



ON CONCEPTS,

MODULES, AND

LANGUAGE

Cognitive Science at Its Core

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Fodor and the Innateness of All (Basic) Concepts

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An Italian colleague, some years ago, defined me as “un Fodoriano di ferro” (an iron-clad Fodorian). I do not know whether I am so clad, but I have no compunction in admitting I am, indeed, a Fodorian. Jerry’s work and innumerable conversations I have had with him over many years, not to mention the privilege of writing a book with him (Fodor & Piattelli-Palmarini, 2010, 2011), have constantly and profitably inspired my own thinking.

I first met Jerry when I organized the Chomsky-Piaget Royaumont debate (Piattelli-Palmarini, 1980)¹ and was there and then favorably impressed by the cogency of his critique of traditional learning theories and unfavorably impressed by the fact that not one of the numerous other participants (with the exception of Noam Chomsky and Jacques Mehler) seemed to have understood what he was saying and how important it was. Ever since, I have had many occasions to present Jerry’s argument and to teach it, almost invariably witnessing, at first, a mixture of misunderstanding and disbelief. As Jerry was (and is) the first to admit, there is something paradoxical to his strongly innatist conclusion, but it’s also inevitable, in the absence of an equally mighty counterargument. None has been offered so far, so I plead for acceptance. The present regrettable return of neo-empiricism, in the shape of Bayesian learning models (Tenenbaum & Griffiths, 2001; Xu & Tenenbaum, 2007),² statistical regularities (Chater, Reali, & Christiansen, 2009), collective exchanges and the extraction of generic cognitive patterns (Chater & Christiansen 2010; Tomasello, 2000, 2003, 2006), recurrent networks (Elman, 1991), and similar notions, makes it relevant to re-propose, clarify, and update his argument. This is what I will be trying to do in this chapter, as a fitting (I hope) homage to Jerry.

FODOR'S ARGUMENT, IN ESSENCE

In order for induction (conceptual learning) to succeed, the "learner" must possess already the concept that she is (allegedly) learning. This applies to all primitive concepts, that is, basically, to all single "words" in the mental lexicon. Any non-composite concept that one can acquire has a full translation into a pre-existing concept of mentalese (Fodor, 1975, 2008). This is deeply paradoxical and counterintuitive, yet this is the way it is.

Prototypical cases: "Brown cow" contains brown and cow. It's a composite concept, built by compositionality. Two words, in fact. But it is not "more powerful" in any interesting sense. Certainly not in the sense of a buildup of progressively more powerful concepts as suggested by Piaget and his school.

It's crucial to acknowledge that this containment relation, unlike in the case of brown cow, does not apply to basic concepts. In particular, just to take the most typical examples: DOG does *not* contain ANIMAL, RED does *not* contain COLORED, KILL does *not* contain CAUSE-TO-DIE (Fodor, 1970).³

As Fodor has amply developed in his subsequent writings, what is being ruled out is the classic notion that the meaning of a concept consists in a set of simple primitives derived from sense data (à la Locke), or a network of mandatory inferential links to other concepts, or a web of beliefs.

Basically, the repertoire of primitive concepts is *very* large (in the order of tens of thousands), is *very* heterogeneous, because *all* they have in common is that they are basic concepts (Piattelli-Palmarini, 2003). Nothing else. They are "atomic" (their meaning is not dependent on the meaning of any other basic concept—no networks). They typically are of "middle" generality, neither as specific as "Turkish angora cat," nor as general as "living being" (Rosch et al., 1976). Basic concepts are not just "potentially" present (whatever that means) in the process of learning, they are *actually* there and ready to be tested.

ON LEARNING

Unless it's just a metaphor (as it often is) covering all instances of somehow managing to do later something cognitive that you did not manage to do earlier, bona fide learning, in the classical sense, requires many trials, some kind of differential feedback (it's OK, it's not OK) and a continuous ascending function of increasing success (the famous curves of the behaviorists).⁴ Monitoring progress in learning requires some precise hypothesis on what is being learned (one needs split experiments, and solid counterfactuals). If what is at stake (as it should be) is an intensional relation, then there must be a relation based on content (meaning)⁵ between input and output. Bare extensional relations are not enough.

The only psychological process of learning that makes some sense, and that has been well studied, is hypothesis testing: the confirmation or dis-confirmation of hypotheses.

As a paradigm case, let's take the kind of experiments carried out, long ago, by Jerome Bruner and his school (Bruner, 1973; Bruner, Goodnow, & Austin, 1956, see Figure 9.1). The experimentalist uncovers and ostensibly sorts cards, one after the other, into two stacks, one is the pile of the exemplars, the other of the non-exemplars. Exemplars of what? Some property (concept, predicate) X that the subject has to "learn" on the basis of the evidence presented and partitioned into those two mutually exclusive sub-sets (is an instance, is not an instance; it's OK, it's not OK; satisfies, does not satisfy; etc.).

In those cards there were four relevant properties, each presenting limited variability: number (1, 2, 3), color (white, gray, black), shape (circle, square, cross), and border (thin, medium, thick). These experiments revealed that there is a spontaneous, quite predictable, hierarchy of hypotheses that subjects try out, one after the other. First come the atomic properties: crosses, squares, circles. Then binary conjunctive properties: three circles, white rectangles, and so on. Then ternary conjunctions: two white squares, three gray circles. The borders are usually disregarded and only come later. Disjunctions (crosses *or* black figures, pairs of figures *or* white circles) come last, if ever.⁶

These data confirmed that there is no learning without a priori constraints on the kind of hypotheses that are actually tested and on their order of precedence. This is a point famously stressed by Carl G. Hempel (Hempel, 1966), Nelson Goodman (Goodman, 1983), Hilary Putnam (Putnam, 1975) and many more ever since.⁷ Curiously, there has been scant consensus on the *origin* of these constraints on learning. Fodor and Chomsky have attributed them to innate factors

