. 1).

Agonistic interactions between the three dominant holders of the common territory and the other two remaining small, territorial Z remained dyadically distinct, which is shown by a summary of dy-

German name by Lorenz	English translation	Definition
agonistic context		
Breitseits-imponieren	lateral display	Opponents parallel to each others, head-to-tail or head-to-head, dorsal, anal and caudal fins fully spread
Schwanzschlag	tailbeat	Follows lateral display, slow, high-amplitude tailbeats sends waves towards the head of the opponent.
Kreiseln	circling	Starting from a parallel, head-to-tail-position, two opponents swim after each others, which results in a quick rotation at the spot around a common axis in the center between the two bodies. May merge into chases in asymmetric, and "racing" in more symmetric dyads.
Wett(Parallel-)schwimmen	"racing", fast parallel swimming	Fish swim very fast head-to-head, in parallel and in a close distance to each others over quite a distance of reef, as usual, only propelled by their pectoral fins. The limited space of a tank forces fish to swim wide circles. Only at sharp turns, the outer fish may use it's caudal fin for seconds. LORENZ quotes W. BEEBES, that during this ceremony synchronized turning gives the impression of a single fish. May occur only in Z and is the most severe form of agonistic interactions within Z holding a common territory and typically ends with each of the opponents approaching the foci of their former territories.
Schwalbenstellung	attack-approach	No description found in LORENZ' notes.
Rammstoßen	ram-attacks	Dominant fish first slowly approaches the other with body slightly tilted downwards along its longitudinal axis, the followed by a quick ram-attack, usually directed towards the flanks of the opponents; biting may be involved.
Scheinrammstoßen	ram-pseudo-attacks	As above, but the ram strike is stopped well before the opponent's body is reached.
vor-zurück	ram-display at a distance	Opponents face each others at their territorial border at 0.2–1 m distance and direct short ram-bursts towards the other, after which this individual slowly backs until approximately it's initial attack position is reached.
Maulzerren	mouth-lock	Fish interlocked at each others jaws in a very cichlid-like way, vigorously pushing and pulling each other.
"Nasenkringeln"	frontal lock	Opponents interlock their fronts, or push towards sides and heads of each others, pulling, circling, trying to bite each other, preferentially towards the opponent's eyes, a form of escalated biting.
Beschädigungs-beißen	escalated biting	May result from "frontal lock" or other agonistic behaviors. No ritualized elements are shown, the opponent is chased and bitten towards fins, belly and other parts of the body.
Flucht und Verfolgung	flight and chase	Rapid escape swimming towards shelter. The fleeing individual may be chased by one or more dominant individuals towards or even within its shelter; may be in sequence with escalated biting.

Table 1: The following social interaction behaviors were reported for *Zanclus* by LORENZ

German name by Lorenz	English translation	Definition
“submission, appeasement”		
graue Schläfen	grey sides of the head	A broad, vertical streak behind the eyes with blurred contours fades from dark-blue to gray. May indicate defensive readiness to fight back in subdominant individuals.
flundern	“floundering”	Subdominant individual tilts, with the front towards the dominant, partially to one of its sides.
Schlängel-schwimmen	“eel-like swimming”	Individual swims on a conspicuous wave-like path towards other Z. These may join, resulting in a whirling ball of fish, swimming in and out. Was rare, context unclear, maybe “appeasement” ceremony; the initiating fish was sometimes just on the brink of being accepted by the common territory holders.
Steinputzen–Scheinablaichen	substrate-cleaning, pseudo-spawning	Two individuals first clean a rock surface in a cichlid-like manner and afterwards perform the belly-to-substrate gliding movements with backwards folded and shivering fins, typical for percomorph benthic spawning, but no gametes are released. Initially interpreted by LORENZ as a pairbond ceremony, but when it came clear, that it is performed with changing partners, was re-interpreted as dyadic appeasement, ritualized from sexual behavior. Was always shown by all dyads at the same place.
zuschwimmen–abwenden	approach-and-turn (“pseudo-attack”)	One fish approaches the other at some distance directly and fast, in a way, an attack is initiated, but performs a sharp turn in front of the approached fish, which does not flee, shortly presenting its lateral side. Within dyads this evolves from attacks upon each others, which may involve ramming after approach and “floundering” of the attacked. Turning of the approaching individual first occurs at territorial borders, but becomes increasingly site-independent. Approach-and-turn is often followed by grazing closely side-to-side. Interpreted as a dyad-specific “greeting ceremony”.
nahes Grasen	grazing side-to-side	Grazing with parallel bodies in a few cm distance to each others, often following “appeasement” ceremonies, in itself interpreted by LORENZ as an expression of closeness.

Table 1: The following social interaction behaviors were reported for *Zanclus* by LORENZ

adic agonistic and “appeasement” interactions (Tab. 2). Against one of the two subdominant Z, two, but not the third of the holders of the common territory ceased to be aggressive. “Pseudospawning” (Tab. 1) also occasionally followed relatively rare dyadic agonistic interactions between the three dominant and schooling fish. The two subdominant Z expanded their territories at the rear of the tank and performed intense agonistic behaviors (“circling”, “racing”, Tab. 1) with each others at their new, common border.

April 18, 1977: As a confrontation experiment, a small tank of opaque plastic with a transparent glass front containing a new Z of approximately the size

of the two subdominant fish, was positioned at the right, frontal corner of the tank. This triggered aggressive actions, notably between of the dominants and one particular subdominant, which frequently left its territory at the rear of the tank and approached the box to perform “lateral displays” there (Tab. 2). Initially, there was hardly any agonistic behavior shown by the three dominant Z towards the newcomer, but the agonistic displays of the subdominant Z in front of the box seemed to “spark over” to the dominant Z, which increasingly showed “lateral displays” and “ram”-attempts towards the fish within the enclosure after they initially ignored it for days. The frequency of agonistic displays towards

the box increased over the next days and the Z within its enclosure replied by "lateral display" (Tab. 1). The attention of the 3 dominant Z towards the new individual released some pressure from the two subdominant Z at the rear of the tank, which engaged into intense agonistic interactions against each others ("circling", "racing", "frontal lock", "biting", Tab. 1). Frequencies of dyadic agonistic and "appeasement" behavior the week before and the week after introduction of the caged Z are summarized in Table 2.

Starting with April 22, one of the two remaining subdominant Z was accepted as a fourth member within the common territory. The remaining subdominant was confined to a crevice between rocks, badly wounded after a severe fight, with torn fins and one eye missing; it again expanded its territory from there during May. It seems from the frequent attacks directed to the eyes, as reported by LORENZ also in later agonistic encounters between Z, that during escalated fighting, Z direct their bites (during frontal-lock-fights and escalated biting, Tab. 1) predominately towards the eyes; the branchiostegal membranes of the opponent are targeted following destruction of the unpaired soft fins, which may cause quick death. There were increased agonistic interactions also between the four dominant Z (though non-escalating and in a dyade-specific manner, "racing" being the most severe form of agonistic interaction observed within the holders of the common territory), when crowding in front of the introduced enclosure.

May 7, 1977: Release of the new Z from its enclosure. Four of the old Z, but not the recently wounded, engaged in intense agonistic interactions against the newcomer, including "chases" (Tab. 1). Its fins were torn until it managed to establish within a shelter at the rear of the tank. Agonistic interactions between the 4 dominant holders of the common territory and the two subordinates (now the one-eyed original Z and the newcomer) were dyadically distinct. One-eye had severe agonistic interactions with only one of the 4 dominants (the one which was accepted last within the group), moderate with two and none at all with the fourth (Tab. 2). Still, occasional agonistic behavior also observed within the 4 dominant Z, related to the focal points of their former territories, but no borders were defended against each other.

May 20, 1977 and following days (Tab. 2): The newly introduced Z was temporarily allowed to join the 4 dominants within their territory. Occasionally, all 6 Z peacefully schooled along the front glass of the aquarium. Till the end of May, most agonistic

interactions occurred between the three smallest, subdominant Z. Occasional aggressions between the dominant territorial holders may have assumed the form of "racing", but never escalated any further. The dominant Z were allowed to enter the territory of the newly introduced Z as a group or individually, but the newcomer was not allowed to enter the common territory.

June 15, 1977 (Tab. 2): Two more Z were introduced, one large in the size of the dominant resident Z, another one small, in the size of the small subordinates. They were immediately and intensely chased by all of the prior 7 residents and took refuge within a tight crevice at the rear surface of the tank. The small new fish was attacked by large new the fish for first time on June 19. Both were frequent targets of escalated attacks by the established residents at any of their strays from shelter. "Frontal locks" and "biting" occurred. On such an occasion, another one of the prior residents lost one eye. When the small new Z sheltered within a crevice out of sight of the others, the latter often "looked for" the opponent, tried to get it out and chase it "as a rabbit is chased by dogs", as LORENZ remarked. It was finally killed June 21. This kind of "delayed response", the search for a subdominant opponent which may have been out of sight for a considerable amount of time, followed by an attack when found, was repeatedly mentioned.

In contrast to the small one, the newly introduced, big Z was quickly accepted by the holders of the collective territory, but, in turn, did not allow them to enter his own territory. The introduction from June 15 seemingly triggered a new wave of agonistic behavior between the old Z (Tab. 2) The same effect was reported by OKAWA for the 1978 introductions. Not directly related to this, one of the old, large Z was sick for nearly one month, stopped feeding and swam slowly high above ground and, was neither joined nor attacked by any of the others. It resumed feeding and active participation in agonistic interactions by end of July. Again, an injured (later recovered) eye was reported after a dyadic agonistic interaction.

What happened after August 1977?

The temporary social climax reached between the 7 surviving Z in summer 1977 is summarized in Table 2. Later, observations and experiments continued with occasional introductions of new individuals. LORENZ's protocols continued till summer 1980 and, less regularly, till spring 1984. Tables with systematic interaction data extracted by LORENZ from his

		Agonistic interactions					Percentage of interactions	
		Kun	Kno	Baj	Fri	Glu	agonistic	appeasement
Appeasement	Kun	—	10	4	10	10	22	21
	Kno	0	—	1	1	28	26	7
	Baj	0	1	—	6	5	10	14
	Fri	3	0	1	—	3	6	43
	Glu	0	0	0	2	—	30	14

Table 2: Interaction matrices (left) between individual *Zanclus* at different stages of social development and percentage of agonistic and appeasing behaviors (right) to underline the presence of specific dyadic relationships. Frequencies of agonistic interactions (lateral display, chases, ram-display, rams, circling, frontal locks, racing); bottom-left: “appeasement” behaviors (mainly approach-and-turn). As total observation effort was unequal between the 5 examples shown, the numbers of the interaction matrix (left) are directly comparable only within an observation period, whereas the percentages (right) are comparable within and between periods.

a: March 8 to April 8, 1977: During this period, individual fish named Glub and Fris, later Bajo join on a common territory (compare Fig.1). Note the high number of agonistic interactions between Glub (dominant) and Knopf (subdominant).

		Agonistic interactions					Percentage of interactions	
		Kun	Kno	Baj	Fri	Glu	agonistic	appeasement
Appeasement	Kun	—	11	3	0	8	26	33
	Kno	0	—	1	1	10	27	0
	Baj	1	0	—	3	4	13	22
	Fri	2	0	1	—	2	7	33
	Glu	0	0	0	1	—	28	11

Table 2b: April 9 to 16, 1977: Bajo, Fris and Glub have established a common territory, Kuna and Knopf are confined to small territories in the rear of the tank. This is particularly reflected in dyadic agonistic interactions.

		Agonistic interactions						Percentage of interactions	
		Kun	Kno	Baj	Fri	Glu	Kni	agonistic	appeasement
Appeasement	Kun	—	6	1	0	12	18	29	44
	Kno	8	—	0	1	1	15	18	13
	Baj	8	0	—	0	1	7	7	12
	Fri	6	0	0	—	0	1	2	12
	Glu	5	0	0	0	—	1	12	9
	Kni	4	2	1	3	2	—	33	15

Table 2c: May 21 to 28, 1977: Bajo, Fris, Glub and Knick patrol a common territory, Kuna and Knopf are still confined to small territories in the rear of the tank. Note that particularly Knick, who joined the group last, is aggressive towards Kuna and Knopf.

notes continue to March 24, 1979. This allows the assumption, that LORENZ intended to proceed with the description much further, but was hindered by circumstances, such as high-priority book projects and health problems. These observations (summer 1977 to 1984) revealed a few more details, but the major results seemed unchanged. No attempt was made (by KK) to continue this report beyond summer 1977, because only LORENZ himself could have possibly distilled crucial results from his notes.

Four of the 5 post-larval Z introduced in January 1977 (Baj, Fri, Glu and Kun) and the single Z released from the confrontation box (Kni) were still alive in May 1978 and had grown to a body length of approx. 13 cm, thus being more than 2 years old. May 30 and June 1 1978, a total of 5 new Z, all approximately 5 cm in body length, probably more than 1 year old, were introduced in the tank. All of the 5 older Z schooled together since summer 1977. The younger Z first interacted agonistically within their cohort, but quickly formed a school of their

	Agonistic interactions								Percentage of interactions		
		Kun	Kno	Baj	Fri	Glu	Kni	L	S	agonistic	appeasement
Appeasement	Kun	—	7	0	0	0	0	4	12	11	17
	Kno	0	—	0	0	0	0	1	11	9	0
	Baj	0	0	—	0	0	0	7	9	9	0
	Fri	0	0	0	—	0	1	8	9	8	0
	Glu	0	0	0	0	—	0	5	6	5	0
	Kni	1	0	0	0	0	—	5	16	10	4
	L	3	0	0	0	0	0	—	7	17	38
	S	0	0	4	0	0	0	6	—	32	42

Table 2d: June 15 to 22, 1977: As May 21 to 28, but two new *Zanclus*, one as large as territorial residents (L), the other one smaller (S) were introduced. The new small Z was killed by the end of this week. Most members of the group were relatively aggressive towards S, but there was also a series of appeasements (approach-and-turn) between Bajo and S.

	Agonistic interactions							Percentage of interactions		
		Kun	Kno	Baj	Fri	Glu	Kni	L	agonistic	appeasement
Appeasement	Kun	—	7	0	0	0	0	4	16	29
	Kno	0	—	0	0	0	0	1	13	0
	Baj	0	0	—	0	0	0	7	12	0
	Fri	0	0	0	—	0	1	8	12	0
	Glu	0	0	0	0	—	0	5	7	0
	Kni	1	0	0	0	0	—	5	15	7
	L	3	0	0	0	0	0	—	25	6

Table 2e: July 23 to August 5, 1977: A stable “social climax” was reached. Bajo, Fris, Glub, Knick and the new L roam their common territory, Kun and Kno, by and large, remain excluded and stay on their small territories. From the frequencies of agonistic interactions and also appeasements, it is still evident that L was last to join the group.

own. Remarkably, there were hardly any interactions between the two age cohorts. At the beginning, they ignored each others as if they would belong to different species.

July 17, 1978, a single, middle-sized individual (approx. 7 cm) was introduced in late morning. The events of this day were recorded in detail by OKAWA (unpubl.). The newcomer was immediately attacked by the younger cohort; in the older group, this induced enhanced swimming. Thirty minutes following this introduction, the first attacks of the older Z towards the newcomer occurred, and also sudden fights erupted between the two established cohorts. Ten minutes later, serial chases developed, where the newcomer was permanently attacked from 1–4 individuals of both cohorts. The individuals of both cohorts preferably started attacks from the centers of their former territories. Two hours after introduction, the newcomer had found a shelter, but agonistic interactions, mainly between the two cohorts of Z continued and only decreased in the afternoon, another two hours later. The newcomer finally died August 8, 1978 from torn branchiostegal mem-

branes. Thus, this intermediately-sized fish did not match body size in either group, was not able to join a group and was attacked by both groups. Remarkably, this also triggered long-lasting agonistic interactions between the two cohorts.

Conclusions and discussion

The lack of field observations on the behavioral events following recruitment in tropical reef fishes underlines the importance of LORENZ’s aquarium observations. Some Pomacentridae, Acanthuridae and *Zanclus* are territorial after recruitment but school later on (LOWE-MCCONNELL 1987). LORENZ has documented the behavioral interactions of Z in territorial disputes and during changes, from individual territoriality to schooling in a common territory. An astonishingly rich repertoire of escalatory and de-escalatory behaviors in dyadic interaction emerged (Tab. 1). Fish are highly orientated in space (comp. REESE 1989), recognize each others individually (which was shown for other reef fish before, FRICKE 1973), as peculiar dyadic

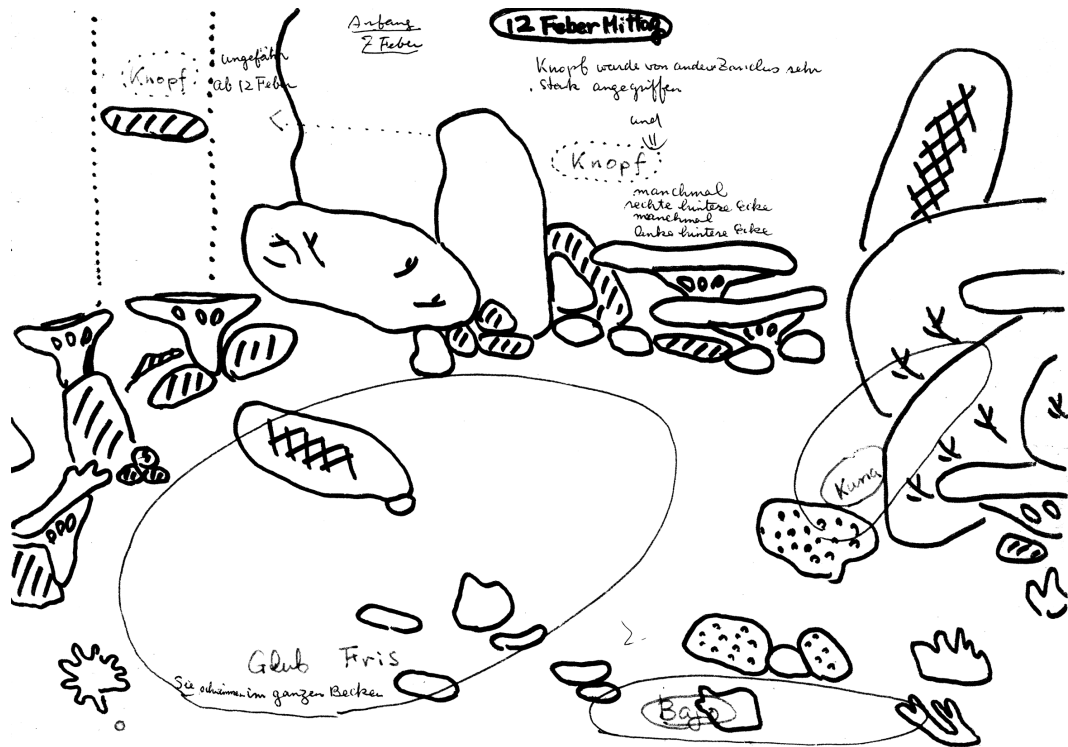


Figure 1: Examples of original sketches by Konrad LORENZ showing territorial boundaries and, by arrows, directions and intensities of dyadic interactions, mapped onto the bottom, at the rear and lateral walls of the tank (1 x w x h: 4 x 4 x 2 m).
a: February 12, 1977. Individuals Glub and Fris hold a common territory as indicated by the central circle. Other territories are held by Bajo (front right) and Kuna (behind Ba, right). Knopf is confined to the rear wall of the tank, frequently chased and occasionally defends either of two shelters.

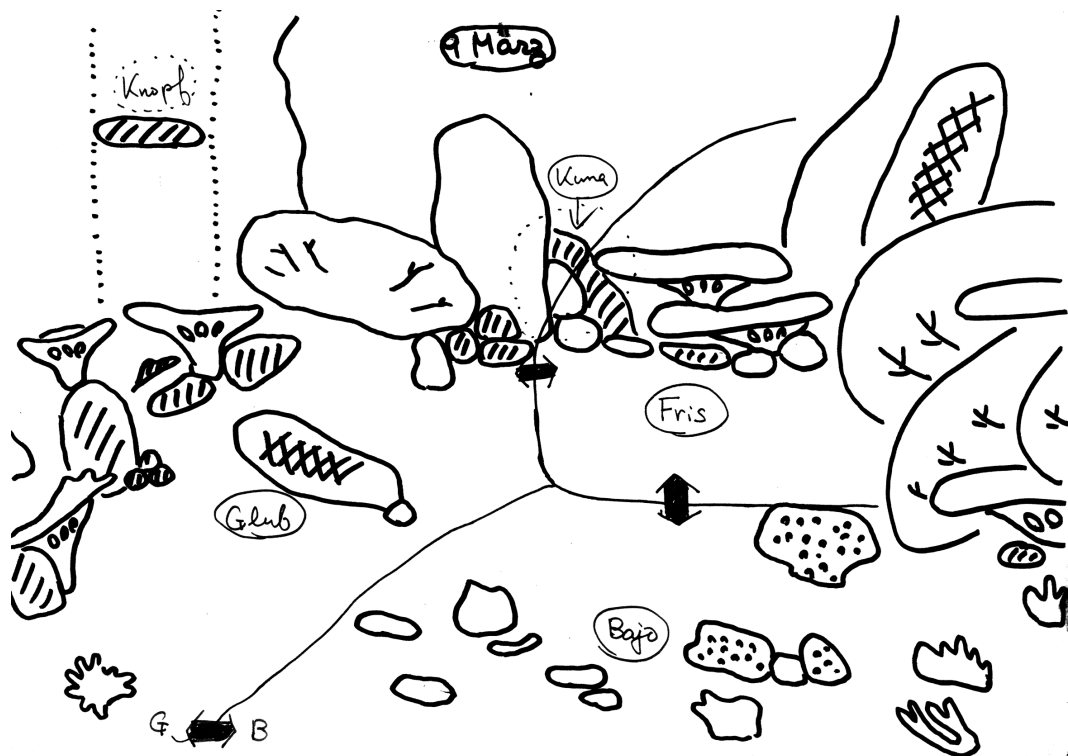


Figure 1b: March 9, 1977. Glub, Bajo and Fris partition the entire bottom of the tank into three individual territories. Knopf is confined to its hole (back left) and Kuna starts expanding its small territory from the rear wall towards the center.

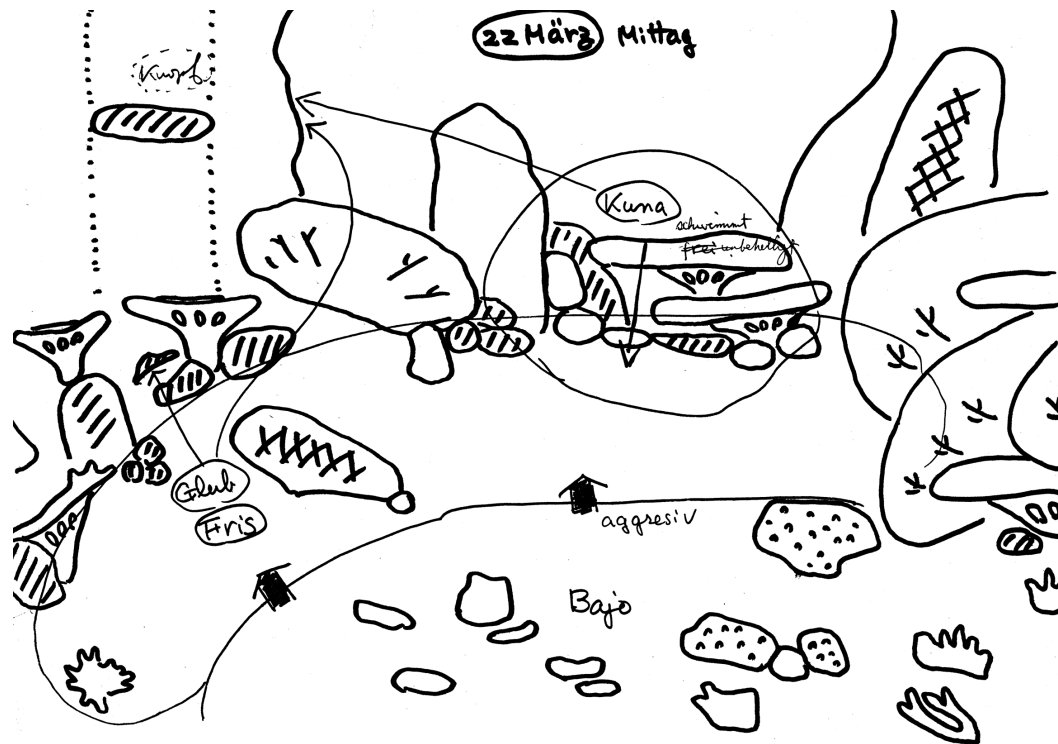


Figure 1c: March 22, 1977. Glub and Fris fused their individual territories, but still exclude Bajo (front), which selectively either attacks Glub or Fris, when either of the two appears in the “wrong” place (the former territory border of the other). Kuna gradually manages to expand its territory from the rear wall towards the center and Kn is still confined to its shelter at the left lateral wall.

relationships are retained for a long time, independent of site, and because dominant fish may search for a subdominant opponent where they expect to find it to engage in agonistic interactions. This points at an “appetence for aggression” (RASA 1971), albeit directed at peculiar individuals. Also, dyadic relationships remain within the school. Thus, individual Z may be able judge their own position and also of the others within the social structure of the school. Such abilities of social cognition were recently attributed to birds and mammals, notably apes (for example, WHITEN/BYRNE 1997; KOTRSCHAL 1995; MARLER 1994; DE WAAL 1982) but are still surprising for fish, even though FRICKE (1975) has shown remarkable problem-solving capacities in a triggerfish.

Initial territories of postlarval Z are first fused within dyads. Even thereafter, individuals tended to remain dominant close to the centers of their former territories. Such group territories are easily formed between Z of simi-

lar size, resulting in a homogenous school. Most agonistic behaviors are indeed directed towards odd or unknown Z. This agrees with observations, that schooling fish match themselves according to size (PITCHER 1986). Also, the possibility to induce schooling by disturbances is consistent with the anti-predator function of groups (EIBL-EIBESFELDT 1962, PITCHER 1986) and on the proximate level is also consistent with LORENZ “hierarchy of instincts”-theory (1978). The causal, evolutionary linkage of peculiarities of Z, which deviate from the usual perciform and acanthurid pattern, such as individualized group territoriality (surgeonfish come as territorial singles or pairs, as “colonies” or as large schools,

EHRlich 1975; FOSTER 1985; ROBERTSON/GAINES 1986); sponge feeding (HOBSON 1974; RUSS 1984) and the absence of a scalpel at their caudal peduncle, remains debatable. Another case of group territoriality was recently described by LUDLOW/ITZKOWITZ (1997).

Territorial conflicts may involve a number of behavioral

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elements which are ritualized to a differing degree. Lateral displays at territorial borders, tail-beats, ram-attacks, mouth-locks, escalated biting and chasing are present in most species of percomorphs (BAERENDS/BAERENDS VAN ROON 1950; ERLACH 1986; KALAS 1975; OEHLERT 1958; OHM 1958; SCHOBER 1988; SEITZ 1940, 1941; WICKLER 1967). Other agonistic interactions, however, such as circling, racing, frontal locks and, to some extent, aiming bites preferentially at branchiostegal membranes and eyes, may be more specific for Z, even though this is hard to judge, because data on behavioral repertoires of other reef fishes are still comparatively rare (SCHOBER 1988). Severe agonistic interactions, such as frontal locks and escalated biting may be exceedingly rare in the wild due to the absence of spatial constraints, and probably due to the fact that high predation pressure may dampen aggression (LORENZ 1978, ERLACH 1986) in the field. In fact, these agonistic (even damaging) behaviors are much more frequently observed in the aquarium than in the wild. This shows, that these behaviors are part of the species' repertoire, available upon demand; the aquarium "artefact" is not the behavior itself, but probably its frequency of occurrence.

Still more remarkable than the agonistic behaviors of territorial disputes are behaviors, which evidently allow socializing and schooling between individually known dyads of former territorial neighbours. Preliminary field observations by LORENZ and others indicate that this transition from postrecruitment territoriality to schooling also occurs in nature. Responses of territorial Z in Hawaii to the release of strangers (return to a certain spot on the reef, from there attacks at the strangers) indicate, that also in the wild, former territorial foci may still function to reassure individual dominance status. Also in the wild, neighbours did not attack each others. These schools do not always contain the same individuals (REESE, pers. comm.), which may mean that individual Z possibly roam extended home ranges in fission-fusion-groups, which dynamically split and fuse according to the position of the individuals relative to their former territories.

Aside from fission-fusion grouping by fish which recognize each others individually, another parallel to primates is the presence of "appeasement" behaviors (WHITEN/BYRNE 1997, DE WAAL 1989). These are by no means necessary attributes of individualized groups; in greylag geese, for example, the major "appeasement"-behavior, "greeting", is only performed by members of triumph-ceremony-

groups, notably within long-term mongamous mates and within families (LORENZ 1988). The repertoire of such behaviors in Z seems greater than in geese and, even though dyadic characteristics remain, their exchange is not exclusive. "Appeasement" in Z may follow agonistic "racing" within a dyad of territorial neighbours, but also submission of one individual by "floundering".

"Side-to-side-grazing", initially along the common borders may first indicate a decrease in the dyadic aggressive potential. In this state of breakdown of territorial borders, dyadic and benhic "pseudo-spawning" may tighten the bond. It remains an open question, whether or not rare "eel-like swimming", which may be derived from pelagic spawning, may be interpreted as an application of the initiating individual to be accepted within the group. Individuals, which accept each others within the common territory, regularly perform "approach-and-turn" behavior after temporary separation, which thus can be interpreted as a "greeting" ceremony, which mutually reassures the non-aggressive nature of the dyadic relationship. Remarkably, approach-and-turn seems to be a ritualized form of attack. The other "appeasing" behaviors originate from the repertoires of feeding and sexuality.

Occasional agonistic interactions between school members center around former territorial foci and consist mainly of "lateral display" and "tailbeats" with "racing" being the most severe form of agonistic interaction observed within a group of common territory holders. This repertoire of "appeasement" only in part parallels such behaviors in mammals, where they may also be ritualized from agonistic and sexual origins, but frequently involve grooming or elements of infantile origin, such as licking the corner of the mouth in canids, which has its roots in the food-begging of pups. It remains to be investigated, whether, and how, this repertoire of agonistic and "appeasing" behaviors of Z is used in the field.

