# 3 A naturalistic ontology for mechanistic explanations in the social sciences

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## A naturalistic ontology for mechanistic explanations

There are several approaches in the social sciences that seek to provide causal explanations of social phenomena neither in terms of general causal laws nor in terms of case-specific narratives, but, at a middle level of generality, in terms of recurrent causal patterns or "mechanisms" (Hedström and Swedberg 1998). Typically, these approaches invoke micro-mechanisms to explain macro-social phenomena. Most of them, "analytical sociology" in particular (Hedström 2005), are versions or offshoots of methodological individualism. These individualistic approaches either stick to the "methodological" in "methodological individualism" and leave aside ontological issues, or else they are also individualistic in the metaphysical sense and deny the existence of supra-individual social phenomena that cannot be analyzed in terms of the aggregation of individual actions (see Ruben 1985).

The ontological challenge to which individualism responds is that presented by holistic approaches that place the social on a supra-individual level of reality. Another possible challenge, coming not from above but from below, that is, from the natural sciences, is generally not considered. The individuals invoked in individualism are not so much the individual organisms recognized in biology as the individual agents recognized in common-sense ontology. Individual agency is taken as a primitive in this approach, rather than as a tentative construct that should be unpacked and possibly questioned by psychology and biology.

Most mechanistic approaches, whether their individualism is just methodological or also metaphysical, show little interest in providing the social sciences with a naturalistic ontology, that is, one continuous with that of the natural science. The main goal of this chapter is to outline such a naturalistic ontology. But why should we want such an ontology in the first place? I don't, by the way, believe that the social sciences in general should systematically work within naturalistic ontology: many

of their goals, concern and programs are better pursued with the usual common-sense ontology. But when it comes to providing a scientific causal explanation of social phenomena, there are at least two reasons to prefer a naturalistic approach. The first reason is trivial: to the extent that it is possible, we would prefer our understanding of the world to be integrated, both for the sake of generality and for that of coherence.

The second, more interesting, reason to want a naturalistic ontology has to do with the quality of our causal explanations. Either the laws of physics admit of exception and social events provides such exceptions (and there is a Nobel Prize in physics to be won by doing sociology!), or else whatever has causal powers in the universe at large and among humans on earth in particular has them in virtue of its physical properties. Of course, this does not mean that social scientists should get involved in the physics of social causality. What it does mean though is that, when we attribute causal powers to some social phenomena, we should be able to describe it in such terms that its physical character is not a total mystery but raises a set of sensible questions that can be passed on to neighboring natural sciences, psychology, biology and ecology in particular, that directly or indirectly do ground their understanding of causal powers in physics.

The social sciences too, or at least scientific causal explanation programs in the social sciences, should ground their understanding of causal powers in physics, obviously in an indirect manner, by grounding it first in other natural sciences. Otherwise, we keep attributing causal powers to phenomena that we are not even able to locate in the time and space of genuine causal processes, and the chances are that we are making spurious causal attribution. At best, the correlations among events we describe might bear some more or less systematic relationship to actual causal processes but we are not in a position to ascertain this relationship, let alone to understand it. Of course, ontologically unconstrained causal-like descriptions may be good enough for one's purpose, but then one is not really aiming at scientific causal explanations of social phenomena. To put it in other terms, limiting oneself to a naturalistic ontology is a favorable – I am tempted to say, necessary – condition to arrive at sound causal claims.

In a tradition quite different from that of individualistic social science, drawing their inspiration from Darwinian evolutionary biology, there are other approaches aiming at providing scientific causal explanations of social, and more specifically cultural, phenomena. These approaches are not only mechanistic but also naturalistic, that is, they are committed to invoking only causal processes that can be

described in natural-science terms. However, I would argue, most of these approaches do not live up to their commitment.

