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## How to investigate linguistic diversity: Lessons from the Pacific Northwest

Henry Davis, Carrie Gillon, Lisa Matthewson

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## PERSPECTIVES

### How to investigate linguistic diversity: Lessons from the Pacific Northwest

HENRY DAVIS

*University of British  
Columbia*

CARRIE GILLON

*Arizona State University*

LISA MATTHEWSON

*University of British  
Columbia*

On the basis of five case studies from languages of the American Pacific Northwest, we argue that, at least in the areas of syntax and semantics, a scientific approach to the study of linguistic diversity must be empirically grounded in theoretically informed, hypothesis-driven fieldwork on individual languages. This runs counter to recent high-profile claims that large-scale typology based on the sampling of descriptive grammars yields superior results. We show that only a hypothesis-driven approach makes falsifiable predictions, and only a methodology that yields negative as well as positive evidence can effectively test those predictions. Targeted elicitation is particularly important for languages with a small number of speakers, where statistical analysis of large-scale corpora is impossible. Given that a large proportion of the world's linguistic diversity is found in such languages, we conclude that formal, hypothesis-driven fieldwork constitutes the best way rapidly and efficiently to document the world's remaining syntactic and semantic diversity.\*

*Keywords:* fieldwork, methodology, syntax, semantics, Salish, Wakashan, Tsimshianic

**1. INTRODUCTION.** At least half of the world's nearly 7,000 languages will no longer be spoken by the end of this century (Harrison 2007, among many others). This imminent large-scale language extinction has alarming consequences for the investigation of linguistic diversity. If we wish to understand the scope and limits of crosslinguistic variation, it is imperative that in the near future we gather as much information about endangered languages as we can, in a form that allows systematic and accurate crosslinguistic comparison.

The need for such work is accepted by linguists of all persuasions; more controversial is how we should go about it. The difficulty of the task is compounded by the fact that nearly all endangered languages are spoken by small and aging populations, and many have already fallen into disuse even among those who speak them fluently. These circumstances pose unique challenges for fieldworkers, who must find the most effective way to probe for linguistic diversity in the limited time frame still available.

In this article we present, illustrate, and defend a methodological approach for detecting linguistic diversity in the areas of syntax and semantics. Our method consists of hypothesis-driven research on a range of languages. Data collection is driven by the need to test the predictions of formal hypotheses, and data are gathered primarily (but not exclusively) by means of fieldwork carried out over an extended time period with a small number of speakers.

\* The authors' names are in alphabetical order. This article is dedicated to Gertrude Ned, St'át'imcets consultant, teacher, language champion, and dear friend, who passed away between submission of this paper and its acceptance. Gertie worked tirelessly to preserve her language, and she will be greatly missed. We gratefully acknowledge all of our language consultants, past and present, including Carl Alexander, the late Beverley Frank, the late Gertrude Ned, Laura Thevarge, and the late Rose Agnes Whitley (St'át'imcets), Vincent Gogag, Hector Hill, and Barbara Sennott (Gitksan), the late Lawrence Baker, the late Tina Cole, the late Lena Jacobs, the late Yvonne Joseph, the late Eva Lewis, Margaret Locke, and the late Frank Miranda (Skwxwú7mesh), and Mary Jane Dick, Katherine Fraser, Barbara Touchie, Sarah Webster, Barney Williams Jr., and Barney Williams Sr. (Nuu-chah-nulth). We are also grateful for helpful feedback from three anonymous referees, as well as from Claire Bower, Greg Carlson, Stanley Dubinsky, John Matthewson, David Psetsky, Michael Rochemont, and audiences at NELS 42 at the University of Toronto, the University of Canterbury, Victoria University of Wellington, the University of Auckland, Carleton University, and the University of British Columbia.

The methodological approach we advocate is already being used by many researchers on minority languages, in our view successfully, but has recently come under attack from two rather different directions. On the one hand, Evans and Levinson (2009, Levinson & Evans 2010), Haspelmath (2012, 2014), and others argue that the enterprise of generative grammar is fundamentally unsuited to the investigation of crosslinguistic diversity, because it suffers from an a priori assumption of crosslinguistic uniformity. On the other hand, Gibson and Fedorenko (2010, 2013) and others maintain that empirical linguistic results are robust only if they are based on data from large pools of speakers, which means in effect that data from endangered and minority languages are inherently suspect.

Our goal here is to demonstrate that formal, hypothesis-driven research on minority languages not only is more efficient at uncovering grammatical generalizations than traditional corpus-based methodologies (including their modern computer-enhanced incarnations), but it also yields certain kinds of data that are either impossible or extremely difficult to extract from textual material, no matter how extensive. We also argue that there is nothing inherently suspect or unscientific about employing grammatical intuitions as a methodological tool in fieldwork situations, provided certain basic safeguards are observed, and we argue that intensive work with a small number of speakers—the normal field situation for endangered languages—is entirely capable of yielding robust and accurate grammatical information.

We support these claims by means of five case studies, drawn from our fieldwork over the past two decades on endangered and underdocumented languages of the Pacific Northwest of North America. In each case study, we show that hypothesis-driven fieldwork has led to a more complete and accurate picture of linguistic diversity than has been produced by methods that rely largely on extracting information from existing descriptive grammars. Formal investigation yields results that have not been, and sometimes cannot be, generated by approaches that avoid the testing of concrete, theoretically informed hypotheses.

In the remainder of the introduction we provide some background on the debate about how best to uncover linguistic diversity and introduce the languages we focus on in the article. We then outline and defend a scientific methodology for crosslinguistic research in syntax and semantics in §2, and present our case studies—on binding condition C, lexical categories, determiner semantics, quantification, and modality—in §§3–7, before concluding (§8).

**1.1. THE DEBATE ON HOW TO STUDY DIVERSITY.** There are many possible ways to approach the study of linguistic diversity, and imminent mass language extinction requires us to find the most efficient and accurate ones. In a recent contribution on the question, Levinson and Evans (2010, henceforth L&E) set up a division between ‘D-linguists’ (‘diversity-driven’) and ‘C-linguists’ (‘the Chomskyans’) and argue that only the former are in a position to uncover linguistic diversity. ‘D-linguists’ are linguists who expect to find linguistic diversity rather than universality. They gather data from many languages (usually using secondary, descriptive sources), and they use ‘more surfacey’ data (L&E: 2734) and ‘only minimal formalism’ (L&E:2737). ‘C-linguists’, by contrast, construct formal theories that generate predictions that can be tested on individual languages.<sup>1</sup>

<sup>1</sup> We are aware that the ‘D-linguist’ and ‘C-linguist’ labels are caricatures, and our use of them does not mean we endorse the dichotomous view of the field that they embody. Like most C-linguistic fieldworkers, we ourselves employ a range of ‘D-linguistic’ methods such as text collection and analysis in addition to ‘C-linguistic’ methods such as direct elicitation, and we see no conflict between the two; we suspect that many D-linguists equally employ C-linguistic methods where the need arises. We emphasize that we are in no way

Why is C-linguistics alleged to be unsuitable for uncovering diversity? According to Levinson and Evans, C-linguists are ‘without serious interest in understanding linguistic diversity’ (L&E:2746). They ‘treat diversity as a distraction from the main enterprise’ (L&E:2734), ‘draw on a very small subset of the data—especially, intuitions about complex clauses’ (L&E:2734), ‘presume, on the basis of strong universal assumptions, that the structural analysis of one language can be imported directly into the analysis of another’ (L&E:2734), and ‘use off-the-shelf categories arising from specific grammatical traditions, and foist them on all languages’ (L&E:2739). In a similar vein, Haspelmath asserts that ‘the assumption of universal categories carries the very real danger of ethnocentrism’ (2012:101), and that ‘on the Chomskyan, aprioristic approach, small languages studied by few linguists cannot have a real impact on general questions of linguistics ... because aprioristic category hypotheses tend to be set up on the basis of the major languages’ (2012:99).

