Review of:
Morten H. Christiansen and Nick Chater, *Creating Language: integrating evolution, acquisition, and processing*; with a foreword by Peter W. Culicover. xiv + 330 pp. Cambridge, Mass. and London: MIT Press, 2016.

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Morten Christiansen and Nick Chater are psychologists whose book is intended as a refutation of the Chomskyan idea that human beings inherit specialized brain mechanisms determining the structure of humanly-usable languages. As they summarize their own, contrary, view, "It is not that people have evolved to learn language; rather, language has evolved to fit the multiple constraints of our learning and processing abilities". The Chomskyan emphasis on modelling the end-state of language acquisition as a generative grammar which uses production rules to define "all and only" the valid sentences comprising the language is seen by Christiansen and Chater as distorting the nature of the language-learner's achievement: they view language acquisition "not as learning a distant and abstract grammar, but as *learning how to process* language" (their italics).

Christiansen and Chater develop several convincing arguments to the effect that generative linguistics has closed off routes to a better understanding of human linguistic ability, by framing its discourse using assumptions which look innocent but in fact embody misleading concepts. For instance, they urge the importance of a distinction that is undoubtedly real but has rarely been emphasized by linguists discussing first-language acquisition, between arriving at a correct understanding of some aspect of the natural world, and acquiring the ability to co-ordinate with other people – they call these "N-induction" and "C-induction", and say that in N-induction "the world imposes an external standard, against which performance is assessed", but in C-induction "the key is that we do the *same* thing, not that we all do an objectively 'right' thing". Christiansen and Chater claim that this distinction matters for two reasons: as a matter of logic, C-induction is "dramatically easier" than N-induction, and also, while natural selection could lead to the creation of "dedicated, domain-specific learning mechanisms" for solving N-induction problems, the same cannot be true for C-induction problems. I shall not attempt to reproduce their arguments for these two positions, but if they are right, it is easy to see how the idea of innate grammatical knowledge might have become spuriously plausible through misconceiving the language acquisition task as if it were an N-induction problem whereas in reality it is largely a C-induction problem.

A very important constraint on our ability both to understand individual utterances of our native language, and to acquire that language in the first place, is what

Christiansen and Chater call the "Now-or-Never bottleneck". That is, we cannot accumulate all the raw linguistic data we encounter and preserve them indefinitely, ready to be reconsidered at any later time in the light of new data: we must economize on mental storage by analysing what comes in as it comes in, keeping the concise product of analysis but throwing away the bulky data-set on which it is based. (To see how plausible this is, contrast it with the special situation created by the invention of writing and other recording technologies. Someone reading a written garden-path sentence, such as the famous *The horse raced past the barn fell*, can easily scan back and reconsider the word *raced* when the word *fell* shows that taking *raced* as past tense must be wrong, and he could do so even if far more than three words intervened – but in speech words vanish as fast as they are uttered, making such reconsideration difficult. Likewise, a scientist can check back and re-evaluate years-old lab records if an observation taken today challenges longstanding theoretical assumptions, but a child hearing an utterance which seems at variance with what he already knows of his native language can only, realistically, accommodate the new datum by adapting his existing model of the language, rather than abandoning it and building a replacement from scratch.) Christiansen and Chater argue that the Now-or-Never bottleneck has far-reaching implications for what human languages would have to be like and how acquisition would have to proceed, and they say that these implications often match features which the generativists explain by postulating innate linguistic knowledge. Again space forbids me to reproduce the details of their argument, but if it works it evidently makes the innate-knowledge postulate redundant.

Much of what Christiansen and Chater say about misleading assumptions made by generative linguistics seems to me very reasonable. But then, I do not take generative linguistics seriously anyway (cf. Sampson forthcoming). I am not sure how successful Christiansen and Chater's book would be at changing the minds of readers who give Chomskyan ideas more credence. When Christiansen and Chater urge that "language acquisition involves learning how to process linguistic structure, rather than inducing a grammar", it is easy to imagine a Chomskyan retorting that these are just two ways of talking about essentially the same thing. No-one imagines that a surgeon examining the brain of an English-speaker could find anything physically akin to a list of rules beginning S → NP VP, VP → V NP (or whatever the generativists think an English grammar looks like nowadays). All the Chomskyans have ever claimed is that grammars, as systems on paper, capture an important aspect of the workings of our mysterious language-processing abilities, in the same way as Newton's laws of motion capture an important aspect of the behaviour of heavenly bodies. However powerful the telescope we use, it will never show us any object in the sky that physically encodes the equation $F = \dfrac{mv - mu}{t}$, and similarly a grammar is not claimed to be a direct picture of anything in a speaker's brain.