The best known example of an evolutionary approach to culture is that of "mimetics" inspired by the work of Richard Dawkins, and according to which culture is made of bits or "memes" that replicate themselves and propagate in a population through imitation (Blackmore 1999; Dawkins 1976). Just as biological evolution, cultural evolution is seen as largely governed by a process of Darwinian selection operating not among genes but among memes. Unlike holistic sociology where people's behavior is largely determined by external forces larger than themselves, and individualism where people are first and foremost agents determining their own behavior, memetics explains behavior, or at least cultural behavior, as determined by micro-forces operating within individuals and to a large extent controlling them, somewhat as viruses do.

Dual Inheritance Theory (Boyd and Richerson 1985; Cavalli-Sforza and Feldman 1981; Durham 1991) is another evolutionary approach to culture, and one that is more compatible with individualism than memetics. It describes people as collectively determining the evolution of culture by individually selecting cultural variants. In their selection, people are influenced by biologically inherited psychological biases that favor, for instance, imitating the majority, or the most prestigious individuals. According to the theory, mechanisms of cultural evolution differ in important respects from those of biological evolution, but the dynamics remain quite similar.

These evolutionary approaches to culture are innovative in many respects. They tend, however, to buy wholesale their catalog of cultural phenomena from the standard social sciences. Their naturalism consists to a large extent in providing natural causes – naturally selected psychological dispositions and ecological factors – for these non-natural social phenomena, or to adapt models of biological causality – more specifically of population genetics – to the cultural case. The cultural phenomena explained include however things such as religion, norms, art, racism, matrilineality, political hierarchy, and so on. Of course, these are postulated to have a proper naturalistic description, but nothing seriously approaching such a description is ever given.

Another mechanistic and naturalistic evolutionary approach to culture is the epidemiological approach that I have contributed to developing (Atran 1990, 2002; Bloch and Sperber 2002; Boyer 1994, 2001; Hirschfeld 1996; Sperber 1985, 1996, 1999, 2006), a hallmark of which is its insistence that a proper understanding of cultural phenomena and their propagation requires a deep understanding of the psychological

mechanisms involved (just as a proper understanding or standard epidemiological phenomena requires a deep understanding of individual pathology). The epidemiological approach takes seriously the ontological challenge of naturalism and suggests a way to provide a truly naturalistic ontology of the social. Here I outline how this can be done.

Relationships among neighboring disciplines may involve a difference of levels or a difference of scale. (The two terms, level and scale, are often used interchangeably, so I am, for expository purposes, sharpening a rather vague distinction.) To illustrate, contrast the case of psychology and neurology on the one hand and that of epidemiology and pathology on the other.

Until the cognitive revolution of the second half of the twentieth century, mental phenomena had no counterpart in the natural sciences. One could, of course, assert that mental phenomena occurred in the brain and postulate that they were wholly material, but there was no understanding whatsoever of how matter in general and brain tissues in particular might realize mental processes. The choice was then between pursuing a non-naturalistic psychology, and, as did behaviorists, pursuing a naturalistic psychology understood as a science not of the mind but of behavior. With the development of the mathematical theory of automata on the one hand, and of the neurosciences on the other, it is now possible to understand how matter in general and brain tissues in particular can process information. It is possible therefore to begin bridging gaps between psychology and biology. Psychological processes can be conceived as brain processes described in functional terms. At present, however, the concepts of psychology are not reducible to those of neurology, and it is contentious that they ever will be. So, the naturalization of psychology involves a matching in greater and greater detail of psychological and neurological descriptions but the two kinds of descriptions remain on quite distinct ontological levels. Neurology and psychology conceptualize different kinds of properties, and the concepts of one level cannot, at least for the time being, be defined in terms of the concepts of the other level.

Whereas the difference between neurology and psychology is one of levels, the difference between individual pathology and epidemiology is one of scale. Epidemiology studies the distribution of individual pathological conditions in a population. Epidemiology has its own concepts but not its own ontology. Its concepts are defined in terms of those of other disciplines: individual pathology, ecology, demography. Because it draws on several other disciplines, epidemiology is in a relationship of mutual relevance with all of them and of reduction with none of them. It is genuinely autonomous discipline, with strong bridges to other

disciplines, and without an autonomous ontology. Because the sciences from which it borrows its ontology are natural sciences, epidemiology is unproblematically a natural science too.