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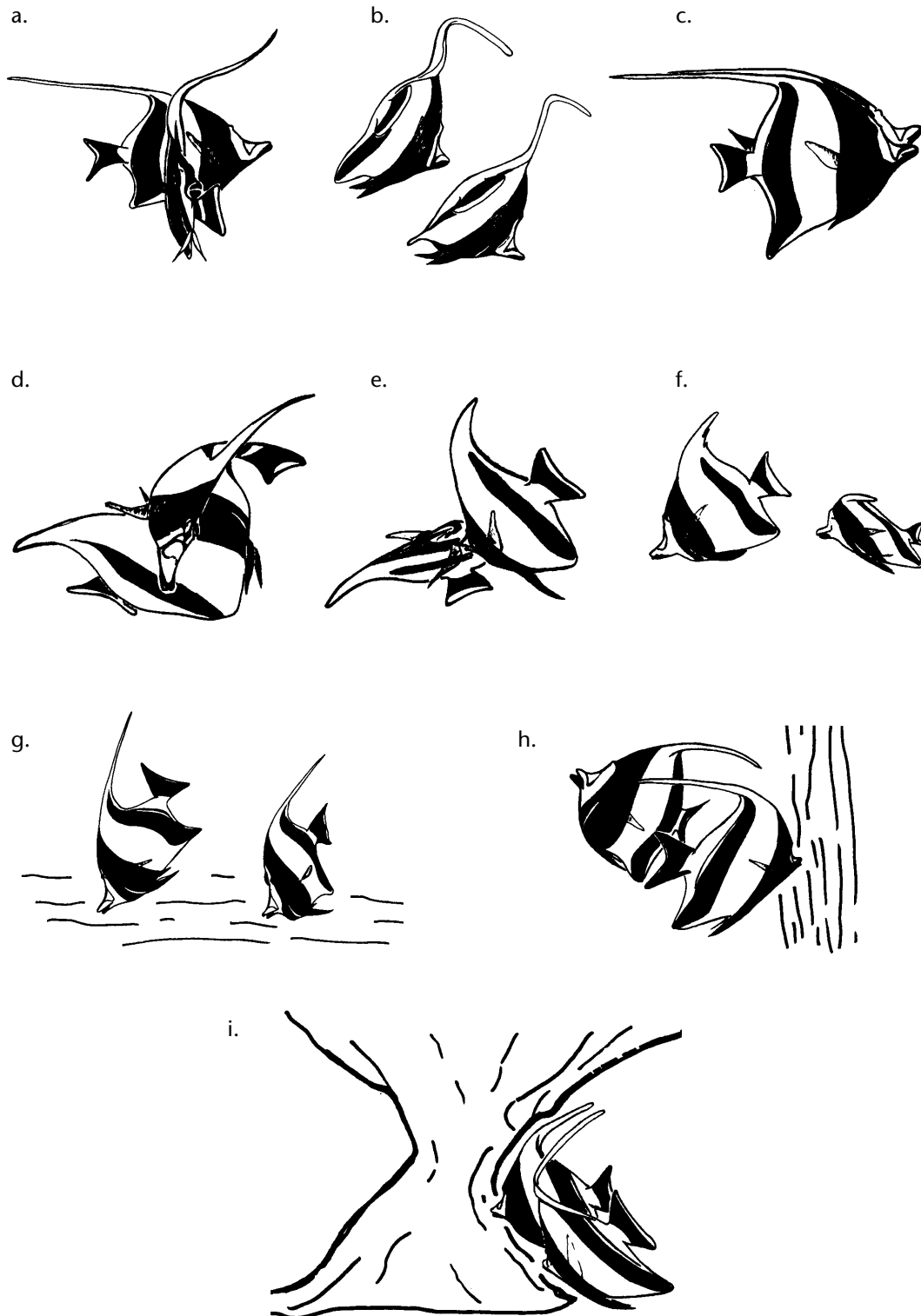


Figure 2: Drawings by Kurt KOTRSCHAL from bw-photographs by Keiko OKAWA showing a number of behaviors defined in Table 1. **a:** Relatively peaceful tail-to-body, with some fin-erect dominance display of the frontal fish. **b:** Head-to-head lateral display. **c:** Fast parallel swimming ("racing"). **d, e:** Scenes of frontal lock and escalated biting. **f:** Chase. **g:** Peaceful grazing side-to-side. **h:** Peaceful standing side-by-side. **i:** "Substrate-cleaning", a sexual behavior which preceds benthic spawning in many perciform fishes. Has probably ritualized in a rare "appeasement" ceremony after fusion of territories in *Zanclus*, which is a pelagic spawner. Gender of the two participating individuals unknown.

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Teleosemantics and the Critique of Adaptationism

It HAS LONG BEEN ASSUMED that there is some (presumably strong) connection between teleological theories of representation and adaptationism in evolutionary biology, but just what that connection is has never been clearly explained. Thus, it is often assumed that any attacks on adaptationism also constitute attacks on teleological theories of representation (or *teleosemantics*, for short). Determining exactly what the connection between these two things is will be the burden of this paper. Before even beginning to try to see what the relationship between adaptationism and teleosemantics is, it will be

helpful to get a rough idea of what the *relata* are. So, roughly, *teleosemantics* is a teleofunctional account of what determines the content of linguistic and mental representations. Thus, teleosemantics determines the content of a representation by determining the representation's function—what it is “supposed to” represent, where the “supposed to” is a *biological* “supposed to”. An object's teleofunction is what it has been *selected* to do, what it has been “designed for”. So, the teleofunction of an object is a function that does not rest on current properties, but instead depends upon that object's history, in particular, its selective history. The most sophisticated version of teleosemantics is found in MILLIKAN 1984, see also MILLIKAN 1993, PAPINEAU 1987, and DENNETT 1995. *Adaptationism*, unlike teleosemantics, has been defined and characterized primarily by those who

Abstract

The point of this essay is to clarify the relationship between adaptationism (and criticisms thereof) and teleosemantics. In order to do this, two questions are discussed: (1) How correct as a methodology does adaptationism have to be to support teleological semantics; and (2) Is adaptationism that correct? In answering the first question, I argue that a teleological account of public language can be separated from a teleological account of mental representations. The teleosemantic account of public language that I suggest is entirely independent of adaptationism, while that of mental representations is importantly dependent upon it. In answering the second question, I examine a few of the more well-known criticisms of adaptationism (problems with optimization, “free rider” criticisms) to determine whether they are successful enough to make a teleosemantics for mental representations impossible.

Key words

Meaning, teleosemantics, MILLIKAN, adaptationism.

wish to criticize it. Adaptationism, sometimes also called “functionalism” (see GOULD 1991), is, again roughly, the tendency of theorists in evolutionary biology to consider organisms as sets of traits, each of which has been optimally designed by natural selection to serve its function. This tendency has been famously ridiculed by GOULD/LEWONTIN 1979 as the “Panglossian paradigm”, see also GOULD/VRBA 1982, KITCHER 1985, and for a defense of adaptationism DENNETT 1983, 1995.

The connection between teleosemantics and adaptationism can be brought somewhat into

focus now. Here it is in a first, rough pass: “Adaptationism is the tendency to assume that all of the individual parts of an organism were selected for, and so have *biological functions*. If, as some have claimed recently, adaptationism is bad methodology, then many parts of living organisms might not have functions. The parts of humans that underly their linguistic and cognitive skills may be among those that do not have functions. So teleosemantics, which determines the content of linguistic and mental representations according to their functions, could be a misguided enterprise.”

Notice that this attempt at spelling out the connection is littered with ‘might’s and ‘could’s. In what follows, I will try to replace these modal words with ones that are more straightforwardly assertoric. To do so, of course, will require considering teleose-

mantics, adaptationism and a few prominent critiques of it in some detail. Here, then, is the plan. First we will consider the occasionally hairy details of teleosemantics, and then, more briefly, what adaptationism amounts to, with the goal of answering the question:

(1) How correct does adaptationism need to be for teleosemantics to be possible?

Next, we will examine some of the more prominent lines of criticism of adaptationism and attempt to determine whether adaptationism is as true as is deemed necessary by our answer to question (1). Question (2), then, is:

(2) And is adaptationism *that* correct?

Before beginning to try to answer these two questions, however, it is worth pointing out that *philosophical* considerations that might cause problems for teleosemantics, like those raised in FODOR 1990, will not be considered here. There are two reasons for this: first, if it turns out that teleosemantics does not survive the *empirical* challenges posed by the foes of adaptationism, any philosophical considerations in its favor are moot; second, FODOR's criticisms have been fairly decisively rebutted by the proponents of teleosemantics (see MILLIKAN 1993 and DENNETT 1995) and so are not really worth considering here.

We shall begin our consideration of question (1) by examining MILLIKAN's teleosemantic theory. Put very roughly, according to MILLIKAN, a thing A has a *proper function*¹ F only if it meets one of the two following conditions:

(1) A originated as a reproduction of some prior item or items that, due in part to possession of properties reproduced, have actually performed F in the past, and A exists because of this performance, or (2) A originated as the product of some prior device that, given its circumstances, had performance of F as a proper function and that, under those circumstances, normally causes F to be performed by means of producing an item like A. (1993, pp13–14).

So, basically, an object has a proper function only if it has been *copied* because of something it can do (satisfying the first clause) or is produced by something with a proper function (satisfying the second clause). MILLIKAN's term *reproductively established family* covers both these cases (1984, pp23–25) So from now on, I will refer to things that were copied or produced by something copied as *members of reproductively established families*. By claiming that to have a function an object must be a member of a reproductively established family, MILLIKAN makes her definition of function different than those of WRIGHT 1973, CUMMINS 1975 and BIGELOW/PARGET-

TER 1987. This is the case because the fact that to have a function something must be a member of such a family makes it the case that an object's functions cannot be determined by merely examining that object's *current* dispositional properties. Instead, the item's function depends upon how and from what it was reproduced, that is, its *history*. In particular, an item depends upon its *selective history*, for otherwise, items which are copied but do not intuitively have functions will count as members of reproductively established families. As MILLIKAN puts it, "Only if an item or trait has been *selected* for reproduction, *as over against other traits*, because it sometimes has a certain effect does that effect count as a function." (1993, pp35–36) So, for example, full (or half or new) moons do not have a function, even though they are members of a reproductively established family, because they were not *selected for*—the moon does not become full (or half or new) because it is somehow better for the moon to be full (or half or new) than to be in another phase. Finally, then, an object has a proper function if and only if it is a member of a reproductively established family and has been selected for. MILLIKAN claims that this definition covers bodily organs, instinctive behaviors, learned behaviors, reasoned behaviors, customs, language devices, as well as conscious intentions and purposes. (1993, p14)²

Now if the critiques of adaptationism to be considered were able to show that nothing has a function in MILLIKAN's sense, we would need delve no further into teleosemantics. They do not, however, and do not even purport to do so³. We will, therefore, press on into the details of MILLIKAN's account. The goal of teleosemantics is to determine the content of *representations* via analysis of their teleofunctions. Thus we must now consider MILLIKAN's theory of representations. Representations are a variety of what she calls *intentional icons*, which, she claims, are distinguished by the following four characteristics:

(1) An intentional icon is a member of a *reproductively established family* and was selected for, thus it has proper functions.

(2) In biologically normal cases (cases similar to those in which the icon has historically fulfilled its function), an intentional icon stands between two cooperating devices, one which produces the icon and one which consumes it. These two devices are *designed* or *standardized* to fit one another.

(3) In biologically normal cases, the intentional icon serves to adapt the consumer device to conditions such that functions of that device can be performed under those conditions.

(4) It is a proper function of the intentional icon to make changes in the consumer in accordance with certain mapping rules. It is the way that the environment must be in order to accord with these mapping rules that determines the *content* of the intentional icon. (1984, pp96–97)⁴

These four features of intentional icons are such that they can be fulfilled *separately* by both linguistic objects, such as sentences of a public language, and mental representations. In other words, it could be the case that sentences of a public language are representations that can be given a teleosemantic account, while mental representations cannot have a teleosemantic account, or vice versa. Furthermore, it could be that both can receive such an account, but *not the same account*.

This is a crucial feature of teleological theories of content. In all such theories, the meaning of sentences is not dependent upon any relation that sentences might have to mental representations. As MILLIKAN puts it, “Thus the intentionality of a public–language sentence is not derived from the *intentionality* of the inner representations that it [biologically n]ormally produces or expresses. Sentences are *basic* intentional items. And they are intentional for reasons that can be described without raising or answering any questions about what the mental is as such.” (1984, p90)

DENNETT, one of the other main proponents of teleosemantics, has made the same claim in many different venues, for example in his disagreements with John SEARLE’s contention that artifacts such as computers can only have *derived intentionality*. DENNETT claims that there is no good distinction between real meaning or intentionality and derived meaning or intentionality. See DENNETT 1995, ch.14 for an example of the denial of this distinction in connection to teleosemantics.

Because sentences and other language devices do not derive their content from mental representations, it is at least a *conceptual possibility* that sentences are intentional icons for reasons independent of the reasons that mental representations are intentional icons. This point is somewhat surprising and crucial for the purposes of this paper. It is *surprising* because it leads to the fact that the meaning of sentences we utter is not necessarily present to consciousness, nor can we know for certain that our sentences mean anything at all. The claim is *crucial* to the purposes of this paper for reasons that will be demonstrated below. So rather than just claim that it is the case that the meaning of language devices can be separated from the meaning

of mental representations, I will prove that it is so. To do so, I will now give an independent teleosemantic account of, first, sentences, and then, mental representations.

Consider a sentence S, “I’d like some pancakes.”

(1s) A token of S is reproduced (that is, repeated) by individual speakers because it causes certain responses in hearers. Namely, the hearers are led to believe that the speaker is in a pancake–desiring state and/or brings it about that the speaker gets some pancakes. Thus S is a member of a reproductively established family (via the first part of the definition). Furthermore, S was selected for, as against other sentences such as S’, “That house is on fire”, because of certain effects that S produces in hearers, which S’ does not. Thus S has a function. So, the first clause of the definition of intentional icons is satisfied.

(2s) The sentence, in normal situations, stands between a speaker and a hearer, who are standardized to fit one another—they speak the same language. This satisfies clause (2).

(3s) The sentence, in normal cases, serves to adapt the hearer so that the hearer can perform some of his or her functions. In the case of S, the hearer may be caused to believe that the speaker desires pancakes or, if the speaker is a waiter or waitress, he or she may bring the speaker some pancakes. This satisfies clause (3)⁵

(4s) The speaker learns to reproduce tokens of the sentence S in certain situations, those in which she or he wishes to produce beliefs or actions in the hearer that correspond to the content of S. That is, S has as its function the production of certain states or actions in the hearer, such that those states correspond to the speaker’s actually wanting some pancakes. Thus S’s function is to map certain environmental conditions, in particular, those that correspond to its content.

Because it satisfies these four clauses, *S is an intentional icon*. Notice, again, that the story told to show that S fulfills these conditions, and so has content, makes no reference to mental representations. A similar story can, of course, be told for any sentence S. Thus, it could be that sentences have teleosemantic content, while mental representations do not.

The story for mental representations is exactly parallel, but it occurs *entirely within the head*. Consider the mental representation M whose content is “There’s food over there”.

(1m) M is produced by a *representation producer*, whose function it is to produce mental representations. The representation producer is part of an or-

ganism's anatomy (presumably, part of its brain) that was reproduced by natural selection because of its ability to produce items like M when environmental circumstances call for it. Thus, M is a member of a reproductively established family (by the second clause of the definition). It was selected for, as against some other variety of representation producer that produces M' (whose content is, say, "The house is on fire.") when environmental circumstances call for M. Thus, M has proper functions and satisfies clause (1) of the definition of intentional icons.

(2m) After being produced, M stands between the representation producer and a *reproduction consumer* (in biologically normal cases). The producer and consumer have been designed by natural selection to cooperate, to "speak the same language".

(3m) In normal cases, M causes a change in the representation consumer such that the consumer can fulfill some of its functions with respect to the conditions indicated by M. For example, M might cause a change in the consumer so that it can initiate motion towards the food. Thus, clause (3) is fulfilled.

(4m) The organism is configured such that its representation producer normally produces M, rather than M', only when environmental circumstances accord with the content of M. This is the case because producing M in when it is true that "There's food over there" causes the representation consumer to enter a state so that it behaves appropriately with respect to the content of M. So the function of M is to cause the representation consumer to enter a state that corresponds to its content by some certain mapping rules. Thus clause (4) is fulfilled by M.

Thus, mental representations are intentional icons and can have teleosemantic content. To the extent that these two separate teleosemantic accounts (1s–4s and 1m–4m) are reasonable, both sentences and mental representations can have contents that are determined by their teleofunctions. Furthermore, they can have them independently. I have labored this point for a reason. The independence of the teleosemantics of language devices and mental representations makes it the case that our initial question (1) will require two separate answers. *That is, the question "How correct does adaptationism need to be for teleosemantics to be possible?" will get one answer for language, and another for mental representations.* Before giving these answers, it will be useful to see in a little more detail what adaptationism is.

Adaptationism, as I mentioned earlier, seems as if it would not be especially easy to characterize because it was initially described by GOULD/LEWONTIN

(in 1979) who wished to criticize it, and thus was from its initial definition something of a caricature of the practice of actual evolutionary biologists. GOULD/LEWONTIN (1979) claim that adaptationist theorizing generally proceeds in two steps. First, "[a]n organism is organized into 'traits' and these traits are explained as structures optimally designed by natural selection for their functions." (1979, p77) And second, "After the failure of part-by-part optimization, interaction is acknowledged via the dictum that an organism cannot optimize each part without imposing expenses on others. The notion of trade-off is introduced and organisms are interpreted as best compromises among competing demands. Thus interaction among parts is retained completely within the adaptationist program. An suboptimality of a part is explained as its contribution to the best possible design for the whole." (1979, p75)

Surprisingly, DENNETT, in his book length defense of adaptationism (1995), endorses a very similar definition of it, proposed later by LEWONTIN. "[Adaptationism] is defined by one of its most eminent critics as the 'growing tendency in evolutionary biology to reconstruct or predict evolutionary events by *assuming* that all characters are established in evolution by direct natural selection of the most adapted state, that is, the state that is the optimal 'solution' to a 'problem' posed by the environment." (LEWONTIN 1983) These critics claim that, although adaptationism plays *some* important role in biology, it is not really all that central or ubiquitous—and, indeed, we should try to balance it with other ways of thinking. I have been showing, however, that it plays a crucial role in the analysis of every biological event at every level from the first self-replicating macromolecule on up." (DENNETT 1995, p238)

So, given that both the most eminent critics and the most ardent defender of adaptationism accept the same definition of adaptationism, we will also accept it. Thus, adaptationism is a methodology⁶ whereby one treats an organism as a set of traits, each of which is *selected for* because it is the optimal structure available to selection for the fulfillment of its function (see also the essays collected in ROSE/LAUDER 1996).

Now, finally, we can see the connection between adaptationism and teleosemantics. If adaptationism is bad methodology, that is, if most features of current plants and animals were not selected for, then most features of current plants and animals *might not have functions* according to MILLIKAN's definition. I say that they *might not* have functions as opposed to saying

that they *do not* have functions because adaptationism is a methodology for understanding natural selection, but things could have been selected for by agents. An object might be a member of a reproductively established family and have been selected for, thereby having a function, in a way that has nothing to do with evolution by natural selection. The account of sentence meaning described in (1s)–(4s) above is just such an account. Evolution by natural selection is not mentioned or relied upon there at all. So here is the answer to the first part of question (1):

Adaptationism could be entirely incorrect—it could be false that anything has a function derived from natural selection—and teleosemantics would still be possible for sentences and other language devices.

This answer makes sense when we consider that sentences and other language devices are *tools*, artifacts designed by people to serve certain purposes. Just as it is unproblematic to assign functions to my computer, independently of the correctness of adaptationism, (1s)–(4s) above show that it is also unproblematic to assign (teleo)functions to sentences.

It is worth pointing out that MILLIKAN would disagree with this answer to the first question. As we can see from MILLIKAN's claim that the meaning of language is basic, not derived, she would certainly acknowledge the *possibility* that language devices get their meaning differently than mental representations do. But she believes that *in fact* sentences are able to perform their functions because they cause mental representations in the hearer. Thus she would claim that question (1), "How correct does adaptationism need to be for teleosemantics to be possible?", should only get one answer. And, as we shall now see, it is not the same one that I have given in the case of language devices.

Unlike the story told for sentences, the account of the teleosemantics of mental representations offered above (1m–4m) does make ineliminable reference to objects and mechanisms that evolved by natural selection. The representation producer and the representation consumer were both "designed" by natural selection to have the functions that they do: the producer was selected for its ability to produce representations that map features of the environment according to the rules that the consumer uses; the consumer was selected for its ability to use the representation produced by the producer to initiate appropriate behavior with respect to the content it (the representation) maps. So, for teleosemantics for mental representations to be possible, it must be the case that certain portions of the brain were selected

for their abilities to produce and consume representations in the same language. Thus, we have the answer to the second part of question (1): *For teleosemantics of mental representations to be possible, adaptationism must be correct enough that the portions of the brain that produce and consume representations were selected for their abilities to do so, and, so, have producing and consuming representations as their functions.* MILLIKAN realizes that this is true, and since she thinks that there needs to be a unified account of linguistic and mental representation, she claims that she "needs to assume the truth of evolutionary theory in order to show that quite mundane functional items such as screwdrivers and kidneys are indeed items with proper functions." (1993, p16) By separating the teleofunctional account of language from that of mental representations we have shown that this is not necessarily the case.

We now have our answer to both parts of question (1), and can proceed to attempt to answer the second question:

(2) Is adaptationism correct enough to support teleosemantics?

Part of the answer to this question comes immediately. Since the teleosemantic story told above for *sentences* makes no reference to functions derived by natural selection, the fate of adaptationism can have no effect whatsoever. *So, whatever our verdict on adaptationism as a methodology, it is trivially sufficient for the teleosemantics of linguistic representations.* The pressing question, then, is "Is adaptationism sufficiently correct for teleosemantics of mental representations?" For it to be so, it must be the case that the critiques of adaptationism that we will now consider do not make it unreasonable to think that certain parts of the brain have producing and consuming representations as their proper function. These critiques will come in two main types—problems with optimization and evolutionary free riders.

The first line of criticism of adaptationism concerns the assumption made by adaptationist evolutionary biologists that natural selection is guaranteed to *optimize*, that is, to produce a feature of an organism that is ideally suited to perform some function made necessary by the organism's environment. It turns out that this assumption is simply false. KITCHER (1985) points out several ways in which natural selection will fail to yield an optimal solution to the "problem" posed by the environment. First, he describes the course of evolution at a locus in a Bantu population. The combination of alleles available in this population and their relative fitnesses combine so as to lead to the demise of the

fittest combination of alleles. (See KITCHER 1985, pp215–216) Furthermore, there are several other ways in which the most fit combination of alleles just cannot be reached by the population, KITCHER calls this the *trajectory problem* (“Intuitively, the idea is that you can’t get there from here.” 1985, p216) Such trajectory problems can be caused by a variety of factors such as small populations, various vagaries of chance, and interactions among gene loci. When there are trajectory problems, the best “solution” that can be hoped for is a *local* optimum, which is best described by altering KITCHER’s description of the trajectory problem—intuitively, the local optimum is “the best you *can* get to from here”. And it is often the case that once the population settles into this local optimum, the global optimum is no longer an available option. Thus, the adaptationist is wrong in assuming that natural selection is guaranteed to optimize.

What concerns us, remember, given the failure of natural selection to guarantee optimality, is the effect that this might have on the likelihood that portions of the brain might have representation production and consumption as their functions. It seems that the failure of guaranteed optimization has no effect on this. These parts of the brain, supposing that they exist, will still have been reproduced because of their ability to produce and consume representations. And it is still likely that they were selected for their ability to do so, even if they are only locally optimal for so doing. In other words, the fact that there might be, somewhere in the vast space of possible brain configurations, some unavailable configuration that would be even better at producing and consuming representations, does nothing to make it the case that the existing structures do not have functions. Furthermore, even if the existing representation producers and consumers are not the local optimum, this does nothing to change the fact that it is overwhelmingly likely that they were selected for their abilities. Thus, this first line of criticism of adaptationism fails to cause any difficulties for teleosemantics.

We must now consider the other main line of criticism of adaptationism: free riders, cases where selection *of* objects, rather than selection *for*, certain traits has occurred. SOBER (1984) has characterized this distinction in an illuminating way. He asks us to imagine a toy with balls of different colors and sizes (which are correlated in that every ball of a certain size is the same color) inside a cylinder that is divided into several chambers. Each divider between two chambers has holes of a particular size, with the largest holes between the top and second chamber, the

next largest holes between the second third and so on all the way down (see SOBER, 1984, p99, for a picture of this toy). With such a toy, if all the balls are placed in the top chamber and the toy is shaken, the balls will eventually descend to the lowest chamber with holes above it that they can fit through. Suppose, as SOBER does, that only the green balls are small enough to fit through all the holes and reach the bottom chamber.

“When the green balls reach the bottom more frequently than the blue ones, we think that there must have been a reason why. Green balls were selected; so they must have had some property that was selected for. But the property in question was not their color. There was *selection of* green objects, but no *selection for* greenness. I offer the following slogan to summarize this logical point: ‘selection of’ does not imply ‘selection for.’” (SOBER 1984, p100)

What this example shows is that since it is only properties that can be *selected for* and any object has many properties, there are likely to be evolutionary *free riders*, properties of objects of which *selection of* occurred, but without *selection for* the particular property in question. The greenness of the balls that were selected is just such a case—there was selection of the green objects, but this selection was selection for size, not greenness. Evolutionary free riders figure importantly in two separate critiques of adaptationism associated with Stephen Jay GOULD (GOULD/VRBA 1982, GOULD/LEWONTIN 1979). We shall now turn our attention to these.

GOULD/VRBA (1982) introduces a new term to evolutionary biology. *Exaptations*, they say, are characteristics that “evolved for other purposes (or for no function at all), and [are] later ‘coopted’ for their current role.” (1982, p6) So exaptations begin as free riders, and only later become useful for some purpose or other. Adaptationism is incorrect, they say, because it fails to consider that certain traits might be exaptations. To return to SOBER’s toy example, imagine that the greenness of the balls, which, remember, came along as a free rider when the smallness was selected for, suddenly becomes quite a useful trait. Imagine, for example, that the balls are seeds and birds in their environment are, for whatever reason, particularly good at spotting green seeds, which they then ingest and excrete at some distant points, thereby spreading these green seeds, and the plants they grow into, widely. There was *selection for* the smallness of the seeds, and *selection of* the seeds themselves, including such traits as their greenness. Thus, GOULD/VRBA would say, the smallness of a seed is an adaptation, the greenness is an exaptation. Since ad-

aptations have functions and exaptations only have effects, the greenness of the seeds has no function⁸ GOULD/VRBA even suggest that the cognitive and language-using abilities of humans may be underwritten by exaptations in a brain whose features were selected for quite different reasons.⁹ This, then, is a direct assault on teleosemantics of mental representations. For if the features of brains that underly our representation producers and consumers are exaptations, if they exist in current humans as free riders that came along with some other trait that was selected for, then they can have no functions. So, teleosemantics for them is impossible.

The GOULD/VRBA critique, however, does not lead us to this conclusion. This is the case because all current adaptations are (or once were) exaptations—they only *begin* as free riders. A thought experiment from DENNETT 1995 can be used to make this point nicely. Imagine that some American engineers invent a device to recognize U.S. Treasury twenty-five cent pieces in American vending machines. Call such a device a “two-bitser” and imagine that it is (re)produced and placed in most vending machines. Each “two-bitser,” then, is a member of a reproductively established family and was selected *for* (by manufacturers of vending machines) because of its ability to recognize quarters, so its function is to do just that. Now imagine that after years of working in the United States, a vending machine that contains a two-bitser is brought to Panama, where there is a coin called (this is true, DENNETT tells us) a “quarter Balboa” that is the same shape and weight and approximate value as U.S. quarters. The two-bitser, it turns out, is also an excellent detector of quarter-balboas, and is used as a “q-balber” for several years. It is then an *exaptation* for the detection quarter balboas. So, GOULD/VRBA would say, it has as an *effect* the detection of quarter balboas, but its *function* is to detect U.S. quarters. (see DENNETT, 1995, 404–407) But now, going a small step beyond DENNETT’s thought experiment, imagine that a Panamanian manufacturer decides to build lots more of these devices precisely because of their ability to detect quarter balboas. The device now meets the conditions of having acting as a “q-balber” as its function; so, it is now also an *adaptation* for detecting quarter balboas.

What this shows is that adaptations must all have been features of an organism that were already present for some other (or no particular) reason, and were then pressed into ser-

vice in a certain job. In other words, all adaptations must have begun as exaptations. So either there are no exaptations (because once they are reproduced *because* they can perform the effect they were exapted for, they become adaptations, and so are no longer exaptations) or exaptations have functions (because adaptations do and all exaptations are also adaptations). Neither of these two options can lead to difficulties for the teleosemantics of mental representations. This is the case because parts of the brain that act as representation producers and consumers, if such there be, might be exaptations, but exaptations have functions too.

The final critique of adaptationism we will consider, and the first that might lead to some problems for the teleosemantics of mental representations, is another free rider criticism—GOULD/LEWONTIN (1979). In this masterpiece of scientific rhetoric¹⁰, GOULD/LEWONTIN decry the excesses of what they term the “adaptationist program”—a name which has stuck. They claim that adaptationism is a bad scientific methodology because its practitioners tell evolutionary “Just-so Stories”—assuming that a trait is the optimum solution to an environmental problem, and imagine scenarios whereby natural selection “found it”—to explain the presence of particular traits, ignoring the possibility that the traits could be free riders of one sort or another. GOULD/LEWONTIN do not primarily protest the fact that adaptationists *prefer* to tell these stories, rather than examine non-selective factors such as pleiotropy, linkage, genetic drift, allometry and developmental constraints (although they are concerned with this). Instead they are concerned about what they call “Panglossianism”, the assumption that *every* trait must be optimized for the environment, even in the face of disconfirmation of their Just-so Stories.

“We would not object so strenuously to the adaptationist program if its invocation, in any particular case, could lead in principle to its rejection for want of evidence. We might still view it as restrictive and object to its status as an argument of first choice. But if it could be dismissed after failing some explicit test, then alternatives would get their chance. Unfortunately a common procedure among evolutionists

does not allow such a definable rejection for two reasons. First, the rejection of one adaptive story usually leads to its replacement by another, rather than to a suspicion that a different kind of explanation might be required. ... Second,

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the criteria for acceptance of a story are so loose that many pass without proper confirmation. Often, evolutionists use *consistency* with natural selection as the sole criterion and consider their work done when they concoct a plausible story. But plausible stories can always be told." (1979, p79)

The point of the critique is that Panglossian adaptationists assume that there *must* be a selectionist history for every trait, even if the ones thought of thus far have been empirically disconfirmed and no others are in the offing. This critique of adaptationism is absolutely correct; evolutionary biologists, especially the sociobiologists who are the primary target of the argument, often do tend to ignore non-selectionist explanations of features of current plants and animals. This does not mean, however, that all or most or even many adaptationist explanations are incorrect. It also does not mean telling selectionist stories for traits is usually bad methodology or that natural selection is not the most important factor in evolution (see p81). All the GOULD/LEWONTIN critique shows is that we need to be *cautious* adaptationists, always remembering that there are alternatives to selectionist histories.

