Reflections on the Study of Language

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INTERVIEW

Reflections on the study of language.
An interview with Robert D. Van Valin, Jr.

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This article reports an interview with Robert D. Van Valin, Jr., which was held on March 2, 2023, with follow-up e-mail exchanges. Robert Van Valin is the primary developer of Role and Reference Grammar (RRG), a syntactic theory whose principles and commitments intersect with those of Cognitive Linguistics (CL). The article discusses RRG vis-à-vis CL and other approaches to the study of language. It aims to raise awareness about the shared principles of RRG and CL, to enhance cross-fertilization between the two approaches and ultimately inspire new research directions in linguistic theory. The paper is organized into three main parts: (i) background information on the birth and development of RRG, (ii) general principles and commitments of RRG and CL, and (iii) specific issues in the study of language.

Keywords: syntactic theory, the lexicon, linguistic constructions, Role and Reference Grammar, Cognitive Linguistics, Computational Linguistics

Introduction:

The opening lines of Van Valin (2005) state: "This book is concerned with how structure, meaning and communicative function interact in human languages. Language is a system of communicative social action in which grammatical structures are employed to express meaning in context" (p. 1). These statements suggest that the outlook on language taken by Role and Reference Grammar (RRG) (Bentley, Mairal-Usón, Nakamura, & Van Valin, 2023; Pavey, 2010; Van Valin, 2005, 2023a; Van Valin & LaPolla, 1997) is similar to that taken by Cognitive Linguistics (CL). Through this interview with Robert D. Van Valin, Jr., we hope to encourage cross-theoretical dialogue between different approaches to the study of language, to shed light on the points of intersection,

and the differences, between RRG and CL, and to inspire new research directions within each of these frameworks and beyond.

Interview:[1]

1. Background: the birth and development of Role and Reference Grammar

DB: You studied and taught at University of California, Berkeley, when prominent cognitive linguists such as Charles Fillmore, George Lakoff, and Len Talmy were there. Our first question is how their thinking shaped your own thinking and the development of Role and Reference Grammar. What urged you to create a new framework as opposed to working within the existing approach of Cognitive Linguistics?

VVL: Cognitive Linguistics was not actually there when I was a graduate student. Len Talmy had finished his PhD the year before I started. George Lakoff was still doing Generative Semantics when I arrived. Charles Fillmore had departed from Case Grammar and was writing about deixis. So, there was no such thing as Cognitive Linguistics. When I was in graduate school, Ron Langacker, who is another important cognitive linguist, called his approach 'Space Grammar'. So, Cognitive Grammar was yet to come.

Fillmore strongly influenced the development of Role and Reference Grammar, as I wrote with David Wilkins in the 1996 paper on agentivity "The case for 'effector'" (Van Valin & Willkins, 1996). Fillmore's framework of Case Grammar is a direct ancestor of Role and Reference Grammar, for several reasons. First, Fillmore divided up the clause into proposition and modality, which is paralleled in RRG by the distinction between the operator projection and the constituent projection (Van Valin, 2005, 2023a; Van Valin & LaPolla, 1997). In addition, in Case Grammar there were subject and object selection rules that took a semantically defined relation in the underlying form and moved it into its surface position. An idea I had as an undergraduate in Göttingen, where I was on my education abroad program, was to take that Fillmorean framework, add what we now call information structure features to the arguments in the underlying form, and have the information structure triggering the transformations. So, if you had an agent and a patient, and the agent was topical, and the patient was focal, then you would get an active voice sentence because the topical agent would be moved to subject, and the focal patient would be moved to object. However, if that was reversed and the patient was topical, and the agent was focal, you would get a passive. This idea was inspired by both Fillmore's Case Grammar (Fillmore, 1968) and Chafe's Meaning-Structure Grammar (Chafe, 1970). Therefore, Fillmore directly influenced the development of RRG, as did Chafe.

Lakoff influenced RRG through his work in Generative Semantics on lexical decomposition. What I mean is that Lakoff convinced me that lexical decomposition was the way to go. Then we used Dowty's (1979) system to realize that. So, there are influences from Lakoff and Fillmore, but they are not related to their later work in Cognitive Linguistics.

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[1] In various places, references were added to the transcript of the interview to clarify which work was referred to.

