Reply to the Reviews of *Case: Its Principles and Parameters*[1]

Mark C. Baker

I want to begin this reply by thanking the three reviewers of my book (B), Haspelmath (H), Malchukov (M), and Stiebels (S), for paying careful attention to it, and for the kind things that they find to say about it, despite significant differences in our basic approaches and perspectives. I am glad that they each found some interest or inspiration in it—although I realize that that may be as much a credit to them as readers as it is to me as a researcher and writer. When each of them reviews my basic claims and proposals, they do so accurately, and there is little need for me to set the record straight at that level. I also welcome these as attempts at having some substantive interchange about matters of general linguistic significance across various theoretical approaches, including some that span the formalist-functionalist divide. I did not intend my book as a direct contribution to dialog between formalists and functionalists (which is why H does not find an explicit statement of my theoretical goals, but has to try to reconstruct them), but simply as a good and fairly accessible example of formal work on a topic of interest to many. But now that the reviewers have brought up more explicitly some of the broader conceptual issues, I can try to advance the discussion a bit further from my perspective, within the bounds of a short reply. I appreciate the opportunity to do so, not just in the abstract, but with the reasonably specific empirical domain of morphological case marking in the foreground.

Perhaps the most substantive way for me to engage some of the central issues is by taking up in some detail the question of why I develop my analysis of the core structural cases (nominative, accusative, ergative, absolute, plus some instances of dative and genitive) in terms of syntactic phrase structure, using the structurally-defined notion of c-command. As the reviewers acknowledge, the leading idea behind what generativists call "dependent case" is available to both formalists and functionalists: it is present as much (and much earlier) in Comrie (1978, 1981) as it is in Marantz (1991), the work that I take as the direct ancestor of my approach (and thanks to H for supplying additional references in this tradition). It is also clearly present in Lexical Decomposition Grammar (LDG)'s core notions of argument roles being marked ±hr and ±lr. Many linguists can share, then, the hypothesis that some structural case marks the "more prominent" or "less prominent" of two nominals within the same linguistic domain, and that this linguistic gadget can have the obvious functional value of helping to disambiguate an expression—the so-called discriminating function of case. The central task of my book was to explore this notion thoroughly, considering how its details can be made precise, how those details might be parameterized (i.e., made precise in slightly different ways in different languages), and what range of phenomena might be usefully addressed in this way—a project of potential relevance to a wide range of linguists.

But against this shared background, the question arises as to why I implement the crucial notion of "prominence" in terms of the specific and somewhat partisan notion of c-command. H poses this question very explicitly, asking why not use semantics rather than syntactic trees, perhaps appealing to a small set of proto-roles, macro-roles, or typological role-types. S poses

the question almost as explicitly, by comparing my approach with LDG, where prominence is defined over the argument structures of individual lexical items rather than over syntactic structure. I take M to be implicitly raising a similar issue when he speaks of functional versions of the core idea being potentially simpler. Why then do I opt to develop the idea in terms of c-command defined over a syntactic tree?

One simple and obvious reason is that c-command is what other people within my research community often use to address analogous issues involving some form of prominence in the topics that they study. And that strikes me as a good reason, all things being approximately equal. I think it is healthy for researchers to strive for a degree of compatibility among their theories, and to have some accountability to one another. Trying to use the same core theoretical notions is one way to pursue those goals.

But there is also a deeper and more subtle reason why I am so fond of c-command, and I now realize that I did not discuss this explicitly in the book—nor is it widely understood outside the generative paradigm. I worked this out for myself in Baker 2001, and I revisit the main point here. What is important and distinctive about c-command (and some related notions) is the way it combines two distinct types of prominence into one unified notion. The first kind of prominence is the widely shared notion of thematic prominence: that the agent of a clause counts as more prominent than the theme and the goal, and that the goal (if expressed as an NP rather than a PP) is more prominent than the theme. The second kind of prominence is what one might call embedding prominence: it is the prominence that (say) the subject of the main clause has over the subject of an embedded clause. The fundamental insight of c-command, in my view, is that these two different-seeming types of prominence are often treated as equivalent by prominence-sensitive linguistic phenomena. To take a classic example from English (one for which c-command was originally invented; see Reinhart 1983), a pronoun cannot be understood as coreferential with a name if the pronoun is more prominent in the sentence than the name (the Disjoint Reference Condition, also known as Condition C of the Binding theory). Thus the pronominal subject in (1a) cannot be referentially dependent on the object; this contrasts with (1b),where coreference is better, especially in certain kinds of contexts.

(1)   a. She$_i$ praised Mary$_k$.   (* with i=k)
        b. Her$_i$ boss praised Mary$_k$.   (OK or ? with i=k)

However one also gets the same sort of referential restriction in a sentence like (2a), where the pronoun subject of the main clause cannot be coreferential with the subject of the embedded clause.