This, however, is enough to cause concern for the proponent of teleosemantics of mental representations. We can see this if we consider again what the theory requires. For there to be a teleosemantic theory of mental representations, it must be the case that (1) there are portions of the brain that produce and consume representations and (2) it must be their function to do so. (2) requires that there be an adaptive story, whereby the representation producers and consumers have been selected for their ability to trade in representations. And this is where the problems arise. The problems arise here because the teleosemantic story requires a selective history be told for a pair of brain structures *that could very well not exist*. That is, representation producers and representation consumers are theoretical entities; no one is certain that there are such things. Indeed if current explanations offered in connectionist artificial intelligence (RAMSEY 1997), dynamical systems theory (VAN GELDER 1995) and artificial life (WHEELER 1996) are generalizable to full explanations of cognition, cognition in general may be non-representational. So to invent an adaptationist story explaining why representation consumers and producers are they way they are is to take part in the Panglossian excesses that GOULD/LEWONTIN so rightly criticized. Thus, unless there is real confirmation that there are portions of the brain devoted to the production and consumption of representations (if there ever is such), we can-

not have a teleosemantics of mental representations because it would require that we tell a Just-so Story for two merely theoretical traits, which is *incautious* adaptationism, to say the least.

It is now time to take stock. What we have seen is that our first question—how correct does adaptationism need to be for teleosemantics to be possible?—requires a separate answer for public language and private mental representations. In the case of public language, we can have a teleosemantics without adaptationism being correct at all. It could even be the case that *no* part of humans evolved by natural selection, yet teleosemantics for public language would still be possible. For inner mental representations to get a teleosemantics, however, it must be the case that certain parts of the brain evolved by natural selection and so have functions. To claim that this is the case, even when there is no direct evidence that there are these parts, is Panglossian bad methodology of the kind that GOULD/LEWONTIN rightly object to. Thus we cannot conclude that representation consumers and producers have proper functions, which they must have for teleosemantics to be possible. Although adaptationism is correct enough for a teleofunctional account of sentences and the like, it fails to be so for mental representation.

The problem we have seen here is that teleosemantics of mental representations requires that the brain evolved in a particular way; it requires a Just-so Story for objects that *might not exist*. These objects might not exist because it is less than certain that what the brain does is manipulate representations. Although it is difficult to doubt that the brain is the organ that organizes and coordinates the activities that constitute thinking or intelligent behavior in some broad sense, it is certainly possible (even likely, perhaps) that most of what we call thinking is not the manipulation of representations. Perhaps our ability to do business in representations at all is dependent upon the fact that we all learn public languages. Thus we ought to be skeptical of the claim that the brain (or parts of it) is an adaptation for producing and consuming representations; we ought, also, to be skeptical of theories of meaning that depend upon this claim.

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Notes

- 1 In this paper, I will use ‘function’, ‘proper function’, and sometimes even ‘teleofunction’ interchangeably. Anything that I will claim has a function (simpliciter) satisfies MILLIKAN’s definition of proper function, and proper functions are a variety of teleofunctions.
- 2 This however is not quite the case. This definition fails to cover newly invented devices that have not (yet) been reproduced. The same holds for neo-logisms which seem to have stipulated meaning even if never spoken again. This fault could be rectified by adding another clause to the definition.
- 3 This is the case because we will be considering what might be called revisionist critiques in this paper. More revolutionary critiques would argue that the problems with evolutionary biology are deeper than the rampant adaptationism that GOULD and LEWONTIN and VRBA decry. Examples of more revolutionary critiques include SAGAN/MARGULIS 1991, BUSS 1987, and some of the essays collected in ROSE/LAUDER 1996.
- 4 Representations are different from other intentional icons in that they are used in inferences. Henceforth, we will not worry about this difference. (see MILLIKAN 1993, pp106–116)
- 5 One may wonder at this stage why it is a *function* of the hearer to respond to tokens of sentences in given ways. In the case where the hearer is a waiter or waitress, it is clearly one of the hearer’s functions to bring what the speaker requests. In the case of coming to believe the content of S, it is the function of the hearer because (1) *beliefs that p* certainly form a reproductively established family, and (2) believing the content of the sentence, in normal circumstances, is something selected for—the hearer believes the spoken content, rather than something else, because doing so has had a great variety of positive effects in past hearing-events.
- 6 I will refer to adaptationism as a methodology (rather than a theory) and wonder whether or not it is correct (rather than whether it is true) in order to avoid controversies over whether or not adaptationism constitutes a theory. I take it as uncontroversial that adaptationism is *at least* a methodology (a set of tools for dealing with a certain class of problems—see e.g. KITCHER 1985), even if some might dispute that it is a full-fledged theory (whether of a first- or higher-order). Resolving this issue is beyond the scope of the present paper. Thanks to an anonymous reviewer for this journal for raising this issue.
- 7 The alleles and fitnesses are as follows: A, S and C are initially present; most members of the population are AA; AS is fitter than AA; SS is lethal; C is recessive to A; CS is less fit than AA; CC is the most fit pair. In such a population, C is eliminated, and so the most fit pair CC is also eliminated, despite the fact that it was present initially. (see KITCHER 1985, p215)
- 8 The (now classic) example GOULD/VRBA discuss is the flippers of sea turtles. The flippers, they claim, are adaptations for swimming that also serve reasonably well as diggers, an activity for which they have been exapted. See MILLIKAN 1993, pp41–50, for a criticism of this particular case.
- 9 “Most of what the brain now does to enhance our survival lies in the domain of exaptation—and does not allow us to make hypotheses about the selective path of human history.” (1982, p13)
- 10 SELZER 1993 is an entire book devoted to analyzing the *writing style* of this paper.

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The Synergetic View of Human Creativity

Synergetics as a promising research program

Synergetics, or the theory of self-organization and evolution, which originated mainly in the natural sciences (nonlinear mathematical analysis, nonequilibrium thermodynamics, the theory of deterministic chaos and fractal research), is increasingly demonstrating its usefulness in analyzing cognitive and cultural processes in humans. Synergetics is fruitful in unravelling some mysteries of the human consciousness and human psychology. The philosophical content of the synergetic concepts and ideas is rather rich. It's reasonable to make an attempt to build synergetics into "the body of culture" carrying out a comparative analysis of the synergetic notions and historical and cultural images. The theory of self-organization is a new interdisciplinary (or multidisciplinary) trend of scientific research. It is the theory that mostly determines a character of the modern post-nonclassical stage of development of science. The rapid development of the theory is connected with the names of such scholars, as HAKEN (1978, 1996), PRIGOGINE (PRIGOGINE/STENGERS 1984), KURDYUMOV (1989, 1990), LASZLO (1995), MANDELBROT (1982), MORIN (1992), VARELA (MATURANA/VARELA 1988).

Within the framework of synergetics, some laws of evolution and self-organization of complex systems are in the process of investigation now. The theory might be used as a general methodological

Abstract

The heuristic value of synergetic models of evolving and self-organizing complex systems as well as their application to epistemological problems is shown in this paper. Nonlinear synergetic models turn out to be fruitful in comprehending epistemological problems such as the nature of human creativity, the functioning of human intuition and imagination, the historical development of science and culture. In the light of synergetics creative thinking can be viewed as a self-organization and self-completion of images and thoughts, filling up gaps in the nets of knowledge. Insight, fast and sudden solutions of scientific problems, instabilities when "an idea is in the air" are considered as examples of blow-up regimes in the cognitive field.

Key words

Attractors, blow-up regime, complexity, creativity, discovery, evolutionary epistemology, mind, nonlinearity, self-organization, synergetics, topology.

tool, since it is oriented towards the search for common patterns of evolution of complex systems of any kind, regardless of concrete nature of their elements or subsystems. The synergetic models can be applied to understand human cognitive and cultural activities as well as the management of complex sociocultural systems (KNYAZEVA 1998).

Synergetics can be considered as one of the modern, most promising research programs. The development of synergetics entails deep changes in the conceptual net through which we comprehend the world. A new synergetic worldview is in

the process of formation. It means a shift of paradigm, a radical conceptual transition from being to becoming, from stability to sustainability, from images of order to chaos generating new ordered evolving structures, from self-maintaining systems to fast evolution through a nonlinear positive feedback, from evolution to coevolution, reciprocal evolution of different complex systems. The new synergetic style of thinking is evolutionary nonlinear and holistic. This is a modern stage of development within the traditions of cybernetics and the general theory of systems. However, many elements of the latter have undergone important changes since their appearance.

Synergetics is a multi-faceted phenomenon in modern science. The epistemological dimension of synergetics constitutes the synergetics of cognition, the application of evolutionary nonlinear, synergetic models to the comprehension of the human

cognitive and creative processes. The investigations fall into the frames of Evolutionary Epistemology (EE) (RIEDL/DELPOS 1996; OESER 1988, 1996; VOLLMER 1984, 1994; WUKETTIS 1991).

Two different research programs have been distinguished in EE (OESER 1996, p16–17; WUKETTIS 1991). The first program includes the investigation of the human cognitive capacities as products of biological evolution. The second program has an aim to explain the development of science in structural analogy to biological evolution. The application of synergetics to epistemology is relevant to the second research program “evolutionary theory of science”, but our current attention in the light of synergetics is focused mostly on the individual level of the scientific knowledge development.

Models which are derived from evolutionary biology are still being widely used in evolutionary epistemology. Synergetics elaborates more profound and fundamental evolutionary models which are based on interdisciplinary knowledge. The application of such models might open up new perspectives of research in the field of evolutionary epistemology.

Several metaphorical notions or thought-images, could be verbalized within a synergetic approach in relation to human beings. These are as follows:

- patterns of self-organization and geometries of human behavior;
- fractal pictures of historical events;
- individual (or sociocultural) mental landscapes, in which yesterday–today–tomorrow are simultaneously and totally available;
- situations “here and now” as those places where an unknown past meets an emergent future;
- cognitive maps of human mind and human action;
- distribution and topological configuration of already occupied social niches in the collective social activities;
- pictures of “thickening of innovations” in culture (the splash of talented people) and “rarefying of innovations”.

It seems that the images originating from synergetics could become points of knowledge growth in the humanities.

Basic model and its methodological consequences

The methodological consequences presented here are based on the analysis of the results of mathematical modeling and computational experiments with the evolutionary processes in open nonlinear media

(or systems), conducted by the Moscow Synergetic School at the Keldysh Institute of Applied Mathematics (Russian Academy of Sciences) headed by Sergei P. KURDYUMOV and at the Institute of Mathematical Modeling (RAS) led by Alexander A. SAMARSKII. A number of these results have been obtained and proven in form of mathematical theorems. Therefore, the new synergetic notions and their applications to epistemology have a solid foundation in the mathematical developments.

The synergetic models of burning and heat conduction (diffusion) processes are the most widely used models claiming to be capable of explaining many paradoxical features of self-organization. The process of self-organization is mainly connected with the appearance of localized (despite heat conduction) sources of burning (chemical reactions): dissipative structures arise on an active (burning) medium. Self-organizing structures of burning serve as one of paradigmatic examples of synergetics.

The scholars of the Keldysh Institute of Applied Mathematics and of the Institute of Mathematical Modeling (both of the Russian Academy of Sciences) managed to discover mechanisms of localization, structure-formation in open nonlinear media (systems) and their evolution (reconstruction, integration and disintegration) (see ACHROMEVA et al. 1989; KURDYUMOV 1990; SAMARSKII et al. 1995 for more details). The internal mechanism of the structures generation is an interplay between two opposite factors in a medium: a nonlinear source and a dissipative factor. On one hand, the work of a nonlinear source leads to creation of inhomogeneities in a continuous medium. The nonlinear source can be of a different kind: source of energy, information or infection. It can be an active medium in the nuclear reactor which generates an avalanche flow of neutrons, or it can be a source of knowledge (for instance, an influential scientific school which creates new knowledge spreading over the scientific community) or a center of infectious diseases. Nonlinearity means the following property of the source: the more is a deviance from equilibrium, the faster the process goes. You may imagine how a skier is going downhill and the hill is becoming more and more steep. Moreover, it may be a distributed nonlinear source which acts in every local area of an open medium (complex system) and produces self-stimulating growth all over the space of the medium (system).

On the other hand, there is the factor which dissipates, scatters about inhomogeneities in the medium. It can be of various nature as well: diffusion, dispersion, hydrodynamics, etc. It can be diffusion

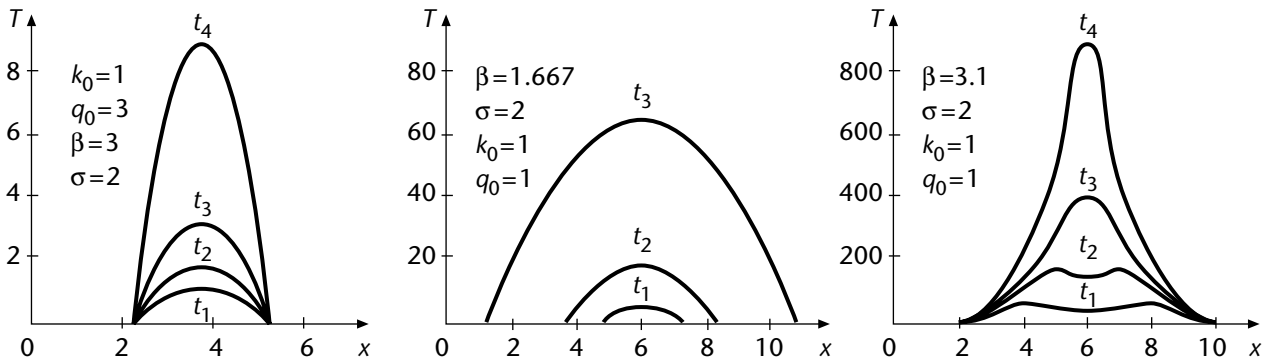


Figure 1: S-Regime (left), HS-regime (middle), and LS-regime (right).

of neutrons, or diffusion (dissemination) of knowledge, or diffusion (spreading) of infectious diseases.

The basic model used is as follows:

$$\frac{\partial T}{\partial t} = \frac{\partial}{\partial x} \left(k_0 T^\sigma \frac{\partial T}{\partial x} \right) + q_0 T^\beta$$

where $\sigma > 0, \beta > 1, \tau > 0, -\infty < x < +\infty$.

σ is coefficient of nonlinearity of diffusion in the equation. It is indicator of dissipative processes of different kinds. β is coefficient of nonlinearity of a source. It shows a strength of factor which creates inhomogeneities in a corresponding continuous medium.

Depending on a result of competition of these two complementary factors, three various regimes of the processes development can be established in a corresponding open nonlinear medium (system; see figure 1).

If $\beta = \sigma + 1$, i.e. a work of nonlinear source is in conformity with dissipative processes in a medium, the process will develop in S-regime.

This is a mode of the process development with peaking when the process becomes localized and develops with peaking inside a certain area, the fundamental length L_s . It is a kind of “standing wave” of burning.

If β is less than $\sigma + 1$, i.e. intensity of dissipative, scattering processes is more than the strength of nonlinear source, HS-regime of evolution will be established. This is a type of the process development in an open nonlinear medium when there is no localization (localized structures), all heterogeneities are washed away. The evolutionary regime is a “wave of burning” which infinitely spreads over the space. It is a “diverging and growing wave of burning”.

If β is more than $\sigma + 1$, i.e. the factor creating heterogeneities in a medium (work of nonlinear source) is considerably stronger than the dissipative,

scattering factor, evolution occurs in LS-regime. This is a certain type of the process development with peaking in an open nonlinear medium when the increasingly intensive development of a process occurs in a more and more narrow area near a maximum. In this case we observe a “converging and growing wave of burning”: the process develops very fast, while its effective area of localization diminishes.

The main feature of the LS-regime is that it develops slower than the S-regime. The feature is reflected in its name: “L” means “lower” than the S-mode. On the contrary, “H” in the name of the HS-regime means “higher” than the S-mode.

The third case is the most interesting. The above mentioned nonlinear equation has here a set of qualitatively different solutions, i.e. eigenfunctions of different complexity. Their number is described by a simple formula:

$$N = \frac{\beta - \sigma}{\beta - \sigma - 1}$$

The set is defined only by inner properties of a corresponding medium and can be considered as a mathematical representation of a spectrum of possible evolutionary paths (structure-attractors) of the medium. If we consider our paradigmatic example of self-organizing structures of burning, we have a spectrum of different forms of localized burning, so to say “crystals of burning”. The phenomenon has been investigated by the Moscow synergetic school and is termed an “inertia of heat” (“inertia of burning”) (ACHROMEEVA et al. 1989).

A number of general and important consequences can be derived from the model (KNYAZEVA/KURDYUMOV 1994). This quite simple mathematical model is very profound. It reflects and describes some general features in the behavior of complex systems: an interplay of two complementary factors

in the systems and ways of localization of processes, structures construction in them.

First. Synergetics discovers laws and conditions for very fast, avalanche-like processes, blow-up regimes. The above described S-, HS- and LS-modes are main kinds of blow-up regimes. These are processes when some characteristic parameters (temperature, energy, information etc.) infinitely grow during a finite period of time. $Q(t) \rightarrow \infty$, when $t \rightarrow t_f$. t_f is the blow-up time, time of peaking. Of course, the infinite growth is impossible in real systems of the world. But the rapid growth of the characteristic values in several orders, sometimes even in several times, allows to observe a number of astonishing effects which have been predicted by the theory of blow-up regimes.

Blow-up regimes are under investigation now in more than sixty different fields of research, starting from plasma physics (laser thermonuclear synthesis) and meteorology (catastrophic phenomena in atmosphere), ecology (rapid growth and sudden dying out of biological population), neurophysiology (the modeling of signal propagation along neural nets) and epidemiology (spreading of infections). There are similar blow-up regimes in human psychology, social and cultural development. The growth of information and the population on the Earth proceed according to hyperbolic rather than exponential law, i.e. the processes occur in blow-up regimes.

It is important to understand how we can initiate such processes in open nonlinear systems, for instance on the field of human brain and consciousness, and what are requirements for avoiding the probabilistic decay of developed complicated structures near the moment of their maximal growth (moment of peaking).

Second. Synergetics reveals the creative role of chaos in the evolutionary processes of nonlinear complex systems. There must be a certain degree of chaos and destruction in the world. Small fluctuations, i.e. chaos on the micro-level of a complex system play an essential role in determining actual trends ("aims") of processes which occur on a macro-level of the system. Chaos is a mechanism underlying the choice of one evolutionary stable structure-attractors. The macro-organization emerges from a disorder, from chaos on micro-level.

Dissipative processes, being a macroscopic manifestation of micro-chaos, constitute a necessary complementary factor in our model of structure-formation in open nonlinear media. Dissipation acts in

the same way as a sculptor, who chisels and shapes a statue from a block of marble.

Order and chaos, organization and disorganization, constructive force (a nonlinear source) and dissipation seem to be well-balanced in the world.

Thus, it is senseless to resist chaos, or to strive for complete eliminating undesirable, chaotic elements from the world. They are necessary conditions for self-organization.

Besides, chaos serves as a basis for integration of relatively simple evolutionary structures into more complex ones. It is a mechanism for coordinating their tempos of evolution. Chaos, fluctuations on micro-level, can also be a way of evolutionary switching, making a transition from one evolutionary regime to another one possible.

Third. The evolution of complex structures undergoes an alternation of various regimes of process development. There can not be sharp growth of a structure without a threat of its fall and destruction. There are some universal laws which govern these rhythms. They are peculiar to living beings as well as to complex structures in inanimate nature. There are cyclical changes of state: upsurge – slump – stagnation – upsurge – slump, etc. Only obeying these "life rhythms", or oscillatory modes, complex systems can maintain their integrity and develop dynamically.

The synergetic models used here allow to describe such mechanism of self-maintenance peculiar to complex structures. In addition to the HS-mode with peaking, there is a HS-regime of "cooling" with decreasing intensity of the processes and infinitely scattering wave. For open media (systems) with strong nonlinearity, periodic change of two opposite regimes, the LS-mode with peaking and the HS-mode of "cooling" takes place with high probability.

Thus, under certain conditions a mode of localization and structure-formation, i.e. the LS-regime, is established in an open nonlinear medium. The mode keeps chaos in a definite form. But developed localized structures turn out to be unstable with regard to chaotic disturbances, fluctuations on a micro-level. The fluctuations destroy an available synchronization of speeds in different fragments of a complex structure, and the structure comes to be in danger of a decay. There is a possibility to avoid the process of decay, if a change-over to an opposite evolutionary regime occurs in good time. This is the HS-regime of "cooling" and diverging wave, a renewal of some old tracks. The mechanism of auto-oscillations between these two complimentary regimes (HS- ↔ LS-) resembles the oriental image of Yin–Yang.

Fourth. One of the most astonishing consequences of synergetics is the notion of discrete spectrum of evolutionary paths. If we choose an arbitrary path of evolution, we have to be aware that this particular option may not be feasible in a given complex system. Only a definite set of evolutionary pathways is “allowed”; only certain kinds evolutionary structure–attractors can emerge. This is a kind of evolutionary prohibition rules (KNYAZEVA/HAKEN 1997).

According to our models, localized meta-stable structures arise in the LS-regime with peaking. The estimated number of possible structures can be quite large, but it is limited. The spectrum of possible structure–attractors is by no means a continuous one. The spectrum corresponds to a set of eigenfunctions of nonlinear equation describing the evolutionary processes in a certain complex system.

Here the question of pre-determination of evolutionary processes arises. Although the future states of complex systems actually escape our control and prediction, and the future is open, not unequivocal, there are definite spectra of “purposes” of development, i.e. of evolutionary structure–attractors, in open nonlinear systems. There is, so to speak, a “tacit knowledge” on the part of complex system themselves, because these spectra are determined exclusively by inner properties of the systems.

Thus, the future is open in form of spectra of pre-determined possibilities.

The evolutionary structure–attractors as possible forms of future organization determine the course of historical events. The future is in some sense available in the present. Patterns precede processes. They can be interpreted as a “memory of the future”. All the attempts that go beyond one of possible basins of attraction are futile attempts. Everything which is not in accordance with the structure–attractors will be wiped out, annihilated. For example, a human can fight unconsciously against those forces (some latent attitudes and plans as structure–attractors) that “pull him” from the future, but all these attempts are doomed to failure.

Fifth. Further consequences of the synergetic models under consideration include some new principles which govern the emergence of complex evolutionary totalities from simpler elements. A complex structure is an integration of structures of “different ages”, that is: structures at different evolutionary stages. The principles which govern the integration of such structures of “different ages” are gradually being revealed. The integration of relatively simple structures into a complex one occurs by the estab-

lishment of a common tempo of development in all unified parts (fragments, simple structures). Structures of ‘different ages’ start to co-exist in one and the same “tempo-world”. The term “tempo-world” proposed here signifies “a world having a certain rate (tempo) of development”. The rate of development is the most important characteristic in the process of assembling of a complex evolutionary whole.

Sixth. Due to synergetics we acquire knowledge how it is possible to multiply reduce time and efforts in order to generate, by a resonant influence, the desirable and, what is no less important, feasible structures in a given complex system. It proves that managing influences must not be energetic, but rightly topologically organized. Weak, but proper, so called resonant, influences upon complex systems are of great efficiency.

This feature of complex organizations had been guessed thousands years ago by the father of Taoism, LAO-TSU. It was expressed as the weak defeats the strong, the soft defeats the hard, the low defeats the loud. Considered from the modern point of view, complex systems turn to possess a property of selective topological sensitivity. They demonstrate unexpected strong replies to excitations which are relevant to their inner structural organizations, i.e. to resonant excitations.

The creative mind from the synergetic point of view

The task of applying synergetic models for a better understanding of cognitive evolution and the growth of scientific knowledge is part of the problems raised by evolutionary epistemology. There are only few publications devoted to the subject so far. E.g. the works of HAKEN (1996), HAKEN/STADLER (1990), KRIZ (1997), VOLLMER (1984).

To consider the creative mind from the synergetic point of view is a challenging, but difficult task. As HAKEN puts it, “Creativity appears to be the deepest of all puzzles concerning the brain. It means the birth of thoughts that have never been generated before and, in particular, whose generation was extremely unlikely. One may compare the creation of a new idea to a jigsaw puzzle” (HAKEN 1996, p304–305).

The application of synergetics to the human cognitive processes is currently under discussion. Synergetics can be applied, because it is oriented to the revelation of the universal patterns of evolution and self-organization of complex systems of any kind.

Synergetics tries to construct certain bridges between inanimate and animate nature, between the quasi-purposes of natural systems and human rationality, between the birth of something new in nature, the so called “creativity of nature”, and the creative and imaginative capabilities of a human being.

There is a complex mutual connection of conscious and unconscious processes, purposeful and spontaneous ones, the processes of organization and self-organization in human mind. Cognitive activities can contain synergetic mechanisms, which occur regardless of the scientists’ intentions and free creative aspirations. The mechanisms concern these processes which are realized, so to say, “above the creating minds”.

In relation to the development of scientific knowledge and creative processes on the individual level, it is reasonable to search the synergetic mechanisms in those processes which are not under the control of consciousness, which occur on both subconscious and unconscious levels. If we consider the growth of knowledge on the level of collective activity of the scientific community, the mechanisms of self-organization are connected with some unpredictable consequences of the scientists creative work as well as with the integration of the actions of individual scientists into general trends of scientific research. To underline the relevance of the synergetic approach to the evolution of scientific knowledge three arguments should be noted. These are as follows:

- first, the role of the cooperative, coherent effects in science (for instance, an appearance of a new scientific paradigm is connected with the establishment of some coherent, synergetic patterns of behavior),
- second, the fruitfulness of the approach developed by post-structuralists (LYOTARD, DELEUZE, DERRIDA, KRISTEVA),
- third, the long approbation and the constructive value of information theory. Formation of structures of scientific knowledge can be described in terms of the theory.

In the light of synergetics, some peculiarities of cognitive and creative activities can be reinterpreted and seen in an unusual way. The alternative ways and the scenarios of creative thinking, the latent attitudes and pre-determinations, self-completion of the whole images, deliverance from gaps in available nets of knowledge are the most important of these peculiarities. In this connection, it’s worth to compare the functioning of human intuition with meditation in yoga and Buddhism. Here a number of new

heuristic images could be proposed and investigated, namely:

- how to erase the old traces of memory and to make room for something new;
- consciousness as a treasure-house carrying in itself traces of the past activities and complete images of the future;
- an hierarchy of subconsciousness – consciousness – super-consciousness as a connection of different tempo-worlds (these are worlds developing with different rates);
- the rhythms of the creative activities which are similar to the Yin–Yang rhythms of the universe;
- a peculiar state of a meditating man, i.e. human being as a device extremely sensitive to all happening in the universe, a kind of resonance of a meditating mind with universe;
- a danger of splitting of the human consciousness due to the construction of a super-complex structure on a field of consciousness–unconsciousness during the meditation experiments;
- ways of starting the “adventures of consciousness”, facilitating the intuitive activity by the topologically rightly organized influences upon the human brain and mind.

The functioning of creative intuition and imagination in general can be considered in the light of synergetics as a process of self-organization and of self-completion of visual and mental images, ideas, notions, and thoughts. The term of “self-organization” means here a *spontaneous* (accidental, unpredictable) *growth* of structures of new knowledge as well as their *own growth* which is determined by inherent laws. The self-completion of visual images and ideas is the filling up gaps in the nets of knowledge and the self-construction of a whole from parts.

Cognitive chaos, in the sense of an openness to multiple alternatives, plays a positive, stimulating role in creative thinking. At the initial stage of creative intuition, a maximal widening of the creative field takes place. The maximally possible variety of elements of knowledge seems to be embraced. Besides, the proper balancing of the main and the subordinate, the essential and the non-essential, that is the radical revaluation of all cognitive values in front of the creative aims (they may be conceivable in a more or less degree) is the base for productive choice of an idea. The initial wandering over a net of mental steps serves as a good preparation to the innovative leap of thoughts.

The notion of evolutionary attractors which determine the trends of development of available knowledge is of great importance for the explana-

tion of the mechanisms for creative thinking. If a system falls into the cone of certain attractor, it will inevitably evolve towards this relatively stable state.

The central thesis can be formulated in the form of a paradox: new knowledge is emergent, it is not derivable from the elements of the available conscious knowledge, and at the same time the knowledge is latently pre-determined in the present elements. The translation of knowledge from the potential to the actual is non-trivial and means the event of discovery. The appearance of a specific creative state—an inspiration—means, from the synergetic point of view, hitting the field of one of the creative attractors. The notions in their essential features concur with the notion of attitude in Gestalt-psychology, namely: anticipation, determining tendency, latent attitude, organizing principle, gradient of purpose, etc.

The mechanism of self-organization and self-completion of the visual and mental images includes, first, the purposefulness to an emerging whole. Of course, there is no certain image of the whole, but only a direction to the whole. The attitude (a plan, a main idea or a conjecture) serves as a “guiding thread” of the search. This is an attractor for creative activities.

Second, the selection, the cutting off “all that is unnecessary” takes place on the basis of the initial increase of variety, the revaluation of cognitive values. The latent attitudes taking a selective role. The mechanism of creative thinking is not an accidental sorting of variants.

It is a choice of the main element in order to organize a whole structure. The self-organization occurs around a key element. The intellectual creative work as well as creative writing are connected with the pitiless exclusion of many ideas and images which were admitted a little bit earlier during the stage of cognitive chaos.

Third, the mechanism of self-organization in creative thinking can be presented as a process of filling up gaps in the nets of knowledge, a self-completion of ideas and a self-assembly of a complete image. Not simply the instantaneous organization of a whole structure takes place, as it was assumed by Gestalt-psychologists. According to the synergetic models, creative thinking is the growth of a whole from its parts as a result of spontaneous and intrinsic complication of the parts. The flow of thoughts and images becomes complicated because this is a way of realization of its inner potentials. The flow builds itself spontaneously. Fourth, the scientific discovery might be interpreted as a reorganization within a

field of questions. It might be seen as a crystallization of knowledge, a transition to a new structure. A successful creative work in science often leads to whole series of new knowledge crystallizations. This process to a great extent is irreversible. Discoveries influence their creator, they transform his personality. A multitude of talent crystallizations may occur in the individual life of a truly creative scholar.

