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The Concept of Innateness and the Destiny of Evolutionary Psychology

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According to a popular version of the current evolutionary attitude in cognitive science, the mind is a massive aggregate of autonomous innate computational devices, each addressing specific adaptive problems. Our aim in this paper is to show that although this version of the attitude, which we call GOFEP (Good Old Fashioned Evolutionary Psychology), does not suffer from fatal flaws that would make it incoherent or otherwise conceptually inadequate, it will nevertheless prove unacceptable to most cognitive scientists today. To show this, we raise a common objection to the concept of innateness, not to mount an attack on GOFEP but to study how its proponents have attempted to meet that challenge. The aim of this move is to show that GOFEP cannot face the challenge without, as it were, losing its soul. There is, or so we will argue, something deep at the heart of GOFEP that prevents its proponents from meeting the challenge.

An *evolutionary attitude* is gaining momentum in the cognitive sciences and it now seems certain that this new attitude will leave its mark on the field just like the cyberneticists' and computer scientists' information processing attitude left its mark a generation or so ago. Although the attitude covers a vast and eclectic domain, including the study of animal behavior, paleoanthropologic studies of cognition and behavioral genetics, an influential group of researchers (Buss, 1999; Cosmides and Tooby, 1997; Pinker, 1997) recently monopolized the expression evolutionary psychology to describe a form of psy-

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chology defined by the three following theses, which we construe here as characterizing the nature of the human cognitive architecture:

1. *(Massive) modularity of mind.* The human cognitive architecture is mainly made up of information processing computational modules, which, to a great extent, are domain specific and informationally encapsulated, *inter alia.*
2. *Adaptationism.* The human cognitive architecture is a product of natural selection.
3. *Innateness.* The human cognitive architecture is the phenotypic expression of the human genetic heritage, which is, more or less, shared by all humans.[1]

In short, the mind, according to this fashionable version of the evolutionary attitude in cognitive science, is a massive aggregate of autonomous innate computational devices, each adapted to solve specific adaptive problems. In the present context, we will need to distinguish between this important version of the evolutionary attitude, and other possible instantiations of the same attitude. Due to its importance and somewhat orthodox character, we shall call this version of the evolutionary attitude *good old-fashioned evolutionary psychology* or GOFEP.[2]

Our aim in this paper is to show that GOFEP, although it does not suffer from fatal flaws that would make it an incoherent version of the attitude, will nevertheless prove unacceptable to most cognitive scientists today. GOFEP is, in many ways, an unpalatable mix of old stuff: orthodox evolutionary theory mixed with classical computational cognitive science.[3] As the title of the paper suggests, much of the blame for this can be put on the concept of innateness as it is used by GOFEPs (*good old-fashioned evolutionary psychologists*). To show this, we will raise a common objection to the concept, not to mount an attack on GOFEP, but to study how its proponents have attempted to meet that challenge. The aim of this move will be to show that GOFEP

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[1]By defining the three theses as characterizing the nature of the human cognitive architecture, we do not mean to suggest that the cognitive architecture of animals is necessarily different from that of humans. Indeed, an evolutionary attitude in cognitive science would, at least prima facie, assume the reverse. We simply want to stress that evolutionary psychology is first and foremost focused on human cognitive abilities.

[2]GOFEP is what Quartz (2001) calls narrow evolutionary psychology and Heyes (2003) human nativist evolutionary psychology.

[3]Although we believe that it is the case that GOFEP is at home in the classical symbolic paradigm in cognitive science (see Fodor, 1975; Pylyshyn, 1984), nothing specific here will turn on this. However, we believe, as we'll explain below, that evolutionary psychology has adopted from the classical computational paradigm some secondary theses that will prove to contribute to its fall from grace.

cannot face the challenge without, as it were, losing its soul. There is something deep at the heart of GOFEP that prevents its proponents from meeting the challenge. In the last section, we will detail one way to face this challenge that we think is true in spirit to the original attitude that motivated proponents of GOFEP (that is true to the desire to reintegrate psychology in a biological framework).

The Explanatory Role of Innateness

As is well known, cognitive science gets the bulk of its explanatory power from computer science. In particular, computer science gives cognitive science license to use certain traditional (philosophical and psychological) intentional notions (information, representation, belief, desire, and so on), notions that were banned from psychology by behaviorists. The rationale was quite simple: If mere computers, machines we could build in our garage, get to process information and have goals, why couldn't humans? And if computer scientists had license to use such notions, again why couldn't psychologists?

Cognitive science also gets to use computer science's engineering (or design) stance. Any device built by an engineer can be taken apart by another engineer who is trying to understand how the device works (usually to copy the device or improve its design). That is, to use philosophical lingo, reverse engineering is undertaken to produce functional explanations of systems. Given that cognitive science's ultimate goal is to understand *how the mind works*, as Pinker (1997) puts it, that is, to produce a functional explanation of the mind as a cognitive system, why couldn't cognitive scientists borrow computer science's (and engineering's) practice of reverse engineering? But the rationale here is trickier. Engineers get to reverse engineer systems *because* these systems were built by a rational agent, with goals (design specs, in the best of cases) and desires (make money, keep her job). Assuming these goals and desires, plus rationality on the part of the engineers that built the device, reverse engineers can figure out the function of the device's various components (or at the very least try to). But imagine irrationality on the part of the designing engineer, say of a car. She wants to design the best car she can and knows that steel is the best material to build sturdy bumpers. She also knows that no obstacles (financial or otherwise) prevent her from using steel in the car's design. She believes and desires all of this (plus everything else philosophers of action say should be present for rational action) and yet she builds the bumper out of . . . bananas! Generalized, such irrational behavior makes the car impossible to reverse engineer. The practice of reverse engineering only works for rationally designed systems.

But are the systems studied by cognitive scientists rationally designed? They certainly were not built by a rational engineer in it for the money. It is true that cognitive systems were, in a way, designed by natural selection. But is natural selection a rational designing process? Natural selection is certainly not rational the way human engineers are. And it certainly doesn't have beliefs, goals and desires the way humans do. But natural selection is akin to human engineering in a way. It can be argued that beliefs, desires, etc., and rationality, make human engineering practices into a design-optimization process. Likewise, the algorithm implemented by evolution (a kind of *generate-and-test* procedure, see Dennett, 1995) makes natural selection into a design-optimizing procedure (or so we can hope — but see Gould and Lewontin, 1979). So at an abstract procedural or algorithmic level, natural selection and rational human engineering are both design optimization procedures, which is all that is needed to license the practice of cognitive reverse engineering.

We saw that natural selection is, or can be thought of as, a design optimization procedure because it implements a *generate-and-test* algorithm. But a necessary element of such algorithms is the *preservation of good designs* (or good tricks, as Dennett puts it). A system simply can not implement the algorithm if it does not find a way to preserve good designs.[4] Now, there are various ways to preserve successful designs. On the orthodox brand of evolutionary theory at the heart of GOFEP, here is the way natural selection does it. Partition an organism into a number of quasi-independent (Cummins, Cummins, and Poirier, 2003) traits. Some of these traits will be better designed than others (some may not be designed at all). And some (maybe most) of these traits will be phenotypical, that is, by definition, determined by the organism's genotype. Genotypes, of course, *are* reliably copied (more or less, but rather more than less). The reliable copying of genotypes ensures, given genetic determination, *inter alia*, the preservation of successful designs. It follows that only phenotypical traits can be *designed* (other traits may be there, and they may even be good, but they will not be *designed*). Since GOFEP understands innateness as genetic determination, it follows that, on that orthodox account, innateness (genetic determinism) is a necessary part of the explanation of the preservation of successful designs, which is why natural selection is a design-optimization procedure, which in turn is why cognitive systems built by natural selection can be reverse engineered.[5]

In short, cognitive scientists, it seems, can only understand their practice as reverse engineering if they can reasonably assume that cognitive systems are

[4]Of course, an important element of the generate phase of the algorithm is that preservation should not be too faithful, since that would freeze the optimization process to its current state. One needs to balance the need for preservation of designs with the need to generate a variety of designs. In the case of natural selection (on the standard account, and simplifying), preservation is provided by the cell's DNA copy mechanism and variety is provided by mutation, cross-over, and so on.

