Chapter 2

Towards a biology of social behavior: a systemic perspective

The aim of the present and the following two chapters is not merely to provide a necessary theoretical and methodological background against which to place the experimental studies presented in the remainder of the thesis. If these studies seem to fall short of achieving a thorough exploration of all the potential avenues of research implied in the theoretical exposition that follows, this is partly owing to the richness and openness of the subject. The objective, therefore, is to present a rough map of the territory and its potential, both thematic and methodological, for scientific research without necessarily fulfilling this potential in its entirety in the actual studies presented afterwards. These concrete studies are intended firstly as contributions in themselves to the particular subjects they address but also as examples of how particular methodologies and sets of ideas can be applied within the general theoretical framework depicted here.

In the present chapter a review will be made of some issues regarding the biology of social behavior, starting by specifying the need for a biological grounding of the terms used to discuss social behavior and by analysing the distinction between functional and operational statements as belonging to two different domains of scientific discourse. Comments will be made on how these domains are related and some of the methodological problems that may arise when the distinction is blurred. The chapter will then proceed with a detailed analysis of an example where it is considered that problems of this kind have arisen in biology. Such is the case of animal communication as studied by evolutionary biologists and behavioral ecologists. The corresponding section will discuss some of the problems that originate in characterizing communication only in functional terms according to selective advantages and information transfer and how this perspective has shaped the set of questions that make sense to investigate. This is followed by the theoretical core of the chapter (section 2.4) where a systemic perspective on social behavior in autonomous entities is presented based mainly on the works of Humberto Maturana and Francisco Varela (although some points will be criticized or further elaborated). The specific case of animal communication is then re-assessed from this new perspective and some pragmatic consequences derived and compared with the more traditional point of view.

2.1 The pragmatic need for a biological grounding

Any good interpretation of a map, if some use is expected out of it, must be guided by some principle so as to avoid, at the initial stages, spending too much time on details and forgetting the bigger picture. The guiding rule that is to be used here is that all the phenomena under consideration in the context of this thesis should be able to be linked in a continuous fashion to phenomena describable in the language of dynamical systems. In particular, descriptions of biological and cognitive phenomena will be expected to be able to be made continuous with the consequences of the basic logic of the autonomy of living systems as material entities.

It is important to emphasize that the aim is not to give supporting arguments for such approaches in themselves [e.g. the use of dynamical systems in cognitive science instead of a more traditional computational approach, (Van Gelder & Port, 1995; Van Gelder, 1999)]; rather, the point is to *use* these approaches mainly as conceptual tools that will enable the research to view the issues of interest from a certain systemic perspective that is believed to be fruitful (and in some cases badly needed) for approaching a scientific understanding.

Some readers may argue that the use of a conceptual framework implicitly endorses such a framework as more acceptable than others. Indeed, this could be said. However, it is preferable to say that the use of a tool indicates an opinion mainly about its suitability and nothing else. In certain parts of this thesis it will be useful to apply game theory to evolutionary problems, simply because it is the right tool for the job. This does not mean that evolutionary game theory is endorsed as the most acceptable approach or as providing any sense of truth or privileged explanatory power.

The hypothesis of continuity of cognitive and social phenomena with biology (and eventually with physical phenomena[1]) demands that all descriptions be traceable to biological, physical or dynamical substrates, without in any way implying that these substrates are all one needs to *know* to generate from first principles the concrete phenomena of interest. Such a form of reductionism would be at odds with the pragmatic stance that is intended to serve as guidance since it would mean that certain distinctions are privileged over others ignoring the fact that such distinctions are made in the first place in order to fulfil a purpose and not as ontological commitments.

The purpose of tracing the phenomena of interest to a biological and dynamical substrate is not to be able to describe how the latter *determines* the former but how the former is *constrained* by the latter. In this way, if one is able to trace social phenomena to biology, it does not mean that social phenomena cannot have a logic of their own, impossible to arrive at from a strictly biological plane. In fact, what will be discovered, is that social phenomena, while traceable to biology, are usually underdetermined by it. Finding a trace of continuity between two domains of description is not meant as a reduction but rather as the identification of how one domain can constrain the other. It is not practical to spend too much time with theories that simply do not fulfill these constraints.

[1] On this issue see for instance Wheeler (1997), Stewart (1996) and in general the works of Maturana and Varela as discussed in section 2.4.

2.2 Functional and operational approaches

Not all descriptions or explanations in science are equivalent. It is often convenient to make a distinction between two main forms of scientific discourse. In one case, an explanation or description is formulated in terms of a set of elements all pitched at a same descriptional level and also in terms of a set of law-like relationships between these elements so that an account can be given of how the phenomena are generated. Explanations and descriptions formulated in this way will be called "operational". Typical examples of these are mechanistic explanations. In a different domain of discourse, it is possible to choose the relevant elements to be used much more freely and to focus only on certain type of regularities not necessarily apparent at the operational level while ignoring the rest (or putting it as part of a less specified context). One may choose to do this because it serves the purpose of facilitating an understanding of the relation between phenomena that operate at different levels or timescales or whose operational relationships are not completely understood or could not be completely understood. Such is the case of intentional, teleological or, in general, functional explanations or descriptions.

This distinction will be elaborated a little further followed by an exploration of the relationship between the two domains of discourse and why they should be kept distinct. Varela (1979) describes the distinction between the operational and the functional (or symbolic) modes of explaining in the following way:

> "In both cases the recorded phenomena are reformulated or reproduced in conceptual terms that are deemed appropriate. The difference lies in the fact that in an operational explanation, the terms of such reformulation and the categories used are assumed to belong to the domains in which the systems that generate the phenomena operate. In a symbolic explanation, the terms of the reformulation are deemed to belong to a more encompassing context, in which the observer provides links and nexuses not supposed to operate in the domain in which the systems that generate the phenomena operate." (Varela, 1979, p. 66)

Consequently, explaining a phenomenon operationally entails making a distinction of a set of elements or components which relate following a corresponding set of laws and relations, all of which play a generative role in the realization of the phenomenon.

> "A characteristic feature of an operational explanation is that it proposes conceptual (or concrete) systems and components that can reproduce the recorded phenomena. This can happen through the specification of the organization and structure of a system [...] This is so because the organization of a machine [...] only states relations between components and rules for their interactions and transformations, in a manner that specifies the conditions of emergence of the different states of the machine, which then arise as a necessary outcome whenever such conditions occur." (Varela, 1979, p. 66)

In contrast, a functional or symbolic explanation relaxes the condition that all its elements must be related through law-like links. The observer may choose to connect different elements only in terms of the correlations she finds interesting or practical between the system and a contextual set of other relevant phenomena. This allows for terms to participate in these explanations that do not necessarily refer to any concrete or observable components of the systems involved and get their meaning from regularities between different observations made by the observer (e.g. the terms "fitness" or "selective history" in evolutionary biology). Thus, functional explanations always *escape* the boundaries of the systems that give rise to the phenomenon being explained by including contextual elements, either concrete or relational and, as a consequence, they can never be equated with an operational account of that phenomenon.

In spite of this, it is all too easy to slip from a functional type of talk into an operational one simply because certain terms in functional accounts can also be identified as concrete components of the system of interest. Consider the DNA molecule. It is possible to assign to it different functional roles depending of the context and regularities that the observer chooses to pay attention to. It could be said that this molecule encodes for sequences of aminoacids that will form proteins *given* the context of the molecular machinery that operates inside the cell (and this could be affirmed even if such a context is poorly understood). Or it could be stated that the DNA molecule encodes for heritable characteristics *given* the context of the reliability of its replication across a history of serial reproductive events. But it is wrong to state that DNA instructs the cell on how to differentiate or what chemicals to secrete in order to produce an organismic trait because, when looked as part of an operation of the internal processes of a cell, the DNA molecule is just as important as any other molecule. It is made up of atoms like all the others. There is nothing special about it. If DNA has any special status at all, it is not an operational one and not even an explanatory one *in general*. The only relevance of DNA is a functional relevance for *certain* types of symbolic explanation and *certain* contexts of discourse such as biological evolution and probably other conditions (see Clark & Wheeler, 1998).