(2)   a. She$_i$ said that Mary$_k$ works hard.   (* with i=k)
        b. Her$_i$ boss said that Mary$_k$ works hard.   (OK or ? with i=k)

(2a) cannot be explained purely in terms thematic prominence, because both the matrix subject and the embedded subject have the same (coarse-grained) thematic role, something like (proto) agent/actor. Nevertheless, the matrix subject has prominence over the embedded subject in (2) in a way that is linguistically equivalent to the subject of a clause having prominence over the object of that clause in (1). Unlike simple thematic hierarchies, c-command captures this equivalence (in essence, by reducing thematic prominence to a form of embedding prominence, claiming that the theme-object but not the agent-subject is a constituent of the verb phrase in

(1a)). Indeed, c-command even successfully manages apparent conflicts between the two types of prominence, as in examples like (3).

(3)   a.   The inspector informed her $_i$ that Mary $_k$ works hard.   (* with i=k)
      b.   The inspector informed her $_i$ boss that Mary $_k$ works hard.   (OK or ? with i=k)

Her in (3a) has a theme semantic role, whereas *Mary* is an agent, so *her* is intrinsically lower than *Mary* in pure thematic prominence. However, *her* belongs to the matrix clause, whereas *Mary* belongs to the embedded clause, so *her* is higher than *Mary* in embedding prominence. Which is the stronger force in this case? In fact, *her* cannot be coreferential with *Mary* in (3a), showing that embedding prominence is the stronger force here. The standard notion of c-command captures this, since *her* does c-command *Mary* in (3a). With facts like this in mind, I concluded in Baker 2001 that phrase structure in contemporary Chomskian theory is an abstract ("primitive") representation of grammatical relations, including prominence relations—more comparable in this way to alternative generative theories like Relational Grammar and Lexical Functional Grammar than one might think. Part of what this abstractness means is that contemporary generative grammar does not give facts about superficial word order or surface constituency any special primacy in deciding what the true syntactic representation of the clause is; prominence-sensitive phenomena like referential dependency can play just as big a role in establishing the syntactic representation. But phrase structure understood in this way is a particularly good system for representing grammatical relations—better than unadorned thematic hierarchies, for example—because it unifies thematic prominence and embedding prominence in a way that fits how some important phenomena in (at least some) natural languages actually work. I believe that this understanding of phrase structures ("trees") as the mode of syntactic representation is not unique to me, but is present in most generative work, although I admit that we do not often clearly explain it in this way. (There is also certainly some idiosyncratic historical residue in the particular way that syntactic trees are conventionally expressed.)

   This conceptual background may shed light on some parts of my book that seem odd to readers from outside the paradigm. For example, H says that "c-command should have to do with constituents, i.e. the grouping of words", and then he is puzzled by the fact that I rarely appeal explicitly to evidence from word order and constituency, with the notable exception of my analysis of differential object marking (DOM) in Sakha. In Sakha, I do show that the case marking of the direct object (accusative or bare), the interpretation of the direct object (specific or nonspecific), and the position of the direct object (before or after an adverb) are all intertwined. Within the style of grammatical representation I use, the word order fact seems to be the driving force behind this cluster: the object in Sakha can move out of the verb phrase, to a position before the adverb, and this movement places it within range of the subject so that it gets accusative case, and also puts it in the region of the clause where it gets a specific interpretation rather than a nonspecific interpretation, according to Diesing's (1992) Mapping Hypothesis. However, I am not shy to assume that the same analysis may hold for DOM in some other languages, which have a similar case alternation and that case alternation is related to specificity in the same way (more or less), but where no word order difference between the two variants of the object has been documented. Why am I phrase structure-centric in this way? Wouldn't it better to just state the relationship between case and specificity directly, as S does within LDG, and cut out the obscure structural middleman?

One reason I do this is simply practical: Sakha is one of the few languages of this type that I have worked on directly, and I uncovered the word order effect by targeted investigation with a native speaker linguist (Nadya Vinokurova). I would not be at all surprised if similar differences in word order and constituency would come to light in other languages if people looked for them carefully, even though they are certainly not the most salient properties of the construction for descriptive purposes (that is true in Sakha too). But from a scientific perspective, there should, I think, be no a priori expectation that the primary causal factor in a cluster of interrelated phenomena is always the most saliently observable factor in the cluster.