[5]Pinker (1997, p. 21): "the module logic is specified by our genetic program."

the result of some optimization procedure which, in the case of natural systems, seems to require innateness. Innateness buys cognitive science reverse engineering (it is a necessary part of a sufficient mechanism for the preservation of successful designs). And reverse engineering is, many would admit, a nice tool for cognitive scientists to have.

But innateness also buys cognitive scientists another nice tool: normative functional talk. The story here exactly parallels the one just told, so we can proceed somewhat faster. For many purposes (e.g., quality control, repair, etc.) it is important for engineers to be able to partition the set of all artifacts implementing a given design into those that function properly and those that don't. We all know that one shouldn't fix things that "ain't broken." Few realize, however, that this everyday maxim is deep to its neck in normative functional talk. Fortunately, engineers have a straightforward means to tell apart functioning from malfunctioning items: refer to the design specification manual, which is a detailed exposition of the intentions or goals that motivated the design process, and then see if, *ceteris paribus*, the actual behavior of the system diverges markedly from the design specifications. Software engineers, for instance, will spend a good amount of time (depending on the size of the software project) writing design specifications manuals *before* the programming process starts and then will evaluate various programs created according to how well they satisfy the design specs.[6] Likewise, it is useful for many purposes (notably treatment) for cognitive scientists to tell those cognitive architectures (modules, sub-systems, etc.) that are functioning properly from those that are not. Unfortunately, cognitive scientists cannot refer to the intentions or goals behind the design organism because natural selection has no goals or plans. And, obviously, natural selection does not spend time laying down its design specifications in manuals that cognitive scientists could consult. But although it does not have goals and plans, and does not write specification manuals, natural selection, like engineers, designs systems to solve problems (or so it may be argued). And one may use knowledge of the problems natural selection solved through its designs as a kind of design specification and, as it were, write down natural selection's design specification manual (see for instance Cosmides and Tooby [1999] for an application of this idea to the domain of psychopathologies). But this is possible only if one thinks that natural selection is in the business of solving problems (problems posed, as it were, by the environment).[7] The belief that natural selection is involved in problem solving is called adaptationism, which, as we saw, depends on innateness.

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[6]They even design methods and programs to evaluate how well programs respect the original design specifications, and methods and programs that evaluate the quality of design specifications, given finished products. Software engineering is big business.

[7]This only works, also, if we can tell what those problems were, but that is another question altogether, which we do not address here.

In short, innateness buys cognitive scientists adaptationism, which, in turn, gives them license to write down natural selection's design specifications for the mind — that is mainly what cognitive science is about, according to GOFEP (and many cognitive scientists inspired by the evolutionary attitude). Once written, that design specification manual can then be used to determine whether token cognitive architectures (modules, sub-systems, etc.) are working properly, or whether they are malfunctioning in some way. That is, innateness buys normative functional talk (it is a necessary part of a sufficient justification for normative functional talk). And normative functional talk is, many would admit, another nice tool for cognitive scientists to have.

Cognitive scientists armed with reverse engineering and normative functional talk will be able to explain things other cognitive scientists can't. That extra explanatory power is the main reason, we believe, why the evolutionary attitude has caught on in cognitive science. And we saw that innateness is a necessary part of the way GOFEP spells out the evolutionary attitude. Hence, one cannot simply dismiss the concept (as an innateness eliminativist would) without also worrying about loss of explanatory power. Of course, one might bite the bullet and claim that it was all an illusion to begin with (such cognitive scientists would want to go back to plain old cognitive science). In what follows, we agree with GOFEP that such explanatory power is worth fighting for.

A Challenge for Evolutionary Psychology

Things, of course, could not be that simple. Many biologists and philosophers of biology now reject the idea that phenotypic structures (including behaviors and cognitive mechanisms) can be genetically determined (Gould, 1976; Griffiths, 2001; Griffiths and Gray, 1994; Nijhout, 2001; Rose, Kamin, and Lewontin, 1984).

The rejection of genetic determinism by contemporary biologists poses a challenge to GOFEP, a challenge that, we believe, it cannot face without losing its soul. Of course, GOFEPs are well aware of this challenge (our purpose here is not to remind them of it), and we shall see below how they intend to handle the challenge. What we propose to do, instead, is frame this challenge in the most general terms possible, by posing and justifying two general methodological constraints in science, which we call the *vertical integration constraint* and the *no explanatory vacuity constraint*. Most will agree, we believe, that any adequate scientific theory must respect the two constraints. We'll then see how the general constraints impose specific demands on GOFEP. In the next section, we'll study how GOFEP has attempted to deal with the challenge *in order to explain why it can't do it*, that is, not without rejecting other central elements of their research program (this is what we mean by losing its soul). We conclude by sketching a form of cognitive science that is in line with the evolutionary attitude and that, or so we claim, respects the two constraints. We first state, explain and justify each constraint in turn.

The vertical integration constraint. Proponents of GOFEP have quite rightly distanced themselves from positivistic models of the unity of science, which were based on a syntactical conception of intertheoretic reduction. Like most contemporary proponents of the unity of science, they posit the relevant kind of intertheoretic relation at the semantical level and recommend what they call "conceptual integration" (Cosmides, Tooby, and Barkow, 1992), which simply claims that theories from various fields should not contradict themselves, that is, theories should be mutually consistent. Since Cosmides et al. have explicitly formulated their conceptual integration principle in normative terms, we'll call their principle the conceptual integration constraint.[8]

Cosmides et al. mention that conceptual integration is identical with Barkow's (1980) vertical integration but mention that they prefer the former to avoid the connotation that vertical relationships between disciplines imply some epistemological or status hierarchy among sciences (p. 13). We agree with the "status" part of this but not the epistemological since vertical relationships do imply at least one epistemological hierarchy among the sciences (we'll explain why in the next paragraph). Moreover, these vertical relationships are directly relevant to the evaluation of mutual constancy among the theories, hence to the conceptual integration constraint. This is why we prefer Barkow's vertical integration and speak henceforth of the vertical integration constraint.

Cosmides et al. point out that "certain disciplines exist in a structured relationship with each other" (p. 13), by which they mean that "lower" disciplines deal with "principles that govern more inclusive sets of phenomena" (p. 13). We agree. And we agree that these relationships do not give lower disciplines immediate epistemological priority over the higher ones (see their Lord Kelvin example, which is quite to the point). But we do not think that these vertical relationships are completely epistemologically inert. To see this, ask yourself what it means for theories from two disciplines to be mutually consistent. It means, as they explicitly state, that they should not contain contradictory sentences. What are contradictory sentences? In predicate logic, contradictory sentences are sentences made up of the same concepts expressing terms (constants and variables), where one is negated and the other not (e.g., snow is white *and* it is not the case that snow is white). Consistency is pretty easy to determine in formal languages, where terms are univocal (express one concept). But this is not the case in science. Take the term mass in Newtonian mechanics and in relativistic mechanics. How can you determine

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8 ". . . the principle that various disciplines in the behavioral and social sciences *should* make themselves mutually consistent, and consistent with what is known in the natural sciences as well" (Cosmides et al., 1992, p. 4, our emphasis).

if the two theories are mutually consistent? That's where vertical relationships between disciplines become relevant. Recall that, by definition, a theory is lower in the structure if it deals with principles that govern more inclusive sets of phenomena. On that account, relativistic mechanics is lower in the structure than Newtonian mechanics since it deals with all masses, regardless of their speed, whereas Newtonian mechanics only deals with slow masses (relative to the speed of light). The fact that relativistic mechanics deals with a more inclusive set of objects that have mass is part and parcel of the fact that relativistic mechanics gets to define mass. And it is because relativistic mechanics defines mass (*inter alia*) that we believe Newtonian mechanics to be inadequate: its concept of mass does not agree with the accepted definition of mass and so it should be rejected.

