

200,000 individuals. The highest density was found in the
central part of the study area, where the population density
was estimated at 200 individuals per square kilometer. The
lowest density was found in the northern part of the study area,
where the population density was estimated at 100 individuals per
square kilometer.

Chapter 7

EPISTEMOLOGY AND COGNITIVE THEORIZING

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INTRODUCTION

The study of cognition as a natural phenomenon can affect epistemological projects in different ways. In this paper we want to examine some aspects of this influence. The paper is divided into two parts. In part one we review several ways in which epistemology can be naturalised, attending especially to the import of computational models of cognition and the different views on folk psychology. In part two we treat, as a specific case, the impact on epistemology of the putative "folk epistemology" capacity, that is, the natural ability to evaluate epistemic situations.

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PART I. WAYS OF NATURALISING EPISTEMOLOGY: THE IMPACT OF COGNITIVE THEORIZING IN EPISTEMOLOGY

1. NATURALISING EPISTEMOLOGY AND THE COGNITIVE TURN

Generally speaking, philosophical naturalism covers three different theses:¹

- a) The methodological continuity between philosophy and science.
- b) The rejection of mind–body metaphysical dualism.
- c) Holding externalism in epistemology.

Almost all philosophers endorse theses (a) to (c) altogether, even though there are exceptions. It is often easy to find a mistake about the term ‘externalism’, that can be conceptually misleading. In the tradition that goes from Kuhn and Lakatos to the Strong Program, ‘externalism’ is understood as the influence of external factors (say, social causes) on epistemic decisions. Some philosophers (i.e., Lakatos [1970, 1971]) allow both internal and external factors; others (i.e., the Strong Program [Barnes & Bloor, 1982; Bloor, 1981]), on the contrary, admit only the external ones. Lakatos’ views are compatible with the rejection of (b), and even with the rejection of (a). For example, even if it is unclear whether Lakatos endorsed explicitly metaphysical dualism, his asymmetry thesis claiming that (in the process of reconstructing the history of science) we should look for (external) causes *only* when we are unable to find a rational explanation, could have been signed by Descartes himself. Further, this thesis seems to be closer to the rejection of (a) than to its acceptance, in so far as it involves either a view of philosophy as autonomous from science, or a view of social sciences as separated from natural ones. The latter can be also true of sociologist approaches.²

In the fields of philosophy of language and philosophy of mind, on the other hand, the terms ‘internalism’ and ‘externalism’ are used in a different sense, this time having to do with mental *content* individuation. This is the

¹ See Papineau (1993; 1996) for a more extended development of naturalism in epistemology involving these three theses.

² Fuller (1993) claims: “In terms of the recent philosophy of science debates, my position is as follows. I am a scientific realist with regard to the discourse of the social sciences. By that I mean that the best explanation for the history of all our knowledge enterprises is provided by the best social scientific theories. However, I am an antirealist about the discourse of the natural sciences, to the extent that I accept the validity of social constructivist accounts of natural scientific practices.” (p. xiv)

case of Putnam [1975] or Burge [1979] (discussed at length by Fodor, 1987). Thus, you can be externalist in this sense, and nonetheless reject (a) (as Putnam does), or even reject (b) (as Kripke). Moreover, being externalist in this sense involves holding epistemological externalism, in so far as it allows epistemic evaluation to take into account factors additional to those consciously accessible to the cognizer. But the opposite need not be held: you can accept the contribution of non-conscious processes in epistemic justification without endorsing a Kripkean-Putnamian semantic view.

In a broad sense, the project of cognitive science is to investigate how information is learned, stored and used. Also in a very general sense, epistemological naturalism amounts to the view that knowledge (both common sense and scientific knowledge) is the result of causal relations between agents (people, scientists) and world (including other agents). These relations should be understood in terms of the available empirical sciences. This view is not accepted by a class of purported naturalists: sociologists claim that this is not the case for scientific knowledge [Bloor, 1991]. Instead, they hold (with Popper) that scientific knowledge is *objective* knowledge, i.e., a kind of knowledge in whose production individual cognizers (in the sense of "individual cognitive mechanisms") do not have any interesting role to play. For them, scientific knowledge is completely a social product. May be this is the reason that explains why sociologists are epistemically relativists. On the other hand, *reliabilism* is a common option for naturalists with a cognitive predilection [Goldman, 1986]: they hold that (at least) one relevant part of epistemic appraisal depends on *how* information is acquired and used. This, in turn, leans heavily on the cognitive mechanisms and constraints disclosed by cognitive science.

The interdisciplinary nature of cognitive science has yielded a great diversity of approaches within the field of epistemology. These approaches can be sorted out basically in three lines of inquiry, although it is possible to cut the cake in other ways depending on which criteria we employ. These lines are:

1. The application of Artificial Intelligence models to the analysis of the historical development of research programs.
2. The analysis of the shortcomings and biases of human reasoning capacities, revealed by experimental psychology. It is worth noting that this approach is quite different from the traditional view in epistemology that endorses and pursues categorical rationality.
3. What we will call "root consequences developers": namely, approaches that rest on whatever basic elements are posited in cognitive theorizing, and then infer large scale consequences for the conditions of possibility of knowledge, and for our second-order thinking about it, namely, epistemology. These approaches appear as the most fundamental, given

that they deal with (at least) two essential issues. The first one concerns the status of folk psychology (on which depends the overall project of traditional epistemology); the second has to do with the prospects of explanation for non-demonstrative inference, holistic or context-sensitive inference, or abductive inference. Three expressions that refer to different perspectives of the same global phenomenon. These kinds of inferences play, presumably, a crucial role in knowledge, in so far as most of our reasoning tasks make use of them.

Those three lines are not mutually exclusive. Therefore, we often find that a particular author pursues two, or even all, of them.

