

What Principles & Parameters Got Wrong*

Cedric Boeckx, ICREA/UAB
Cedric.Boeckx@uab.cat

August 2010—*Submitted draft*

1 In critical condition

The present essay is based on a presentation I made at the *Linguistic Variation in the Minimalist Framework* workshop. Quite appropriately, the workshop, which focused on the character of linguistic variation and the role of parameters in the context of linguistic minimalism, took place in the Sant Pau hospital complex in

*Thanks to Josep Maria Brucart and Carme Picallo for organizing, and inviting me to, one of the most stimulating workshops that I ever attended, on which the present paper is based. Apart from minor elaboration and reorganization of the material, I have not deviated from what I presented at the workshop referred to in the text as the “Barcelona meeting”. I have tried as much as possible to incorporate the questions and reactions I got during the event. Where appropriate, I have also addressed issues raised by other presenters, or re-emphasize some of the points I raised which I felt had not been adequately addressed by others. The present work is a book chapter, not a treatise. For the latter, I refer the reader to Boeckx (In progress). Some of the issues raised here were originally voiced in presentations dating back to 2004 and now documented in Boeckx (2008b, 2009b, In pressa). I remain grateful to the organizers of these venues for the opportunity they offered me to begin to reflect on parameters and the nature of linguistic variation. I am also extremely grateful to Fritz Newmeyer and Guillermo Lorenzo for writing very thoughtful reviews of Boeckx (2006) (see Newmeyer (2008), Lorenzo (2007)), where some of my needlessly conservative statements were correctly criticized. I am indebted to Bridget Samuels, Carlos Rubio, and Adriana Fasanella-Seligrat for comments, and (in the case of Adriana) a superb transcribing job following the workshop. Last, but not least, I want to thank the participants at the Barcelona meeting, especially Ian Roberts and Anders Holmberg for engaging with the issues I raised. The present work is supported by a Marie Curie International Reintegration Grant from the European Union (PIRG-GA-2009-256413), research funds from the Universitat Autònoma de Barcelona Vice-Rector for Research, as well as grants from the Spanish Ministry of Science and Innovation (FFI-2010-20634; PI: Boeckx), and from the Generalitat de Catalunya (Grant 2009SGR1079 to the Centre de Lingüística Teòrica).

Barcelona—a distinguished, UNESCO-protected, setting for a distinguished patient: the Principles-and-Parameters model. Judging from textbooks, the Principles-and-Parameters model counts as the standard model or consensus view within generative grammar. It is customarily said to be the model that solved “Plato’s problem” (the logical problem of language acquisition), and made minimalist explorations possible. Yet, I contend that some of the most deeply-embedded tenets of the Principles-and-Parameters approach, and in particular the idea of Parameter, have outlived their usefulness. I claim (for reasons to be developed below) that if one takes minimalism and biolinguistics seriously, one should abandon the notion of Parameter, the more so given its diminishing empirical validity (on the latter, see especially Newmeyer (2005)).

Please note that I am here referring to the substantive notion of Parameter (hence the upper-case *P*), a notion introduced into linguistic theory by Noam Chomsky (see Chomsky (1980, 1981)).¹ I should note right away that I am aware of the existence of a much watered-down notion of parameter (lower-case *p*), which I think is currently used massively in the relevant literature to provide artificial life support to the Principles-and-Parameters model. This notion of parameter is not what I am focusing on here, as it is clearly devoid of any theoretical teeth, hence for me does not even begin to exist in a theoretical context. But, of course, if one wishes to use the term ‘parameter’ as a synonym for ‘difference’, then who am I to deny that there are “parameters” between John’s English and Koji’s Japanese? But when one does use ‘parameter’ thus, one should explicitly recognize that ‘parameter’ is “nothing but jargon for language-particular rule” (Newmeyer (2005, 53) . I take it that advocates of Principles-and-Parameters model are trying (or at any rate, should try) to advocate something stronger (and more interesting), though (but see section 2).

I am also aware that many colleagues that share my ‘Chomskyan’ persuasion think that by claiming that there are no parameters, I am throwing the baby with

¹Rizzi (1978), often given as the source of the notion ‘parameter,’ credits Chomsky for the suggestion. For what is perhaps the earliest mention of the term ‘parameter’ in the generative literature, see Chomsky (1977, 175).

Even if conditions are language- or rule-particular, there are limits to the possible diversity of grammar. Thus, such conditions can be regarded as parameters that have to be fixed (for the language, or for particular rules, in the worst case), in language learning. . . . It has often been supposed that conditions on application of rules must be quite general, even universal, to be significant, but that need not be the case if establishing a “parametric” condition permits us to reduce substantially the class of possible rules.

It is interesting to observe, in the context of what follows in the text, that Chomsky talks about rules.

the bathwater, that without parameters we are going back to the days of Skinner, or Tomasello, or Joos, that I forget that Principles-and-Parameters is an open and flexible program, that it is so much more superior to the rule-based approaches that preceded it, that no one is *that* kind of parameter-advocate any more (referring to the classical notion of Parameter articulated in Chomsky (1981)), and so on, and so on.

My overall reaction to these objections is that no, I am not trying to belittle the achievements of research on “comparative syntax”. I agree that the Principles-and-Parameters model was a very significant move in the field. Contrary to what preceded it, the Parameter-based model has the (scientific) merit of being wrong. And yes, I am aware that the Principles-and-Parameters model is a broad framework that can be stretched in many different directions, but—much like Fodor and Piattelli-Palmarini (2010) pointed out recently in a different, though not altogether dissimilar context²—there is a point at which even stretchable material breaks.

As for the idea that no one entertains any more the idea of Parameter I am criticizing, I beg to differ. For one thing, I do not know of many other notions of Parameters—at least, if one insists (as I think one should) on a substantive, non-vacuous notion of Parameter. As for the fear of Skinner’s ghost, I think it can be safely put to rest. Yes, I will put more emphasis on environmental factors when I sketch my alternative (section 4), suggesting that we ignore insights from Piaget to Tomosallo at our own peril, but from there to say that I am giving up on Chomsky and buying into Skinner is too much of a stretch. I will argue in favor of a very lean (and invariant) Universal Grammar. But I will not reject UG completely. Finally, to those who think that by discarding the notion of Parameter, I am reviving the specter of infinite variation (the notorious Joos statement that “languages can differ without limit as to either extent or direction” so often cited by Chomsky), let me point out a few things:

(i) It is not at all clear that the idea of actual infinite variation was ever entertained even by scholars of Joos’s persuasion (see Biberauer (2008, Introduction) for relevant discussion);

(ii) Even a minimal amount of syntactic invariance suffices to avoid infinite variation;

²The focus of Fodor and Piattelli-Palmarini’s (2010) critical examination of natural selection and Darwinism is not altogether dissimilar from the current one because like them, I am questioning on a consensus view that appears to be quite successful and, in fact, looks like ‘the only game in town.’ Like them, I also want to show that the notion at the heart of this consensus view does not have the causal/explanatory power it is claimed, or assumed, to have. Finally, like them, I also think that the proponents of the view I am criticizing are prone to mischaracterize my critique.

(iii) It is not at all clear that the exponential growth of parameters that syntacticians are willing to entertain is so much better a situation for the learner than a model without parameters at all;

(iv) I am reminded of an observation once made by Tony Kroch to the effect that after first denying the existence of error in language acquisition (an obvious rhetorical move), we have come to acknowledge the existence of errors, and treat them as a rich source of information. We should do the same in the context of rule-learning. It is interesting to note that after receiving much opposition during talks I gave where I deny the existence of Parameters, many an advocate of Parameters came to me and confessed that they also felt the number of parameters had gotten out of hand, that when it exceeded 30, they became suspicious, that they felt the term is used as a taxonomic device only, etc. I am reminded of an opening passage in Gould (1977): “I have had the same most curious experience more than twenty times: A colleague takes me aside, make sure that no one is looking, check for bugging devices, and admits in markedly lowered voice: “You know, just between you, me and this wall, I think there really is something to it after all.” The clothing of disrepute is diaphanous before any good naturalist’s experience. I feel like the honest little boy before the naked emperor.”