We can derive a general epistemological principle from cases like this. In situations of conceptual conflict, that is, where two theories use the same term but give them different meanings, lower theories have conceptual priority. This is one hierarchical relationship between theories that we believe should be preserved, even by philosophers of science opposed to positivism's hierarchy. This is why we prefer Barkow's original term and why we speak of the vertical integration constraint. Two theories are vertically integrated if they are mutually consistent and the lower theory provides the definition of concepts they share.

The no explanatory vacuity constraint. This constraint is an epistemological extension of Ockam's razor: one shouldn't introduce concepts that don't bring epistemic rewards; in short, theoretical concepts should earn their keep. Just like some folks won't shop in stores that don't give out travel air-miles, scientists should not buy into concepts that add no points to their theory's epistemic air-mile card. This is one aspect in which science is different from natural selection: because of phenomena such as gene-linkage, natural selection sometimes allows free-loaders to be selected along with hard-working, productive genes. There is something like gene-linkage (call it concept-linkage) in science. Concepts, as it were, hang together (for semantic, epistemic and psychological reasons we care not to dig into). Be that as it may, scientists, unlike the mindless processes governing genetics, are intelligent agents who should strive to rid their theories of epistemic free-loaders. This means finding them — since free-loaders, in theories as in life, hide their game — and expulsing them from the theories they parasite upon. We take this constraint to be pretty uncontroversial and so shall not spend more time on it. The logic of its introduction should be evident. As we saw, the concept of innateness provides GOFEP with tools that make it more explanatorily powerful than plain cognitive science. Thus, GOFEP, in that regard, respects the no explanatory vacuity constraint. We are about to see, however, that it does not respect the vertical integration constraint. In order to respect that constraint, GOPEPs will have to revise the meaning of their concept of innateness to make it more consistent with biology's. But in doing so, they will have to make sure they do not strip GOFEP of its extra explanatory power. As we'll see, this is easier said than done.

In what follows, we assume that most scientists would agree to fashion their theories so as to respect these constraints. In particular, we believe that GOFEPs will agree with the constraints since we have drawn the more contentious of the two constraints from their own work. Now that we have justified both constraints, that is, shown that any respectable scientific theory should strive to satisfy them, we can state the challenge to GOFEP as follows: Does GOFEP mutually satisfy the two constraints? Given a failure to meet the challenge, are there ways for GOFEP to satisfy these constraints? We contend that there are a few ways one could go. One can attempt to, as it were, revamp the concept of innateness in order to make it in tune with contemporary evolutionary biology. Or one can attempt to define another type of evolutionary psychology, or cognitive science, founded upon biologically respectable notions, and make sure that these evolutionary notions do play an explanatory role in cognitive science. We shall call the first option "revamping innateness," that is, construct a notion of innateness that can be inserted in a GOFEP that satisfies both constraints. And we call the second option "revamping evolutionary psychology," that is, construct a non innateness-based evolutionary psychology that satisfies both constraints. In the next section, we review one substantive proposal to re-vamp innateness and one clever deflationary tactic.

Revamping Innateness: The Canalization Proposal

André Ariew (1996, 1999) claims that biologists should replace the concept of innateness with Waddington's notion of canalization:

> . . . despite the arguments of critics, there really is a biological phenomenon underlying the concept of innateness. On my view, innateness in best understood in terms of C.H. Waddington's concept of canalization . . . (Ariew, 1999, p. 117)

Waddington's notion is meant to explain the fact that phenotypic variance is always much lower than genetic or environmental variance (to say nothing of the variance that would result from their interaction). His key insight was to attribute the difference in variance to development (Siegal and Bergman, 2002). Development, or epigenesis, in some way that is yet to be fully understood, buffers the phenotype against genetic and environmental variation. To use the imagery of dynamical systems theory (to which he himself contributed through his notion of epigenetic landscape), development warps phenotypic space (the space of all possible phenotypes) in such a way that only a few general phenotypes serve as attractors. The developing organism, in this sense, *is canalized* towards one of a few phenotypes among a large set of possible phenotypes, given genetic and environmental variety. Just like all roads lead to Rome (or used to), all developmental pathways lead to one end-state (or one of a few).

As Ariew argues, this makes canalization a good candidate for revamping innateness. Innateness was initially meant, at least in part, to account for phenotypic invariance in a species (universality of body, behavior and cognitive types), the fact for instance that all of Lorenz's chicks would produce certain behavioral patterns. As a bonus, this frees the notion of innateness of its biologically embarrassing all-or-nothing flavor. It is now possible to say that some traits, say limb position or, if we follow Chomsky, grammar, are highly canalized, while others, say native language, are less canalized (i.e., remember, buffered against genetic and environmental variation) and others yet, say hairstyle, are not canalized.

We previously noted that GOFEPs were well aware of the problem facing the use of the concept of innateness in evolutionary biology today. Canalization is how most solve that problem, that is, to put it in terms of the present challenge to GOFEP, canalization is how GOFEPs anticipate the vertical integration constraint will be met, according to Cosmides and Tooby:

> The cognitive architecture, like all aspects of the phenotype from molars to memory circuits, is the joint product of genes and environment. But the development of architecture is buffered against both genetic and environmental insults, such that it reliably develops across the (ancestrally) normal range of human environments. (1997, p. 16)

In short, GOFEP's canalization proposal agrees with contemporary biologists that no cognitive mechanism is genetically determined. However, they believe that the notion of canalization can be put to work in evolutionary psychology: the development of a cognitive mechanism can be said to be more or less canalized to the extent it is insensitive to genetic and environmental variations.

Our point here will not be that canalization is an unacceptable notion in biology nor even that it is inappropriate for biologists to use the notion to revamp innateness. The point, rather, is that GOFEP cannot use the notion to re-vamp *its* concept of innateness. To see this, let's use the expressions innate$_c$ and nativist$_c$ to refer to the canalization-based revamped notion of innateness (and other corollary notions). Note that, on the canalization account, every psychologist who ever said anything about language (Skinner, Piaget, Chomsky) is a nativist$_c$ about language (see Samuels, 2002 for a similar complaint).

Take Skinner, hardly a nativist on anybody's account. He believed that verbal behaviors are events whose probability is conditioned by the presence or absence of certain stimuli (which are called reinforcers if they augment the probability of verbal behavioral events). His ontogenetical account of the presence of verbal behavior was basically a selectional explanation of the occurrence/non-occurrence of utterances. But whatever his account of language might have been, he could not but claim that the development of language is strongly buffered against genetic and environmental variation; in short, that it is canalized. It is a plain observational fact about language that it develops in a variety of environments (geographical, political, cultural, economical, etc.). And it is a plain observational fact that people with various genetic endowments (ethnically, individually) develop language. Only a very few genetic disorders make people mute. Skinner's very account of language learning, whatever it was, had to be constrained by those facts. Had he proposed an account of learning that made the acquisition of language vary with, say, someone's hair color (brown hair reinforcing certain grammatical forms while blond reinforced others, etc.), it would have been blown right out of the waters (even more so than it was by Chomsky's famous attack). Actually, it is worth belaboring this point. No serious scientist would have proposed a theory that so blatantly defies observation. On this nativist$_c$ account, antinativism$_c$ (or empiricism$_c$) not only turns out to be false, but to be irrational. It is a plain fact that constrains all theories of language acquisition that the end-state (possession of a language) is robustly acquired, resistant to all but extreme genetic and environmental insults (like being raised by wolves — an environmental insult if there ever was one!).