These beliefs about C-linguistics are quite widespread, although not usually expressed in such an explicit and provocative way. If they were correct, C-linguistics should indeed be rejected by anyone whose goal is to understand the nature of crosslinguistic variation.

However, these claims are false. Accurate information about diversity is crucial for the generative program, and especially crucial for anyone with a belief in a universal language faculty. We provide examples below that are representative of the vast amount of linguistic diversity that has been uncovered by C-linguists, much of which is only accessible via formal approaches. We conclusively show that C-linguists do not force languages into a universalist mold. Our case studies not only destroy the myth that C-linguists fail to discover linguistic diversity, but they also show that formal, hypothesis-driven research often gives us a more accurate picture of crosslinguistic variation than does typology based on sources that do not use a hypothesis-driven approach.

Once we debunk the myths about C-linguistics, we eliminate any conflict between formal linguistics and the study of diversity. Linguistic typology, or ‘comparative linguistics’, as Haspelmath (2014) suggests we refer to it, is simply a methodology that looks at phenomena in a wide range of languages, with the goal of extracting generalizations and uncovering regularities; it is not intrinsically linked to any particular theoretical framework. We therefore reject any contrast or separation between typology and formal research, between typology and ‘Chomskyan’ research, or between typology and universal grammar-based research. Many others have made this point before us; see for example Polinsky & Kluender 2006, Baker & McCloskey 2007, Cinque 2007, Polinsky 2010, among others. Our novel contribution here takes the form of five specific case studies from the Pacific Northwest of North America, which exemplify the

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AGAINST traditional text-based models (whether in their original Boasian guise, or in technologically enhanced current incarnations); what we object to is the criticism of other methodologies, which have proven to be very effective in providing types of data that are beyond the reach of corpus-based models.

It is also worth pointing out that there are ‘narrow’ and ‘broad’ interpretations of C-linguistics. The narrow version is explicitly equated in L&E with the MINIMALIST PROGRAM established by Chomsky (1995 and subsequent work), and therefore excludes most generative fieldworkers. The broad version, by contrast, encompasses a range of theoretical frameworks, which differ (sometimes substantially) in their architecture, but share a commitment to certain methodological principles and practices. These include a theory-driven approach to fieldwork, in which predictions are generated by an explicit formal model and then tested against data from a target language; a commitment to the use of grammatical intuitions, particularly in order to elicit negative data; and a view of the grammar as a set of representations in the minds of individual speakers. It is the latter interpretation of C-linguistics that we defend here.

kind of crosslinguistic research that is being successfully carried out all over the world by C-linguists.

**1.2. THE LANGUAGES.** The Pacific Northwest of North America is an ideal laboratory for the investigation of linguistic diversity. First, it is one of the world's most genetically diverse linguistic regions, second in North America only to California (Thompson & Kinkade 1990).<sup>2</sup> Furthermore, unlike in California, a substantial proportion of Pacific Northwest languages are still spoken as first languages, albeit by a rapidly shrinking number of elderly speakers. Third, the Pacific Northwest has a relatively long and rich linguistic history, beginning with Franz Boas, who worked on a number of northwest families, including Salishan, Tsimshianic, and Wakashan; this is particularly pertinent in view of the frequent invocation of the Boasian tradition by D-linguists wishing to emphasize a particularistic rather than a universalist approach to language. Fourth, the area has long been known as a hotbed of linguistic 'exotica', some of which we examine below. And finally, it is also an extinction hotspot, with a very brief window of opportunity remaining to do primary fieldwork.

The case studies below are drawn from our research since 1992 on several languages spoken in the Pacific Northwest; all data presented are from our own fieldwork, unless otherwise stated. We draw on both our published and unpublished work, and cite original sources where appropriate. The languages come from three unrelated families: Salish, Tsimshianic, and Wakashan. From the Salish family, we discuss ST'ÁT'IMCETS, SKW̱WÚ7MESH, and NSYÍXCEN. St'át'imcets (a.k.a. Lillooet) is a Northern Interior Salish language spoken in the southwest interior of British Columbia, with fewer than 100 speakers remaining. Skw̱wú7mesh (a.k.a. Squamish) is a Central Salish language spoken in the Burrard Inlet and Howe Sound areas of coastal British Columbia, with fewer than ten speakers remaining. Nsyíxcen (a.k.a. Okanagan) is a Southern Interior Salish language spoken in south central British Columbia and adjoining areas of Washington State, with at most 200 remaining speakers.

From the Interior branch of the Tsimshianic family, we discuss GITKSAN. 'Gitksan' is the term conventionally used to cover that part of the Nass-Gitksan dialect continuum spoken along the upper drainage of the Skeena River in northwestern interior British Columbia. It has fewer than 500 speakers.

From the Wakashan family, we discuss NUU-CHAH-NULTH (a.k.a. Nootka). Nuuchah-nulth is a Southern Wakashan language, with fewer than 200 remaining speakers. It consists of a chain of fourteen mutually intelligible dialects, and is spoken on the west coast of Vancouver Island.

Because the languages we investigate are all critically endangered, it is important to point out that our concern in this article is linguistic research, whose goal is to establish the limits and nature of diversity in the world's languages. Our proposals in no way imply that formal linguistic research should be practiced to the exclusion of other types of language documentation. Descriptive grammars, text collections, and dictionaries are all vital parts of language documentation and retention initiatives. Our point, however, is that this type of resource in itself—or typological work based primarily on this type of research—is not sufficient to accurately establish the nature and extent of linguistic diversity.

<sup>2</sup> There are certainly sprachbund-based similarities between unrelated Pacific Northwest language families (Thompson & Kinkade 1990, Beck 2000). These are orthogonal to our concerns here, however. Our aim is not to produce a geographically balanced survey, but to highlight specific areas of syntax and semantics where hypothesis-driven research sheds light on variation between Pacific Northwest languages and better-studied (largely Standard Average European) languages.

**2. A METHODOLOGY FOR CROSSLINGUISTIC RESEARCH.** In this section we outline and defend a scientific methodology used by many formal linguists in their crosslinguistic investigation of syntax and semantics. In later sections we illustrate the successful application of this methodology and compare it with other methods, such as those advocated by Levinson and Evans or practiced by Dryer and Haspelmath (2011) and many others in the D-linguistic tradition.

Along with a large proportion of modern linguists, we assume that research should follow some version of the scientific method, and in particular that research should rely on empirically falsifiable hypotheses (see for example Krifka 2011:245, among countless others). When applying the scientific method to the study of syntax and semantics, two important questions arise, which we address in the next two subsections. First, how do we formulate the initial hypotheses that guide data collection? And second, how do we gather the required data to test our hypotheses?

**2.2. FORMULATION OF THE INITIAL HYPOTHESIS.** The first point to make here is obvious, but worth restating: SOME hypothesis must be adopted at or near the beginning of any crosslinguistic investigation. This initial hypothesis may, of course, be informed by prior investigation of corpora, spontaneous natural data, and so on. But fairly quickly, an explicit hypothesis needs to be formulated that makes concrete empirical predictions. Without a guiding hypothesis, fieldwork becomes a haphazard exercise in data collection, yielding only surface generalizations. Data collected in this way may be useful for documentation purposes, but not for making scientific claims about a language.<sup>3</sup>

A more difficult question is WHICH initial hypothesis one should choose when testing for crosslinguistic diversity. Standard scientific procedure tells us that we should choose the one that makes the most easily falsifiable predictions. However, for any given research question, there is no a priori way to select which hypothesis will be most easily falsifiable; that will depend on the individual phenomenon under investigation and on the kind of evidence available. For each case study below, we justify our choice of initial hypothesis by outlining what counts as NECESSARY and SUFFICIENT evidence to falsify it.