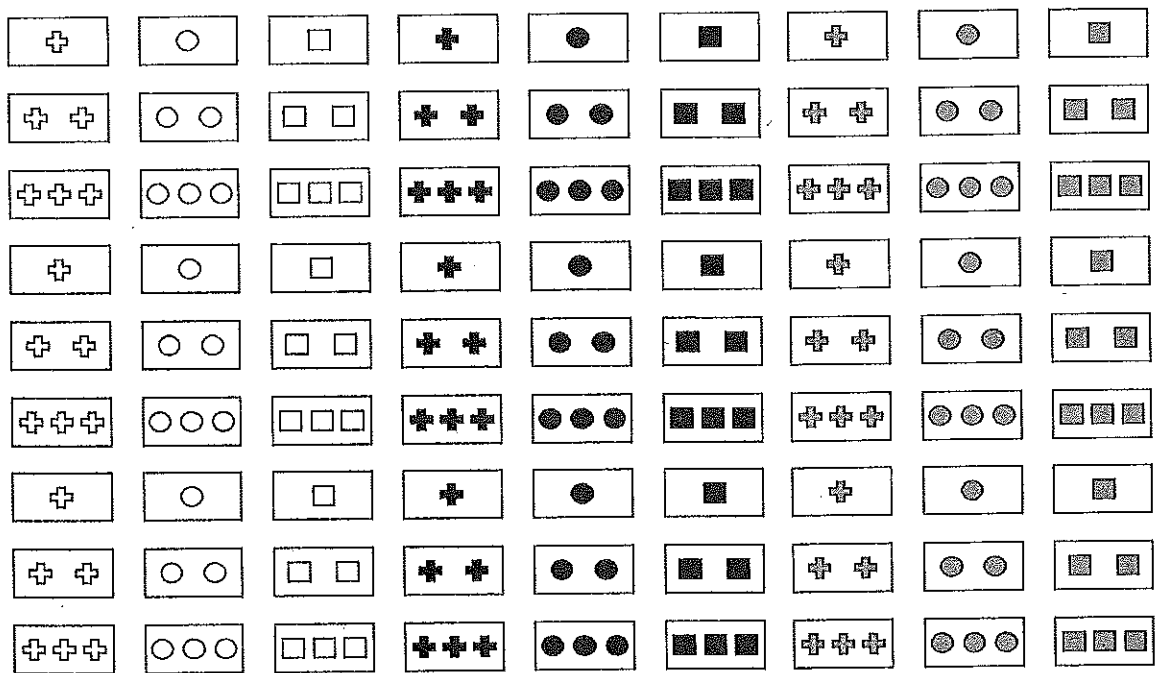


Figure 9.1 Materials from Bruner, Goodnow, & Austin (1956). See text for discussion.

(an explanation that I share), Goodman has invoked an “entrenchment” due to a record of collective past success, while Quine attributed it to Darwinian natural selection. Putnam brings several factors into the picture, some of them social, some cognitive, and keeping innate factors to a minimum.

Let’s notice, however, as Fodor rightly does, that all these hypotheses—whatever they are and in whatever order they are tested—in order to be tested, must be “there” already, not just potentially present (as Piaget claimed), but actually there.

Many authors, notably including Piaget and Seymour Papert (who participated in the Royaumont debate, and on whose theses we will come back in a moment) insist that the process of hypothesis testing and the process of hypothesis formation are strictly associated. Piaget was not thrifty in postulating a variety of processes to explain the origin of concepts. These bear names such as thematization, reversibilization, reflective abstraction, abstracting reflection, and more. In the following years, the connectionists and other enemies of innatism have proposed other variants of such processes, for instance “representational redescription” (Elman et al., 1996; Karmiloff & Karmiloff-Smith, 2001; Karmiloff-Smith, 1992).

Fodor is adamant in wanting to keep the source of hypotheses separate from any process of hypothesis confirmation/dis-confirmation, finding himself, for once, in agreement with Hilary Putnam. Their agreement, however, collapses when the nature of the source comes into play. For Fodor the source is innate, while for Putnam meanings are “not in the head.” In the published Royaumont exchange with Putnam (see the section Fodor’s Suggestions (in the Royaumont Debate)), Jerry accuses him of being “thunderously silent” (*sic*) on the source of concepts and hypotheses. Not only does Fodor stress that the concepts and the hypotheses that are actually tested have to be innate, but adds that the test must be based on their content. This is a crucial point in his argument.

FODOR’S ARGUMENT

All basic concepts are innate.

This is based on three converging, but distinct, lines of evidence and reasoning:

- (1) No induction (no learning) is possible without severe a priori constraints on the kinds of hypotheses that the learner is going to try out (this is hardly objectionable, at least ever since the Hempel-Goodman’s paradox and Putnam’s paradox, and confirmed experimentally in many experiments).
- (2) The failure of Locke’s project. Concepts cannot be grounded on a restricted set of sensory impressions. More on this anon.
- (3) Richer (more powerful) concepts cannot be developed out of poorer ones by means of learning (in any of the models of learning that have been proposed so far).

THE THREE MOST POPULAR LEARNING PARADIGMS

Paradigm 1

The learner already has a repertoire of relevant concepts (predicates, hypotheses), X_1, X_2, \dots, X_n

He/she/(it if an automaton) tries them out in some order of decreasing a priori plausibility and selects the best guess compatible with all the evidence seen so far.

Inductive logic will tell you (not an easy task)⁸ which hypotheses will be tried out first, second, third, and so forth, and what constitutes "sufficient" confirmation.

But inductive logic is totally silent on the *origins* of the repertoire.

This suggests the innatist explanation for the origin of concepts. We have some understanding of how it works. In fact, it's the only paradigm which we begin to understand.

Paradigm 2

The learner has a repertoire of vaguely relevant, but weaker concepts (properties, predicates, hypotheses), x_1, x_2, \dots, x_n (Piaget, Papert, and connectionism; see the section Papert and Connectionism).

He/she/it must find the means to develop (acquire, generate, compute) a "more powerful" concept X.

Thesis: The methods for testing concepts do also tell you how the more powerful concept is generated (see Piaget's theory). Call this: Feed-back, variational re-computation, abstraction, representational re-description, whatever.

Fodor shows that no such schema could possibly work.

Why? I'll try to expand the following issue a bit.

Sub-Option 1

The learner generates X by sheer luck, and X fits the available evidence by sheer luck. Otherwise he/she dies, along with all the descendants (the neo-Darwinian approach; if it's a computer program, then it's a piece of junk soon to be forgotten).

In fact, most of the time, X is wrong. Only rarely do such guesses work (extreme neo-Darwinian approach).

Then, no learning has taken place, just the biological reproductive fixation of lucky blind guesses.

Special case: The target concept X and the wildly guessed concept Y happen to be co-extensive. They mean quite different things (they are semantically distinct). We may have to conclude that Y (not X) has been "fixed" by natural selection. Deciding what has been fixed may not be easy, requiring split

experiments and a host of counterfactuals (Gallistel, 1990; Gallistel & King, 2011). The “lucky” component of this fixation may (just may) apply to genuine triggers, as described by the ethologists. But from ethology, we know that a trigger and what it “triggers” need not have any structural relation. Pecking on a red spot on the mother’s gorge brings about the regurgitation of food. We will go back to the issue of triggers in a moment. Triggers are not semantically related to the ensuing result. No learning has occurred. In fact this is a paradigm of lucky, senseless wild guesses, hardly a scheme for genuine learning. Let’s then consider a different tack.

Sub-Option 2

The learner, somehow, “tracks” the content of X, and why it is adequate with respect to (true of) the available evidence. The process of progressive convergence is, somehow, guided. The content of X, and some sensitivity of the process to the truth/falsehood of X, supply the required “guidance” (tracking).⁹

Nothing else could supply it. It so turns out that what selection is selection *for* can only be a correct detection of truth-values, individuating which real or possible extensions make the concept true or false. In that case, indeed, there is learning (inductive fixation/rejection) of the *meaning* of X. But X cannot be fixed/rejected unless it is *actually* available to the learner and it is exploited in the process by the learner. We are back to the previous paradigm. Nature endows the learner with a sensitivity for truth and meaning, these are innate predispositions. In the innatist theoretical frame also mental *contents* and *rules* are innate, not just sensitivities and predispositions. This is a point deemed to be unacceptable by the anti-innatists, but any approach that appears to avoid this innatist conclusion is doomed to fail.