We take it that any revamping of the notion of innateness that makes traditionally held positions irrational has gone terribly wrong (again, see Samuels, 2002 for a similar point). The interesting question to ask, here, is why. What makes canalization such an inadequate candidate to revamp GOFEP's concept of innateness, while it is, we agree with Ariew, an interesting candidate to revamp biology's concept?

Canalization exclusively focuses on end-states (or products): something is canalized to the extent its end-state is robust (buffered against genetic and environmental variation). Canalization is absolutely silent about the process that buffers end-states against genetic and environmental variation. Innateness in psychology (including GOFEP), on the other hand, is *all* about process. To say that something is innate it to say that it is in no way learned. Perhaps it grows, to use Chomsky's image, but it is never learned. The fact that something is learned (or not) does not imply anything about whether it is canalized (or not). A canalized mechanism may be learned (learning-based canalization) or genetically determined (gene-based canalization) or a mix of both. To confuse the two is to confuse a quality of the product with a quality of its process.

Couldn't GOFEP simply bite that bullet and reinvent itself with the product-based notion? No. The explanatory power it derives from the use of the concept of innateness depends on innateness being a thesis about process (learning or absence thereof). Both the use of the reverse-engineering heuristic and of normative functional talk require that the cognitive architecture be non-learned (in fact, both require that it be genetically determined).

A Deflationary Tactic: Psychological Primitivism

Faced with the failure of substantive attempts to revamp innateness, GOFEP has another dialectical card up its sleeve. It can adopt a deflationary tactic recently proposed by Cowie (1999) and Samuels (2002, 2004).[9] The tactic is *deflationary* because it meets the challenge trivially. Instead of attempting to revamp the notion of innateness in a substantive way, which would allow GOFEP to satisfy both constraints, deflationists seek to re-interpret what cognitive scientists mean by innateness in way that makes GOFEP's use of innateness automatically respect the vertical integration constraint. In short, the deflationary tactic makes substantive revamping *useless*: there is no need for a cure if no one's ill.

Before we discuss the deflationary tactic, it is important to be clear about what it attempts to do. Doing so will be beneficial in two ways. First, it will help us better understand the dialectic involved in this debate and, second, it will point towards another possible solution to the challenge, in fact the one we'll ultimately favour. We said that the tactic we're about to discuss is deflationary because it makes GOFEP's use of innateness trivially respect the vertical integration constraint. It does so by arguing that the notion of innateness is, and has always been, a psychological notion (the nature of which we'll explain in a moment).[10] Lorenz (and all who followed him up to Pinker and Fodor) was simply wrong in thinking that the notion has any biological import. Now recall that the vertical integration constraint claims that two theories are vertically integrated if they are mutually consistent and the lower theory provides the definition of concepts they share. But on the deflationary tactic, innateness is not a concept psychology and biology share and so biology, the lower-level science according to Barkow's vertical integration scheme, which we wholeheartedly adopt, does not get to define the purely psychological notion. Whatever biologists discover about evolution, genetics and development, and whatever meaning (if any) they attach to their use of the word innateness has no impact on psychology. Hence, the challenge is met, albeit trivially (it is literally like winning because the opponent does not show up).

[9]See also Scholl (2005) for a similar argument.

[10]Like many notions in modern philosophy, "innateness" was then a psychological notion with epistemic properties: innate concepts or ideas were in-born (a psychological property) and a priori (an epistemic property). We only care here about the psychological aspects of the notion, as do the proponents of GOFEP.

In his "Nativism in Cognitive Science" (2002; see also 2004), Richard Samuels proposes an account of the notion of innateness he calls "psychological primitivism." According to this view, a representation or a mechanism *S* is a psychological primitive if it respects the following two conditions:

1. *S* is a structure posited by some correct scientific psychological theory.
2. There is no correct scientific psychological theory that explains the acquisition of *S* (in the baseline sense of "acquisition"). [2002, p. 246]

A psychological primitive is thus a structure that cannot be explained using the resources of correct scientific psychological theories. As Samuels points out, this does not mean that the structure is not in fact acquired or that there is no explanation of how it was acquired, but only that the explanation in question will be given by some discipline other than psychology. To use Samuels' own example, if you can explain the presence of representation *S* in a subject by saying that it was acquired through inference (or any other psychological process), then *S* is not a psychological primitive. If, on the other hand, *S* is the result of triggering or architectural constraints (Samuels, 1998), then it is a psychological primitive.

Given this notion of a psychological primitive, we can explain what it is for a representation or mechanism to be innate: a representation or mechanism *S* is innate just in case it is a psychological primitive (Samuels, 2002, p. 248). On this account, the core of the nativism debate in cognitive sciences concerns the "size of the inventory of psychological primitives": evolutionary psychologists guess that the mind is packed with hundreds, perhaps thousands, of psychological primitives (Cosmides and Tooby, 1995), while others, like Chomsky (1984) and Fodor (1984), limit that figure to a few psychological primitives, while others yet, such as Piaget and Skinner, posit only very few primitives. As we will explain later, we do not side with GOFEPs on this issue, but, for the moment, we would like to consider how good is the deflationist strategy for GOFEP.

It should be clear from the on-going discussion that, although it is a move opened to them, few GOFEPs will actually make the move joyfully, at least if they understand what it involves. Our discussion shows that any evolutionary psychology that bought its concept of innateness with deflationary dollars would be evolutionary only in name. It's like orange juice and orange drink. The first is the real thing, actual juice from an orange, while the other only looks and tastes (somewhat) like an orange. Many psychologists were attracted to GOFEP by the prospect of a research program that genuinely integrates psychology with evolutionary biology. But this is not what primitivism promises. Primitivism is not about the integration of psychology with biology but is all about curing psychologists of the illusion they are doing so through concepts like innateness. For a GOFEP, accepting primitivism must be very much like what it is, for a child, to accept that Santa Claus doesn't exist. GOFEPs might reply that, although primitivism does not promise an integrated evolutionary psychological research program, it does promise a psychology that works hand in hand with evolutionary biology. On this optimistic view, psychologists would be working alone, trying to explain whatever they feel is of interest to psychology, until they come upon something, some representation or mechanism *S*, they cannot explain. Using primitivism, they would then claim *S* to be innate and, as it were (showing here our Canadian origin), they would pass the puck to some other zone. On this "pass the puck tactic," what is innate is what psychology cannot explain using its own explanatory resources, so that it has to pass the explanatory puck to some adjacent zone to do the hard work. But the "pass the puck tactic" will work only if there is someone willing to receive the pass. We will argue in the next section that this is not the case. While the psychology team is busy trying to dump the puck in biology's zone, the biology team is now playing quite a different game.

When GOFEP Meets the Developmental Cognitive Neurosciences

As Samuels notes (2002, pp. 262–263), his characterization of the notion of innateness implies that the debate concerning nativism frequently turns on the question of what should be considered a proper psychological explanation, especially a proper psychological explanation of the *acquisition* of mechanisms or representations. In other words, what the proper domain of psychology and its explanatory resources are taken to be will determine what mechanisms are thought of as innate. Our argument in this section is that GOFEP has adopted a very narrow view of both psychology's domain and explanatory resources; a view that, in particular, posits a clear-cut border between psychology and the neurosciences. The main problem with this view, for what concerns us here, is that it has led GOFEPs to neglect a field of research that should have had a profound impact on evolutionary psychology. This field is developmental cognitive neuroscience. Developmental cognitive neuroscience is a recent interdisciplinary field at the interface of developmental psychology and cognitive neuroscience (for examples of work in this field, see Johnson, 1997, 2000, 2003; Nelson and Luciana, 2001). More specifically, the developmental cognitive neurosciences propose to increase the knowledge of the relation between the developing brain and the cognitive development as studied by psychologists. This increase of knowledge is made possible by the availability of tools and methods to investigate the growing brain, such as high-density event-related potentials (HD–ERP), functional magnetic resonance imaging (fMRI) and the development of "marker tasks," that is, behavioural tasks known to be related to one or many regions of the brain in humans or in non-human primates and that are used at different stages of development (this is made possible by the development of new experimental paradigms that allows us to study those tasks in young infants, by using habituation or surprise for instance).