Notice that the error in assigning operational power to functional statements does not depend on the criterion by which the functional statement is justified. Therefore, any arguments defending a context of legitimation for certain functional accounts instead of others [for instance, Millikan's proper functions as those that are such in virtue of their selective history (Millikan, 1984)] is beside the point. No functional statement should be confused with an operational one or used in its derivation.

Given the distinction between the two modes of discourse, the question arises of whether one mode should be intrinsically preferable to the other. The answer is negative. Varela (1979, ch. 9) proposes explanatory complementarity as a pragmatic possibility. One can never hope to give a full operational account of phenomena as complex as human cognition even if it is certain that such phenomena could be logically derived from available operational statements. Such is the case of operational concept of autopoiesis [see Maturana & Varela (1980) and section 2.4] which, according to Varela, is capable of logically generating all the phenomenology of living systems but it is incapable, *per se*, of providing a cognitively satisfying explanation of specific instances of this phenomenology. An observer would have to follow an enormous body of historical contingencies (probably unknowable) to give a full operational account of a specific pattern of behavior in a given species. In contrast, symbolic abstraction may provide such a source of understanding by ignoring much of this historical detail and following a set of simpler principles.

In a recent article, Faith (1997) seems to be saying something similar to this when he also points to the practical impossibility of giving a full operational account of a cognitive system purely in terms of dynamics, state variables and attractors. One may agree with Faith in that, for understanding systems as complex as living cognitive entities, functional analysis is pragmatically unavoidable. However, Faith downplays the explanatory role of a dynamical systems approach to the study of adaptive behavior (and by extension one would add of other operational accounts of life, cognition and social phenomena) by equating this approach with Laplacean determinism and, consequently, impossible to achieve. This goes against the pragmatic spirit upheld in this work. What seems to be implied is that the *only* role an operational description or explanation can have is one where the *whole* issue of interest is addressed purely in operational terms. If one accepts the explanatory complementarity suggested by Varela, then an operational account need not be a complete or final one. It may be sufficient to derive from some basic operational givens a set of constraints which may be used in different ways: in the formulation of an operationally informed functional story, as proof that certain statements go against what is operationally possible, etc.

Therefore, it is hardly necessary, *contra* Faith, for an operational story to be useful that it be complete and final. For instance, if it is accepted as an operational given that the nervous system is a system that exhibits a closed organization (Maturana & Varela, 1980), it follows logically that it cannot operate with representations of the medium external to the organism, even if one does not know the first thing about the operational complexity of the nervous system of the particular organism in question. The statement, derived from an operational description, is telling the researcher something useful about the pragmatic value of functional explanations that postulate such representations. It will only be possible to do a limited number of things with them since there is going to be a guaranteed conflict as one starts testing their usefulness in domains where the actual mechanisms become more relevant (e.g. when building a biologically inspired autonomous agent).

This less restrictive use of operational statements is already indicating something about a possible useful relationship between the two domains of discourse. If a symbolic explanation can be formulated as an abbreviation of a network of lawlike operational relations which pertain both to the system being explained and to a more encompassing context, then *preferable functional explanations should be those which are constrained by a lack of contradiction with existing operational descriptions* (i.e. with a description of those law-like links which are abbreviated). And such constraining power may also arise from operational statements that are not aimed at providing a complete account of the phenomenon.

This kind of constraint may also be used to fill what appears to be a gap in Varela's proposal for the complementarity of both modes of explaining. There is an apparent contradiction between saying that functional explanations are symptomatic of the lack of appropriate knowledge of the context that provides the background to the system of interest (Varela, 1979, p. 65) and saying that functional explanations (in particular those of the teleonomic kind) are abbreviations of nomic relationships (Varela, 1979, pp. 67 and 73). How is it possible to abbreviate nomic relationships which are not known? Clearly, functional explanations will be especially helpful when trying to understand phenomena too complex to formulate in operational terms, at least with the tools available at the time. However, this is not to say that function cannot be constrained by existing operational knowledge even if it is not complete. Consequently, while it is possible to *postulate* functional explanations as being abbreviations of (not fully known) operational relationships in a greater context this by no means implies that the functional explanations need to be *constructed* as such. A functional account can be formulated by an observer by simply bringing forward the correlations in which she is interested. Even if one may say that the resulting functional account abbreviates nomic links there is no need to know those operational relationships beforehand. Most of the time it may be sufficient to have an incomplete operational knowledge in order to restrict, as suggested, the type of functional explanations that make sense to formulate in the chosen context. Such is the spirit with which operational accounts will be given in the present chapter and in general in the whole thesis.

Another issue that is sidestepped in Varela's account is the influence existing functional explanations can have in the formulation of new operational ones. Clearly, the latter may act as constraints for the former but it makes little sense to say that the same happens the other way around. Operational discourse must follow the logic of the elements that constitute the domain in which the phenomena of interest is assumed to be generated and the lawlike relationships between them. The laws that operate in a chemical system cannot change depending on the function assigned to that system in different contexts.

One would like to suggest that functional explanations may indeed exert an influence on how operational sense is made of a system, albeit in a much subtler way. A functional explanation may provide hints as to which is an adequate operational level of description for that system in order to explain how the associated phenomenology is generated. And so, different functional perspectives may suggest different operational levels of description.

To take an example close to the themes of this thesis, if the process of language acquisition in humans were taken to be the result of an individual capability for learning an existing system of rules and representations then one could try to formulate adequate operational descriptions in the domain of individual mechanisms which may give rise to such a capability (i.e., in terms of neurons, genes, etc.). In contrast, if the ability to participate in linguistic behavior were to be taken as arising from the interplay between individual behavior and the dynamics of social interactions (using functional terms like internalization, parental scaffolding, etc.) then one could choose a level of description extended beyond the individual as the appropriate one to give an adequate operational account. In the first case the system is bounded by the individual organism and the rest is the (relatively static) context. (This would probably give rise to a Chomskyan-type view of language acquisition). In the other possibility, the system is extended to include part of that context. (The inclusion of social dynamics as part of the system could perhaps give rise to a Vygotskyan-style operational description)[2].

Different functional understandings will provide different hints as to how to choose the boundaries and composition of the system whose phenomenology one would like to explain operationally. This is not to say, however, that this is the only criterion for preferring an operational explanation instead of another and in the example given above there may be additional reasons (besides the preferred functional understanding) for deciding for one of the cases (e.g. other operational statements which one would not want to contradict).

Some of the issues discussed in this section would deserve a further exploration which cannot

[2]For an explanation of Chomsky's theory of language learning see for instance chapter 1 of (Chomsky, 1965). For Vygotsky's theories of cognitive development see (Vygotsky, 1962, 1978; Wertsch, 1979, 1991). In none of these cases it is claimed that these theories are operational in themselves, rather that operational theories similar to them could perhaps be formulated depending on the type of preferred functional perspective.

be provided here without losing track of the main issues of the chapter. The distinction was made between two modes of scientific discourse emphasizing the point that they can be used complementarily although they should remain distinct. It has also been suggested how the two modes could be usefully related: functional discourse could be constrained by operational discourse by not contradicting it and at the same time it could suggest which is an appropriate level of description where the elements of further operational accounts should be distinguished. One important point to recall in the following sections is that insisting on addressing a phenomenon operationally should not be equated with a reductionist attitude nor should the usefulness of operational descriptions be measured exclusively by their degree of completeness.

2.3 Confusing the domains of discourse: the case of animal communication

That the two explanatory domains, the functional and the operational, may coexist in scientific practice does not imply, as insisted, a lifting of the distinction. They both serve different purposes and can be related but should not be confused. Not to fall into this trap may not be as easy as it sounds. To illustrate this point, a discussion will be presented of how theoretical evolutionary biology has addressed a paradigmatic example of animal social behavior: communication.

2.3.1 Selective advantages as a defining feature of communication

Despite the intensive attention received by biologists, animal communication remains a confused subject with "leading theorists even disagreeing as to what should be properly called a 'signal' or 'communication' " (Dawkins, 1995, p. 72). Here it will be suggested that much of this confusion is rooted on the misapplication of a functional concepts (selective advantages) into an operational domain.

Consider the following selection of relatively recent definitions of animal communication given by behavioral ecologists and evolutionary biologists.