Let’s continue: the learner “works on” the previously available, weaker (primitive) concepts by means of combinations, re-descriptions, thematizations, whatever (or Quinean bootstrapping à la Susan Carey; Carey, 2001, 2009) and thereby generates a genuinely new and more powerful concept Y.

One possibility: Y is literally a composite concept, composed out of the x’s (brown cow) and what it means is that way of composing them. We have the syntax and the compositional semantics of the composition, no less, no more. This is perfectly OK, but then the new concept is not “more powerful” in any interesting way. Moreover, not all concepts can be composite, one has to compose something into them in order to get them, therefore some concepts must be primitive. On these we must concentrate our approach.

This is where the failure of Locke’s program becomes important. If all primitive concepts could be derived from sense-impressions and if the “rule of composition” were just association, then, we would have a bona fide empiricist psychology (apt to be simulated in a connection machine). But, as amply shown, this is not a possibility.¹⁰ The acquisition of even very simple concepts requires things like a theory of mind, the understanding of relevant aspects of a situation, understanding the syntax of the sentences that contain them, as shown by

a rich literature on the acquisition of the lexicon (see Paul Bloom, 2000, 2001; Lila R. Gleitman [Medina, Snedeker, Trueswell, & Gleitman, 2011; Gleitman & Landau, 2013]; Legate & Yang, 2013).

So, we must offer a very different story:

ANOTHER SUGGESTION DOOMED TO FAIL

One is acquiring genuinely “more powerful” primitive concepts by means of *definitions* that contain and organize more basic ones. It’s not “just” the syntax and the compositional semantics, but the articulated net of obligatory, possible, and impossible inferences that the definition specifies. But Jerry has shown, quite persuasively, that this is not a viable possibility. Genuinely more powerful concepts cannot be exhaustively “defined” in terms of less powerful ones (this is what goes under the name of the “plus-X” problem). KILL cannot be CAUSE-TO-BECOME-NOT-ALIVE, because many situations that make the second true are not also true of the first (a calumny propagated on Monday that causes the suicide of the affected person on Saturday, and similar cases). So, is it must be KILL = CAUSE-TO-BECOME-NOT-ALIVE + X? What can X be? Well, as Jerry shows, X cannot be other than KILL itself. So no gain. Truth conditions on formulas containing more powerful concepts cannot be characterized with formulas containing genuinely less powerful concepts. Evidence, suitably labeled (what Jerry calls Mode of Presentation (MOP)), can “activate” them, but not “engender” them, for all the above reasons. Lila R. Gleitman and collaborators have carried out, over the years, an impressive series of experiments showing that word meanings are very frequently acquired upon one single exposure, under clear conditions that correspond to MOPs (in Fodor’s terminology).

In other words: the manipulation of primitive concepts can (in fact, it typically does) produce “brown cow” from “brown” and “cow,” and the syntax of the composition, but no repetition of “This cow is brown,” and “This cow is brown,” and “This cow is brown” . . . can generate “All cows are brown,” unless you have the universal quantifier (“every,” “all”) already in your conceptual repertoire. You must have a record of past observations of As and Bs involving some general uniform way of representing “All ___ are ___.” Otherwise you cannot do that, no matter how many As and Bs you observe.

We are now ready for Jerry’s final line of the argument.

Learning a concept is learning its unique semantic properties. At some stage you must entertain the following formula (F) in mentalese:

(F) For every x, P is true of x, if and only if Q(x).

Q is supposed to be a “new” concept of mentalese. The one you have (allegedly) “learned,” while P is some concept you had already. As a necessary, but not sufficient condition, P must be coextensive with Q, if (F) is correct. But this is plainly not enough: P must be coextensive with Q *in virtue of the intensional properties*

of *P*, in virtue of the content of *P*. Otherwise (F) is not a correct semantic formula. So *Q* is *synonymous* with *P*, but *P* you had already, ex hypothesis, so you also had *Q*. End of the story.

To repeat: So *Q* is synonymous with *P*. So you had *Q* already in your "language of thought," because you had *P*. So *Q* is not "learned." Iterate this for every primitive concept, keeping in mind the failure of Locke's program.

Conclusion: All primitive concepts are innate. And (due to the failure of Locke's program) they are not all mere constructs from sensory impressions.

It is a shocking conclusion, but it is also unavoidable.

FODOR'S SUGGESTIONS (IN THE ROYAUMONT DEBATE)

So, where do new concepts come from?

Three possibilities:

- (1) God whispers them to you on Tuesdays.
- (2) You acquire them by falling on your head.
- (3) They are innate.

In order to lay out an indubitable, certified example of a more powerful logic and contrast it with a less powerful one, Jerry mentioned sentential logic versus first-order logic.¹¹ The second, but not the first, countenances quantifiers. The punch line was that no matter how many instances of "brown cow" (and thereby of confirmations of the predicate "brown cow") the learner encounters, the hypothesis "all cows are brown" (or "most cows are brown, or "few cows are brown" and so on) will never become accessible via learning, unless the learner does possess quantifiers already and he/she/it is ready to apply them to the available evidence.

Hilary Putnam (Putnam, 1960) in his critique of Carnap's (notoriously weak) theory of induction, has a fine example pointing in the same direction as Jerry's. Imagine an induction automaton *A* that has all the number predicates and all the relative frequencies predicates. It is presented with a series of balls of different colors. It may correctly produce and test and finally, with a good coefficient of confirmation, converge on the hypothesis: "one out of five balls is red." However, it will never produce and test the more powerful (and true) hypothesis "every fifth ball is red." In order to do this, we need a more powerful automaton *B*, that possesses all the predicates that *A* possesses, plus quantifiers and predicates of ordering. The power to make the transition between *A*'s hypothesis and *B*'s hypothesis has to be built into the machine from the start, it cannot be the piecemeal result of learning from successive inputs of colored balls.

The Piagetian notion of "constructing" (*sic*) a piecemeal ascending succession of genuinely more and more powerful logics by means of abstraction, generalization, and new assemblages of concepts, via hypotheses testing and learning, is untenable.

So is the idea of a transfer from some other (non-semantic) source (pragmatics, usage, general intelligence, constraints satisfaction, social exchanges, etc.). Pace several authors who claim this to be the case, Jerry rightly qualifies it as a miraculous solution.

MEANING VERSUS SORTING

A theory to which Jerry has come back in successive work, persuasively demolishing it, is that possessing the concept C should be assimilated, or reduced, to some pragmatic capacity to sort things-in-the-world into the Cs and the-not-Cs. If that were the case, then such sorting should be done on the C-things “as such,” not on any collateral property P, by happenstance extensionally co-instantiated with C. What must be involved is an intensional sorting. Take the (supposedly) extensionally equivalent predicates CAT versus THE-ANIMAL-TO-WHICH-AUNTIE-IS-ALLERGIC. These correspond to different sorting criteria, even if cats indeed are the animals to which auntie is allergic (see Jerry’s book “Psychosemantics” [Fodor, 1987]) and LOT Revisited (Fodor, 2008). But sorting cats “as such” is something only God can do. No one can recognize just any old cat (or very small ones, or very big ones, or dead ones, or yet-unborn ones, etc.) under any circumstance (in a very dark night, or under polarized light, etc.). Sorting things into Cs and not-Cs is enormously context-dependent. Appealing to “standard (or normalcy) conditions” will not solve the problem, as the notorious unsolvable problems with verificationism have amply shown. Moreover, sorting is also C-dependent. Standard conditions for cat-sorting are not the same as for fish-sorting, oboe-sorting etc. But, even admitting that there are predicate-independent standard sorting conditions for sorting Cs, they cannot be part of the *content* of C. Normalcy conditions do not compose. For example, take NIGHT-FLYING BLUEBIRDS. Got it? Sure. But normalcy conditions here are patently conflictual. Maybe you recognize them by way of a unique song they sing, or a unique smell they produce, but that cannot be constructed by composing the normalcy conditions for things that fly at night and those of birds in general and those of bluebirds in particular. Even if that song (or smell or whatever) is criterial and unique, it’s so in virtue of the property of being night-flying bluebirds. No way out. So, even if the sorting criterion did apply to primitive concepts (but it doesn’t), it would not apply to composite ones.