2. HISTORICAL ANALYSIS WITH NEW CONCEPTUAL (COGNITIVE) TOOLS

Philosophers who work on lines (1) and (2) are the new practitioners of the "historical meta-methodologies" tradition that began with Kuhn [1970]. The main difference with respect to this tradition is that historians are reproached for their "epistemological intuitionism", given that they try to assess the rationality of past epistemic decisions in the light of our current philosophical intuitions about rationality.³ The problem for the epistemological intuitionist is that he seems unable to show that, for instance, past scientists were rational in their epistemic decisions (according to the categorical norms of rationality established by philosophy). This need not be a worry to those who, like Lakatos, are prone to apply what Popper [1972] called the "transfer of logical (rationality) to history" principle. Nonetheless, it *should* worry all philosophers that claim that we can learn our epistemic justification norms from a careful examination of the history of science (for example, Laudan [1987]). The solution of the new (cognitive) practitioners is to apply the cognitive models at hand, so as to analyse and reconstruct the history of science. This way, the result would be much more accurate and empirically based. We can include within this line authors like Bechtel, Giere, Nersessian, Solomon, and Thagard.

The focus of Giere's work [1988, 1994] has been to answer the sceptical and relativist challenges put forward by the sociologists' view of scientific knowledge. To this effect, Giere has elaborated a version of the semantic-model account of scientific theories based on the notion of *Mental Models* proposed by Johnson-Laird (1983). Nersessian [1992, 1995] and Thagard [1988, 1992], on the other hand, apply the resources of cognitive science to the analysis and reconstruction of scientific change, especially conceptual

³ Rouse (1998) nicely describes these two lines.

change. Both share a common feature: the extensive use of *soft reasoning models*, such as analogical and similarity reasoning, as well as other ways of reasoning used in communication and presentation, such as diagrams, images, and so forth [Suchman, L. 1990]. Solomon's approach [1992; 1994; 1996] can be considered as a development of lines (1) and (2), given that she uses the experimental results of cognitive psychology in order to reconstruct historical cases. Further, she also shares with Thagard the interest on those epistemological issues related to social organisation of science and collective rationality, typical of current science.

Complementing his work on the cognitive reconstruction of history of science cases, Thagard has developed several computational models for problem solving heuristics, specifically, for abductive reasoning. He has designed some programs (PI, ECHO) that run in associative networks. In this context, he has proposed an explanation —the theory of explanatory coherence— that sets multiple constraints on the reasoning processes carried out by the associative algorithms. Using these computational tools, Thagard and his team have implemented computational simulations of some historical cases of conceptual change. In addition to his work on building computational programs, the main difference between Nersessian's and Thagard's approaches lies in that whereas the former centres her interest on the *creative* processes involved in building new concepts (context of discovery), the latter adopts a clearly *evaluative* perspective (context of justification).

Some remarks are in order. We could have worries about the philosophical relevance of this kind of approach. We could criticise it for being circular, so that it would lack interest from a philosophical perspective. This would be the case of Thagard, in so far as the results of his simulations are crucially dependent on the *input* properties (its content), which in turn depends exclusively on what the designer does at will. In other words, if we introduce in the system a set of evidence data and hypotheses, all of them adequately codified, as well as the corresponding set of constraints, the fact that the system/machine is able to do the remaining task does not matter at all. This is so because the interesting labour would be just *handmade*, say, the evidence, hypotheses, and constraints introduced. But this, as it happens in all AI simulations of problem solving, depends crucially on previous conceptual work that implies that those who are able to build a computational model to solve a problem *already* have a way of solving it *before* building the algorithm. From this perspective, our simulations would amount to a sort of mechanisation of cognitive or rational processes, but not to an explanation of them.

Thus, in simulating historical cases of problem solving, we would need a previous rational reconstruction. Once we have one at hand, all that remains

is the routine, not philosophically interesting work of designing the algorithm. Hence, the traditional historian can claim that there is no *real* difference from his own approach. The reason is that the results of the simulation would depend on the rational intuitions employed in the previously made reconstruction. Moreover, computational simulation would remain *neutral* concerning the epistemic values employed during such a reconstruction: "give me a rational reconstruction of a case of problem solving and I will give you a program simulating it", could be the slogan,

We have to be cautious here, however. It often happens that machines can surprise us. It might be argued that these surprises are only apparent, not genuine, in the sense that machines are not able to create knowledge. They just limit themselves to manipulate previously introduced information. Thus, at the best, they can deploy explicitly information already contained (implicitly) within the information we previously input. Yet, even for this modest job it would be worth doing simulations. But there is more: it turns out that the situation of trying to simulate an intelligent task *forces* us to learn a lot of things about the nature of the target task, as well as about the nature and capacities of the systems capable to accomplish it. It is possible that a lot of details and constraints can remain unknown to the pure conceptual analysis, whereas they reveal themselves when we try to build a specific system capable to do the job. For this reason, the naturalist philosopher, even if she is a normativist, should not reject this type of project.

Bechtel [1995] can be included within this line. He combines a close familiarity with connectionism with an interest on the epistemological and normative issues of scientific knowledge. His main contribution is the development of heuristics and cognitive strategies —decomposition and localization— that he offers both as methodological-normative proposals and as a conceptual frame to analyse historical cases of theory change, mainly in the fields of biology and psychology. He centres his attention on the psychological constraints operating over the development of scientific theories, and his main goal is to propose dynamical models of rationality plausible from a psychological viewpoint.

So far our description of some ways in which cognitive theorizing is currently influencing epistemological issues has focused especially on the philosophical accounts of scientific knowledge (lines (1) and (2)). Nonetheless, we must note that all the aforementioned approaches involve deep, basic epistemological and ontological commitments that we have not touched yet. It is plain that these accounts use folk psychology in one way or another. They assume a view of agents (scientists) as *believers*, as beings that have *representations* and engage in reasoning processes that operate on representations.