To better grasp the difference between differences of level and of scale, think of a zoom (a metaphor that was suggested to me by Bruno Latour, who however himself questions its appropriateness). You might initially be looking at, say, a single neuron. You might then zoom in to the scale of molecules, or you might zoom out to the scale of neuron assemblies, brain regions, or the whole brain, but at no point in this zooming out do the objects you see become psychological rather than just neurological: you don't come to see thoughts or intentions with their contents. Even if, of course, what you are looking at includes the neurological realizers of such mental states, what you see is brain tissue all the way. Suppose by contrast that you are looking at an individual case of, say, the measles. You might zoom in within the organism to the scale of individual cells infected with the virus, or you might zoom out to the individual's environment, to her household, her community, the whole population to which she belongs. When zooming out from the individual to the population level the objects you see are still the same but they also become epidemiological ones. Whereas psychological objects are not neural objects seen at the scale of population of neurons – the difference is one of levels – epidemiological objects are just that: pathological objects seen at a the scale of a population in its environment – the difference is one of scale.

I propose a naturalization of the social science domain not on the model of psychology but on that of epidemiology. That is, I want to argue that social phenomena are patterns of psychological and ecological phenomena at a population scale. This project of naturalization of the social is made possible by the naturalization of the mental that is under way in the cognitive sciences. Let me explain how.

What makes a cognitive process cognitive is that it has as its function to secure a content relationship between its input and its output. In the case of perception, the input is a stimulus, the output is a mental representation, and the perception process aims at securing that the content of the mental representation should be a true identification of the input stimulus. In the case of memory processes, the input and the output are both mental representations and memory processes aimed at securing relevant content similarity between the two. In the case of inferential processes the input and the output are both mental representations and the inferential process aims at securing a relationship of justification: the content of the input, or premises, should justify that of the output, or conclusion. In the case of psycho-motor control, the

input is an intention, the output is a modification of the environment, and the psycho-motor control process aims at securing a relationship of realization: the modification of the environment should realize the intention. What the cognitive sciences are in the process of doing is explaining how material processes can reliably secure such relationships among representational contents (as in inference or memory) or between contents and the states of affairs they represent (as in perception and action).

Cognitive psychology studies the various processes that together make up the causal chains that go from the inputs to perception to the outputs of motor control. Actually, most cognitive psychologists study only one type of process involved in these causal chains: for instance perception, or memory, or inference. Among the criticisms that have been addressed to the standard cognitive psychology paradigm, many have to do with this relatively narrow view of cognitive processes. As critics have insisted, actual cognition is embodied, situated and distributed. I think that these criticisms are essentially correct, even if they are not as damaging as they are often claimed to be. Cognition is embodied: the brain is part of the body, and cognitive processes involve relationships not just between the environment and the central nervous system but also with the rest of the body (see, for example, Clark 1997). This is a truism, of course, the consequences of which are only now being systematically explored. Cognition is situated: the situations in which it occurs, in particular social situations, structure and guide cognitive processes (see, for example, Lave 1988). This is particularly obvious in the case of teaching and learning situations, but extends readily to all social situations and beyond. Cognition is distributed: many cognitive processes are realized not by a single individual but by a network that typically involves several individuals and artifacts (see, for example, Hutchins 1995). Is cognition in the brain, in the body, in the situation, in the network? In all of these and more. These descriptions should not be viewed as alternative theories but as complementary foci and scales.

The notion of a cognitive process should be understood so as not to be limited to processes located at an individual brain and its immediate periphery. Thinking of a cognitive process as a causal process that has the function of securing a content relationship (among representations or between representations and the state of affairs they represent) provides a simple and sensible way to broaden the picture. In particular, it justifies seeing all social processes as being also cognitive processes. Indeed, whatever else they do, social processes secure content relationships among the mental states and the actions of the people involved.

