Forum

*On Language Savants and the Structure of the Mind**

Review of: *The Mind of a Savant: Language Learning and Modularity* by Neil Smith and Ianthi-Maria Tsimpli, 1995

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The mind of a savant by Smith and Tsimpli (henceforth S&T) is one of four books that have appeared in the last few years in which a single case study is used as evidence for the modularity of language (see also Cromer, 1991; Rondal, 1995; Yamada, 1990). In all four books, the protagonist has linguistic abilities at a normal or near-normal level despite mild to moderate retardation in other cognitive domains. The Rondal book describes a rather unusual case of Down Syndrome (unusual because the profile for Down Syndrome typically involves language abilities at or below mental age—Chapman, 1995; Miller, 1987). The other three describe individuals with an uncertain etiology, although hydrocephalus is the expected cause. Of all these studies, the case of Christopher described by S&T is the most interesting, because Christopher's language abilities extend far beyond the boundaries of English. He shows partial mastery of at least 16 different foreign languages, and a talent for learning new ones that is clearly demonstrated even when the authors present him with the problem of learning Epun, an artificial language with peculiar properties that (according to the theory of grammar embraced by the authors) do not exist in the real world and could not be acquired by any normal child.

I was convinced by the end of the book that Christopher is indeed a fascinating young man, but the authors' agenda goes far beyond biography. They believe that they are describing a true savant; as we shall soon see, I am not sure that premise is correct. They also believe that they have provided incontrovertible evidence for the independence of language from cognition, for the modularity of the various subcomponents that make up the language faculty, and for the idea that Universal Grammar is an innate property of the human mind with tremendous explanatory value in the study of first- and second-language acquisition. In the next few pages, I will try to explain why I believe that this book fails in its efforts to support these strong conclusions. In the end, it is a case study in scientific over-reach, and does not do justice to Christopher, its remarkable subject.

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Introducing Christopher

Christopher (henceforth "C") was born in 1962. By the time the authors uncovered this case and began their formal studies, C was already a young adult, so that any information we have about the early stages of linguistic, cognitive, and social development is retrospective, based primarily on anecdotes reported by the family. C's etiology is equally cloudy. His mother was 45 years old when he was born, the pregnancy was problematic, and (for reasons that are not clear) the parents were told when C was six months of age that he was brain damaged. An MRI scan taken in 1993 is not particularly helpful, reporting "Moderate cerebral atrophy with wide sulci over both hemispheres. The cisterna magna was slightly larger than usual and the cerebellar vermis was hypoplastic" (p.4). In other words, there is no evidence for localized brain injury, although C's brain is unusual in size and shape. According to neurological reports, C has "severe impairment of his motor co-ordination, amounting to apraxia" (p.4) He is also reported to have poor eyesight, and a minor speech defect (it is perhaps for the latter reason that this book, which is supposed to be about a second-language savant, has almost nothing to say about C's accent and/or phonetic skill in the various languages that he has tried).

Two different diagnoses have been offered at various points, including hydrocephalus (a distortion of the brain caused by the buildup of fluid, often secondary to spina bifida) and high-functioning autism. The hydrocephalus diagnosis would be compatible with a number of reports in the literature suggesting that language is an area of strength for children with this etiology (Tew, 1975). The autism diagnosis is more puzzling. It is compatible with the reported abnormality of the cerebellar vermis (Courchesne, Yeung-Courchesne, Press, Hesselink, & Jernigan, 1988), and with several behavioral features including an obsessive interest in a single topic (in this case, foreign languages). However, most individuals with autism have language abilities well below their nonlinguistic cognitive functions, which is (as we shall see) most certainly not the case for C. Most people with autism also present with social deficits that are not reported in this case, although C does perform poorly on some "theory of mind" tasks that require reasoning about the way that other people think.

We are told that C was late in walking and talking, a report that is compatible with both autism and/or hydrocephalus. However, by age three he had begun to display his lifelong fascination with language, including an obsession with books that had little to do with their content (e.g., technical manuals and other turgid texts were equally interesting), coupled with early evidence of reading ability. This latter phenomenon, known as hyperlexia, is relatively rare but has been reported in some forms of autism and mental retardation (even though parents invariably find this kind of precocious reading encouraging, it is not always a good sign). C's interest in foreign languages was reported to begin around 6–7 years of age, coinciding with the appearance of the Mexican Olympics on television. He began then to dress up and play games in which he came from a foreign country and spoke a foreign language, a fascination that persisted over time and eventually led to the special skills in second-language learning that are the central topic of the book.

Standardized intelligence tests reveal a pattern of "relatively low performance IQ with an average or above average verbal IQ" (p.4). C's scores on various nonverbal intelligence tests range from a low of 42 to a high of 76 (against a mean IQ for normals of 100). By contrast, his performance on verbal IQ tests range from 89–102. On the Peabody Picture Vocabulary Test, C scored at 121 in English, 114 in German, 110 in French, and 89 in Spanish. His reading

scores for English are equivalent to those of a normal 16-year-old adolescent. To put these scores into context, one may conclude that C has nonverbal intelligence equivalent to a normal child between five and 10 years of age (depending on the task), but his language abilities are in the normal range for young adults, a level observed in at least three languages.