But there is another more conceptual and methodological reason to seriously consider using the same analysis of DOM, even if no word order or constituency evidence is discovered by careful inquiry into the matter. In my mind, it is a perfectly coherent possibility that the basic grammatical representation of a clause might have direct consequences for the case marking and interpretation of NPs in that clause for some language, but not for word order—presumably because some other factors intervene. There will be *some* relationship between the tree structure of a sentence in a given language and its word order or surface constituency, but that relationship need not be trivial. Within the Chomskian framework, a confluence of case marking facts, interpretation facts, and facts from prominence-sensitive phenomena like anaphora and binding can be enough to establish the syntactic representation, even in the absence of evidence from one particular domain, (say) word order. This is also why I am comfortable assuming essentially without new evidence that agentive subjects c-command patient objects in all languages (a question raised by H). I take it as widely agreed on that agents are more prominent than themes according to a thematic hierarchy in essentially all languages—in ergative languages as well as in accusative ones—and for me the claim that agents c-command themes is essentially how that basic claim is expressed in my syntactic representation of choice, which is designed to represent thematic prominence and embedding prominence in the same format. That representation is syntactic rather than lexical in that it represents a sentence as a whole (not just a single predicate and its arguments); it is syntactic not just semantic in that the elements are arranged in a particular formally definable way within the representation, although there may of course be a natural isomorphism between the syntactic structure and its semantic interpretation (it is desirable that there would be). Familiar questions about the use of abstractness, of objectivity and replicability, come up now, no doubt, which might bear on whether this is the right way to go (see below for some discussion). But before engaging those questions, it is good to understand what claim is being made by the proponent of the theory—in this case, the theory that uses tree structure representations, with c-command as the central prominence relation.

While my use of c-command is largely motivated by my background framework, I believe that it does pay some direct dividends in the study of structural case. One striking fact about the Sakha language is that it can use accusative case not only on the direct object when it is in the presence of a subject, as in (4a), but also on the embedded subject when it is in the presence of a matrix subject. In Sakha, this is possible even when the embedded clause is fully finite, as in (4b) (see also von Heusinger et al. 2011 for similar data from Mongolian).

(4)   a.   Erel   kinige-ni   atyylas-ta.   (B: 112)
            Erel.NOM book-ACC buy-PST.3SG
            'Erel bought the book.'

b. Min ehigi-ni   bügün   kyaj-yax-xyt   dien   erem-mit-im.   (B: 114)
I   you-ACC   today   win-FUT-2SG   that   hope-PST-1SG
'I hoped you would win today.'

To my mind, this pair is interestingly parallel to the pair in (1a) and (2a): it shows again that thematic prominence (in (4a)) is fundamentally equivalent to embedding prominence (in (4b)) in the way that c-command expresses. Both kinds of prominence cause accusative case to be used on the less prominent NP (when certain locality conditions involving phases hold—conditions that are equally relevant to direct objects and embedded subjects and that may cause both to be in bare-nominative form instead). Again, a simple thematic hierarchy cannot capture this, since the two NPs in (4b) are equal on the thematic hierarchy (or perhaps the matrix subject is actually lower thematically, if it counts as an experiencer rather than an agent). Nor can a lexical approach like LDG capture this generalization directly, as I understand it, precisely because of its lexicalism: 'I' and 'you' are arguments of different predicates in (4b), so they will not be marked as +hr or +lr with respect to each other, such that 'you' gets accusative case.

S mentions that LDG, like other lexicalist frameworks, generally accounts for "exceptional case marking" (ECM) by saying that certain matrix verbs lexically select an object that they do not associate with a thematic role; therefore, there is an argument structure connection between this object and the verb's subject, even though there is not a semantic connection. That approach may be adequate for English and similar languages, where the ECM phenomenon is clearly a property of some verbs and not others. But it seems much less appropriate for Sakha or Mongolian, for two reasons. First, ECM in these languages is not lexically restricted, in that essentially every verb allows it. It is not even necessary that the matrix verb select a clausal complement, since ECM is possible in adjunct clauses as well in complement clauses, as shown in (5) (from Vinokurova 2005).

(5)   Masha [Misha-ny   kel-ie   dien] djie-ni   xomuj-da.
        Masha Misha-ACC come-FUT that house-ACC tidy-PAST.3sS
        'Masha tidied up the house (thinking) that Misha would come.'

Does one want to say, then, that 'tidy up' in Sakha lexically selects an optional nonthematic object as well as its usual thematic object—and perhaps nearly every other verb in the language does as well? This may be a coherent option within LDG's framework of assumptions, but to my mind this evidence suggests that the phenomenon is *not* lexically governed, so argument structure notions are not the right tools for understanding it—whereas c-command is. Secondly, there are syntactic arguments that the accusative NPs in sentences like (5) clearly do not take on all the properties of direct objects of the matrix clause along their accusative case (Vinokurova 2009 on Sakha; von Heusinger et al. (2011) on Mongolian). Thus, 'Misha' is not literally an object of 'tidy' in (5). Therefore, a c-command approach seems to have important advantages here over nonsyntactic and nonstructural alternatives.