Wilson (1975) defines communication as the altering by one organism of the probability pattern of behavior in another organism in a manner adaptive to either one of them or to both. Lewis and Gower (1980, p. 2) define communication as "the transmission of signals between two or more organisms where selection has favoured both the production and reception of the signal(s)". Krebs and Davies (1993, p. 349) define it as the "process in which actors use specially designed signals or displays to modify the behaviour or a reactor". They later make clear that they understand "specially designed signals" as those that have been favoured by natural selection. Burghardt defines it as a behavior that is "likely to influence the receiver in a way that benefits, in a probabilistic manner, the signaller or some group of which it is a member" (Burghardt, 1970; MacLennan & Burghardt, 1994, p. 163). Maynard-Smith and Harper (1995, p. 306) define a signal "as an action or structure that increases the fitness of an individual by altering the behaviours of other organisms detecting it, and that has characteristics that have evolved because they have that effect".

Although these definitions are not equivalent and are carefully phrased in order to emphasize differences in what the authors consider to be the key aspects of communication, they all share a common feature: communication is characterized in terms of selective advantages. A communicative event between organisms is such only in virtue of a history of selection of similar patterns in the behavior of their ancestors. Accordingly, any complex interaction between organisms, no matter how ritualized or similar to known cases of communication, cannot be considered to be an instance of communication until its adaptive value has been figured out or its selective history postulated. Surely, this is not a common situation. Even in evolutionary biology, where contribution to Darwinian fitness is a functional property of paramount importance in any trait, adaptive value rarely constitutes a *defining* feature of that trait. It is possible to define wings in terms of their structure and even in terms of functions relative to the observed behavior of a winged animal[3], but there is no need to say that they provide advantages for survival in order to say what they are even if one appeals to those advantages to explain why they are there.

Marian Stamp Dawkins (1995) comes close to a recognition of the confusion whose roots can be traced to a requirement that signals have been especially designed to fulfil a communicative effect. According to this, one would expect all signals to be clearly defined and even exaggerated so that their conspicuousness is augmented. But such a requirement leaves as non-communicative lots of "more subtle and unritualized ways in which animals influence each other's behaviors" (Dawkins, 1995, p. 75) such as coordinated flock movements. However, she fails to stress the inherent methodological problems created by the requirement of selective advantages.

All the above definitions could easily be questioned on methodological grounds as they present a (sometimes dangerous) mixture of definition and explanation. Communication is characterized in terms of a possible (and admittedly plausible) explanation of it. But what is "it" exactly? The danger evidently resides in the potential for tautological situations in which all instances of communication can only be explained in terms of selective advantages and if they cannot then they are not really instances of communication or if they are suspected be, then the selective advantages must be there to be "discovered". While not all circularities are necessarily vicious this one is certainly problematic because the phenomenon remains poorly characterized and one must always rely on intuitions or informal notions such as signals or information to tell whether communication is happening or not. Some evolutionary biologists might respond to this by saying that everybody intuitively knows what a signal is. Such a statement, however, would not seem to be justified by the constant succession of refinements in the biological literature to the way communication should be defined.

2.3.2 Some ill-defined terms

Interestingly, in none of the modifications just mentioned has the requirement of selective advantage been abandoned in favour of an operational definition. On the contrary, it could be argued that the history of refinements to definitions of communication has followed the path of using the property of communication as conferring selective advantages as the mould other more diffuse terms (such as "signal" and "information") should fit. Given the fact that terms like these are needed to provide an intuitive grounding which cannot be provided by selective advantages alone, trying to tie less fuzzy versions of these terms to the very property that they are supposed to complement makes the potential for vicious circularity a very real possibility.

Notice, for instance, the emphasis on "specially designed signals" in the definition by Krebs and Davies (1993). Is it not the case that, according to their own logic, if signals were not specially

[3] Although it makes little difference to the point being made, it is debatable if such non-selective functions will make sense; see for instance the work of Ruth Millikan (1984). Thanks to Mike Wheeler for pointing this out.

designed by natural selection they would not be signals (in the sense of communicative interactions) at all since they would not participate in any type of communicative behavior? Selective advantage plays the normative role of specifying which actions constitute a signal and which actions do not but at the same time selective advantage is used to distinguish communication from non-communication. In the end, what a signal is remains a mystery.

Consider another logical consequence of defining communication as a self-benefiting activity that strips the word "signal" of all its intuitive meaning. Can it be said that camouflage, the resemblance of an organism to a part of the background in order to make itself difficult to be detected by a predator, is a signal from the former to the latter? According to Hasson (1994) this would be the case. Camouflage would be a signal that "reduces" or "hides" information from the receiver. Maynard-Smith and Harper (1995, p. 307) admit that this is an odd situation but they support Hasson by saying that "the camouflage is an evolved adaptation which changes an animal that once did not signal to one that signals 'I am a leaf, or other piece of background, and not edible'." As they say, if camouflage were not a signal, then they would be faced with some hard distinctions as there is no logical dividing line between camouflage and what they consider to be more intuitive instances of signals such as warning coloration. But, at the same time, if camouflage is accepted as a signal an internal contradiction becomes apparent. A structure or behavior that supposedly has evolved in order to modify the behavior of other organisms becomes effective when it "fails" to do so. The predator "acknowledges" the signal by *not* perceiving it and acting in exactly the same way as if the signaller was not there. Communication occurs without interaction! Of course, a possible answer to this criticism would state that the predator's behavior has changed from a situation in which the prey was easy to detect to a situation in which the prey is camouflaged, and *in that sense* camouflage can be considered a signal. But these two situations can only be compared on an evolutionary time-scale, from a time where camouflage was less than a poor resemblance to the background to a time where the prey actually exploits specific features of the background to become very hard to detect. Consequently, this answer only makes the situation worse because the meaning of a signal becomes even more diluted as it is supposed to be an action or structure that operates on a behavioral time-scale and not on an evolutionary one.

Equally confusing is the meaning given to the word "information" in its semantic sense. Such a notion remains very diffuse and can be taken to signify different things for different authors. However, the concept has remained intuitively important given the reigning functional framework in which animal communication is studied. Accordingly, Maynard-Smith and Harper have recently presented a taxonomy of different types of signals according to the type of information they confer. They make clear that they refer to the semantic, "colloquial" meaning of the term rather than the technical one, (Maynard-Smith & Harper, 1995, p. 305).

Indeed, the mathematical concept of information as reduction of uncertainty over noisy channels between largely congruent systems (Shannon & Weaver, 1949) can play no semantic role (see for instance von Foerster, 1980; Oyama, 1985, p. 65). Shannon information is contentless and therefore of no use for the functional type of explanations usually sought after in evolutionary biology. It will be seen later (section 2.4.3) that it is also of little use for an operational definition of communication since once it has been explained how to get to a situation in which one may legitimately speak of Shannon information being transferred between two communicating organisms, all that needs to be explained has already been explained[4].

It is the semantic use of the word "information" that is problematic, especially when it is intended to be applied operationally. It should be clear that information in this sense lacks operational efficacy simply because it can never play a generative role in the phenomenon of communication. It is not to be found as a part of the systems involved (if these are taken to be the communicating organisms). Semantic information always refers to a nexus made by an observer between the communicative behaviors and a more general context in which those behaviors take place but which does not form part of the behaviors themselves. It is, therefore, an inherently functional concept.

So, why is this term still used in a pseudo-operational tone for making statements like "[a] signal alters the information available to the receiver" (Maynard-Smith & Harper, 1995, p. 307)? It should not be surprising to see that the answer is again related to the fact that communication has been defined in terms of selective advantages. The information gained in a communicative act is important because it has fitness value (the location of the piece of food, the presence of the predator). If it has fitness value then it is relevant to actually confirm whether, according to the conventional definition, the act was communicative or not. Information is used as a common currency to compare changes in fitness in different situations. To see to what extend this is so, one may point to what could be called "Hasson's convertibility rule", i.e. his suggestion of how changes of information should be measured in terms of changes of fitness (Hasson, 1994). According to Maynard-Smith & Harper (1995, p. 307), who endorse Hasson's criterion, a "signal alters the information available to the receiver. The change of information may be positive, in which case the fitness of the receiver is increased, or it may be negative, so that the fitness of the receiver is reduced". In fact, for Hasson the entailment is double (Hasson, 1994, p. 225); his suggestion is that changes of fitness should be easier to measure or deduce and then the corresponding change of information estimated. Such a refinement to the idea of semantic information (now a quasi-measurable quantity) follows, as argued before, the notion of selective advantages as the mould to fit.