The developmental cognitive neurosciences should have had an impact on evolutionary psychology at least for two important reasons. First, it is a trivial fact that, whatever the relation between evolutionary theory and psychology turns out to be, it will necessarily include genetics and neuroscience. The selective pressures of the environment of evolutionary adaptation do not have a magical effect on psychological mechanisms. Whenever selective pressures do have an effect on psychological mechanisms, they do so by selecting genes which have an effect on the differential production of proteins in various neural structures. Lacking relevant knowledge in genetics, proteomics, and neuroscience, such as was the case even in the 1980s, early evolutionary theorists had to bypass the mediating influence of genes, proteins and neurons on, say, behaviour. But, given the important developments in all three fields in the 1990s, and given GOFEP's own beliefs about the importance of vertical integration (Barkow, 1980; Cosmides, Tooby, and Barkow, 1992), GOFEPs should have been sensitive to the developments in the cognitive neurosciences generally. Evolutionary psychology and cognitive neuroscience were born at about the same time, and evolutionary psychologists may perhaps be forgiven for having centered their attention exclusively on evolutionary theory and thus forgotten to pay attention to another developing young discipline; but no longer. Second, some evolutionary theorists (Oyama, 1985; Rose, Kamin, and Lewontin, 1985) have long complained that evolutionary theory has unduly neglected development. As long as no one had much to say about development anyway, this neglect could be forgiven. But, with the advent of the developmental cognitive neurosciences, which promise to give a brain-level account of the development of cognitive mechanisms, this is no longer the case.[11]

GOFEP has adopted the classical computationalist paradigm as its preferred form of explanation for psychology. The classical computationalist paradigm

[11]There are other, perhaps mainly rhetorical and sociological (i.e., sociology of science), reasons why GOFEP should have paid attention to cognitive neuroscience. First, it is a secret to no one that evolutionary psychology is a close kin to sociobiology, a discipline that has generated heated emotional, and sometimes acrimonious, debates (even feuds) mainly because of its political implications. To insure that the same fate does not befall their own discipline, evolutionary psychologists have to be squeaky clean from an epistemological point of view. In particular, it has to offer, whenever possible, detailed causal explanations of the mechanisms by which genes are involved in the production of psychological mechanisms. And developmental cognitive neuroscience promises to be an important player (along with molecular genetics and proteomics) in these explanations. Second, every evolutionary theorist has been properly, and justifiably, warned by Stephen J. Gould and others about the pitfalls of *just-so stories*. A just-so story is a powerful heuristic to drive research but it should never be mistaken for an explanation. To make sure they are never accused of having offered a just-so story where a genuine explanation is expected, evolutionary psychologists must, once again, offer detailed causal explanations of the mechanisms by which genes are involved in the production of psychological mechanisms. And, once again, developmental cognitive neuroscience promises to be an important player in this story.

views cognition as the result of systematic manipulation of symbolic representations. According to this tradition, the goal of cognitive science is to identify the kind of representations used in various cognitive tasks as well as the algorithms that produce given outcomes (whether a new representation or a behaviour). Following Marr (1982), the classical computationalist paradigm's preferred strategy to explain a given cognitive capacity is to start by defining the capacity in computational terms (computational level analysis), then to identify the representations and algorithms that generate the capacity (algorithm level analysis), and finally to find out which neural structure(s), or physical structure generally, implements these representations and algorithms (implementation level analysis).

A thesis that has been linked (contingently) with the classical computationalist paradigm is that the algorithm level can be described independently of the implementation level or, to put it differently, that the mechanism described at the algorithmic level can be realized by various physical substrates (silicone chips, neurons, beer cans). This is the well-known *multiple realization thesis*. This thesis justified the view that psychology was *epistemologically autonomous* which translated in a clear-cut division of labour between cognitive scientists (among whom, psychologists) on the one hand, whose job it was to identify the abstract structure of the mind, and neuroscientists on the other, whose job it was to find out how that abstract structure is implemented in the brain. On this working arrangement, devised by cognitive scientists, the cognitive sciences have epistemological priority over the neurosciences since the job of describing a cognitive mechanism's implementation can only be accomplished once the mechanisms in question have been identified. As a consequence, cognitive scientists have in general shown little interest in the work of neuroscientists (while it was expected that neuroscientists would pay supreme attention to work of cognitive scientists). This attitude characterizes GOFEP as well.[12] The goal of GOFEP is to use adaptationist thinking to carve up space at Marr's computational level, thus, perhaps, discovering some dark corners of the mind, invisible from the standard reverse-engineering stance of traditional cognitive science, and, once this is done, to find out how the computationally described capacities of the mind are algorithmically realized.

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[12]See for instance Cosmides and Tooby: ". . . precise descriptions of these cognitive programs can capture the way in which information is used to generate adaptive behavior . . . . Knowledge of the hardware, however, is not necessary for understanding the programs as information-processing systems" (2000, p. 1165) or "Facts about the properties of neurons, neurotransmitters, and cellular development cannot tell you which of these billions of programs will develop reliably in the human (or, e.g., the rhesus) mind" (2000, p. 1164). We agree with the latter. It is true that those facts are not, in themselves, sufficient to explain why we end up with a particular cognitive architecture. But that does not mean we should not pay close attention to them. Such facts can indeed provide non-trivial constraints on the type of explanation that will in the end account for why we end up with a given cognitive architecture.

Only once that is done will GOFEP turn to the neurosciences, looking for possible implementations of their favoured algorithms. It will then typically look for confirmation of their preferred carving of the mind (for an example of this attitude, see Duchaine, Cosmides, and Tooby, 2001).

GOFEP's relation to the neurosciences is thus completely top–down, looking, as it is, to the neurosciences in search of confirmation for its preferred theses about the cognitive architecture. This attitude, we contend, has been detrimental to GOFEP in that it has blinded researchers from a recent alternative model of psychological explanation, inspired by exciting new work in the brain sciences (as well as in computer science and in biology).[13] This work, as we intend to show, is of major significance to the evolutionary attitude in psychology.

In a recent review paper, Segalowitz and Hiscock (2002) have noted that developmental neuroscience is slowly bridging the gap that separated it from developmental psychology and that the result, the developmental cognitive neurosciences, offers a startling support for a constructivist developmental psychology (p. 7). What exactly are those results? Given the scope of this paper, we cannot review all of them here, so we will mention only two: brain growth and plasticity.

Brain maturation has long been viewed as consisting of an early period of massive proliferation of cells and synaptic connections followed by a life-long period of pruning (elimination of excess connections; see Changeux, 1983; Edelman, 1987). Recent discoveries have changed this picture. It now seems that the brain, more especially the neo-cortex, keeps building itself up until the late teens. Indeed, 90% of development is achieved by five years of age and is completed by 17.[14] Contrary to what was once thought, the brain goes through rounds of cortical neurogenesis and pruning. These are far from being the only changes going on in the brain as there are also changes in dendritic and axonal arborisation (see Quartz and Sejnowski, 1997). It has been shown that these changes are activity-dependent, that is, that synaptic development and dendritic or axonal arborisation are a function of the (internally and/or externally generated) activity of the organism. These changes follow a developmental schedule in which the primary sensory and motor cortical areas are both closer to their mature states at birth than are areas of association in the temporal and parietal region as well as in the prefrontal-cortex (which is involved in organization of action, working memory, planning, concept formation, abstract thinking and so on). As Segalowitz and Hiscock put it:

[13]For work in computer science, see for instance Shultz (2003); in biology, see Griffiths and Gray (1994).

[14]The pre-frontal cortex seems to be the region that gets to its mature state last. This does not mean that it is not functional before. Johnson (1999, 2003) is keen on insisting that the frontal regions of the brain play an important role in the specialization process despite the fact that they are immature.