S acknowledges this argument from Sakha as a possible difference-maker between my theory and LDG, along with my argument that dependent case marking applies to adverbs as well as to arguments in some languages (the issue she finds most interesting). I think that there is also a third argument that is directly relevant, which S and H do not take up in their reviews. This is my argument from applicatives of unaccusative verbs in Shipibo (recently replicated in Nez Perce by Amy Rose Deal (2016); see below), which I recap briefly here. One additional way

in which a syntactic approach using tree structures and c-command is different from a semantic approach (H) or a lexical one (S) is that there is the possibility of syntactic movement changing the prominence relations. This possibility is fairly rare, since locality conditions on movement often imply that only the highest NP in a domain can move to a still higher position, and in those cases the movement does not change the relative prominence relationships. But some cases of prominence reversal are known to exist, such as (6a) versus (6b) from English.

(6)   a. It seems to John $_i$ that he $_k$ is tall.   (i = k OK)
        b. He$_k$ seems to John$_i$ – to be tall.   (* with i = k)

In (6a), *he* is not more prominent than *John*, hence it can refer to John in accordance with the Disjoint Reference Condition. But in (6b) the highest argument *John* cannot move to the empty matrix subject position because it is embedded in a PP, so *he* moves there instead. This reverses the prominence relationship, with the result that *he* cannot refer to John in (6b).

      Does this sort of syntactic prominence reversal ever affect structural case marking? I claimed that the answer is yes, in applicatives of unaccusatives in Shipibo. I assume that applicative is a process that adds a benefactive/malefactive/goal argument to a clause that otherwise would not have had one, as in (7b), where a transitive clause has become a ditransitive clause (compare (7a)).

(7)   a. Jose-kan-ra   atapa rete -ke.
          Jose-ERG-PRT hen   kill-PRF
          'Jose killed a hen.'

        b. Jose-kan-ra   Rosa atapa   rete-xon-ke.   (B: 242)
          Jose-ERG-PRT Rosa hen   kill-APPL-PRF
          'Jose killed a hen for Rosa.'

A crucial question, then, is where this added argument fits into the clause in terms of prominence. It is widely held in the generative literature that these goal-like arguments are middle arguments, lower than the agent but higher than the theme, and this hypothesis is compatible with the available facts about (7b) in Shipibo in particular (see Baker 2014). Now compare this with a base verb which has a theme argument but no agent argument—a so-called unaccusative verb. Such verbs can undergo applicative formation productively in Shipibo, as seen in (8).[2]

(8)   a. Kokoti-ra joshin-ke.
          fruit-PRT ripen-PRF
          'The fruit ripened.'

        b. Bimi-n-ra   Rosa joshin-xon-ke.   (B: 244)
          fruit-ERG-PRT Rosa ripen-APPL-PRF
          'The fruit ripened for Rosa.'

---

[2] Note that (8b) cannot be analyzed as a causative construction, a possible instance of causative-applicative homophony. If it were, one would expect 'Rosa' to be the subject, not 'fruit'.

Now according to a simple linear thematic/semantic hierarchy, the goal should be more prominent than the theme in (8b), as it is in (7b). But this is not borne out: in fact, the theme argument behaves like the most prominent NP in the clause in (8b)—it counts as the syntactic subject as measured by the most common word order (with the theme argument first; see the examples in Valenzuela 2003: 691, 694), by the fact that the theme argument acts like the subject for purposes of plural number agreement on the verb, and by the fact that same/different subject marking references the theme argument in sentences like (8b). Therefore, I assume that something prevents the initially higher goal argument from becoming the subject in Shipibo (8b), as in English (6b) (indeed, I said that the preventing factor was basically the same, that the benefactive/goal argument is really a PP in Shipibo too, but that abstract and controversial claim is not crucial here). Therefore, the theme NP assumes the subject role ("moves into the subject position") instead, reversing the prominence between the two arguments that we otherwise would have expected by comparison with (7b). Finally, this independently detectable reversal of prominence is absolutely relevant to ergative case marking in Shipibo, which goes on the most prominent NP in the clause when there is more than one. In (7b) the ergative marked nominal is the agent, as expected by a simple linear thematic hierarchy, but in (8b) it is the theme, contrary to that hierarchy. I conclude from this that the prominence that is relevant for ergative case marking is not simple thematic/semantic prominence but rather syntactic prominence (c-command). In short, a simple semantic or lexicalist account faces a dilemma here: if the argument added by applicative formation is taken to be intrinsically higher than the theme argument, (8b) is problematic; if it is taken to be intrinsically lower than the theme argument, then (7b) is problematic. Within my syntactic framework, the tension is resolved by the independently known device of movement to the subject position. C-command matches thematic prominence in many cases, but the two can be different in a well-defined class of situations, and when there is a difference, c-command is better.