There is no denying that there is something wrong (deeply wrong) with Parameters. Not logically, of course. The beautiful simplicity of the logic of Parameters (well captured in Baker (2001)) was in fact what I suspect persuaded many linguists to adopt it. As I will indicate below, the problem is not logical, but *bio*-logical.³ This may come as a surprise to some readers, as the idea of parameter clearly originated from Chomsky’s familiarity with work in biology (an inspiration acknowledge by Chomsky on various occasions; see, e.g., Chomsky (1980), Berwick and Chomsky (In press)). But it is important to bear in mind that the regulatory networks that are now part and parcel of biology (molecular Evo-Devo) are used to account for differences across species, not within species. The linguistic variation we are talking about is intra-specific. So, when I say below that biology tells us it is high time we rethink the role of Parameters, I appeal here to biolinguistics—biology relativized to the language organ. Moreover, theoretically (in the context of the minimalist program), the notion of Parameter so obviously does not fit. Even at the empirical level (the weakest in my opinion, in terms of persuasion), cracks have started to show. The empirical challenge mounted by Newmeyer (2005)—the now obvious break-downs of all alleged macro-parameters—has not been met (Roberts and Holmberg (2005) should be given credit for trying, but the response did little to alleviate Newmeyer’s

³In this I strongly disagree with Narita (In press), where Fukui’s work on macroparameters is said to not only meet “biological adequacy” but is even said to be preferable on biological grounds.

doubts (see Newmeyer (2006)), and recent statements like the following should make us pause:

As for parameters, things are perhaps even worse. I cannot get into this topic here, but I believe that the notion “parameter” has hardly been developed beyond the traditional observation that there are “differences” among languages, like with respect to pro-drop or the order of head and complement. In short, the interesting principles were mostly discovered before Minimalism and the notion “parameter” has always remained underdeveloped from a theoretical point of view. (Koster (2010))

It is interesting to note in this context that after observing that very few linguists have taken the time to lay down a few guidelines for what counts as a Parameter, and after trying to offer such guidelines (“definitions”), Smith and Law (2009) conclude on a grim note (confirming the suspicion of Newmeyer (2005), but also of others (Culicover (1999))): “The preceding discussion implies that many of the parameters postulated in the literature are, by our criteria, accidents rather than reflecting genuine, but not exceptionless, generalizations”. Smith and Law are far from explicit about which of the parameters postulated in the literature remain as genuine parameters by their standard. They only mention *pro*-drop and head-directionality, but those are precisely the ‘parameters’ that began their theoretical lives as *bona fide* (macro-)parameters, only to see their scope diminish to the level of micro-parameters and possibly item-specific rules. If these are the standing parameters Smith and Law have in mind, it is conceivable that there are even fewer parameters than they think—perhaps as few as zero.

The clearest indication of the problematic trajectory of the Principles-and-Parameters model is to be found in the gap between theoretical work and language acquisition studies. Recall that the *raison d’être* of the model is “Plato’s problem,” the logical problem of language acquisition. However, even a quick survey of the current literature on parameters reveals that it is increasingly used as a tool to investigate what, following Fasanella-Seligat (2009), we may call “Greenberg’s problem” (consider Baker (2010), Baker and McCloskey (2007)).⁴ Principles and Parameters have

⁴At the Barcelona meeting, Ian Roberts clearly illustrated this typological tendency, when he said “So, for example looking at the head parameter again, we know that if we phrase it in maximally category-neutral terms, then it just doesn’t work. There are too many disharmonic languages around. And if you phrase it in maximally microparametric terms, maybe restating it for each category in each language, or even for each lexical item in each language, we just don’t make any crosslinguistic predictions.” This is correct, but it again points to focus on Greenberg’s problem. The focus should be on I-languages.

been pressed into typological service.⁵ But as Newmeyer (2005) correctly stresses, the model is one that was designed to answer what is the set of possible languages, not what is the set of probably languages (on this point, see also Hale and Reiss (2008)). In and of itself, the typological extension would not be so problematic if it weren't for the fact that as work on formal typology increased, productive work linking theoretical constructs with acquisition data is in a state of free fall. Part of the reason for this rests, I believe, with theoretical linguists, whose works fail to make new testable acquisitional predictions, hence the disengagement on the part of acquisition specialists.

At the workshop on which this paper is based, Luigi Rizzi expressed his disagreement with me on this point, adding that it is the field of language acquisition as a whole that is once again dominated by empiricist biases. Perhaps Rizzi is right, but I think it's too easy to move all the blame away from the theoretical linguists. By blackboxing development, as the standard Principles-and-Parameters model undeniably does (see Longa and Lorenzo (2008)), it is certainly hard for acquisitions to find something to work with.⁶ For example, Wexler's (1998) proposal, which takes

⁵Luigi Rizzi points out the existence of significant results arrived by focusing on typology, such as (in his view) Cinque (2005), and much of the work on cartography. Without getting into the adequacy of Cinque's account (see Abels and Neeleman (2006)), or the cartography project as a whole (see Boeckx (2008a, chap.4); Boeckx (2010b), Rubio (2010), Fortuny (2008) for relevant discussion), I note that Cinque's conclusion does not bear on patterns of variation (Parameters), but on patterns of *non*-variation (Universals). As such, they do not diminish my feeling that talk of Parameters is appropriate in a typological context.

In the context of typology, let me remind the reader of Odden's correct remark that "It is misguided to attribute every accidentally true statement about human language to UG, for doing so trivializes the theory of UG itself." (Odden, 1988, 461)

⁶After the Barcelona meeting, I came across the following passage, from Yang (2010), whose opinion converges with mine:

There was a time when parameters featured in child language as prominently as in comparative studies. Nina Hyams's (1986) ground-breaking work was the first major effort to directly apply the parameter theory of variation to the problem of acquisition. In recent years, however, parameters have been relegated to the background. The retreat is predictable when broad claims are made that children and adults share the identical grammatical system (Pinker 1984) or that linguistic parameters are set very early (Wexler 1998). Even if we accepted these broad assertions, a responsible account of acquisition would still require the articulation of a learning process: a child born in Beijing will acquire a different grammatical system or parameter setting from a child born in New York City, and it would be nice to know how that happens. Unfortunately, influential models of parameter setting (e.g. Gibson and Wexler 1994, but see Sakas and Fodor 2001) have failed to deliver formal results (Berwick and Niyogi 1996), and it has been difficult to bridge the empirical gap between child language and specific parameter settings in the UG space (Bloom 1993; Valian 1991; Wang et al. 1992; Yang

many parameters to be set before experimental work could detect the parameter setting process has the net effect of alienating the acquisition community. True, Rizzi (2006) discusses examples of late parameter setting, and non-uniformity between child language and adult language. Tellingly, he ends up appealing to performance and maturational factors to account for the late setting of the relevant parameters, which illustrates perfectly the point that I want to make: on its own, the structure of parametric theory is silent on the acquisition/developmental process.

It is interesting to note that this very state of affairs was anticipated back in the early 1980s, when the notion of Parameter setting and its attendant switchboard metaphor (Chomsky (1986)) emerged. Lasnik (2002) points out the following:

...in the very very early 1980s, maybe 1980 or '81, when Noam in his class was laying out the theory in relation to the question of language acquisition and there was a lot of discussion in the class about how the big problem was why language acquisition is so rapid, given that language is such a complicated thing — but as the theory was laid out it occurred to me: Jeez, we've almost reached the point where the question should be turned around. So I raised my hand and said: "Don't we have a new question, now — Why is language acquisition so slow?" ... Why doesn't it take six minutes? Interestingly, at that same era when the theory of parameters began to be very popular, there was a lot of work in theory and acquisition and learnability. Parameters was just the breakthrough we had been waiting for. Its been observed all around the world that kids go through discrete stages independent of the language, etc. That's an interesting fact we have to explain and the theory of parameters is designed to explain that. But I never completely believed that at the time and I still don't completely believe it. If the theory of parameters explains stages, those stages shouldn't last more than a couple of minutes each. There's gotta be something else that explains stages.