Of course, having this use for semantic information has afforded evolutionary biologists with ammunition to argue about what sort of signals could be expected to be found under selective pressure for increased individual fitness. The pressing question becomes, for whom should information have fitness value? Given the neo-Darwinian emphasis on individual fitness, in principle this should be for the originator of the signal[5] because the originator supposedly has the choice (in the evolutionary sense) not to send the signal if it has detrimental effects. But, in contrast, the receiver also has a choice to ignore signals that carry no valuable information.

Based on this argument, Krebs and Dawkins (1984) criticize the use of semantic information in describing communicative behaviors. According to them it makes little evolutionary sense to im-

[4] Shannon information, however, may be put to practical use as a tool in ethological studies, animal communication being a particular case. The application usually involves dividing the behavioral space into a number of discrete options and estimating from observation the amount of transmitted information by correlating discrete events and subsequent reduction in entropy, (Krebs & Dawkins, 1984; Dawkins, 1995). This method, however, is not without problems, in particular those arising from the use of different choices in the partition of behavioral spaces.

[5] Notice, by the way, how easy it is, when an operational characterization is lacking, to fall into the simplistic assumption that signals are discrete behavioral events which necessarily *have* a single originator. Think of howling in wolves. See chapter 9 for other natural examples and a dynamical model where statements of this kind cannot be so easily made.

prove the efficacy of a signal in conveying information if the originator is not going to benefit from the production of that signal. For them, it makes more sense to dispose with the notion of information altogether and consider communication as actions with which individuals manipulate others by making them behave in ways which are convenient to the manipulator. Forgetting for a moment that this approach may have problems of its own[6], one may ask instead if the notion of information is completely disposed of. Initially this would seem to be the case. But when the complementary aspect of manipulation is considered, which Krebs and Dawkins call "mind-reading", information slips back in. Mind-reading is intended to account for the capacity to predict the future behavior of an animal by observation of its present behavior and therefore it suggests an informational interpretation. Mind-readers take advantage of the manipulative signals of other animals and act according to their own benefit. But this is tantamount to saying that they assign an informational content to those signals; a content which is clearly contextual on the current pool of behaviors in the population but which is informational nonetheless. The ensuing co-evolutionary arms-races that may occur according to Krebs and Dawkins are indeed possible because the reliability of the information gained through mind-reading is under the selective control of the manipulators. Consequently, from the mind-reader perspective, manipulation is about the transfer of information or, rather, the distortion of such information[7].

Here too, as with Hasson's convertibility rule, the understanding of information, a notion originally invoked to intuitively complement a definition of communication poorly characterized by selective advantages alone, is made dependent upon selective advantages in a way that it almost acquires operational efficacy in contradiction with its functional origins. As with the notion of signal, information becomes so tied to the idea of adaptive value that it makes it even more difficult to act as a grounding for the concept of communication.

2.3.3 Practical consequences for research

What does it mean to study the evolution of communication under this framework? For the traditional standpoint the ability to communicate is an established fact. This is paradoxically so even when the issue of interest is the *origin* or *evolutionary change* of a communicative system. What is meant by this is that the issue that is usually addressed in conventional research practice is the evolution and stability of *content* in communication. Or, more specifically, how a selective scenario is capable of imposing meaning on a set of behaviors which initially have none. In most models on the evolution of animal communication the organisms/agents always have the ability to send a signal, make a move in the game, produce a warning call, etc. The form and structure of communication are always pre-defined and pre-existent. What these models seek to find out, be they verbal, mathematical or computational, is what kind of signal, which move or warning call the organisms/agents will produce in different situations after many generations of selective pressure and not how the communication system ever came to be.

In spite of the inherent paradox, such a methodology is far from futile since according to the

[6] For instance, distinguishing manipulation from cooperative communication depends on how one defines a conflict of interest, which may be problematic in itself.

[7] Notice that the argument does not really depend on the occurrence of an actual arms-race. As Maynard-Smith and Harper note (1995, p. 305), "it is not evolutionarily stable for the receiver to alter its behaviour unless, on average, the signal carries information of value to it. The fitness of the receiver need not be increased on every occasion that it responds to a signal".

results the researcher could derive interesting conclusions about other issues. As an example, the evolutionary stabilization of altruistic signalling can tell the researcher under what circumstances cooperative behaviors will obtain (e.g. when agents communicate with relatives or when signals are costly, etc.). And so the approach can be useful for answering these questions. But it is clear that one is missing the chance of investigating other important issues, such as why should an agent respond to a signal in the first place, why should a particular game be played at all, how can novel games appear in evolutionary history, are there any constraints in the form and structure of communicative interactions, etc.

What it is being suggested is that the main reason for questions like these being rarely asked in research practice has to do with the influences of the traditional theoretical understanding of communication. As a response, it could be argued that this is not the case. Maybe most researchers find the question of the origin of communicative behavior not as interesting as the question of the functional role played by an evolutionarily stable set of signals in pre-existing communicative patterns. Or maybe the current tools of research do not allow one to address issues such as why should an agent produce and "interpret" certain behaviors as signals in the first place, etc. Maybe the traditional theoretical framework is blameless after all.

It could be. Clearly, it is not possible to give a definite answer to this argument. But, if one considers what sort of phenomenon is described in the traditional view (apart from all the contradictions and problems previously pointed out) it is clear that what is highlighted of communication is the ability to provide adaptive meaning to certain types of interactions in the form of transfer of information or in the form of exploitative manipulations and that such ability must provide (generally individual) selective advantages as the definition of communication requires. If this is what is relevant about communication then the type of research carried out so far should not be unexpected because the questions being asked are precisely the natural questions that the framework suggests, i.e. questions about informational content, about function and about selective stability of communicative behaviors[8].

As for other practical consequences of this type of definition of communication, it is clear that providing one possible explanatory factor with the credentials of being the defining feature of a phenomenon introduces a class distinction in the domain of explanations. Accordingly, any factor other than natural selection which may be relevant for explaining the operation and evolution of a communicative system becomes *ad hoc* and second rate. If this were just the criticism of a purist, then maybe the situation would not be pragmatically very damaging since, as some biologists would probably believe, those additional explanatory factors could indeed be second rate or *ad hoc* in the face of natural selection. But the evidence points in the opposite direction. The assertion that behaviors with a strong interactive component (such as social behaviors) can only be explained in terms of individual selective advantages is constantly being challenged and other factors arise at the level of the dynamics of dyads, groups and whole populations that constrain the action of natural selection in ways that they become elements of equal or greater importance in understanding the final evolutionary outcome[9].

[8]More concrete evidence of how the informational exchange metaphor has affected the construction of many computational models is given in chapter 8. The basic design specification in these models is that information should be unequally shared because otherwise there is no reason for communication to arise. The model presented in that chapter is meant to serve as a counterexample in which this requirement is abandoned.

[9]Examples of how such factors can be manifested are given in the models presented in chapters 6, 7 and 9 where

Surely, if these problems are to be avoided it is necessary to look for ways of operationalizing the definition of communication, thus dodging the circularity of defining it in terms of functional properties which depend on what communication is. Once this is done, functional accounts and selective stories will certainly become key ingredients in understanding features of specific communicative systems. But if the operational definition is lacking these functional accounts will remain grounded only on intuitions and will provide a poor notion of what communication is in terms of what it (supposedly) is for.

2.4 A systemic perspective on social behavior

The previous section shows an actual and quite relevant example of the kind of problems that may arise when the operational and functional domains of discourse are confused. For this reason, it is the purpose of the present section to introduce a purely operational perspective on social behavior in autonomous systems and then come back to the issue of animal communication.