Cognitive processes are chained to one another, the output of some serving as input to others. This is true not only within individuals, but also across individuals. Communication, for instance, is a social cognitive process with two components, one of public expression on the part of the communicator, the other of interpretation on the part of the receiver. Interpretation, of course, takes as input the output of the expression process. The content relationship that the communication process aims at securing is a match in content between the communicator's meaning and the interpretation of the receiver. Communication itself is typically embedded in more complex processes that are both social – they involve interactions among individuals – and cognitive – they secure content relationships. Thus when one individual asks another to perform some action – be it an order or a request – the intentions of the first individual are satisfied through the action of another. When one individual gives testimony, her perceptions and inferences feed into the mind of others. When a group of people debate ideas or a course of action, they engage in a joint cognitive process that often none of them could have achieved on their own. All social interactions involve people acting on other people's minds. Conversely, too, any action on another person's mind is a social action.

Acting on other people's minds may not be the main feature or the main goal of social interaction. People may be after goods, space, food, sex, or whatever, but if their goals are social at all, they involve a cognitive dimension. When you buy an object, for instance, what you want is the object, but there is information transfer about the intentions of the buyer and of the seller, about the price, about the object. The price itself has an informational content the interpretation of which involves situating it in a historically extended causal chain that gives its value to the currency used. As another example, compare a person accidentally hitting another – not, or not yet a social interaction – and a person hitting another in an openly intentional way – unquestionably a social interaction. The difference that makes the second interaction social is that the hitter is not just transferring energy, he is also transferring information about his attitude to the victim, for instance about the grudge he may have or the rights he wants to enjoy.

A human group is criss-crossed by a complex flow of information. In this flow, not only information, but also people and things are being altered and moved around in an endless variety of ways. Still, let me insist: you can have social interactions without goods being made and handled, without bodies being moved, without sex, or without food, but you cannot have a social interaction without transmission of

information, and whenever information is being transmitted you have a social interaction. Let me also add, much of this flow of information goes through individuals and groups without being intentionally or even consciously transmitted. We transmit information through behaviors that do not have such transmission as their goal. Even when we engage in intentional communication, the information we actually transmit consists typically in both less and more than we intended. Most cognitive processes are unconscious, and so-called conscious processes are only partly conscious. Similarly, much of human behavior is unintentional, and many aspects of intentional behavior are not controlled by intentions.

I see no a priori reasons to give pride of place to intentions among the cognitive determinants of behavior. I am not, in other terms, indirectly arguing for yet another cognitive version of methodological individualism. When Peter Hedström writes: "Since changes in … social properties must be either intended or unintended outcomes of individuals' actions – how else could they possibly be brought about – they should be analyzed as such" (Hedström 2005: 5), I fail to see how the conclusion follows from the premises. Such outcomes of actions must also be expected or unexpected, planned or unplanned, emotionally loaded or not, and so on, but it does not follow that they should be analyzed as such (which is not to deny that it may, in some cases, be relevant to do so). The social cognitive causal chains I am talking about are typically infra-individual, or, if you prefer, sub-personal (a notion introduced by Dennett 1969), and, at the same time, trans-individual. They involve a variety of mental mechanisms, the formation and carrying out of intentions being one or, more plausibly, a subset of them, an important one, no doubt, but not obviously more important than, for instance, the mechanisms of attention or of memory.

Standard epidemiological phenomena are causal chains of pathological events inside organisms and of events in the environment of organisms. Similarly, I claim, social phenomena are causal chains of mental events inside people and events in their common environment. These environmental events comprise behaviors and effects of behaviors such as transformation and movement of objects and people. Mental events can be better described in terms of a naturalized psychology. Environmental events can be described in plain materialistic terms, drawing on the appropriate natural sciences when relevant. So the nodes and links in social cognitive causal chains can be characterized naturalistically. The naturalistic challenge is to reconceptualize the social just on the basis of these causal chains.