In our case studies we often—but not always—adopt an initial hypothesis whereby the language under investigation is assumed to behave like a previously studied language, such as English. Initial hypotheses of this nature are often misconstrued as indicating that C-linguists expect all languages to be uniform. This misunderstanding may be what underlies Evans and Levinson's (2009, henceforth E&L) claim that there is a 'widespread misconception of language uniformity' that has 'grow[n] out of the generative tradition' (E&L:429, 430).

However, it is false that C-linguists assume that languages are uniform; the literature is full of discoveries generativists have made about crosslinguistic variation, and we offer several case studies of this type below. But even at a conceptual level, an initial hypothesis of uniformity is not to be equated with believing that languages are uniform. Under-

<sup>3</sup> Commentators on an earlier version of this article have objected that this characterization is an idealization of actual fieldwork practice. To quote one of them: 'Everyone who has done serious field work on a language knows that one starts by swimming around in the language with little clear notion of what might turn out to be important'. We do not wish to disparage the value of exploratory fieldwork, nor of serendipity, but we disagree with the premise: we think that in practice, fieldworkers start off with a set of provisional hypotheses, most of which get rejected, while a few get refined and sharpened, leading to increasingly precise empirical predictions. In other words, while we agree that the interplay between hypothesis and data collection is complex and sometimes messy, hypothesis formation and testing are not just cosmetic tools used to disguise a fundamentally inductive process: fieldwork is deductive from the outset.

standing this follows directly from the scientific method, in which there is a crucial distinction between one's INITIAL HYPOTHESIS and one's EVENTUAL ANALYSIS, after potentially many iterations.<sup>4</sup> An initial hypothesis is not a belief: it is a heuristic. In fact, in linguistics, just as in other social and physical sciences, researchers are often specifically attempting to disprove their initial hypothesis through empirical testing. Our case studies on binding condition C, determiners, quantification, and modals are instances where an initial hypothesis of uniformity is disproven, leading to the discovery of diversity.

Our assertion that strong initial hypotheses can be generated, and that it is possible to find necessary and sufficient evidence to falsify them, is at odds with the methodological relativism of authors such as Croft (2001, 2005, 2009), Haspelmath (2010), and others in the 'particularist' school of typology. These authors, who often cite Boas (1911) as their inspiration, claim that ALL tests are language-specific, and therefore it is in principle impossible to generalize across languages. For example, Haspelmath (2010:663) claims that '[d]escriptive formal categories cannot be equated across languages because the criteria for category-assignment are different from language to language', and Croft (2013:216) propounds that 'there are no grammatical categories independent of constructions, since each construction defines its own distribution'.<sup>5</sup>

But their premise is wrong. To a surprising—and illuminating—extent, test after test picks out the same set of categories IN THE SAME WAY. For example, the syntactic tests we employ in §4 below to demonstrate the existence of a noun-verb distinction in St'át'imcets yield results that, as far as we know, are invariant; and, as shown by Baker (2003) in a C-linguistic typological study of lexical category distinctions, they are by no means the only crosslinguistically valid tests for lexical categories.<sup>6</sup>

Not only is the premise that there are no crosslinguistically valid categories wrong, but data collection based on this premise also often fails to uncover the full range of facts. If one's initial assumption is that languages vary in unpredictable ways, it is very difficult to advance from this weak assumption to a stronger one (such as that languages display similarities), since in general it is difficult to 'discover' uniformity using purely inductive reasoning. This is particularly true since (as generative linguists have repeatedly pointed out) linguistic diversity is often surface-visible, while uniformity is often more subtle, and only detected via hypothesis-driven discovery procedures. For example, the evidence against the syntactic categories of noun and verb in Salish languages is surface-obvious: all open-class lexical items can function as main predicates or (when combined with a determiner) as arguments. As we discuss in §4, however, there ARE nouns and verbs in Salish; the evidence is harder to find, but strong (pace E&L:434–35).<sup>7</sup>

<sup>4</sup> The failure to make this distinction seems to underlie Haspelmath's (2012, 2014) use of the label 'aprioristic' for C-linguists.

<sup>5</sup> The rejection of any universal basis to crosslinguistic typology leaves the particularists in a quandary if they wish to compare languages. One move is to attempt to set up a theory-neutral metalanguage, as advocated by Haspelmath (2010) in the form of 'comparative concepts'. But such concepts simply amount to a thinly disguised, impoverished universal theory, as observed by Newmeyer (2010), who points out that the closer comparative concepts come to being useful, the more closely they approximate the very crosslinguistic universals they are meant to replace.

<sup>6</sup> It is important to distinguish universal VALIDITY from universal APPLICABILITY. Universally valid tests are frequently inapplicable to particular languages because of independent factors: for example, not all languages possess a complex nominal predicate construction. But as elucidated in §4, in every language that does, only individual-level nouns or adjectives may modify a nominal head. Universally valid but locally applicable generalizations correspond more or less directly to implicational universals.

<sup>7</sup> See van Lier (2012), who denies the existence of a noun-verb distinction in Polynesian languages on precisely these surface-obvious grounds, rejecting subtler evidence for categorial distinctions (Chung 2012a).

**2.3. HOW TO GATHER THE DATA.** Hypotheses make predictions, which must be empirically tested through the gathering of data. The principal methods of data collection include corpus study, fieldwork, psycholinguistic experiments, and neurolinguistic experiments.<sup>8</sup> In line with the majority opinion in the literature, we believe that a range of methodologies should be applied wherever possible. In this section we outline two main desiderata for data collection methods, and briefly outline and defend the principal methodology for working with endangered languages: one-on-one targeted fieldwork.

**DESIDERATUM 1: NEGATIVE EVIDENCE.** Knowing a language entails knowing what one cannot say, as well as what one can. It also entails knowing what things cannot mean. It follows from this that the data must include not only positive examples of what is said, but also information about what cannot be said and which meanings are not available: negative evidence.

The need for negative evidence means that scientific testing of hypotheses about language cannot rely exclusively on the study of corpora (although they are a useful tool). Corpora do not include negative evidence; they only give information about what was attested, not information about what is (im)possible. Many before us have made this point; we mention only two here. Krifka (2011) observes that even large text collections may not provide evidence that distinguishes between different hypotheses. He gives the example of superlatives, and the question of whether *John is the tallest student* can be true if Mary is the same height as John. The chance of finding the answer to this question on the basis of naturally occurring data is vanishingly small. And den Dikken and colleagues (2007:336) give the example of parasitic gaps, which have clear but complex conditions of use: they ‘are restricted to A-bar dependencies, are subject to an anti-c-command condition, and can only be licensed by S-structure movement’. The distributional restrictions on parasitic gaps have been replicated using a range of methods including introspective judgments, elicitation, and experimental studies (e.g. Phillips 2010), but they would have been impossible to distill via examination of corpora, as they are very rarely attested. (See also Schütze 2009, 2011, Davis 2012, Matthewson 2012, among many others, on problems with relying solely on corpora.)