Blow-up in the cognitive field

Insight. Fast and sudden solution of a problem.

Insight, i.e. very rapid and sudden finding of ways how to solve a problem, is one of the most astonishing events in human creative activities.

Insight is still hardly explainable by pure logical means. A huge concentration of cognitive activities serves as a prerequisite, a necessary, but not sufficient, pre-condition for unexpected flashes of insight. A similar concentration, a kind of tightening into a point, constitutes a due stage of meditation in practice of yoga as well as of CHAN and ZEN Buddhism.

Here one could try to apply the synergetic model of the LS-regime with peaking. A localized meta-stable dissipative structure converges to a center in the regime. In addition, the structure develops tremendously fast when it reaches a blow-up point. Near the blow-up moment, changes occur only in a narrow area around a center of the structure. The main fragments of the structure are already stopped, “frozen” processes which, in a sense, have fallen into the past. A final architecture of the structure includes these “frozen pieces” of the past as well as the squeezed, swiftly completing (near the blow-up point) future. Insight really means an innovative crystallization of knowledge as well as a breakthrough into the future.

A culminating moment of insight—an enlightenment, or an Aha-experience—looks as the most mysterious one. In our mathematical model, there is an infinite growth of function near the blow-up point.

There is a special condition for establishing the LS-regime in an open nonlinear media: a nonlinear source should act more intensive than a dissipative, scattering factor. In order to stimulate the work of intuition and to create conditions for insight flashes, a human has to concentrate his own energy. He has to organize such a blow-up regime, when the inner sources of energy are stronger than factors of distraction, scattering and dissipative flows of an ordinary way of life.

As a result of intensive mental activity a highly complicated structure of the LS-regimes, probably with a number of different maxima of intensity, may develop in a field of consciousness–subconsciousness. The structure becomes unstable near the blow-up point and is exposed to danger of decay.

A breach of synchronization between different fragments of the complicated structure (different maxima of intensity of the processes) may lead to the rupture of interactions between them. The extremely complicated structure developed in the hierarchical field of consciousness may disintegrate into a number of separate and isolated consciousnesses.

In this connection it's worth to remember repeated warnings in the Eastern studies that one should be occupied with meditation only under the supervision and observation of a teacher, Guru, and that it is dangerous to follow this path of meditation completely independent, especially for the first time. What was the sense of these warnings? Was it, perhaps, a conjecture about the danger of disintegration of the complex structure in the field of consciousness? Was it the threat of its splitting? Some scientists who investigate human creative abilities actually argue that genius people often have various mental diseases, including schizophrenia (literally means from Greek "to split the mind"). They try to find schizophrenic deviations in EINSTEIN's activity and draw up whole lists of geniuses who supposedly suffered from mental disorders (DESCARTES, PASCAL, NEWTON, KANT, SCHOPENHAUER, NIETZSCHE, etc.).

According to the modern theory of self-organized criticality which is a special point of interest at the Santa Fe Institute (New Mexico, USA), any complex organization is wise, but very fragile. It is poised on the "edge of chaos" in such a way that the best next step towards its further complication and even improvement may lead to a destructive avalanche (KAUFFMAN 1995, p29). In this sense, a genius who operates with complicated structures of knowledge poises between the wisdom and the madness. The complicated cognitive structures evolve to a dangerous edge of chaos.

Human creativity is subjected to certain rhythms. The break-through to a new and the fast growth of knowledge, the explosion of creative activities usually follow the periods of delay and stopping, the slowing down of processes. According to the synergetic models, the HS-regime of "cooling" and "scattering wave" precedes the LS-regime of localization, the rapid growth and the formation of structures. The experience of thousand years in the study in

creativity shows the necessity of the stage of relaxation against the back-cloth of an intensive mental work and the turning to another forms of activities or to the regime of sleep.

Therefore, it is senseless to hasten the events. Until the stage of spreading over the old tracks in the HS-regime, the revival of the processes in subconsciousness and the maturing of hypotheses and ideas have not been passed through, there can be no stage of the LS-regime when the fast formulation and verbalization of something already matured in subconsciousness takes place. Until there was no dipping into a slowed down world of subconsciousness, there will be no active work of consciousness.

Instabilities. The idea is in the air. The situations when "an idea is in the air" happen rather often in science. These situations lead to simultaneous, or parallel, and independent discoveries. The examples of such discoveries are numerous in the history of science. It's well worth mentioning the dispute between NEWTON and LEIBNIZ about the priority of development of mathematical analysis, the simultaneous construction of non-EUCLIDEAN geometry by Russian mathematician LOBACHEVSKY and Hungarian scholar BOLYAI, the parallel developments in special theory of relativity obtained by EINSTEIN, LORENZ and POINCARÉ.

These situations look like it is not people who are looking for ideas, but the ideas themselves are looking for people. The very scientific medium prepares and 'pushes forward' its heroes, because corresponding discoveries are completely matured in its depths.

Such kind of situations were described by DUHEM: "The idea is in the air, carried from one country to another by a gust of wind, and is ready to fertilize any genius who is disposed to welcome it and develop it, as with pollen giving birth to a fruit wherever it meets a ripe calyx. In the course of his studies, the historian of the sciences constantly has opportunities to observe this simultaneous emergence of the same doctrine in countries far from one another, but no matter how frequently this phenomenon occurs, he can never contemplate it without astonishment" (DUHEM 1991, p255). He gives an example: "The system of universal gravity germinate in the minds of Hooke, Wren, and Halley at the same time that it was being organized in the mind of NEWTON" (DUHEM 1991, *ibid.*).

Is there a synergetic analogy of such situations generating discoveries? According to synergetic comprehension of evolutionary processes, "the idea is in the air", "spirit of the times" ("Zeitgeist") and

the other similar clichés are ways of describing a specific state of the scientific medium, or a peculiar mental disposition in the corresponding scientific community. This is the state of instability.

Instability means a high sensitivity of a scientific medium towards small changes and advances in solutions of scientific problems. Synergetics shows that a medium in such a state of instability can (due to a nonlinear positive feedback) repeatedly multiply small fluctuations and perturbations and develop them into some new macroscopic ordered states. In the states of instability, there is a mutual connection of different levels of reality. Depending on the instability of the scientific medium, a certain connection between the level of individual creative activity and scientific discoveries on the one hand, and the level of activity of scientific community and scientific innovations on the other hand, can be established. In such states, small shifts on the individual level may lead to appearance of a new collective cognitive pattern. The very medium in which scientists work in such a state of instability spawns scientific innovations. It is possible to supposedly find even a mathematical equivalent to these situations in the history of science. If β is more than $\sigma + 3$ (the work of nonlinear source in a open medium is essentially stronger than the action of dissipative factor), i.e. there is an idea with very good prospects, a corresponding nonlinear equation has a non-localized solution of falling amplitude. This mathematical solution turns to be unstable to small deviations. A small group of scholars or even a single scientist may drastically change the situation. The level of understanding of a certain scientific problem begins to increase very rapidly. A new trend in science may appear. At the initial stage of the LS-regime with peaking, even some decrease of intensity and some scattering of processes may be observed. But when this scattering wave fills up a whole "effective area of localization", as it should be at the developed stage, the process starts its fast growth.

The situations generating multiple and simultaneous discoveries are sometimes described in a similar way. American psychologists SIMONTON draws the following picture: "Discoveries and inventions become virtually inevitable (1) as prerequisite kinds of knowledge accumulate in man's store; (2) as the attention of sufficient number of investigators is focused on a problem" (SIMONTON 1988, p136).

Boom. Explosion of investigations in a scientific field. An explosion of investigations, or so-called boom of activities, can sometimes be observed in specific fields of science. Synergetics could propose an interpretation of such a phenomenon. There are some reasons to apply the synergetic model of avalanche-like growth, development in LS-regime with peaking. The characteristics of the process seems to be as follows:

- there is very fast increment of knowledge, or rapid rise in the understanding of scientific problems and methods of their solution;
- in spite of an observable increase in the number of scientists and scientific schools involved into investigation of corresponding problems, a certain localization of the process takes place. It means that only few scientific schools or eminent single scholars determine a real level of current research in the field;
- if there is a complicated localized structure with several maxima of intensity of the process (a competition of several scientific schools or scholars in a scientific field subjected to rapid growth), some further development of the process leads to a gradual rapprochement of different leading centers (maxima) within this complex configuration of the engaged scientists. According to our model, we have "many-headed" structure of burning with maxima of intensity drawing closer to one another. A proper interpretation of the feature of avalanche-like processes in application to epistemology could be a subject of further research.

Synergetics as a positive heuristics: how far can we go?

Trying to explain some purely human phenomena, such as human creativity, synergetics challenges the humanities. It leads to a profound question. How could the synergetic intentions to reveal some universal patterns of evolution and self-organization be justified? How is synergetics as a new worldview and a new way of transdisciplinary and cross-professional communication in general possible? Because the theory which explains all explains nothing. Wolfgang PAULI once set for himself a rule: "If theoretician says 'universal' it just means pure nonsense". It can be argued however that synergetics is something more than a kind of intellectual yoga, a kind of sophisticated exercises on the mental field.

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The development of synergetics as a positive heuristics has a profound background. Synergetics has rather good elaborated "hard kernel" in form of already described evolutionary mechanisms of the complex systems. Some results have been proven even as mathematical theorems.

Two fundamental discoveries form the basis of the new theory of self-organization and complexity as well. These are the discovery of strange attractors and the discovery of blow-up regimes. Both of the discoveries have profound philosophical impact. They open a possibility to build a bridge between synergetics, which originated mainly in the natural sciences, and the humanities (cognitive sciences, psychology, epistemology, etc.). Due to the interdisciplinary character of the fundamental discoveries, synergetics moves towards a new dialogue between the natural sciences and the humanities.

Thus, I have tried to show some innovative achievements of synergetics in the field of epistemology. The synergetic models of the evolution of scientific knowledge considered here have, so far, mostly a phenomenological character. But they enable us to comprehend some epistemological problems in a new light and also indicate promising avenues for further research. The synergetic aspect of consideration is an evolutionary nonlinear and integrated, holistic one.

Synergetics does not teach us to be wise, it discloses the evolutionary wisdom of nature. Synergetics is a wisdom of a soft management, management through advices and recommendations, through weak, appropriate influences.

As a matter of fact, it is a self-management and self-control.

Synergetics in general opens the possibility to understand how to imitate nature in solving conflicts, in assembling parts into a whole, in developing from one stage to another. Synergetics uncovers evolutionary, historical strata of wisdom in each of us. It shows that temporal transformations of a structure can be distributed in space.

Synergetics prompts that it's possible to reveal and to put into account still latent, hidden structures in a complex system and that there is, as a rule, a lot of such unrevealed possibilities of evolution and that the path into the future is not pre-determined.

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On Natural Selection and Hume's Second Problem

Introduction

Perhaps the stumbling block of scientific rationality is represented by the famous problem of induction, first stated in the XVIIIth century by David HUME. The standard formulation of the problem says that induction is logically untenable as the foundation of science, because it is impossible to justify a universal law by observation or experiment, since it transcends experience. In other words, the problem of induction refers to whether a particular set of observational results is enough to extrapolate such an experience to other circumstances. In this century, Karl POPPER

suggested a solution to the empirical problem of induction. According to POPPER, the acceptance of a scientific law or theory is always tentative, thus suggesting that all laws and theories are conjectures or tentative hypotheses. Every theory is accepted so long as it stands up to the most stringent empirical tests, otherwise it is rejected; *but it is never inferred from the empirical evidence*. Therefore, only the falsity of the theory can be inferred from empirical evidence, and such an inference is purely deductive (POPPER 1983).

It is not the purpose of this paper to discuss whether the widely popular suggestion made by POPPER, has truly solved the empirical riddle of induction. However, GODDARD has cogently argued that HUME's problem of induction implies a further prob-

Abstract

David HUME's famous riddle of induction, implies a second problem related to the question of whether the laws and principles of nature may change in the course of time. Claims have been made that modern developments in physics and astrophysics corroborate the translational invariance of the laws of physics in time. However, the appearance of a new general principle of nature, which might not be derivable from the known laws of physics, or that might actually be a non-physical one (this means completely independent of physical science), supports the notion that the course of nature can change in time. Here it is argued that natural selection satisfies the criteria that identify a general principle of nature which so far, appears to be nonderivable from the known laws of physics and therefore, it is likely that it arose in the course of time, thus leaving open again the quest for a true solution to HUME's second problem.

Key words

Biogenesis, DARWIN, evolution, HUME, natural law, natural principle, natural selection, origin of life.

lem that was left untouched by POPPER's solution (GODDARD 1993). The following lines from HUME are the best introduction to such a second problem: "If there be any suspicion that the course of nature may change, and that the past may be no rule for the future, all experience becomes useless, and can give no rise to inference or conclusion" (HUME 1952) and "We can at least conceive a change in the course of nature; which sufficiently proves, that such a change is not absolutely impossible" (HUME 1992).

However, such words have been construed so as to only mean whether the laws of physics, and the relevant physical con-

stants, will remain stable in the future, a condition which appears to be compulsory in order to extrapolate our experiences into the future. For example, GODDARD argues (quite convincingly in my opinion), that our current knowledge of physics supports the translational invariance of the laws of physics in time, and modern large-scale observations support the temporal stability of the laws of physics over a large sector of space-time. Using OCCAM's razor as a cornerstone for his argument, GODDARD affirms that in the absence of any falsifying evidence the simplest hypothesis prevails and nature is reasonably presumed to be stable in time. Thus a metaphysical proposition (the invariance of the laws of physics in time), has been transformed by the advances of modern science into a scientific conjecture that has with-

stood an amazing number of tests, and HUME's second problem has been practically, if not formally, solved.

The supposed invariance of the laws of nature in time derives from the assumed stability and uniformity of nature. Indeed, the existence of pattern or symmetry in nature, has suggested the existence of laws or rules of change. Classical laws of change correspond to the invariance of some quantity or pattern. These equivalencies were established only long after the formulation of the laws of motion which govern allowed changes. The PLATONIC outlook suggests that timeless, unchanging attributes exist. These attributes have emerged within the realm of science in the form of laws of nature or the invariances and conserved quantities of modern physics (like energy and momentum). Currently, modern physicists consider symmetry as the primary guide into the structure of the elementary particle world, and the corresponding theories and laws of change are derived from the requirement that particular symmetries (often quite abstract in nature) be preserved. These so-called gauge theories require the existence of the forces they describe in order to preserve the invariances upon which they are based. They also dictate the character of the elementary particles that they govern, because the subatomic world is populated by sets of identical particles. The powerful concept of symmetry has enabled whole systems of natural laws to be derived from the requirement that a certain abstract pattern be invariant in the universe. This has led to the generation of successful theories to explain each of the separate physical interactions observed in nature. Currently, the search for a unification of those theories into more comprehensive ones, based on larger symmetries, is in the forefront of research in physics. The unification of the electromagnetic and weak interactions into a single theoretical framework is one of the outstanding achievements of this search for a *theory of everything* (e.g., BARROW 1991)

However, I want to contend that the quoted paragraphs from HUME might also refer to the question of whether the nature of reality can change so that new natural laws and principles may emerge, and this means not only new laws of physics *but any sort of natural law or principle*. Therefore, we can ask the question: are there any natural laws or principles which are not known to be derivable from the current laws of physics, or which might actually be non-physical (in the sense that they belong to a level of explanation that is completely independent from

physical science), but that might have emerged in the course of time? If such is the case, then HUME's second riddle remains as valid as ever, and awaiting for a further solution. The following discussion is directed to suggest that there is such a law or principle of nature, namely, *natural selection*, which was apparently not inherent to the fabric of the universe but arose in the course of time. In order to develop this argument, it is necessary first to discuss some generalities about the major trends in biogenesis, and then to use a particular theory on the origin of life as a concrete example to illustrate that natural selection emerged at an specific epoch in time. I want to emphasise that my choice of such a theory does not mean I regard it as the true explanation for the origin of life. However, the chosen theory clearly illustrates a fundamental assumption common to all rational schemes for explaining the origin of life: prebiotic and early biotic evolution are characterised by a series of evolutionary thresholds which must be successfully crossed in order for evolution to proceed.

Generalities About Biogenesis

Biogenesis refers to the study of the origin of life. This field of enquiry can be traced back to the nineteenth century, when the success of mechanistic research programmes in physiology and physiological chemistry made biologists increasingly reluctant to accept any impenetrable barriers between the living and non-living domains. On the other hand, cosmological theory (via thermodynamics) moved away from the notion of a more or less unchangeable and eternal universe. Once the eternity of the universe began to be questioned, the idea of the eternity of life lost its basis, and the origin of living things required explanation (KAMMINGA 1991). For example, HAECKEL's views on the origin of life were part of a monistic philosophy which asserted the unity of nature, and according to which all natural processes stand in a material, causal and historical connection. There could be no fundamental distinction between the living and non-living; and in order to explain how biological evolution had begun on a lifeless Earth, it was necessary to postulate that life was the historical product of inorganic nature (HAECKEL 1929).

In the present century, OPARIN was the first to suggest a theory of the origin of life which presented a full evolutionary narrative and explicitly showed each step to be consistent with empirical and theo-

retical knowledge accepted at the time (OPARIN 1957). The main impact of OPARIN's theory was to pose, for the first time, the fundamental questions that should be addressed by any theory of the origin of life. Such questions would concern the properties that the first organisms should have in order to be recognised as being alive; the order in which different biological functions evolved during the early history of life; the environmental conditions at each stage of life's development; the location of sites at which such conditions prevailed; and the mechanisms that brought about the origin and development of systems displaying the required properties under the conditions specified. Thus, the constraints on any concrete theory of the origin of life are quite severe: it must be consistent with a wide range of background theories, pertaining to several fields of science, as well as with any empirical evidence bearing on the theory. Nevertheless, an overall coherence of the theory in question is not a guarantee of its truth content.

More than a hundred theories of the origin of life have been published (GUTIÉRREZ-LOMBARDO/LUGOWSKI 1991), such theories can be divided in two major groups, namely, *cells-first* and *genes-first*. The *cells-first* theories represent the oldest group, and argue that the first forms of life were primitive cells which constituted metabolically active systems, the genetic mechanisms of which evolved only later. However, the impact of molecular biology led to a shift of emphasis, and the relatively new *genes-first* theories addressed the question of the origin of self-replicating molecules which later developed the cells containing them. However, all these theories can be further divided into three hypothetical groups. The first group is formed by the non-historical theories, which stand upon the advance of molecular biology, and explain the processes of prebiotic and early biotic evolution exclusively on the basis of physical and chemical principles. The second group is formed by suprahistorical theories, which accept the historical nature of prebiotic and early biotic evolution, but preserving the invariance of the principles which support the mechanisms of evolution. The third group is formed by the truly historical theories, which accept that all principles and mechanisms of evolution belong to a single historical process and as such are also subject to change in the course of time (LUGOWSKI/GUTIÉRREZ-LOMBARDO 1991). Some theories in the second group and all theories in the third group constitute a set of theories which imply that new, nonimmanent natural principles, have appeared during the course of time.

Hans Kuhn Theory as a Plausible Scheme for Biogenesis

It has already been suggested that the theory proposed by Hans KUHN, might be an example of a truly historical theory of the origin of life, even though the very author of such a theory might not be aware that such is the case (GUTIÉRREZ-LOMBARDO/LUGOWSKI 1991). KUHN's theory satisfies two fundamental conditions for any plausible hypothesis on the origin of life: it respects any known physical-chemical law or principle, and it presents a logical continuous chain of events which are causally linked, and are reasonably probable both individually and as a whole set (KUHN 1976a). For KUHN the problem of the origin of life poses the questions of which principles guided the evolution of biological macromolecules, what limiting conditions emerged during the successive steps of prebiotic and early biotic evolution, and which mechanisms allowed to surpass such limiting conditions.

The starting point for such a theory is that the origin of life took place in an environment that was highly structured in the spatio-temporal sense. KUHN suggests that the basic spatio-temporal rhythms constitute the *primum mobile* of prebiotic evolution. Therefore, such an evolution is characterised by a series of consecutive phases which are determined by the cyclical changes of the environment (like day and night; drought and rainfall): every active phase of multiplication and propagation it is followed by a passive phase of selection determining which molecules are able to withstand a tendency towards disorganisation. This means that the "survival" of specific molecular configurations it is not linked to any global function which may drive such systems towards survival in a particular environment, the apparent adaptive capacity of such molecular configurations is ephemeral because it is dependent on the selective periods determined by external changes.

KUHN suggests that the two phases determined by the spatio-temporal structure of the environment were gradually substituted by a series of convergent and divergent phases. Every time that the prebiotic evolutionary trend stagnated, there was a phase of experimentation in which fortuitous events diversified the structures of the early molecular aggregates. Such is a divergent phase, characterised by the fact that the eventual complexity of the systems involved is not contributing any real selective advantage to such systems. Evolution then falls in a sort of vicious circle in which chance is not leading towards

the appearance of novel functions in the evolving systems. Such a kind of chance has been termed *divergent chance* (WETTSTEIN 1982, p42). However, the circumstances of prebiotic evolution can be suddenly transformed, when such an evolution enters into a convergent phase that might be determined by a single fortuitous event that contributes a novel selective advantage able to break the evolutionary impasse. Such a sort of chance has been termed *convergent chance* (WETTSTEIN 1982, p42). Within KUHN's theory, there must have been at least five of such events leading towards the appearance of the first unicellular living system:

1. formation of molecular aggregates,
2. emergence of catalytic properties within such aggregates,
3. the formation of a double-stranded polynucleotide which corresponds to the dextro (*d*) isomer of DNA,
4. formation of polypeptide films that function as primitive membranes surrounding such molecular aggregates,
5. formation of a primitive ribosome that may control and increase the efficiency of the mechanisms of self-replication acting upon the molecular aggregates.

KUHN's theory for the origin of life was formulated quite before that it was discovered the existence of self-catalytic RNA molecules, the so-called ribozymes. However, it will not make any difference to KUHN's general theory or to my current argument, if we consider that the molecular aggregates suggested in the events numbered from 1 to 3 were RNA molecules, in a world as yet devoid of DNA. Here I am using KUHN's theory to exemplify the general structure of any plausible modern theory about the origin of life, and I am not concerned with particular molecules or molecular mechanisms that made possible the actual crossing of particular evolutionary thresholds during pre-biotic and early biotic evolution on Earth.

It is a fact that the overwhelming majority of terrestrial organisms use DNA as the primary genetic material and protein as the chief agent of catalytic function. However, it has been suggested that the DNA-protein based life was preceded by RNA-based life, with RNA serving as both the genetic material and principal catalyst (GILBERT 1986). There are many uncertainties concerning how this RNA world arose. But a defining feature of the RNA world is that it must have contained RNA molecules that were capable of undergoing DARWINIAN evolution *based on natural selection*. This requires that RNA (an informational molecule) must be replicated efficiently and accurately in a reaction catalysed by the RNA itself (JOYCE 1996).

Several experts on the origin-of-life problem have serious doubts about the 'RNA first' notion. Particularly notable are the objections put forward by Gerald JOYCE as a leading contributor to the field of self-catalytic RNAs and *in vitro* evolution. JOYCE has stressed the obvious superiority of proteins over RNA in terms of catalytic power, as well as the unlikelihood of the prebiotic synthesis of RNA. Indeed, JOYCE suggests that life did not start with RNA as a self-replicating molecule, but rather as a result of chemical evolution through non-instructed processes which allowed the chemical ordering of complex peptides, and possibly the formation of membranous vesicles. This stage made it easier for the later appearance of self-replicating molecular systems (JOYCE 1991). Thus, a period of chemical non-genetic evolution was necessary so that a genetic system based on some simple RNA-like molecules will eventually be able to arise (JOYCE/ORGEL 1993).

Once more, I must stress that my purpose is not to prove that KUHN's theory is right, but only to use that theory to illustrate the fact that a series of thresholds must be traversed in order for life to appear. Recently, it has been argued that most contemporary theories on the origin of life support the continuity thesis that the development of life from matter is a gradual process to be explained on the basis of physical principles and as such, the gap between inanimate matter and life was not bridged by a unique, miraculous event (FRY 1995). However, stated as such, this 'continuity thesis' seems not to consider the fact that once there is life, there is no need that such life may be subjected to any kind of evolutionary process, nor that this process may be based on natural selection acting upon self-replicating genetic systems. Moreover, it is quite reasonable to imagine a living world populated by a single kind of primeval organism able to replicate itself and possessing a genetic system that is so well insulated from the environment and so faithfully copied in every round of replication, that there is no need or scope for further biotic evolution as we know it. Some may claim that any system having a material basis, such as a self-replicating genetic system, is naturally subjected to some form of variation and transformation derived from diverse factors like thermal noise and spontaneous degradative processes acting upon the system. Indeed, that sort of argument is at the basis of further theories (e.g., KIMURA 1983), that question or reduce the role played by natural selection in biotic evolution. However, here I am only concerned with biotic evolution as we know it, and thus I consider that natural selection is relevant to biotic evo-

lution, without entering any debate about the actual specific importance of natural selection in the process of biotic evolution. Therefore, the naive 'continuity thesis' ignores the problem that there is no apparent reason of why life should keep evolving further complexity. Moreover, rigorous experiments with isolated "replicators" (in this case viral genomic RNA molecules), lacking any phenotypic organisation beyond the presence of a replicating enzyme, the template nucleic acid and the appropriate molecular precursors, show that DARWINIAN evolution by means of natural selection will always lead the replicating system towards simplicity (Mills et al. 1967). These results are in agreement with the fact that there is no obvious theoretical reason to expect evolutionary lineages to increase in complexity with time (SZATHMÁRY/MAYNARD-SMITH 1995), but on the other hand, they are against the obvious fact that evolution on Earth has gone from simplicity towards complexity (ARANDA-ANZALDO 1997).

WETTSTEIN has suggested that in order to understand the evolutionary process leading to the appearance of life on Earth, it is necessary to make a distinction between a general (cosmic) evolution and a particular evolution that must exist from the moment that specific evolutionary thresholds delimit the main stages of the early evolution of life on Earth (WETTSTEIN 1982, p42). The crossing of each of such thresholds is determined in each case by a single event, nonpredictable and unique, termed a convergent chance. KUHN himself says that within every convergent phase it occurs an oriented evolution which explores a new domain, and gradually, such a convergent phase is transformed into a divergent phase which lacks any particular trend; the evolving systems stagnate within such a divergent phase until it happens the crossing of a further threshold, event that introduces the evolving systems into a new ecodomain which defines a new convergent phase (KUHN 1976b). This implies that at some time in the course of prebiotic evolution, such a process entered into an ecological stage, where the selective pressure upon the molecular aggregates was exerted not only by the environment but also *by the molecules themselves onto one another*. The increasingly complex inter-molecular relations that ensued lead to the appearance of complex structures and a complex spatio-temporal order within such structures. Actually, the evident increasing complexity of living systems over evolutionary time, suggests that the living systems participate themselves in generating the conditions that allow the evolution of further levels of life's complexity, a point that was appreciated by

DARWIN since the first edition of *The Origin of the Species*.

A common worry, shared by most current theories of the origin of life, is to show that the theory's particular scheme avoids assuming any principle of nature other than those given by physics and chemistry, as if physical-chemical laws would represent the sole valid input for a rational, simple and parsimonious theory of the origin of life. However, as KUHN's and many other theories suggest, the difference between living and non-living systems is determined by the fact that living systems have properties which allow them to survive in a given environment. In other words, 'they know how to adapt to their environment'. This knowledge is manifested as the *usefulness* of a certain inherited information for a given system surviving within an environment that displays a particular spatio-temporal structure. The emergence of such an information coupled with the first self-replicating molecules implied the end of true prebiotic evolution, in which the rhythms and changes of the environment were the original driving force behind evolution. The nature of evolution radically changed after such a crucial event, and from then onwards it shifted towards new trends that were independent of those strictly determined by the sole environment. Therefore, the appearance of early biotic evolution implied a change in the fundamental mechanism leading towards further evolutionary developments. In KUHN's own words: "[T]he appearance of the first systems that reproduce themselves and that eventually change by error during the copying process represents a *jump in quality* (cursives mine), in which a fundamental property of matter suddenly manifests itself. Systems begin to be carriers of information, of a meaningful message" (KUHN 1983), and "[W]ith the first occurrence of a self-reproducing entity a new quality of matter appears: the quality of knowing how to survive as a species" (LEHMANN/KUHN 1984).

Thus, true biological evolution begins as a process of 'learning' within populations of macromolecules in order for some of such populations to overcome successive barriers of stagnation.

Herewith appears a central problem faced by all theories about the origin of life which rest on the fundamental assumption that life arose from non-life: how it was possible that non-biotic molecules could reach the level of self-organisation necessary to produce systems for keeping and replicating information (genetic systems), considering that so far, it has not been possible to obtain in the laboratory any evidence for the existence of non-enzymatic infor-

mational replicating systems (ORGEL 1992). Current experience shows that one could not maintain a long polynucleotide sequence without informed enzymes and one could not have informed enzymes without a long polynucleotide to code for them (MAYNARD-SMITH 1982). This is a serious problem faced by all modern *genes-first* theories about the origin of life.

The proponents of the RNA world agree that for DARWINIAN evolution to occur RNA replication must be template-directed and energetically favourable, but the most crucial feature is that the RNA replicase must operate with sufficiently high fidelity to produce accurate copies of RNA molecules that are at least as long as itself (JOYCE 1996). So far, the best candidate for an RNA-based RNA replicase is a ribozyme which contains 98 nucleotides but allows the propagation of an RNA containing only some 12 nucleotides (EKLAND et al. 1996). The demonstration of an RNA enzyme with replicase activity will put the previous existence of an RNA world on a firm basis, but researchers in this field agree that a process of DARWINIAN evolution would be required to develop such a molecule in the laboratory (WRIGHT/JOYCE 1997). Thus, the question would remain as to how DARWINIAN evolution could have begun in the first place (JOYCE 1996).