Although this is certainly an interesting and unusual profile, we need to think very carefully about what it can and cannot tell us about fundamental issues in the study of the mind.

On Cognitive Prerequisites to Language

The independence of language from other forms of cognition is one of the major themes of this book. The existence of a language savant like C is offered as one half of the argument, complemented by the existence of syndromes (including adult aphasia and specific language impairment in children) in which language falls far behind nonverbal cognition. This argument is set out early in the book, in a very strong form:

> "The existence of these varied conditions provides a classical example of *double dissociation:* language can be impaired in someone of otherwise normal intelligence, and—more surprisingly—someone with intelligence impaired by brain damage may none the less have normal, or even enhanced, linguistic ability. *It is worth emphasizing that this latter possibility constitutes a refutation of any position that insists on 'cognitive prerequisites' for the development of language*" (p. 3, italics mine).

This view is reiterated with equal vigor at the end of the book, where the authors conclude that:

> "It is no longer plausible to talk of 'cognitive prerequisites' to language. This has been apparent on the basis of many studies, especially of Williams Syndrome. Christopher's case confirms it" (p. 190).

There is a fundamental flaw in this sweeping conclusion: *The amount and type of cognition required to learn a grammar cannot be more than the amount and type of cognition that is available to healthy normal children between 1.5 and 3 years of age, for that is the period in which the bulk of grammatical development takes place.* In other words, normal development already sets an upper bound on the class of possible cognitive prerequisites to language, in the absence of any information about development in special populations. In the case of C and in all the other case studies cited above, the dissociation between language and cognition is observed at a mental age well beyond this window of cognitive development. If a child has a mental age of five or above (depending on the test), we should not be surprised to find that he has near-perfect mastery of grammar, in English or in any other natural language. This is even more true if the individual in question has hovered around a mental age of five or greater for many years before coming to the attention of scientists. One does not have to have a set of power tools to build a two-story house; a simple hammer and a handsaw may be enough if we give the solitary carpenter enough time, and allow him to focus on one job to the exclusion of all others.

In order to prove that cognitive abilities are unnecessary for language, we would have to find a case in which grammar is acquired in the absence of the specific cognitive abilities that two-year-olds have at their disposal during the language-learning process. Williams Syndrome provides an interesting test case in this regard. Williams is, as the authors note in several places, a form of mental retardation in which unusual language abilities are sometimes

observed, despite IQs that average between 45–60 (Bellugi, Wang, & Jernigan, 1994; Karmiloff-Smith & Grant, 1993; Giannotti & Vicari, 1994). Although early reports of this interesting population suggested that language may be well ahead of mental age, more recent studies have led to a more circumscribed conclusion. On most language tests, older children and adolescents with Williams Syndrome perform very close to their mental age, and below their chronological age (Karmiloff-Smith, Grant, Berthoud, Davies, Howlin & Udwin, in press; Grant, Karmiloff-Smith, Berthoud, & Christophe, 1996). However, their spontaneous use of language is often more colorful and florid than free speech by normal controls or by Down Syndrome individuals at the same mental age (Reilly, Klima, & Bellugi, 1991), and they show an unusually good auditory short-term memory (Vicari, Brizzolara, Carlesimo, Pezzini, & Volterra, 1996; Wang & Bellugi, 1994), including an unusual ability to remember and repeat novel words (Karmiloff-Smith & Grant, 1993; Grant, Karmiloff-Smith, Gathercole, Howlin, Davies, & Udwin, in press). More important for our purposes here, studies of very young children with Williams Syndrome have revealed severe delays in all aspects of language learning (Thal, Bates, & Bellugi, 1989; Volterra, Sabbadini, & Capirci, 1993). Grammar does not get off the ground in Williams children until they have a vocabulary size and general cognitive level similar to those of a normal two-year-old child (Singer, Bellugi, Bates, Jones, & Rossen, in press). And in adults and older children with Williams Syndrome, numerous grammatical problems continue to exist (Karmiloff-Smith et al., in press, Giannotti & Vicari, 1995; Volterra et al., 1993.

In short, the individuals or populations cited by S&T do not provide evidence against *"any position that insists on 'cognitive prerequisites' for the development of language"* (p. 5). With a mental age above five years, C does not provide such evidence, nor do any of the other putative language savants that have been reported to date (e.g., Cromer, 1991; Rondal, 1995; Yamada, 1990).