Why does it not apply to primitive concepts? Because, as we saw, also sorting things into primitive concepts requires normalcy conditions, and there are no concept-independent general criteria of normalcy conditions. These conditions would have to compose, because concept possession and identification are systematic and productive, but no epistemic criteria do compose. Jerry, therefore, urges us to conclude that possession conditions for concepts aren’t epistemic, they are (as he would put it) metaphysical. Confusing issues of metaphysics with issues of epistemology in the domain of semantics is a capital sin, against which Jerry has thundered relentlessly for a good part of his career. Having concept C “just”

is being able to *think* about Cs *as such*. Sorting, inferences, perceptual accessibility, ease of representation, relevant beliefs, and so forth, are all secondary to this. Abilities to think about Cs do compose, as they should. Minds are for thinking and concepts are for thinking with. Can some kind of dispositions do the job? No, because, in Jerry's own words: "Mere dispositions don't make anything happen." What causes a fragile glass to break is not its being fragile. It is its being dropped.

ABOUT TRIGGERS

A very interesting cautionary proviso was made by Jerry in the debate. After concluding that, for all the reasons he had offered, we have to admit that all primitive concepts are innate, he adds:

Unless there is "some notion of learning that is so incredibly different from the one we have imagined that we don't even know what it would be like, as things now stand."¹²

A different notion of learning, usually now replaced by the term acquisition, has indeed been offered in the domain of language. It's the very idea of principles and parameters, where learning is assimilated to the fixation by the child of a specific value for a restricted set of binary parameters (Roeper & Williams, 1987; Gibson & Wexler, 1994; Breitbarth & Van Riemsdijk, 2004). One of its first proponents, Luigi Rizzi, has recently succinctly explained the very idea:

"The fundamental idea is that Universal Grammar is a system of principles, expressing universal properties, and parameters, binary choice points expressing possible variation. The grammar of a particular language is UG with parameters fixed in a certain way. The acquisition of syntax is fundamentally an operation of parameter setting: the child fixes the parameters of UG on the basis of his/her early linguistic experience. This approach introduced a powerful conceptual and formal tool to study language invariance and variation, as the system was particularly well suited to carefully identify what varies and what remains constant across languages. And in fact, comparative syntax using this tool boomed as of the early 1980's, generating theoretically conscious descriptions of dozens of different languages." (Rizzi, 2013)

This is certainly a "different" notion of learning, which I could characterize as revolutionary, if the term had not been tarnished by too frequent and too sloppy use. Some years ago, in fact, Noam Chomsky has said that the notion of principles-and-parameters is the most significant and most productive innovation that the field of generative grammar has introduced into the study of language. It was initially introduced as quintessentially applicable to syntax, with some plausible specific candidates. Later on, and for some researchers still today, parameters are rather circumscribed to the functional morpho-lexicon (Borer & Wexler, 1987; Boeckx & Leivada, 2013; Boeckx, 2006), narrow syntax being

genuinely universal and not parameterized. But this is not the place to delve into the interesting complex issue of the nature of parameters. See the special issue of *Linguistic Analysis* "Parameters: What are They? Where are They?" Edited by Simin Karimi and Massimo Piattelli-Palmarini (2018, forthcoming). A vast specialized literature is available. What counts here, in the present context, is a conceptual consequence of that model, well condensed by Wexler and Gibson with the notion of "triggers" (Gibson & Wexler, 1994).

This notion comes from ethology and Jerry has used it as a prompt retort in the Royauumont debate. Piaget and his close collaborator Bärbel Inhelder in previous writings and during the debate had suggested that language is literally constructed (*sic*) upon primitives present in motor control.¹³ The French biologist Jacques Monod was quick to question this hypothesis (and was absolutely right in doing so) by saying that, in that case, quadruplegics should never develop language, but they do. Inhelder replied that very little movement suffices, "even just moving your eyes" (*sic*). Jerry's quick retort was that, even admitting that it's so, then eye movement is a trigger, exactly in the sense given to the term by ethologists, and not a motor primitive suitable for construction.

It is none other than Jerry, in fact, who has wisely stated that cognitive science begins with the poverty of the stimulus (for a recent multi-disciplinary characterization of this notion see (Piattelli-Palmarini & Berwick, 2013)). He and I surely are among those who are grateful to the generative enterprise for having dissociated the child's acquisition of her mother language from any model invoking inductive processes and trial-and-error. The effects on the growth of the child's mind of receiving relevant linguistic data bear a close analogy to the effect of triggers. One significant quote from Chomsky (among many more in his work) stresses this point:

"Language is not really taught, for the most part. Rather, it is learned, by mere exposure to the data. No one has been taught the principle of structure dependence of rules (. . .), or language-specific properties of such rules (. . .). Nor is there any reason to suppose that people are taught the meaning of words. (. . .) The study of how a system is learned cannot be identified with the study of how it is taught; nor can we assume that what is learned has been taught. To consider an analogy that is perhaps not too remote, consider what happens when I turn on the ignition in my automobile. A change of state takes place. (. . .) A careful study of the interaction between me and the car that led to the attainment of this new state would not be very illuminating. Similarly, certain interactions between me and my child result in his learning (hence knowing) English." (Chomsky, 1975)

In subsequent years, in his 1998 book on concepts, modestly ensconced in a footnote, Jerry says:

"As far as I can tell, linguists just take it for granted that the data that set a parameter in the course of language learning should generally bear some natural

unarbitrary relation to the value of the parameter that they set. It's hearing sentences without subjects that sets the null subject parameter what could be more reasonable? But, on second thought, the notion of triggering as such, unlike the notion of hypothesis testing as such, requires no particular relation between the state that's acquired and the experience that occasions its acquisition. In principle any trigger could set any parameter. So, prima facie, it is an embarrassment for the triggering theory if the grammar that the child acquires is reasonable in the light of his data. It may be that here too the polemical resources of the hypothesis-testing model have been less than fully appreciated." (italics as in the original; Fodor, 1998a, p. 128, n 8, "Linguistic footnote")

This is a very interesting remark, and in a personal communication (September 2013) Jerry says he still thinks it's correct and that he does not have anything much to add to it.

Being a fan of principles-and-parameters and of something like triggers (more or less à la Gibson and Wexler), I have been puzzled by this remark.