On the other hand, the customary employment of heuristics involves the assumption that most reasoning processes are like non-demonstrative inference, in contrast with classical (logical) inference. This is a fact that no one wants to dispute, of course, but it raises a deep problem that has to do with the sheer possibility of naturalising epistemology, i.e., about the prospects that cognitive science has of illuminating our epistemological discourse.

3. COGNITIVE APPROACHES AND THE STATUS OF FOLK PSYCHOLOGY

A first question about folk psychology is whether or nor it has to be regarded as a *theory*. This is a controversial issue. The reason is that it seems a matter of conceptual commitment that if you consider **X** as a theory, then you are admitting a lot of things, e.g., as such a theory it may be true or false. To decide this, you can appeal to a number of reasons, being the most important among them whether or not its posits (its ontology: the things and properties postulated) *do refer* (really hold) within the domain. And if it turns out that the theory is false, then you should look for a replacement of such a theory. In addition, there are other constraints to be satisfied by a theory, and these, in turn, play a relevant role in deciding whether to maintain or reject it. It must have explanatory power, allow progress and integration with other theories we may have about the world, and so forth.

On the other hand, there are several features that are often attributed to folk psychology, features that we can synthesise in the following four:

- a) Mental states are semantically interpretable, that is, they have *content*, in the sense that they are *about* something.
- b) You can have different *attitudes* towards the same content. You can believe it, fear it, desire it, and so forth. These two things, the kind of attitude and its content, allow us to individuate the complex mental states that are the propositional attitudes.
- c) Mental states play a causal/explanatory role: mental states explain behaviour *because* mental states *cause* the behaviour.
- d) Mental states are *functionally discrete*. By this it is meant that they are separable entities. You can have a propositional attitude independently of your having or not others. They can be distinguished from each other.⁴

⁴ This is the typical characterisation used in the literature, in order to analyse whether or not connectionist systems are able to deal with these commitments. See, for example, Horgan & Tienson (1995).

It is unclear whether psychology satisfies either the requirements for being a scientific theory, or even the four features just mentioned. Even if folk psychology were unable to satisfy almost all of them, this fact should not prevent us from regarding it as a theory, although of a special kind: a theory whose status would not be exactly like ordinary scientific theories. All this is highly dependent on your favourite basic approach to cognitive systems, together with additional considerations. There are two (and a half) influential cognitive approaches in the market: (a) classical; (b) connectionist; and, finally, the half, (c) evolutionary psychology. The reason to call it "a half" is to reconcile the following two features of the evolutionary approach: (i) its aim to constitute a full-blooded account of cognition (an alternative paradigm, if you like); and (ii) the fact that this account is not committed to a specific way of characterising the nature of psychological processes. For example, Cosmides & Tooby [1994] claim that psychological processes are computational. That's all. So it seems that this approach could be compatible both with (a) and (b).

With respect to the ontological status of folk psychology, the main controversy arises between realist and eliminativist positions, which are typically represented by Fodor [1981] and Churchland [1989], respectively. It is also noticeable that they are conspicuous defenders of the classical and connectionist approaches, respectively. But the link between classicism with realism, on one side, and connectionism with eliminativism, on the other, seems to be more a *de facto* than a *de iure* situation. Actually, the realism/eliminativism controversy began before connectionism appeared on the scene (we could trace it back to Sellars's [1963] scientific and manifest images, for example). There is also the (quasi)instrumentalist approach favoured typically by Dennett [1987], that would deny features (3) and (4) above. For the sake of space, we will concentrate on the other two positions.

As it happens, Fodor would tell us, you do not need too much effort to be a realist about folk psychology. It is, we would say, a *non-negotiable fact*. If it turns out that your favourite approach (be it classical or connectionist) gets in trouble with folk psychology, so much the worse for your approach. Thus, the task to be done is to develop a cognitive theory that maintains the basic folk psychological ontology, and whose goodness is subordinated to its capacity to abide by the explanatory standards of folk psychology. The defenders of this position, however, have argued at length that connectionism is unable to render an account faithful to folk psychology. This is a point about which eliminativists and instrumentalists do not worry, even if for different reasons. Dennett [1991] would agree with the practical indispensability of folk psychology, but he argues that we should no longer treat it as a *theory*, but as a *craft*. Being things so, folk psychology and cognitive theorizing do not compete, given that their goals are quite

different. It may well be that the real underlying mechanisms responsible of behaviour have nothing to do with our folk psychological descriptions and explanations. Eliminativists, on the other hand, reply that if folk psychology does not match connectionist systems, it is folk psychology that must go (given that connectionism is a better theory of cognition than folk psychology). Nonetheless, things are not so easy to those that try to endorse together realism and connectionism.

The problem with this position is to make compatible the four typical features (1-4) of folk psychology with the putative commitments of connectionism. Horgan and Tienson [1994, 1996] summarise the main commitments in the following three:

- i. Its codification is massively distributed (non-local).
- ii. Hidden units are sub-symbolic: they do not have semantic interpretation.
- iii. Connectionism is proposed as a full account of cognition (not only as yielding implementation models).

So, from the perspective of commitments (i)-(iii), the problem is to make them compatible with feature (4) of folk psychology, functional discreteness [cf. Ramsey et al, 1991]. Horgan and Tienson (1996) distinguish three different ways in which a psychological state type can be possessed by a cognitive system: (a) occurrently; (b) dispositionally; (c) morphologically. These three ways can distinguish kinds of functional discreteness. While folk psychology is usually committed to (a) and (b), they claim that it is also compatible with (c). In fact, they argue, folk psychology usually deals with psychological states that are both occurrent and conscious, and connectionist systems (of the sort of those proposed by Ramsey *et al.*) have no trouble to accommodate all kinds of functional discrete mental states. In fact, they argue, connectionist systems meet Marr's (1982) three-level description just as classical systems do. And if classicists do not have trouble to accommodate the higher (cognitive) level, connectionists do not either.