You asked why I wanted to create a new framework. Well, that's a good question. Like I said, I spent my last year as an undergraduate at the University of Göttingen in Germany-- well, West Germany at the time. The graduate students in the department did seminars on *Aspects of the Theory of Syntax* (Chomsky, 1965) and on Fillmore's theory, and I was taken with Fillmore's approach. However, it seemed imperative to me to pursue Chafe's introduction of Prague School notions into American linguistics. None of the existing theories did that at the time. We were before Conditions on Transformations (Chomsky, 1973). So, Chomsky was still arguing with Lakoff, McCawley, and Ross about Generative Semantics versus Interpretive Semantics. It seemed to me that these theories were woefully inadequate and didn't get into the pragmatic motivations for movement rules and other phenomena. So, I could say in retrospect, I rather boldly tried to put together an alternative framework. When I got to Berkeley for graduate school, no one was interested in talking to me about it, until I met Bill (William) Foley. We discovered we had a lot in common in terms of our perspectives. Thus, we decided to develop the framework in part because he was working on Austronesian languages, and I was doing Lakota in field methods, and it seemed to us that the existing theories were inadequate when applied to these languages. So, we decided to develop this framework.

DB: Although this is something you mention in Van Valin (2009), for the benefit of those who are unfamiliar with RRG, could you briefly explain where the name *Role and Reference Grammar* comes from and add a brief history of its birth?

VVL: In 1977, I was writing a paper for Chicago Linguistic Society on ergativity—I think that was the first time I used the term Role and Reference Grammar (see Van Valin, 1977). It came from a paper by Paul Schachter on Tagalog subjects. He had made his famous contribution from 1976 (Schachter, 1976) in the *Subject and Topic* book, which introduced Tagalog to the larger community, and in 1977, he published a paper called 'Reference-related and role-related properties of subjects' (Schachter, 1977). Well, RRG started out as a theory of grammatical relations—the first papers are all about grammatical relations and why they are not universal. That's, by the way, another similarity with Fillmore's Case Grammar. In 'The Case for Case' paper (Fillmore, 1968), he talks about how subjects and objects are not universal—a position he later abandoned, I think.

So, I needed a name for the theory, and even though the way we divided up role properties and referential properties was different from Schachter, it seemed a reasonable name for what we were trying to do, which was to come up with the universal theory of grammatical relations. This captured the idea that grammatical relations are grammaticalizations of semantic role properties, on the one hand, and discourse pragmatic properties, on the other, and that these grammaticalizations could vary from language to language, explaining cross-linguistic differences. Therefore, calling it 'Role and Reference Grammar' seemed to fit rather nicely. When we expanded the theory to become a general theory of syntax, that was no longer so obvious. Michael Silverstein told me I should have changed the name with the 1997 book (Van Valin & LaPolla, 1997), because that was an opportunity to call it something else. But I don't know what we would have called it.

DB: My third point is about the importance of cross-theoretical fertilization or the importance of learning from other frameworks. You have told me personally that you think we should talk more to people who have other approaches to the study of language. I think it is worth

stating your views on this matter in this interview, and why it is important to you. Maybe you can think of a good example of why it is important, apart from the obvious fact that we should not all operate in our silos if what we are interested in is the study of languages and language as opposed to the study of a particular approach to language.

VVL: When I was a graduate student, the linguistic wars were raging. I was an undergraduate at UC, San Diego, and I had an introduction to linguistics from Ron Langacker, who was a generative semanticist. I also talked to people at the University of Texas as a possible place to go to graduate school, and Lee Baker, who had been a fellow graduate student in Illinois with Langacker. When I talked to him and told him I was going back to San Diego, he said with a smile, «Well, tell Ron he's wrong», and Ron said «Well, next time you see Lee, tell him he's wrong», also with a smile. It was a friendly exchange, unlike most at that time. There was nothing but cross-theoretical argumentation because you had Lakoff, Fillmore, McCawley, and Ross, arguing mostly against Chomsky. Ray Jackendoff was an important figure on the interpretive semantics side. And then in the early 1970s, Chomsky wrote *Conditions on Transformations*, which was published in 1973. He did not mention McCawley, Lakoff or Fillmore. And cross-theoretical argumentation just stopped. Chomsky's practice has been to only cite people and argue with people within his generative framework, philosophers and psychologists excepted. I tried to continue to foster this cross-theoretical debate. I published a squib in *Linguistic Inquiry* (Van Valin, 1986). I wrote articles that argued directly against Government and Binding Theory (Van Valin, 1985, 1987). I did a paper in 2003 called 'Minimalism and Explanation', and I tried to make an argument for RRG versus Minimalism (see Van Valin, 2003). I used to go around saying RRG was the original minimalist program because it allowed no transformations or movement, no traces or copies, etc. In fact, I gave a talk at a conference with a group of generative linguists in the audience. When I gave my talk, none of them had a comment, even though I had made an argument against their theoretical framework. I asked one of them about this, and he replied: «Yes, it's just not possible to do cross-theoretical argumentation anymore». It is frustrating. There is cross-theoretical influence on work, but it does not show up in citations.