Before proceeding, the reader should be reminded that the purpose of offering an operational account is not necessarily to act as a total replacement of functional statements. This may be too narrow a point of view given the earlier discussion regarding the purpose of an operational explanation as acting as a constraint to, rather than a replacement for, possible functional interpretations. For this reason, it is a worthwhile enterprise to try to identify such operational constraints using dynamical systems theory and other systemic concepts even if a complete dynamical description cannot be given in concrete cases. This is particularly so in the light of the pragmatic claim that a theory of social behavior should be biologically grounded. The purpose of that claim was not for the researcher to be able to provide ways in which social behavior could in general be reduced to biology, but rather to constrain a theory of social behavior within the realm of biological plausibility. Therefore, if biological grounding is to serve a constraining purpose, it makes sense that this should be done using an operational account.

2.4.1 Basic concepts

When one thinks about social behavior the first thing that comes to mind is some notion of coordinated activity between two or more autonomous entities. In order to understand what it is meant by this it is necessary to describe the meaning of autonomy, interaction and coordination.

When speaking about a system in general a distinction will be made between its structure and its organization following (Maturana & Varela, 1980, p. 77). By *organization* of a system it is understood the set of relations that define that system “as a unity, and determine the dynamics of interactions and transformations which it may undergo as such a unity”. In contrast, *structure* refers to “[t]he actual relations which hold among the components which integrate a concrete machine [or system] in a given space”. The organization, therefore, does not specify the properties of the specific components that realize the system, it only defines the relations that these components must hold in order for the system (or machine) to belong to a given class. Thus, the organization of a car may be described as being of a certain size, with four wheels placed in a certain way, and engine that provides power for moving the wheels, a set of controls, and so on. The structure of

the influences of spatio-temporal dynamics, social development and embodiment are investigated respectively.

a particular car refers to the actual physical properties of the components that realize it as an instance of the class of cars. It will include details regarding bodywork, state of the engine, mileage, plate number, etc. It follows that many concrete systems may exhibit different structures while preserving a same organization[10].

The word "structural" originating from this distinction is well defined if one is dealing with a material system. This is how the term will be used in this chapter; in particular, when speaking of an organism's or an agent's structure, this will refer to the set of components that constitute the whole of its body including its nervous system or controller. In contrast, the term would not be as clearly defined if one is dealing with non-physical systems which are presumed autonomous (say, a financial market), which for this reason are left out of the discussion.

It will be assumed that, at any moment, the state of a well defined dynamical system is completely determined by a previous state and by its structure at that moment. This is the hypothesis of *structural determinism* (Maturana & Varela, 1987, pp. 96 - 97). If this is not so, then either the system is not well defined and its boundaries must be extended or reduced, or the observer is not in a domain where it is possible to speak of the regularities of the operation of the system as something to be found in the properties of the system (for instance, if the operation of a system could be explained by supernatural causes). The latter option would make a scientific approach very difficult if not simply impossible, (see Maturana, 1978, p. 34).

Autonomy can then be defined in non-functional terms as the property of the organization of a system of being operationally closed[11]. This does not mean that the system does not interact with its external environment. It means that the organization of the system is defined by a network of internal processes and that the operation of this network is sufficient for the those constituting processes to be generated and sustained (constituted) without any of them being driven from outside the system. However, the autonomous system is never isolated from its context. This would be a thermodynamic absurdity which would result only in the trivial case where the system does not change at all. Hence the emphasis on the term "process" which is meant to highlight the dynamical character of the definition and which places autonomous system always far from their thermodynamic equilibrium. In what sense can then a network of processes be operationally closed? In the sense that although the system relates to its medium through other processes (perturbations, exchanges of matter and energy, etc.) these do not constitute part of the organization of the system.

The *identity* of the system is, at the same time, defined as long as it remains operationally closed, (Varela, 1979, p. 57). This is possible because autonomy becomes an invariant of the system, i.e. a way of being able to point unequivocally to the same system or the same class of systems. As soon as autonomy is lost, the operation of the system is "opened" by allowing processes that were once external to the closed network to enter into the internal operation. Consequently, its identity becomes ambiguous.

This characterization of autonomy is general, living systems constituting a particular case where the processes involved are processes of production, transformation and destruction of components in the molecular space including a boundary as a special component, in which case the organizational property is called *autopoiesis* (Maturana & Varela, 1980, pp. 78 - 79).

What sort of relation can an autonomous system have with its environment in order to remain

[10] See (Varela, 1979, pp. 8 - 12) for further discussion on the duality between structure and organization.

[11] In fact, this is a thesis rather than a definition, (see Varela, 1979, p. 55).

autonomous? It is clear that as soon as this relation is one where the closure of the internal organization of the system is disrupted from the outside, autonomy will be almost certainly lost[12]. Preservation of autonomy divides the space of possible interactions into those that are allowed and those that are not, and this space is obviously contingent on the present state of the system. Allowed interactions will be manifested as perturbations to the system that do not break its operational closure and not as instructions of the dynamical path that the system must follow. A process whereby the system interacting with its environment undergoes a succession of allowed perturbations (resulting in changes in its structure) without losing its autonomy is called a process of *structural coupling*, (Maturana & Varela, 1980, p. xx). As long as the organization of a structure-determined system is conserved during its coupling with the medium, the system remains distinguished from the medium and operates independently (i.e. its dynamics is always a consequence of its own structure at a given moment). Obviously, this conservation of organization does not entail a conservation of structure since, as noted earlier, different structures may realize a given organization. This is why, during structural coupling, the structure of the systems involved does indeed suffer changes.

As long as the present structure can be realized without a disruption of autonomy, the system is said to be *adapted* to its medium. Structural coupling, then, is equated with conservation of adaptation and loss of adaptation with destruction of the autonomous system or transformation into a different one[13].

Concepts similar to the ideas of structural coupling and adaptation were introduced in the middle years of cybernetics, (Ashby, 1960; Pask, 1961). Ashby arrived at a similar definition of adaptation in terms of stability and homeostasis: "... a form of behaviour is adaptive if it maintains the essential variables ... within physiological limits" (Ashby, 1960, p. 58). And a generalized version of structural coupling is used in physics to describe coupled systems governed by equations, where it can be easily translated into the rule that distinct systems can affect each other by perturbing parameters but not variables in a direct way (otherwise the systems are not really distinct)[14].

[12]Consider the nervous system as a candidate operationally closed system where the internal constituting process is the recursive transformation of relative states of electrical activity of the neurons. If one wants a dog to perform a trick and one imagines that this can be achieved by inserting electric currents in specific locations of its brain, then in that case the dog's nervous system will cease to act as operationally closed. The same happens if the system is prevented from operating normally, for instance if an animal is anaesthetized and clamped in order to measure the response of its neurons to certain stimuli.

[13]As with other relevant concepts, such as operational closure, adaptation seems to be an "all or nothing" feature of the system (in this case, in relation to its medium). This characteristic plays an important role in the re-interpretation of evolutionary change as "natural drift", see chapter 3 and (Maturana & Varela, 1987, ch. 5) and (Varela, Thompson, & Rosch, 1991, ch. 9). Similar "all or nothing" interpretations of adaptation have recently been used to explain the dynamics of speciation as percolation in a two-state (viable and non-viable) genetic hypercube (Gavrilets & Gravner, 1996; Gavrilets, Li, & Vose, 1998). However, these clear-cut distinction can introduce some problems when one wants to account for processes where organizational properties can be said to change *gradually*. Although it remains clear that, according to the above definitions, a system is either autonomous or not, is either adapted to its medium or not, one would like to be able to speak of changes that are certain to drive a system, for instance, towards the loss of its autonomy (e.g. a serious wound). Such language is sometimes used by the Maturana and Varela on different occasions. They refer to "facilitation" of autopoiesis (Maturana & Varela, 1980, p. 108) and Varela finds it useful to speak of "minor" and "major" breakdowns in autopoiesis (Varela, 1997, p. 80). This seems to be a blind spot in the theory but not an unrecoverable one. However, since the purpose here is to present the basic concepts, the existence of problems like this will be merely pointed out without attempting to come to a resolution within the current context.

[14]This is by no means the only restriction, see (Ashby, 1960) particularly chapters 19 and 21 for discussions on state-determined systems and parameters.