In general, it is typical of those connectionists that want to keep folk psychology, in contrast to eliminativist connectionists, to share a *revisionist* view. In other words, they give theoretical and ontological primacy to the algorithm and implementation levels, in such a way that the development of our theories on these levels can involve a revision of our ontological and epistemological intuitions based on folk psychology.

4. COGNITIVE THEORIZING AND EPISTEMOLOGY

To conclude this quick revision of the relations between cognitive theorizing and epistemology, we can draw several morals. In general, we can say that

all the different positions are *naturalist*, in the sense that all of them share the three naturalist theses (a-c), presented in section 1, even if they do not do so in the same way. Even when they share the thesis of methodological continuity, there is an important difference between realism, on the one hand, and revisionist and pragmatic approaches, on the other. The realist + classical account brings about a sort of methodological dualism internal to cognitive theorizing: the higher level (cognitive) would be autonomous with respect to the lower levels. Classicists claim that unless we admit a cognitive architecture based on the computational theory of mind (with a set of processes that operate on symbolic-syntactically structured representations, really tokened on the algorithm level), it is impossible to account for cognitive behaviour, more particularly, to account for *rational* performance. The idea behind this is that an explanation of a rational behaviour cannot be considered as such *unless* it shows that the behaviour is caused by a process instantiating a justifying argument. This is what Cummins calls the *rationale constraint*.

Classicists claim that they have no problem concerning this constraint. Thus, the troubles fall again on connectionism. Again, if it is possible to show that classicist arguments are wrong, or if we can offer alternative ways of describing the algorithm level (e.g., in terms of dynamical systems) that account for structure sensitive processes, then connectionist systems could be capable to deal with the rationale constraint. On the other hand, we must note the inability of classicism to account for the more interesting cases of cognition, say, the ones that deal with non-demonstrative inference, abductive inference, context-sensitive inference, and so forth. In fact, Fodor [1983; 2000] explicitly sees it as a hopeless project, maintaining that computational accounts are limited to the modular systems. Yet, the claim of the connectionist camp is that neural networks seem to be better equipped to deal with this kind of inference. But the price to pay is to renounce, in some way, to the rationale constraint.⁵

⁵ In this respect, it is worth mentioning a recent paper by Cummins *et al.* (2001), where the authors try to show that connectionist systems of the sort of Smolensky, Legendre and Miyata (1992) do meet the challenge of systematicity (Fodor and Pylyshyn, 1988) by means of structural encoding (in contrast to structural representations). Being able to deal with systematicity is a necessary requisite to meet the rationale constraint, in so far as reasoning involves cognitive processing on structured domains. It is too early to evaluate this proposal. Anyway, the issue is still open.

PART II. FOLK EPISTEMOLOGY AND EPISTEMIC INTUITION

1. FOLK WISDOM

Ordinary people are said to be endowed with several systems of knowledge: one that allows them to predict and explain the behaviour of others in many normal circumstances; another that allows them to predict and explain physical events in everyday contingencies; a third one that allows them to differentiate animal and vegetal species attending to their most distinctive features. These systems have received the names, respectively, of folk psychology, folk physics, and folk biology. There is much controversy about the nature and the extent of this knowledge. In the case of folk psychology, it is disputed whether it is innate or acquired (Scholl and Leslie, 1999), whether it is encapsulated or available for general processing (Gopnik and Meltzoff, 1997), whether it consists of bodies of specific knowledge or it requires specific processing mechanisms (Samuels, 1998), whether it is culturally determined or evolutionarily prearranged (Tooby and Cosmides, 1992).

There are other possible candidates for folk wisdom: folk mathematics, folk linguistics, and so on. We want to consider a variety that we can call "folk epistemology". Folk epistemology (FE) is concerned with people's knowledge about knowledge. Some questions that FE is interested in are: how people know (believe, etc) that other people know (believe, etc); in what cases people judge that a person "knows" instead of "believes"; what elements are relevant to these judgements, etc. There are two obvious relationships that come to front when one considers FE: its relation to folk psychology, and its relation to epistemology. *Prima facie*, FE seems to be a part of folk psychology. Epistemic notions such as knowing, believing, understanding, remembering, etc, are part of the set of propositional attitudes which we use in our explanations and predictions of behaviour. Hence whatever things people know about knowledge is simply a province of whatever things they know about each other's minds.

However, if we take a closer look, folk epistemology does not seem to be concerned so much with the prediction or explanation of behaviour as with a particular *categorisation*. We can compare it with folk biology. Folk biology is not concerned with, say, what the tiger will do next or why it crouched instead of running. This is a piece, if anything, of "folk tiger psychology". Folk biology is more concerned with telling a tiger from a lion, recognising different types of plants, distinguishing which plants or animals may be

edible, and so on. It is a system of knowledge directed to classify different parts of the world in intelligible categories. There may be explanation and prediction in folk biology, but this is not its primary purpose. This contrasts with folk psychology, where explanation and prediction are in the foreground.

Similarly to folk biology, FE deals principally with a particular kind of categorisation, namely, epistemic categorisation. Its chief purpose is to reach a judgement about the epistemic state of an individual (who can be the judge herself). Typical questions that FE makes are: "does he know X?", "does she believe Y?", "is he justified in believing that Z?", and so on.