In order to convince readers that they have a better account of language than

Chomsky, Christiansen and Chater have to identify facts about language which Chomsky explains in one way, in order to show that they can explain the same facts in a more convincing way. One of the main lines of evidence used by generative linguists is "UG", i.e. "Universal Grammar" – the alleged existence of apparently unmotivated features common to the structure of all human languages, suggesting that they are fixed by the contents of the human genome. Near the beginning of their book, Christiansen and Chater agree that there are such features, which they undertake to explain differently. Yet their example seems strangely weak, considering the importance of the issue for their overall thesis. They quote four simple English sentences: 1 *John sees himself*, 2 *John sees him*, 3 *John said he won*, and 4 *He said John won*; and they say that *himself* in (1) must be John, while *him* in (2) cannot be John, and that *he* in (3) can be either John or someone else, while in (4) *He* must be someone else. From a functional point of view, they say, "numerous alternative patterns" of pronoun usage would "serve equally well", so the fact that the patterns of (1)–(4) are universal means that they "may be presumed to be part of the genetically encoded UG".

I have no detailed knowledge of "binding theory" (the aspect of generative linguistics which Christiansen and Chater cite in this connexion), but surely it must be a bit more substantial than this? I do not see what the "numerous alternative patterns" of pronoun usage could be (Christiansen and Chater do not identify any of them). A pronoun is a short way of referring to an entity that needs no fuller specification because it is already before the hearer's mind, so it seems inevitable that *He* in (4) cannot be John (if prior circumstances have brought John to the focus of attention, then *he* should appear in both clauses, and if not then *he* won't work before John is mentioned). One "alternative pattern" would be that *him* in (2) could be either John or someone else – but although that would not be usual in standard English, it seems to me that there are dialects in which it is normal enough (cf. the well-known saying "A daughter's a daughter for all of her life, a son is a son till he *gets him* a wife." Does it make a difference that *him* is indirect object here? Why is that relevant?) The reason for Christiansen and Chater to quote these examples at an early point is in order to establish that the generativists have a serious case which will need serious argument to answer. But I cannot imagine many readers finding this convincing. They will think either "If this is all there is to generative linguistics, it is not worth writing a long book to refute it", or else "These authors must have misunderstood the theory they are attacking, so there is little point in examining their arguments against that theory".

It gets worse. Later in the book Christiansen and Chater point out that various leading generative theorists seem in recent years to have retracted the claim that there is much or any Universal Grammar (they quote Hauser, Chomsky, and Fitch 2002 and Pinker and Jackendoff 2005 – and they might have added e.g. Culicover 1999: 137–8); but Christiansen and Chater do not accept this retraction. They say that the generative theorists they quote do in fact assume "highly complex language-specific

mechanisms that are unlikely to be the result of general laws of nature", while their own book argues that there are "few true language universals", and that, properly considered, "the usefulness of the term UG seems to evaporate". Here the likely reader reaction might be along the lines "The generativists had an implausible idea, they admit now that they were mistaken, but you won't accept their admission because you want it to be you who do the refuting".