2002). The explanation of child language, which does differ from adult language, falls upon either performance limitations or discontinuities in the grammatical system, both of which presumably mature with age and general cognitive development—not thanks to parameters.

The failure (typical of the breakdown of macroparameters reviewed in Newmeyer (2005)) of what is perhaps the major attempt in recent years to find converging evidence from cross-linguistic and acquisition data, Snyder's (1995, 2001, 2002) predictions regarding the "Compounding Parameter" (see, e.g., Son (2006), Boeckx (In progress)), is another blow to the Principles-and-Parameters model.

What Lasnik is stressing is that the burden of the acquisition problem falls on something other than Parameters. At the very least, it shows that Parameters are not sufficient to “solve” Plato’s problem. You need to appeal to (non-grammar-based) “strategies” (see, e.g., Yang (2004), Pearl (2007)). The discussion below will suggest that Parameters may not even be necessary. But before making that point I want to stress how inappropriate the Principles and Parameters approach is when applied to Greenberg’s problem. Because of the very nature of the problem (distribution of grammatical systems that are, of necessity, full of historical residues and arbitrary properties), one is led to entertain incoherent notions such as the existence of a “High Analyticity” parameter (Huang (2005)) (a continuous notion disguised as a discrete state), or, when not all expected consequences of a parameter hold in a particular grammatical system, one is led to untenable conclusions such as “th[is] language is in flux” (Bošković (2009)). Grammatical systems may be highly analytic or in flux, but only in the E-language sense, not in the I-language sense with which Parameters are necessarily associated.

As I pointed out in passing, it is no surprise that such incoherent notions are entertained, due to the fact that what typologists describe are not proper objects of biolinguistic inquiry. In the words of Chomsky (1995, Introduction, note 11),

Thus, what we call “English” or “French” or “Spanish” and so on, even under idealizations to idiolects in homogeneous speech communities, reflect the Norman conquest, proximity to Germanic areas, a Basque substratum, and other factors that cannot be regarded as properties of the language faculty. Pursuing the obvious reasoning, it is hard to imagine that the properties of the language faculty — a real object of the natural world — are instantiated in any observed system. Similar assumptions are taken for granted in the study of organisms generally.

As a result, Parameter-based typological inquiry (especially those of the macro-parameter type) fall into the same problems that plagued most claims about holistic types from the 19th century and the pre-Greenbergian 20th century: “they have not been substantiated and have fallen into oblivion” [Haspelmath (2008)]. As Otero (1976) pointed out almost 40 years ago, “[i]t hardly needs to be added that these archetypes are nowhere to be found.” So, why look for them through Parameter-lenses?⁷

⁷At the Barcelona meeting, Ian Roberts pointed out that a line in Chinese really looks different from a line in Mohawk. True, but nothing ought to follow from this at the level of abstraction that one is used to in generative linguistics. Perhaps the problem is Mark Baker’s (1999) confidently stated assumption that “languages [do not] differ only in relatively superficial ways”, that they are

In addition to this undesirable move towards typology, work on Parameters suffers from the disappearance of principles caused by the advent of linguistic minimalism. The dramatic reduction of principles has been pushed to the limit in recent years, with the recognition that movement is just another instance of Merge. This leaves virtually no room for Parameters, in the classical sense of the term. Recall that Parameters in Chomsky (1981) were not independent from Principles. Contrary to what the name ‘Principles-and-Parameters’ may suggest, it is not the case that some condition can be a Principle or a Parameter in that model: Parameters are principles (more precisely, principles with a choice point to be fixed embedded in them). If Principles disappear, Parameters can’t be maintained. Although theoretical linguists have been slow at recognizing this (or at least, slow at making this explicit),⁸ researchers in language acquisition have had it very clear, hence their “maximalist” (as opposed to “minimalist”), top-down (as opposed to “bottom-up”) theoretical assumptions (well illustrated in Longa and Lorenzo (2008), Lorenzo and Longa (2009)).⁹ Incidentally, the same maximalist assumptions appear necessary in the works of Mark Baker, who seeks to maintain the idea that “[t]here are some parameters within the statements of the general principles that shape natural language syntax” (Baker, 2008).

The clash between Parameters and the minimalist drive is well captured in the following quote from van Riemsdijk (2008, 243f.):

One of the main problems that we now face is the question of how the actual repercussions of such highly general principles of physical/biological organization in the grammar of specific languages can be insightfully represented. ...It would be absurd to propose that the constraint[s] [them]sel[ves] [are] parametrized.^[10]

more like “Swiss watches than piles of sand.” See Boeckx (In progress) for detailed discussion of this choice of metaphor, and why it leads us astray.

⁸When I began to reflect on this issue, I could only find the following statement by Eduardo Raposo: “There are no real objects called “parameters” in UG. This in no way implies that the search for the systematic ways in which languages vary [notice again the typological as opposed to acquisitional concern — CB] has no place in linguistics. It just means that the search is lexical in nature.” (Raposo (2002)). More recently, Hornstein (2009) has also expressed skepticism towards the notion of parameter in a minimalist context. For relevant discussion, see also Richards (2008, 2010), Samuels (2010).

⁹None of the parameters illustrating the logic of Principles-and-Parameters (the switchboard metaphor with cascading effects) are embedded in principles that have retained currency (cf. bounding nodes, theta-criterion). Other illustrations of this Parametric logic still await a technical formulation (many examples in Bakers hierarchy, notions like analyticity, Compounding, etc.).

¹⁰Do we really want to say, as Baker and Collins (2006) do, that general economy principles like Attract Closest are choice points for language learners—CB?

Koster (2010) is right in saying that “the notion “parameter” has always remained underdeveloped from a theoretical point of view.” With the advent of the minimalist program, I claim that it is impossible to entertain a theoretically sound, substantive, contentful notion of Parameter. (I examine below the claims to the contrary that were expressed at the Barcelona meeting by Luigi Rizzi, Anders Holmberg, Ian Roberts, and Ángel Gallego).

Of course, it may well be the case that the minimalist trend towards approaching UG from below is on the wrong track. There may well be many principles with room in them for parameters, but recent trends in biology gives us reason to doubt this possibility. As I discuss in Boeckx (In progress,I), the revival of embryology (“generative biology”) under the rubric of “Evo-Devo”, with its emphasis on developmental and phenotypic plasticity, epigenetics, and the emergence of theories such as niche construction, stresses organismic processes as opposed to genetic blueprints, interactions (the interactome as opposed to the genome; the triple helix as opposed to the double helix),¹¹ as opposed to programs. As such it seems tailor-made for minimalist explorations, especially once these discard lexical blueprints or programs (i.e., numerations, parameter-hierarchies, pre-formed functional sequences, etc.), and truly explore interface-based explanations. Much like the emerging expanded synthesis in biology, linguistics will have to embrace pluralism, get rid of isolationist (i.e., modular, self-sufficient) tendencies, and revisit the works of old foes to treat them as friends. Like the modern synthesis did, the classical Principles-and-Parameters model blackboxed development, and dreamt of a single-level, reductionist theory to capture the generation of variation. Much like what happened in linguistics, biologists were in part attempting to exorcize the ghosts of Lamarck. Linguists were attempting to minimize if not Skinnerian, at least Piagetian tendencies. But biology (and, I contend, linguistics) is now mature enough to accommodate some of the insights of alternative visions without any existentialist dilemma. Much like modern biology, modern linguistics will have to soften its stance of various issues,¹² especially

¹¹Here linguists have to be particularly careful and appreciate the interactionism implied by terms like “triple helix.” The three strands identified by Lewontin correspond fairly closely to the three factors in Chomsky (2005), but linguists seem to have the unfortunate tendency to view these three factors as separate (or at least separatable from one another)—for example, when they ask if a given process is a third factor principle, or when they ask—as Ángel Gallego did at the Barcelona meeting; see also Gallego (In press)—which factor is the source of variation. Biologists like Lewontin are right to stress that the explanation lies in how *all* the factors interact with one another.