There are at least some natural situations for FE to operate. For instance, in a *learning situation* a teacher wants to find out if her pupil knows the lesson, if he really understands, if his erroneous answers are due to his ignorance or to some teaching failures (e.g., a bad book), etc. A second example is *cheating*, in which a potential cheater wants to find out if someone knows X, or a suspicious individual wants to find out if someone is telling a lie.

One might object that those are cases that properly belong to folk psychology, or the art of mindreading. Mindreading is all about attributing mental states to other people (Davies and Stone, 1995), so that their behaviour can be explained in terms of those mental states. However, there are two aspects to mindreading that do not have to be conflated. One of them is *mindreading* in the strict sense, that is, it is directed to find out what mental state the other person is in. The other aspect is *mindreading* in a *projective* sense: we use our knowledge of the other person's mental states to determine what she is going to do next or why he did what he did.

The difference between both aspects is much like the difference between theoretical and practical reasoning, distinguishable even if closely related (Harman, 1986). Sometimes we just stop short of reaching an explanation or prediction, i.e., our goal can be merely to attribute a particular mental state. For instance, in the mentioned learning situation the teacher's task ends when she finds out the state of knowledge of the pupil: there is not any further behaviour or mental state to explain or predict on the basis of that finding. At any rate, both processes, *mindreading* in the strict sense, and *mindreading* in the projective sense, can be kept separate. Now we can see that FE has to do with *mindreading* in the strict sense. It deals with the attribution of mental states, but not any kind of mental state, only with those of some epistemic bearing. The point is to find out whether someone really knows, merely believes, and so on. Even if we understand folk psychology in a broad sense so that FE is a province of it, FE can still be regarded as a set of distinct abilities within the complex system of capacities that constitute folk psychology.

This can be also observed in the case of folk biology. Suppose we are wondering why Peter is running scared. We look around him, see a snake, and understand his behaviour. Now, in this process we have used a piece of our folk biological knowledge, namely, that the creature over there is a snake, and that snakes are generally dangerous. Yet, we would not say that in doing this we make folk biology a part of folk psychology. Just as we do not say that any of the other cognitive capacities involved in explaining his behaviour belong to folk psychology —e.g., capacities like vision or hearing.

The second important relation of folk epistemology is with epistemology. In what sense is FE, as a mental ability or set of abilities, a part of epistemology, as a philosophical project? Traditional analytical epistemology seems to depend on FE. It relies on epistemic intuitions in order to set forth a number of principles or criteria, that constitute the epistemic notion which is the focus of the analysis. In other words, if a proposed analysis maintains that, in order to qualify as knowledge, a state has to possess such and such features, then there cannot be a paradigmatic example of knowledge that lacks those features, and there cannot be a clear example of non-knowledge that has those features. This analysis can be made for mere descriptive purposes. However, for many epistemologists the proper end is to reach a normative stage, in which we can say, for instance, that in order to attain a state of knowledge such and such conditions must be met, and such and such epistemic states and processes are the right ones.

In the following sections we will try to clarify the notion of folk epistemology, examining its putative role as a cornerstone of a number of epistemological projects, and its relation with folk psychology.

2. THE VARIABILITY OF EPISTEMIC INTUITIONS

People can judge immediately and effortless about epistemic states, both their own and alien. Part of the job of a clever epistemologist is to arrange situations to test our judgements, in order to reveal hidden features of the epistemic situation. The best cases for analysis are those that, by triggering a widespread intuitive judgement about the attribution in question, get to challenge established analyses of an epistemic notion. This is the strategy followed in the well-known Gettier examples (Gettier, 1963), and this is why they drew so much attention.

Now, suppose we have cases in which people's intuitive judgements do not agree. Then their usefulness for the epistemologist would not be so clear. Hence it might be the case that they could not support the epistemological project because the variance within epistemic intuitions is too large. For instance, Weinberg, Nichols and Stich (forth.) have argued that our intuitive

judgements about when somebody knows or does not know X are subject to the influence of cultural and socioeconomical factors. In a series of experiments, they compared the epistemic intuitions of European Americans with those from people of Asian origin. Their results showed significant differences between both groups. For instance, when they used Gettier cases as the testing material, Asians tended to attribute knowledge to the person described in the case, a tendency opposite to Western subjects (and contrary to widespread intuitions in Western philosophical community). They also compared epistemic intuitions from people with low and high socioeconomical status, showing that the former were more ready to attribute knowledge in problematic cases than the latter.

Weinberg et al. use their results as an argument to attack what they call "intuition driven romanticism". This is a family of epistemological projects that take epistemic intuitions as data in order to produce normative claims about epistemic matters, such as evaluating the merits of different belief formation strategies or judging that certain doxastic structures constitute real knowledge. The fact that different sets of intuitions can lead to different normative epistemological theories should cast doubt on those normative claims. Given that there are no independent grounds to assert that Western epistemic intuitions are more granted than intuitions from Asian people, the epistemologist that relies on typical Western intuitions cannot avoid the charge of being engaged in an ethnocentrist project.

It seems that Weinberg et al.'s experiments are, to some extent, a case of overkilling. Interesting as their results are, the point they are hammering could have been made merely looking at divergences between Westerners in judging the different examples. In fact, these divergences may appear even within the philosophical community. Even if it is a minority that dissents from the most prevailing opinion, we cannot rule out their intuition simply by "counting heads". So the point could simply be stated thus: how is it that normative principles following from your intuitions are more sound than normative principles that derive from my intuitions, or vice versa? Are they merely differences in opinion?

To clarify these matters, we need first to examine what sort of thing is an epistemic intuition. However, there is not a univocal way to treat this notion.