Objection: two materially identical environmental events, say, a wink and a mock wink, may be quite different social events (simplifying an example of Gilbert Ryle made famous by Clifford Geertz; see Geertz 1973; Ryle 1971). The typical winker is aiming at secret communication with just one other person. The mock winker is drawing the attention of third parties on the failure of the winker to keep her communication secret. A description in terms of their mere material properties cannot account for the socially crucial difference between a wink and a mock wink. An eye twitch is an eye twitch. True, the twitch in a mock wink tends to be exaggerated, but it need not be. Is then the only way to go to replace the superficial material description with a 'thick' interpretative description, as Geertz argues? The alternative I am suggesting is to stick to the material description of the public events and to explain the difference by the fact that the wink and the mock wink occur at different places in social cognitive causal chains, and in particular have different mental causes and effects. To interpret an eye twitch as a wink is to attribute to it one kind of mental cause in one kind of chain of events, to interpret it as a mock wink is to attribute to it another causal history.

These two approaches – the interpretative and the naturalistic – don't differ in their intuitive understanding of what is happening. They differ in their ontology of meaning. For the interpretativist, meaning is public, it is in the public event, and is therefore beyond the reach of a naturalistic approach. For the naturalist, meaning is in the causal relationships of the public event to other events, in particular mental ones.

Just as cognitive events owe many of their properties to the fact that they are embodied, situated and distributed, social events owe many of their properties to the fact that they are mentalized, situated and distributed. Or, in terms I prefer, human cognitive and social events are what they are because they are embedded in social cognitive causal chains, and the chains involved in individual cognition and in social interaction are the same. They are just considered at different scales.

Even if you grant me that we might naturalize the difference between a wink and a mock wink, you may sensibly feel that the challenge of reconceptualizing the social just in terms of social cognitive causal chains is an excessively difficult or even impossible one. You might, more importantly, object that the project of reconceptualizing the whole domain of the social sciences in naturalistic terms, even if feasible, would be counterproductive since there is a wealth of accumulated knowledge and competence formulated in the current conceptual

framework that might be lost in the process. However, as the example of the wink suggest, the kind of naturalistic concepts of the social I am advocating are both significantly different but also closely related to more standard conceptualizations.

Let me give some very simple examples of the kind of reconceptualization I am advocating. Take a folktale such as Little Red Riding Hood. You can think of it as a collective representation that has evolved over time in European societies, has been taken up by a literary tradition aimed at a different social class from Charles Perrault and the Grimm brothers onwards, that expresses cultural attitudes toward unmarried women as preys, and so on. Naturalists ask: where in space, where in time is Little Red Riding Hood? Where, when and how does it enter into causal processes? And their answer is: the tale of Little Red Riding Hood is an abstraction, useful as such, but not to be confused with something with causal powers. What you have rather, in the world of causes and effects, is a social cognitive causal chain that extends over countries and centuries, that is made of public tellings and mental rememberings of indefinitely many versions of Little Red Riding Hood, millions and millions of micro events inside and among people. Causal forces apply at the level of these micro events and processes. Most stories told never reach a cultural level of distribution. Few culturally stabilized stories are as resilient as Little Red Riding Hood. One of the questions to ask then is what stable or variable properties of individual minds, of inter-individual encounters, and of the local environments where these occur explain the resilience of the tale – that is, the fact that its many versions stay close to one another – and also its evolution.

Take prestige, or to be a bit more concrete, take the intellectual prestige of Professor Jones. Prestige is characterized both by a content and by a distribution. The content has two aspects: an outstandingly positive evaluation of the intellectual merit of Jones, and the representation of this evaluation as being widely accepted. It is not important for prestige that the positive evaluation be justified – prestige need not be deserved – but it is essential that this evaluation be widely distributed and represented as such – prestige must be recognized to be prestige. To explain the prestige of Jones is to explain the joint distribution of two representations: that Jones is an outstanding professor and that a great many people agree that he is outstanding. To develop this explanation, one must identify the social and cognitive factors that, at every micro step, secure this distribution and stabilize its contents.