¹²If they don’t, they are doomed to face what may well be called ‘Piattelli-Palmarini’s dilemma’. As I pointed out in Boeckx (2006, 2010a), when Piattelli-Palmarini (1989) (rightly) cast doubt on adaptationist scenarios in the context of language evolution, there was no alternative, given the rich-UG model he assumed. The non-adaptationist alternative invoking laws of form didn’t look

those touching on specificity and innateness (Massimo Piattelli-Palmarini would talk about this in terms of leaving behind the (necessary) age of specificity; see Piattelli-Palmarini (2010)). The range of processes explored are likely to be more abstract (less-task-dependent) and generic, nothing like the Parameters of old.

As Yang (2010, 1160) points out, “one needs to be mindful of the limited structural modification that would have been plausible under the extremely brief history of *Homo sapiens* evolution.” In Hornstein’s words, “[t]he short time scale suggests that the linguistic specificity of FL as envisaged by GB must be a mirage. (Hornstein, 2009, 4).

These passages express well the point of view at the heart of Hauser, Chomsky, and Fitch (2002), who, more than anything, want to draw attention to the richness of the Faculty of Language in the Broad Sense, and the many difficulties of assuming a high degree of linguistic specificity (a rich Faculty of Language in the Narrow Sense). This is the point where (as Jackendoff and Pinker correctly pointed out) the Hauser-Chomsky-and-Fitch vision meets minimalism, which takes a deflationist stance on Universal Grammar (“approaching it from below”). To repeat, minimalism may well be wrong, but (unlike the Principles-and-Parameters model) is at least on firmer biological ground. Given what we already know about the biological foundations of language, and what we can reasonably anticipate from future developments in biology, there won’t be any explanatory room, or causal role for Parameters. True, both minimalism and the new biology (“Evo-Devo”) are works in progress (programs not theories), and, as Yogi Berra reminded us, it’s hard to make predictions, especially about the future, but right now, Parameters go against the grain in both cases, and the model in which the notion of Parameter is based is out of step with both movements. As a result, a linguist sensitive to biolinguistics should be suspicious of Parameters.

To sum up this section, I have argued that one finds four instances of worrisome disconnects in the context the Parametric model:¹³

(i) A serious discrepancy between the rhetoric of success (“having solved Plato’s problem, . . .”) and the empirical results to show for it;

(ii) A growing distance between theoretical work on Parameters and the use of the latter in acquisition studies;

too promising either. How could very general laws of form yield the degree of specificity that the UG model he assumed was made of?. It took minimalism (and the extended synthesis in biology) to help us bridge this gap, and make the laws of form conjecture plausible.

¹³Especially worrisome are the theoretical/conceptual disconnects. The empirical problems, though interesting, are always very weak on their own. As Darwin once remarked (in a letter to Henry Fawcett), “How odd is it that anyone should not see that all observation must be for or against some view if it is to be of any service.”

(iii) A clash between the minimalist elimination of principles and the maintenance of parametrized principles;

(iv) An unquestionable feeling of lagging behind in light of new directions in biology regarding the origin of diversity and the underlying specificity of traits

2 A last-gasp attempt: “*Le paramètre est mort; vive le paramètre!*”

In light of the range of problems for Parameters raised by F. Newmeyer, myself, and others, a few generative syntacticians have decided to rise to the challenge and defend the notion. Anders Holmberg and Ian Roberts have done just that, but, as I will show in this section, I remain unconvinced, in part because they end up endorsing a definition of parameter which I used in Boeckx (2009b) to demonstrate the absence of Parameters. As far as I can see, they agree with me in many respects, but fail to draw the obvious conclusion: that Parameters don’t exist.

Many syntacticians now realize that a minimalist, bottom-up approach to Universal Grammar doesn’t fit well with the classical notion of Parameter (with upper case *P* — the only notion worth its theoretical salt, in my opinion), but either implicitly or explicitly, they have adopted a slogan suggested to me by Henk van Riemsdijk (personal communication): “*Le paramètre est mort; vive le paramètre!*” This is a version of the formula used in France to mark the unbroken chain of command, the continuity of the monarchy following the death of the king (“*Le roi est mort; vive le roi!*”). Paraphrasing Henk, we may say that ‘the GB parameter is dead, long live the minimalist parameter!’

The problem with this view (which I am not ascribing to Henk) is that it is not clear what a minimalist notion of a parameter could be. As I discussed in Boeckx (2009b), minimalist inquiry points to the idea that Parameters don’t exist; they are not real theoretical objects, they are epiphenomena. Points of variation (lower-case *parameter*, if you want) arise where properties of the biologically-determined initial state of the language faculty remains silent, and where systems with which the language faculty (in the narrow sense) interacts forces a choice to be made.¹⁴ For example, taking linear order to be determined outside narrow syntax (following

¹⁴Smith and Law (2009, 340) points out that such epiphenomena may have been genetically assimilated, making them real parameters. Although I take genetic assimilation (or accommodation) to be real, I seriously doubt that genes accommodate epigenetic effects of this specificity. For the non-specificity of genetic coding, especially in the context of complex cognitive traits like language, see Benítez-Burraco (2009), Lorenzo and Longa (2003). To the extent that one can speak of assimilation in the case of linguistic variation, it takes place at the phenotypic level, where assimilation

Chomsky (1995)), I suggested we may think of the effect of a micro-parametric, head-specific head-parameter as arising from the fact that Merge produces an unordered pair which must be linearized one way or another to satisfy demands ultimately due to the linear requirement imposed by the physics of speech (or externalization more generally). Holmberg and Roberts seem to adopt this point of view when they say, as they did at the Barcelona meeting (see also Holmberg and Roberts (2009, Introduction)), that “P&P theory [i.e., the notion of Parameter] is compatible with current minimalist theorizing, once parameters are seen as effects of the absence of UG specification, but where the range of variation allowed is nevertheless constrained (often by extralinguistic factors).” Whereas they continue to defend the notion of parameter after endorsing this view, I reject it. The reason I do so is that as Holmberg and Robert themselves acknowledged at the Barcelona meeting, once this underspecification view is adopted, “the notion of parameter is almost empty; it really doesn’t have much content.” Well, if it doesn’t have much content, if it’s almost empty, why do we maintain its existence?

Let me repeat that if by parameter we simply mean difference (within a limited range), then everyone (even linguists of a non-Chomskyan persuasion) would be willing to recognize the existence of parameters. But P&P theory, and the notion of Parameter in particular, would be far away from the alleged breakthrough it was. (Try to impose the reading where ‘parameter’ is almost empty onto Chomsky (1981), Baker (2001) and you will see what I mean.) I tend to agree with Baker (2005) and Luigi Rizzi (in Boeckx (2009b), Rizzi (2009)) that the only notion of parameter worth fighting for is one that treats variation in terms of overspecification within UG, not underspecification—precisely the notion that clashes with the “Approaching UG from Below” movement and with the new biology.

Elsewhere, Holmberg (2009) has again defended the notion of ‘parameter’ in terms of underspecification, saying that “[a] parameter is not a principle plus something, its a principle minus something.” The problem is that “a principle minus something” is just a *façon de parler*, not a Parameter (*qua* parametrized principle), at least in

is simply another term for learning (see West-Eberhard (2003)).