On the other hand, it has been pointed out that the three main proponents of the *cell-first* models: OPARIN, HALDANE and BERNAL, were adherents of dialectical materialism, which explicitly assumes an intrinsic tendency of nature to create higher forms of organisation, although they do not provide any hint about the reason for such a tendency (DE DUVE 1991). These older theories emphasised the production of membranes, micelles, vesicles and so on, as a fundamental event leading to the origin of life, as they provide mechanisms for keeping together soluble molecules that have been involved in each other's synthesis. But this already seems to imply an essential precondition for the efficient action of natural selection, a situation that was neglected by the authors of such theories (MAYNARD-SMITH 1979). For example, in a modern theory of the *cell-first* kind proposed by DYSON, it is suggested that early in evolution appeared cell-like systems devoid of any sort of genetic system; such systems can exist for a very long time, so that for practical purposes they do not die, but also they do not interact with one another. Thus, according to DYSON, under such conditions there cannot be a DARWINIAN evolution based on natural selection, and evolution of the population of molecules within such cell-like systems proceeds by

random drift (DYSON 1985). In principle, such systems can diversify into many mutant populations as they split, but in the absence of exact replication, nothing of the parent-cell's qualities survive in the next generation above the chance level. Therefore, *without faithful replication and natural selection*, there can be no irreversible changes in the population, no real memory, only minor random fluctuations. By denying to such early cell-like systems the peril of death, they are also deprived of all the advantages of differential reproduction, which is so important in biological evolution as we know it.

The Emergent Principle of Natural Selection

It has been argued that life is a process capable of both doing work and incorporating information continuously, without being irreversibly worn out in the process. Every replicating system is both an order-maintaining and an order-inhibiting system. As long as it exists, preserving its own order, it hinders the creation of other ordered systems. The destruction of any autocatalytic system enhances the creation of another, because it releases reactants as it decomposes, increasing the likelihood for the formation of another molecule. When such a destruction is systematic, i.e. it eliminates only the systems with certain traits, then the reaction of the population is also systematic, increasing the number of systems with the opposite traits (ELITZUR 1994). Natural selection determines that such a destruction is truly systematic, because is exerted by rather constant environmental conditions that prevail for long periods over large areas.

Thus, it can now be properly emphasised that the only distinctive trait of the so-called convergent chance, rests on the true selective advantage which is due to the fortuitous appearance of a novel structure. WETTSTEIN distinguishes three different kinds of chance acting upon the early evolution leading to life: 1. a divergent chance, which sometimes can innovate at the expense of former conditions created by previous fortuitous events, but without leading to the crossing of an evolutionary threshold, 2. a decisive divergent chance, also termed non-authentic convergent chance, which innovates and destroys the conditions created by the previous divergent chance, thus allowing the crossing of an evolutionary threshold, 3. the true convergent chance which innovates without eliminating the participation of the previous divergent chance, and allows the crossing of an evolutionary threshold so that the resulting

new systems may explore further ecological niches (WETTSTEIN 1982, p49). A chance of the second kind was necessary to create the conditions for crossing the barrier between prebiotic and truly biotic evolution as we know it.

The current evidence available shows that all evolutionary thresholds, which occurred before the appearance of the first unicellular organism, have left no independent trace of themselves. It seems to be an empirical fact that the principles of self-organisation described by the logic of the unicellular organism have not been substituted or eliminated after their emergence in the course of time. For WETTSTEIN, the decisive divergent chance leading to the emergence of the first unicellular being, it is not only the last of its kind, but also the first of the so-called true convergent chances, because it allowed the expansion of the ecological niches to be explored by life (WETTSTEIN 1982, p50).

KUHN himself confuses the different categories of evolutionary transitions suggested by his theory, when he affirms that the process of prebiotic evolution is also DARWINIAN (KUHN 1974). However, other proponents of well-structured, modern theories of the origin of life, suggest that prebiotic evolution was not subjected to Darwinian selection (MAYNARD-SMITH 1979; EIGEN et al. 1982; CAIRNS-SMITH 1982; ELITZUR 1994; JOYCE 1996). Since non-informational replicating systems are not genetic systems. They cannot evolve by natural selection, because they do not store information in a stable way (ORGEL 1992; Joyce 1996). That 'natural selection' is a technical term which implies amplification through replication is illustrated by the following quotation from a review of the book *The Natural Selection of Chemical Elements* (WILLIAMS/DA SILVA 1996): "I have a gripe about the title, though. The book is all about selection at various levels, but it is a pity that these are discussed under the blanket term 'natural selection'. I would like to have ignored this as a minor gaffe... but there it is, blazoned in the title... So I don't think something should be described as natural selection just because it is natural and results in selection. One consequence of this is that we may overlook the problem of the origin of life: the origin of a long-term process of evolution through, yes, *natural selection*—which is not just another example of the sort of thing we find here, there and everywhere...But actually the origin of life is difficult in an extremely interesting sense: it hardly seems possible that it could have happened at all. This is largely because natural selection depends on high-quality replication; finding molecular machinery that can do this under simple

conditions is a major challenge for modern chemistry...(this is) a wonderful book. Pity about its title." (CAIRNS-SMITH 1996)

This fact do not denies that non-informational, non-genetic, replicating systems may have been important for the origin of life, because they may well have been necessary for the creation of an environment in which informational replication could get started (MAYR 1988, p16; JOYCE 1989).

Thus, selection in the prebiotic stage of evolution is a consequence of the periodicity of the environmental conditions acting upon early macromolecular societies. Within such societies appear molecular populations displaying certain functional capabilities which determine whether the molecules may survive or not. However, such functions are not programmed in any way by the structure proper to those molecules. There is no relationship between the logic of such macromolecules and their associated functions which allowed them to survive, because such functions appeared only through the action of the external environment. But after the decisive divergent chance (non-authentic convergent chance) that made possible the emergence of informational self-replicating systems, such new systems were able to enter into direct competition with the previous macromolecular systems. Moreover, the event represented by the first true convergent chance (according to the definition by WETTSTEIN), establishes the logic of the unicellular system which might explore new habitats in direct competition with previous existing systems, but without implying the complete elimination of the former systems. On the contrary, the mutual competition between systems renders them more able to adapt and survive. Natural selection becomes a factor tied to the logic of societies of evolving living systems, because such systems have lost their ephemeral character that was only subjected to external periodicities. It is clear that once the first unicellular beings appeared, they rapidly went to diverge into species, and the appearance of a new species did not imply the compulsory elimination of the former species. From then on, a new system (species) introducing a new selective advantage will devote a certain time to explore different niches until it emerges the eventual competition with other species. The fact that the new selective advantage does not completely abolish the logic of the less-complex systems, and thus the less-complex systems have the opportunity to survive as long as they find how to adapt to the new circumstances created by the new competitor, implies a

radical departure from the logic behind the series of divergent phases of prebiotic evolution, characterised by the lack of continuity of the organisation represented by the ancient systems. The logical innovation represented by the unicellular organisation is the consequence of the last decisive divergent chance which becomes the parent of all the future true converging chances.

The Question of Causality in Biology and the Principle of Natural Selection

The concept of law is fundamental in science. Most scientific explanations base their assertions on scientific laws. According to BUNGE, scientific explanation is explanation by law, not by causes (BUNGE 1979; CASTI 1992, p409). Cause-and-effect relationships are comfortable for the human mind, but since HUME's time, a scientific epistemology based exclusively on material and efficient causality looks rather weak as the source of transcendental knowledge. Moreover, not all scientific laws are causal. Causal determination is only a special type of determination. Many important phenomena are explained by phenomenological laws having no causal component, such as the second law of thermodynamics (CASTI 1992, p37).

In classical physics laws were considered universal. A belief in universal, deterministic laws implies a belief in absolute prediction. For the classical theory of causality a causal explanation is as good as its predictive value (BUNGE 1959). However, SCRIVEN has suggested that an important contribution of biological evolutionary theory to philosophy is that it has shown the independence of explanation and prediction, considering that, according to SCRIVEN, the theory of natural selection can describe and explain phenomena with considerable precision, but it cannot make reliable predictions (SCRIVEN 1959). A similar situation is found in the field of meteorology which belongs to the physical sciences. Indeed, the belief in micro-precise causality (micro-determinism) has been untenable since the enunciation of HEISENBERG's uncertainty principle. Scientists now recognise that most physical laws are not universal but are statistical in nature, leading to probabilistic predictions in most cases.

The conceptual framework of biology is quite different from that one in the physical sciences. Physical-chemical principles are operant in biological processes, but a purely physical description of them is at best incomplete if not irrelevant (MAYR 1982). Generalisations in biology tend to be statistical and

probabilistic and often have numerous exceptions. Nevertheless, comprehensive biological theories appear to certain authors as comparable in explanatory power to those of the physical sciences (MUNSON 1975; VOLLMER 1995). But for other authors the laws of biology are no more than high-level generalisations (KITCHER 1984).

According to PANTIN, physics and chemistry are exact sciences because much of the wealth of natural phenomena is excluded from their study (PANTIN 1968, pp123-128). For SIMPSON, all known material processes and explanatory principles apply to organisms, while only a limited number of them apply to non living systems (SIMPSON 1964). It has been suggested that there is not one single inferential chain which leads from anything important in physics to anything important in biology, and in every direct confrontation between universal physics and special biology, it is physics which has to give ground (ROSEN 1991). Biology poses the need of adopting an enlarged scientific vocabulary that includes concepts such as biopopulation, teleonomy and program (MAYR 1988, p21).

The notion of causality in biology is quite different from that one in classical mechanics, because the complexity of the living systems is an *a posteriori* product of natural selection, which acts as a "tinkerer", being completely opportunistic considering that it starts from scratch in every generation (JACOB 1977; MAYR 1988, p210). Thus, for many a scientist natural selection is permissive rather than constructive (PANTIN 1968, pp73-74). The descriptive and explanatory power of the principle of natural selection (PNS), has been compared to that of the second law of thermodynamics which describes the fate of matter and energy in complex systems and tells us the direction in which change is taking place (PEIRCE 1877; MAYR 1988, p36). It is a fact that natural selection is regarded by many a biologist as the universal direction-giving factor in evolution, without implying that we can know in advance the direction of the evolutionary changes (MAYR 1988, p109; ARANDA-ANZALDO 1996).

The PNS is understood in the following way: over a long period of time the environment of a given species is never stable but it will change in various ways. As it does so the characters which best fit the individuals to the changed environment will be selected (not consciously of course) and the species will change. The environment may change only in part of the range of the species and thus lead to divergence and the production of a new species alongside the old one.

For several modern theoreticians the theory of natural selection (TNS) has a statistical nature, but there has been debate about the source of the statistical character of evolutionary theory. ROSENBERG has suggested that the DARWINIAN dictum about the survival of the fittest is the basis of the statistical character of the TNS (ROSENBERG 1985, p217). In fact, ROSENBERG has axiomatised the TNS in order to identify its biological principles of undoubted generality, which may be unrestricted in expression and independent of the actual course of evolution in our planet (ROSENBERG 1985, pp212–213). Thus, the fourth axiom of the TNS states the following: if D is a physically or behaviourally homogeneous subclass of a species, and D is superior enough in fitness to the rest of the members of the species for sufficiently many generations, then the proportion of D in the species will increase. For ROSENBERG, this axiom is of the same form as the statistical version of the second law of thermodynamics which states that given enough time and enough interacting bodies a physical system will always move towards equilibrium and maximum entropy, even though we cannot completely disregard the possibility of a highly improbable opposite outcome. The fourth axiom of the TNS reflects the possibility that evolution need not and does not move in a straight line towards equilibrium levels of populations size for various species and their subpopulations, but it also asserts that in the long run, evolution must move in this direction and that the length of the long run is a function of these differences (ROSENBERG 1985, p217).

The arguments against the existence of a principle of natural selection

There are several scientists and philosophers which think that there are no universal and distinctively biological laws of nature. For them, the biological laws are indeed contingent (STEBBINS 1982, p14; BURIAN/RICHARDSON 1996). According to this view, the PNS is not a law but something similar to a plot or narrative that requires specification by an account of the physical–chemical and environmental factors which affect survival and reproduction (BRANDON 1978). Moreover, it has been suggested that most biochemical rules are contingent on the physiological contexts secured in the course of evolution (BURIAN/RICHARDSON 1996). Therefore, the contingencies of evolution underlie the biochemical, developmental and ecological regularities and as such, the biological ‘laws’ are no more than temporary regularities which result from the process of

evolution itself. Such ‘laws’ cannot be generalised over evolutionary time (BEATTY 1995).

SHIMONY has provided an important argument against the existence of a principle of natural selection. For him the theory of natural selection is only a rich systematisation of biological knowledge without a first principle. He suggests that whenever the fitness of an organic variety is well defined in a given biological situation, its sources are local contingencies together with the background of laws from disciplines other than the theory of natural selection. Thus, any generalities that may hold in that theory are derivative rather than fundamental (SHIMONY 1989a). SHIMONY grounds his argument on the notion that the fundamental concepts of any theory should be presented explicitly and what makes a concept fundamental is that it is not explicitly defined in terms of other concepts within the theory; therefore, such a concept is underived within the theory (SHIMONY 1989b). Nevertheless, SHIMONY is ready to attribute the category of ‘principles’ to the laws which underlie the theory of Mendelian heredity. For SHIMONY it is enough that such principles impose constraints upon the temporal development of the biosphere, even though the constraints in question are restricted to terrestrial life and they are probabilistic. In contrast, SHIMONY argues that the theory of natural selection imposes no constraints of its own, and it is rather a systematic study of temporal development of the biosphere after all constraints from elsewhere are acknowledged: constraints imposed by the general laws of physics and chemistry, constraints imposed by general laws from other branches of biology, constraints imposed by the relevant singular facts of the biological situation. Therefore, SHIMONY will only accept the existence of the PNS if someone demonstrates that such principle imposes any sort of *unborrowed* constraint upon the biosphere (SHIMONY 1989b).

But philosophers of biology like Elliot SOBER, have shown that the arguments offered by SHIMONY against the existence of the PNS are rather dependent on the particular definition of ‘principle’ used by SHIMONY (SOBER 1989). Moreover, the argument of SHIMONY is clearly weakened by the fact that he acknowledges a ‘principle’ status to MENDEL laws in spite of the contingent fact that only some living systems on Earth follow such laws (organisms undergoing sexual reproduction), while a large number of organisms and genetic systems do not follow such laws (unicellulars, transposons), but nevertheless they are known to evolve. Thus, whichever principle or principles underlie the evolution of living sys-

tems, they seem to stand at a deeper level than the so-called MENDEL laws.

At first glance, if we accept that there is not a PNS, then HUME's second problem becomes a perpetual stumbling-block for any attempt to develop a rational, scientific biology that might offer something more than mere descriptions of current events. This poses a tremendous problem for the coherence of mainstream biological science, since a large part of current research in biology is grounded on the theory of natural selection which implies the existence of the PNS. Moreover, the radical view that denies the existence of specific biological laws or principles (sponsored by some contemporary philosophers and biologists), implies that there is no possibility to extrapolate our present biological knowledge into the future. It also implies that the rules that guide the evolutionary process may change at any time. Therefore, prediction in large areas of biology becomes an impossible task and most biological explanations are reduced to historical reconstructions of past scenarios. Such reconstructions will always rest on the shaky ground provided by circumstantial evidence that may always change in the course of time. Thus, such a radical position undermines the rationality of biological science and condemns it to a second rate scientific status.

Differences Between Biotic and Nonbiotic Evolution

It is possible that the radical view which denies the existence of autonomous biological laws and principles, is based on a lack of distinction between biotic and non-biotic evolution. It is true that certain 'laws' of biology such as MENDEL's laws of heredity are too narrow in scope, so as not to guarantee their status as truly general biological laws. As it was mentioned before, MENDEL's laws are only applied to sexually reproducing organisms and even within such organisms there are genetic entities, such as the transposons, that do not follow those laws (LEWIN 1994). However, the empirical evidence suggests that all living systems are subjected to the PNS. Biology is by definition the science of life or living systems; therefore, the PNS seems to stand at a deeper level within biological science, since apparently, all living systems evolve. At the core of my argument for the autonomy of the PNS, lies the question of whether the PNS can be applied to material non-living systems, in which case it is part of physical science and quite possibly inherent to the fabric of the universe, or whether the PNS arose

at a certain epoch in time, perhaps alongside the first living systems which are its subject matter.

WILLIAMS, in a now classical book, stated that the modern theory of evolution is: "...based on the assumption that the laws of physical science *plus natural selection* [cursives mine] can furnish a complete explanation for any biological phenomenon, and that these principles can explain...any particular example of an adaptation" (WILLIAMS 1966).

I share the worries of those who raise a question mark about the claims of the synthetic theory in the sense that it is capable of furnishing a complete explanation of biotic evolution (ARANDA-ANZALDO 1996). Also, I share the doubts about the idea that natural selection is the *only* driving force in biotic evolution. HALDANE (as quoted by WADDINGTON) already stated that '...natural selection is an important cause in evolution', thus suggesting that it is not the *only* cause (WADDINGTON 1969). Here it is not my purpose to develop an argument about which are the other possible factors or principles which may guide or influence biological evolution. Nevertheless, I cite the passage by WILLIAMS in order to illustrate the idea that natural selection is a principle different and not derivable from the laws of physics.

Very soon after the big-bang (if such a thing was the case), the laws of physics, as embodied in quantum mechanics, began to rule the physical universe, but the future laws of chemistry were already implied in the early quantum world. This fact is independent of whether the universe could have evolved in a different way that do not leads to the appearance of complex chemical molecules. I rise this point in order to illustrate that some *emergent* laws, such as the laws of chemistry, are no more than particular cases of older and more general principles. The fundamental laws of chemistry are wholly derivable from quantum mechanics. Actually, there is much more agreement between chemistry and quantum mechanics than between quantum mechanics and general relativity. So far, none has been able to develop a consistent quantum theory of gravity. However, to my knowledge, up to this day, there is not the faintest evidence for the existence of a chain of inferences or speculations that may lead to the derivation of the PNS from either quantum mechanics or general relativity. Of course, the previous statement do not proves that there would never be such a derivation, but the burden of finding it lies with those that might pretend to include the PNS within the realm of physical science. On the other hand, there is a time honoured tradition of finding analogies between thermodynamics and biological evolu-

tion as I have previously shown. Moreover, in a now classical paper, LEWONTIN already attempted to axiomatise not the principle, but the theory of natural selection, and as such he tried to define the minimal general characteristics that a system must satisfy in order to be suitable for evolution by means of natural selection. According to LEWONTIN, thermodynamic factors as well as very general boundary conditions that apply to all complex molecular systems are among the obvious necessary conditions for the action of natural selection (LEWONTIN 1970). However, from the previous statement some readers have derived the unwarranted suggestion that this is equivalent to proving that the PNS is derivable from thermodynamics. Also, there have been some recent attempts to translate certain concepts derived from the PNS, such as fitness, into the language of thermodynamics (MADDOX 1991; TORRES 1991). But I believe that such attempts represent a valid effort to find rules of correspondence between two basically incommensurable theoretical frameworks.

Many people confuse nonliving with living evolution. The claims that the evolution of galaxies or radioactive decay are similar to biological evolutive processes are quite erroneous, but this has not deterred some authors from suggesting that the fine-tuned values of the physical constants are the result of a process of cosmological natural selection, because causally isolated 'universes', such as a black hole and our own universe, display the properties of variation, multiplication and heredity (MAYNARD-SMITH/SZATHMÁRY 1996). But so far, there is no clear formulation of where might it lay the so-called heredity of a given universe, or what do they mean when talking about fitness and differential survival in relation to such 'universes'. Without any proper definition of the system or physical feature responsible for storing and transmitting the "hereditary" information of the universe, and in absence of definite evidence that any universe is able to replicate and leave progeny akin to itself, this extrapolation of the PNS to the problem of cosmological evolution seems a case of far fetched speculation.

LEWONTIN has already shown that evolution in galaxies is transformational, not variational evolution; while MAYR has shown that radioactive decay, controlled by physical laws, is teleomatic, not teleonomic as biotic evolution (LEWONTIN 1983; MAYR 1988, p17). For MAYR, any change in an object or system which is a result of its intrinsic potential, such as the change of a white star into a red star, is developmental evolution, entirely due to the action of teleomatic (physical) processes. But the evolution

of living systems is variational evolution, and is due to the selection of certain entities from highly variable populations of unique individuals and the production of new variation in every generation. Thus, MAYR classifies as teleomatic processes those regulated only by external forces or conditions, since the end-state of such inanimate processes is automatically achieved. All teleomatic processes come to an end when the potential is used up, they simply follow physical laws such as gravity or the second law of thermodynamics. On the other hand, teleonomic processes, display goal-directed behaviour which is guided by a program and it depends on the existence of some endpoint. In the case of living organisms as we know them, there is a genetic program which, within the context of General Systems Theory, is in some way causally responsible for the teleonomic nature of the organisms (VON BERTALANFFY 1976). Whether such a program originated through a lucky event or through a slow process of gradual selection, or through learning and conditioning, is quite irrelevant for the classification of a process as teleonomic (MAYR 1988, p45).

Complexity by itself is not a valid criteria to distinguish between living and nonliving systems, because some nonliving things (such as the weather system) are highly complex. But living systems are complex at every hierarchical level. Organisms are unique at the molecular level because they have a mechanism for the storage of information that has been historically acquired. Inanimate systems lack such a property. MAYR himself speculates that perhaps there was an intermediate condition at the time of the origin of life, but the distinction between living and nonliving matter has been complete for about three billion years, because all known organisms possess a historically evolved genetic program (MAYR 1988, p16).

We must bear in mind that natural selection acts through differential reproduction (JACOB 1973). Thus, adaptation is the result of a subtle game between the organism and the environment. The environment selects the organism in the same way as the organism selects the environment, and reproduction becomes an amplifier that enhances the spontaneous variations. Once set up in a particular evolutive path, an organism is constrained by natural selection (alone or in association with other as yet not well established factors), either to follow that path or to disappear. Here we find a radical distinction between living and non-living evolution, because, for example, the disappearance of all atoms of a given atomic number does not entail the disap-

pearance of a place or space in the periodic table of the elements, whereas the disappearance of all members of a species entails its extinction. As ROSENBERG has pointed out, the eventual appearance of new organisms qualitatively indistinguishable from the former members of an extinguished species does not constitute the reappearance of the same species, since it did not arise in any line of descent from the old one which became extinct (ROSENBERG 1985, p206).

Artificial life and the PNS

The growing research on artificial life models has provided interesting insights which suggest that natural selection corresponds, indeed, to a particular stage in the history of evolution on Earth; because complex molecular systems can achieve an important degree of self-organisation even in the absence of natural selection. But the realisation that complex systems display a self-organising potential implies that it is necessary to study not only such self-organising properties but to understand how such an spontaneous organisation guides, allows, constrains or interacts with natural selection (GOUJON 1995). Therefore, it might be the case that there is an as yet unknown fundamental law that links the self-organising properties of complex molecular systems with the principle of natural selection thus leading to biotic evolution as we know it. KAUFFMAN, a leading figure in the field of artificial life research, states the following in the Epilogue of his influential book *The Origins of Order*: "I have tried to take modest steps toward characterizing the interaction of *selection and self-organization*" (cursives mine) (KAUFFMAN 1993).

We must bear in mind that cellular automata and other computer-generated 'beings' which display adaptive evolutionary trends, do so because they are the result of computer simulations designed to imitate the properties of life as we know it. It is a fact that natural selection was 'invented' by nature itself, not by an artificial life scientist. However, some may claim that the PNS is only valid for life on Earth, considering that we have as yet no formal evidence of life elsewhere in the universe. Thus, it is possible to design artificial life scenarios in which the evolution of complex systems occurs according to fixed and well defined rules or 'laws' which are independent of natural selection (LANGTON 1996). Therefore, it is not apparently unreasonable to suppose the existence, in another part of the universe, of living systems that do not need to evolve, or they do evolve

but according to laws or rules which do not include the PNS.

Nevertheless, in spite of the fact that we still lack any hard evidence for the existence of life elsewhere beyond the Earth, there is a consensus suggesting that there may be life elsewhere in the universe. And yet, some debaters might argue that life elsewhere is different from life on Earth, because such alien life obeys to local, contingent biological rules which cannot be applied to other regions of the universe. But this argument leads to a blind alley, since it implies that in the absence of any common set of biological principles or laws extrapolable to any region of the universe, it is not possible to speak of life occurring elsewhere beyond the Earth, because we lack those common principles which allow us to define and to identify the basic characteristics of life. According to this view, stones in Mars might be alive but we have no means to determine that. A reasonable option to undo such a tangle of hair-splitting arguments, is to dig up Occam's razor and then to assume, as most physicists do, that there is a general trend towards regularity in nature which suggests that life on Earth, as well as elsewhere in the universe, is based upon some common principles, including some biological principles such as the PNS.

Conclusion

The material parts of both living systems and artificial machines can only be constructed on certain structural principles, determined by physics and chemistry, and only limited kinds of material are available to make them. The objects of the natural world constitute a vast array of varying complexity but nevertheless with severe limitations. Natural selection has in fact enforced evolving species of organisms to traverse certain routes through that complex array of possible systems. Whatever the complexity of possible living systems at different levels of organisation, it is natural selection which ensures the survival of such systems, but the classes of organisms which attain reality are selected from the classes of possible material configurations, because natural selection is permissive rather than constructive.

Thus I restate my original contention that natural selection constitutes a new general principle of nature, underived from any known physical or chemical law, that emerged only after the appearance of the first living informational self-replicating systems. On the other hand, I do not want to imply that life is equal to an informational self-replicating sys-

tem. Actually, it is not my purpose to offer any definition of life or to enter any debate about the items to be included in such a definition, because this is a complex issue beyond the scope of the present work (WUKETITS 1995).

On the question of why it was possible for such self-replicating informational system to appear, there seems to be no current definite explanation based on fundamental physics, beyond the suggestion implied in what is known as the *strong anthropic principle*, which contends that physical laws and fine-tuned physical constants can be explained by the fact that they have given rise to intelligent observers, able to discuss their nature. Thus, according to such a controversial principle, the universe must have those properties that allow life to develop in it at some stage of its history (BARROW/TIPPLER 1986). But there is nothing in the argument behind the anthropic principle, suggesting that the pathway towards intelligent life must specifically depend on evolution by means of natural selection. This fact reinforces the notion that the PNS is underived from any known physical principle.

Life is quite old for terrestrial standards (about 3.5 to 4 billion years old). However, it is a relatively recent phenomena when compared with the currently estimated age of the universe (around 15 billion years). The laws of physics and chemistry ruled alone until natural selection appeared; a new principle of nature which did not abolish any of the previous general principles but that it was also not implied or contained in any of such ancient natural principles (as far as current knowledge allows us to conclude). Some recent models suggest that DARWINIAN biotic evolution is consistent with the general principles of thermodynamics, but they do not imply that natural selection belongs to the realm of thermodynamics (MADDOX 1991; TORRES 1991). Of course, if someone demonstrates that the PNS is fully derivable from thermodynamics or from any well established theory from physics or chemistry, then the main argument of the present discussion shall be falsified, but that remains to be shown.

Once more, I must stress that it is not my point to argue whether natural selection is the only principle acting upon biotic evolution; it is quite likely that several other principles, many of which belong to physics and chemistry, are actively involved in driving such an evolution (DOOLITTLE/SAPIENZA 1980). Also, it is not my intention to enter any discussion on whether biological evolution is a sudden or a gradual process (GOULD 1980). However, if as some thinkers suggest: the PNS is only a local 'rule of

thumb' because it is completely contingent on the particulars of evolution on Earth (STEBBINS 1982; SHIMONY 1989a), then both, HUME's first and second problems (the problem of induction and the possibility that the laws of nature may change in the course of time), become harder than ever to solve, unless we regard biological science as a local exercise in description without any pretension to a universal character. I believe that those who suggest that the PNS is nothing more than a 'meta-principle' or 'rule of thumb', must consider biology as a *sui generis* natural science, closer to history than to true natural sciences such as physics and chemistry. This position supports, in quite a radical way, HUME's suggestion that the course of nature might change in a rather arbitrary way. Since an 'unprincipled' theory of natural selection implies that the evident process of biotic evolution will always escape from rational analysis, and that whatever rules we assumed were involved in the evolution of past organisms they are likely to change at any time in the future. A rather naive way to overcome the extreme epistemological conclusions and problems implied by those which deny any law-like status to the PNS, is to consider life as a local phenomenon of limited scientific or epistemological interest. If such is the case, then HUME second problem is only relevant for the physical-chemical sciences, which then become the only 'possible' sciences, at least within an epistemological outlook which believes in the rationality and coherence of the scientific enterprise.

Therefore, I suggest that the second problem posed by HUME, namely, *whether the course of nature may change in time*, can be currently answered in the affirmative, because as far as reason allows us to tell, natural selection constitutes the evidence that new general principles could emerge in time. However, I believe that this point of view has less radical consequences than those which deny any law-like status to the PNS, thus implying that biotic evolution, as we know it, is a basically unintelligible accident and hence, that biological science is a truly odd discipline. Economy of thought suggests that the PNS appeared when nature arrived at a certain point in the evolution of complexity (which none may attribute to the PNS itself), otherwise the PNS becomes, indeed, a meta-magical principle, a weird entity that was wandering in the abstract depths of nothingness, just awaiting for the right time to manifest itself.

Nowadays, it is rather common to find in the specialised literature further arguments showing that biological science is basically a historical science with an epistemological framework completely dif-

ferent from physical science, and as such, biology is not then subjected to the traditional criteria that might define a mature science based on law-like principles (as exemplified in C.G. HEMPEL's classic *Aspects of Scientific Explanation* 1965). This position, shared by a number of philosophers of biology, leads to an unavoidable clash with the research programmes of disciplines such as biochemistry and molecular biology which nevertheless, are very influential in modern biology. On the one hand, biochemistry and molecular biology strive to achieve the level of rigour associated with physics and chemistry. Indeed, biochemistry can already provide very specific, quantitative predictions about the future of a given molecule incorporated in the transformational steps of a specific metabolic pathway. On the other hand, we are still waiting for a foolproof explanation of how the giraffe got its long neck, not to mention the fact that although we have a reasonable idea about the future of our Sun, we cannot provide any reasonable scenario about the features of the species that might be alive on Earth, within one or two million years ahead. Therefore, although biochemistry and molecular biology provide a powerful material basis for certain neo-DARWINIAN claims (as explicitly stated in MONOD's book *L'hasard et la nécessité*), those disciplines do not seem to require an evolutionary theory of any sort, because the explanations in biochemistry and molecular biology are synchronic, while evolutionary biology needs diachronic explanations.