> The advantage of such an extended dynamic growth period is increased sensitivity to the environmental factors that may shape the development, i.e., the child is sensitive to experience allowing for structural changes in the brain to reflect influences of the experience even into complex thought process. (2002, p. 17)

In other words, while Piaget viewed the interaction between the child and her environment as necessary for the emergence of new cognitive structures (new cognitive stages, as Piaget calls them), the developmental cognitive neurosciences view these interactions as factors changing the brain's computational properties and thus its cognitive structures. Thus, the brain is a non-static system, its computational capacities or properties are changed by its own activity. If such is the case, a distinction crucial for the deflationary tactic of primitivism goes by the board, that is, the distinction between brain growth and learning. As Quartz and Sejnowski write:

> This distinction [between brain growth and learning] is what makes developmental psychology and developmental neurobiology different disciplines, with psychologists in charge of studying how infants learn and neurobiologists in charge of studying how the brain grows. According to this time-honored distinction, brain growth was precisely programmed maturation, regulated internally with little environmental dependence, just as legs and arms mature. Because arms and legs mature without instructions from the environment, psychologists don't study how you learn to grow them. Learning, on the other hand, was akin to making software changes. As a matter of principle, it was believed that learning could not change the brain's hardware. Your computer does not grow new circuits as you type, for example. *But the idea of self-organization blurs the distinction between software and hardware*. The new models show how interacting with the world is a special kind of learning. It actually changes the brain's hardware and helps build it, through a very slow type of learning that computer models showed to be feasible. *In so doing, they overturned one of the cardinal distinctions of psychology: The divide between learning and brain maturation no longer made sense*. (2002, p. 50, our emphasis)

This is obviously bad news for the advocates of psychological primitivism who placed so much weight on the distinction between psychological processes and biological processes!

The second result we consider concerns brain plasticity. Once again, this result applies more to cortical than subcortical structures (which appear much less plastic, if at all, than the cortex). This type of plasticity is not restricted to humans as it has been observed in other species as well. For example, Johnson (1999, p. 78) mentions experiments in neonatal ferret pups where the projections from the retina are induced to project to auditory thalamic areas instead of visual thalamic areas. As a result, the auditory cortex becomes visually sensitive, as some cells become orientation and direction-selective and organise themselves in a two-dimensional visual field map. Another example comes from work on brain reorganization in blind subjects in which the "visual" cortex comes to mediate the sensory processing demanded by Braille discrimination tasks (Quartz, 2003). This has been shown using transcranial magnetic stimulation (a technique that delivers a magnetic pulse to a region of the brain, shutting down that part for a short period) on the visual cortex of congenitally blind subjects. As it turns out, the ability to read Braille characters is impaired when a pulse is delivered to the visual cortex, thus demonstrating the visual cortex's involvement in the task. As Quartz puts it:

> At a neural level, it appears that the tactile processing pathways usually linked to a somato-sensory area are rerouted in blind subjects to the visual cortical regions originally reserved for visual shape discrimination. (2003, p. 35; for more examples see Buller and Hardcastle, 2000 as well as Johnson, 1999)

What this suggests is that cortical specialization depends, to a certain extent, on experience, as it appears that a variety of representations can be supported by the same region of the cortex. We will see in more detail how this can be the case in the next section.

Taken together the results just presented suggest that the brain goes through a process of specialization in which ongoing experience with the environment plays a large role. The idea that the brain goes through a process of specialization is the core of a new approach of cognitive development called neural constructivism according to which:

> . . . modularization [is] not . . . a starting point of development, but rather its endpoint, as cortex becomes increasingly specialized through the interaction between afferent input carrying domain specific information and intrinsic cortical properties. (Quartz, 1999, p. 51)[15]

These results also suggest that the brain may start with lower-order representations and build on them to construct more abstract representations. This picture of development is also backed by work in computer science (constructivist neural network algorithms) showing that many of the initial limits of connectionist networks were due to the fact that their architecture was fixed. Problems that are computationally intractable for a connectionist network with a fixed architecture become tractable when the network is allowed to recruit new units (either by producing them or by establishing new links with them). Elman (1993) has shown for instance that a network that starts with limited resources but is allowed to grow can learn a linguistic task that was deemed unlearnable by cognitivists such as Chomsky.

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[15]See also Karmiloff–Smith for a similar position: ". . . biological constraints on the developing brain might have produced a number of mechanisms that do not start out as strictly domain-specific, that is, dedicated to the exclusive processing of one and only one kind of input. Instead, a mechanism starts out as somewhat more relevant to one kind of input over others . . . . Once a domain-relevant mechanism is repeatedly used to process a certain type of input, it becomes domain-specific as a result of its developmental history" (1998, p. 390; Panksepp and Panksepp, 2000).

None of this is good news for GOFEP. First, to a large extent the cognitive architecture of the mind is not fixed at birth. Evolution seems to have selected a different strategy than the one favoured by GOFEP, i.e., plasticity instead of specialization. Second, the interaction of the organism and the environment play a crucial role (together with the organism's genome) in the construction of these representations. In short, the brain might look much more like the general-learning mechanism GOFEPs abhor! As Buller and Hardcastle acurately put it:

> . . . the very complex adaptive mechanism by which our brains structure themselves *is* equipotential, content free, content independent, general purpose, and domain general. . . . [Y]ou don't need (and in fact *don't have*) genetically specified cognitive processes tailored to meet highly specific environmental task demands. Though our cognitive processes are highly specific indeed, our brains learned how to produce them . . . . (2000, p. 322)[16]

The Brain Doesn't Work That Way: The Case of Face Recognition (A Case Study)

Since the developmental cognitive neurosciences are a new field of research, and since the ongoing discussion is a bit abstract, we would like to end our paper by discussing one detailed example (face recognition) of how the developmental cognitive neurosciences might change the way all evolutionary psychologists should think about the mind. The example will not only serve to concretely present the developmental cognitive neurosciences' program, but allows us to offer a sketch of what the future evolutionary psychology might look like or, to put it differently, how evolutionary psychology should be revamped.

Duchaine, Cosmides, and Tooby (2001) mention face recognition as a domain for which there is an innate module. They approvingly cite the work of Kanwisher (2000) and Kanwisher et al. (1997), which suggests that face processing is domain specific and accomplished by a cerebral structure dedicated to this task (the fusiform gyrus); as well as the work of Farah, Rabinovitz, Quinn, and Liu (2000), where some distinction between face and object recognition, and the anatomical localization of face recognition, *are explicitly specified in the genome* (p. 122, our emphasis).[17]

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[16]Some GOFEPs consider what they call the "developmentally relevant environment" as a second system of inheritance on a par with genes (Tooby, Cosmides, and Barrett, 2003, p. 863), which seems to make their position more congenial to the constructivists' one. But that should not fool us. If they were to seriously adopt this point of view, they might end up having to abandon many of the modules they have postulated to be present at birth in infants and settle for the much lighter version of the initial architecture of the mind of the kind defended by Buller and Hardcastle.

[17]Slater and Quinn (2001, p. 22) go along the same line affirming that ". . . the infant enters the world with a detailed representation of the human face."