Chomsky's reaction, if I understand it correctly,¹⁴ is basically that evidence is always pre-structured somehow by the innate endowment and, even if marginally, by a previous history of exposure. Evidence thus pre-filtered always has some structure, and it may impact the speaker's internal state (I-Language) producing an internal change of state (see Yang's 2002 book for a detailed picture). No appeal to induction is needed. Moreover, there is no such thing as a "relation" between the speaker X and X's I-language, such that X examines linguistic data, accesses some internal representation of his/her I-Language, and tries out hypotheses "about" it. There is, simply, an internal state of the speaker's mind-brain, and this state may vary marginally over time, under the impact of relevant evidence. No "relation," no induction, but just the speaker's being in a certain internal state.

A much simplified vignette of parametric acquisition upon single exposure to a relevant sentence, the one Fodor is alluding to, would be, for the child acquiring Italian: "andiamo e poi giochiamo" ((we) go and then (we) play). No subject is manifestly expressed, therefore the local language is a pro-drop language. The child fixes the pro-drop parameter to the value +. Real life instances are more complex than this simplified case (Berwick & Niyogi, 1996; Janet Dean Fodor, 1998b; Kam & Janet Dean Fodor, 2013; Roeper & Williams, 1987), but it may convey the essentials. No classic hypothesis testing, no induction, no trial and error. But it's not an arbitrary pairing either, as it would be for a bona fide trigger. Fodor's perplexity bears upon the relation between the form of this sentence and the ensuing specific parameter value fixation by the child.

My own take on this issue is that the child innately has in her mind, in the domain of language, several formulae like the following (borrowing the expression "doing OK with" from Putnam).

(L) Given linguistic input L, I will be doing OK with parameter value X.

Several conditions apply to the application of (L) (see Gibson & Wexler, 1994; Thornton and Crain, 2013), but parametric acquisition comports something like (L).¹⁵ Single stimulus learning is an idealization, a useful one, but still an idealization. Some frequency threshold of incoming linguistic stimuli may well have to be attained for parametric fixation to succeed (Yang, 2002, 2011a, 2011b; Legate & Yang, 2013). The nature of the relation between L and X is pre-wired, not the result of induction, but L is not an arbitrary trigger either. In fact, some distinction ought to be introduced between releasing mechanisms and bona fide triggers. In immunology we have many examples of releasing mechanisms that bear a functional relation to the final output. When the organism makes contact with pathogens, this encounter normally switches on automatic reactions of recognition, binding, and rejection, approximately in this order. The net result is the maintenance of a healthy state. Many spontaneous reflexes are appropriate responses to the releasing stimulus: pupil contraction and closing one's eyes in the presence of a blinding flash of light, head and body retraction in the presence of a dangerously incoming object, vomiting when unhealthy substances are swallowed, and so on. Granted, these stereotyped reactions can also be activated by irrelevant stimuli. For example, the intravenous injection of minute quantities of harmless egg yolk induces a powerful immune reaction. Such systems can be "fooled" and a certain degree of arbitrariness of the releasing input does exist, but it's far from being total arbitrariness. In the case of parametric acquisition, therefore, we do not have to choose exclusively between induction and arbitrary triggers. The structure of suitable linguistic inputs and the ensuing fixation of an appropriate parametric value bear complex non-arbitrary relations that researchers have been investigating with some success (Yang, 2011a, 2011b).

It is indeed "a notion of learning that is so incredibly different from the one we have imagined," just as Fodor has wisely suggested. The innate components of the process are manifold, complex, and language specific. There is no "construction" from any kind of non-linguistic primitives.¹⁶

OBJECTIONS AND COUNTERS

There have been two interesting objections to Fodor's innatist conclusion that I think are worth summarizing here, along with Fodor's retort. The first was made there and then, at Royaumont, by Seymour Papert, the second in a commentary written by Hilary Putnam after the debate and published in the proceedings. Let's start with Papert.

SEYMOUR PAPERT ON CONNECTEDNESS

Papert introduced in the debate, in his critique of Fodor's innateness, a device called a perceptron, in hindsight an early, elementary artificial intelligence device, similar to present-day connection machines. Basically, this device has an artificial retina connected to a parallel computer. There are several interconnected

local mechanisms, none of which covers the whole retina. None of these has any "global knowledge." The device computes weighted sums of the local "decisions" reached by each sub-machine. The result is global decisions, not localized in any sub-part. Papert insists that there is a "learning function" sensitive to positive and negative feedback supplied from the outside, until the device converges onto a correct global decision, such that new relevant instances can be presented and recognized correctly. What can it learn? The answer is, according to Papert, far, far from obvious. It can easily learn to discriminate, say, between "triangular" and "square," by looking at local angles in the retinal image. What about the property or predicate "connected"? Can it learn to decide whether the image is made up of one single connected piece, or several connected pieces? The answer (far from obvious) is that it can.

Imagine an investigator (a Fodorian) who, therefore, concludes that "connectedness" is innate (prewired in the machine), but the wiring diagram cannot reveal anything that corresponds to "connectedness." Big surprise! Therefore, Papert concludes, one has to be very careful in concluding that a predicate is innate.

EULER'S THEOREM

A centerpiece of what Papert presented is a theorem due to Leonhard Euler, proving that, if and only if the algebraic sum total of all the curvatures along the borders of a blob is 2π , then the blob is connected, whatever its shape. Otherwise it's not (Figure 9.2).

If it is $n2\pi$, then we have n distinct connected objects.

No piece of the machinery "possesses" (is sensitive to) the concept of connectedness, nor does it "contain" such concept. Angles do not even have to be measured continuously along the borders of each blob, provided all are, in the end, measured and given the appropriate algebraic sign (say, plus if the rotation is clockwise, minus if counter-clockwise).

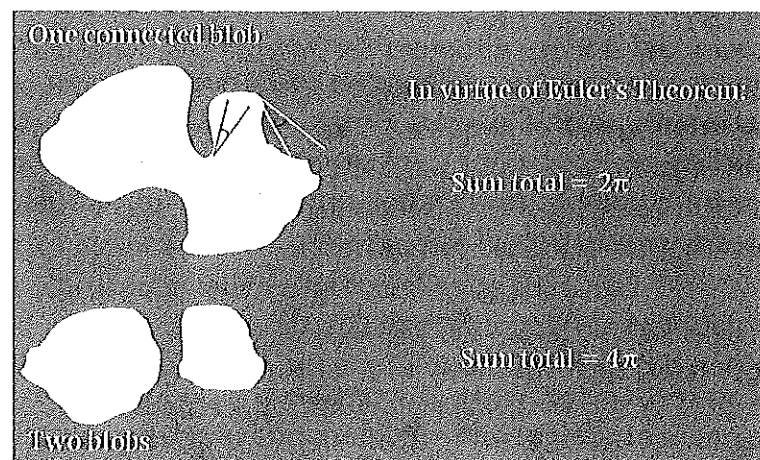


Figure 9.2 Application of Euler's Theorem. See text for discussion.