3. KINDS OF INTUITIONS

Intuition is usually understood in the vague sense of immediate knowledge whose origin we are not able to state precisely (e.g., it does not come from the senses). Hence if you ask me what kind of tree this is and I just fetch for my botany book and answer "it is a pine", my statement does

not count as an intuitive judgement. In an intuitive judgement one "just knows" that this is a pine and not a fir. Yet between both extremes there is a range of possible judgements that do not fall clearly into one side of the divide. What if I remember having read about it some time? What if I think somebody told me about it? When does it count as "just knowing" it? Even if it is not totally clear when something counts as an intuition, it seems to be the case that we call intuition to disparate types of judgments. We will distinguish four different kinds of cases of intuition.

Case 1: Linguistic intuitions.

We can tell without much effort which sentences are correct in our language (or our idiolect). These intuitions are not learned, neither in a formal nor in an informal setting, in the sense of receiving feedback about the well formedness of sentences. They can be refined, e.g., by taking a course in linguistics or any other method that makes one more aware about the structure of his language. But speakers of the language, even the illiterates, can agree about a vast number of well-formed sentences. Moreover, linguistic intuitions have normative force, in the sense that we can require that "you ought to say it this way, rather than that way".

Linguistic intuitions are not innate, i.e., we cannot have intuitions about language until we have acquired language. The debate about the innateness of language is irrelevant here, because even if there is something like an innate universal grammar that every new born child shares, the linguistic intuitions in question are not about this grammar. The relevant intuitions are about the grammar of the acquired language.

Linguistic intuitions are productive, in the sense that once a person has mastered a language she is able to judge about the well formedness of a (possibly infinite) number of sentences. (We say '*possibly* infinite', even if an infinite number of sentences can be recursively generated by a natural language, because there may be processing limitations that constrain the class of sentences that can be reasonably comprehended by the subject).

Case 2: Folk biological intuitions

A point can be made for the existence of an innate body of knowledge about the natural world. It may be very basic, comprising things like being able to tell an animal from a plant, to tell a human from a non-human, to tell a living being from a non-living being, to expect certain things from animals that one does not expect from non-living beings, and so on. It might not even include the ability to tell a dog from a cat. At any rate, the knowledge that it does and it does not include is something to be determined empirically.

We can locate within this class of intuitions other putative examples of folk wisdom, like folk physics or folk psychology. In this case it is important

whether the alleged pieces of knowledge are innate or not, especially to distinguish them from the third class of intuitions that we will present. In other words, the assumption is that if there is such a thing like folk knowledge, it is hardwired. It was provided by evolution through a lengthy period of time as the individuals coped with various environmental challenges. A related assumption is that this knowledge is (more or less) fixed and universal, so that individuals going through different ontogenetical processes would end up with (more or less) the same set of folk intuitions. So it is not productive in the sense explained for linguistic intuitions.

Case 3: Biological intuitions

Most of our wisdom about the natural world is acquired through a process of enculturation. We learn facts about dogs, cats, apple trees and the like and we begin to accumulate a database that is increasingly organized in a web of implicit knowledge. Maybe "intuition" does not sound as a good label for this kind of knowledge, because we tend to regard intuitions as something more obscure, whose origin is somewhat deep and mysterious. But the chief feature of intuition is that it drives spontaneous, immediate judgements, so there is no reason why we should not treat all this culturally acquired knowledge as the basis for intuitions.

In contrast with folk biological intuitions, biological intuitions are not innate. They are not hardwired in any sense. They are learned, sometimes in formal settings, sometimes simply picking information from the interaction with conspecifics or with the world. Biological intuitions are not productive either, at least they are not so in the sense in which linguistic intuitions are. One can be happily ignorant about vast portions of biological facts, say, that X is a wolf and not a dog. However, one cannot be happily ignorant about his language. If you think that it is possible to say "the dog runs" but not "the wolf runs", then there is something deeply wrong about your mastery of English. Maybe the reason for this difference has to be found in the nature of the domain to which biological and linguistic intuitions apply, i.e., maybe our linguistic intuitions are productive in the referred sense because language is productive itself. Be it as it may, the difference with biological intuitions still remains.

Another difference with linguistic intuitions is that the latter do not arise through a process of enculturation. Even if you want to say that learning a language is a form of enculturation, there is still a notable difference. Learning a language is not a matter of acquiring a great number of facts systematically related, in contrast with the sense of enculturation that seems to be in charge of learning biology. In fact, it is reasonable to assume that most of our tacit knowledge about the world has been acquired in this way. Thus it is possible to have intuitions of this type about almost everything.

An important difference between linguistic intuitions and the other three classes is the normative force of the former in order to construct a theory about the domain in which they apply. In other words, it would be a failure to propose a theory of French grammar or a taxonomy of French sentences that took as well-formed expressions which were unacceptable by French speakers. In contrast, it would not be preposterous to propose some animal taxonomy that overtly clashed with ordinary intuitions (e.g., by classifying fungi closer to animals than to plants). Yet, there is a sense in which biology is also a normative project: it relies on an attempt to obtain the *right* classification by attending to the relevant features.

Case 4: Moral intuitions

Many people seem to have strong intuitions about what is right and what is wrong. In one sense these intuitions seem to belong to the same class as the biological intuitions presented just above. For one thing, they are also clearly obtained through a process of enculturation. There are attempts to uncover whether there are general moral rules that are endorsed universally by people from very different cultural backgrounds. Yet, even if such standards exist, here more than in any other domain variability is the norm. In fact, it is the extreme disagreement in moral judgements and its extreme resilience to change that suggests that they constitute a different category of intuitions. Barring the existence of a "moral core", moral intuitions would not be innate nor productive. They have, however, a strong normative nature, telling the person who holds the intuition what ought and ought not to be done.