2.4.2 Coordination

Structural coupling occurs between an unity and its medium which may include other autonomous unities, in which case one can speak of an *interaction* between them. However, mere interaction between autonomous entities does not seem to be sufficient to describe the resulting behavior as social even if it happens to have an adaptive function. There is something lacking in two animals just bumping into one another while trying to escape from a predator to call *that* a social interaction. What is being looked for is a concept that will allow a description of the complex patterns of social behavior observed in humans and other species. This is the idea of *coordination* or *orientation* [15]:

> "An organism can modify the behavior of another organism in two basic ways:
>
> (a) By interaction with it in a manner that directs both organisms toward each other in such a way that the ensuing behavior of each of them depends strictly on the following behavior of the other, e.g.: courtship and fight. A chain of interlocked behavior can thus be generated by the two organisms.
>
> (b) By orienting the behavior of the other organism to some part of its domain of interactions different from the present interaction, but comparable to the orientation of that of the orienting organism. This can take place only if the domains of interactions of the two organisms are widely coincident; in this case no interlocked chain of behaviour is elicited because the subsequent conduct of the two organisms depends on the outcome of independent, although parallel, interactions.
>
> In the first case it can be said that the two organisms interact, in the second case that they communicate." (Maturana & Varela, 1980, p. 27 – 28)

According to Maturana and Varela, a behavior can be considered communicative when a pattern of interactions elicits some form of coordination between the participants. But what does this mean exactly? Coordination is a subtle concept. In one interpretation it involves the fact that many organisms can have a complex behavioral repertoire that allows what, for an observer, seems to be a simultaneous instantiation of different behaviors, (for instance, walking and visually scanning for the activity of a moving con-specific). When two or more organisms are interacting only a part of this behavioral space may be occupied directly in the interactive activity (say, keeping mutual visual contact). However, if a coherence is observed between behaviors *not* involved directly in the interaction (say, walking together), then one is in the presence of coordination. Another, equally valid, interpretation would *not* require that the different behavioral domains, the one in which the interaction occurs and the one in which coordination is elicited, be *simultaneously* instantiated but coherence in the latter still needs to show dependency on the outcome of the interactive activity in the former. Such would be the case of the coordinated response of a group to an alarm call.

By coherence it is meant an observable agreement between behaviors of different organisms. Such an agreement may range from simple instances of synchronized activity or other types of temporal consistency (such as the group response to an alarm call just mentioned) to more complex cases such as the patterns of large prey hunting by members of a wolf pack or the approaching behavior and maintenance of the pair bond in monogamous species of tropical birds by means of antiphonal duetting (see section 9.2). One may ask why should there be any relation between

[15] The term "orientation" will be reserved, when possible, to refer in the model presented in chapter 9 to the *angular* orientation of moving agents.

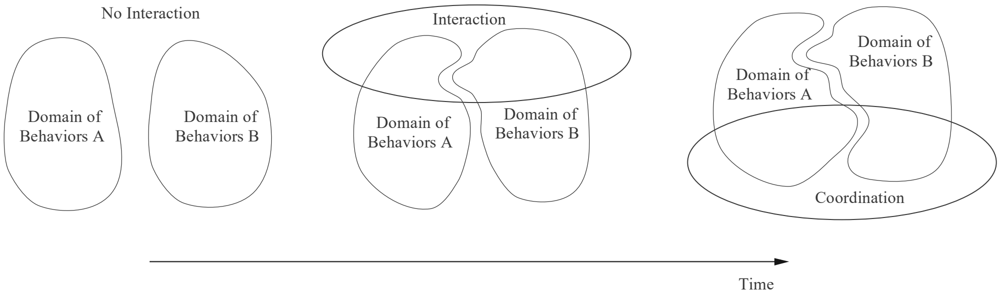

Figure 2.1: Illustration of the concept of coordination.

the coordinated behavior (in the last example, song synchronization and approaching) and the interaction (singing), unless both organisms were somehow congruent enough in structural terms so that 1) the coordinated behavior is *possible* for both of them, and 2) their structures are such that the coordinated behavior is somehow related operationally to the fact that they are undergoing a specific pattern of interaction.

See figure 2.1 for an illustration where the state of the behavioral domains of two organisms in interaction is shown through time. Each closed curve is meant to represent the behavioral domain of an autonomous system. Coherence is depicted by correspondence in shape between the two curves. Interaction is shown as a single activity in which both organism engage through structural coupling (top). Coordination is shown as additional coherence which depends (operationally) on the *existence* of interaction and the internal operation of each system (bottom part).

It would be preferable to avoid a possible reading of the above quotation which is considered misleading. Paragraph (b) in the quotation could be taken as saying that during coordination there are necessarily two different roles, the orienter and the orientee, and that the former orients the latter towards a interactional mode *previously specified* by it. This would entail an asymmetry in the coordinating systems, which need not be the general case. When the behavior of the orientee is changed towards a form of interaction which is comparable to the orientation of the orienter, this should generally be interpreted as the orientation of the orienter *once coordination has been achieved* and not necessarily before. The last case, although possible, is not general and could be wrongly interpreted as an intention on the part of the orienter in modifying the behavior of the orientee.

This is why in figure 2.1 the attainment of coordination is represented as a coherence in the behavioral domains of the participants in the "area" of their behavioral space (bottom) where a predefined disposition is not necessarily there in the first place. Figure 2.2, in contrast, shows a *particular* case where one of the participants already has a strong behavioral disposition towards a specific sub-domain of its behavioral repertoire and the other participant coordinates its own behaviors accordingly. Such asymmetries, although not the general case, are not uncommon either. A comment will be made below on one possible cause for asymmetries of this kind being a difference in ontogenetic stages between the coordinating organisms and their corresponding difference in susceptibility to plastic changes (e.g. in the case of mother and offspring).

Each perturbation that an autonomous system undergoes during structural coupling induces structural changes in it and some of these changes may be plastic. Plastic changes occur when

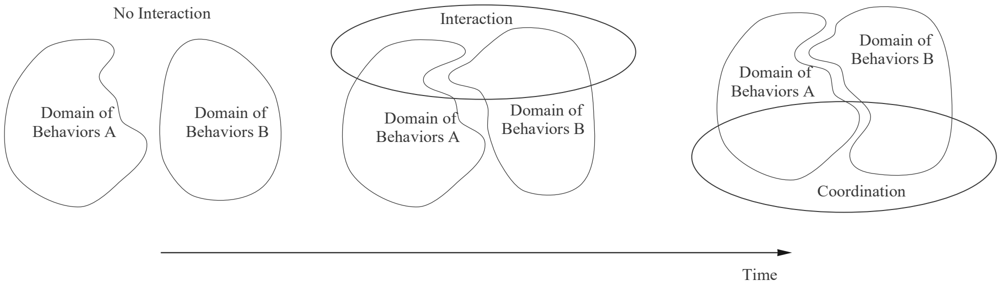

Figure 2.2: A special case of coordination through interaction where system B is oriented towards system A.

the structure of a system undergoes an alteration from which it does not recover within the same time-scale with which the change happened but with a much longer one. Clearly, some of these changes may be permanent.

The domain of coordinated behaviors established by two or more autonomous unities in structural coupling, during the course of which the systems mutually trigger in each other plastic structural changes, is sometimes referred to as a *consensual domain* (Maturana & Varela, 1980, p. 120) to emphasize the fact that although the operation of each system always depends only on its own structure, the observable behaviors arise as a consequence of an interlocked history of mutual structural perturbations between the systems; and so coordinated behaviors seem to depend on (or connote) this history. *Communication* as defined in the last quotation is coordinated behavior in a consensual domain.

The process of coordination in autonomous systems is, at the same time, a process of mutual selection of plastic changes in their respective structures, so that not only the ensuing behaviors result in a coherent pattern but also the corresponding structural changes may show some degree of coherence. The resulting relation between the structures of the coupled systems is known as *structural congruence* and it is to be found particularly between organisms that engage in interaction repeatedly and recursively. Sustained patterns of interaction tend to become embodied in the participants in the form of a history of plastic structural changes during each individual's lifetime. As a result, following encounters may be affected either in ways that facilitate the reproduction of the pattern of interaction or in ways that do not. If facilitation of future encounters is the result of certain patterns of interaction, it is clear that those patterns will tend to be conserved. For certain plastic systems this process could constitute the basis of social affinity, (an example is presented in chapter 9). In such cases quite complicated patterns of coordinated behavior can follow from very simple initial interactions just because the systems are already "tuned" to one another.