S looks over my work, comparing it to LDG, and concludes that "in my view Baker has provided very few cases that could challenge the lexical account of dependent case and would require a configurational explanation." M makes a similar claim for his theory, although he doesn't go into details in his review. I agree with this: I claim to have presented three or four main challenges to a nonsyntactic theory of case marking: accusative subjects in embedded clauses in Sakha (and Mongolian), case marked adverbs in Quechua and Diyari/Warlpiri, and ergative themes in applicatives in Shipibo (and Nez Perce).[3] That is not a large number of cases compared to the number of cases which both theories would handle in a similar way. But how many differences should we expect, between conceptually related theories designed to account for the same first-order phenomena? How many distinguishing cases should be necessary to affect what we believe? According to my (limited) understanding of the history of science, the only decisive observations that show that the earth goes around the sun rather than vice versa were the motion of Foucault's pendulum, and (only in the 20th century) very delicate observations of parallax among the fixed stars—not very much evidence! Similarly, a primary difference-maker between Newton's celestial mechanics and Einstein's was the precession of the planet Mercury. It would be hubris to equate what we are discussing here in linguistics to these all-time greats from the history of science, but it is appropriate to be inspired by them, and what they suggest to me is that we should expect the crucial differences between profoundly different

---

[3] A fourth possible difference-maker is my analysis of double nominative and double absolutive clauses in languages like Amharic, Choctaw, Japanese, and Korean, from chapter 3 of *Case*. However, that argument is more complex and rests on a less directly observable assumption (null headed PPs), so I do not review it here.

visions of the subject matter to show up only in a rather small number of second-order observations.

Moving on to more general, quasi-methodological matters, both H and M express concern that the ideas in my book start out simple and intuitive enough, but become increasingly complex as the discussion progresses. For example, M likes my basic dependent case rules, but not the way they interact with Chomskian phase theory, discerning a "huge machinery" at work. Similarly, H turns my phrase of "hideously complex" back on me. Now I think I get many positive new results from these efforts. For example, by combining dependent case marking with phase theory, one gets an analysis of one kind of DOM, and an analysis of structural dative which I think is superior to the LDG one that S mentions. LDG treats dative as the case of the middle argument out of three, whereas I treat it as the case of the higher of two arguments within a smaller domain, the verb phrase. My account has the advantage of extending to explain the use of dative case on goal arguments in passives of ditransitive verbs and on experiencer arguments of dyadic experiencer verbs. However, in other cases I admit to being a bit disappointed about the final level of complexity myself; there are parts of my account that I wish had turned out to be simpler.

But should we be surprised that things go that way? Should we take that as being a bad sign, suggesting that a theory is on the wrong track? Not necessarily: it seems to me that there is a bad complexity that is a symptom of starting off on the right foot, and a good complexity that comes from being explicit about the details. Almost every effort to make something more precise in its coverage involves making how it is expressed more complex; for example, legal language is more complex than informal talk of necessity, not just because lawyers seem to like to make things difficult for others. If one wants to be precise about where accusative case can and cannot be used in Sakha, it gets a bit complicated. Perhaps this can be avoided if one is content to talk about general tendencies for an NP in certain circumstances to get accusative case, without worrying too much about where the boundary lines fall in particular languages. Conceivably that is the best we can do (H may be suggesting this). But I think that we can and should aspire to more, that the details sometimes matter—in part for reasons involving high-level theory comparison mentioned above. Perhaps this is a basic difference between the formalist muse and the dominant functionalist muse.

My years in linguistics have convinced me many times over that human languages are complex systems of interacting constructions and constraints. If so, the best linguistic theories will want to do justice to those complexities, and trace out those interactions. I don't by any means think that this dooms linguistic theories to be "hideous" and baroque: we can hope that many of language's complexities (the ones that aren't "just" historical quirks) are complex interactions of what are intrinsically simple, even elegant principles. I suspect that in this hope I am not so different from H, M, and S, all of whom point toward theories that they think will meet this standard in one way or another: H's upstream/downstream hypothesis expressed in his (7); M's interplay of distinguishability, faithfulness/indexing, and economy; S's invocation of Optimality Theory-style constraint interaction. The difference is, perhaps, more in our instincts of where to look for these underlying principles. But even if the principles/constraints at work are intrinsically simple, a relatively full discussion of how their interactions produce the observed complexity will necessarily be complex. That is not at all a bad sign theoretically, although it may make for more effortful reading. I want to claim that, contrary to the reviews, my principles of case assignment never become that complex (although they do become somewhat

more precise). The most complex parts of the book, I think, are about how these principles of case assignment interact with other aspects of linguistic theory: with differences in basic clause structure, with derivations involving movement/displacement of nominals, with derivations that are built up cyclically in phases. But these other aspects of linguistic theory—or their equivalents—have independent motivation, and we should not be surprised that there is quite a bit to say about how the various pieces interact with one another. Anyway, that's my story, and I'm sticking to it.