As an example of an attempt to foster theoretical pluralism, in my 2001 *An introduction to syntax* (Van Valin, 2001), students were expected to learn both constituent structure analysis and dependency grammar, in order to acquaint them with multiple ways of analyzing the same data, and the final chapter compares five different syntactic theories, with the goal of introducing students to the theoretical diversity that exists.

One of the things I was particularly proud about the UB program was,[2] that if a student specializes in syntax, they had to write term papers in Government and Binding Theory, or Minimalism, RRG, and HPSG, so that people got a well-rounded education. But that's rowing against the tide. In addition, virtually all students interested in syntax and semantics took Talmy's courses on cognitive semantics.

DB: I think it would be interesting to have at least one example of how ideas in RRG have inspired other frameworks.

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[2] Professor Van Valin taught at University at Buffalo (UB) (1990–2006, 2017–2020) and at Heinrich Heine University Düsseldorf (2007-2017).

VVL: This was 10 or 15 years ago. A Dutch generative linguist told me that all discussions on unaccusativity started from my paper in *Language* (see Van Valin, 1990), even if it's not cited—which it often wasn't.

There are also some weird coincidences. I taught a course on RRG at Stanford in 1985, and a graduate student, Mark Johnson, wrote the short paper in which he proposed the Projection Grammar Formalism for RRG (Johnson, 1987).[3] He went on to MIT as a postdoc, and shortly after he arrived there, they started talking about the split between the lexical projections and the grammatical projections, and the scholar who proposed that sent me a number of papers about this idea out of the blue. I never quite figured out why he sent them to me, as they did not mention Johnson or RRG, until I realized that the split Infl hypothesis correlated with Johnson being a postdoc at MIT. So, it looks like that's a possible influence of RRG on Government and Binding Theory, but we'll never know.

2. General principles and commitments of CL and RRG

2.1 Cognitive Commitment

KT: Upon providing an overview of CL, Evans (2012, p. 2) states that CL is an approach centered on two commitments. One is the Cognitive Commitment whereby "Cognitive linguists attempt to describe and model language in the light of convergent evidence from other cognitive and brain sciences", continuing: "the Cognitive Commitment asserts that the models of language proposed should reflect what is known about the human mind, rather than purely esthetic dictates such as the use of particular kinds of formalisms or economy of representation."

To what extent is the Cognitive Commitment of CL relevant to RRG?

VVL: RRG has long agreed with Simon C. Dik's notion of *psychological adequacy* and that has expanded to psycholinguistics, if you take processing, and from psycholinguistic work to neurolinguistic work. If you look in *The Cambridge Handbook of Role and Reference Grammar* (Bentley et al., 2023), Brian Nolan's paper has got a lot of computational work that is related to cognitive modeling. I've done some work on cognitive modeling and neuro work with Ina Bornkessel-Schlesewsky and Matthias Schlesewsky and they wrote a volume on neuro processing of language that assumes RRG as the grammatical model in my Oxford series (Bornkessel-Schlesewsky & Schlesewsky, 2009). Interestingly, they were critiqued by one of the reviewers for using such an obscure theory in their book. And in RRG we've also stated that we agree with Bresnan and Kaplan that theories of linguistic competence should be tied to testable or computationally implementable models of speech production. So, I would say that RRG has a cognitive commitment. This is compatible with what Evans describes.

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[3] "Mark Johnson" in the main text refers to Mark Edward Johnson, distinct from philosopher Mark Johnson, the co-author of *Metaphors We Live By*.

2.2 Generalization Commitment

KT: The other is Generalization Commitment, which "represents a dedication to characterizing general principles that apply to all aspects of human language. This goal reflects the standard commitment in science to seek the broadest generalizations, possible" (Evans, 2012, p. 3).

       Could you comment on the view of RRG on the Generalization Commitment of CL?