On Modularity and Innateness

Even though data from adult savants cannot be used to rule out cognitive prerequisites to language, they can still be used to argue for a modular architecture, one that emerges over time after certain key cognitive infrastructures are in place. Karmiloff-Smith (1992) has referred to this hypothetical process as "modularization," where modules are the end-product of learning rather than its cause. Bates, Bretherton, & Snyder (1988) make a similar argument, suggesting that "Modules are made, not born." The emergence of such a modular architecture must be constrained by innate biases of some kind. For example, Johnson and Morton (1991) note that newborn infants are strongly attracted to stimuli that contain two circles in a horizontal plane, a bias that leads to extensive learning about faces, and perhaps (eventually) to the emergence of a specialized system for face perception and recognition. If it is also true that the adult brain contains a specialized and compactly localized system of this kind, then we should not be surprised to find that the ability to process faces can be selectively disrupted with focal brain injury. Note, however, that the route from innate biases to a mature modular architecture is very indirect on this account, and highly dependent on experience. Something is innate, but perhaps not very much (for an extended discussion of this point, see Elman, Bates, Johnson, Karmiloff-Smith, Parisi, & Plunkett, 1996).

S&T have proposed a version of modularity that is closer to Fodor's original formulation (Fodor, 1983), requiring far more innate structure than the scenario that I have just described:

> "According to the modularity hypothesis, the human mind is not an unstructured entity but consists of components which can be distinguished by their functional properties. The basic distinction relevant to cognitive architecture is that between perceptual and cognitive systems, where the former pertains to the sensorium plus language, while the latter refers to 'central' systems responsible for the fixation of belief, for thought and for storing knowledge. ... Modules also differ from central systems in being equipped with a body of genetically determined information specific to the module in question which, in the case of language, is UG [Universal Grammar]. This information, in conjunction with algorithms necessary to account for language learning, constitute the basis for claims of innateness. Thus, modularity and innateness within Fodor's theory are intertwined notions." (p. 30–31)

To what extent can data from adolescent and adult savants be used to argue in favor of the strongly nativist variant of modularity? Can any theory account for this kind of architecture without invoking innate grammar? As John Marshall points out in his preface to the book, we have known for some time that linguistic abilities can dissociate to a considerable degree from visual-spatial cognition. This was the first major insight to arise from the intelligence-testing literature, suggesting some degree of dissociation in normal adults between verbal and performance IQ. It has also been known for more than 100 years that language deficits are more likely with left-hemisphere damage, while many (though not all) visual-spatial disorders seem to be associated with damage to the right hemisphere. Although this classic dissociation is robust and well attested, it is ambiguous with regard to the two forms of modularity that I have just described. The language/space dissociation may reflect an innate modular contrast between language and space (specified in detail), but it is also compatible with a situation in which hemispheric specialization arises across the course of development from very small differences between the two hemispheres in the way that information is processed (e.g., a bias toward sequential versus simultaneous stimuli, analytic versus integrative processing, and/or small differences in maturational gradients between the two hemispheres (Allen, 1983; Bradshaw, 1988; Bradshaw & Nettleton, 1981; Bryden, 1982; Corballis & Morgan, 1978; Hellige, 1993). The latter scenario would be more compatible with a number of facts, including the capacity of the human brain to reorganize following early unilateral injury (Bates, Vicari, & Trauner, in press; Stiles & Thal, 1993; Vargha-Khadem, Isaacs, & Muter, 1994), the repeated finding in the last 20 years that the left hemisphere does contribute to specific aspects of visual-spatial cognition (e.g., local details in a visual array (Stiles & Thal, 1993), together with evidence showing that the right hemisphere does make a unique contribution to language (Joanette & Brownell, 1990)).

C's case fits one half of this classic story very well: verbal abilities close to chronological age, with severe deficits in visual-spatial cognition. Stated in that form, however, C's case does not distinguish between direct (innate) and indirect (epigenetic) variants of modularity. In addition, this characterization raises an important question that S&T do not discuss: Is C a linguistic savant, or a man with normal language but severely impaired visual-spatial skills? C's performance in the English language is (as we shall see below) not at all remarkable, particularly when we keep in mind that his worst performance on nonverbal IQ tests reflects a mental age of at least five years, more than enough conceptual power to sustain language learning. Perhaps C's case constitutes one more example of a well-attested visual-spatial defect, with no implications one way or another for the innateness of language (or, for that matter, the innateness of spatial cognition).

What about C's extraordinary ability to acquire a second language? Do unusual profiles of performance like this always arise from innate modules (Gardner, 1983)? Not necessarily. There are, for example, reports of individuals with mental retardation and/or high-functioning autism who can recognize every make and model of American car since World War II. This does not mean, ipso facto, that the normal mind/brain contains an innate module for automobiles! In fact, it has been demonstrated many times that an ordinary mind can do extraordinary things with dogged determination and hard work. This point was clearly demonstrated by Ericsson, Chase, and Faloon (1980), who took a normal college student with an IQ of around 100 and turned him into a wizard capable of remembering strings of close to 80 random digits. This miracle was performed not through neurosurgery or the administration of some new pharmaceutical, but by teaching the subjects well-known mnemonic strategies that convert arbitrary input into meaningful chunks of information. The whole learning process took approximately 230 hours.

To understand why S&T believe that C's profile makes the the case for an innate linguistic module, we need to turn to S&T's core arguments concerning the innateness of Universal Grammar.