4. THE NATURE OF EPISTEMIC INTUITIONS

So, what kind of intuition is a folk epistemic intuition? The answer to this question seems to constrain the uses to which epistemic intuitions may be put. To begin with, we have been conflating two different kinds of intuitions in the notion of epistemic intuition. We began by talking of folk epistemology, and then we moved to talk about epistemic intuitions. Yet, it may be the case that they do not have much in common. To put it another way, it may happen that folk epistemic intuitions are just a small subset of epistemic intuition *tout court*. Folk epistemology would be with respect to epistemic intuition much the same as folk biology is with respect to biological intuition: a nucleus or foundational cornerstone on which the rest of the intuitions can be built.

Let us consider first that epistemic intuitions are like linguistic intuitions. This appears as the most attractive hypothesis for the epistemologist, because linguistic intuitions have exactly the features she needs for her

project: they have normative force and they are shared within a linguistic community. If epistemic intuition is like linguistic intuition, we can make then a comparison between variation in epistemic judgements, and variation in language. Each set of (culturally or socioeconomically influenced) epistemic intuitions would count as a "different language" and the task of the epistemologist would be to disclose the normative structure of such epistemic language.

Yet, it seems that this is not what many would desire for epistemology. The whole point, one can protest, is precisely to produce a normative framework that is over and above any sources of variation. This would be similar, thus, to the linguistic task of devising a universal grammar. Epistemic intuitions from all sources would provide the data for the epistemologist to abstract and organise. Comparative studies would be needed, appropriate taxonomies should be furnished, and so on and so forth. Ideally, the end product would be a theory of the competence that complemented with the pertinent performance factors would account for all the range of variation in epistemic judgements.

There is a problem with this view, though. There is a sense in which theories of particular grammars are not defeasible. Suppose we find in a remote part of England a community that shares the entire English vocabulary, but that uses postpositions instead of prepositions. It may not reasonable to conclude that "the woman red in is dancing you with" is an English sentence. Rather, we might conclude that "they do not speak English", i.e., that the peculiar community speaks a different language (call it Schmenglish) with its own particular grammar. Yet, a theory of universal grammars can be defeated by such findings. Suppose that it is a rule of our UG that interrogative sentences are not constructed by reversing the order of the words in a declarative sentence (cf. Pinker, 1994), and suppose we find a remote community that follows exactly this procedure for interrogative sentences. Then it would be reasonable to call for a revision of our UG. (Notice that taking a line similar to the English/Schmenglish case would amount to the absurd claim that the community in question "does not speak a (human) language"). It is precisely its universality aims that make the search for a UG prone to those kinds of revisions.

Thus, if the comparison between epistemology and linguistic is apposite, and we want to characterise the former as the search for a "universal epistemic grammar", then the conclusion is that epistemology is also revisable in the face of diverging epistemic intuitions. It is not enough to state that, say, Asians and Westerners have different "epistemic grammars"; we need, in addition, a general framework that encompasses the intuitions of those divergent grammars. The trouble is that while the lack of normative standards to prefer a language over another is no obstacle to the linguist, it

seems that the epistemologist *must* decide between the divergent intuitions offered by different people. The (linguistic) UG has to offer a scheme that allows for the possibility of the distinct linguistic variations. The "universal epistemic grammar", in contrast, has to explain how the variations are possible, but also how and why some of them are better than others.

A second possibility about the nature of epistemic intuitions is that they are much like artistic or moral intuitions. But to argue this is not an easy task. First, variability is much greater in moral or artistic intuitions than in epistemic ones. There are not only marked differences between individuals from disparate cultural or socioeconomical groups, but even between individuals that belong to the same group. Second, while epistemic intuitions seem to be much more malleable by explicit formal training (e.g., taking epistemology classes), moral intuitions seem to be much more resilient to change from that kind of pressure. If epistemology lectures were just another form of indoctrination, then it is difficult to see why explicit formal indoctrination of that kind fails in the case of inculcating a particular set of moral values.

Let us consider now the third possibility that epistemic intuitions are like biological intuitions. On the one hand, like the latter, epistemic intuitions are not innate. Whatever concepts and beliefs we may attribute to newborn babies, it is unreasonable to impute epistemic judgements to them. We do not imagine babies going on in the business of trying to determine whether their mothers "really know" that they are wet. Epistemic intuitions are not explicitly acquired, at least for the vast majority of the mortals that do not attend epistemology courses. So, where do epistemic intuitions of epistemologically untrained individuals come from? An appealing hypothesis is that they come from *informal* enculturation, just like a substantial part of our ordinary bodies of knowledge. The common fellow does not attend epistemology seminars but he is engaged in social interactions in which people are judged to know something, behaviours are labelled as rational or irrational, beliefs are considered warranted or unwarranted, and so on.

Supposing, then, that epistemic intuitions are much like biological intuitions, it seems that epistemology is doomed. Because it is an outright feature of biological intuitions that they can be defeated: they do not have normative force. We can save the day, however, accepting that, even if biological intuitions do not have normative force, *biology does*. In other words, the reason why an intuitive judgement like, say, that fungi belong to the same broad class as plants, can be overruled, is that there is a systematised, reasoned and thoroughly elaborated corpus of biological findings. To be sure, any of the statements that belong to a theory of biology can be defeated as well, but they cannot be defeated without a good reason.

Namely, the fact that they clash with non-reflective folk intuitions is not such a good reason.

These considerations obviously appeal to the role of expertise in deferring our common biological intuitions. The point is now whether there is a similar role for expertise in epistemology. Weinberg et al. reject this possibility. They consider a number of objections which say that in their experiments they are probing the wrong sort of intuitions, because the right sort is accompanied by a sense of necessity, or require a minimum of reflection, or emerge after an extended period of discussion and reflection. In fact, all these objections can be regarded as variations in the single theme of the role of expertise. The idea is that some people may have a greater epistemological authority simply because they have devoted more time to think about epistemic affairs, an idea that they discard.