VVL: If we are talking about general principles to apply to all aspects of human language, you could say RRG does not achieve this, because there is no RRG phonology. We have generalizations about semantics, syntax, morphology, and information structure, but we don't have anything really on segmental phonology, although O'Connor (2008) proposed a prosodic projection for the clause in RRG. That said, RRG is committed to stating significant generalizations about language or those domains about language that it deals with. So, I would say, we agree with the generalization commitment, acknowledging that someone is going to need to come up with an RRG theory of phonology. I don't know what that would look like but, hopefully, someone will be inspired and want to try developing that part of the theory.

3. Specific issues in the study of language

3.1 Usage-based analysis

KT: Like many cognitive linguists, some RRG researchers use corpora to offer their analyses (e.g., Guerrero & Belloro, 2010). In this regard, some of the RRG analyses may be classed as usage based. Could you comment on usage-based analyses in RRG as opposed to those in CL, for instance, do linguistic structures solely derive from usage in RRG? What is the RRG notion of linguistic competence?

VVL: From my perspective, competence is communicative competence, not just linguistic or grammatical competence, but rather more generally knowledge of social rules of language use, for example, which would be part of communicative competence, but not part of grammatical or linguistic competence.

       I also think it is reasonable to draw a competence-performance distinction, because we can study the rules and principles that we know but this knowledge is different from what we do in particular instances where language is used. Therefore, the distinction between knowledge and use is not irrelevant. It's clear that people have rules and principles that they know, which can be studied independently of the use of those principles. But having said that, the data from the actual use informs our knowledge of the principles. I think it is right to say that RRG analyses can be classed as usage-based, because there are people who use texts primarily, like Jan Ullrich, who has an extensive collection of Lakota texts going back to 1820, and when you ask him about some feature in the language, he goes to his text

corpus to find whether there's evidence for the particular form or construction. And he's not alone in the RRG Community in using corpora and text.

DB: I think it is fair to say that there is this distinction between data from actual use, which is essential to inform our knowledge of the principles, but it is distinct from the principles. So, the distinction is valid, and it is important to understand how the language works by looking at how it is used.

VVL: Yes, I agree completely.

DB: A related question is why there is no sociolinguistic work in RRG.

VVL: Well, the introductory chapter to the 1984 book (Foley & Van Valin, 1984) talks about that. I guess the kind of sociolinguistics that I would be interested in seeing doesn't correlate with mainstream sociolinguistics in the U.S., which for a long time was variation theory, which I don't think has, as it was practiced, too much relevance to RRG. However, if you look at the introduction to the 1984 book and to my 1980 paper, *Meaning and Interpretation* (Van Valin, 1980), a place is put in the conceptual scheme of RRG for sociolinguistics. It has just never been picked up. Nobody has come along. No graduate student has wanted to do RRG sociolinguistics. It's like phonology. In principle, there's no exclusion of sociolinguistics. It's just a matter of interest of the people working in the theory. There wasn't any work on formalization of RRG, aside from Mark Johnson's paper and some of Brian Nolan's work for many years. It really got going with Laura Kallmeyer at Düsseldorf and colleagues there. For years I hoped that some formal linguists would get interested in RRG and develop it formally. And now that has happened. I do not agree with all their proposals, but it is great to have them working on it, giving papers at formal linguistics conferences and making clear that the theory has lots of interesting features that are relevant to the concerns of formalism, and this is formal in a real sense of "formal".

DB: What is "real" formal? Can you tell us more about this?

VVL: Formal means mathematically based, relating to computational models as opposed to saying that you have a computational model when you don't.

3.2 Meaning-centered theories

KT: For both CL and RRG, meaning plays a particularly important role. To focus on verbs, in RRG, the meaning of the verb is represented by Logical Structures, which are lexical decompositions, adapting Vendler's (1957/1967) classification and using some of Dowty's (1979) representations, shown in (1).

(1) *Aktionsart* class   Logical structure
    STATE   **predicate′** (x) or (x, y)
    ACTIVITY   **do′** (x, [**predicate′** (x) or (x, y)])
    ACHIEVEMENT   INGR **predicate′** (x) or (x, y), or
                                       INGR **do′** (x, [**predicate′** (x) or (x, y)])
    SEMELFACTIVE   SEML **predicate′** (x) or (x, y), or

	SEML **do′** (x, [**predicate′** (x) or (x, y)])
ACCOMPLISHMENT	BECOME **predicate′** (x) or (x, y)
	BECOME **do′** (x, [**predicate′** (x) or (x, y)])
ACTIVE ACCOMPLISHMENT	**do′** (x, [**predicate₁′** (x, (y))])) & INGR **predicate₂′** (z, x) or (y)
CAUSATIVE	α CAUSE β, where α, β are logical structures of any type

<div align="right">(Van Valin, 2005, p. 45)</div>

In CL, word meaning is assumed to be encyclopedic. Adopting symbols from formal semantics, (1) may suggest a dictionary view of word meaning. Could you comment on why RRG uses tools from formal semantics to represent word meaning? What are the advantages and disadvantages of this system? Is there a reflection of an encyclopedic view of word meaning at all in the architecture of RRG?