In their Preface the authors explain that the book has been assembled from a set of previously-published journal articles, which have been substantially rewritten to fit them together as a single work. This can be a worthwhile way to produce a book, but only so long as all the original articles express ideas which jointly contribute to a coherent thesis. In the present case it is not always clear how some lengthy passage is intended to fit into the book as a whole. Christiansen and Chater's chapter 5 is about the pervasiveness of sound symbolism. All English-speaking linguists are aware of word-families like *sneer*, *snivel*, *snow*, *snot*, *snide* ... which share a consonant-cluster /sn-/ and a connotation of nastiness, despite the fact that /sn-/ seems never to have been an independent root morpheme. And Christiansen and Chater mention psychological experiments in which subjects are asked to assign the invented names *kiki* and *bouba* to two shapes, one a star with sharp points and the other a blob with a rounded outline; speakers of diverse languages agree in calling the star *kiki* and the blob *bouba*. Facts like these are usually seen by linguists as marginal curiosities. Christiansen and Chater quote Steven Pinker as writing (1999: 2) "onomatopoeia and sound symbolism certainly exist, but they are asterisks to the far more important principle of the arbitrary sign". But Christiansen and Chater believe this understates the pervasiveness of sound symbolism. They discuss research by Christiansen and colleagues which looked for phonetic/semantic correlations among English words that are not obviously onomatopoeic, such as *cat* or *dog*. Using standard phonological features, and semantic features based on sources such as WordNet, words were assigned to locations in phonological and semantic similarity spaces, and statistical techniques were used to look for correlations between the respective spaces with respect to the placement of words within them. The researchers found "a small, but highly significant, positive correlation ($r^2 \approx 0.002$) suggesting that there is some overall systematicity in English sound–meaning correspondences". This is taken as confirmation of their earlier suggestion that "the sign is not entirely arbitrary and that some, perhaps even considerable, systematicity does seem to exist in form–meaning mappings".

This result is certainly unexpected and interesting, though no research quoted by Christiansen and Chater (nor their more recent, cross-linguistic research in Blasi et al. 2016, which is making a splash as I write this review) justifies a suggestion of "considerable" systematicity. Pinker's remark about asterisks to the principle of the arbitrary sign still seems right, though perhaps the asterisks are in bolder type than Pinker believed. But suppose that such correlations eventually turned out to be even stronger than this research has shown – what would follow? Christiansen and Chater

attempt to relate such a scenario to their anti-generativist thesis, but this part of their discussion is weak. The obvious conclusion to draw would be the reverse. If, in languages of different families, words for 'cat', 'dog', etc. always sounded similar to /kæt/, /dɒg/ ... respectively, that would be excellent evidence for innate language-specific brain mechanisms. (But they don't.)

It is hard not to think that the real reason for including chapter 5 in this book must be that one co-author has researched sound-symbolism and could not bear to leave the findings gathering dust in journal back-issues, when there was a chance of giving them wider exposure in a book. But the result in this case is a book which feels unsure of where it is going.

Another unusual feature of the book structure may or may not have originated in the same way. Scattered through its pages are many "boxes" – passages enclosed in rectangular borders, with their own titles and set in smaller print than the main text. One might see these as akin to footnotes, but they are not tied (as footnotes are) to precise points in the main text, and they are far longer than normal footnotes, sometimes comprising more than half of the total wordage on their page. Boxes are familiar in popular-science magazines and undergraduate textbooks, whose readers often have short attention spans and need to be tempted to persevere with difficult ideas. But attention spans ought not to be a problem for Christiansen and Chater's intended readership of professional academics, and the trouble with boxes is that they avoid the need for an author to identify precisely how the content is claimed to fit into a wider line of argument. The advertising industry has a maxim "Sell the sizzle, not the steak", and boxes are an unwelcome application of this maxim to the genre of academic monograph.

There are many small inaccuracies and oddities in Christiansen and Chater's exposition. The German who is sometimes seen as the earliest corpus linguist was F.W. Kaeding, not "J. Kading" (as Christiansen and Chater repeatedly write). The oddity of the question *How many animals of each kind did Moses take on the Ark?* has nothing to do with lexical semantics, it is a matter of reference rather than sense (the Ark was built by Noah, not Moses). One of Christiansen's psycholinguistic experiments involved strings of letters composed of trigrams varying in frequency, and the authors illustrate this by offering *x p l n c r n g l* as an example of a high-frequency trigram string: yet according to a standard electronic English dictionary, three of its seven trigrams (*p l n, l n c, r n g*) have zero frequency. Points like these may be fairly trivial in themselves, but carelessness in small things is bound to raise questions about the reliability of the treatment of larger scholarly issues.

In sum: this book contains a number of individual insights about language and the language-acquisition process which seem valid and valuable. But, if Christiansen and Chater hope that their book will serve as the Saint George who finally vanquishes the dragon of generative linguistics, I fear they will be disappointed.

References

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