Smith and Law also question the reasoning that led me to claim that parameters are epiphenomena on grounds that “the physical necessity for linearization may be the ultimate cause of the parameter but the skew distribution of the world’s languages and the consistency of head direction within a language suggest that the parameter does exist: The physical constraint has led to grammaticalization [genetic assimilation] of the parameter.” I disagree. Notice, first of all, the typological concern again. But setting this aside, Smith and Law’s argument is factually incorrect. They still seem to assume that parameters like head-directionality have a macro-parameter, language-consistent profile. That this is not the case was one of the motivations behind Kayne (1994). I thought this much could be taken for granted by now.

a minimalist context, where principles are (in the best-case scenario) generic processes or laws. Minimalist principles are completely divorced from differences, they do not contain ‘minuses’. The minuses arise at the meta level, when linguists look at how these principles interact with the rest of the mind. Not being language-specific, their formulation cannot contain language-specific vocabulary by means of which the ‘minuses’ could be defined. The correct conclusion to draw from the statement that parameters are not principles plus something is that parameters aren’t, period. Their fate is that of the passive and other constructions in Chomsky (1981): taxonomic devices that are not genuine properties of the language organ.

In addition to endorsing an underspecification view, Holmberg (2009) defends the notion of Parameter by pointing out that, contrary to claims in Newmeyer (2005) (see also Boeckx (In progress, Part III)) one can find empirical effects of parameters of the sort that motivated the whole parametric approach: ‘octopus’ or cascade effects that were intended to show how parameters facilitates the acquisition task (“macroparameter”). Remember the following passages from Chomsky (1981)

If these parameters are embedded in a theory of UG that is sufficiently rich in structure, then the languages that are determined by fixing their values one way or another will appear to be quite diverse (...); yet at the same time, limited evidence, just sufficient to fix the parameters of UG, will determine a grammar that may be very intricate and will in general lack grounding in experience in the sense of an inductive basis.” (p. 4)
“[...] there are certain complexes of properties typical of particular types of language; such collections of properties should be explained in terms of the choice of parameters in one or another subsystem. In a tightly integrated theory with fairly rich internal structure, change in a single parameter may have complex effects, Ideally, we hope to find that complexes of properties ... are reducible to a single parameter, fixed in one or another way. For analogous considerations concerning language change, see Lightfoot 1979.” (p. 6)

Holmberg (2009) discusses contrasting data from Mainland Scandinavian and Insular Scandinavian (as well as parallel data from Finnish) to show how differences in properties like Stylistic Fronting, Quirky subjects, and the like can be made to follow from agreement properties (along the lines originally argued for in Holmberg and Platzack (1995); see also Ott (2009)). However, upon closer scrutiny, this kind of empirical evidence does not militate in favor of maintaining ‘parameters’ as interesting theoretical constructs. The reason for this is that for a given grammatical property to have

collateral effects does not speak directly to Plato’s problem.¹⁵ The reason cascade effects were seen as evidence in favor of the Principles-and-Parameters in the early days of the model (when such effects seemed much more numerous than they turned out to be; cf. Newmeyer (2005), Boeckx (In progress)) is that they were effects for which it was hard to imagine what kind of evidence the child could use to learn them from the available data. If these effects could be made to follow automatically from other properties of the grammar for which the child could use the available data as evidence, the acquisition task was dramatically simplified. The lack of *that-t*-effects in *pro*-drop languages discussed in Rizzi (1982) was just such an effect. (Unfortunately, this particular prediction, as so many others with the same profile, turned out to be empirically incorrect; see Newmeyer (2005), Nicolis (2008), Rizzi and Shlonsky (2007).) As I stressed above, Parameters were intended to be used in the context of Plato’s problem, not in the context of Greenberg’s problem. The mere fact of finding cascade ‘effects’ is not an argument for the existence of a parameter if these effects could be learned by the child acquiring the language using primary linguistic data.¹⁶ I do not have time to go through Holmberg’s evidence here, but I suspect most of the effects he discussed are reasonably salient in the data available to the child, and as such could be learned even in the absence of a parametric structure. Once again, I cannot stress enough that typological concerns should take a backseat in a generative context. As a final illustration of this methodological imperative, let me mention the existence of “fundamental *syntactic* (and semantic) difference between English and Serbo-Croatian” that led Bošković (2008) to postulate a parameter according to which language may or may not make syntactic use of a D-layer in nominal structures (if they don’t, nominal structures are NPs). Bošković shows that assuming this difference leads to significant generalizations of the following sort (Bošković (2010) list many more):

- Only languages without articles may allow left-branch extraction of the sort il-

¹⁵I remember discussing this point with Juan Uriagereka many years ago. I am glad he voiced concerns similar to mine in Lohndal and Uriagereka (In press).

¹⁶I note here in passing that I am not at all convinced that the specific effects discussed by Holmberg, which were repeated at the Barcelona meeting, really are that different from one another (a very real possibility, as Holmberg himself acknowledged during the meeting). It is always possible to make numerous cascade effects emerge if one cuts the theoretical vocabulary of constructions very thinly: e.g., instead of Quirky (i.e., non-nominative) subjects, one could speak of Genitive Subjects, Dative Subjects, and Accusative Subjects, thereby making three effects emerge where there is only one. Kayne (2005) seems to make the same point when he writes “It has occasionally been thought that the term ‘parameter’ itself should only be used when there is such a notable or ‘dramatic’ range of effects. I will not, however, pursue that way of thinking here. In part that is because what seems ‘dramatic’ depends on expectations that may themselves be somewhat arbitrary.”

lustrated here by means of Serbo-Croatian *lijepa je on vidio djevojke* “beautiful he saw [*t* girls]”

- Only languages without articles may allow adjunct extraction from NPs
- Only languages without articles may allow scrambling
- Only languages with articles may allow clitic doubling

Notice the important modal auxiliary “may” in all of these statements. As Bošković discusses, some languages lacking an overt definite article (hence, *prima facie* qualifying for an NP-, as opposed to a DP-status) do not necessarily allow for the options opened to them. What this means is that these options are sub-‘parameters’ to be set by the child. But now notice that the DP-/NP-parameter does no work for this particular instance of Plato’s problem: if the child does not know whether her language will allow left branch extraction even once it has set the DP/NP-parameter appropriately, she will have to look for evidence in the primary linguistic data to find out. Given the paucity of examples of adjunct extraction from NPs, for example, one can see that the NP/DP macro-parameter is of little help. Once again, Plato’s problem got confused with Greenberg’s problem. Bošković (2008) explicitly commits what I like to call the typological fallacy when he writes in the context of the generalizations he has identified: “My main argument for a fundamental difference in the structure of [NPs] in languages with and those without articles concerns a number of generalizations where articles play a crucial role . . . The generalizations could turn out to be strong tendencies, which would still call for an explanation.” It is true that an explanation is called for, but why should it be an explanation *in terms of parameters*?¹⁷

3 How did we get there? The root of the problem

In this section I would like to turn to the reason why the notion of Parameter has retained currency despite all the problems it has faced (for a long time), and show that once this reason is identified, ‘parameter’ becomes an even more dubious notion, biologically speaking. The reason I have in mind is the belief that the well-documented shift from Parameter as specification on principle to Parameter as

¹⁷I agree with Smith and Law (2009) that assuming that all differences must be treated in parametric terms—as Kayne (2005) does when he writes “I will consequently freely use the term ‘parameter’ to characterize all cross-linguistic syntactic differences”—renders the notion of parameter completely vacuous.

lexical specification nonetheless allowed us to retain a restrictive notion of Parameter. This belief was well expressed by Luigi Rizzi at the Barcelona meeting (see also Rizzi (2009)), who took the shift (known as the ‘Borer-Chomsky’ conjecture) to have been “significant”. Although it changed the locus of variation, according to Rizzi, it maintained “a version of the switchboard model”.

The Borer-Chomsky conjecture is standardly formulated as follows (from Baker (2008)):

All parameters of variation are attributable to differences in features of particular items (e.g. the functional heads) in the lexicon.