VVL: Why do we use tools from formal semantics? I guess it's historical. I mentioned earlier that I was convinced by Lakoff's and also McCawley's work on lexical decomposition in Generative Semantics. In Generative Semantics lexical decomposition was done in the syntax, and the words were the pieces that were assembled by the operation of the transformations, which, ironically, is the case in the Minimalist Program now. I was convinced, as Bill [William Foley] was, that doing lexical decomposition in the syntax had a number of drawbacks. So, we started our own system of lexical decomposition, and then, we were pointed to Dowty's (1979) book on *Word Meaning and Montague Grammar*, which seemed to have the essence of what we were looking for. It was intended to capture the insights which Generative Semantics had, but with the relationships being lexical rather than syntactic. The representations of RRG are simplified, compared to Dowty's representations. RRG reflects the fact that verbs are normally the central part of the clause, and so word meaning is important for the analysis of many phenomena.

Now, one of the key differences from Cognitive Linguistics is the idea that meaning is just encyclopedic, i.e., there's no language-specific lexical semantics. That seems completely wrong to me. That is, there's knowledge of the world, which is encyclopedic, and there's knowledge of language, which is linguistic. Take the verb 'kill'. There's no universal language independent representation of 'kill'. There are similarities across languages, but in some languages the actor of 'kill' must be animate, and in others it doesn't have to be. In some languages like Japanese, the actor of 'kill' has to be volitional. In other languages, it does not. In some languages, 'kill 'is a causative accomplishment. In others it is a causative achievement. Therefore, while there are a lot of similarities across languages, you can't ignore the language particular properties, and these are not encyclopedic, but these are properties of the lexicon of a particular language. Now, there is an encyclopedia as well, but it doesn't replace the language particular semantic information. I had many spirited conversations and disagreements with Len Talmy about this. So, there is a place for encyclopedic knowledge of the world, but it doesn't replace the language-specific knowledge that you have for each and every language.

DB: In his lexical typology, Talmy (1985, 2000) talks about the lexicon of languages, and he compares different languages that have different lexica. Surely, even in his world view, there must be a lexical dimension that is purely linguistic.

VVL: I think you're right. When Len Talmy would give a talk about no distinction between linguistic knowledge of the world and knowledge of language, I had to object. The work

that he's most known for is lexical, like lexical typology, which obviously, as you say, doesn't involve encyclopedic knowledge of language.

A handy way of including world knowledge into your lexical representations is to incorporate James Pustejovsky's notion of *qualia*, which are linguistic representations of knowledge of the world (see Pustejovsky, 1995).

DB: Can I just go back to one point that you made earlier? You said that with Bill Foley you were against the idea of building meaning in syntax. Could you just mention some of the disadvantages of doing that? I mean, there is an obvious one, i.e., that everything becomes possible: you can't explain why 'kill' behaves in a certain way and 'murder' behaves in a different way, not on independent principles. Whether they merge in syntax in the same way or different ways there is no independent justification for it.

VVL: Well, that's why Generative Semantics died. I watched Generative Semantics die in the T4 library at Berkeley in the spring of 1974. When I got to Berkeley in the fall of 1973, George Lakoff was still doing Generative Semantics, and by the end of the spring quarter, it was dead. It was dead because it was unconstrained, and you had world knowledge in the underlying form of sentences, and presuppositions represented explicitly in the underlying form of sentences. It just got out of control, and there was no principled way to constrain it. Lakoff cited Harris (1954), saying that something is linguistic if it affects the distribution of a morpheme. Well, if you think about all the possible things that could affect the distribution of a morpheme, in principle, there's no limit to what you can construe as affecting the distribution of some morpheme or other in a language. The disadvantage you mentioned is exactly the fatal fault. I used to say that in the underlying form of the tree was the human brain, because you needed everything in that brain to account for all the linguistic phenomena. It was unconstrained, and that is proven to be the case in its resurrected form in the Minimalist Program. I wrote a review of Baker's incorporation book (Van Valin, 1992). He was talking about examples like "X put the book on the shelf" vs. "X shelved the book", where 'shelf 'is incorporated into the syntax, and then turned into a verb. I said down this road lies Generative Semantics and all that goes with that. Baker wrote me a letter and said, "I don't have an argument against your claim, but I'm sure it won't happen." But it has happened.