The pieces of the perceptron just detect angles and measure them. And the machine as a whole then sums up all the angles. It does not have to "know" (keep track of) whether two non-successive observations of angles are concerned or not with the same blob. As a result (not of innateness, but of the process itself), the machine is sensitive to connectedness, in exactly the right way, thanks to Euler's theorem. Connectedness is neither innate (prewired) nor learned. It is the inevitable consequence of the dynamics of the process, the development of the process, and the deep property discovered by Euler. He insisted that terms like "concept," "notion," "predicate" are generic and misleading. We need better ones.

Papert's Piagetian "lesson" is that, similarly, the cognitive capacities of the adult may well be neither innate, nor learned. They have a developmental history, as shown by Piaget. They emerge from other, different, components in a process of construction. Whatever is innate will not resemble in the least what you find in the adult's mind. The real search will have to track precursors, intermediate entities, and constructions. The point is: The perceptron, indeed, has the concept "connected," but it's precisely and exhaustively defined on the basis of other predicates the machine is sensitive to (local angles of curvature and their algebraic sum). So contra Fodor's thesis of the innateness of all concepts, this global concept is genuinely constructed from strictly local ones. If you had searched the "genome" of this machine to find where "connected" was encoded, the answer would have been: Nowhere! Yet the machine has it.

FODOR'S COUNTER

The machine has the concept "connected," since it necessarily (not by sheer luck) applies the concept correctly to all and only the connected blobs. You would not have noticed that it had this concept, and why, if you were not as clever as Euler.

But it does have the concept "connected," exactly for the reasons explained by Papert, based on Euler's theorem.

If all one has to decide are mere extensional criteria (the behavior of the device, what it prints out), then one will never know whether the device is "answering": "Yes, the figure is connected," or "Yes, I am printing 'yes,'" or "Yes, I am printing 'Yes, it's connected,'" or innumerable other possible printouts. It's just like in the extensional behaviorist approach to learning. Suppose the mouse manages in the end to learn to make the right turn in a maze. Has it learned to turn right, or to turn (say) North, or to move his left legs faster, or to go away from the light, and so on? The learning curve cannot make any distinction among these possibilities. One needs to plan split experiments (turn the maze 90 degrees, or turn it away from the light, or flood it with water, etc.) and develop relevant counterfactuals.¹⁷ If Papert's device is remotely similar to a human mind, then judging about connectedness, it must have an internal representation of something like

(C) If, and only if, total sum = 2π , then the figure is connected.

Otherwise, we succumb to Fodor's essential indeterminacy.

Being able to determine which predicates a device has involves it having a repertoire of internal representations and several computational options. That is, a whole system of predicates, and the quantifiers (as Putnam's case of the two automata shows). It may not be easy to determine which innate predicates a child has, but ease of discovery cannot count as a criterion for the existence of innate concepts. A cognitive system does not have only the concepts that it's easy for us, cognitivists, to ascertain that it has.

So, in Papert's case, it may take a mathematical genius like Euler to actually discover that the device, somehow, has the predicate "connected" and determine the way this relates to total curvature, but since (C) is the only criterion one can envision, then the device must have (C) as an internal state.

PUTNAM'S CRITIQUE OF FODOR

It is true that learning must be based on dispositions to learn (or "prejudices") that are not themselves the result of learning, "on pain of infinite regress." Everyone, notably including the empiricists, granted that. The causal explanation for these dispositions is, quite plausibly, some functional organization of our brain. But one has to be careful here: if any device that can give correct answers on property P is said to possess some P-related predicate, then thermometers "possess" the predicate "70 degrees," and speedometers possess the predicate "60 miles per hour." This is patently absurd. The dividing line is with systems that can learn about properties, and that can master a language. This needs, however, great caution. Let's imagine two devices: the "inductron" and the "Carnaptron." The inductron is capable of making only an extremely limited set of inductions (say, just 1), monitoring the attainment of a certain threshold of confirming instances, over all instances.

The more sophisticated Carnaptron accepts or rejects certain "observation sentences" in simple first order language, under appropriate circumstances that it can detect. It might well be monitoring some Bayesian degree of confirmation and print out probabilities that the sentences are true. It uses an inductively defined computation program, whose definition is over the set of sentences it can accept or reject.

It is minimally appropriate to describe the Carnaptron as "having" a language. One can well generalize this to a hypothetico-deductive device that carries out eliminative inductions, given a simplicity ordering of hypotheses in a certain language. Now, the inductron is a dismally weak machine, totally unable to account for the mastery of natural language, it does not "have predicates." What is in need of careful analysis is whether a hypothetico-deductive device has predicates.

Let's repeat Fodor's formula:

(A) For every x, P is true of x iff Q(x)

Formula (A) is in "machine language" or "brain language" (or in the "Language of Thought" [LOT], according to Fodor). So is Q, by hypothesis. What about P?

Fodor purports to have shown that P must have a full translation into LOT. So P is synonymous with some predicate that LOT possesses already.

Let's see how this can be false.

Imagine a programmable digital computer, a hypothetico-deductive machine like the one we just saw. Its machine language has "add," "subtract," "go to step N," "store x in register R," etc. But no quantifiers. Generalization (A) cannot even be stated in that language. Formula (A) is, therefore, *contra* Fodor, not in "machine language." Maybe, it's in some formalized Inductive Logic Language (ILL), according to some program of eliminative induction. Suppose, then, that Fodor really means ILL, not LOT. Well, his argument does not hold even in this weaker case, because Fodor's P must be equivalent to some subroutine in machine language (something a compiler can understand and process). Call it S. Even granting that the brain-mind learns P by making an induction, the conclusion will not be formula (A), but formula (B).

(B) I will be doing OK with P, if S is employed.

This (but not [A]) can be stated in ILL, provided ILL contains machine language and has the concept "doing OK with." But this does not require ILL to have synonyms for "cow," "face," "kills," and so on. Fodor's argument has failed.

The punchline is that notions such as "rules of use" or "doing OK with" have been (tacitly) unduly extrapolated, making Fodor's argument seem cogent. But it's not. "Doing OK with P" is not problematic: The device does not really have to "understand" it the way we do. It's just a signal to the device to add S to its repertoire of subroutines. Nothing more. The hypothetico-deductive device, or a collection of such devices, is the best model we have (*contra* associationist models), but not for the reasons offered by Chomsky and Fodor. Better reasons can be offered. And indeed they must.

PUTNAM'S OWN CRITIQUE OF ASSOCIATIONISM

Associationist models can accommodate a very rich repertoire of categorizations and inductions. This is not the problem. But they are all first-level inductions.

These models cannot accommodate higher-order inductions (cross-inductions).

They cannot accommodate inductions on types of inductions (such as: "Conclusions about internal structures from just the observation of manifest properties are usually unreliable"). In order to do this, observations and first-order inductions must be represented in some uniform way. We need classes-of-classes, classes-of-classes-of-classes, and so on, not just things and properties, but inductions as such have to be represented internally in some uniform way, and they have to be quantified over.

The model ceases to be an associationist one. We have to model it as having a (complex) language. This is a persuasive (though not "knockdown") argument that Chomsky and Fodor should have used.

But associationism is not as hopeless as Chomsky and Fodor claim it is.

It allows one to carry out any amount of independent first-level inductions, on any amount of categories and classes, and some can be pretty complicated. One, then, needs a great variety of dedicated subunits, indeed the modules proposed by Chomsky and Fodor. Maybe a strong modular hypothesis is compatible with associationism, after all.