On Innateness and Universal Grammar

Chapter 1 lays out the ideology that motivates this book, including some claims about innateness and Universal Grammar (UG) that are assumed but never tested. The authors' overall views on the innateness of language are summarized in the following quote:

> "That some aspects of language are innate (more accurately, 'genetically determined') follows from a number of considerations. The most compelling of these are on the one hand, the existence of universal properties of language and, on the other, the poverty of the stimulus: the fact that as speakers of a language we know more than it is possible for us to have learned on the basis of the input we are exposed to. In the present context, postulating the innateness of a body of information specific to language accounts for a variety of psycholinguistic phenomena: first, the uniformity of the mature state of competence attributed to all native speakers of a language; second, the existence of common developmental patterns in the process of first language acquisition; third, the occurrence of cases of neurological breakdown resulting in selective impairment of the linguistic component of our mental architecture." (p.22)

These views are presented as though they were accepted by all qualified authorities in the fields of linguistics and psycholinguistics, but the fact is that all these claims are highly controversial (for reviews, see Bates, Bretherton, & Snyder, 1988; Bates & Elman, 1996; Bates et al., in press; Elman et al., 1996; Karmiloff-Smith, 1992).

First, the existence of language universals does not provide compelling evidence for the innateness of language, because such universals could arise for a variety of reasons that are not specific to language itself (e.g., universal properties of cognition, memory, perception, and attention). To offer a simple analogy, in every human culture that has been studied to date the great majority of normal children and adults eat with their hands (with or without an intervening tool, e.g., a fork or a chopstick). To explain this universal, we do not need to posit an innate hand-feeding module, subserved by a hand-feeding gene. A simpler explanation can be found in the multi-purpose structure of the human hand, the position of the mouth, and the nature of the foodstuffs that we eat, which (taken together) dictate that eating with the hands

will be the simplest and most efficient solution to the problem. In the same vein, we may view language as the solution (or class of solutions) to a difficult and idiosyncratic problem: how to map a rich, high-dimensional meaning space onto a low-dimensional channel under heavy information-processing constraints, guaranteeing that the sender and the receiver of the message will end up with approximately the same high-dimensional meaning state. Given the size and complexity of this constraint satisfaction problem, the class of possible solutions may be very small, and (unlike the hand-feeding example) not at all transparent from an a priori examination of the problem itself.

To illustrate the latter point, consider the peculiar fact that primary visual cortex contains neurons that are tuned to the orientation of lines. Should the existence of these odd little universals provide evidence for innate modules in the visual system? Indeed, it is possible (in principle) that nature has evolved a way to set these universals up in advance. However, it has now been demonstrated that line orientation cells arise again and again when ignorant neural networks with no prior knowledge are forced to solve the problem of mapping three-dimensional information onto a two-dimensional retinal display (Miller, 1994; Shatz, 1996). Such cells seem to be a necessary part of the solution to this particular mapping problem, for reasons that are not obvious a priori. Furthermore, learning machines of this particular type (i.e., multi-layered neural networks) are able to find the solution in the absence of innate knowledge. That which is inevitable does not have to be innate! The same may be true for universal properties of grammar in the mature state, and for those regularities in language development that are observed across children and communities.

In view of such discoveries, the poverty-of-the-stimulus argument must be re-examined as well. Linguists of a nativist orientation tend to recite this argument like a mantra, but we must remember that it is a conjecture, not a proof. Gold's theorem (Gold, 1967) is often cited as a proof that grammars of the sort that characterize natural language cannot be learned in the absence of negative evidence (i.e., in the absence of explicit information about structures that are forbidden in the language). However, Gold's theorem is only relevant if we make some unrealistic assumptions about the nature of the learning device, the nature of the input, and the nature of grammar itself. If we change any of these assumptions, then we are back in the domain of the unknown. Neural network simulations of language learning are still in their infancy, and it remains to be seen how much of human language learning they are able to capture, but some critical existence proofs are available that work against the poverty-of-the-stimulus argument (Elman, 1993; Hare & Elman, 1995; MacWhinney, Leinbach, Taraban, & McDonald, 1989; Plunkett & Marchman, 1991, 1993; Rumelhart & McClelland, 1986; Seidenberg, 1992). For example, we now know that systems of this kind are conservative (they stick close to their data and do not "guess" wildly implausible grammars), that they are nevertheless capable of going beyond their data (e.g., generalizing to novel instances, with occasional overgeneralization errors on familiar items), that they are able to recover from error in the absence of explicit negative evidence, and that they can master long-distance dependencies that were once believed to be beyond the capacity of any inductive device. I am not declaring victory here. A great deal remains to be done. The point is, simply, that the case for the unlearnability of language has not been settled, one way or the other.