3.3 Lexicon vs. Grammar

KT: CL takes a position that there is no sharp division between lexicon and grammar. In contrast, RRG makes a clear distinction between lexical and syntactic phenomena, as outlined in the 1997 book:

> the criteria for distinguishing the two classes of phenomena [i.e., lexical and syntactic phenomena] are ultimately theory-internal. In the framework we are presenting, the line between the two is clear-cut and falls out from the linking system [between semantics and syntax] ... : lexical phenomena affect the logical structure of the predicate, its argument structure, and actor and undergoer assignment .... whereas syntactic phenomena deal

with the morphosyntactic realization of the macroroles and other core arguments...." (Van Valin & LaPolla 1997, p. 389)

This does not directly state what the lexicon looks like in RRG. Could you comment on how RRG posits the lexicon? For instance, can anything larger than 'a word 'be in the lexicon? Is the RRG lexicon hierarchically organized, similarly to that of Construction Morphology (Booij, 2010)?

VVL: Yes, I think the lexical vs. grammar distinction is important. There is very little cross-linguistic variation on the lexical side; it's not identical across languages, but it's very similar, whereas syntactic phenomena are quite varied and can be astonishingly different. This goes along with the claim that the more semantically motivated something is, the less cross-linguistic variation there is. We propose that in the 1997 book (Van Valin & LaPolla, 1997)—I think it's the end of Chapter 6, the chapter on Grammatical Relations.

The lexicon looks like two buildings side-by-side, one big and the other small. The big building is the warehouse, where the words and morphemes are stored. Yes, something larger than a word can be in lexicon. You can have idiom chunks, for example. That would be in the lexicon. That's all in the storage, or the warehouse, where all the words and morphemes are stored. Then, you have the workshop, where the linguistic equivalent of Santa's elves – I like to think – are working to assemble the semantic representations in the workshop. This is where lexical rules apply. Then, the completed semantic representation gets sent out into the syntax.

The hierarchical organization of the kind found in Construction Morphology is not really part of RRG. But we do have differences of opinions about morphology in RRG. To give an example, we have Dan Everett's ideas about the layered structure of the word (Everett, 2002) and then, you've got Javier Martín Arista's rather different approach to RRG morphology (Martín Arista, 2008), which figured prominently in Cortés-Rodríguez (2023). So, there are differences in opinion. But I don't think either of the two major approaches has a hierarchically organized lexicon like Construction Grammar and HPSG. Maybe, someone will try to develop such a thing.

DB: Some of the people who try to build meaning in syntax deny the role of the lexicon as an independent module. They would argue that what looks like one lexical item can behave in different ways, and so they would look at this as evidence in support of the syntactic lexicon, as opposed to the independent lexicon. Obviously, RRG doesn't do that. To take just one example, RRG has pairs of lexical entries, like monovalent 'freeze', and bivalent 'freeze'. Why is it not a problem to have such a rich and complex lexicon?

VVL: You mentioned causative and anti-causative and how they alternate. I worked with a colleague from Cologne and looked at what the psycholinguistic evidence was for 'break', which would be like 'freeze', and other alternating verbs (Brocher & Van Valin, 2017). From the evidence we got from our experiment, it looks like those alternating verbs are underspecified in the lexicon. The system has an intransitive and transitive form as prototypes. And the actual lexical item is underspecified. So, it is compatible with either. Thus, it's not the case that there are two copies of 'freeze', two copies 'break 'and two copies of 'shatter'. But there's an underspecified version of each lexical item, and then there are general abstract templates.

The argument structure alternations are of two kinds -- the lexical kind, which would be like dative shift, the transfer alternation, and the removal alternation. These involve the selection of macroroles,[4] which is something that happens in the lexicon, in the workshop. And then you've got things like passive or voice alternations, which are for the most part syntactic and by and large unrestricted in terms of the classes of verbs that participate in them. Argument structure alternations are not all the same: if some scholars want to argue that the fact that some alternations seem to be syntactic shows that they all have to be syntactic, this does not make sense.