In the words of the authors of the conjecture themselves:

Parametric variation is restricted to the lexicon, and insofar as syntactic computation is concerned, to a narrow category of morphological properties, primarily inflectional. (Chomsky, 2001, 2)

The availability of variation [is restricted] to the possibilities which are offered by one single component: the inflectional component. (Borer, 1984, 3)

Not only did this shift to lexical parameters solve problems that became obvious shortly after Chomsky (1981) (reviewed by Rizzi at the Barcelona meeting), it was seen as a step forward in the direction of solving Plato’s problem, as “[a]ssociating parameter values with lexical entries reduces them to the one part of a language which clearly must be learned anyway: the lexicon.” (Borer, 1984, 29)

In my opinion, the problem was indeed right there: what we wanted to understand (the nature of variation) was relegated to the part of the language organ that we understand the least: the lexicon. Consider Rizzi’s proposal (made at the Barcelona meeting; see also Rizzi (2009)):

- (1) A parameter is an instruction for a certain syntactic action expressed as a feature on a lexical item and made operative when the lexical item enters syntax as a head.

Rizzi takes this statement to be a model of simplicity and restrictiveness, but I beg to differ. It would be, if we had an idea of what counts as a possible feature on a lexical item, and what is a head. Notions like lexical feature and head are very common in syntactic discourse, but from a minimalist/biolinguistic perspective, they remain poorly understood. Kayne (2005) recognizes this, when he writes “What this brings out is something that I think has always been implicit in the proposal that parameters are restricted to features of functional elements, namely that the features

in question must be simple and limited in type, in some sense to be made precise. ... Reaching an adequate characterization of what it means to be an appropriate feature in this sense is one of the primary challenges faced by (comparative) syntax.” In other words, in the absence of a restrictive theory of what a lexical entry is, the Borer-Chomsky conjecture brings us no closer to understanding the nature of linguistic diversity.

Very few attempts have been made at coming up with such a theory of lexical entries. Adger (2010) (alongside Adger and Svenonius (In press)) is the most explicit discussion of the nature of lexical entries (/pre-syntactic feature bundles) within minimalism that I know of, and, as I argue in Boeckx (In progress), Adger’s discussion and specific proposal shows how far we are from a restrictive, biolinguistically plausible theory of lexical entries. Adger opens his paper with a (biolinguistic) challenge. Its goal is to “explore the consequences of the idea that structure embedding in human language is only ever syntactic (that is, that there is a single engine for the generation of structure and the engine is the syntax” (an idea familiar in recent works by Marantz, and Borer, and directly related to the proposal in Hauser, Chomsky, and Fitch (2002))”. As Adger correctly observes “if structure embedding is only syntactic, then the feature structures that are the basic atoms of syntax (i.e., lexical items) cannot involve embedding of one feature inside another.” In so doing, Adger notes, this minimalist approach “contrasts rather starkly with work in other approaches which take lexical items to have rich featural structure” (all feature-unification frameworks, such as HPSG, and LFG). I think Adger is exactly right in his desire to restrict embedding to the domain of syntax (i.e., pre-syntactic lexical entries should be completely flat), but as readers familiar with his paper know, Adger is forced to propose a fair amount of embedding inside his minimalist lexical entries. As far as I can see, *all* proposals concerning the format of parameters within the Chomsky-Borer conjecture have to do so as well: they have to encode parametric properties as features of features, which implies embedding.

As I have discussed in a series of publications (Boeckx (2009c, In press, I, 2010c, d, In progress)), the reason why pre-syntactic embedding is unavoidable in current syntactic models is because *all* syntactic frameworks (not only minimalist models) suffer from what I have called ‘lexicocentrism’—the view that “derivations are driven by morphological [i.e., featural] properties to which syntactic variation of languages is restricted.” (Chomsky, 1993, 44). This is in fact the view enshrined in most minimalist textbooks (see, e.g., Adger (2003), Hornstein et al. (2006)), the view that lies behind such notions as “Last Resort” and “triggered Merge”, and that makes it possible to claim that “labels can be eliminated” (Collins (2002)), that “syntax is crash-proof” (Frampton and Gutmann (2002)), etc.. This is the view that Rizzi’s

format for lexical parameters relies on. A similar view was endorsed by Holmberg and Roberts at the Barcelona meeting, who take “parameters to correspond to the things that are let open by UG, such as the distribution of formal features.” This is also the view advocated by Ángel Gallego at the same meeting, for it is necessary for him to be able to entertain the idea that there is variation pre-syntactically. Gallego claims that this sort of variation is “restricted to (i) Selection of the set of features for a given language L from the set of features made available by UG; and (ii) the way these features are assembled to create lexical items of L.” But what is “assembling”, exactly? Well, we are not told. The only reasonable answer within minimalism is Merge, meaning that the assembling takes place in narrow syntax, not pre-syntactically; meaning that the (‘parametric’) consequences of this assembling will only be detected post-syntactically.¹⁸

This effectively means that if as Rizzi, Holmberg, Roberts and Gallego claim, variation arises solely as a result of how features are assembled (which features are found on a given lexical item), this result will, of necessity, be felt post-syntactically. We are thus led to the assertion in Boeckx (In pressa) that narrow syntax is invariant (symmetric to variation). Not only don’t we find good examples of syntactic parameters (Newmeyer (2005), Boeckx (In progress), among others), there cannot be any syntactic parameters if we adopt the minimalist idea that principles are not parametrizable (due to their natural law-like character) and the biolinguistically motivated ban on pre-syntactic embedding.

It should now be clear to the reader that the problem with the attempts to make the Borer-Chomsky conjecture precise is just one aspect of a bigger problem: the heavy dependence on the lexicon, and what counts as a possible feature/lexical entry. This is a problem that plagues the cartographic approach, and indeed minimalism as a whole. It is too easy to invent features and “bundle them” pre-syntactically because once in the pre-syntactic lexicon, they are taken for granted. What is needed is, as I argue in Boeckx (In progress), a full-blown adoption of Borer’s exoskeletal

¹⁸This confinement of variation to post-syntactic components is one of the ways in which the model advocated here differs from the nano-syntactic model (Starke (2010)). Although nano-syntacticians also dramatically reduce the size of pre-syntactic lexical entries, they allow for syntactic compounds to re-enter (feed back into) the syntactic derivation and therefore influence subsequent stages. Another difference, which became clear following a question raised to Michal Starke by Luigi Rizzi at the Barcelona meeting, is that at least some instances of movement (of the ‘long-distance’ kind) are assumed to be feature-driven in this model. This is because the nano-features used in the model have projecting semantic properties (crucial for the establishment of the functional sequence, the version of cartography assumed in nano-syntax), unlike in the model I defend. This makes nano-syntax a model much closer to Generative Semantics than what is claimed by its proponents.

model (a more radical model than even Borer was willing to explore), one that leads to the idea that all lexical entries are alike: all lexical items are flat, consisting of a single property (the property that makes them mergeable). With such a lexicon, Merge becomes free (what I like to call a “Merge α ” model) and becomes crucial in every grammatical explanation. Defeating lexicocentrism means endorsing a super version of syntactocentrism, where Parameters are but one of the casualties.

Ian Roberts pointed out at the Barcelona meeting that “to exclude parameters from narrow syntax would be to impose a condition on this part of the grammar which has neither conceptual motivation, nor, as far as we are aware, any empirical motivation.” The above discussion makes it clear that my claim that narrow syntax is immune to variation is not an extra condition imposed; it follows from a truly minimalist view on what narrow syntax is. Although good evidence for syntactic parameters is hard to find (in fact, I think, it is completely lacking), it is always been hard, for me at least, to see what would make this true. That is, why shouldn’t narrow syntax be subject to variation? As long as narrow syntax is lexically determined (feature-driven), it is indeed impossible to exclude variation from narrow syntax other than by fiat (as Roberts notes). But as soon as we adopt a truly exoskeletal approach to the lexicon, as soon as we allow ourselves to construct (i.e., explain) properties of the lexicon, we make it theoretically impossible to state ‘parameters’ at the level of narrow syntax. The only option left would be to parametrize the basic processes like Merge and Transfer, which I take to be ruled out a priori. What we end up with is a situation reminiscent of what Kayne (1994) achieved: by formulating a more restrictive theory of syntax, one excludes patterns of variation. To borrow a line from Kayne’s presentation at the Barcelona meeting, “some properties of the language faculty are too deeply built in to be possible loci of variation.” The minimalist program, allied to biolinguistic desiderata, suggests that the whole of narrow syntax is just too deeply built in to be a locus of variation.