There are related concerns about whether my theory is restrictive at all, about what typological space it defines, and about whether it contributes to typological explanation and an account of universals. In this connection, M notes that I have a penchant for adopting parametric proposals, such that competing hypotheses A and B are both valid for human language as a whole, but one language picks A and another picks B—perhaps pursuing a spirit of compromise. Both he and S express some preference for related contributions within the generative idiom that are more "disciplined" in this, by Bobaljik and Levin and Preminger. I acknowledge some truth to this: one of my distinctive moves within the generative community is to entertain parameters where others see only universal principles. That may not prove right, but I think that it should be tried. My justification for this is that it may be worthwhile to double the mechanisms available to linguistic theory (e.g., admitting both A and B) if doing so gives (say) a tenfold increase in the range of languages that can be brought under insightful analysis. A simplistic but useful way of thinking about the explanatory quality of a theory is in terms of its ratio of facts explained to assumptions made. In these terms, there are two strategies for improving the explanatory value of a given theory: one can try to increase its empirical coverage significantly, or one can try to reduce the number of assumptions that it makes (or both, of course). Many of my generative compeers pursue primarily the second strategy; I am committed to pursuing the first strategy, and am willing to pay a modest price in terms of the number of linguistic devices countenanced in order to do so. Broadly speaking, however, the two strategies are very much part of the same overall project of improving theoretical explanation.

And it is not true that I am quick to parameterize anything. To take one important example, my book takes the view strong that ergative case is *always* dependent case, never inherent case, as many generativists believe (M makes a small error on this point). Translated into more functionalist terms, the claim is that ergative case *always* has a discriminating function, never a pure indexing function in which it marks a subject as having an agent thematic role. The most obvious typological consequence of this restrictive claim would be that there are no true active case marking languages, in which one case marker indicates agents and another one indicates patients, regardless of the transitivity of the verb. (I realized this prediction more clearly just as I was finishing the book; see Baker (2015: 288); I have explored it and defended it more thoroughly in follow up work with Jonathan Bobaljik: see Baker and Bobaljik 2017. This work also includes discussion of the small but not null set of apparent counterexamples.) In this, I may disagree rather sharply with M, who proposes that case phenomena are always the result of an interplay of discrimination and indexing factors, even for a structural case like ergative. M criticizes my view about the tripartite language Nez Perce, where it should be unnecessary for purposes of discrimination to use both ergative case and accusative case in the same clause. This motivates him to propose that these cases in Nez Perce have an indexing function instead. But if I understand his suggestion properly, it does not fit the facts very well, particularly some described in recent work by Amy Rose Deal (2016). First, ergative case is certainly not used on

agents of intransitive verbs, nor is accusative case used on the patients of intransitive verbs (unaccusatives), as shown in (9). These cases thus cannot have a pure indexing function; transitivity must play a crucial role too (hence discrimination).

(9)   a. Kit'ic   hi-wii-qa-na.   (Deal 2016:11)
           Kit'ic.NOM   3SG-cry-HAB.PST-REM.PST
           'Kit'ic used to meow.'

       b. Haacwal   hi-lloy-ca   (Deal 2016:10)
           boy.NOM   3SG-be.happy-IPFV
           'The boy is happy.'

More subtly and strikingly, Deal (2016) replicates my Shipibo argument concerning applicatives of unaccusatives in Nez Perce. In this construction, ergative is used on theme arguments, which are not agentive:

(10)   K'olalk'olal-nim hi-'leese-nuu-ye.   (Deal 2016:13)
       bell-ERG   3SG.noise-APPL-REM.PST
       'The bell rang at me.'

So indexing does not work here, but the generative version of discrimination does. Yet another striking fact about Nez Perce (discussed in B) is that when the theme-object is a nonspecific indefinite NP, there is no accusative case on the object (a type of DOM) *and* there is no ergative case on the subject either—both arguments are in bare nominative form. This makes little sense from a pure indexing perspective. Indeed, it seems a bit perverse even from a vague/general/functionalist discrimination perspective, since one might think that having the object case marker suppressed would make having an overt subject case marker all the more important. However, it makes a lot of sense from a well-articulated formal perspective, in which ergative case and accusative case are both dependent cases, triggered by having two NPs near each other in the syntactic representation, as I discuss in Baker (2015: 129).