In fact, sometimes even relatively mundane grammatical phenomena are difficult to elucidate on the basis of textual evidence alone. Here is an example from our own work on St’át’imcets. In his excellent text-based descriptive grammar, van Eijk (1997) is circumspect about the unmarked word order of transitive clauses with two overt arguments. The reason is simple: his corpus contains exactly six examples, two of them with VOS order and the other four with VSO order. The reason for the paucity of clauses with two overt arguments is also straightforward: subject to discourse recoverability, St’át’imcets freely permits null arguments, and these are particularly prevalent in the narratives that constitute van Eijk’s corpus. However, two-argument transitive clauses are perfectly grammatical in St’át’imcets and very easy to elicit; it turns out that the four-to-two split in the corpus represents a dialect difference, with Upper St’át’imcets having unmarked VOS order and Lower St’át’imcets unmarked VSO order. The moral here is that formal elicitation is useful not just for complex structures, but even sometimes for rather basic syntactic phenomena, which, due to independent factors (here, the prevalence of null arguments in narrative contexts), do not appear with sufficient frequency in texts to allow generalizations to be made.

<sup>8</sup> Since crosslinguistic research often involves research on a language one does not natively speak, we do not discuss here the option of researchers simply accessing their own judgments.

**DESIDERATUM 2: REPRODUCIBILITY.** A second core principle of the scientific method is that results should be reproducible by other researchers. The way to make this possible is to provide full and explicit information about one's methodology. Any method of data collection—corpus study, fieldwork, or experimentation—can in principle meet the desideratum of reproducibility.

**FIELDWORK AND EXPERIMENTS.** Given the need for negative evidence, the two primary methods of crosslinguistic data collection are targeted one-on-one fieldwork, and large-scale experiments. All else being equal, evidence from both methodologies for each phenomenon would be desirable. However, large-scale experiments are impossible for many of the languages we most urgently need to investigate, if our goal is to uncover linguistic diversity. Minority and endangered languages are exactly the languages for which it may be impossible to gather large numbers of participants, and where the absence of literacy, the age of the speakers, and other factors make certain types of experiments unfeasible.

The impossibility of conducting large-scale experiments for many (if not most) of the world's languages is not a problem, however. For these languages, we simply conduct small-scale experiments—otherwise known as fieldwork. This choice is mandated by practical considerations, but is also fully supported from a scientific point of view. In spite of what has been claimed by researchers such as Edelman and Christiansen (2003), Ferreira (2005), Wasow and Arnold (2005), Featherston (2007), Gibson and Fedorenko (2010, 2013), and Gibson and colleagues (2013), fieldwork with a small number of speakers can be just as reliable a source of data as large-scale experiments. In this claim we follow the many researchers who have offered defenses of methodologies involving small numbers of speakers, including, for example, den Dikken and colleagues (2007), Fanselow (2007), Grewendorf (2007), Haider (2007), Weskott and Fanselow (2008), Phillips (2010), and Sprouse and Almeida (2012a,b, 2013); see also Featherston 2009.

Recall the principles of the scientific method: empirically falsifiable hypotheses, and reproducibility. Hypothesis-driven fieldwork with a small number of speakers satisfies both of these criteria. As pointed out by Sprouse and Almeida (2012b), among others, the informal experiments carried out by fieldworkers are identical to the experiments carried out in labs in many crucial respects. Both methodologies involve the careful construction of a set of conditions to test the relevant minimal contrasts, and both attempt to rule out nuisance variables and 'lexically driven extraneous factors (such as plausibility or word frequency)' (Sprouse & Almeida 2012b:3). Targeted, hypothesis-driven elicitation is designed to test the predictions of hypotheses about language, and as such it meets our first scientific criterion.

The results of small-scale fieldwork experiments are also reproducible, both within and across speakers. In fact, even entirely nonexperimental data-gathering techniques such as accessing the researcher's own intuitions have been shown to be overwhelmingly confirmed by large-scale experiments testing the same phenomena. Phillips (2010:53) gives several examples showing that 'carefully constructed tests of well-known grammatical generalizations overwhelmingly corroborate the results of "arm-chair linguistics"'. In the same vein, Sprouse and colleagues (2013) randomly selected 146 two-condition phenomena from articles in *Linguistic Inquiry*, which were originally gathered using nonexperimental methods. They tested each of these data points experimentally using magnitude estimation, and found a replication rate of 95% (with a margin of error of just over 5%).

For many endangered languages, however, fieldwork cannot be replicated across large numbers of speakers for practical reasons. Does this mean that data from minority

and endangered languages are inherently less reliable? Again, not necessarily so, since even with a single speaker, fieldworkers can still confirm INTRASPEAKER reproducibility. That is, for any one speaker, the results of a variety of grammatical tests over a number of different stimuli should (and usually do) converge on the same results. Note that intraspeaker reproducibility is sufficient, given that our object of investigation is the grammatical competence of individual speakers.<sup>9</sup> Though speakers of the same language have similar grammars, they need not be identical; individuals speaking a mutually intelligible language can vary in the grammars that generate that language (in fact, this is a basic engine of language change). Given this, using large numbers of speakers does not necessarily lead to clearer results, since averaging superficial results over 200 different grammars can be more misleading than investigating one or two different grammars in depth (see den Dikken et al. 2007, Fanselow 2007, Grewendorf 2007, Phillips 2010, among others).

An important consequence of the reproducibility of fieldwork results (both within speakers and also across speakers) is that a large  $N$  is not a prerequisite for a reliable investigation of grammatical competence. This means that one oft-repeated criticism of small-scale experiments—that they fail to yield statistical significance—does not hold water (Gibson & Fedorenko 2013, among others). Reproducibility, NOT statistical significance, is the criterion for scientific discovery. Fortunately, then, we can reject Gibson and Fedorenko's (2013:94) claim—a damaging one for the prospects of uncovering linguistic diversity—that 'the conclusions that can be drawn from [data from endangered languages] will be weaker and more speculative in nature than the conclusions based on quantitative data'.<sup>10</sup>

Finally, there are advantages to one-on-one fieldwork that are absent from large-scale experiments; these relate to the time spent with each speaker, and to the fact that the fieldworker-consultant relationship is not fully parallel to the experimenter-subject relationship. For a start, problems such as those mentioned by Schütze (2005)—having to do with whether speakers fully understand the tasks they are asked to perform—are likely to be noticed early, and mitigated easily, in a context where the investigator is sitting face-to-face with a language consultant and assessing the success of the task in real time, particularly if the test session forms part of a long-term collaborative research partnership.<sup>11</sup>

<sup>9</sup> Linguistics is a cognitive science—we are interested in what is going on in speakers' brains. Hence, we are interested in grammars as mental states. It is important to understand that this does NOT mean we are interested in an 'ideal' speaker, and that the concept of a grammar as a mental state is held by all generative grammarians, not just 'Chomskyans'.

<sup>10</sup> Most of Gibson and Fedorenko's criticisms of nonexperimental methodologies are actually criticisms of a caricature of intuition-based methodology, in which the experimenter consults his/her own intuitions (see also the final section in Gibson et al. 2013). Their criticisms do not apply to fieldwork, where there is typically more than one participant, and the participants are not the researcher and therefore do not have a confirmation bias. Moreover, no decent fieldworker gives only one experimental stimulus for each point, and all responsible fieldworkers control for preceding context; these points nullify Gibson and Fedorenko's other main criticisms of nonexperimental research.

<sup>11</sup> Questionnaire-type methodologies (as employed by e.g. the Leipzig Valency Project; <http://www.eva.mpg.de/lingua/valency/index.php>) offer a kind of compromise between experimental and traditional fieldwork protocols. As such, they enjoy some of the advantages of the former: they are standardized, allowing for easy interspeaker and cross-language comparison, and they can—at least in principle—be administered more efficiently and to a larger pool of speakers than is possible with traditional fieldwork techniques. However, in our experience, they also have certain drawbacks. Because they are by nature inflexible, they can lead to answers being shoehorned into preconceived categories, and unless administered by fieldworkers already familiar with the languages being tested, they are susceptible to the kind of misunderstandings discussed by

Furthermore, fieldworkers are able to engage speakers in discussion of why particular examples sound bad, how they could be improved, and so on. As den Dikken and colleagues (2007:350) point out:

although stimuli should be presented in carefully constructed contexts to control for ... unexpected interpretations, an informant may nevertheless judge a sentence as unacceptable, not because of ungrammaticality, but because the informant had a particular interpretation in mind that the researcher could not have imagined or predicted. These discussions with the informant can clarify the reasoning behind the unexpected judgment, providing insight that might not have been ascertained otherwise.