Let me add two more comments regarding the absence of variation within narrow syntax.

Elsewhere (Roberts (2010)), Ian Roberts suggests that reducing everything to PF-variation is a wrong move, as “we expect PF parameters to be symmetrical, in the sense that the entire logical space of variation ought to be filled, and attested in the world’s languages.” I see no reason to adopt this point of view, and every to adopt the opposite. There is massive evidence that PF conditions are far more asymmetric in their effects than narrow syntax processes. If Merge is free and symmetric, the evidence of gaps are likely to be the result of non-syntactic factors (witness Blevins (2004)).

On numerous occasions Richard Kayne has told me that my claim that syntactic

variation does not exist, but morphological, or morphophonological variation does seem to depend on an seemingly arbitrary definition of the term ‘syntax.’ Kayne takes recent trends such as Hale and Keyser (1993, 2002) and Halle and Marantz (1993) to be attempts to reduce morphology to syntax. But I disagree. I do not think it is a trivial matter of terminology, it’s an issue of level of representation and explanation. This is a point that Hale and Keyser were well aware of, and which they addressed explicitly in the context of their “l-syntax” vs. ”s-syntax” distinction. Here is what they wrote:

We have proposed that argument structure is a syntax, but we have also separated it from s-syntax, . . . probably an onerous distinction, perhaps nothing more than a temporary terminological convenience. [BUT] We must nevertheless assume that there is *something* lexical about any verbal/lexical entry. . . . What is it that is lexical about the entry corresponding to *shelve*? Clearly, it is a lexical fact that *shelve* exists as a simple transitive verb in English. . . . in reality all verbs are to some extent phrasal idioms, that is, syntactic structures that must be learned as the conventional “names” for various dynamic events.

In effect, Hale and Keyser are pointing out that their “l-syntax” is a syntax in the representational sense (a post-syntax, a morphology, in my terminology), whereas “s-syntax” is a syntax in the dynamic, derivational sense (narrow syntax, for me). Confusing the two would be like confusing genetics and epigenetics. One is static, the other one dynamic. One relies on the other, but they are not to be collapsed. Hence, it is important to be clear about which level is subject to variation. That is to say, it is important to distinguish between the (I am claiming, invariant) mechanism that produces the variation (narrow/s-syntax) and the selection step that corresponds to l-syntax/morphology.

4 An alternative, in light of, and in line with the new biology

At this point it is worth reflecting on what an alternative approach to Plato’s problem might look like. In this section I will only be able to make a few general remarks, and hint at a few research directions (see Boeckx (In progress) for development).

It seems to me that the first thing to be clear about is that one should resist the temptation to take the Principles-and-Parameters model to be the only game in town. (Neo-Darwinians made the same mistake with natural selection; cf. Fodor

and Piattelli-Palmarini (2010), Pigliucci and Müller (2010)). The major mistake made by the Principles-and-Parameters model, much like the one made by the neo-Darwinians, was to rely on metaphors (cf. the switchboard metaphor, the artificial selection imagery, etc.) more than on actual causal-mechanistic processes. It was also deeply wrong to think that a single-level theory (a single mechanism) would be sufficient. As the new biology is telling us, a more pluralistic, multi-factorial, interactionist approach is needed. Blevins (2004) has begun to do it in the context of phonology. I think this sort of approach should be pursued. Instead of putting genes in the driver’s seat, biologists like West-Eberhard (2003) (see also the various contributions in Pigliucci and Müller (2010)) are now urging everyone to take genes to be followers, not leaders, stabilizing structural options, rather than generating these. We should adopt the same perspective in linguistics. As I suggest in Boeckx (In progress), the neo-constructionist, realizational, post-syntactic PF-models that are becoming more and more influential should be used to view lexical features not as leaders, but as followers, as stabilizing, rather than dictating the construction of structural options.

The emerging picture is one where at least as far as the post-syntactic grammatical component is concerned, we are dealing with a variety of “constructions” in something like the notion of construction in Construction Grammar, although crucially for me, these constructions are constructed by a narrow syntactic component that looks nothing like a Construction Grammar.¹⁹ The post-syntactic constructions will be stabilized, selected (l-syntactic/morphological) forms, and will invariably be language-specific,²⁰ and often morpheme-specific. Sometimes, they will even be lexical-item-specific (word-islands in Tomasello’s sense). We’ll thus find idioms of varying lexical specificity, as Jackendoff (2005, 2010) correctly points out. (The mistake is to confuse this with the syntactic engine that constructs all of these.) The varying degree of specificity could, and in my view should, be related to the debate about micro- vs. macro-parameters.

In Boeckx (In pressa) I suggested that points of underspecification (in the sense also used by Holmberg and Roberts cited above) would indeed be very local effects (I called them “nano-parameters” to make this clear), but, once fixed (as local rules) could ‘grow’ into macro-parametric effects if coupled with what I called a Superset Bias, a learning strategy (an economy guideline for memory) that would seek to retain the same direction of stabilization in subsequent learning acts, unless there is too

¹⁹Construction Grammar takes Constructions as idiomatic, non-decomposable templates, which is clearly non-explanatory.

²⁰They may even be sociolect-specific, which would allow us to incorporate some of the insights made in their respective presentations by David Adger and Sjeff Barbiers at the Barcelona meeting.

much evidence against it in the primary linguistic data (this sort of epigenetic bias is frequent in nature; cf. West-Eberhard (2003); it also makes a lot of computational sense, see Mobbs (2008)). Independently, Holmberg and Roberts (2009) formulate a similar idea (going back to Roberts (2007)), which they call a Markedness convention. According to them, “there is a preference for a given feature of a functional head to generalize to other functional heads.” Details of implementation aside, the Markedness Convention and the Superset Bias have the potential effect of capturing typological tendencies (the focus of Greenberg’s problem). Because it’s only a bias, it predicts exceptions, and is this better equipped than macro-parametric approaches of the classical type.²¹

Through this learning bias (and, no doubt, other non-language-specific, “third-factor” principles) it may be possible to *construct*²² something like a ‘parametric’ structure like Baker’s (2001) Parameter Hierarchy, as opposed to assuming that it is part of our biological endowment (an instance of the classic debate between epigenetics vs. preformationism). But I want to stress that the resulting ‘parametric’ structure, built inductively,²³ is likely to be more web-like than tree-like. It will be more like a subway map than a vertical hierarchy of the sort Baker (2001) anticipated. It will be reminiscent of networks explored in complex systems. Due to the numerous intersections it will contain, it is likely to be a factor in explaining why language acquisition takes time (cf. Lasnik’s dilemma discussed above), and why acquisitionists have found evidence of parallel learning paths (cf. the works of Stephen Crain and Rozz Thornston discussed in Boeckx (2009a, chap. 6)).

Although Holmberg and Roberts adopt a similar idea with their Markedness convention (correctly pointing out that with it there is no need to formulate a theoretical difference between micro- and macro-parametric variation), they are wrong in using expressions like “parameters become more micro” over time (the micro-/macro-options are entertained in parallel at the point of selection/learning). They are also wrong in taking their Markedness Convention to help construct top-down

²¹Contra Newmeyer (2005) I do not think that processing biases will be sufficient to account for typological generalizations, although they too may play a role.