However, the surprising consequence of this view is that it implicitly endorses the assumption, shared by the "intuition driven romanticists", that intuitions are incorrigible in a relevant sense. Or to be more precise, in this view intuitions are malleable but incorrigible. They are malleable because they are subject to cultural and socioeconomical and other kinds of influences. But they are incorrigible because we are not entitled to say that a particular set of intuitions is wrong, on pain of committing the sin of ethnocentrism.

In our view, the possibility of scepticism tells against the idea of incorrigibility of our epistemic intuitions. If (at least some variety of) scepticism is correct, then claims about knowledge are mere loose talk. The most we would be able to reach is a state of belief. If scepticism were a strong position, then it would seriously undermine most of the epistemic intuitions which apparently are universal, namely, that knowledge constitutes a distinct class of mental state attributable to people. In fact, the extraordinary urge to fight scepticism may well be motivated by its threat to this strong and widely shared intuition. This does not prove the primacy of the intuition, only its strong appeal, which is undermined by the sheer possibility that scepticism might be right. But it is not possible to maintain at the same time both that epistemic intuitions are incorrigible, and that scepticism is a possibility, because one of the consequences of scepticism is that intuitions are not incorrigible after all. We are not sure if somebody would like to use this as an argument against scepticism. We regard it as an argument against the incorrigibility of epistemic intuitions, because the possibility of scepticism must be taken for granted in any sensible epistemological project.

Maybe the claim is not, after all, that epistemic intuitions are incorrigible. The claim may be that even if some of them were right and others were wrong, a problem remains to tell which are the good ones. This is something,

the argument goes, for which we do not have an appropriate set of criteria. If intuitions diverge through, say, different social groups, what basis do we have to claim that an epistemological theory based on a particular set of intuitions has greater normative force than its alternatives?

But now the question looks much like the classical problem in philosophy of science of determining what makes a scientific theory a good theory. This is not to say that it is an easy problem, but it is not hopeless, in the sense that there are at least a number of alternatives to handle it. A possible answer to the question of when an epistemological theory is good, as in the biological case, lies in the success of the theory, in terms of accounting for a larger number of facts, or its internal coherence, or its predictive power, and so on. This is a theme to be pursued in philosophy of science terms, as a variation on the theme of how to evaluate different scientific theories. And this amounts to saying that to do epistemology is to do a particular kind of science. The best experts will be those that take into account the greater range of available data, including data from cognitive science, and that can explain the factors that make some epistemic intuitions right and others wrong. This would be good news for those that envision a naturalistic project for epistemology, whose aims and methods would be continuous with those from cognitive science.

5. FOLK EPISTEMOLOGY AND FOLK PSYCHOLOGY

We began part II talking about the relation between folk epistemology and folk psychology, and we want to conclude by drawing some morals that are common for both of them. First, we pointed out that folk epistemology shares its ontology with folk psychology. Yet, as we remarked in part I, the status of this ontology is very much in dispute. Folk epistemology inherits these problems. On the one hand, it may happen that typical epistemic notions do not carve epistemology at the proper joints. What we call knowledge may refer to a number of disparate states that might correspond to types of neural states, types of environmental states, or both. In addition, it may be the case that we are utterly wrong when we judge that somebody (or even ourselves) knows or believes or understands X. Phenomena like confabulation (Nisbett and Wilson, 1977) or self-deception (McLaughlin and Rorty, 1988) cast some doubt on the possibility that epistemic conditions for the attribution of knowledge can customarily be satisfied. If there are no clear and distinct conditions of success for the case of self-knowledge, and if attributions of knowledge depend to some extent on the own case, then the

project of developing an epistemology based on folk epistemology seems to be in trouble.

Conversely, the reflections that we just made about the nature of epistemic intuitions can be made applicable to folk psychology. In other words, it might be the case that in both cases there is a core of "folk wisdom", a core that may well be evolutionarily fixed and universally shared; but it might also be the case that much of the discourse that is taken for folk psychology could be just like the discourse of epistemic intuitions, i.e., utterly subject to cultural, socioeconomical, psychosocial or other kinds of influences. If this is the case, the conclusion is not necessarily a relativistic position towards epistemology and psychology. Rather the conclusion is that both of them may be revisable in the face of empirical findings and theoretical developments obtained underneath a scientific umbrella. This umbrella is not other than cognitive science. In this case, much of the epistemological debate as well as much of the folk psychology debate will have the shape that debates take in philosophy of science, and they will have to be tackled in the terms and limits proposed by this discipline.⁶

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MUSIC, LANGUAGE AND COGNITION: WHICH DOESN'T BELONG?

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INTRODUCTION

When the topic of "Music, Language and Cognition" was first proposed to me by the organizers of this Colloquium, I at once was reminded of the game where a child is shown, for example, pictures of an apple, a banana and trumpet and asked "Which one doesn't belong?" I assume you all know the answer.

But which doesn't belong in the triad of music, language and cognition? If you answer that they *all* belong, as some people might, then you are in disagreement with me. For my answer is: *language* doesn't belong. Music is, indeed, language-like in certain respects. Nevertheless, it is not language: it is not a language or part of a language. And thinking it is any one of these things has caused a good deal of confusion.

Certainly, though, music and *cognition* belong together. For, on my view, anyway, the enjoyment and appreciation of music are deeply involved with the cognitive processes. As I put the point some years ago, "music is not a stimulus...: it is an object of perception and cognition, which understanding opens up for...appreciation."¹

¹ Peter Kivy, *Music Alone: Philosophical Reflections on the Purely Musical Experience* (Ithaca: Cornell University Press, 1990), p. 41.