By the way, in my paper on Grammatical Aspects of Language Processing in the Brain (Van Valin, 2023b), I uncover a real problem for the syntax-based lexicon theories. That is, split-brain patients can recognize individual words and correctly assess their meaning. Now, if that's possible, that makes sense in a theory where you've got bilateral lexical storage in the brain—some lexical items would be stored in one hemisphere and other lexical items in the other hemisphere, or maybe even redundantly. But crucially, the right hemisphere is cut off from the syntax, which is in the left hemisphere. If split brain speakers of English can recognize individual words in their right hemisphere, which has no access to the syntax, then theories that assume that there are no lexical entries, but rather just roots or something that must be syntactically processed to get a form can't account for the behavior of the split-brain patients.

DB: Yes, absolutely.

VVL: In sum, there might actually be neurolinguistic evidence against this no-lexicon-all-syntax theory.

3.4 Constructions

DB: Although RRG has a separate lexical module, while Construction Grammar does not, both Construction Grammar and RRG make use of the notion of construction. "Constructions" in Construction Grammar seem to subsume what RRG has classed as "constructional schemas" and "syntactic templates".

Could you first comment on the difference between RRG and Construction Grammar in terms of the place constructions hold within each framework? With specific respect to RRG, what is the benefit of separating constructional schemas from syntactic templates? Can they be merged into one? If not, what will be the disadvantage?

VVL: Construction Grammar has a hierarchically organized lexicon, and it claims that everything is a construction, whereas RRG doesn't claim everything is a construction and there is no hierarchically organized lexicon. If you have a lexicon full of hierarchically organized constructions, the constructions are, by definition, language specific. And it becomes difficult to express cross-linguistic generalizations, as many construction grammarians have found. Croft in his radical construction grammar (Croft, 2001) starts off the book by saying that there are no cross-linguistic generalizations, and that all apparent cross-linguistic generalizations are cognitive, not linguistic. Relational Grammar (Perlmutter,

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[4] Macroroles cover 'actor' and 'undergoer', which are generalized semantic roles in RRG (see Van Valin, 2005: 60).

1983; Perlmutter & Rosen, 1984; Postal & Joseph, 1990) faced the language specific construction problem, and by abandoning phrase structure and going to grammatical relations, they could have more abstract rules which would then allow them to capture cross-linguistic generalizations. In RRG, we avoided the construction specific rule problem by having constructions containing language specific information, as well as cross-linguistically valid principles. So, the constructional schemas in RRG are a combination of language particular information and cross-linguistically valid principles. So, there's no problem expressing cross-linguistic generalizations, using constructional schemas.

Now, constructional schemas and syntactic template are not the same thing. In the 1997 book, we made the mistake of using the terminology 'syntactic templates' and 'constructional templates'. We naively assumed that people would see the difference between a syntactic template and a constructional template. People confused them constantly. That's why in the 2005 book (Van Valin, 2005), I changed the terminology to *constructional schema* and *syntactic template*. The advantage of separating them is that constructional schemas contain both cross-linguistic and language specific grammatical information, and, in fact, are the primary locus of language specific information in the system. Syntactic templates are just bare pieces of structure. They are in many cases quite underspecified, and with the underspecified syntactic templates, you don't need that many in a language to capture, say, basic core patterns which depend on the valence of the verb for the predicate in the nucleus. And if they are underspecified, and by that I mean if you don't specify the categories (e.g., RP) in the templates,[5] then you can get by with many fewer templates in the inventory and the categories are projected into them by the linking rules, anyway. So, it's redundant to have a template that says RP, Nucleus, RP, and then have a linking rule that takes the actor and links it to the first position and undergoer links it to the last position. Since the undergoer is going to carry categorial information as is, you can have one template for English, that is, where you have a branch, a core, a daughter of core branch followed by the nucleus, followed by another daughter of core branch, and that can accommodate RP… Nucleus… RP or, alternatively, RP… Nucleus… prepositional phrase. There are examples in the 2005 book. So, by representing phrase structure or constituent structure by bare syntactic templates, you can have a rather economical way of representing the syntactic structures in the language.

If you call it a construction, then it's by definition language specific. And things like that seem to me not to require anything special. You don't treat them as constructions, because they are part of the linking system, and as such they can be generalized across languages. You don't want simple things like that to be treated as a construction, because that leaves you in the constructional specific trap. One thing you have in constructional schemas is the basic word order properties of a language (cf. Van Valin, 2023a, pp. 125–136), but then, that's language specific. So it doesn't cause any larger problems.