Putnam's punch-line is: Unless one accommodates a lot of cross-inductions and high-level inductions, uniform representations, quantification, and multi-purpose strategies, one has no reasons to exclude an associationist (empiricist) model for language and language learning. Chomsky's and Fodor's rejection of learning altogether, and of anything resembling general intelligence, weakens their anti-empiricist position instead of consolidating it.

FODOR STRIKES BACK

Putnam's suggestion, basically is: What if learning a concept is not learning its content, but something else? Say, its rules of use. Well, the same kind of argument applies. Then "rules of use" are not the result of learning either. You tell me what is necessary and sufficient to learn a concept. Call it X. Fodor's argument will show you that X (whatever X is) has to be innately available to the "learner."

The inference from perceptrons to thermometers and speedometers can be blocked easily. As Putnam points out, perceptrons are supposed to be learning devices, and it is the case that every device capable of learning must have predicates, because these devices must test hypotheses, and there can be no hypotheses without predicates. We have many reasons to say that the inductron has no predicates (as Putnam also concedes). One is that we could not say which predicates it has, even imagining it had some. "Yes there are tree white blobs," "Yes, I am printing 'There are three white blobs,'" "Yes, I am now typing 'Yes,'" and innumerable more, are all coextensive with the machine's rudimentary "behavior."

Only a machine that has access to a sufficiently rich set of computational options can be said to have predicates. Predicates come in systems.

Putnam offers a slippery slope argument: since we cannot set a precise threshold for deciding when a device is sufficiently complex to say it has predicates, then we can only resort to Wittgenstein's criterion of a full set of rules of use and inductive definitions (a full language). Indeed, we have no precise threshold, but we cannot tell precisely, either, when an acorn turns into an oak. This does not mean that nothing is an oak. All we want is to be able to stop the inference short of thermometers and speedometers, even if, for the sake of the argument, we would admit that the inductron has predicates. No real problem here.

The assumptions behind Fodor's argument for predicate innateness are extremely simple and unquestioned by anyone. To repeat: Learning a predicate

is learning its meaning/content. Language learning involves (inter alia) the projection and confirmation/refutation of hypotheses.

Putnam proposes his formula (B):

(B) I will be doing OK with P, iff S employed.

S, indeed, (Fodor agrees) must be specifiable in machine language (in LOT), but Putnam is "thunderously silent" on the origins of S. If learning P really is identifiable with learning S, then the device truly concludes (B). Now, the question is: What can S be? Answer: Some procedure for sorting things into those that satisfy P and those that don't, by reference to whether they exhibit, or not, some property Q. Just add (as one must) that exhibiting Q determines satisfaction of P as a consequence of the meaning/content of P, and you are back to Fodor's formula (A). Fodor's (A) and Putnam's (B) are in fact equivalent. The difference is that Putnam (after Wittgenstein, and with all procedural semanticists) suggests that meaning is "rules of use," and this leads to some operational definition of P. Even granting that, for the sake of the argument, the rules of use (or operational definitions) have to be innate. The argument goes through regardless. You tell me what you think is learned when P is learned, call it X and I show you that X (whatever that is) must be assumed to be innately available to the learner. Period.

In "The Meaning of Meaning" Putnam had taken a different stand. Learning P is to be connected to two things: Some prototypical exemplar and a certain extension. The second component is "not in the mind," not under the governance of the individual speaker. It's determined socially. Only the progress of science may ultimately determine what is necessary and sufficient for a thing to be a P. Fodor remarks that, even in this theory, P is not learned. There is no internal subroutine S, but a complicated collective causal story to be told.¹⁸ In this causal-collective story something may well satisfy S and still not be P (Putnam's famous examples of grug, molybdenum, twin-water etc.). In this story, in fact, P is neither innate nor learned.

It's quite possible, in this story, that nobody "really" ever understood the meaning of P, and nobody will for another (say) two centuries. There is no such thing as essential conditions for being a P (there is no S and no Q). "Meaning ain't in the head." A totally different story.

On cross-induction, Putnam is right: Cross-induction forces us to go well beyond associations, and to impute mental representations and mental computations to the organism, but this does not entail that the organism *only* needs them when it makes cross-inductions. It needs them long before that (for instance for beliefs about the past, the future, false beliefs, counterfactuals etc.). Mental ontology must be separated from epistemology. Suppose we are forced to admit the existence of molecules only when we consider phenomena of solubility. We would not conclude that only soluble materials are composed of molecules. One thing is to admit that there is a mental medium of computation (the Representational-Computational Theory of Mind), another to suppose that a lot of specific and

structured contents are innately present in this medium. Fodor and Chomsky endorse both these hypotheses, while Putnam (and other defenders of "General Intelligence") accept the first, but not the second. Looking at other species, we see a lot of specialized behaviors and specialized innate dispositions (not much of "general intelligence"). It's reasonable to infer that our species is not so different, that our mind-brain is heavily modular.

PUTNAM'S REJOINDER

Quite a lot is known about general learning strategies (Bayesian probability metrics, inductive logic, extrapolation of functions, etc.). So the notion of multipurpose learning strategies is no more vague than Chomsky's "language faculty," or "universal grammar."

Putnam denies that a grammar is a description of properties of his brain, but he does not deny that a grammar is represented in his brain. Putnam says: "*The geography of the White Mountains is represented in my brain, but the geography of the White Mountains is not a description of my brain.*"

There is a referential component to meaning, and it is rooted in causal interactions of whole communities of speakers to stuff out there.¹⁹ Concepts are not in the head. There is also a use component. And Fodor's argument fails even if we limit ourselves to just that component. A subroutine is the description of the employment of a concept, not the concept (the predicate) itself. Even if use were all there is to meaning, then Fodor's argument would show that "*Mentalese contains devices for representing the employment of all predicates, not that mentalese already contains all predicates.*"

CONCLUSION (MINE)

There still is widespread and often fierce resistance to the notion that there can be innate mental *contents*. It's considered OK that there are innate dispositions and innate cognitive processes (devices for representing the employment of mental states, to put it in Putnam's terms), but the innateness of all basic concepts appears to be in a different league of plausibility. Also unacceptable to many is the hypothesis that several formulae like my (L) are innately available to the child, allowing non-inductive parametric language acquisition. A general consideration that supports this hypothesis bears upon a mini-max solution to the problem of language acquisition (Vercelli & Piattelli Palmarini, 2009). Two biological solutions to the problem can be envisioned, in the abstract: (1) Make every linguistic trait innate and be ready to accommodate a quite heavy genetic load. (2) Make every linguistic trait learned and be ready to accommodate a lot of neuronal plasticity and a long and tortuous path of inductive attempts. We think that it could be shown, quantitatively, that the best compromise between these two possible solutions is parametric acquisition. A restricted set of innate pre-wired dispositions to apply formulae like (L), implying the detection of the

relevance of input L toward the parametric value X, and rapid convergence upon parametric value X, as shown by Charles Yang. This should assuage Fodor's perplexity about triggers.