But what about the long list of detailed and idiosyncratic properties described by Universal Grammar? Is there any way that these eccentric structures could be learned? Although this is an open issue, one has the right to ask a prior question: How do we know (and

why are the authors so sure) that this particular theory of grammar is a correct description of the human language faculty? S&T assume throughout their book that UG is the only theory worth testing in the framework of their case study. A reader who is naive about the range of options available in modern linguistics will come away with the impression that UG is a widely accepted doctrine, as well established and well accepted among linguists as quantum mechanics in physics or Darwinian evolution in biology. No alternative theories are mentioned, for linguistics proper or for the fields that study language learning. For example, when the audience is introduced to issues in first-language acquisition (p. 23), we are told that there are two competing theories of the acquisition process: *continuity*, that is, the theory that all the options of UG are there from the beginning, with selection of alternative parameter settings determined by linguistic input, versus *maturation*, that is, the theory that some of the options contained in UG emerge over time on a genetically determined schedule. There is, of course, a third possibility that the authors do not entertain: that UG is not innate in any form, and may not be a veridical account of the representations that comprise linguistic knowledge in children or adults. Of course we all have the right to analyze our data from a chosen theoretical framework, but it is usually considered wise in the course of scientific inquiry to set up an experiment in which the theory could be proven wrong. Assuming that the theory is correct, the authors make a crucial logical leap, which can be paraphrased as follows:

1. English has property P.
2. UG describes this property of English with Construct P'.
3. Children who are exposed to English, eventually display the ability to comprehend and produce English sentences containing property P.
4. Therefore English children can be said to know Construct P'.

There is, of course, another possibility: Children derive Property P from their input, and Construct P' has nothing to do with it.

An equally limited theoretical menu is offered to explain second-language acquisition (pp. 35–36). Two contrasting theories of this process are proposed. In the first theory, first (L1) and second (L2) language acquisition are qualitatively different because:

> "For first language acquisition, the language module, i.e., UG and parameter-setting, can provide an adequate description of the process involved, whereas learning a second language crucially involves general learning mechanisms."

On this account, L2 learning is inferior to L1 learning because these general learning mechanisms or GLMs are not up to the task. This GLM approach is contrasted with the second theory, in which "The principles of UG constrain L2 grammars much as they constrain L1 acquisition." On the latter account, learners will construct intermediate grammars in the course of learning in which they transfer the parameter settings of their native language onto L2; eventually L2 learners can go on to reset those parameters in the correct direction, but this is a protracted process and many individuals never achieve it (which is why L2 learning is generally inferior to L1). Both theories assume the accuracy of UG as a model of the language faculty. There is no box where the reader can check a third alternative: "None of the above." In fact, when these two UG-based theories of L2 learning are applied to C's data and that of normal controls in the Epun experiment (see below), neither of them fit the data particularly well. S&T end up falling back on ad hoc strategies for circumventing UG, and a variant of the

general-learning-mechanism approach that performs so well at the language-learning task that one wonders why an innate language-specific learning device ever evolved in the first place.

It is difficult to conduct a clear empirical test of acquisition theories based on UG, because the theory itself permits so many possible outcomes. Readers who are unfamiliar with modern linguistic theory may assume that the term "universal" refers to the intersect of all natural languages, that is, the properties that they all display. However, as generative grammar has been extended to a wider array of languages, the definition of "universal" has shifted from the intersect to the union, that is, to the set of structural options that are possible across natural languages (called "parameters") and to the process by which the learner figures out which options apply in his/her language (called "parameter setting"). Three features of this new approach serve to insulate UG from a rigorous empirical test.

1. *Disjunctive universals.* Some proposed universals take the form "A or Not-A." An example would be the Null Subject Parameter, which dictates that a language either *will* or *will not* permit the omission of overt subjects in a free-standing declarative sentence. If we assume that parameter setting is binary, and we disallow "in-between" settings, then disjunctive universals exhaust the set of logical possibilities, and cannot be disproven.
2. *Silent universals.* Some proposed universals are allowed to be silent or unexpressed if a language does not offer the features to which those universals apply. For example, universal constraints on inflectional morphology cannot be applied in Chinese, a language that has no inflectional morphology of any kind. This apparent anomaly for UG is resolved by insisting that the requisite universal structures are present, but have no overt effect on this particular language. It is difficult to disprove a theory that permits invisible entities with no causal consequences.
3. *Sentence-level universals.* Assuming that a parameter does apply within a language, and takes a binary value (e.g., A), one does occasionally have to deal with apparent exceptions (e.g., Not-A). For example, English is a language in which omission of the subject is not permitted in free-standing declarative sentences. And yet we often do hear English speakers saying things like "Got it, thanks." To deal with such apparent exceptions, it has become customary to distinguish between sentence-level grammar (the domain to which UG applies) and utterances that have to be explained at a discourse level (a domain to which UG does not apply, handled by some separate module or by the General Learning Mechanism cited above). This may be a legitimate distinction, but it is a risky one. In the absence of a clear and independent metric for distinguishing between sentential phenomena and discourse-driven facts, the theorist may be tempted to throw all inconvenient phenomena into the discourse bin. The problem is especially serious for the language learner, who needs still more innate machinery in order to distinguish between those input types that can be used to set parameters, and those that would result in a false setting if they were applied.