²²Another case of neo-constructionism/exoskeletality in linguistic analysis. As I stress in Boeckx (In progress), if UG is to be approached from below, all hierarchies from the cartographic representations/function sequences to Baker’s Parameter Hierarchy, must be constructed. As Epstein and Seely (2006, 7) correctly point out, “if you have not grown [/constructed] it, you have not explained it.”

²³In work in progress I am exploring the possibility to arriving at such a structure using a hierarchical Bayesian model of the sort proposed by Kemp et al. (2007), which I think makes sense given the success of linguistically well-informed Bayesian learning in the context of language acquisition (Yang (2002, 2004, 2010), Pearl (2007)).

decision trees (networks, which they liken to epigenetic landscapes) that mirror the child’s learning path. I would advocate a more bottom-up strategy for constructing the decision tree, in line with the new biology’s recognition that epigenetic landscapes, useful as they are, are static representations that must also be constructed from the ground up, lest they miss the actual dynamical process that has true causal power (see again West-Eberhard (2003)). Until we do so, we cannot claim that we are describing the actual learning path taken by the child.

I would like to conclude this section by pointing out that the emerging research program to address Plato’s problem is actually much closer to Hagit Borer’s original vision, a vision obscured by the blanket statement of the “Chomsky-Borer Conjecture”. It is true that Borer (1984) wrote that “the availability of variation [is restricted] to the possibilities which are offered by one single component: the inflectional component.” But I don’t think she meant this in the way that was explored subsequently in Ouhalla (1991), Webelhuth (1992), Fukui (2006), which Chomsky made standard (“Parametric variation is restricted to the lexicon, and insofar as syntactic computation is concerned, to a narrow category of morphological properties, primarily inflectional”). As the following passage (much richer than the portion of it that is usually quoted “Associating parameter values with lexical entries reduces them to the one part of a language which clearly must be learned anyway: the lexicon”) reveals, Borer was talking about learning (constructing) rules. ((Like Yogi Berra, Borer could say that she never said half of the things she said.)

The inventory of inflectional rules and of grammatical formatives is idiosyncratic and learned on the basis of input data. If all interlanguage variation is attributable to that system, the burden of learning is placed exactly on that component of grammar for which there is strong evidence of learning: the vocabulary and its idiosyncratic properties. We no longer have to assume that the data to which the child is exposed bear directly on universal principles, nor do we have to assume that the child actively selects between competing grammatical systems. (Borer, 1984, 29)

By saying that “We no longer have to assume that the data to which the child is exposed bear directly on universal principles, nor do we have to assume that the child actively selects between competing grammatical systems”, I think Borer was essentially saying that by divorcing variation from syntactic principles, we no longer need a parametric theory to support language acquisition. This is the view I have expressed in this essay.

Note again that this return to rules is not a return to the dark ages of unconstrained variation. After all, the parameter format proposed by Holmberg and

Roberts (2009), like the parameter schemata of Longobardi (2005), are much like rule formats in Chomsky and Halle (1968).²⁴

5 Conclusion: Why this is all good news and real progress

It is to be expected in a new field like (bio-)linguistics that conceptual change happens fairly rapidly. Linguists who have grown with the Principles-and-Parameters model have gradually updated their thinking (say, from parametrized principles to lexical parameters), but without necessarily paying attention to the fact that in so doing, they have stepped well outside of the original boundaries of the model. In and of itself, this is not a bad thing, so long as explanatory demands continue to be met. Unfortunately, in the case of parameters, this is not the case. Greenberg’s problem has too frequently replaced Plato’s problem, and the notion of Parameter (upper-case *p*) has lost its explanatory, causal role, to the point of being replaced by a misleading homonym, parameter (lower-case *p*), a fancy term for difference. As we saw, when one attempts to formulate a notion of parameter that is consistent with minimalist/biolinguistic demands, one is forced to conclude that “the notion of parameter is almost empty; it really doesn’t have much content” (Holmberg/Roberts). What a pyrrhic victory for defenders of the Principles-and-Parameters model!

However, I have been at pains to show that the demise of the concept of Parameter is very good news for minimalists. It’s a move in the right direction (in the direction of better integration with the rest of biolinguistics). It is sometimes said that Minimalism “led to relatively few new insights in our understanding of phenomena in the first half of the nineties. This is probably because it did not generate new analytical tools, and thus failed to generate novel ways of looking at well-known paradigms or expand and solve old problems, an essential ingredient for progress to be made at this point” (Koopman (2000)). I completely disagree with this statement, and believe that the gradual move away from Parameters indicates great progress. Minimalism—with its emphasis on movement as (internal) merge, post-syntactic morphology, and so on—has made it possible to convert once-syntactic

²⁴It is interesting to note that the format for parameters put forth by Holmberg and Roberts at the Barcelona meeting— $Q(\text{ff} \in C) [P(\text{f})]$ (for some quantification *Q* over a set of features *FF* included in the set of categories *C*, some predicate *P* defined by the theory of grammar like “is a label of”, “agrees”, “attracts” holds of this set) —does not contain any explicit choice point, unlike the parameters of old. It’s really a rule/construction format; an idiomatic template à la Hale and Keyser (1993). It is in fact the very same schema argued for by Reiss (2003) and Samuels (2009) for the formulation of phonological rules.

parameters into post-syntactic construction schemata. That is to say, minimalism has contributed to showing that the classical Principles-and-Parameters vision was wrong; the character of linguistic variation is not infinite, but it is not Parametric either. Let me repeat that this is not a return to Skinner and Joos, much like the new biology is not a return to Lamarck. As the debate between Newmeyer and Holmberg/Roberts made clear (Newmeyer (2005), Roberts and Holmberg (2005), Newmeyer (2006)), statements like “Place the Verb before its Object” could be seen as either a rule (Newmeyer) or a parameter value. Minimalism (of the “Merge α kind) gives us what Kayne (1994) rightly characterized as the “all too infrequent pleasure of seeing the theory choose the analysis.”

The recognition that Parameters do not survive the move to beyond explanatory adequacy is really good news. As Frisch (1999, 600) notes,

For the traditional formalist, it is actually desirable for some linguistic patterns, especially those that are gradient, to be explained by [other] principles. The remainder (...) might be a simpler, cleaner, and more accurate picture of the nature of the innate language faculty and its role in delimiting the set of possible human languages

Newmeyer (2005, 96) writes that statement once made by Pierre Pica to the effect that there are no (macro)parameters “is a cause for disappointment, not rejoicing.” I disagree. It is a cause for rejoicing in light of the attempt to approach UG from below. Once UG is seen to be much more underspecified than we thought, the very existence of variation receives a straightforward rationale: there is variation precisely because the genome does not fix all the details of Universal Grammar. There is in fact so much underspecification that the explosion of parameters we have witnessed in the past 20 years is exactly what we expect. A Minimalist view of language makes variation inevitable.

Yang and Roeper (In press) write that “Minimalism has not supplemented the basic architecture of P&P for the task for language acquisition”. But this statement (and similar ones, like my claim that parametric variation is a matter of virtual conceptual necessity in Boeckx (2006)) can only be maintained at the descriptive level,²⁵ that is, if we understand parameter as devoid of causal effect. At what Marr (1982) would call the algorithmic level, Parameters are nowhere to be found. It now remains to work out the pluralistic alternative I have hinted at in the preceding page,

²⁵A similar conclusion holds for the very remark Massimo Piattelli-Palmarini and I made in Boeckx and Piattelli-Palmarini (2005, 453n.3), where we said that “a parametric model of language acquisition is “logically” necessary.”

or some other, and to rewrite our history of the field,²⁶ bearing in mind the words of the late Tony Judt, who said that “[t]he historian’s task is not to disrupt for the sake of it, but it is to tell what is almost always an uncomfortable story and explain why the discomfort is part of the truth we need to live well and live properly. . . . A well-organized society is one in which we know the truth about ourselves collectively, not one in which we tell pleasant lies about ourselves.”

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²⁶Amundson (2005) has done exactly this in the context of the alleged explanatory success of the Modern Synthesis in biology.

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