In a fieldwork relationship, unlike in many large-scale experiments, the consultant is not always entirely ignorant of the object of investigation: both the fieldworker and the consultant are working together to understand the way the consultant's grammar works. This of course demands a high degree of integrity on both parts—but for consultants from minority languages, and even more so for speakers of endangered languages, the stakes are very high to make sure the fieldworker 'gets it right'.

**FIELDWORK METHODOLOGIES.** Fieldwork methodologies are well documented elsewhere, and we give only a brief overview here (see Crain & Thornton 1998, Matthewson 2004, Hellwig 2006, Bower 2008, Krifka 2011, Burton & Matthewson 2015, among many others). The basic principle is that we construct a set of conditions to test the relevant minimal contrasts.

Syntactic fieldwork involves a range of data collection methodologies, from the recording of naturalistic discourse across multiple registers and speech situations (speeches, traditional and contemporary narratives, conversations, and so on) through to tightly controlled elicitation procedures. However, because the kinds of data that are crucial for deciding between different grammatical analyses frequently are complex and involve ungrammatical as well as grammatical utterances, simple inspection of corpora is almost never adequate to establish interesting syntactic generalizations. This is why C-syntacticians rely heavily on the grammatical intuitions of native speakers. Of course, this entails a methodological commitment to the validity of such intuitions. In our own experience, we have found that our consultants—many of whom do not read or write their first language, and almost none of whom have any formal training in linguistics—are remarkably consistent in their judgments of very complex structures, over literally years of elicitation. Of course, like all methodologies, intuition-based methodologies are subject to abuse. Among the common pitfalls are failure to provide adequate contextual support, asking the speakers to provide explicit analysis rather than drawing on their implicit knowledge, failing to control for confounding variables in constructing example sentences, and asking for absolute rather than comparative judgments.<sup>12</sup> None of these problems, however, are inherent to the intuition-based methodology itself, and all of them can be avoided or minimized by a careful and conscientious fieldworker, just as any other experimental protocol is only as effective as the experimenter who designs and implements it.

In semantic fieldwork, a common data collection method is the **FELICITY JUDGMENT TASK**, which is very similar to the **TRUTH-VALUE JUDGMENT TASK** (e.g. Crain & McKee

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Schütze (2005) and mentioned above. The latter problem can be mitigated by an 'expert consortium' approach, in which a group of experienced fieldworkers develops the questionnaire together, and administers it to speakers of languages with which they are already familiar; in this guise, however, questionnaires supplement rather than replace traditional fieldwork approaches.

<sup>12</sup> See Schütze 1996, Cornips & Poletto 2005, among others, on the desirability of a comparative rating scale.

1985, Crain & Thornton 1998). Consultants judge the acceptability of utterances in discourse contexts that are described verbally, or using pictures, props, or storyboards. Acceptability entails truth in a context (consultants never accept a sentence that is false in the given context), but unacceptability may arise for different reasons, including falsity, presupposition failure, or other pragmatic inappropriateness. The researcher then often discusses the utterance with the consultant in more detail, eliciting comments on the acceptability level, how the consultant interprets the utterance, why she feels it is unacceptable, how either the sentence or the context can be repaired, and so on. Other techniques include elicited production as a response either to verbal stimuli or to visual cues. In all cases, judgments and productions are elicited on sentences in context, rather than as isolated utterances.

**2.4. SUMMARY: SCIENTIFIC TYPOLOGY.** In the preceding subsections we have outlined a scientific approach to the study of linguistic diversity. We have claimed that one should adopt an initial hypothesis that is falsifiable via necessary and sufficient evidence, and test the predictions of the hypothesis in as many languages as possible, using a range of data collection techniques wherever possible. We have also argued that the small-scale experiments that comprise fieldwork not only are legitimate as a source of reliable data, but also are often the only practical methodology, particularly when investigating linguistic diversity through the study of endangered languages. The five case studies we present below illustrate the success of this approach, as well as some of the shortcomings of alternative, non-hypothesis-driven methodologies.

Obviously, we are far from the first to advocate a formal approach to the study of linguistic diversity; see Polinsky & Kluender 2006, Baker & McCloskey 2007, Cinque 2007, and Polinsky 2010, among many others, for proposals along these lines. We are also far from the first to practice such an approach; serious crosslinguistic studies by C-linguists are too numerous to cite here. The recent attacks by Evans and Levinson (E&L 2009, L&E 2010), and various responses to them, however, reveal that the case for our brand of scientific typology—including the postulation of an initial formal hypothesis, testing of that hypothesis, and the collection of data that includes negative evidence—is far from settled in the wider cognitive science community. We believe that the case studies we present here, along with the methodological lessons they impart, make a strong case for C-linguistics as a way to uncover linguistic diversity.

**2.5. FORMAT OF THE CASE STUDIES.** Each case study begins with an empirical question (e.g. ‘Do all languages show binding condition C effects?’) and adopts an initial hypothesis for that question. We provide the background needed to investigate the question, and then test our initial hypothesis in at least one Pacific Northwest language. Each case study concludes with a brief discussion of methodological implications.

Due to space constraints, we cannot explore all of the theoretical implications of each individual study. We focus only on the facts that are relevant to the case for hypothesis-driven methodology. As a consequence, we do not provide all the details of our formal analyses here (for further information, see the references given with each case study). The details of the analyses are not the point; the point is that concrete theoretical hypotheses lead to more extensive and accurate empirical discoveries.

**3. CASE STUDY 1: CONDITION C EFFECTS IN NUU-CHAH-NULTH.** The empirical question underlying our first case study is ‘Do all languages show binding condition C effects?’. Our initial hypothesis is that condition C effects reflect universal constraints on anaphoric dependencies. Testing this hypothesis leads us to conclude that some Pacific Northwest languages systematically fail to show condition C effects. The result is an in-

crease in observed crosslinguistic diversity, which is detectable only through a theoretically informed, hypothesis-driven approach.

'Condition C' as understood here does not refer to the original version of BINDING THEORY in Chomsky 1981 so much as to the generalizations it was designed to account for. In fact, for the purposes of this demonstration, it is not particularly important which version of condition C we adopt, as long as it covers the following familiar contrasts in English and other condition C-observing languages.

- (1) a. Christine<sub>i</sub> said [that she<sub>i</sub> would leave tomorrow].  
b. \*She<sub>i</sub> said [that Christine<sub>i</sub> would leave tomorrow].
- (2) a. Jim<sub>i</sub> will be happy [if he<sub>i</sub> gets a lot of money].  
b. \*He<sub>i</sub> will be happy [if Jim<sub>i</sub> gets a lot of money].
- (3) a. Christine<sub>i</sub> saw the person who she<sub>i</sub> gave a book to.  
b. \*She<sub>i</sub> saw the person who Christine<sub>i</sub> gave a book to.
- (4) a. Christine<sub>i</sub> broke her<sub>i</sub> relative's chair.  
b. \*She<sub>i</sub> broke Christine's<sub>i</sub> relative's chair.

In each of the (a) cases, a pronoun is covalued with an antecedent name. Reversing the positions of the name and pronoun in the (b) cases leads to a failure of covaluation.<sup>13</sup>

As is well known, condition C effects are sensitive to hierarchical structure: a purely linear condition on anaphora would fail to deal with cases such as those in 5–8.