I am among those who worry about the fact that the current progress of biological science takes place within a dichotomous epistemology: on the one hand, the search for biological covering laws; on the other hand, the positive assertion that biology needs not such covering laws. I believe this is not a problem of reductionism vs holism, nor a revival of the old dispute of materialism vs vitalism, because it is not a question of reducing biology to physics. On the contrary, this is a more serious problem that reflects a deep incoherence in the structure of biological science itself. This can be illustrated by the recent discoveries in the molecular biology of development, which show that a set of genes, collectively known as *Hox* genes, orchestrate the patterning of the embryo in animals as diverse as the fly and man. Moreover, highly homologous genes belonging to this *Hox* family, are

directly involved in controlling the development of appendages as diverse as wings, fins, arms, antennae and lobopodia. The molecular data supporting this conclusions are both elegant and straightforward, but the alternative evolutionary explanations for this facts are very contrasting. One hypothesis suggests that genes or genetic circuits were convergently recruited for limb development during the evolution of vertebrates and arthropods. These genes would not be involved in appendage development in the common ancestor of vertebrates and arthropods, so each gene or circuit was involved in other developmental events. This situation would require the parallel co-option of members of similar gene families, acting along different developmental axes to pattern an outgrowth of the body wall in at least two taxa. Thus, the evolution of limbs in each group would necessary involve the convergent recruitment of numerous genes to define similar developmental axes. If such was the case, this would be a most baffling case of highly-improbable convergent evolution. The alternative explanation suggests that some genes or circuits were components of an ancestral genetic regulatory system that was used to pattern a structure in the common ancestor of vertebrates and arthropods, but this ancestral structure need not to be homologous to arthropod or vertebrate limbs. The genetic circuit could have originally patterned any one of a number of outgrowths of the body wall in a primitive bilaterian. It is assumed that such genes were initially involved in other developmental events and the key step in limb evolution was the establishment of an integrated genetic system to promote and pattern the development of certain outgrowths. So this system provided the foundation for the evolution of quite heterologous structures such as wings, arms, antennae and lobopodia (SHUBIN et al. 1997). Thus, in spite of the hard molecular data on which the aforementioned hypotheses rest, it is difficult not to conclude that we end with a pair of just-so stories in our hands. If we add to this situation the possibility that the PNS is devoid of any law-like status, then evolutionary scenarios, as those previously described, become truly arbitrary schemes.

It is not possible to say whether further new general principles will eventually emerge, but I think that the example of natural selection is enough to support the idea that HUME'S second problem is alive and well. Moreover, in

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Jacques MONOD own words: “La biosphère ne contient pas une classe prévisible d’object ou de phénomènes, mais constitue un événement particulier, compatible certes avec les premiers principes, mais *non déductible* de ces principes. Donc essentiellement imprévisible.” (The biosphere does not contain any foreseeable class of objects or phenomena, but it constitutes a particular event that is compatible with general principles, but that it is *not deducible* from such principles. Therefore, the biosphere is essentially unpredictable) (MONOD 1970).

I think HUME could have construed such a statement so as to mean that biological science is an impossible task. Unless we consider a further possibility: that chance-driven evolution by means of natural selection is not the truly fundamental process that supports the evolution of life on Earth (and possibly elsewhere), but that there must exist some constant and rational biological laws which are truly responsible for guiding and organising the varied complexity that we call life. Then, the PNS might happen to be wholly derivable from such, more fundamental, biological evolutionary principles, which may impose the true constraints upon biological evolution. Such laws might be derivable or non-derivable from physical-chemical laws, and so they might or might not overcome the burden of HUME’s second problem, a point that is beyond the scope of the present discussion.

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In his classic *What is life?*, SCHRÖDINGER says: “...from the knowledge we have gained of the structure of living matter. We must be prepared to find a new type of physical law prevailing on it. *Or are we to term it a non-physical, not to say super-physical law?*” (cursives mine) (SCHRÖDINGER 1945).

Indeed, besides SCHRÖDINGER, some of the leaders of quantum mechanics, such as BOHR, HEISENBERG, and PAULI, postulated that someday someone would discover laws in organisms that were different from those which operate in inert matter. Actually, when Max DELBRÜCK switched from physics to biology, one of his original objectives was to discover such laws (KAY 1985).

Later on, León BRILLOUIN argued: “It may well happen that the discovery of new laws and of some new principles in biology could result in a broad redefinition of our present laws of physics and chemistry, and produce a complete change in point of view” (BRILLOUIN 1964).

Thus, it might be the case that biological science needs to reduce its dependence on the DARWINIAN paradigm (based on the theory of natural selection), in order to shift towards a further paradigm, perhaps along ARISTOTELIAN lines, so as to overcome (fully or partially) the burden posed by HUME’s second problem, and then to become as rational as physical science.

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Man's Special Position in Nature

The Relationship between Biological and Cultural Development

1. Man's special position: scientific knowledge and dogma

Together with ethnology, contemporary biology, psychology and epistemology basically and unanimously start out from an special position taken by man (cf. OESER 1996; GUILLE-ESCURET 1994). In the framework of Evolutionary Epistemology, this statement relates to man as both a biological and sociocultural being—the starting point being the comparison between man and other living beings. Continuity between biological evolution and cultural development is a significant precondition in this respect, yet man is considered to have a special position in nature due to his quite specific development. DARWIN already referred to the “effet réversif de l'évolution” in the sociocultural domain (TORT 1983, 1992). The unresolved question must therefore be addressed as to what extent today DARWINIAN biological theory of evolution is important for a specifically sociocultural approach.

Even though various sciences start out from man's “special position”, this does not necessarily imply that the conclusions drawn from such an insight are the same in all approaches. In DURKHEIMIAN sociology, a classical example, the premise of man as a social being is intended, from the very beginning, to rule out extrasocial causes of social phenomena or

Abstract

With regard to various theories of development, this paper addresses the approaches and premises of Evolutionary Epistemology and other evolutionary-oriented biological sciences, alongside Ethnology as a cultural science. Furthermore, the intricacies and sources of mistakes connected with interdisciplinary research are critically outlined. With the particular example of analogy as a cognitive instrument, the author brings up the problem of “biologization of society” which has often been implemented in this framework. Reference is not only made to the misapplication but also to the genuine options of applicability of “analogy as a source of knowledge”. Emphasis is put on the necessity to integrate basic interdisciplinary research on man as a cultural being in the theoretical framework of Ethnology.

Key words

Evolution, cultural development, Evolutionary Epistemology, ethnology, analogy as a cognitive tool, interdisciplinarity.

change. Against this background, man is regarded a priori to be a social being: Human nature is meant to be seen as social, while other factors considered as external to society are basically to be omitted from perspective (cf. CHEVRON 1990; BARKOW/COSMIDES/TOOBY 1992). This approach has exerted a deep influence on several generations of sociologists. The theory of autopoiesis in LUHMANN's understanding should also be mentioned here, as it considers social development to be the mere result of a “self-referential” procedure—in spite of its basic intent to biologize social development (cf. LIPP 1987).

These scientists who see in man an exclusively cultural being found their argumentation on the premise of man as a social being, without acknowledging the concomitant necessity of an interdisciplinary debate of their basic arguments. Likewise, little attention is paid to the possible consequences ensuing from such a discussion, taking into account cultural as well as biological factors.

In the cultural sciences, especially in ethnology, an occasional starting point was the assumption of transition from a natural to a cultural state. For instance, LEVI-STRAUSS and other writers touched on this issue of transition, yet their investigations in this connection were seldom a focus of their scientific endeavors. As to contemporary populations, rough schemes made out a difference between those

living in a natural state and others of high culture. Such a differentiation indicated that the former category was not conceived of as lacking culture but rather as representative of a lower cultural level, since the latter category was referred to not as simply cultured—but as highly cultured.

Most scientists would agree that man assumes a special position in nature only as a cultural being. But, at times, humans are considered to be cultural because they are social beings—equipped with language and symbolic abilities and developing these abilities in social interaction (cf. *inter alia* CARRITHER 1992). Nevertheless, man's sociability is only a necessary yet by no means sufficient prerequisite of "culturability", because—as is sufficiently known—man is not the only social being on earth. Many other living beings share this character with him.

Of course, the premise of man's special position can also be substantiated religiously, on the assumption that man as God's creature is to be considered as set apart from other offspring of "Creation". (Cf. the discussion in the Catholic church on human singularity; extremist positions such as Creationism are referred to here for the sake of completion only).

This paper deals with two questions. The first question is, how do the different findings on man's special position—as a natural, social and/or cultural being—fit together? And the second question is, to what extent can these insights be made consistent? Or more precisely: To what extent can the statements elaborated by the theory of evolution—encompassing all sciences that present an evolutionary approach to certain issues, including Theoretical Biology, Evolutionary Epistemology and Evolutionary Psychology—be conciliated with sociocultural theories of development?

Yet even when taking into account none of these various premises and theories, the scientific issue remains unresolved as to man's special status in nature. One intricacy that cannot be bypassed refers to human beings' actual nature and cultural development. In other words, we have to ask, if man's special position, be it assumed or established on the basis of specific findings, dismisses the social and cultural sciences from approximating the biological preconditions of human nature? Or rather—put more precisely—is a purely biological answer to this question sufficient and are additional insights in a sociocultural perspective perhaps superfluous?

Ethnologists' technical focus does not allow them to venture out too far while dealing with this problem. On the other hand, we are faced with the question as to whether such issues can indeed be avoided

or even ignored. For theories of cultural development and change have always been elaborated in ethnology. Therefore, it is doubtlessly advisable to investigate such theories alongside their innate conclusiveness. Consideration should also be given, however, to existing approaches and results from other sciences which chiefly deal with such issues.

This is because the premise of man as a social being should under no circumstances become a dogma—such as in DURKHEIM's sociology—which a priori obliges the dialogue with other sciences to follow along a preconceived track. This kind of approaches preclude any open scientific inquiry into the nature of man and culture, together with the mechanisms of cultural development.

2. The relationship between biological and cultural development

The postulate of man's special position in nature results in the scientific question as to the very nature and emergence of such a position. This question is particularly important against the background of an evolutionary approach. Only if we are aware of the many little steps and findings in various sciences that are necessary to expand our knowledge in this field of research, it becomes clear to what extent we at times have to rely on suppositions. In an ethnological perspective, for example, it thus also seems appropriate to address the possibilities of interaction between biological and cultural development.

Therefore, the starting point in our observations is made up of the concepts of both evolution itself and of how the social and cultural sciences, alongside Evolutionary Epistemology, consider the relationship between evolution and cultural development. Our approach will for the present limit itself exclusively to specific aspects in the history and theory of science, as dealing with this issue on a larger scale would certainly exceed the scope of any single paper.

2.1 Evolution and cultural development in the social and cultural sciences

Taking a historical perspective, and in view of various social- and cultural-scientific approaches, three positions can be roughly determined. In the first position, considerations and theoretical approaches—as in the sociological theories mentioned above—primarily focus on cultural development in terms of sociocultural change and thus

largely deny the significance of exogenous factors, i.e. extrasocial causes (cf. LIPP 1968, p83). In turn, such non-consideration contains an indirect, yet clear statement regarding the present problem. It means, that it is only with regard to human social nature that man is an object of our scientific inquiry. In this regard, the relationship between cultural development and/or change and evolution fails to be taken into account due to the prevailing premise of man as a social being.

The second position pays attention to the question as to what cultural development really is, bringing it into connection with evolution, e.g. in terms of theoretical considerations of the tension between history and evolution. Originally, inquiries into regular sequences of cultural development were never free of the notion that something may be concealed behind social and cultural change: that is, directed development toward ever greater complexity or—in COMTE's and SPENCER's terms—a "succession of social conditions" (TJADEN 1972, p122).

Especially since the success of Social DARWINISM of the SPENCERIAN type, the concept of progress¹, in terms of development toward an ever more complex and complete state, has become part and parcel of most sociocultural theories of development or evolution. In the closing years of the 19th and at the beginning of the 20th century, the concept of sociocultural development was in surprising agreement among both biological and social-scientific approaches. Thus, ENGELS (1995) speaks of the methodological model function shown by DARWIN's Selection Theory in 19th-century biology.

For ethnology, a third theoretical standing was to prove more crucial: the various evolutionist theories of the turn of the century, along with the theory of multilinear development presented by the North American cultural anthropologist Julian STEWARD in the 1950s. The most frequent starting point in older theories of evolution had been unilinear development. Characteristically, such evolutionism conceived of uniform, i.e. unilinear development, linking this conception to the idea that the Western civilized world should be regarded as the standard for future development in all populations of the world. This was a significant constituent of the above-mentioned belief in progress, having gained importance from the times of SPENCER's Social DARWINISM onward. Toward the close of the 19th century, theories of evolution gave much prominence to the sequence of living and economic forms within the continuum of human history. Cultural development, for instance, was understood as a result of adaptation to

environmental conditions. However, one of the major drawbacks of these theories was their understanding, based on the above superficiality of approach, of present-day cultures as representing levels of development. Such levels were frequently established on the basis of "speculative" considerations, so that fragmentary data was conceptually drawn upon without investigating cultural surroundings.

On the other hand, STEWARD's theory of multilinear evolution (1955) showed that a theory of development that took evolutionary aspects into account could very well be accepted by a science devoted to empirical experience. On account of the empirical approach and limited scope of validity shown by STEWARD's theory, valuable considerations about the mechanisms and paths of development in human cultures were formulated in terms of adaptation to clearly defined environmental conditions. In this sense, an optimal connection between theoretical and empirical research was ensured. Incidentally, this example shows particularly well that useful (partial) results and serious approaches are only brought about by theoretical considerations and open debate of existing theories.

For ethnology today, there can be no doubt that previous evolutionist approaches should only be applied with great care and that the question must be reformulated in consideration of present-day multidisciplinary knowledge. Substantial progress has been made, for example, by at all realizing that in a theoretical perspective, there is no doing without a clarification and critical scrutiny of the relationship between biological evolution and cultural development (cf. MÜHLMANN 1952; WERNHART 1971, 1987).

If history were thought to be associated with the field of culture, and evolution with that of biology, then dealing with biological approaches in ethnology would represent one out of many chapters in the history of science. Yet if development is seen as the cultural continuation (of whatever kind) of an evolutionary process, and if it can be claimed that "evolution continues to maintain control of the functionality of *Homo sapiens* as a species" (RIEDL 1981; translation by MFCh), then the issue of the relationship between biological and cultural theories of development appears in a very new light.

2.2 Evolution and cultural development in the perspective of Evolutionary Epistemology

At an initial level, the issue of the relationship between evolution and cultural development is quite easily resolved in the perspective of biology

and Evolutionary Epistemology. For it is possible and even assumable—as indicated by OESER—that biological evolution endures today, albeit in a way imperceptible to man. However, this general statement fails to facilitate conclusions from the said upon the kind of relationship between the two domains. OESER continues by stating that: “The sociocultural evolution of man is thus a second kind of evolution, based upon—yet not representing the linear extension of—organic evolution” (1996, p244; translation by MFCh). For DARWIN as well, OESER emphasizes, the theory of evolution was “the foundation and starting point for a theory of human self-transcendence (“Selbstüberwindung”)

(1996, p244; translation by MFCh). Thus, beginning with a period of transition from animal to man, in which “progressive encephalization” is said to have taken place, DARWIN considered the history of man to represent a chain of sociocultural development which was by no means governed by genetic principles (cf. OESER 1996, p246f). Only in this regard, it is possible to speak of evolution at different levels and of the continuation of evolution as a biological process within a sociocultural setting.

In classical Evolutionary Epistemology, certain insights are maintained that have an impact on our considerations:

a) The conception of biological evolution itself as the result of a historical development (LORENZ 1992, p161f). This simple statement ensues from year-long research aimed at reconstructing “the features and laws of the singular, phylogenetic becoming of living beings” (1992, p208; translation by MFCh);

b) The conception of sociocultural development in terms of—whatever kind of—continuation of biological development: As opposed to what is often assumed, the starting point in this relationship is not direct continuity between biological evolution and cultural development but rather the emergence of a new phenomenon. In this view, LORENZ (1977, p167) related to the “uniqueness of man” and rejected any reductionist perspective, which “clings to the fiction of the continuity of the evolutionary process, believing that it can only produce differences of degree”. Furthermore, he noticed that “every step in evolution causes a change not only of degree but also of essence”. In this respect, LORENZ considered “the life of the human mind” as “a new kind of life” (LORENZ 1977, p172);

c) The conception of cognitive development in terms of a special, autonomous continuation of cultural development. This refers less to LORENZ’ “fulguration of the human mind” than to a special case of

continued development of human mind—i.e. the progress of sciences as further-reaching development according to autonomous regularities—and increasing cognitive competence. However, this third issue is still very much disputed within Evolutionary Epistemology—and was incidentally elaborated in particular by POPPER, resulting in a specific discussion between him and LORENZ, alongside other scientists in this field of research (cf. VOLLMER 1995, p133ff; CALLEBAUT 1996, p128).

While much emphasis is given in French science to the aspect of autonomous development of man as a social being (GUILLE-ESCURRET 1994), there are many approaches in the English and Anglo-American literature attempting to explain the broad field of transition between animal and man. The problem has in the last years been approached by seeking the connection between the social nature of man and his cultural being. This has been a continuous part of the debate, as exemplified by CARRITHER (1992), BARKOW/COSMIDES/TOOBY (1992) and DEACON (1997). The standing of Evolutionary Psychology has now been crucial to understand this approach: “The central premise of the Adopted Mind is that there is a universal human nature, but that this universality exists primarily at the level of psychological mechanisms, not of expressed cultural behaviors (BARKOW/COSMIDES/TOOBY 1992, p5). It is argued that “much of human intelligence is social intelligence, the product of selection for success in social competition” (1992, p628), considering processes of adaptation—ascertained with the help of biology on the one hand and psychology on the other—to the ancient living conditions of Pleistocene hunters and gatherers as the foundations for interpreting present-day patterns of behavior. In this connection, it is stated that “Freed from the Standard Social Science Model’s stricture to locate all causes outside of psychology, modern approaches to culture can explore the ancient psychological mechanisms that underlie and explain recently emerging cultural phenomena”. This approach is similar to that outlined above, the psychological aspects, however, being interlarded between biological and cultural explanations as an additional interpretive option. Here, we are again confronted with the fact that this is an interdisciplinary field of research in which no science alone may offer a solution. Another arising question relates to the degree of integratability shown by the theory of evolution after DARWIN. As aptly indicated by VOLLMER (1995, p59), this question will require a pronounced answer, since the DARWINIAN theory of evolution—a theory of the

evolution of living beings which can certainly be extended to other fields—does not represent a “universal theory of evolution”. In this regard, VOLLMER has commented that “It is the concept of evolution, not the available theory of evolution, which is universally applicable” (1995, p59; translation by MFCh).

2.3 Analogy as a cognitive tool: between biology and culture

2.3.1 Concepts of analogy and the “biologization of society”. As outlined above, evolutionary and developmental approaches are not unfamiliar to ethnology as a science that itself theoretically and practically deals with cultural diversity, specificity and change. Despite this, many theories holding such a claim have in the past failed on account of their largely speculative considerations. At the turn of the last century, research in human development, be it biological or sociocultural, was the prevailing paradigm in the “human sciences”. At once, a concept of sociocultural development evolved—under the influence of DARWIN and, in particular, SPENCER’s theory of development—which was in surprising agreement in approaches of both biology and the social sciences (cf. ENGELS 1995).

From the very beginning, evolutionary observations had been present in ethnology along two different, characteristic patterns: First, the search for levels of development, leading to the large-scale theories elaborated by BACHOFEN, MORGAN and other evolutionists; and second, thinking in analogies, making it possible to compare biological and cultural phenomena and integrate such phenomena into a broader theoretical framework.

As indicated by OESER, this kind of analogy is located at an “initial, ‘superficial’ and thus largely unsecured, yet heuristically precious level of comparison” between biological and cultural phenomena (1996, p214; translation by MFCh; cf. LORENZ 1974). OESER, referring to the initial level of analogy formation, speaks of “metaphorical similarity” (1996, p214).

The image of society as an organism is one of the most widely acknowledged examples of such an analogy. This analogy was already in use by SPENCER who started out from the “growth” of society resembling organic growth (cf. OESER 1996, p240). Some ethnologists also made use of this image. LEROI-GOURHAN, for one, did not consider himself an evolutionist, yet his is definitely an approach from

within a specific evolutionary perspective upon society. This becomes evident in his attempt to sketch the most complete possible reconstruction of human development from prehistoric to present-day man. Moreover, LEROI-GOURHAN also intended to identify general lines of development of societies. The starting point of his scientific interest was the technological equipment of populations throughout the world, comparing the respective states of technology they had arrived at. Although he avoids the term “progression” because of its evolutionist tinge (1971, p39), the concept of “state of technology” is equated with that of technical level in purely evolutionary terms. The mechanisms of surmounting one specific “state of technology” and passing over into another represent a core issue in LEROI-GOURHAN’s conception of technology. With regard to technology, his considerations reflect upon the concept of “milieu”, distinguishing—like DURKHEIM—an internal milieu (society itself) from an external milieu (environmental conditions and other societies). The technical milieu, one of the constituents of the internal milieu, becomes very important in this connection. This milieu is the point where the internal milieu comes into contact with the external milieu. Both milieus bear clear traces of this contact and mutual penetration (cf. 1973, p333). LEROI-GOURHAN conceives the technical milieu as a kind of projection of the external and internal milieus into social reality, a quite faithful description of the phenomenon of adaptation in a biological sense of the word. In this connection, LEROI-GOURHAN regards the objects of material culture as resulting from the contact between internal and external milieus. Such objects play a central role in human environmental perception due to their intermediate relationship to the two milieus, and man in the end is only able to experience the environment through the objects of his own respective culture (cf. CHEVRON 1998 on this aspect).

Yet LEROI-GOURHAN moves way beyond this step by comparing objects created by man with the specialized organs of animals and plants, the outcome of slow processes of adaptation to the environment, as to their functions: “In nature, human groups behave like living organisms; similar to animals and plants, which cannot directly appropriate the products of nature, since they depend on the function of organs processing various basic substances, human groups appropriate their environment by means of a ‘curtain’ of objects (tools and devices)” (1973, p321, quoted in CHEVRON 1998; translation by MFCh). LEROI-GOURHAN basically considers the tech-

nical milieu as a continuous sectional area or field of contact between the internal and external milieu. This consideration is given primary importance in his concept of society in terms of organisms engaged in a continual process of adaptation and developing ever-new technomorphological features (cf. LEROI-GOURHAN 1973, p332f).

The present paper has considered the superficial application of analogy in detail because it is in frequent use in the social and cultural sciences, as well as biology. However, the issue becomes more intricate in cases in which analogy can indeed be clearly recognized as analogy, while the way the applied terms are theoretically embedded conceals the relationships between various levels of comparison.

The theory of autopoiesis in LUHMANN's view is such an example which largely builds upon an analogy from biology. In this case, a theory is adopted from biology and transferred to another framework. This is the typical case of analogy: It is not the image of adaptation to the environment that comes to be applied, but rather that of the self-organization of a system in parallel to the self-organization showed by organic systems, i.e. biological organisms. LUHMANN has been deemed one of the main advocates of autopoiesis in sociology. He was correctly reproached, however, for merely borrowing the relevant concepts from biology and incorporating them into new conceptual connections without reflecting upon the deeper meaning of comparison (LIPP 1987, p453).

LIPP has raised a question in connection with LUHMANN's approach, which directly touches on our issue: "Are the categorical constituents—and structures according to which they are linked—maintained in detail in the process of translation, such that the cognitive profits originally warranted by the concepts are perpetuated into new generational levels?" (1987, p453; translation by MFCh). This question expresses exactly what analogy is in classical terms: A relationship between two phenomena is transferred from one field into another (cf. REMANE 1971, p216). To pose this question, however, also implies to disclose the ways in which analogies can at times be manipulated. LUHMANN transmitted the biological concept of autopoiesis in VARELA's and MATUREANA's classical sense and the related problem of self-organization with reference to systemically proceeding biological processes (LIPP 1987, p457) into sociology. This does not primarily address the relationship between organisms and the environment, yet consideration is given to the processes taking place between systems and the envi-

ronment, "which—in quite DARWINIAN terms—appear as selection, adaptation and evolution" (1987, p457; translation by MFCh). It is shown in the case of autopoiesis as conceived by sociology that transmitting the theory of autopoiesis does not function that smoothly. For instance, determining an entirety and its parts in a given social and/or cultural system proves far more complicated than in a biological system (1987, p461). In forming analogies, in particular, this difficulty to determine the levels and delimitation of areas often escapes notice. Thus, constant shifts between levels result in a misleading usage of concepts. As amply demonstrated by LIPP with the example of selection, these basic concepts of the theory of evolution—against which a theory of autopoiesis in a sociological sense would need to prove its value according to its own claims—are linked to other contexts (1987, p462f).

Finally, it should be borne in mind that, at an early date, various writers in biology made use of analogies between cultural and biological phenomena. For instance, recall the comparison put forth by VIRCHOW in 1855 or Ernst HAECKEL in 1904 between citizens of a society and the cells of an organism (cf. SANDMANN 1995, p331). SANDMANN has indicated the issue alluded to here: "While Virchow and other authors do recognize the relativity of this analogy, HAECKEL rejects it on account of his monistic conception of reality" (1995, p331; translation by MFCh). However, SANDMANN fails to see the fact that HAECKEL not only absolutized but also inverted the analogy along the lines of which a mechanism of social coexistence is applied to interpret a biological phenomenon. Again, "biologization of society" was the final result in this case.

One basic problem that prevails is that analogies are not designated as such and that the demands made on such analogies are seldom expressed clearly. Rather, the impression is given in most cases that we are dealing with "authentic" analogies, i.e. equality of relationships in the classical sense of the word (cf. REMANE 1971, p214). This suggestive use of analogy does more harm than elucidate connections. The process is reminiscent of KOZENY's notion of "psychological process of conceptualization" which points out the subjective attributions and categorizations involved in the development of prejudice (1997, p50ff). Thus, such approaches are rather counterproductive in terms of establishing knowledge. They are not useful with regard to the claim to consider biological approaches in the social and cultural sciences, since they purport to deal with certain aspects while in fact simply neutralizing them.

2.3.2 “Analogy as a source of knowledge” in science. However, the significance of analogy is not limited to its abuse, and it does not suffice to simply indicate the limits of such an instrument without demonstrating genuine options of applicability. In a theoretical perspective, therefore, we must round off the problem by adding some supplementary observations.

Comparatively applying analogy to determine the causes of similarities, biology is a discipline in which analogy has developed to be a major methodological tool. In this framework, analogy—“as the form of similarity that develops on the basis of independent adaptation to identical environmental conditions” (RIEDL 1981, p209)—is seen in contrast with homology—“as the form of biological similarity that is established by the identical regularity of epigenetic systems” (1981, p211; translation by MFCh). In the case of analogy, similarity is attributable to identical external conditions in terms of the functional adaptation of an organism or organ to such conditions. In the case of homology, on the other hand, the cause of similarity is to be identified within the system itself (1981, p211).

Yet regardless of this technical understanding of the concept of analogy, “analogy as a source of knowledge” (LORENZ 1974) has been more closely investigated in terms of its validity as a theoretical tool. Relatively few scientists have attended to this issue, although it is a fundamental problem in connection with the basic conditions and mechanisms that govern the generation of knowledge. Of course, this fact is chiefly problematic since practically every scientist makes use of analogy. Analogy is particularly significant in the initial, purely intuitive phases of research, i.e. the setting up of hypotheses. This form of analogy would fit into OESER's second type—following Beer's systematization—of analogy formation. After the metaphor, this kind of analogy was the second form of analogy that proved itself as genuine, or as LORENZ commented: “However, no such thing as a false analogy exists: an analogy can be more or less detailed and hence more or less informative” (LORENZ 1974, p186).

A typical and particularly suitable example for such a process is the development of the concept of natural selection which is one of the corner stones of DARWIN's Descendence Theory. For DARWIN, professional selective breeding represented the starting point for the question as to whether this mechanism may function in nature as well (TORT 1992, p28). The comparison between artificial and natural selection is appropriate because the former to some extent ex-

perimentally confirms the hypothesis of natural selection (cf. OESER 1996, p105). Here, the formed analogy does not only lead to a primary thinking process and thus facilitates an initial superficial comparison. There is also a deeper-lying reason for the comparison: Human beings tend to use the natural variability of organisms in the case of selective breeding to their own advantage. Selected out by man, such an advantage suggests to see the same phenomenon in both processes, although it may once take place to the advantage of the given organisms and otherwise to that of man (cf. TORT 1997, p33f).

The example outlined above presents a relatively simple—scientifically straight-lined—explanation of a complex phenomenon. Explanations are less overt and thus more complicated in terms of comparing the spheres of biology and culture, especially—and frequently—if they are located between science and ideology. This is because genuine problems develop less in connection with analogy formation but rather with the interpretation of analogy. As observed by RIEDL (1981, p132f), “analogy as a source of knowledge” is thought little of by scientists who rather confuse it with a kind of pseudo-knowledge. This is not primarily because of analogy itself but rather due to its interpretation, i.e. the act of determining the reasons for actual similarity.

On account of their inherent latitude, analogies can become vehicles for other contents, since they can be easily associated with other ideas at a second level. In any case, it appears befitting to refer to this latitude, since false associations may very well gain quick access and, under certain circumstances, become more powerful than the original scientific idea that was meant to be illustrated and disseminated. This problem indicates a second level: the level of RIEDL's “second-degree analogisms” (1981, p132). The first step of analogy formation applies to a kind of hypothesis about the world, starting out from the notion that “similar things admit of anticipation as to further similarities” (RIEDL 1981, p93; translation by MFCh), and thus rendering it possible to perceive and interpret the world. However, it is imperative to form a second hypothesis to interpret the very similarity perceived and represented.

Yet the use of analogy in an interspace between biological and sociocultural phenomena sets many snares which let the above problem appear even more intricate and complex. The difficulty in analyzing the degree of validity held by analogy stems from the fact that not only can the compared features and complexes of features be located at different levels of comparison. Rather, other elements

such as the *Zeitgeist* or ideology must be given due consideration. Thus, for example, we can take the interaction between SPENCER'S Social DARWINISM and DARWIN'S Descendence Theory (cf. BOWLER 1995, p309ff).

Such interaction between science and ideology is increasingly coming to the fore in natural-scientific debates on the formation of theories (cf. MATALON 1998). The "ideological dimension of DARWINISM" has often been neglected, although this very area of complex interaction between ideology and theorization set up the foundation for popularizing this scientific idea. It was SPENCER who first employed the concept of evolution in terms of biological and cultural development, acknowledging "a manifestation of a universal trend toward progress" (BOWLER 1995, p313; translation by MFCh). Comparisons between DARWIN'S and SPENCER'S approaches to the problem of evolution make it clear to what large extent analogies merely represent cognitive tools, while their adequate and useful application finally depends on the theoretical construction in which they are embedded. At an early date, SPENCER'S Social DARWINISM made itself felt as a social philosophy showing strongly ideological components. On the other hand, the fact that DARWIN'S thought was heavily influenced by certain notions prevailing in those days did no further harm to his theorization. This is because although DARWIN was inspired by such ideas, and on account of his concrete observations and interpretations of "experiments", he was also able to detach himself from those notions in the course of further-reaching scientific work. Of course, the theory of science should draw attention to this kind of basic problems of knowledge generation, yet it is imperative that individual specialized disciplines, p192ff).