DB: The basic word order of the language also depends on the syntactic templates that are in the inventory, right?

VVL: English is so consistent in the position of the nucleus that your templates can have a nucleus position in them, just like Japanese is so consistently verb final that you could put the

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[5] RP stands for "reference phrase" (Van Valin, 2008: 168).

nucleus in the final core. In German, on the other hand, you wouldn't want to do that, because the nucleus can occur virtually in any position in the clause. In sum, you have languages like German, Croatian, or Russian, where anything can be linked into any core slot and obviously information structure plays a crucial role in determining the order. Having constructional schemas with the language specific information, as well as ones that include cross linguistically valid principles, allows you the flexibility you need to describe the language, and capture cross linguistic generalizations.

As for the lexical module, this is important for many reasons in RRG, for example in the study of the acquisition of verbs. Acquisition-wise, RRG fares pretty well. Studies like those of Richard Weist on Polish (Weist, 1990) and Matthew Rispoli on Japanese (e.g., Rispoli, 1989) are quite insightful. They capture important generalizations about the acquisition of verbs, and Michael Tomasello said that the level of detail that he captures is prior to the Aktionsart distinctions in RRG. An RRG acquisition story for the acquisition of English verbs can be built on top of Tomasello's work.

There's a paper by a couple of generative acquisition people which claims that children actively ignore the input and so there are things kids acquire without any evidence in the input whatsoever. That's a challenge I can't resist. So, I've done papers on the acquisition of extraction phenomena (Van Valin, 1994, 1998), showing that, lo and behold, there is evidence available to the child, and that my account makes a testable prediction which no one, no expert in psycholinguistics, has ever tried to test.

3.5 Computational Linguistics

DB: You have already touched upon computational linguistics in RRG. But would you like to say more about the unique contribution of RRG to computational linguistics, or the future development of computational linguistics that can benefit from RRG.

VVL: I think that the work that is being done in Düsseldorf is really important. They have set up tree banks. They've converted the Penn treebank from phrase structure trees to RRG trees. They've set up treebanks on a number of languages for the development of parsers. There's quite a lot of really interesting work going on in Düsseldorf, headed by Laura Kallmeyer and Rainer Osswald, and other postdocs and PhD students there (e.g., Kallmeyer & Osswald, 2023). Their formalization of RRG eliminates the things I like best about the formalism. It gets rid of the Operator Projection and represents it as features, and leaves out the arrows for modifiers, replacing them with features. I'm not too thrilled about features replacing the projections, because it's hard to see some of the distinctions, but we've given talks at computational conferences, and they've been well received.

3.6 Future directions

KT: RRG has benefited from other theories to make it more comprehensive or architecturally sound: for instance, RRG has incorporated insight from Discourse Representation Theory (DRT) (Asher, 1999; von Heusinger, 1999; Kamp & Reyle, 1993), to represent the contextual information, or Lambrecht's (1994) Information Structure to build the pragmatics component of the theory. Conversely, part of RRG has been used in a different

theory (e.g., Ruiz de Mendoza & Mairal Usón, 2007). Do you encourage more interactions with other theories and approaches, including Cognitive Linguistics, in the future?

VVL: Yes, I would like to see much more of that. I know the work you're referring to, FunGramKB, the Functional Grammar Knowledge Base, developed in Spain. That was developed by Carlos Periñán-Pascual, Ricardo Mairal Usón, Francisco Ruiz de Mendoza and other Spanish scholars and computational linguists. In this connection, on April 21, 2023, I presented "From grammar to processing" at *RRG del español y en español: de la tipología al procesamiento del lenguaje y la inteligencia artificial* [RRG of Spanish and in Spanish: From typology to natural language processing and artificial intelligence], organized by Javier Martin Arista and his colleagues at the University of La Rioja.[6]

I'm happy when people borrow ideas from RRG as long as they acknowledge it. There's a postdoc in Düsseldorf, Kata Balogh, who's writing an RRG description of Hungarian, proposing alternative ways of representing information structure. We will see how that turns out.

KT: This completes our questions. Thank you very much for your time and this stimulating conversation.

DB: Thank you!

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[6] More information can be found at:
https://www.unirioja.es/apnoticias/servlet/Noticias?codnot=8107&accion=detnot. For the archived video, visit https://www.youtube.com/live/58YOdNus7tU?feature=share.

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