- (5) [That she<sub>i</sub> would leave tomorrow] was hinted at by Christine<sub>i</sub>.
- (6) [If he<sub>i</sub> gets a lot of money], Jim<sub>i</sub> will be happy.
- (7) [The person who she<sub>i</sub> gave a book to] saw Christine<sub>i</sub>.
- (8) [Her relative's chair] broke under Christine<sub>i</sub>'s weight.

We assume here, with most of the literature, that the relevant structural relation is one of c-command (Reinhart 1983), and for current purposes we adopt the 'independence' condition in 9.

- (9) A dependent pronoun may not c-command its antecedent.<sup>14</sup>

As just stated, our initial hypothesis is that condition C effects will be found in all languages; in other words, we postulate that 9 is a universal condition. If, in a particular language, we find that dependent pronouns may c-command their antecedents, this will constitute necessary and sufficient evidence that our initial hypothesis is false.<sup>15</sup>

Our initial hypothesis for this case study is in line with the more general universalist approach to anaphoric dependencies, expressed for example by Safir (2004:57), who states that:

all of the principles and operations [governing anaphora] are universal and unparametrized. If the pattern of dependencies differs across languages, then it does so because those languages have derivational or lexical properties that interact with the universal principles proposed here to produce a different pattern.

<sup>13</sup> As has been known at least since G. Evans 1980, these judgments can be affected by focus, or more broadly by a distinction between presupposed and asserted content. Focus has been controlled for in the Nuuchah-nulth examples given here.

<sup>14</sup> This condition is a parochial version of Safir's (2004:3) INDEPENDENCE PRINCIPLE, which adopts the notion of 'dependent identity' from Fiengo & May 1994.

(i) Independence principle: If *x* depends on *y*, then *x* cannot c-command *y*.

Recent work (Barker 2012, Bruening 2014) has questioned the applicability of c-command to various binding phenomena. These criticisms do not, however, affect the main points we are making here, which rely only on a structural asymmetry between subjects and nonsubjects.

<sup>15</sup> Whenever the research question involves a CONSTRAINT on the grammar, it is likely that the initial hypothesis will be that the constraint holds. Positive evidence of the constraint failing to hold will constitute falsification of the initial hypothesis.

The universalist approach constitutes a strong, falsifiable hypothesis—a good thing, from the point of view of science. We now show that it is false, on the basis of data from the Southern Wakashan language Nuu-chah-nulth.

**3.1. CONDITION C MEETS NUU-CHAH-NULTH.** The Nuu-chah-nulth condition C data presented here are from the Ucluelet dialect; identical findings obtain for the Ahousaht dialect, as shown in Davis et al. 2007. Parallel findings also hold for a number of Salish languages; see Davis 2009 and references therein.

The data in 10 form a minimal pair involving complement clauses. In 10a, a name in the matrix clause c-commands a dependent pronoun in an embedded clause; as in English, covaluation is permitted. In 10b, by contrast, the dependent pronoun (which is a null *pro*, licensed by subject agreement morphology on the predicate) c-commands the name inside the embedded clause. Unlike in English, covaluation is perfectly grammatical.<sup>16</sup>

- (10) a. wawa:ma **Lucy** [ʔanič            p̄apačaqʷi:ʔw̄iʔas ʔami:ʷik]  
 say=3IND **Lucy** [COMP=3SBRD bread-make-ASP tomorrow=FUT]  
 ‘Lucy<sub>i</sub> said that she<sub>i</sub> will make bread tomorrow.’  
 b. wawa:ma [ʔanič            **Lucy** p̄apačaqʷi:ʔw̄iʔas ʔami:ʷik]  
 say=3IND [COMP=3SBRD **Lucy** bread-make-ASP tomorrow=FUT]  
 ‘Lucy<sub>i</sub> said that she<sub>i</sub> will make bread tomorrow.’  
 (lit. ‘She<sub>i</sub> said that Lucy<sub>i</sub> will make bread tomorrow.’)

The data in 11 illustrate conditional adjunct clauses. Again, as in English, a pronoun in the embedded clause may be covalued with a name in the matrix clause, as in 11a; but unlike in English, it is also fine in Nuu-chah-nulth for a name in the embedded clause to be covalued with a dependent matrix clause pronoun, as shown in 11b.

- (11) a. čimqcʷ.ʔaqʷma **Jim** [ʔayi:pqu:            ta:na]  
 happy=FUT=3IND **Jim** [lots-receive=3COND money]  
 ‘Jim<sub>i</sub> will be happy if he<sub>i</sub> gets a lot of money.’  
 b. čimqcʷ.ʔaqʷma [ʔayi:pqu:            **Jim** ta:na]  
 happy=FUT=3IND [lots-receive=3COND **Jim** money]  
 ‘Jim<sub>i</sub> will be happy if he<sub>i</sub> gets a lot of money.’  
 (lit. ‘He<sub>i</sub> will be happy if Jim<sub>i</sub> gets a lot of money.’)

Condition C-defying behavior is further illustrated in 12b for relative clauses, and in 13b for possessors. (In 13, readings are given for the specific bracketing shown; we do not discuss irrelevant bracketings and readings.)

- (12) a. ʎa:tsi:č̄iʎitma **Christine** [ya:qʷiʔiti:            hini: ʎača:ʔȳak]  
 see-PRF=PST=3IND **Christine** [REL-do.to=PST=3RLT give book]  
 ‘Christine<sub>i</sub> saw the one who she<sub>i</sub> gave a book to.’  
 b. ʎa:tsi:č̄iʎitma [ya:qʷiʔiti:            **Christine** hini: ʎača:ʔȳak]  
 see-PRF=PST=3IND [REL-do.to=PST=3RLT **Christine** give book]  
 ‘Christine<sub>i</sub> saw the one who she<sub>i</sub> gave a book to.’  
 (lit. ‘She<sub>i</sub> saw the one who Christine<sub>i</sub> gave a book to.’)

<sup>16</sup> Abbreviations used in morpheme glosses follow the Leipzig glossing rules, with the following exceptions: AFF: affirmative particle, CIRC: circumstantial modal, CISL: cislocative, CN: common noun connective, DIR: directive transitivizer, EXIS: assertion of existence, HORT: hortative, PTC: particle, RLT: relative mood, RED: redirective applicative, REDUP: reduplication, SBRD: subordinate subject, SX: subject extraction marker. Due to the complex morphophonology of Nuu-chah-nulth, which obscures many morpheme boundaries, morpheme breaks are not indicated in the first line of the Nuu-chah-nulth data.

- (13) a. kʷaʔakʷaʔapma **Christine** [ʔuʂhʔumsuk kʷa:saʔusuk]  
 break-CAUS=3IND **Christine** [relative=POSS chair=POSS]  
 ‘Christine<sub>i</sub> broke her<sub>i</sub> relative’s chair.’
- b. kʷaʔakʷaʔapma [[ʔuʂhʔumsuk **Christine**] [kʷa:saʔusuk]]  
 break-CAUS=3IND [[relative=POSS **Christine**] [chair=POSS]]  
 ‘Christine<sub>i</sub> broke her<sub>i</sub> relative’s chair.’  
 (lit. ‘She<sub>i</sub> broke Christine<sub>i</sub>’s relative’s chair.’)

The data in 10–13 show that in Nuu-chah-nulth, a subject pronoun in a main clause may be dependent on a name inside a complement clause, a conditional adjunct clause, or a relative clause, as well as on the possessor of a main clause object. These are all environments where covaluation is ungrammatical in English and other well-studied languages, due to the operation of whatever principle or principles are ultimately responsible for the independence condition in 9.