This paper will not deal with the topic in all its facets. Rather, our intention is to indicate how this fundamental problem of scientific research has failed to attract sufficient consideration, notwithstanding some notable exceptions. To some degree, furthermore, it still represents a basic difficulty of interdisciplinary work in a certain field of research.

Finally, it should be noted—because it represents an additional difficulty in research and is often a reason for misunderstandings between scientists—that analogy is a philosophical and methodological instrument which is not alien to the sciences of culture. In these sciences as well as others, a more precise application of such instruments of conceptualization and representation seems appropriate.

This refers to scientists' professional ethics, as self-deceit and ideological conviction may all too easily dull our perspective. This aspect is a considerable concern in the present field of research, and even though there may be no such thing as false analogy (LORENZ 1973), there are certainly misleading analogies.

Analogy has proven useful in the intuitive process of conceptualization, yet it seems advisable to closely consider how it can be put to precise use to explain and elucidate specific processes and phenomena. Unfortunately, it is to be expected that analogies maintain a stronger hold than the scientific facts themselves.

3. Cultural evolution and biological development

It has so far been the objective of this paper to indicate basic difficulties and sources of mistakes in the interdisciplinary study of the relationship between biological evolution and cultural development, as well as to emphasize the basic necessity and usefulness of such research. From a cultural science perspective, important roles are played by both considerations in the history of science and methodology and interdisciplinary investigations about individual issues. In addition, it seems absolutely essential in the cultural sciences to investigate the ongoing theoretical discourse in biology and Evolutionary Epistemology in order to better evaluate the implications of recent research results and integrate them into their own theoretical considerations. However, this also applies to biologists who intend to incorporate the phenomenon of cultural development into their analyses and who thus must become familiar with the approaches of cultural sciences and their inherent logic. It is crucial to know not only others' limits and frailties but also one's own, in addition to the full scope of current discussion in alien disciplines, in order to truly comprehend the respective issue on hand. This is because interdisciplinary work can never consist in arbitrarily taking elements of another science out of their context.

As to content-relevant discussion, it has long been clear to many researchers who deal with this issue that the strict distinction between evolution as a biological process and development as a cultural process cannot go without the slightest contradiction. Yet frequently, the two fields are still either dogmatically separated or otherwise uncritically confused.

3.1 The problem of universality of the biological theory of evolution

A relatively frequent mistake in the theory of evolution has been the attempt to—literally—transfer the DARWINIAN theory of evolution to other than purely biological events. Awareness has not been sharp that although the concept of evolution is claimed to be universally valid, such validity has yet to be proven with regard to the theory of evolution (cf. VOLLMER 1995, p59). This naturally does not rule out that evolution can be the major issue in various fields paying attention to certain regularities and law-governed phenomena within a developmental event.

This fact is substantial in a biological perspective, since it implies that the theory of evolution perceive “the necessity to include non-DARWINIAN evolutionary factors” (VOLLMER 1995, p59; translation by MFCh) in order to faithfully address all the effective components to be considered.

At once, it seems appropriate to think about the large integrative power emanating from the theory of evolution in terms of a “pure theory that is universal and biological” (OESER 1996, p145f), i.e. to reflect upon the need to deal with basic concepts such as selection and adaptation (OESER 1996). BLUTE already referred to this problem two decades ago (1979, p46f). Rejecting any significance in approaches deriving from a purely biological theory of evolution for the cultural and social sciences, this author nevertheless considered cultural-science inquiries into biological approaches to be a rewarding endeavor. The fact that thought is given to the mechanisms of analogy formation, and thus the reasons for such parallel ways of looking at the issue, can be very productive in an epistemological perspective.

For some time now, a discussion has been under way in biology, which has concentrated on the issue of interaction between evolution and development (cf. WIESER 1994, p35f). With regard to evolution as a historical process, LORENZ argued that “the fact of the development of both human beings and oaks is merely the outcome of a very specific, unique historical event” (1992, p162; translation by MFCh). In view of its implications, this simple remark is highly significant in theoretical terms as it serves to clarify the connection between becoming and having become, between biological systematics and the theory of evolution: “Every question as to the causes of ‘being this and nothing else’ in a given organism’s structure and performance is responded to with an analogous, basically historical answer: All causal

analysis of structure, as well as performance in all living beings, is conclusively and unconditionally dependent on phylogenetic inquiries into these features’ development” (1992, p163; translation by MFCh).

More recent progress in biology has also shown that attention is paid in Evolutionary Developmental Biology to the importance of historical processes—in terms of an analogous connection between such processes and evolution as well as the possible interaction between the two fields. Therefore, and amending the classical synthetic theory of evolution, solutions are sought not only “in locally specific genetic expressions but especially in the epigenetic dynamics of developmental system” (MÜLLER 1994, p185; cf. WIESER 1994, p35ff; translation by MFCh). A call is made—and research is ongoing—in this framework for a “synthesis of the theory of development and the theory of evolution” (MÜLLER 1994, p185; translation by MFCh). Deriving exclusively from Evolutionary Biology, this insight is still particularly interesting if the complexity of such phenomena is to be given emphasis.

3.2 Culture as a long-past process of learning

The above considerations have argued that we must first and foremost address the question as to how interaction between biological evolution and cultural development takes place—in cultural sciences generally and ethnology specifically. Or perhaps more correctly: What section of this phenomenon do we at all perceive? Apart from the basic question as to how important the theory of evolution is for cultural development, the problem of the kind of relationships and interaction between the different levels of observation still remains unresolved.

Addressed by MÜHLMANN in 1952, the connection between history and evolution is far from being disclosed, and individual researchers depend increasingly on interdisciplinary cooperation to which they must remain susceptible. In this regard, a purely cultural-scientific perspective is as little constructive as an approach deriving solely from Evolutionary Biology. Thus, it is an indispensable precondition to consider human, i.e. sociocultural evolution as an outcome of phylogenetic adaptation leading to specific human aptitudes. Cultural scientists participating in serious and profound human research must beware not to disregard this kind of development—which unfortunately often is the case. As indicated elsewhere (CHEVRON 1998, p38f), it would appear useful and profitable to integrate results from a science

such as ethology as “the more basic field of knowledge” (OESER 1996, p183). That means that we have to include biological knowledge in relation to our behavior and the cognitive apparatus (a priori notions and categories of thought) for the benefit of research into culture and society.

For ethnology, a science phenomenologically investigating all cultures as equal (historical) entities, the question as to the status of evolutionary observations arises, against the empirical background of cultural comparison, in a way that is different from other sciences. It is generally agreed in ethnology that it would prove inadequate to raise this question along the lines of the old theories of evolution—regardless of the fact that dealing with such approaches is in fact advisable. This problem can and should be addressed anew on the basis of results and insights gathered by other sciences.

The major difficulty—perhaps a unique opportunity for ethnological research—is that we are confronted with empirical facts which are as yet alien to the constructs of Western science. This allows to more easily filter off cultural bias from the theoretical approaches, while making the ethnological access to knowledge an ideal pilot light for a critical, not merely receptive theory and history of science. Thus, it has been possible at least since TYLOR’s criticism of sources (1871)—and his examination of data both collected on one’s own and taken over from other researchers—to observe and recognize cultural and ideological bias. For instance, such cultural bias includes belief in incessant progress, which gained access to all fields of evolutionism, i.e. the theory of unilinear development.

In this connection, a number of ethnicities all over the world were classified as representing previous levels of development—regardless of the fact that these groups represented contemporary cultures. By 1895, SPENCER was not the first scholar to write that “the mental characteristics of the uncivilized ... are features that are present in the children of civilized peoples” (quoted by OESER 1987, p145; translation by MFCh). Likewise, DARWIN considered the Fuegians to be prehistorical ancestors (cf. OESER 1987). The problem attached to the primitiveness of peoples as a constituent of evolutionism is one of the notions most frequently handed down from ethnology. Certain definitions long discarded by ethnology were occasionally adopted by other sciences, and the image of

“Stone Age men” living in the most remote parts of the world certainly remains more strongly present in public conscience than any single concrete report on those ethnicities’ actual ways of living. This topic will be detailed elsewhere, yet it is already clear that such notions regarding primitivism as an adopted feature of human cultures must be seen as remnants of old evolutionism. They have been handed down as persistently as the dichotomy of innateness and acquiredness, long considered as refuted in biology, has at times been uncritically repeated in the cultural and social sciences. At the same time, much more nuanced comments are seldom, if at all, taken note of.

In cultural and social sciences, traditional theories of cultural development are rarely considered in a higher-level perspective. Theories of cultural change have led to completely divergent results, depending on whether they were set up from a social- or cultural-science perspective and in accordance with the respectively other premises. The decisive fact in this regard was that exogenous factors were indeed considered in one case, while being utterly disregarded in another, such as in autopoiesis deriving from system theory. In yet other theories, development was regarded as the cultural continuation of an evolutionary process. Some writers focused on the transition from nature to culture and from anthropogenesis to higher levels of development. From the start, however, this phylogenetic perspective was heavily biased by certain premises, such as the above-mentioned arbitrary definition of developmental levels according to unclear criteria or the equation of present-day populations with children or Stone Age people.

This problem is much more closely approximated by approaches in search of transcultural universals of human or cultural ethology and the “ratiomorphic apparatus” as outlined by BRUNSWICK (1955). Be it defined as “primitive” or not, the focus here is on what is generally human as well as what are identical phenomena in different cultures of the world. This is seen by some researchers as one of the substantial operational directions in ethnology, as it “seeks to recognize common grounds and repetitions in all cultures ... the same generally human basis, i.e. such universals” (SCHUSTER 1978, p547; translation by MFCh). This does not refer to the universal historical perspective in the old sense of the word. Rather, the question concentrates on

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certain common foundations and regularities of cultural development—a more constructive objective, for sure.

WERNHART'S concept of "Universalia humana et cultura" (1987, p19) is one ethnological endeavor to interpret basic universal patterns of human existence along with their specific cultural superstructures. Reacting to the "universal cultural insights" gathered by cultural history (WERNHART 1984, p66), the Vienna school of historical ethnology had an early-date critical look at the universals postulated by human and cultural ethology (cf. HIRSCHBERG 1976; WERNHART 1987). The objective has not only been to investigate the biological foundations of actually emerging common properties among various populations

but also to explain cultural differences and historically evidenced development. It is interesting that this interdisciplinary and now strongly ramified field of research bears nuanced comments on the problems connected with the issue of universals, while some issues are readressed in a novel perspective.

At this level, it has nevertheless become obvious that the (basic or initial) conditions of sociocultural development are given by evolution. Whatever individual implications may derive from the theory of "human self-transcendence ("Selbstüberwindung")" (OESER 1996), this field of research, rich in hypotheses, represents the most exciting challenge for today's pure interdisciplinary research on man as a cultural being.

Note

- 1 BOWLER (1995, pp309–325), referring to "Social LAMARCKISM", has precisely documented the significance of belief in progress in DARWIN'S Theory of Evolution.

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Evolution, Categorization and Values

1. Introduction and overview

The present paper provides an evolutionary framework for discussing some fundamental features of perception and cognition and tries to point out a number of current controversies in cognitive science that can be resolved by adopting this perspective. The main questions are: What can provide a firm ground for a theory of categorization, if we insert it into an evolutionary framework? Is it enough to build a theory of categorization on sensory input alone, and what place does “reality” have in such a theory?

An evolutionary perspective on categorization¹ provides natural constraints on what categorization can be like. First and foremost, evolutionary theories need some form of *evaluation mechanism*—categorization has to be *about* something for evolution to work, for example *about* finding food or avoiding danger. These evaluation mechanisms I have called values.² The organism needs substances with *food value* for its survival, needs to find a partner with *reproduction value* to produce offspring, must protect itself from predators with *destruction value*. These are *objective* constraints that all living systems have respected throughout their evolutionary history. One of the major themes in this paper is to present values as the driving force of categorization (section 2).

Thus, categorization is built upon cognitive and sensory processes, but is *about*, for example, finding food, which is not used for feeding cognition, but for

Abstract

The aim of this paper is to present an evolutionary framework for categorization. Evolution needs an evaluation mechanism to work, and it is argued that primary values that the organism needs for its survival—such as food, mates for reproduction, and shelter—can drive the evolution of categories. Sensory stimulation is needed to build up the cognitive apparatus, but cannot in itself provide the evaluation mechanism for evolution. Categorization constrained by values will be dependent on the availability of sensory information, and its power as predictive of values. As perception and categorization are tied to the actions of the organism, it is argued that the unit of perception should be seen as larger than the usual single-dimension stimulus, and evidence is reviewed to support this claim. Covarying stimuli will also provide a much greater predictive power than single-dimension stimuli alone.

Key words

Categorization, cognitive processes vs. life processes, affordances, covariation, evolutionary essentialism.

feeding the body. The value systems constitute the *life processes*. Categorization is *about* keeping the creature alive. Eating is not primarily a cognitive process, with the aim of stimulating the senses in the digestive system, but consists of uniting substances possessing food value with our body to maintain the homeostasis. See figure 1.³

These physiological processes are (rightly) taken for granted in the literature of psychology and philosophy, but the distinction between cognitive processes and life processes cannot be overlooked in an evolutionary account, as the life processes constitute the mechanism of evaluation of the evolutionary process.

One of the main themes of this paper is to account for the connection between cognitive processes and life processes, or, as I call it, *sense domains* and *value domains*.

Consider, as an example, the two meanings of the word *hurt*. It refers both to the *sensation of pain*, and to the *physiological process of injury*.⁴ Thus, there is a close connection in our minds between sense processes and value processes. And the only means we have of escaping injury is to escape the pain! How would we be able to protect ourselves if the sensory impulses did not correctly predict the values? In section 2.1, I examine the correspondence between sense domains and value domains.

There are some other nontrivial consequences of considering the closed loop of the life processes. Like any other cognitive feat, categorization must for example respect the time limits set up by the survival

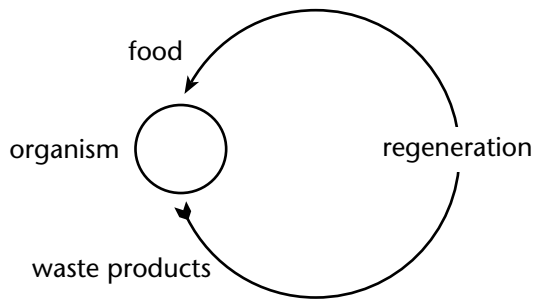


Figure 1: The value loop for substances with food value.

of the organism. Thus, categorization can be seen as a trade-off between the availability of information and its predictive power. See section 2.2.

Another consequence is that all categorization is seen as embodied and situated. The cognitive functions are seen in relation to the functions of the organism, in a context. The actions of the organism always take place in a multidimensional environment. In this environment, a multitude of information is available for use as sensory information, whether or not it is used by the organism. The bacteria in the Petri dish on my desk have for example access to the same potential of sensory information as I have. They could look at me as I look at them, but they don't.

This condition, that all organisms are surrounded by a vast potential of sensory information, is used to challenge the common view that categorization is built up from inferences in *single dimensions*. Rather, in section 3, I argue that the basic units of categorization are complexes of *covarying properties*. The unit of perception will thereby lie closer to the unit of interaction.

The phenomenon of categorization is ascribed in the literature to a vast range of organisms, from protozoa to humans. Informally, it may be described as follows: "Given the motivational state of the organism, it has to find useful situations with food, mates and shelter, and must avoid danger, such as enemies, cliffs or excessive sunlight. In each situation, there is a choice to be made as to how to proceed, and this choice represents the categorization of the organism."⁵

There are of course several ways of performing categorization. Finding my way out of a house of horrors, I can proceed by reasoning to work out where I came in, following the sensory information in the trails in the dust or "blindly" reacting to the weak daylight coming through the shuttered windows. A dog could use the trails and the daylight. An amoeba only the daylight.

In most theories of human categorization, it is said to serve *reasoning*. (See KOMATSU (1992) for a review.) In principle, there is nothing wrong in assuming that the main function of *human* categorization and concept formation is to serve reasoning. But if we want to attain a deeper understanding of fundamental human cognition, if we want to understand the connection between animal and human cognition, or build artificial systems with categorization capabilities, then categorization must be based on something that can support evolutionary constraints, and does not rely on the advanced abilities of humans, notably language.

According to some traditions, humans use linguistic faculties for all forms of cognitive processing—the so-called Language of Thought (FODOR/PYLYSHYN 1988). I have chosen not to follow this tradition, but rather to find the foundations of categorization in nonlinguistic cognition. This will provide the common ground for human and nonhuman categorization that an evolutionary approach will need.

Although I devote this paper to aspects of categorization that are general to all organisms, there are a number of important features of *human* cognition that are necessary to remember as uniquely human when discussing categorization: peeling off the cognitive characteristics of language will uncover underlying similarities between animal and human cognition. In this paper, I will limit myself to laying a stable ground for categorization in the value systems that are common to all organisms.

2. Values, senses and brains

In this section I go deeper into the concept of values and show its relation with perception and cognition.⁶ The method I use is akin to those of JENNINGS (1906) and von UEXKÜLL (1909/1985; 1982). I construct a world with some basic properties, and this will provide the starting point for the discussion of some themes of perception and cognition.

The world I assume has to be stable to some extent. At least, the aspects of the world that are captured by cognitive generalizations *have to be* stable, otherwise there would be no generalizations to capture! The organisms inhabiting the world—descendants of the Vehicles of BRAITENBERG (1984), and the Berry creatures of GULZ (1991) and BALKENIUS (1995)—are theoretical creatures, but exhibiting many behaviors common to living organisms.⁷

All creatures have to comply with the restrictions imposed by the closed loop of survival: intake of food, reproduction, homeostasis of temperature and

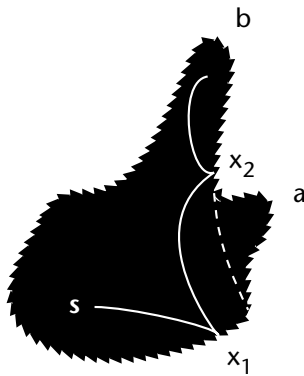


Figure 2: The organism starts at s , “hits reality” at x_1 and x_2 .

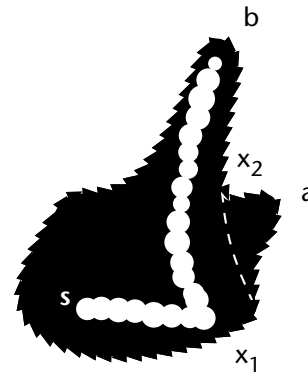


Figure 3: The mental map of a timid organism. The white area represents the subjective safe zone.

other conditions of life, such as oxygen content, air pressure etc. For the lowest organisms, the sensory requirements are at a minimum: for bacteria in a heap of dung, no senses or sensory information are needed to find the food—it is always there. In higher organisms a radical shift has taken place due to symmetry breaking (STEWART/GOLUBITSKY 1992). We eat at regular *intervals*, rather than continuously, and as long as the intervals are not greater than the loop admits, we have the freedom to leave the food and do other things. But then we will need sensory organs to find the food again.

The world we move around in is a complex world. The theoretical analogy that I want to use is that of a *cave* that is not known to us in advance. We move around in the cave, and as long as we keep clear of the walls, nothing prevents our exploration. This world is *objective* in an ontological sense. It poses *constraints* on us that we have to comply with (STEWART 1996). I have illustrated this in figure 2.⁸

The black area in the figure represents the world, in terms of objective possibilities: wherever the creature tries to go, inside the area, there will be nothing (in these two dimensions) that will stop it. The hinder that the “end of the world” represents has nothing to do with sensory stimulation. It is purely a matter of physical constraints.

The movement does not have to be restricted to *spatial* movement. Also other aspects of our experience can be represented dimensionally (GÄRDENFORS 1996). When we look for something to eat, we explore an area of potential food that extends over several dimensions. Some food is beneficial to us and will continue to be part of our world. Some food is dangerous and will stop our exploration.

The values that substances have will vary depending on species. This does not alter the physical char-

acteristics of the substances, however, only their relation to the organism, and what aspects the organisms are likely to include in their mental representation. Thus, I argue that the substances with value do not in themselves need to be mentally represented. Rather, mental representation is concerned with strategies for finding and avoiding these substances.

As an illustration of the difference between sense domains and value domains, please put your hand on your desk. Push downwards. Further. When you sense the pain, ignore it and push harder. Did you get through? No. Again, we take for granted the association of the sense dimensions and what I have called the value dimensions—what prevents your hand from passing through the table is not the pain, it is the physical characteristics of the table, which correspond to the edges of the black world in the figures.

The organism in figure 2 starts its trajectory at s . When it comes to x_1 it “hits reality.” The knowledge gained from this experience depends on the sensory apparatus of the creature. The one in the figure seems to have noticed the wall, but couldn’t predict it. It changes direction, hits the “wall” again at x_2 , and we leave it for the moment.

Irrespective of the objective layout of the world, the creature will, if it is complex enough, try to establish its own subjective map showing which areas are allowed. A system of inductive heuristics will allow the creature to extend its mental map based on its experience, as pictured in figure 3. The white area represents the parts of the space that the animal will treat *as if* it were safe.

Inside the white areas the animal is less likely to check carefully. It can switch from the slower attended, context-dependent mode of processing re-

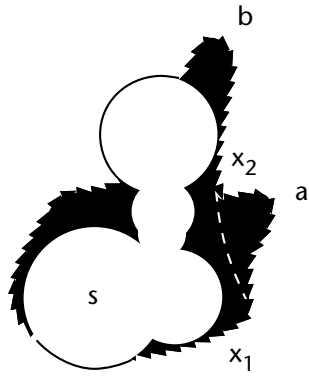


Figure 4: The white space represents the mental map of a bold organism.

quired when the informational predictability is low, to automated processing which is more rapid and error-free and can be performed in parallel with other tasks (GIVÓN 1989, ch. 7).

We do not so far have to assume anything about the perception of the organism, it could create a representation from dead reckoning (GALLISTEL 1990), based on the trajectory. But it could also use, for example, landmarks or smell gradients (BALKENIUS 1995; 1996).

What says, then, that the creature should delimit its expected subjective harmful zone to a small area around the experienced trajectory? Figure 4 shows the mental map of an “epistemically bold” creature.

Here, the creature still avoids the two places x_1 and x_2 of the “reality encounters.” (This is the creature’s response to the stability requirement.⁹) In this case, the creature has drawn too optimistic inferences that have extended *beyond* the limits of the objectively “possible.” However, as long as it does not try these possibilities, it will never get the negative feedback from reality, and will not have to revise its inferences.¹⁰ Furthermore, the bold creature has the advantage of inductively knowing a far greater portion of the allowed space than has the timid creature, and this might counterbalance the negative effects of over-generalization.¹¹ As I have tried to illustrate by the number of intersecting circles, the creature also gains *simplicity* in the representation.¹²

2.1 “Direct perception” and the function of the senses

I would like to compare my approach above with the theory of affordances of GIBSON (1979). A succinct characterization is given by NEISSER (1987): “Affordances, as J. J. GIBSON (1979) defined them, are

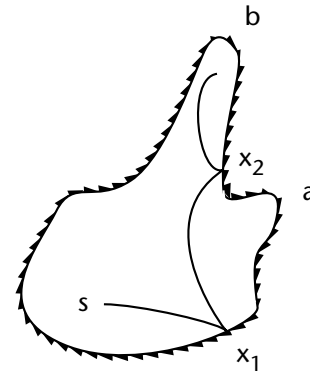


Figure 5: The mental map of an informed organism. Perfect correspondence between inner and outer worlds.

relations of possibility between animals and their environments. A particular environment has a given affordance if and only if it makes a given kind of action possible, whether that action is actually executed or not. The claim that a given affordance exists is an objective claim, always either true or false: I may or may not be able to walk on that surface, for example.”

This is the first part of GIBSON’s affordance theory, and the least discussed in a general framework—the *objective* character of affordances. Of course, and GIBSON also notes this, the affordances are species-specific. Thin ice supports a mouse but not a cow. The affordances correspond to the black areas—“reality”—in the figures above.

The second part of GIBSON’s argument is that affordances are *directly perceived*, and this I would like to discuss further.¹³ I have tried above to evoke a picture where the sensory dimensions are separated from the value dimensions, and I would like to follow that line of reasoning for a while before returning to GIBSON again.

If we assume direct perception of affordances, this would yield a perfect match between inner and outer environment, as in figure 5. GIBSON studied mainly visual perception, and for vision it might be easier to obtain a “true image” of the environment, although some parts of the cave are obscured from certain vantage points.

As I said before, the space that the creature investigates does not have to be spatial. Take a common example: the creature has to find food that is edible by exploring two food dimensions. The black space in the figures now represents the objectively safe zone, and the organism has to find sensory cues to get to know the space. Now, the negative feedback is harder: a “reality hit,” i.e. eating something outside

the borders, causes illness or death. The problem is that there is no a priori correspondence between the sensory dimensions and the value dimensions. Or is there?

Sensory organs represent an inductive heuristic that if we base our actions *in the real environment* upon the categories that we can find *in the sensory input*, then we survive. The “only” problem is that there are very many worlds compatible with our sensory input. Evolution, however, has helped us solve this problem of the adaptation of the senses to our environment:¹⁴ “The single individual is still a prisoner in his constructed world, but the system as such will slowly, over millions of generations, improve its correspondence between what goes on inside the brain and outside it” (SJÖLANDER 1995).

GIBSON’S theory is directly appealing in that it stresses the way we tie perception to *action* without reflection. Perception is not seen as passive intake of information into a storage unit, but as a direct guide to action. This thought also makes it possible to link human perception to earlier evolutionary stages, for example down to bacteria, which base their locomotion on sucrose gradients (STEWART 1996).

As humans we envision the problem as one of picking mushrooms in the forest. A combination of visual and olfactory cues guide us, and neither the visual nor the olfactory information provides information enough to form a true map of the space of edible mushrooms.

For lower organisms, there is the direct link from perception to action, which in some sense makes the border between the sense domains and the value domains disappear. Thus, GIBSON’S theory predicts a state where it is possible to *take the sense information for the value information*—i.e. there is a correspondence between the *salience* in a sensory dimension and the values that we need for survival. We can react directly to the sensory stimuli. We don’t spit out “bitter” food. We spit out “dangerous” food. This is a heuristic that saves much cognitive effort, and as long as the species survives it is a viable heuristic.¹⁵ For higher organisms, however, the case is more complicated, as we have cognitive structures mediating between perception and action (see section 2.3).

Evolution has helped us evolve sense organs that allow us to make distinctions corresponding to useful divisions of our environment. However, there are several situations where the correspondence does not hold. “Fruits and berries, for example, have more food value when they are ripe, and this is specified by the color of the surface” (GIBSON 1979, p131).

In the quotation from GIBSON, we see again how the association of the sense domain and the value domain is taken for granted. But just because we cannot take the association for granted, the correspondence between sensory properties and values is not completely arbitrary. In the case of fruits and berries there *are* for example chemical reactions in the value domain that change color when fruits grow ripe and have more food value. Some changes in skin color in fruits and berries, on the other hand, can be seen as a property in the sense domain that has co-evolved with animal *use* of the property. Certain plants depend on birds for disseminating their seeds, and thus the sensory stimulus that the birds use as a cue will be reinforced by evolution.

The preceding discussion touches the old problem of our direct access to KANT’S “Ding an sich,” and provides an evolutionary answer to the question of whether we can get to know the world directly: Insofar as the sensory organs really have adapted to the outer reality, what we get from them is knowledge about reality. Unfortunately, there does not seem to be any fundamental way of knowing *to what degree this adaptation has taken place*. However, the only conclusion granted by the theory of evolution is of course that the sensory organs have adapted to a degree where they have let the organisms survive in the specific environment where they have lived. Whether this means that we are exploring the borders of the cave, or stay somewhere in the middle, remains an open question.

2.2 Availability vs. predictive power

The constitution of our sense organs makes some properties more *available* than others. The eye is sensitive only to a certain frequency range of light, the ear to another range of sound. Some substances lack smell for our species. This is of course not because there is an absence of molecules of the substances to be picked up by our olfactory system, but because we have not adapted to this particular substance during evolution.

On the other hand, it is not certain that the most available properties will lead us to the right place in the value space: their *predictive power* is not necessarily very great.

To determine the utility of categorization, I propose to see categorization as a trade-off between availability and predictive power. In fact, this is a *consequence* of discussing categorization in an evolutionary framework: it is necessary to adopt a pragmatic point of view, where categorization is coupled

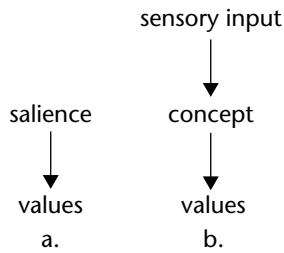


Figure 6: In some cases, we can act directly upon salient sensory input. In others, salience is downplayed by concepts.

with action, and this real-world action will be the mechanism of evaluation in evolution. Due to the limitations imposed by the maintenance of life processes, all categorization in nature will be bounded.

So, on the one hand we must investigate what information is readily available to us, through our sensory organs, and on the other we must see what kind of information we need for different purposes.

Availability will thus in practice put a limit on categorization in many situations. Many edible mushrooms are left uneaten in the forest, not because it is not *possible* to distinguish them from poisonous ones, but because the information needed is not *readily available*. The categorization procedure will change depending on the degree of *risk* involved.

2.3 Senses vs. brains—the emergence of concepts

We might use olfactory sense information to distinguish an edible mushroom from a poisonous one, and we do this because there are generalizable situations where it works. The introduction of a “nicely” smelling but poisonous mushroom into such a situation will force us to look for another distinguishing property, either a finer olfactory distinction, or a distinction in another domain, such as the visual.¹⁶ All such refinements will need additional representational feats: we will need some primitive “concept.”

The concept is an intermediary layer between sensory input and value domains (see figure 6).¹⁷ Concepts in this sense will comprise everything from a temporary downplaying of salience, due to attention, to human cognition.

One proposed model to consider here is the *subsumption architecture* by BROOKS.¹⁸ Cognitive representations (“concepts”) are seen as layered modules that can suppress lower layers, for example sensory input. There is also an element of competition between different modules, giving the cognitive system a certain amount of flexibility.

2.4 Extending the world of values—conditioning

To take but one example of the dichotomy between categorization with and without concepts, let us briefly compare the approach of this paper with *conditioning*. Conditioning is a term borrowed from behaviorism, traditionally accused of disregarding the role of internal representations. However, as we will see, it is also one of the few theories to account for the immediate association of the sensory domain and the value domain.¹⁹

In classical conditioning, there is a fundamental distinction between two kinds of stimulus. The first kind, for historical reasons called *unconditioned stimulus* (US), is directly connected to the value domains, and thus has *meaning*²⁰ for the animal. Unconditioned stimuli include food, which makes the animal salivate (PAVLOV 1927), and various stimuli causing pain, such as electric shocks.