As to the learning of concepts, Fodor's argument is perfectly cogent and is supported by the awesome rate of acquisition of words by the infant and the modalities of acquisition. Recent work by Lila R. Gleitman and co-authors (Medina, Gleitman et al., 2011; Gleitman & Landau, 2013) reports accumulating evidence that child and adult word learning share essential commonalities. Moreover, learners form a single conjecture, retaining neither the context that engendered it nor alternative meaning hypotheses. This rules out, in my opinion at least, a Bayesian process of convergence upon the meaning of basic concepts, unless there is a vast repertoire of strong a priori probability assignments, requiring no multiplicity of exposures and no carrying over of alternative candidates. This repertoire would have to be itself innate. Recent experiments tell us that even in the recognition of possible words, the ability to compute nontrivial statistical relationships "*becomes fully effective relatively late in development, when infants have already acquired a considerable amount of linguistic knowledge. Thus, mechanisms for structure extraction that do not rely on extensive sampling of the input are likely to have a much larger role in language acquisition than general-purpose statistical abilities*" (Marchetto & Bonatti, 2013).

Other suggestions (motor schemata, pragmatic inferences, rules of use, patterns of interpersonal exchanges, general cognitive processes, and so on) bring us back to Fodor's reply to Putnam: You tell me what you think is learned when a concept is learned, call it X, and I show you that X (whatever that is) must be assumed to be innately available to the learner. In a nutshell, it seems to me clear and unquestionable that learning word meanings is a process of activation, not of construction by means of progressive guesses and trial-end-error. Obviously, one cannot activate something that is not there already. Therefore . . .

AUTHOR'S NOTE

I am indebted to Robert Berwick, Luca Bonatti, Stephen Crain, Lila R. Gleitman, and Dan Osherson for their constructive criticisms and suggestions that led to various revisions and improvements.

NOTES

1. This book has been translated into 11 languages, and I am told it is still adopted as a textbook in several courses in several places. But it is presently unavailable, except on the used books market, because Harvard University Press has decided not to reprint it. So be it. I will summarize or transcribe several of the relevant passages here.
2. One caveat must be entered here: some of these authors admit an innate load on the acquisition of concepts, their task being rather to explain the acquisition of

- beliefs and the learning of general categories. Fodor has written extensively on beliefs, but I cannot include a discussion of this issue in the present chapter.
3. I will conform to Fodor's notation, writing the concepts in capitals.
 4. Recent work by Lila R. Gleitman and co-authors (and several prior theorists including, over the years, Irving Rock, Gordon Bower, Charles Randy Gallistel, Roddy Roediger) shows that these "gradual learning graphs" are just a misuse of statistics because, in fact, each individual is essentially a one trial learner (i.e., has the right immediate epiphany when the right situation comes along) and the 'gradual learning' curves are generated only by an illegitimate use of cumulative statistics on data pooled across subjects/items (Gallistel, 1990, Gallistel & King, 2011; Medina, Gleitman, et al. 2011; Gleitman & Landau, 2013).
 5. As I am writing (October 2013), Jerry Fodor and Zenon Pylyshyn have finished a new monograph, "Minds without Meanings: An Essay on the Content of Concepts," which has as its goal to expunge the very notion of meaning and replace it with a causal connection between the speaker, the lexical-syntactic form of a concept in mentalese, and the truth-makers of the concept. They write: "*reference supervenes on a causal chain from percepts to the tokening of a Mentalese symbol by the perceiver.*" I cannot go into this new development in the present context. I will continue to use meaning here, leaving it open whether it can be replaced by the ingredients of the new Fodorian approach.
 6. Early perceptrons did also show that the learning of exclusive "or" (XOR) is impossible, for deep reasons bearing on the separability of predicates. These issues have been well examined in the meantime (Harnish, 2002).
 7. The necessity of prior constraints on hypotheses is a separate thesis from the impossibility of a construction of what is to be learned from preexisting mental primitives. Abstractly, one can imagine that the constraints operate on such constructions or, at the opposite, that constructions from simpler primitives may apply without constraints (making the process of learning indefinitely slow, but this is yet another kind of consideration).
 8. For a formal treatment of the complexity of learning theory and inductive logic see (Jain, Osherson et al. 1999) and the rich bibliography therein.
 9. A subtle development of this notion and its importance is due to the late Robert Nozick, especially in (Nozick, 1981).
 10. For a development of his critique of an empiricist approach to semantics, see Fodor (2003).
 11. This turned out to be strategically unobjectionable but tactically misleading, because several minutes of discussion were wasted by some participants with remarks on the history of the discipline of logic, missing the point that Jerry was making. This segment did not survive into the published version.
 12. It strikes me how similar this consideration about learning is to one made much more recently, in a different context (one we have extensively dealt with in our book on Darwin) by a highly qualified evolutionary biologist: Leonard Kruglyak, Professor of Ecology and Evolutionary Biology at Princeton University, about the genotype-phenotype relation for complex diseases, but the same can be said, I think, for complex traits more generally. He says (*Nature*, 456 (6) November 2008, p. 21: "*It's a possibility that there's something we just don't fundamentally understand, that it's so different from what we're thinking about that we're not thinking about it yet*".

13. One recent revamping of hypotheses linking language and motor control is based on the discovery of mirror neurons, an approach that explicitly goes against Generative Grammar. For an early statement, see (Rizzolatti and Arbib, 1998, 1999); for a more detailed evolutionary reconstruction see (Arbib, 2005, 2012) for counters to this approach to language, see (Tettamanti and Moro, 2012; Lotto et al. 2009; Piattelli-Palmarini and Bever 2002). For doubts that mirror neurons exist at all in humans, see (Lingnau, Caramazza et al. 2009). For a counter to the modularity and specificity of language based on the EEG of motor control and lexical inputs see (Pulvermüller et al. 2005). A rather different recent approach to syntax and motor control is to be found in the reference in footnote 16.
14. Personal communication.
15. Of course, the trigger must be a linguistic input. Italian children do not fix the null subject parameter by eating spaghetti. Interesting issues relate to the set-subset problem. Simplifying drastically, if a child started with a parameter choice that identifies a “larger” language, while the local language is a “smaller” one, then no input would cause her to revise this choice. If, on the contrary, the child starts with a value that identifies a “smaller” language, then a sentence that discloses the local language to be “larger” would cause her to revise that choice. Pro-drop languages are “larger”, because they do also admit sentences with explicit subjects, while non-pro-drop languages are “smaller” because they do not admit sentences without overt subjects. The vignette presented here is, therefore, to be taken with caution. It’s only meant to convey the basic intuition. (I am grateful to Stephen Crain for stressing this point in personal communication). For a recent re-analysis of the very notion of parameter, see (Thornton and Crain 2013).
16. For a brave recent attempt to connect syntax and motor schemata see (Roy et al., 2013)
17. The radical under-determination of what actually has been learned by sheer quantitative data on progressive behaviors eventually made behaviorism implode. See (Gallistel, 1990, 2002)
18. This issue, the nature of inter-personal transmission over time of causal links between a formula in mentalese and its extension, is developed in detail in the new manuscript by Fodor and Pylyshyn (see footnote 4)
19. For insightful developments of this collectivist approach to semantics see the work of Tyler Burge (Burge, 1979, 1996)

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