Of special interest is the case in which structural congruence is achieved between unevenly plastic organisms, as in the case of parent/offspring social interaction mentioned before. If structural congruence is understood as the meeting of two distinct, though not completely dissimilar, structures in some common ground, it is clear that those interaction patterns that facilitate their own reproduction by inducing structural changes, will tend to produce what for an observer would look like a *directed* structural change in the more plastic organism towards a structure that is congruent with the less plastic organism. This phenomenon and its opposition (the structural "rejection" of patterns that make their own reproduction difficult) could be used as an operational basis for explaining many instances of social learning. This matter, however, will not be developed in the current context

It should be stressed that there is nothing magical about coordination. Consider one of its possible manifestation in rhythmic types of behavior: entrainment or synchrony. There is a growing literature on synchronization of coupled oscillators in biology and chemistry (see Winfree, 1980; Kuramoto, 1984, for "classic" introductions). The striking fact is that under a vast set of conditions synchronization is the expected result. Additionally, coordination in rhythmic behavior is not just manifested in phase-locking, but more remarkably in tendencies to correct phase deviations. Kelso calls this phenomenon *relative coordination*, (Kelso, 1995, p. 100 and ff.). Entrained behavior may be difficult to maintain in the case where the interacting systems are not very similar. However, the systems, under certain conditions, may manage to compensate phase slippage. Such is the case of an adult and child walking together at the same speed in spite of differences in their individual "natural" speeds. In spite of being able to provide an operational description, in many natural cases an observer would tend to *interpret* this compensating coordination as if the systems involved had an "interest" in maintaining certain types of interaction.

2.4.3 Changing the practical issues of interest: animal communication again

How much of a difference will the adoption of a theoretical framework such as the one described above make in practice? Is it possible that, after becoming convinced by arguments that the above is the "right" theoretical stance to adopt, both research interests and methods utilized will remain unchanged? These are important questions because they will help in deciding whether this is a purely argumentative exercise or whether it has pragmatic consequences for the way research is conducted. Some of these consequences will take the form of dissolution of problems which arose as part of a confusion between operational and functional discourse. But, perhaps the most interesting repercussion of adopting the systemic perspective is the opening of new issues that can be investigated within this framework but were traditionally by-passed because of the use of functional primitives in a pseudo-operational manner.

In order to explore some of these issues more specifically, one may concentrate again on animal communication. The given description of the phenomenology of social behavior and its explanation from a systemic perspective is fairly general and should be able to account for all types of social phenomena in autonomous structure-determined systems. To compare some of the practical consequences of this perspective with those of more traditional ones it is necessary to describe in more detail what is highlighted by the term "communication" within this framework and in what aspects it differs from the conventional understanding. This will be helpful for distinguishing some important shifts of interests.

Consider what would be the natural questions to ask when communication is described from the systemic standpoint in comparison to the discussion given in section 2.3.3. As mentioned before, a characterization of communication in terms of transfer of information is problematic in itself and especially so under the systemic perspective where autonomy is equated with operational closure and therefore interactions of the instructive kind (an animal telling the other one what to do) are not allowed if autonomy is to be preserved. So how is it possible to account for instances in which it seems right to say that an animal is telling the other what to do and the other one is responding accordingly (say, a mother signalling for an infant to come closer)? Despite being a legitimate problem in the traditional view, this question was rarely asked because it never became apparent that there was a need to answer it. In the systemic perspective such observations demand a deeper investigations of the behaviors involved.

Another related issue that is highlighted is the issue of denotation. In the informational view, information is a stuff that is intended to play an pseudo-operational role. Such a role is given by the (to this point magical) ability of communicative behaviors to denote; so that it can be said that such and such a signal denotes an object, an emotion or a state of affairs. But if information is to be deprived of this pseudo-operational role, as the systemic view insists, then denotation, as a primitive operation, becomes mysterious. How can it be accounted for? Again, from the traditional standpoint there was, in all appearance, never a need to formulate this question. Both denotation and the apparent instructive character of certain types of communication were never evident as problems in virtue of the mixture of operationality and functionality of primitive terms like information. But once such use for this and other terms has been criticized in the systemic view, these issues become pressing .

Consider denotation. What sort of operation/behavior is it? Indeed, it is possible to understand it in systemic terms but this involves the recognition that it is not a primitive operation but requires an agreement (consensus) which can only be attained in an existing consensual domain, (Maturana, 1978, p. 50). An operational understanding of how such a consensual situation is achieved becomes *the* issue of interest. This point is repeated by Maturana and Varela in a comment on the evolution of language:

> The understanding of the evolutionary origin of natural languages requires the recognition in them of a basic biological function which, properly selected, could originate them. So far this understanding has been impossible because language has been considered as a denotative symbolic system for the transmission of information. In fact, if such were the biological function of language, its evolutionary origin would demand the pre-existence of the function of denotation as necessary to develop the symbolic system for the transmission of information, but this function is the very one whose evolutionary origin should be explained." (Maturana & Varela, 1980, p. 30)[16]

Similar themes reappear if one tries to make the case for a more formal use of the term information as reduction of uncertainty, as mentioned earlier. If one grants that certain communicative systems can be thought of as constituted by a sender and a receiver who are connected via a channel and for every change in the state of the sender, a single new state is generated in the receiver, then it must be assumed that both sender and receiver possess some degree of operational congruence because otherwise the correspondence between states could only be a consequence of instructive interactions (the emitter specifying the new state in the receiver instead of this new state being specified autonomously by the receiver's structure) which must be discarded if autonomy is to be preserved. Two extremely dissimilar systems cannot be in this situation. Maturana (1978, p. 54)

[16]Maturana and Varela are able to explain denotation in systemic terms. That explanation, though, is beside the point that is intended here and too complicated to treat fairly. Maturana, in his solo writings and in a few collaborations, goes on to extend this explanation to account for the capability of linguistic creatures to become observers and self-conscious. The reader is referred to (Maturana, 1978, 1988a, 1988b; Maturana, Mpodozis, & Letelier, 1995).

describes the situation as a homomorphic relation between the domains of possible states in the sender and receiver. Such homomorphism must be assumed to exist by the observer unless she designed the system herself.

If such homomorphism, in fact, exists then any interaction can be trivially considered as an instance of communication and information makes sense as the degree of reduction in the uncertainty produced by the interaction under noisy circumstances.

> "What is not trivial, however, is what takes place in the process of attaining communication through the establishment of ontogenic structural coupling and the shaping of the consensual domain. During this process there is no behavioral homomorphism between the interacting organisms and, although individually they operate strictly as structure-determined systems, everything that takes place through their interactions is novel, anticommunicative, in the system that they then constitute together, even if they otherwise participate in other consensual domains. If this process leads to a consensual domain, it is, in the strict sense, a conversation, a turning around together in such a manner that all participants undergo nontrivial structural changes until a behavioral homomorphism is established and communication takes place. These precommunicative or anticommunicative interactions that take place during a conversation, then, are creative interactions that lead to novel behavior. The conditions under which a conversation takes place (common interest, spatial confinement, friendship, love, or whatever keeps the organisms together), and which determine that the organisms should continue to interact until a consensual domain is established, constitute the domain in which selection for the ontogenic structural coupling takes place. Without them, a consensual domain could never be established, and communication, as the coordination of noncreative ontogenically acquired modes of behavior, would never take place." (Maturana, 1978, pp. 54 - 55)

Paradoxically (but, in fact, not so), it seems that one of the most interesting issues in the ontogeny and evolution of communicative behaviors is what happens *before* they become established. What sort of interactions can lead to coordinated behavior? How are they related to the other activities of the organism? How do these behaviors influence future encounters (for instance by promoting spatial proximity or by triggering structural changes that favour the repetition of the same behaviors)? Related to this, it is also possible to ask questions regarding the occurrence of novelty in existing communicative systems. It would be of interest to inquire about the nature of those interactions that somehow manage to "escape" from an existing consensual domain and produce a disruption which may eventually create a new domain of communication.