The other stimuli in the environment do not have this connection to the value domains, but are nevertheless salient: flashing lights, ringing bells and the like were used in the experiments.

It turned out that if a meaningless stimulus is presented in connection with a meaningful one, the animal forms some kind of association—the formerly meaningless bell becomes a predictor of the meaningful food. The bell functions as a *conditioned stimulus* (CS). After a sequence of trials, the connection has grown so strong that the CS alone produces the response. Observe that the foundation of this form of learning always rests on the association of stimuli with the value domain. This is a way for the animal to *create meaning*.

From one perspective, the association of an unconditioned stimulus with a response must be seen as direct, as in figure 6 a, and the conditioned stimulus producing a response as mediated, as in figure 6 b. However, as for example the behaviorists maintained, there is perhaps no need to postulate cognitive representations at all to describe these processes. This question will remain open.²¹

2.5 Evolutionary essentialism

In the literature, the *essence* of categories is often discussed as properties of an instance that must be present for the instance to be an instance of that category (GELMAN/COLEY 1991). An albino tiger is still a tiger, as long as it possesses tiger DNA, for example. The essence in essentialist theories provides an *evaluation* of the category, but is not postulated against some criterion *outside* the system of categorization.

In my framework, where the *raison d'être* for the creation of categories is that they orient us towards the value domain, the essence will be given by the association with the value domain. The only thing that we require of a category in its simplest form is that it points the way to primary values.²² Thus, an evolutionary essentialism will not require our *knowing* what the essences *are*—they are chiseled out by the evolutionary pressure on the categories.

Depending on whether a category represents something that an animal wants to get or to avoid, we will have to distinguish two cases. A category that represents a *positive* primary value, i.e. something that the individual must have for its survival, will always have an “essence,” otherwise the animals with this kind of categories die. If, for example, we base our survival on a cereal that does not give us essential amino acids, we will not survive.²³

Categories representing *negative* primary values will not be subject to the same constraints: we can fear something that is not dangerous without ever getting negative feedback on this categorization. We can continue avoiding the dark cave in the forest, even though the dragon died many years ago—we will never know if we don't see for ourselves. Thus, for these categories we do not have to postulate that they are grounded in an essence, only that the categorizations are subject to constraints for reasons of cognitive economy.

3. The unit of interaction and the unit of perception

I have pictured a scene that is common to all living beings, where the basic primitives are the survival loop, and the substances with food, danger and reproduction value. Values are not cognitive, and when we benefit from them, it is not basically from sensory stimulation, although even the simplest organisms have evolved senses as a *guide* to values. The next question in this paper is *in what form* we conceive of these substances—as single stimuli clustering to form objects, as holistic objects being decomposed into dimensions, as whole environments or as combinations of the three. I will not reach a definitive conclusion on these matters,²⁴ I want rather to put the searchlight on some evidence that has existed for a while but that has not received the attention it deserves. Although the evidence is inconclusive it comes from a range of cognitive disciplines, and that might help as a corroboration.²⁵

3.1 A complex unit of perception

As I argued above, the essential interaction of the organism with its environment is in the *value* domains. Due to the physical properties of organism and environment, this interaction is complex—it is not possible to imagine the interaction as an interface only letting through one dimension at a time. Furthermore, there is often much *potential sensory information* in the organism's environment, that the organism might use as a guide to the value domains.

Given this complexity in the environment, I will argue that the best level of description of perception is on a more complex level than in most classical psychology and philosophy. This is in accordance with the characterization by CAMPBELL (1966, p82):²⁶ “Both psychology and philosophy are emerging from an epoch in which the *quest for punctiform certainty* seemed the optimal approach to knowledge. To both PAVLOV and WATSON, single retinal cell activations and single muscle activations seemed more certainly reidentifiable and specifiable than perceptions of objects or adaptive acts.”

From an evolutionary perspective it is arguable that the unit of perception would lie closer to the unit of interaction. I will show evidence of representations at least on an intermediate level between the “objects” of interaction and the more primitive analytical level common in psychology. One such example is the concept of affordance in GIBSON's (1979) ecological theory of perception: “[...] what we perceive when we look at objects are their affordances, not their qualities. We can discriminate the dimensions of difference if required to do so in an experiment, but what the object affords us is what we normally pay attention to. [...] If this is true for the adult, what about the young child? There is much evidence to show that the infant does not begin by first discriminating the qualities of objects and then learning the combinations of qualities that specify them. Phenomenal objects are *not* built up of qualities; it is the other way around. The affordance of an object is what the infant begins by noticing. The meaning is observed before the substance and surface, the color and form, are seen as such. *An affordance is an invariant combination of variables, and one might guess that it is easier to perceive such an invariant unit than it is to perceive all the variables separately.*” (GIBSON 1979, p134–5, my emphasis).

“Invariant combination of variables” should be read as variables that *covary* with each other, but not with other properties. I will therefore examine the concept of covariation over the next subsections.

3.2 The covariation heuristic

Even our most basic practices involve complex sensory input. As I have discussed in section 2.2, categorization is dependent on the predictive power of the available sensory information. However, if the organism can detect *covariation* in the sensory input, the rudiments of concepts can be formed. The various combinations of stimuli that are obtained when covariation is taken into account have a much greater predictive power than the stimuli in isolation. This heuristic can be characterized as one of inductive inference:

“Inference from clustering of categorial properties:

(a) ‘Individual members of a natural category do not share only a single criterial property. Rather, they most often share many properties, which are thus the *definitional core* of their categorial membership.’

(b) ‘Therefore, if known members of a group exhibit properties *A, B, C* etc., and if a sample subgroup also exhibits property *Z* (to a statistically-significant degree), then it is *highly likely* that the rest—untested—members also exhibit property *Z*.’” (GIVÓN 1989, p276)

However, it seems to be clear that not all animals utilize all possible covariation information. As reported in SJÖLANDER (1995), snakes, for example cannot use cross-modal covariation. Snakes use different modalities for completing different sub-tasks, such as catching the prey and swallowing it. This should not be considered as odd from an evolutionary point of view. In the same way as not all potential sensory information is used by animals, there is a great potential of covariation information that *could* be used. However, it is likely that different sensory stimuli will provide a basis for covariation for different organisms. Covariation detection will be an *economical* solution for categorization, but only up to a certain breakpoint, which will be decided by the evolution of the organism.

Cross-modal covariation can be used by human infants to integrate different senses. PIAGET (1968/1970, p90) relates the case of an infant of 3 months at the developmental stage when the visual system is gaining autonomy. There is a functioning “*réaction circulaire*” consisting of hand movements mainly towards the mouth. The child uses hand movements as a source of covariation in connection with vision: the hand can be moved with relative autonomy, and the first step for the visual system is to follow the movements of the hand. The opposite is not yet true, so the

infant cannot look at an object, and then reach out and grasp it, a faculty that it will soon achieve. Thus, there is a possibility to integrate sensory information by means of covariation with a sense that already has meaning in that it can be used to obtain values. Vision, working at a distance, is not so easy to integrate with value loops, and thus needs the initial support of the haptic sense.

3.3 Basic level objects

In psychology we find another account that parallels GIBSON’s approach.²⁷ ROSCH (1978, p31) presents the *basic level* of categorization: “A working assumption of the research on basic objects is that (1) in the perceived world, information-rich bundles of perceptual and functional attributes occur that form natural discontinuities, and that (2) basic cuts in categorization are made at these discontinuities.”

I believe that in the present evolutionary framework it is safe to extend her framework from the “perceived” world to the “real” world. We could not have an evolution of a conceptual system without a counterpart in the value domain.²⁸ (Given this firm evolutionary conviction, it would also be possible to try to reconcile the psychologicistic position in ROSCH’s account with the realist discussion about *natural kinds*.²⁹)

3.4 Perception of complex properties

What then is the evidence to corroborate that we do in fact detect covarying properties directly, rather than by composition from more primitive sense domains? From semantics, for example, we are used to thinking about concepts as decomposable into primitive features that are then processed bottom-up to form complex concepts by production rules.³⁰ My argument in this section is that this is not the most fruitful level of explanation, since it has no connection to the value domains. Rather, we will look for representations that correspond more closely to the level of interaction of the animal. TANAKA (1993) shows that in the anterior infero-temporal cortex (TE), we find *single-cell activation of complex* features: “The critical features were more complex than orientation, size, color, and simple texture, which are known to be extracted by cells in V1. Some of the features were shapes that were moderately complex, whereas others were combinations of such shapes with color or texture. The individual critical features were not complex enough to specify a particular object seen in nature through activation of a single

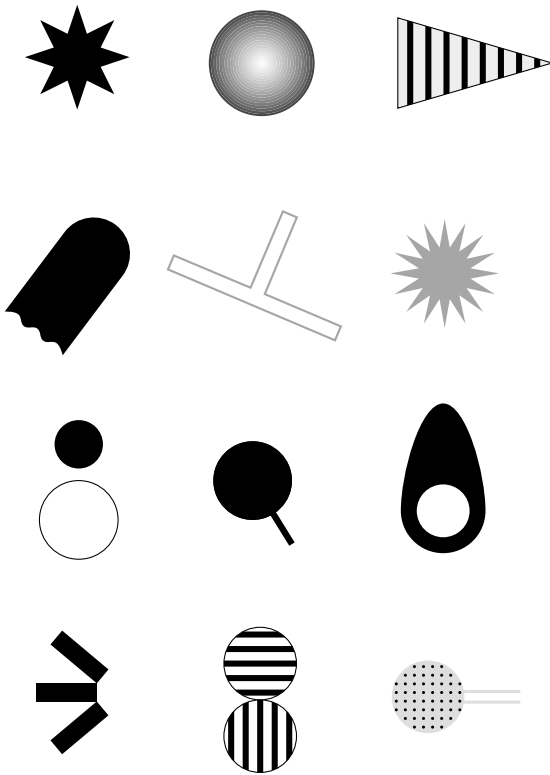


Figure 7: Twelve examples of critical features for the activation of single cells in area TE. From TANAKA (1993). Some features are also colored in the original.

cell. Activation of a few to several tens of cells with different critical features seems necessary to specify a particular natural object” (TANAKA 1993, p686).

Figure 7 shows some of the critical features. The importance of TANAKA’S results is that they challenge our view of features as being describable as simple concepts in EUCLIDEAN geometry that generate more complex representations by production rules.

Given the stimuli that the visual system must distinguish, *various* intermediate levels could be imagined. It is very hard to predict the stimulus set that a combination of such a kind of “moderately complex” features would be able to distinguish, and even more impossible to find the *best* intermediate set for a given set of stimuli. But what could be learned from this is (a) that there exists a level of representation that does not benefit from being decomposed into primitives, and (b) that this level is not generated by geometrical primitives, but by evolutionary pressure on real-world categorizations.³¹

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The theory that I have developed above proposes that categorization and perception are closely tied to our interaction with elements in the environment, and in particular those elements that carry values with a direct impact on ourselves. As humans we share the need for values with lower organisms, and we are as dependent as they are upon food, mates and shelter. But we have also evolved higher representational skills that are not closely tied to our basic interaction with the world.

4. Conclusions

In the paper, I have provided an evolutionary framework to open up a discussion about some fundamentals of categorization and cognition. As a summary, I would give only a few points of reference to the issues discussed.

- The organism needs substances with primary value for its survival. These substances are not *stimuli* in that their important function is to stimulate the senses of the organism. Rather the organism *merges* with them. Values represent the *essence* of the categories.

- Senses are our means of making contact with the substances that have value for us. Together with conceptual representations they let us diverge from the essential substances for a time and find them again more efficiently afterwards, but “we must respect our meals,” and be careful not to break the loop of life processes.

- Evolution has shaped our senses so that for *some* categories we can react directly to our sense impressions *as if* they represented the essence/value. This explains what is sometimes called direct perception.

- There are both beneficial and harmful values. There is an imbalance in how we can react toward them. We can continue to avoid things that are not dangerous, but we have discontinued eating things that made us die.

- There is a moment in time when the categorization takes place, and it is signaled by the *behavior* of the individual. The categorization has to be done with the available sensory information that does

not necessarily carry the optimal predictive power. Thus, there is a trade-off between availability and predictive power.

- In many theories of categorization, categories are built up from singular dimensions. In the current approach, the

fact that we always *act* in complex environments is exploited as an advantage. The unit of perception will reflect the unit of interaction. In the organism–value loop there is always a multitude of information available that can provide a basis for sensorial inferences.

■ Both the theory of values and the theory of covariation originate in the assumption that we interact with real-world objects rather than with singular sensory stimuli.

■ A theory of values is necessary to maintain the evolutionary continuity between animal and human categorization, since symbolic (or memetic)

evolution is subject to partly different evolutionary constraints.

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Notes

- 1 Early works on evolutionary epistemology include CAMPBELL (1974/1987) and LORENZ (1973/1977).
- 2 Mainly in accordance with GIBSON (1979). See further below. This term is close to von UEXKÜLL's (1982) concept of "meaning."
- 3 Cf. the "functional circle" of von UEXKÜLL (1982).
- 4 Likewise in other languages, e.g. *göra ont* in Swedish.
- 5 It is always the *situation* that must be categorized as beneficial or detrimental, not a singular object. For example, if the animal finds good food in a dangerous place, it must have a means of judging advantages against disadvantages. Not all animals are good at this, when manipulated by "smarter" animals. A mouse-trap is a familiar example.
- 6 Readers acquainted with constructivism will recognize most of the terms and arguments, but will perhaps be surprised at the stress on *reality* as an important part of the theory. Constructivist literature includes VON GLASERSFELD (1976; 1977; 1984; 1995), STEWART (1996), SJÖLANDER (1995; 1998), WATZLAWICK (1984).
- 7 As von UEXKÜLL (1909/1985) observed, when discussing the inner worlds of lower organisms, we as researchers always adopt the perspective of the organism in question. I will therefore allow myself to use the pronoun "we" even when I talk of bacteria and other lower organisms.
- 8 For the sake of familiarity, I will assume that the dimensions are spatial during the discussion.
- 9 The limits of reality do not generally move. "Agents" are exceptions (GIVÓN 1989; PREMACK 1996).
- 10 "Reality is what makes your expectations fail." (Per JOHANSSON, pc)
- 11 Depending on the negative feedback in the dimension in question.
- 12 One possibility is to see the bold creature as generalizing on a coarser scale (BALKENIUS 1996).
- 13 This is the most common interpretation of GIBSON's thoughts, and the part that provides the foundations of the theory of "visibility" of NORMAN (1988)—we act *directly* on cues in the environment.
- 14 This idea originates in LORENZ (1973/1977, p 6–7).
- 15 As a heuristic it is quite fixed, and an *evolutionary* heuristic rather than an individual one!
- 16 Downplaying the most salient attributes of objects is equivalent to postulating the existence of *nonobvious* properties as important for categorization. A commonly used example is the distinction between bird and bat, where overall similarity is outweighed by genetic relation. See the brief discussion of nonobvious properties in GELMAN/COLEY (1991).
- 17 In this very general discussion, I have chosen the term "concept" for the intermediary layer, although as will soon be clear, some of the concepts are *very* rudimentary. Other terms that could be used are "theory," "schema," or "representation."
- 18 BROOKS (1991), reviewed in BALKENIUS (1995).
- 19 See BALKENIUS (1995, ch. 5) for an overview of conditioning, and RESCORLA (1988) for some relatively late developments within the behaviorist tradition. Following a suggestion by Christian BALKENIUS (pc), it is possible to see conditioning as a *performance test* rather than a foundational property of cognition. This view makes conditioning easy to reconcile with representational theories.
- 20 Especially in the sense of von UEXKÜLL (1982).
- 21 As a general epistemological standpoint: in scientific developments there often arises the need for a *dialectical contrast* between positions. The content a concept has will be dependent on what it is *contrasted against*. If we discuss human concepts, we may say that they clearly mediate between perception and values, and then an association like conditioning will in this simple model be direct. See ANDERSSON (1994) for this kind of polemic concept formation.
- 22 In higher-level cognition there are of course also second-order categories that are not coupled to a primary value domain. A simple example is money. It is not useful in itself, but we "reward ourselves" when we get it.
- 23 MEDIN/ORTONY (1989) advocates a view almost contrary to mine: psychological essentialism, described as "not the view that *things* have essences, but rather the view that people's *representations* of things might reflect such a belief (erroneous as it may be)." Also GELMAN/MEDIN (1993, p163): "Essences are typically not known, almost always unobservable, and may not exist. So, the essence itself cannot usually serve as the basis of how people categorize or identify items." However, when studying human categorization that is connected to language and intentionality, this position is more tenable.
- 24 The debate on category coherence is long and continuing. See for example KEIL (1989), MILLIKAN (1998), QUINE (1969), GELMAN/COLEY (1991), MEDIN (1983).

- 25 Early developments of this question are discussed in CAMPBELL (1966, p89). Cf. also the tradition of Gestalt psychology (KÖHLER 1947), and the excellent critique in LEYTON (1992) in terms of reduction of Gestalts to causal histories with the aid of symmetry principles.
- 26 Linguistic semantics has suffered the same decomposition into minimal features.
- 27 There are also other theoretical developments in various traditions. SHANKS et al. (1996), following a suggestion by RESCORLA, talk about *configural* stimuli: co-occurring stimuli should be treated as unique and not as the simple sum of their parts.
- 28 What we of course cannot say anything about is whether there are *other values* that could be utilized with another conceptual system, or whether there are *other combinations of sensory domains* that could provide a better foundation for categorization.
- 29 Richard BOYD (1991) provides similar evidence, based on ideas indicating that natural kinds should be viewed as *homeostatic property clusters*: this is the same idea about cova-

riation, supplemented with a claim regarding category coherence by homeostasis. There is a striking resemblance between this account of homeostasis and the description of life processes in terms of "autopoiesis" in MATURANA/VARELA (1987/1992).

30 E.g. LYONS (1977, ch. 9).

31 See also LETTVIN et al. (1959) for a discussion of the receptors in the frog's eye, and that "the language in which they are best described is the language of *complex abstractions from the visual image*. We have been tempted, for example, to call the convexity detectors 'bug perceivers.'" (p1951, my emphasis)

There is some literature explicitly dealing with covariation detection, e.g. KAREEV (1995), BILLMAN (1983), BILLMAN/HEIT (1988), NISBETT/ROSS (1980), but most of the stimuli used in their experiments require language, and linguistically based representations are subject to other evolutionary laws than the ones discussed in this article (DENNETT 1995/1996, ch. 12).

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Zusammenfassungen der Artikel in deutscher Sprache

K. Lorenz/K. Okawa/K. Kotrschal

Non-anonymous, Collective Territoriality in a Fish, the Moorish Idol (*Zanclus cornutus*): Agonistic and Appeasement Behaviours

Konrad LORENZ arbeitete lebenslang mit Fischen, was sich zwar in seiner Theorieentwicklung, kaum aber in entsprechenden Originalarbeiten niederschlug. Dies gilt besonders für die in seinem Altenberger Riffaquarium von April 1976 bis September 1980 durchgeführten Beobachtungen zum Sozialverhalten barschartiger Fische. Da die Ergebnisse dieser Arbeit nicht nur von historischem Interesse, sondern auch wissenschaftlich relevant sind, lag es nahe, einen Teil, beruhend auf LORENZ'schen Textfragmenten, zu veröffentlichen.

Beobachtungen im Aquarium (1954–1973 an den MAX-PLANCK-Instituten in Deutschland) und im Freiland (1976 in Kanehoe Bay, Oahu, Hawaii) ergaben, daß Halfterfische (*Zanclus cornutus*) ein bisher für Fische unbekanntes, potentiell im Zusammenhang mit Befriedung stehendes Spektrum an Verhaltensweisen zeigen. Wie andere Riffische auch, etablieren Halfterfische anschließend an ein planktonisches Larvenstadium im Riff zunächst ein heftig verteidigtes Territorium, patrouillieren aber später Streifgebiete in Gruppen. Die im Zuge dieser ontogenetischen Veränderung gezeigten Verhaltensweisen wurden zwischen April 1976 und Juli 1978 in einem 4 × 4 × 2 m (32 m²) messenden Riffbeken in Altenberg/NÖ. protokolliert. Ein offenbar von Konrad Lorenz im Februar 1979 erstelltes Manuskript enthält eine detaillierte Beschreibung der Ereignisse in Tagebuchform. Aus diesem Fragment wurde von Kurt KOTRSCHAL eine ausführliche Zusammenfassung konzentriert und mit Hilfe von Keiko OKAWA, einer ehemaligen Diplomandin von Konrad Lorenz um die fachliche Einbettung und Interpretation ergänzt.

Fünf postlarvale *Zanclus* wurden im Februar 1977 eingesetzt. Die Fische etablierten Territorien entlang von Boden und Wänden des Aquariums. In den darauffolgenden Monaten verschmolzen sie, zunächst paarweise, sukzessiv ihre Territorien. Neu eingesetzte *Zanclus* schlossen sich entweder nach einer

Periode agonistischer Interaktionen der etablierten Gruppe an, oder sie wurden im Zuge von Beschädigungskämpfen getötet. Eine etwa ein Jahr später eingesetzte Gruppe juveniler *Zanclus* zeigte ein ähnliches Muster der Gruppenbildung. Die älteren und jüngeren Fische bildeten getrennte, kaum interagierende Kohorten. Die dyadischen Interaktionen innerhalb der Gruppen, sowie zwischen diesen und den Außenseitern machen es wahrscheinlich, daß die beteiligten Fische einander individuell erkennen. Das agonistische Verhaltensrepertoire gleicht dem der meisten Barschartigen Fische. Erstmals beschrieben wurde hingegen ein Spektrum von Verhaltensweisen, welche im Zuge dyadischer Befriedung gezeigt wurden. Dazu zählt etwa "nahes Gassen", zuerst entlang der gemeinsamen Grenze des Territoriums, dann davon unabhängig. Das häufig nach einer Trennung gezeigte "zuschwimmen-abwenden" ist als Kontaktverhalten zu deuten, welches aus Angriffsverhalten ritualisiert. "Schlängelschwimmen" und "Steinputzen-Scheinablaichen" entspringen wahrscheinlich dem sexuellen Kontext. Befriedungsverhalten wurde von Tieren mit komplexer sozialer Organisation und erheblicher "sozialer Intelligenz" bekannt, beispielsweise von Primaten. Bei Fischen wurde aber erstmals derart komplexes Befriedungsverhalten beschrieben.

Anthony Chemero Teleosemantics and the Critique of Adaptationism

In diesem Artikel werden die Bezüge zwischen Adaptationismus – jener Position im Rahmen der Evolutionsbiologie, die organismische Eigenschaften, Merkmale und deren Funktionen als Produkte natürlicher Selektion interpretiert – und „Teleosemantik“ diskutiert. Darunter versteht man teleofunktionale Ansätze, die sich vor allem mit dem Inhalt linguistischer und mentaler Repräsentationen beschäftigen. Der Inhalt dieser Repräsentationen wird vor allem als Produkt seiner evolutionären Vergangenheit gesehen.

Hinsichtlich der Bezüge zwischen Teleosemantik und Adaptationismus werden zwei Fragestellungen

behandelt: (1) Wie exakt muß die Methodologie des Adaptionismus sein um die Teleosemantik zu unterstützen, und (2) ist der Adaptionismus derartig exakt?

Hinsichtlich der ersten Frage wird die Ansicht vertreten, daß ein teleologischer Ansatz der Umgangssprache von einem teleologischen Ansatz mentaler Repräsentationen unterschieden werden kann. Der teleosemantische Ansatz der Umgangssprache, den ich vertrete ist völlig unabhängig vom Adaptionismus, während derjenige der mentalen Repräsentationen stark vom Adaptionismus abhängt.

Bezüglich der zweiten Frage werden einige bekannte Formen der Kritik am Adaptionismus überprüft (Probleme der Optimierung; „free rider“-Kritik) um festzustellen, ob diese Kritiken eine Teleosemantik für mentale Repräsentationen als unmöglich erscheinen lassen. Da jedoch die Teleosemantik der Umgangssprache unabhängig vom Adaptionismus ist hat diese Form der Kritik keinen direkten Einfluß. Ein Kritikstrom aus dem Bereich der „free rider“-Kritik jedoch bereitet einer Teleosemantik mentaler Repräsentationen beträchtliche Probleme. Dieser erinnert uns daran, vorsichtige Adaptionisten zu sein, die zumindest zugestehen sollten, daß wenigstens einige Teile von Organismen nicht direkt Resultat selektiver Prozesse sind. Jedoch verlangt die Teleosemantik mentaler Repräsentationen eine selektiv – evolutionäre Vergangenheit für rein theoretische Gebilde, was weit von einem vorsichtigen Adaptionismus angesiedelt ist – vor allem im Lichte gegenwärtiger Versuche der Erklärung von Kognition ohne Bezugnahme auf mentale Repräsentationen.

Helena Knyazeva

The Synergetic View of Human Creativity

In diesem Artikel wird die heuristische Bedeutung der synergetischen Modelle evolvierender und selbstorganisierender komplexer Systeme, sowie deren Anwendung auf erkenntnistheoretische Fragestellungen erörtert. Im Umgang mit Problemen der Kreativität, dem Funktionieren menschlicher Intuition, der produktiven Einbildungskraft und der historischen Entwicklung von Wissenschaft und Kultur erweisen sich nichtlineare, synergetische Modelle als überaus fruchtbar. Das kreative Denken kann aus synergetischer Sicht als Selbstorganisation und Selbstergänzung von Gestalten und Gedanken, als Ausfüllen von Lücken in den Netzen des Wissens betrachtet werden.

Die „Erleuchtung“, das „Aha-Erlebnis“ bzw. die schnelle und plötzliche Lösung eines wissenschaftlichen Problems, sowie Instabilitäten, die entstehen, wenn „eine Idee in der Luft ist“ und der Boom der Forschung in gewissen Fachbereichen werden als Beispiele der „Blow-up Regime“ im kognitiven Bereich diskutiert.“

Armando Aranda-Anzaldo On Natural Selection and Hume's Second Problem

David Humes berühmtes Induktionsproblem beinhaltet ein zweites Problem welches sich auf die Frage bezieht, ob sich die Gesetze und Prinzipien der Natur im Verlauf der Zeit ändern. Unter Berufung auf Daten der modernen Physik und Astrophysik wird behauptet, daß sich in zeitlicher Hinsicht die Gesetze der Physik als Invarianten erweisen. Hier wird die Frage in den Raum gestellt, ob Prinzipien bzw. Gesetzmäßigkeiten in der Natur entstanden sind, die nicht von den derzeit bekannten Gesetzen der Physik abgeleitet werden können, bzw. die „nicht physikalisch“ in dem Sinne sind, als sie einer Erklärungsebene angehören, die völlig unabhängig von der Physik ist. Wenn das der Fall ist, bleibt Humes zweites Problem aktuell.

In diesem Artikel wird die Existenz eines derartigen „nicht physikalischen“ Prinzipes behauptet: die natürliche Selektion, welche bei der Entstehung des Universums noch nicht gegeben war, sondern im Verlauf der Zeit entstanden ist. Um diese Behauptung zu begründen werden einige zentrale Trends im Bereich der Theorien der Lebensentstehung untersucht, um sodann eine bestimmte Theorie als ein konkretes Beispiel dafür anzuführen, daß natürliche Selektion erst später entstanden ist.

Marie-France Chevron Man's Special Position in Nature The Relationship between Biological and Cultural Development

In verschiedenen Wissenschaften, in welchen von einer Sonderstellung des Menschen ausgegangen wird, konnte diese wissenschaftliche Erkenntnis doch bisweilen zu völlig entgegengesetzten Schlußfolgerungen führen und sogar zu einem Dogma

erstarren, das ein unvoreingenommenes wissenschaftliches und interdisziplinäres Fragen nach dem Wesen des Menschen und der Kultur, aber auch der kulturellen Entwicklung, unmöglich machte.

In dieser Arbeit wird einerseits nach den Ansätzen und Prämissen der Evolutionären Erkenntnistheorie (sprich der evolutionär ausgerichteten biologischen Wissenschaften) und der Ethnologie als Kulturwissenschaft in Zusammenhang mit den Theorien der Entwicklung gefragt, andererseits werden die Schwierigkeiten und Fehlerquellen einer interdisziplinären Auseinandersetzung kritisch dargestellt. Besonders am Beispiel der Analogie als Instrument der Erkenntnis wird das Problem der in diesem Rahmen oft vollzogenen „Biologisierung der Gesellschaft“ angerissen, wobei nicht nur auf den Mißbrauch, sondern auf echte Anwendungsmöglichkeiten der „Analogie als Wissensquelle“ hingewiesen wird. Wissenschaftsgeschichtliche und methodische Überlegungen, aber auch den Stellenwert der rein sachbezogenen interdisziplinären Besprechung der Forschungsergebnisse einzelner Disziplinen werden erwähnt. So wird auf die Notwendigkeit einer interdisziplinären Grundlagenforschung über den Menschen als Kulturwesen hingewiesen, wobei aus ethnologischer Sicht die Erforschung transkultureller Universalien als universell vorhandene Grundmuster menschlichen Seins mit ihrem speziellen kulturellen Überbau wesentlich zur Klärung des Zusammenhangs zwi-

schen biologischer und kultureller Entwicklung vor dem Hintergrund einer evolutionären Betrachtungsweise beitragen kann.

Simon Winter

Evolution, Categorization and Values

In diesem Artikel soll ein evolutionärer Rahmen für die Kategorienbildung entwickelt werden. Ausgangspunkt ist dabei ein Bewertungsmechanismus, der hinsichtlich primärer, überlebensrelevanter Bereiche (Nahrung, Schutz, Reproduktion...) die Evolution von Kategorien wesentlich mitbedingt hat. Dabei wird zwischen Lebensprozessen und kognitiven Prozessen insofern unterschieden, als Lebensprozesse bewertende Komponenten beinhalten, während kognitive Prozesse überwiegend perzeptive Eigenschaften aufweisen. Beide gehören zusammen indem die sensorische Stimulation mit der Entstehung des kognitiven Apparates zusammenhängt, der selbst jedoch keine Bewertungsvorgänge vornehmen kann. Kategorisierungsvorgänge hängen dahingehend sowohl vom „Wertsystem“, wie auch von der Verfügbarkeit sensorischer Information ab. Ausgehend von diesen Bezügen wird vorgeschlagen, die Einheit der Perzeption nicht wie bisher in eindimensionalen Stimuluskonfigurationen zu sehen, sondern den gesamten Kontext mit seinen Wertdimensionen mit einzubeziehen.