I see a lesson here not just for the specific issue of indexing versus discriminating factors in case marking, but for broader issues of functional and formal explanation in linguistics. From a certain distance, it makes a great deal of sense to say that the function of case marking is to distinguish the arguments in a single clause, with ergative and accusative case in Nez Perce as cases in point. But looking at the grammar of Nez Perce closer up, the simple functional intuition does not account well for the details: it is somewhat uneconomical for Nez Perce to have two case markers in one type of clause, and it is somewhat unperspicuous of it to have no case markers in another type of clause. These details make more sense (I claim) when case marking is taken to be the result of formal rules, with some rather distant functional motivation no doubt, but also with their own internal logic. Broadly speaking, case in Nez Perce has communicative value, but what happens in particular sentences is the result of formal case rules interacting with particular structural configurations in ways that do not necessarily maximize either clarity or economy for that particular sentence. (Here I agree with H's observation that "functional explanations account for universals [of one sort–MCB], not for language-particular rules.") And if the pure indexing view fails for case on subjects and objects in Nez Perce, and there are in fact no languages with a true active case system, then a big question arises as to why natural human

languages never make use of such obviously useful rules as "mark all agents with ergative" and "mark all themes as accusative." We may want a formal explanation of this, along the lines of "case rules have access to syntactic representation, but no direct access to thematic roles", because of contingent properties of the human language capacity (what my folk call "universal grammar"). I suppose that functionalists know this, at least on one level, so that they talk about a certain pattern becoming grammaticized. But then let's talk (among other things) about the specifics of how such processes work once they have been grammaticized, and of any tendencies there may be to grammaticize them in one way rather than another.

There is of course room here for different linguists to be interested in different topics; indeed, one might even hope for some healthy complementarity in this. H and M are pleased to note that I do not entirely eschew functional explanation in my book, even though I am clearly more interested in formal explanation. Both observations are true, and intentional. When working through linguistic data that puzzles me and sifting out the factors that seem to be at work, I sometimes say to myself "but of course THAT part of it has an obvious functional explanation"—and then I tend to put that part aside, downplaying it so as to concentrate on other parts of the overall puzzle. This must strike some linguists as exactly backwards; they would think that the part that is functionally explainable is the interesting and significant part, and the formal/grammatical "residue" is not. I can understand that, but then add that a formalist (me) can have a parallel sense of disappointment when a functionalist researcher concentrates on one set of factors and downplays others which the formalist considers more interesting or more important. Hopefully, as we make somewhat different choices at these points it will help the whole story to come out over time.

H's review is very interesting in this respect. First, he constructs a substantial list of universal claims about case marking that he discovered in or deduced from my book (his (4))—a very helpful contribution. Then he gives a second list, his (5), of universals of coding that my book does not discuss very explicitly, and may not explain, commenting that (5) are "some of the most famous universals of coding," and that "they are far more robustly attested than most of those in (4)." He goes on to say that "readers…who are primarily interested in the explanation of universals…are bound to be somewhat disappointed" or "not seriously challenged in their views". I think that H's juxtaposition of the two lists is quite fascinating; I plan to ponder them further, and invite others to do so too. As H partly foresees, I have disparate reactions to the various things on his list in (5). Some of them I think I have explained to a significant degree ((5c, f)); some I think are not true ((5a)); some I think are true but don't fall within my primary topic (which was the syntax of case, not its morphology, (5d)); some I just don't find very interesting ((5g), too functional); some I wish I could derive from my theory, but cannot (so far) ((5b)); and one ((5e)) refers to a phenomenon that was not present in my sample, but I now realize that I might have missed something significant here. There is clearly more I could do on the topic of case (starting perhaps with Yukaghir), and I should await H's forthcoming work on his (7) with curiosity. But at the same time, I do take some pride in the list in (4)—especially if several of the generalizations hold up under further investigation. Inasmuch as they are not the most famous universals of coding, they are also not merely rehashing familiar material; rather, I am trying to discover new patterns and derive new results as well. My book was definitely intended to be a research monograph, not a general reference work (hence the attractive blue cover from Cambridge University Press), and I think we should try to advance the frontiers of

the discipline as we can (see also S, who acknowledges that some of the topics that I take up later in the book have been under-researched from other perspectives, including LDG).

H also questions the objectiveness, testability, and replicability of my research. This type of concern is rather commonly expressed when functionalists critique formal research, but I do not see any fundamental problem here. To illustrate with one particular case that I already had cause to mention above, Deal (2016) explicitly claims to have replicated within Nez Perce one of my key claims about ergative case, which I originally based on data from applicatives derived from unaccusative verbs in Shipibo. Perhaps this kind of direct testing and replication is less common in the field than one would like, but this may have more to do with what sorts of contributions we tend to assign prestige too, rather than to the intrinsic qualities of one particular framework as compared to another. It is certainly true that I do not give any mechanical recipe for how to test and replicate my results in other languages. I would if I could, but I simply don't know enough at this point about what language-particular resources and complications might be present in other languages that affect how this testing and replication would have to be done. Consider what was actually involved in Deal's effort to replicate my result. First she found a test for distinguishing unaccusative verbs from unergative verbs in Nez Perce, namely the formation of adjectival participles. This test is well-grounded in the literature, and she sketches a theoretical basis for it, but it is entirely different from the tests I used in Shipibo. Then she found diagnostics for what is the subject and what is the object in Nez Perce. These too are plausible and well-grounded but quite different from what I used in Shipibo: agreement is relevant in both languages (although the details of the two agreement systems are very different), but switch reference marking is relevant in Shipibo, not in Nez Perce, whereas referentiality conditions on the binding of possessors and possessor raising are relevant in Nez Perce, not in Shipibo. Then once the hard work about how to recognize the relevant constructions has been done, a clear commonality appears across the two unrelated languages: in both, ergative case is used on theme subjects when there is another structurally lower argument in the clause (compare (8b) and (10)). This supports a dependent/discriminating case view, and challenges an inherent/indexing view, as discussed above.