It is important to point out that there is no STRUCTURAL solution to condition C-defying behavior in Nuu-chah-nulth. Other tests, including weak crossover, incorporation, and possessor raising, show that the language is conventionally configurational; see Davis et al. 2007 for details. In other words, in the relevant cases, the name really is c-commanded by a dependent pronoun. We have to conclude that there is a set of contexts where Nuu-chah-nulth systematically violates the condition in 9.

It is also significant that both in Nuu-chah-nulth and in condition C-violating Salish languages (Davis 2009), the offending configurations always involve a PRONOUN c-commanding a referring expression. Dependencies consisting of two names respect condition C, as in English and other well-studied languages, and as illustrated by the contrast in 14.

- (14) a. wawa:mitma **Jim** [ʔanič nanaʔaqaq]  
 say-PST-3IND **Jim** [COMP-3SBRD smart-AUG]  
 ‘Jim<sub>i</sub> said that he<sub>i</sub> is well educated.’
- b. #wawa:mitma **Jim** [ʔanič **Jim** nanaʔaqaq]  
 say-PST-3IND **Jim** [COMP-3SBRD **Jim** smart-AUG]  
 ‘Jim<sub>i</sub> said that Jim<sub>i</sub> is well educated.’  
*Consultant’s comment (laughing): ‘Is there two Jims?’*
- c. wawa:mitma [ʔanič **Jim** nanaʔaqaq]  
 say-PST-3IND [COMP-3SBRD **Jim** smart-AUG]  
 ‘Jim<sub>i</sub> said that he<sub>i</sub> is well educated.’  
 (lit. ‘He<sub>i</sub> said that Jim<sub>i</sub> is well educated.’)

In 14a, we see a well-behaved cross-clausal name-pronoun dependency; in 14b, where the dependent pronoun is replaced by a name, we get a standard condition C effect; and in 14c, we find a condition C-violating pronoun-name dependency of the type already illustrated in 10.

The contrast between 14b and 14c provides evidence that (i) condition C effects do surface in the subset of anaphoric dependencies that involve covaluation between names, and (ii) the independence condition in 9 must be separate from whatever condition governs name-name dependencies. The study of condition C effects in the Pacific Northwest thus not only reveals an unexpected pattern of crosslinguistic variation, but also brings new evidence to bear on the correct formulation of the conditions governing anaphoric (in)dependence.

**3.2. METHODOLOGICAL ISSUES.** The Nuu-chah-nulth facts (and parallel findings in Salish; Davis 2009) pose a direct challenge to the universalist approach to anaphora. As

such, they constitute a clear case of C-linguistic research that has successfully uncovered linguistic diversity. But there is an even stronger methodological point to be made here. Notice that it is only by adopting the strongest (and therefore most falsifiable) hypothesis that these facts were discoverable. Data such as the (b) examples in 10–13 were not stumbled upon serendipitously; they were elicited by researchers specifically testing the initial hypothesis that Nuu-chah-nulth possesses condition C effects. The discovery of variation in condition C effects could only be made by systematic elicitation of grammatical intuitions, employing carefully constructed test sentences in contexts that pragmatically favor the relevant covaluations. Furthermore, the findings we presented are couched within a specific formal theory of anaphoric dependencies (though we have presented a simplified version here), which makes testable predictions for a range of different constructions. In other words, rather than ignoring or obscuring linguistic diversity, hypothesis-driven formal syntax is directly responsible for discovering it.<sup>17</sup>

Could searching corpora have uncovered the Nuu-chah-nulth condition C facts? Unlikely. In addition to the condition C-defying cases, Nuu-chah-nulth speakers more commonly produce standard condition C-obeying configurations such as the (a) cases in 10–14, probably because of a general preference for anaphoric over cataphoric dependencies. And even if condition C-defying cases did turn up in texts, they might not be recognized as such: to properly probe for condition C effects, one needs to present test sentences in a controlled context, to eliminate irrelevant covaluations. Moreover, since condition C rules OUT certain covaluations, the relevant stimuli necessarily involve the possibility of negative data, and as such, even million-sentence corpora—which do not exist for understudied languages such as Nuu-chah-nulth—could only very indirectly establish the presence of condition C effects.

A further consequence of the necessity for hypothesis-driven fieldwork in testing for condition C effects is that D-linguistic typologists working with databases compiled from descriptive grammars (e.g. Dryer & Haspelmath 2011) have nothing to say about these effects. Contrary to the claims of Evans and Levinson that C-linguists are uninterested in and incapable of uncovering diversity, ONLY a C-linguistic approach is capable of discovering the type of syntactic diversity exemplified by the difference in anaphoric possibilities between English and Nuu-chah-nulth. And in fact, a generative literature has been gradually developing on condition C-violating languages, which include, aside from Wakashan and Salish languages in the Pacific Northwest, members of the Athabaskan, Algonquian, and Iroquoian families in North America (Hale 1973, Bruening 2001, Koenig & Michelson 2015, respectively), Kadiweu in South America (Sandalo 1997), and the Circassian language Adyghe (Testeleets 2007).

The condition C-defying results reported on here should be replicable—and they are. Over the course of our investigation, three different experimenters have worked with several different speakers from two different dialects of Nuu-chah-nulth, with the same

<sup>17</sup> There are many attempts in the literature to reduce condition C partially or completely to pragmatic principles, including by Reinhart and her colleagues (Reinhart 1983, 2006, Grodzinsky & Reinhart 1993), as well as by Levinson (1991) and Büring (2005). The existence of crosslinguistic variation of the type reported in this section suggests either that the relevant pragmatic principles must be subject to crosslinguistic variation, or that the pragmatic story cannot be correct. The former seems unlikely, given that there is considerable variation in condition C effects within the Pacific Northwest sprachbund, including between related languages (e.g. Nuu-chah-nulth versus Kwak'wala in the Wakashan family) and even between dialects of the same language (e.g. Upper versus Lower St'át'imcets). It seems to us unlikely that Gricean principles are subject to microparametric variation of this type; the grammar is a more appropriate locus for the variation.

results. The total number of speakers tested is obviously small, since there are fewer than 200 speakers of the entire language. As argued in §2.3 above, this does not obviate the results. And if we were forced to adopt rigid experimental protocols, we would not be able to say ANYTHING about condition C in Nuu-chah-nulth; we would miss the chance to detect diversity altogether.

**4. CASE STUDY 2: LEXICAL CATEGORIES.** The empirical question addressed in our second case study is: ‘Do all languages have a distinction between nouns and verbs?’. Unlike condition C effects, this is a long-standing and familiar question in the typological literature, and has received much attention from both D- and C-linguists. Our main goal in this case study is to illustrate how C-linguistic methods (in particular, targeted syntactic elicitation based on speaker intuitions, yielding negative as well as positive data) can provide crucial evidence where other methods yield inconclusive or faulty results.

Our initial hypothesis this time is NOT one of uniformity. Instead, we adopt the initial hypothesis that Pacific Northwest languages are category-neutral, lacking a distinction between nouns and verbs. This initial hypothesis is falsifiable via the necessary and sufficient evidence of systematic distributional restrictions on either nouns, verbs, or both. The alternative approach—an initial hypothesis that there is a noun-verb distinction—is more difficult to falsify. It is falsified only after every construction in the entire language has been tested and shown to lack distributional differences between noun and verb.