The question of origin becomes the question of the establishment, either in evolutionary or in ontogenetic terms, of the consensual situation that allows for communicative behaviors to exist at all. The original functional questions may still be asked, but the answers will be partly grounded on how the consensual domain is established in each case, i.e. giving ample space for issues related to embodiment and natural and cultural history to enter naturally in the picture. Similarly, the study of evolutionary change is enriched by the introduction of a mechanism that allows for novelty and creative interactions to take place and play a role that does not exist in the traditional framework. Pre-communicative behavior is historically antecedent to the establishment of the consensual domain and it is at the same time a constraint and a medium of realization of the structure of communicative behaviors. As evolutionary change can also occur in these pre-communicative stage, novelty is not restricted to a change in the repertoire of signals an animal uses, but it is expanded to include non-communicative behaviors which can become part of a communicative event[17].

So far this shift of interest seems to provide with positive additions to the set of questions that are relevant to investigate. This is particularly so because the original questions regarding the functional aspects of communicative phenomena and their selective contribution need not discarded, although some of these questions will surely need reformulation. But is it also possible to identify what may look like "negative" pragmatic consequences in the adoption of the systemic framework? It is clear that a focus on the achievement of structural congruence and behavioral homomorphism through sustained structural coupling posits a greater emphasis on communication as a species-specific activity while many inter-specific cases of communication in the traditional view would probably need to be recast as non-communicative.

The emphasis in the systemic literature seems to be on the attainment of communicative situations purely at an ontogenetic time-scale, i.e. through sustained interactions in the life-history of individual organisms belonging to a group. It is because of this that the systemic perspective seems more focused on species-specific, ontogenetically attainable, forms of communication. However, it is possible to extend the operational arguments to account for similar phenomena occurring at an evolutionary time-scale although the transition is not trivial. This transition involves the fact that certain patterns of species-specific behavior become stable through successive generations by being selected or by influencing the likelihood of being themselves reproduced (see previous section). These patterns become identifiable (at least in principle) for an observer that is able to witness (or deduce) their resurgence generation after generation. As such, these patterns are therefore subject to changes which can be traced through evolution. However, the origin of those changes remains the same as in the ontogenetic scale. In the case of communicative systems, the origin of evolutionary novelty in communicative patterns is precisely the same pre-communicative interactions plus (re)-attainment of consensuality that generates novelty on the ontogenetic time-scale. This need not be interpreted as a form of Lamarckism because it is not being said that all ontogenetic novelty will necessarily re-appear in future generations, but that evolutionary novelty in communicative behaviors must first appear on the ontogenetic and behavioral time-scales and when they do, they do so in the form of pre-communicative interactions as described in the last quotation by Maturana.

In principle, similar arguments could be advanced for the case inter-specific patterns of communication. But a general treatment here is more difficult in virtue of the inherently dissimilar structures frequently presented by organisms of different species. The domains of consensuality must rely on the degree of potential congruence between the participants in communication. To achieve this across species requires at least some pre-existing dimension where these domains can be attained. It is indeed possible to observe this across species sharing a common habitat and a joint history. An example close to home is the ability of human beings to establish communicative domains with other animals such as dogs whose own social codes are easily interpretable even by small children and for whom many human moods and intentions seem to be quite transparent.

[17]The closest that the traditional framework comes to a treatment of these issues is in the discussions regarding signal ritualization (Krebs & Dawkins, 1984) as originating in pre-existing non-signalling behaviors thus following the principle of *derived activities* (Tinbergen, 1952). Accordingly, some present day signals in birds are derived from non-signalling activities such as preening or feather-settling (Krebs & Dawkins, 1984).

2.5 Summary

The reader may have noticed that many of the presented theoretical concepts regarding the logic of autonomous systems would benefit from a development deeper than the one it was possible to offer here. Moreover, much of this conceptual framework has found a more sophisticated expression in recent years, especially in connection with what is known as the enactive approach to the cognitive sciences (Varela et al., 1991) as well as in specific areas of neuroscience and theoretical immunology[18]. Enaction combines the main precepts of biological autonomy with another fundamental ingredient: embodied experience. A whole new angle is thus opened for the study of cognition, which breaks with the traditional computational approach. In spite of the lack of an extensive treatment in these pages, these ideas should not be interpreted as exceeding the thematic interest of the thesis. Quite on the contrary, there is a whole area for fertile development of issues regarding social behavior, cultural history, language and intersubjectivity within the framework provided by the biology of autonomous systems and the enactive approach. These issues, however, do exceed the practical limits of the thesis and therefore they are only mentioned here.

It was practical concern as well which prompted the claim that any framework used for studying social behaviors should be provided with a proper biological grounding intended to act as a constraining rather than as a reductive factor. What kind of biological grounding became clear after the introduction of the distinction between operational and functional discourse. Only after understanding the possible relations between these two modes of describing and explaining it was apparent that operational statements cannot in general work as a replacement of functional ones, but they can indeed be used as constraints to the latter by requiring that a functional account be in accord with what is operationally known about the systems giving rise to the phenomenon of interest. Functional explanations and descriptions, in contrast, can only exert a much subtler influence on operational discourse, by suggesting appropriate levels for distinguishing the systems and components which generate the phenomenon. It is because of this asymmetry that only operational explanations are deemed appropriate to fulfill the task of grounding social behavior in biology.

The actual need for providing this grounding was exemplified by the current state of confusion surrounding the issue of animal communication as viewed from the classical framework. Some methodological and conceptual problems of not properly distinguishing the two modes of discourse were examined and it was found that the main defining feature of communication was not an operational one but a functional one: selective advantages. The behavior proper was only intuitively characterized by making use of terms like signals and information. Ironically, much of this intuitive grounding is lost when an attempt is made to formalize these concepts *in terms of* selective functionality. This results in logical contradictions as well as in a poor characterization of communicative behaviors. The consequences of this go beyond the lack of a consistent theoretical framework, something quite grave in itself, and are manifested in the way the origins and evolution of communication are addressed in practice and the sort of questions that are asked and the sort of questions that are avoided. Thus, the evolution of communication is mainly viewed as

[18] See for instance (Varela & Stewart, 1990; Varela & Coutinho, 1991; Varela, Stewart, & Coutinho, 1993, and others) for an approach to immune networks based on autonomy and dynamical closure, (Thompson, Palacios, & Varela, 1992) for an enactive approach to color perception and (Neuenschwander & Varela, 1993) for an example in the neurosciences.

the evolution of an appropriate selection of a repertoire of actions or signals within a pre-existent behavioral context whose origins are rarely questioned.

Fortunately, the systemic perspective on the biology of autonomous systems has provided an appropriate body of operational theory that serves the intended purpose of grounding social behaviors biologically. After explaining key concepts such as the difference between structure and organization, autonomy, structural coupling and adaptation, it has been possible to introduce the notion of coordination as the achievement through interaction of a consensual state of orientations in the behavioral domains of the systems involved. Thus, systems undergoing sustained structural coupling are able to mutually trigger changes in their respective structures in ways that certain homologous areas in their spaces of interactions become coherent in spite of not being necessarily involved in direct coupling between the systems. Such coordination observed in the behavioral domain is accompanied by a state of congruence in the structural domain of the systems.

Communication, defined as behavior in a consensual domain established through coordination, can therefore be characterized in operational terms. An interesting consequence for research of adopting this definition is that it highlights a set of questions which were not actively pursued in classical investigations while, at the same time, many of those questions that were pursued can still be asked, perhaps after being re-formulated. Thus, the focus of interest is shifted from questions regarding what sort of action or move in a game is going to be selected to fit an existing behavioral slot, to an inquiry into how do those slots come to exist in the first place, be that in behavioral, developmental or evolutionary terms.

The presentation of the systemic view on social behavior, as well as the justification of its scientific potential, is intended partly as a goal in itself. This view will also be used as a guide for the concrete investigations offered later in this thesis. However, some of these investigations are not necessarily rooted directly on the systemic framework. An actual direct test of its suitability for research will have to wait until chapter 9 where the notions of coordination and structural congruence will play a fundamental explanatory role.

The rest of the concrete models will explore other derived aspects of the general systemic framework, which have to do with the dynamics of complex systems, historical processes, and self-organization, issues which are further developed in the following chapter. Those ideas will be specifically applied to evolutionary, ecological and developmental aspects of social coordination in simple games. Some (but not all) of the questions asked within those models could well be fitted within a classical framework, which is not a proof that the systemic view has been abandoned but evidence of its more encompassing scope.