In their application of UG to C's data and that of normal controls, S&T have added some additional mechanisms that protect the theory from disconfirmation, in a complex variant of the old competence/performance distinction. They start out with a learning model (a hybrid of Fodor, 1983 and Anderson, 1992) in which the language module is encapsulated from central

processing, but they end up with a model in which intimate interactions between UG and the general learning mechanism are permitted throughout the language-learning process. The GLM now serves as a rather fickle *deus ex machina*, rescuing the L2 learner from old parameter settings in some situations (e.g., learning a language with null subjects) but permitting L1 structures to sneak into L2 on others (e.g., transfer of English word order preferences).

This brings us at last to a brief examination of the empirical findings for C in first- and second-language acquisition, including the ingenious Epun experiment.

How Christopher Learns a Second Language

Chapter 2 is devoted to a qualitative examination of C's abilities in English, his first language. Recall that C's verbal IQ is in the average or low-average range, depending on the test. Chapter 2 supplements these standardized test findings with the classic methodology in generative grammar, asking C to make a series of well-formedness judgments for sentences containing fine-grained morphological, syntactic, semantic, and pragmatic distinctions. In contrast with the L2 results presented later, S&T do not offer any quantification of the findings, and no information from normal controls. Instead, they follow the standard practice of assuming that all native speakers agree about the structures in question. This assumption is on shaky grounds (Blackwell, Bates, & Fischer, 1996; Levelt, 1972, 1977). Studies of grammaticality judgment in naive native speakers invariably reveal ample evidence for variability, with agreement ranging from 60% to 95% depending on the structures in question (where 50% would reflect chance performance). The authors find that C performs very well (they use the word "perfect") on sentences testing for morphological and syntactic violations, but he does occasionally miss on items that are presumed to reflect semantic and pragmatic judgments that (we are told) require input from central cognition. It would have been useful to know if normals also show more agreement on the morphosyntactic items, and less agreement on the semantic/pragmatic set. Be that as it may, I am willing to stipulate that C knows English, at a level comparable to any native speaker with a verbal IQ of 100 and a mental age between 5–10 years of age.

Chapter 3 summarizes evidence relevant to C's abilities in the various foreign languages that he has studied. The data here are exceptionally rich: C is asked to translate words, sentences or short discourse fragments out of or into the many languages that he has worked with over the years, and he is also asked to make well-formedness judgments about sentences in a subset of these languages. In some cases, evidence from normal controls is also provided. The evidence shows that C's abilities are quite rudimentary in some cases (e.g., Hindi), even though he says he "knows" the language in question. However, his ability to translate back and forth from French, Greek, Spanish, German, and Italian would be the envy of any Anglosaxon diplomat.

It is at this point that S&T begin to ask about C's profile of strength and weakness within L2 learning, raising crucial questions about the internal structure of the language module. If C's abilities are lined up in a rough order, from best to worst, we can derive the following hierarchy:

LEXICON > MORPHOLOGY > SYNTAX > PRAGMATICS

What accounts for this hierarchy? Does each term refer to a separate submodule, or can we draw the lines in between in a more parsimonious way? In the course of this discussion (and in Chapter 4), S&T's characterization of the line that separates one subcomponent from another seems to shift. Earlier in the book (p.41), they had suggested that the critical boundary might lie between grammar and the lexicon, citing controversial claims by Gopnik (1990; Gopnik & Crago, 1991) concerning a putative single-gene defect in a family of individuals (the K family) who are incapable of learning regular grammatical morphemes (which lie within the grammar), even though their ability to deal with irregular morphemes (attributed to the lexicon) is supposed to be intact. It is worth noting here that the Gopnik report has been criticized by Vargha-Khadem, Watkins, Alcock, Fletcher, and Passingham (1995), who have carried out extensive testing of the K family across an eight-year period. Vargha-Khadem et al. have shown that affected members of the K family perform equally badly on both regular and irregular morphemes (i.e., there is no dissociation), and they are significantly impaired relative to unaffected family members on a host of other language and nonlanguage tasks (i.e., the disorder is not specific to language, much less to grammar). However, this criticism may be moot, because the critical border has moved by the end of Chapter 3.

In Chapter 3, we learn that C's greatest strengths lie in learning the new words of a foreign language, and in pulling out morphological paradigms to explain inflectional variants of those words. When C was introduced for the first time to Berber (a Hamito-Semitic language that he did not know), he "concentrated his efforts to a considerable extent on identifying morphological distinctions and trying to form paradigms for newly acquired items" (p.83; see Chapter 4 for more details). Shortly after this, S&T offer what may be the best definition in the book of C's gift:

"Christopher's performance reflects his enhanced ability to register pairings of morphological form and semantic content on minimal exposure. As a result of induction or instruction, this process gives rise to the construction of a rule of derivational or inflectional morphology" (p.83).