I think that this is typical and to be expected given where we are as a field. Results can perfectly well be tested and replicated, but it takes a measure of creativity and insight to figure out how best to do so in particular languages. This cannot yet be reduced to an "objective" recipe or mechanical procedure. One could wonder from afar whether the creativity and insight into particular languages required to do this has been abused. But in practice, I doubt that anyone who looks seriously into how Deal made her case and how I made mine would deny that an "objective" (i.e. real) similarity between Shipibo and Nez Perce has been uncovered here. (And note that Deal is quite content to criticize an important part of the mechanisms of my analysis, and to propose her own; there is no spirit of mindless agreement here.)

This is also why it was hard for me to be precise about the typological space that my theory defines at this stage. The range of possible case-assigning rules that my theory countenances is clear enough, I think (although it is not out of the question that more types would come to light), but how those case rules might interact with all sorts of unanticipated structures and constructions may not be yet. Since language is a complex system of interacting structures and constraints, we have to have patience as we try to tease apart the various interacting strands, and be ready to encounter new twists as we go. I hope that my efforts to clarify what range of phenomena can and cannot naturally be handled by dependent case rules will be an important step forward toward a typological explanation of case systems in natural

human language, but similar contributions will have to be made in other interacting areas for the typological space to be fully clear. I don't think we can get there all in one go.

Finally, I might add that I for one am not especially sad about this. I will have bittersweet feelings when we do reach the point of giving something more like a formula for replicating results from one language within another language for all pairs of languages, and when the typological space has been completely worked out. It will certainly be a sign of maturity in the field, and we should be working toward that goal with zeal. But the other side of that coin is that it will mean that we are almost done with the exciting work that we are doing now, which involves uncovering many fascinating linguistic phenomena for essentially the first time, and beginning to see in detail how one topic might be interlocked with another topic. Then a great Age of Discovery in linguistics will be coming to a close, and there may be little more to do other than tidying up, at least at the level of description we have been working at so far.

References

Baker, Mark. 2001. Phrase structure as a representation of "primitive" grammatical relations. In William Davies and Stan Dubinsky (eds.), *Objects and other subjects: grammatical functions, functional categories and configurationality*, 21–52. Dordrecht: Kluwer.

Baker, Mark. 2014. On dependent ergative case (in Shipibo) and its derivation by phase. *Linguistic Inquiry* 45.341–379.

Baker, Mark. 2015. *Case: Its principles and its parameters*. Cambridge: Cambridge University Press.

Baker, Mark and Jonathan Bobaljik. 2017. On inherent and dependent theories of ergative case. In Jessica Coon, Diane Massam, and Lisa Travis (eds.), *The Oxford Handbook of Ergativity,* 111–134. New York: Oxford University Press.

Comrie, Bernard. 1978. Ergativity. In Winfred Lehmann (ed.), *Syntactic Typology*, 329-394. Austin, Texas: University of Texas Press.

Comrie, Bernard. 1981. *Language universals and linguistic typology*. Chicago: University of Chicago Press.

Deal, Amy Rose. 2016. Raising to ergative: Remarks on applicatives of unergatives. Ms. University of California Berkeley. Available at http://ling.auf.net/lingbuzz/003024

Diesing, Molly. 1992. *Indefinites*. Cambridge, Mass.: MIT Press.

Heusinger, Klaus. von, Udo Klein, and Dolgor Guntsetseg. 2011. The case of accusative embedded subjects in Mongolian. *Lingua* 121.48–59.

Marantz, Alec. 1991. Case and licensing. Paper presented at *The 8th Eastern States Conference on Linguistics*, University of Maryland, Baltimore.

Reinhart, Tanya. 1983. *Anaphora and semantic interpretation*. Chicago: University of Chicago Press.

Valenzuela, Pilar. 2003. Transitivity in Shipibo-Konibo Grammar. Eugene, Ore.: University of Oregon Ph.D. dissertation.

Vinokurova, Nadezhda. 2005. Lexical categories and argument structure: a study with reference to Sakha. Utrecht: University of Utrecht Ph.D. dissertation.

Vinokurova, Nadezhda. 2009. On raising and proleptic objects in Sakha. Unpublished ms., Rutgers University.

Mark Baker
Department of Linguistics
Rutgers University
18 Seminary Place
New Brunswick, NJ 08901
USA

mabaker@linguistics.rutgers.edu