The kinds of evidence that have been used to put forward the existence of such a specialized mechanism come from two related sources. The first one (and the strongest evidence for the existence of a specialized mechanism according to Kanwisher, 2000) is the existence of double dissociation between prosopagnosia (the difficulty in recognizing faces but not objects) and visual agnosia (the difficulty in recognizing objects but not faces; Moscovitch, Winocur, and Behrmann, 1997). According to many (Farah, Wilson, Drain, and Tanaka, 1998), recognizing faces and objects calls for different kinds of processing. Face recognition requires the representation of the face as a complex and unique whole, while objects can be decomposed into their parts. More precisely, gestalt properties are important for recognizing faces but not objects. This would translate behaviorally to the fact that face recognition is more sensitive to inversion than object recognition, that is, it takes longer to recognize an inverted face than an inverted object. The "inversion effect," as it has been named, is indeed seen as the behavioral signature of face recognition (this phenomenon will play an important role in what follows). A second source of evidence is the case, reported by Farah et al. (2000), of a child that was stricken with meningitis at one day of age. This caused the part of his brain containing the mechanism in charge of face recognition to be destroyed. As a result, the child showed infantile prosopagnosia (being able to identify the features of a face, but not being able to identify faces as wholes or categorize them, that is, recognize two pictures of the same person from different angles). Because the destruction of a specific part of the brain has selectively impaired the child's ability to recognize faces, Farah et al. have concluded that this ability and the neural architecture that underlies it are innate. Is this conclusion justified? We think not. We will quickly consider three recent areas of research that invalidate the kind of innateness claim made by Farah and by evolutionists after her.

The acquisition of expertise. The first domain we would like to consider comes from work on expertise for different objects by Isabel Gauthier and her colleagues (Gauthier and Nelson, 2001; Gauthier, Tarr, Anderson, Skudlarski, and Gore, 1999). They have shown that expertise for objects such as cars or birds and even expertise for faceless, made-up little figures that she calls "greebles" (an expertise acquired after only a few hours of training) recruits the same ventro-temporal regions that are selective for faces (that is, the fusiform gyrus). Moreover, it seems that experts are also showing the inversion effects for the object of their expertise.

Gauthier's hypothesis is that the fusiform gyrus is used in tasks that require that one matches a specific or individual object to memory, rather than to a category, and that this is done through the use of relational information (Grelotti, Gauthier, Schultz, 2002). For instance, the fusiform gyrus would be good for differentiating a particular model of Saab but not for recognizing that it is a car and not a table. This seems to be confirmed by the fact that prosopagnosic patients who have impaired ability to recognize faces have similar deficits in subordinate-level recognition of everyday objects and Greebles (Tarr and Chang, 2003, p. 24). Results of this kind have led researchers to conclude that cortical specialization for faces results from fine-tuning by expertise of parts of the visual system especially well-suited for fine visual discrimination (Gauthier and Nelson, 2001, p. 219).[18]

Evolutionary psychologists may object that faces are, to use Dan Sperber's (1996; Sperber and Hirschfeld, 2004) expression, the "proper domain" of the fusiform gyrus, that is, that this structure has been selected for face recognition and that the input it has been designed to process is faces. On this account, the structure might then get co-opted in development for tasks with similar cognitive demands, such as recognizing individual cars or birds. The objects for which the fusiform gyrus has not been designed, but that are still processed by it, fall in what Sperber calls its "actual domain."[19] We think that, at the present time, there is no way to adjudicate between the two positions (apart for the fact that Gauthier's hypothesis does not require the position of additional conceptual machinery, i.e., the proper/actual domain distinction; see for instance Behrman and Moscovitch [2001] for a review of the argument for and against a specialized module for face recognition). But this is no big deal: our point at this stage is not about function but about development. We would be happy to say that the function of the fusiform gyrus is face recognition on the basis that face recognition is what that structure is typically used for in most humans. What we are after is the fact that somehow, this face recognition is genetically determined or programmed, or that we have some innate representations of faces. We think that this is not the case.

The development of face recognition. GOFEP's strategy gets less attractive, we think, when we consider how face recognition develops in babies. A first thing to note about the development of this capacity is that babies have a really mediocre visual acuity (see Dannemiller, 2001). This can be determined simply by looking at the retina: the packing of cones is not as dense as in adults, the aperture of the cones is larger than in adults, the length of cones is smaller than in adults, all of which shows that visual resolution is almost non-existent for high spatial frequencies, those very frequencies that would allow babies to see the details of a face. This is why babies actually do not use the

[18]Tarr and Chang are making a similar claim: "Face recognition should be considered as a case of perceptual expertise acquired by almost everyone" (2003, p. 23).

[19]Such a move as been suggested to us in Clark Barrett's (personal communication) reply to the present paper as well as by Richard Samuels, but has first been proposed by Kahnwisher (2000). Another possible move is for the evolutionists to back off a little and say that they have been wrong in thinking that face recognition is a specialization and that instead we are in presence of a more domain-general expertise mechanism. The same answer applies here: that is, our argument is about development and not about which function to attribute to the mechanism.

same information we do to recognize faces. Indeed, it appears that babies do not use configural information as adults do, but instead, they use the contour of the face (this is why an infant might not recognize her mother if the mother changes her haircut and why infants don't prefer their mothers' faces over others when you hide the faces' contours [Karmiloff–Smith, 1995]).

Facts about the brain's development of the visual system in infants help us understand the typical sequence of functional development. According to Johnson (1997), orientation of gaze and saccade during the first month of life is under the control of a subcortical pathway going from the retina to the superior colliculus (this does not mean that frontal regions are not playing any role). Descending pathways from the cortex get to their mature state only around the second month of life. Since the 1980s, we know that the visual system relies on two retinocortical pathways (Milner and Goodale, 1995): the magnocellular or dorsal pathway (in charge of detecting movement and producing ego-centered representations) and the parvocellular or ventral pathway (in charge of detecting color and shape and producing perceptual representations). It appears that the magnocellular or dorsal pathway is functional at a later time than the parvocellular or ventral pathway.

Johnson (1997) claims that the sub-cortical pathway in charge of control of visual behavior uses a very crude representation of faces (this structure is fed principally by peripheral visual fields while the fovea feeds more directly to the cortex). He has shown that, indeed, neonates have a preference for blob-faces (the CONSPEC representation, as he calls it) over scrambled faces, that is, faces constituted by only two dots in the place of the eyes, and one dot each for the nose and for the mouth. This first kind of representation would be necessary to direct attention to faces but then would get discarded later in development for cortical representations of faces.[20] In fact, Simion, Cassia, Turati, and Valenza (2001) as well as Easterbrook, Kisilevsky, Hains, and Muir (1999) have shown that Johnson's crude face representations are not crude enough. They have shown that cruder stimuli are preferred by newborns: indeed they prefer representations which have their most salient parts in the upper part of the visual field (so they would prefer a T-shape figure to an inverted T-shape figure, for instance). The differential development of the two retino-cortical pathways is also helpful in explaining other features of the functional development of face recognition. As we just said, the parvocellular or ventral pathway develops first, before the magnocellular or dorsal pathway does. It is only around the second month that infants stop preferring blob faces to fully drawn

[20]Heyes (2003) describes the bias towards face as "non-cognitive." The function of the bias would be to send new inputs to the already-existing recognition "cognitive" mechanism. If so, an evolutionary story could be told about the bias that would be different from the evolutionary story of the recognition mechanism. According to Heyes, ". . . it cannot be assumed that it [the recognition mechanism] was put there by natural selection." (2003, p. 720)

faces and only around the fifth month that they start preferring moving faces over non-moving faces.

de Haan, Oliver, and Johnson (1998) report that apparently, until they are 6 months of age, children show an "inversion effect" both with human and monkey faces. Human adults and adult monkeys show the inversion effect only in response to the faces of members of their own species. Also it appears that 6 month-old children can discern different monkey faces, a capacity that adults typically lose (except if they work with monkeys, in which case they presumably regain it) [Pascalis, de Haan, and Nelson, 2002]. This process of specialization at the cognitive level is supported at the neural level by a progressive localization of the regions involved in the task. It appears that development is accompanied by changes in the spatial extent of cortical activation such that experience with a class of stimuli ends up diminishing the number of areas activated. For instance, in face processing, both left and right ventral visual pathways are activated by faces in infancy while it generally ends up being localized in the right ventral pathway in adults. As Johnson puts it, [f]ewer pathways become activated by a given stimulus because most of them become tuned to other functions and therefore are no longer engaged by the broad range of stimuli they responded to earlier in development (2000, p. 78). Choice of a particular cortical region for the processing of a particular type of stimuli depends not only on which kind of input the thalamus is feeding it and what other region it is connected to, but also depends on particular architectural or temporal properties of an area:

> Whichever parts of the cortex are receiving the correct sensory inputs, and are in the appropriate plastic state, will configure themselves in response to this input set. According to a broadly similar analysis of the development of face recognition by de Schonen and Mathivet (1989), particular regions of the right hemisphere are timed to be in a plastic and receptive state just as polysensory information about faces is being attended to most avidly by the young infants. (Johnson, 1999, p. 87)

This kind of fact would support Karmiloff–Smith's idea that we don't start development with "domain-specific" mechanisms, but with "domain-relevant" mechanism. As she says:

> It suggests that biological constraints on the developing brain might have produced a number of mechanisms that do not start out as strictly domain-specific, that is, dedicated to the exclusive processing of one and only one kind of input. Instead, a mechanism starts out as somewhat more relevant to one kind of input over others . . . . Once a domain-relevant mechanism is repeatedly used to process a certain type of input, it becomes domain-specific as a result of its developmental history. (1998, p. 390)

Autism and face recognition. It is a well known fact that autistics focus their attention on different parts of the face than do normal individuals. Indeed, they tend to use the mouth and the inferior parts of the face instead of the configurational information to recognize faces. It has also been shown that the inversion effect is less important in autistics than in normals and that autistics do not exhibit categorical perception for faces (they have problems recognizing pictures of faces taken from different angles), which suggests that they might be using a different strategy to recognize faces (Schultz, Gauthier, Klin, Fulbright, Anderson, Volkmar, Skudlarski, Lacadie, Cohen, and Gore, 2000). Autistics also display other abnormalities in face processing such as an equivalent memory for objects and for faces (while normal individuals have a better memory for faces than objects).

Recent work by Schultz and colleagues has shown that these abnormalities are not caused by a defective fusiform gyrus. Indeed, in the case they report of an autistic child, this structure seems to be in perfect working order. This child is passionate about Pokemon characters and spends hours every day watching Pokemon cartoons on TV. He can recognize them without difficulty. What Schultz has shown is that when this child is recognizing the characters, the fusiform gyrus is activated, while it is not when the child sees a face. This remains true even when the experimenters put a blind over the character's face, so that the child cannot use information about their face but just information about the way the characters are structured.

Schultz's hypothesis is that the strange results and the abnormal way of treating faces seen in autistics might be due to an abnormality in the amygdala. Research in neuroimagery and histology has shown that the organization of the autistic's amygdala is abnormal; for instance, its cell density is higher and consequently there is a reduction in cell size. Abnormalities in the amygdala's functioning have also been found. There is a reduction of activity when the task is to judge what someone thinks by using the direction of his or her gaze. And, as Hirstein and his collegues (2001) have shown, the amygdala of autistic children, contrary to normals, is equally activated by an image of their mother as it is by that of a cup. Animal models, for their part, seem to indicate this abnormality might be an important factor in the aetiology of autism. Bachevalier (1994, 1996) has shown that ablation of the amygdala in monkeys results in many behaviors characteristic of autism (like stereotypic behaviors or prostration, for instance).

These facts have led Schultz et al. to propose that "[t]he amygdala's role in the development of face-recognition skills may be in signalling the emotional salience of face, thereby motivating the development of expertise in the face discrimination across time" (2000, p. 338; see Baron–Cohen et al., 2000, for a similar idea). To borrow an expression from Elgar and Campbell (2001), the amygdala would be the social-affective driver necessary to motivate children to pay attention to faces and to acquire face expertise. Without a functional amygdala, faces are just not salient.

Conclusion

We draw some lessons we believe researchers moved by the evolutionary attitude in cognitive science should take home from all of this. GOFEP sees the mind of infants as immature versions of the mind of adults: massively modular minds already programmed in the genome and waiting for the appropriate triggering signals from the environment to unfold properly, that is, to unfold according to plan. The various results we have presented point towards a different view of the relationship between the evolved genome, the mind of newborns and the mind of adults. Cognitive scientists should not think of the mind of infants as immature replicas of adult minds. Children are born with fully functional modular minds whose *evolved* functions are not to find mates, raise children or flee from danger but to interact with the local environment in order to ensure the child's survival, a huge part of which depends on the child's proper inclusion into the local human population. From the moment a child is born, its modular mind interacts with its local environment in a way that changes them both but, and this is crucial, that also changes gene expression. Indeed, that interaction changes the sites, timing and rates of gene expression. The result of this complex dynamical dance is the adult modular mind studied by cognitive scientists. The massively modular mind of adults is not a given, but an end-result. Modularization results from the progressive specialization of a plastic and growing cortex; a process that cannot be understood outside of the interaction between the child's mind and its environment. In the various countries that have adopted the British parliamentary system, the government's opposition acts as a "government in waiting." For this reason, the opposition is a powerless replica of the government: it has its own ministers in waiting and so on. The first lesson to draw from the results we have presented is that this is not a good analogy when thinking about infant minds: their minds are not minds in waiting but fully functional minds that, as a result of their action in a local environment, develop into adult minds.

The second lesson concerns that development. The results we surveyed show that development of adult minds relies on stable features of the environment much more than GOFEPs have supposed. The process by which it does so is what Quartz and Sejnowski have called progressive externalization (2002, p. 55), a process which we fully endorse. The structure of the adult modular mind is not only the result of genetic structure as GOFEPs thought, but the result of genetic structure together with the structure of the infant mind and the structure of the environment it finds itself in.[21]

[21]It is true that the structure of the newborn mind is a result of genetic structure (together with the uterine environment), but it is the newborn, with its mind, that interacts with its environment. This is why it is important to distinguish the three.

Because of this, and this is the third lesson, the adult mind can be unloaded of most of what were thought by GOFEPs to be its "innate representations" (at least it can be unloaded of the cortical ones). The brain, being a non-stationary system, is able to use the richness of the environment in a way that stationary systems cannot.

This does not mean that, as Quartz notes in a recent paper, the developmental cognitive neurosciences are a radically empiricist version of the development of the mind. In fact, and this is the last lesson we draw for evolutionary psychology, these sciences are committed to the existence of some structural constraints present at birth — probably much less plastic than the cortical ones, as constructive learning is possible only against a background of those constraints. As Quartz puts it: "From a developmental perspective, subcortical structures, which are developmentally precocious, may both play a central, but overlooked, role in directing, or bootstrapping, the emergence of cortical representations" (Quartz, 2003, p. 37; for a similar position see Panksepp and Panksepp, 2000, and Panskepp, Moskal, Panskepp, and Kroes, 2002).

GOFEPs might say that these lessons simply point the way towards a form of canalization, a process that, as we saw, they endorsed long ago. Perhaps our lessons *do* point towards a form of canalization, but as we argued, it is a form of canalization that GOFEP cannot adhere to without losing its soul. GOFEPs convinced by our argument will come to see that GOFEP is a chimera (the unpalatable mix of old stuff we talked about) and that there isn't even an evolutionary psychology proper to talk about. What there is (or should be) instead is a multi-level discipline in which the study of cognitive mechanisms at each level is mutually constrained by:

1. Evolutionary considerations (adaptiveness, biological function, genetics, etc.);
2. Developmental considerations (developmental dynamics, canalization, developmental contingencies, learning mechanisms and contingencies, generative entrenchment, etc.);
3. Mechanistic considerations (componential implementation, localization, functional-role, etc.); and finally
4. Local causal considerations (what activates the mechanism, what behaviour it causes, etc.).

GOFEP, being a type of psychology, cannot give all of these factors their fair share of the puzzle. The science that could, to put a name on it, would be evolutionary developmental cognitive neuroscience. If innateness has a future in science, it is within the bounds of that discipline.

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