Unfortunately, the term "minimal exposure" is not defined. Because we know that C spends most of his waking hours working on the learning of foreign languages, it is not at all clear that his ability to pick up vocabulary is supranormal in any interesting sense. In any case, S&T go on from this account of C's lexical gifts to discuss how this process results in overgeneralization errors, insisting that such errors are definitive evidence of a rule-based system, a conclusion that has been hotly contested in recent years following the discovery of overgeneralization errors in neural network models of morphological learning (cf. Plunkett & Marchman, 1991, 1993; Elman, 1990, 1993; Elman et al., 1996, Chapter 2). At the same time, however, S&T also stress that the mechanisms supporting overgeneralization are not part of parameter setting within UG:

> "Morphological and lexical aspects of language acquisition are in large part independent of parameterization, are nondeterministic and allow of correspondingly different developmental processes. However familiar or unfamiliar he is with the language concerned, the most impressive aspect of Christopher's linguistic talent is his learning, accurate or inaccurate, of lexical and morphological information. Assuming that the morphological component is a distinct sub-part of the human mind-brain whose internal structure can be independently characterized, we wish to argue that learning lexical and morphological properties does not entail learning the syntax associated with those properties" (p.84).

In other words, the very attributes that are C's greatest gift are not part of UG in the strict sense.

S&T spend some time considering whether the lexical and morphological components are independent from each other (as well as from syntax). Their experiment on the translation of cognates is particularly important in this regard, because it is intended to show whether C and/or normal controls display interactions between the lexical and morphophonological dimensions required for this task. They conclude that C performs very much like normal controls, both qualitatively (performing worse on cognates than noncognates, with systematic errors on polymorphemic items) and quantitatively (coming in fourth best out of 14 college students who represent various degrees of L2 learning in the test languages). From my point of view (the authors are a bit less certain in the matter), this suggests a significant degree of interaction between the lexical and morphophonological components, which indicates in turn that the Big Modular Boundary may lie between lexical and morphological processes on the one hand, and syntax on the other.

This conclusion is underscored by various results in Chapters 3 and 4 showing that C makes numerous syntactic errors, in production and in grammaticality judgment. In particular, he shows significant transfer from English to his subsequent languages in all aspects of syntax except subject omission, which he uses readily in any language in which this option is appropriate. In known languages and in the impossible language Epun, C resists word orders that are incompatible with English. To what extent is C deviant in this respect? Here too, the authors proceed with a certain degree of ambivalence. In the Epun experiment in Chapter 4, S&T present C and four normal controls with an invented language containing various rule types, each associated with a different result, as follows:

1. Structure-independent rules that violate constituency proved impossible for everyone.
2. Structure-dependent rules that are impossible in UG proved to be within the abilities of controls, but C could not acquire them until he had had prolonged exposure.
3. Structure-dependent rules that are plausible in UG, but do not follow the parameter settings of English, proved to be learnable by everyone but to a different degree, with different errors displayed by C and controls.

In general, C was "significantly inferior to the controls in learning arbitrary syntactic patterns" (p. 154), but "better than them in learning anomalous agreement paradigms where there was overt morphological evidence of the irregularity" (pp. 154–155). This seems to contribute to a general picture in which C is better at morphology and worse at syntax. However, there were some clear exceptions to this generalization. First, S&T note (p. 151) that C failed to master a past tense peculiarity of Epun that proved very easy for three of the four normal controls. This anomaly is explained by suggesting that three of the normals were able to hit upon "some inductive strategy" (p. 151) that was not available to C (nor, by extension, to the one normal who found this structure very hard). Also, C appeared to be "quite unable to master the auxiliary system, despite the fact that it was modeled directly on English and so presumably 'possible,' and putatively accessible on the basis of transfer" (p. 155). S&T speculate that C's problems with the auxiliary system stem from the fact that Epun is a richly inflected language, a fact which leads C to expect a different kind of auxiliary system. Although this inference is attributed to the influence of UG, it is quite possible that it is simply

another example of transfer, in this case transfer from one or more of the richly inflected languages that C has studied in the past. In fact, it is worth pointing out that C has far more second-language history than any of the four normal controls who participated in the Epun experiment, and hence he should be vulnerable to transfer from those languages as well as transfer from English.

At the end of Chapter 4, S&T provide the following summary of their results:

> "The combined results from the experiment to teach Christopher new languages under conditions of controlled input are more than suggestive. It seems clear that, while there is no evidence for complete mastery, there is support for the directing role of transfer from the first language and for the importance of UG; there is clear indication that the learning of morphology and the lexicon is different in kind from the learning of syntax; there is evidence that, at least in this unnatural context, second-language learning exploits inductive strategies as well as modular capabilities; and of course there is yet another demonstration of Christopher's remarkable talent in mastering (parts of) the structure of new languages." (p. 155)

It seems to me that UG is honored more in the breach than in the observance in all of these results. Unexplained inductive strategies and general learning mechanisms are allowed in to account for exceptions to UG. As for those aspects of the data that fit within UG, all of them (as best I can tell) could be explained in terms of transfer or generalizations based on English (the first language for all subjects) and/or on the many other languages to which C has been exposed. There is nothing in these learning patterns that could not be explained by powerful inductive learning mechanisms of the sort that have been proposed in the last few years (Bates & Elman, 1996). This is true not only for the structure-dependent rules and error types, but also for the fact that subjects find it difficult to learn structure-independent rules that violate constituency. (Object identity and consistent part/whole relations are important for all sophisticated learning mechanisms, and this is what grammatical constituency is really about.)