Examples such as tales or prestige illustrate, if anything, too easily the notion of a social cognitive causal chain. After all, the causal chains involved consist in an alternation of public and mental representations of similar content. The cognitive dimension is hardly contentious. But what about, for instance, an institution? An institution need not be mentally represented, and, in the case of complex institutions, it does not even lend itself to being mentally represented in an individual mind. Institutions are the paradigmatic examples of social things that seem irreducibly social. Of course, once we accept that cognition can be, and often is, distributed among people and artifacts, we have no difficulty in recognizing that institutions, however complex, involve such distributed cognition. But can we *characterize* institutions in terms of social cognitive causal chains?

Here is a proposal: institutions are characterized by an articulation of hierarchically related causal chains. At higher levels, are distributed representations that prescribe how lower level representations (and behaviors, and artifacts) should be distributed, and the distribution of these higher level representations plays a causal role in the distribution of the lower level items. Take a folktale distributed by an extended social cognitive causal chain, what I would call a cultural cognitive causal chain. Add to it an extended distribution of a higher level representation with a normative content that prescribe, say, that this folktale is to be told on Christmas Eve. The distribution of this higher level representation indeed causes the tale to be told on Christmas Eve. Now instead of having, so to speak, a free-floating folktale, we have an elementary institution: a Christmas tale. More complex institutions, universities, churches, armies, markets, for instance, involve the articulation of many more social cognitive causal chains with a much greater variety of changes in the environment, but the principle is the same.

Most standard concepts in the social sciences are generalized and regimented versions of concepts deployed by social agents themselves. The social science concepts of status, class, caste, law, rights, contract, politics, state, religion, ritual, marriage, war, art and so on are borrowed and adapted from folk sociology. Social agents and social scientists alike attribute causal powers to the social phenomena denoted by these concepts. A marriage, say, is described as causing changes in rights and duties. From a naturalistic point of view, these are misattributions of causal powers. However, these attributions are generally not wide of the mark. On the contrary, there is a fairly systematic closeness between standardly misattributed causal powers and genuine causal processes.

The effects that social agents and social scientists attribute to, say, a marriage, closely correspond to the effects of causal chains that distribute representations of this marriage. The effects that social agents and social scientists attribute to, say, a law, correspond to the effects of causal chains that distribute representations of this law, and so on.

The naturalist social scientist reconceptualizes the social in terms of causal chains that distribute representations. The idea is not at all, let me insist, that mental representations are the social, or cause the social. Social cognitive causal chains contain events not just in minds but also in the environments and the relative causal weight of mental and environmental events vary with the type of social phenomenon: mental events are relatively more important in literature as a social phenomenon, and environmental events are relatively more important in war as a social phenomenon. But the thread that links all social events is a cognitive thread that goes through minds, through the environment, through minds, through the environment, and so on, securing content relationships along the way.

So, to conclude, here is what I have argued:

- A naturalization of the domain of the social sciences is made possible by the ongoing naturalization of psychology.
- The ontology of a naturalized social science is a composite ontology, articulating naturalistic description of mental and environmental events.
- Precisely because, on this view, naturalized social sciences borrow the ingredients of their ontology from several different disciplines, their concepts and theories cannot be reduced to the concepts or theories of any one of these disciplines.
- The way in which naturalized social sciences renounce ontological autonomy secures their theoretical autonomy. In other terms, I am arguing for an ontological reduction without theoretical reduction.

Explaining social phenomena, in this perspective, is identifying the recurrent causal patterns or causal mechanisms that produce regularities in social cognitive causal chains. These regularities permit in turn to identify types of social phenomena (in a "population thinking" way, that is, without ever essentialising them; see Mayr 1970). Many of the type so indentified are likely to have close counterparts in folk and scholarly sociology, but they have a different ontology, one that comes with sound methodological constraints. Such constraints should be welcome when the goal is scientific causal explanation.[1]

[1] This chapter expands and revises an earlier text published in French (Sperber 2007).

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