In the end, C's gift for L2 learning is somewhat disappointing from the point of view of an innate language module. He is, it seems, particularly good at learning words—although it is not clear that he is better at learning words than any normal child or adolescent would be if they spent their lives working at this process. C has also developed a "system" (as they say in the casinos) for extracting morphological paradigms, a conscious strategy of the sort that I myself have applied (albeit with less success) in approaching a new foreign language. In Chapter 5, we are reminded that this "system" is good but far from perfect. C does make quite a few morphological errors, especially errors of agreement. S&T comment on this fact by noting that "These are syntactically determined matters of inflectional morphology, and his command of (lexically based) derivational morphology is far better" (p. 157). In other words, C's talent is primarily lexical in nature. The farther we move from the lexicon along the continuum described above, the less successful C seems to be. And what of all those failures in syntax? S&T suggest that these failures provide support for the second of the two UG-based approaches to L2 learning, that is, parameter setting shuts down after L1 learning so that the bulk of L2 learning must be accomplished by a general learning mechanism. However, they do allow for the possibility of a hybrid model, where some aspects of UG are available but others are not. At this point, the UG-based position has been weakened to the point where I think it is fair to question whether it contributes anything at all.

Looking once again at the above continuum of strengths and weaknesses, I propose an alternative view: C's special talent lies in word learning, and (to a lesser degree) in all the inflectional and derivational phenomena that are overtly marked on single words. This part of the argument is in line with S&T's conclusions, but the next part is not. In particular, I propose that C is relatively weak at syntax, and falls back on L1 and L2 transfer, because the syntactic dimensions at issue (e.g., that-trace, coreference phenomena, highly marked word orders) occur less frequently in the input, and because they are governed by subtle discourse constraints and pragmatic inferences that are (we are told) another of C's weakest areas. In this regard, recall that subject omission is the one arena of syntax that C picks up with alacrity. Linguists working in the UG framework have argued that this parameter controls many other aspects of the grammar, including the presence of empty pronominal subjects (e.g. "it" in "it is raining"), the amount and type of morphological marking that is required, and the nature of the auxiliary system. For C, these features of the grammar do not apply as a group. However, as S&T point out, more recent linguistic studies have shown that they do not necessarily apply as a group in natural languages either (e.g., Chinese is pro-drop, but it has no inflectional morphology at all). If the pro-drop parameter governs no feature other than subject omission, then we should not be surprised to find that C picks up this parameter quite easily. Subject omission is a syntactic option that children learn very early, and it generally poses no serious problem for second-language learners. As a 19-year-old exposed to Italian for the first time, it seemed to me that I had known all my life how and when to omit subjects. I simply generalized from informal English options like "Got it, thanks," moving the boundary of acceptable omissions further along some dimension of "amount of givenness." Subject omission may be the one syntactic variable that is particularly easy for C because it is a pervasive part of discourse grammar, an arena where languages differ by degree rather than some arbitrary, discrete parameter.

Putting these strengths and weaknesses together, I suggest that C's data provide evidence for a boundary between pragmatics and the lexicon. The more C has to rely on discourse and pragmatic inferences, the harder things are; the more he can rely on a pattern-matching strategy, the easier he finds it to acquire a bit of L2. One can account for all these data (and for those of other putative language savants) without invoking an innate Universal Grammar, or its popular cousin the Language Instinct (Pinker, 1994).

Who is Christopher?

In the end, it seems that Christopher's gift can be explained by an unusual reliance on relatively simple, widely available mechanisms for learning and pattern recognition. These mechanisms are not specific to Christopher, and may not be specific to language. However, Christopher has made the application of these mechanisms to foreign language his life's work and greatest achievement. There is a great beauty and a genius in this, even if it tells us nothing about the modularity of brain and mind. The title of this book reminds us (intentionally I think) of Alexander Luria's book *Mind of a mnemonist* (1968). In that book, Luria reviews a mass of fascinating evidence for a single case that displays what the common man calls "photographic memory," complemented by a form of synaesthetic perception that may be integral to the subject's mnemonic strengths. At the same time, however, Luria gives us a haunting sense of what it must be like to live inside such a mind, thereby integrating science and biography to produce a masterwork. Smith and Tsimpli have given us a mass of

interesting data about Christopher's first- and second-language abilities, and for that reason alone I would recommend this book to anyone interested in the language-learning process. Unfortunately, in their zealous efforts to provide evidence for one particular linguistic theory, they have told us very little about Christopher as a human being. What is it like to live inside that mind? Perhaps someday Christopher will tell us himself.

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