After hypothesis testing, we conclude that Pacific Northwest languages do have a lexical category inventory that contrasts nouns and verbs. So this time, hypothesis-driven testing reveals uniformity, rather than diversity. We do not, of course, claim that the inventory of nouns and verbs is identical across the different languages, or even that elements with the same lexical content must belong to the same category in each language. That could only happen either if lexical categories were purely semantic phenomena (something that is well known to be false; see Pullum 2010, among many others), or if an innate language capacity predetermined every lexical item in human languages (clearly impossible). What we show instead is that language-specific syntactic diagnostic tests reveal the existence of subcategories of lexical items, which turn out to match up surprisingly well in terms of their core semantic content with those of English nouns and verbs.

**4.1. THE NOUN-VERB QUESTION IN THE PACIFIC NORTHWEST.** For nearly a century, there has been a controversy over lexical category distinctions in Salish and Wakashan languages. These languages are routinely cited in the typological literature as counterevidence to the universality of the noun-verb distinction, on the basis of work such as Sapir 1911 and Swadesh 1933, 1948 on Nuu-chah-nulth, Boas 1947 on Kwak’waka, Kuipers 1967 on Skwxwú7mesh, Kinkade 1983 on the Salish family, and Jelinek & Demers 1994 and Jelinek 1995 on Northern Straits Salish. The issue of category neutrality arises for these languages because of what we term PREDICATE-ARGUMENT FLEXIBILITY. This refers to the ability of any open-class lexical item in Salish and Wakashan to function indiscriminately as predicate (in initial position, accompanied by pronominal agreement) or as argument (in noninitial position, introduced by a determiner-like element).

Let us look at some examples. We are using St’át’imcets as representative of the general Pacific Northwest pattern, replicable with minor variations in all Wakashan and Salishan languages.

Examples 15–22 illustrate predicate-argument flexibility in St’át’imcets. Either a nominal, verbal, or adjectival lexical item can serve as the main predicate, without requiring a copula; either a nominal, verbal, or adjectival lexical item can serve as an argument, when introduced by a determiner.

- (15) šmúlač ta=k<sup>w</sup>úk<sup>w</sup>pi?<sup>?</sup>=a  
 woman DET=chief=EXIS  
 ‘The chief is a woman.’ (nominal predicate, nominal argument)
- (16) k<sup>w</sup>úk<sup>w</sup>pi? ta=šmúlač=a  
 chief DET=woman=EXIS  
 ‘The woman is a chief.’ (nominal predicate, nominal argument)
- (17) lóχləχ ta=k<sup>w</sup>úk<sup>w</sup>pi?<sup>?</sup>=a  
 smart DET=chief=EXIS  
 ‘The chief is smart.’ (adjectival predicate, nominal argument)
- (18) k<sup>w</sup>úk<sup>w</sup>pi? ta=lóχləχ=a  
 chief DET=smart=EXIS  
 ‘The smart one is a chief.’ (nominal predicate, adjectival argument)
- (19) ʔíq ta=k<sup>w</sup>úk<sup>w</sup>pi?<sup>?</sup>=a  
 arrive DET=chief=EXIS  
 ‘The chief arrived.’ (intransitive verbal predicate, nominal argument)
- (20) k<sup>w</sup>úk<sup>w</sup>pi? ta=ʔíq=a  
 chief DET=arrive=EXIS  
 ‘The one who arrived is a chief.’ (nominal predicate, intransitive verbal argument)
- (21) ʔáčχ-ən-č-aš ta=k<sup>w</sup>úk<sup>w</sup>pi?<sup>?</sup>=a  
 see-DIR-1SG.OBJ-3ERG DET=chief=EXIS  
 ‘The chief saw me.’ (transitive verbal predicate, nominal argument)
- (22) k<sup>w</sup>úk<sup>w</sup>pi? ta=ʔáčχ-ən-č-áš=a  
 chief DET=see-DIR-1SG.OBJ-3ERG=EXIS  
 ‘The one who saw me is a chief.’ (nominal predicate, transitive verbal argument)

Data such as these have led proponents of category neutrality (e.g. Kinkade 1983, Jelinek & Demers 1994, Jelinek 1995) to claim that there are no lexical category distinctions in Salish and Wakashan, since ‘nouns’, ‘verbs’, and ‘adjectives’ have the same syntactic distribution, occurring indiscriminately in predicate or argument position.

This conclusion is premature, however, and, it turns out, incorrect. This is because in and of itself, predicate-argument flexibility is compatible EITHER with category neutrality OR with a ‘standard’ categorial inventory. In other words, predicate-argument flexibility is a NECESSARY, but NOT A SUFFICIENT, condition for category neutrality. This predicts that there could be languages with predicate-argument flexibility and a clear noun-verb distinction—and there are. Time to bring the Tsimshianic language Gitksan into the picture.

Just like St’át’imcets, Gitksan displays predicate-argument flexibility. This is shown in 23–28.

- (23) hanaq=ł sim?o:gid(-id)=ist  
 woman=CN chief(-SX)=AFF  
 ‘The chief is a woman.’ (nominal predicate, nominal argument)
- (24) sim?o:git=ł hanaq=ast  
 chief=CN woman=AFF  
 ‘The woman is a chief.’ (nominal predicate, nominal argument)
- (25) hanaq=ł caw-ad=ast  
 woman=CN smart-SX=AFF  
 ‘The smart one is a woman.’ (nominal predicate, adjectival argument)
- (26) cax<sup>w</sup>=ł hanaq=ast  
 smart=CN woman=AFF  
 ‘The woman is smart.’ (adjectival predicate, nominal argument)

- (27)  $\acute{w}itx^w=\acute{t}$  hana $\acute{q}$ =ast  
 arrive=CN woman=AFF  
 ‘The woman arrived.’ (verbal predicate, nominal argument)
- (28) hana $\acute{q}$ = $\acute{t}$   $\acute{w}itx^w$ -id=ist  
 woman=CN arrive-SX=AFF  
 ‘The one who arrived is a woman.’ (nominal predicate, verbal argument)

However, although predicates and arguments are flexible in Gitksan, it is easy to isolate a category of nouns, because there is a consistent, morphologically marked distributional difference between nouns and nonnouns inside argument phrases. Subject extraction morphology (-sx), which marks the presence of a null-headed relative clause, is obligatorily present on any argument that does not contain a nominal head, whereas on nominal arguments, extraction morphology is either obligatorily or optionally absent.<sup>18</sup> This is illustrated in 29–32: the nominal arguments in 29 and 31 lack -sx, but the non-nominal arguments in 30 and 32 require it.

- (29)  $\acute{c}ax^w=\acute{t}$  hana $\acute{q}$ (\*-ad)=ast  
 smart=CN woman(\*-SX)=AFF  
 ‘The woman is smart.’ (nominal argument)
- (30) hana $\acute{q}$ = $\acute{t}$   $\acute{c}aw^*$ (-ad)=ast  
 woman=CN smart\*(-SX)=AFF  
 ‘The smart one is a woman.’ (adjectival argument)
- (31)  $\acute{w}itx^w=\acute{t}$  hana $\acute{q}$ (\*-ad)=ast  
 arrive=CN woman(\*-SX)=AFF  
 ‘The woman arrived.’ (nominal argument)
- (32) hana $\acute{q}$ = $\acute{t}$   $\acute{w}itx^w$ \*(-id)=ist  
 woman=CN arrive\*(-SX)=AFF  
 ‘The one who arrived is a woman.’ (verbal argument)

We thus see that Gitksan has a robust (and relatively uncontroversial) noun-verb distinction.<sup>19</sup> This proves that predicate-argument flexibility is perfectly possible in a language with a conventional lexical category inventory. Predicate-argument flexibility simply involves (i) the ability for nouns to act as predicates without an overt copula, and (ii) the possibility of ‘headless’ relative clauses (cf. Swadesh 1953:30).<sup>20</sup>

If predicate-argument flexibility is a necessary, but not a sufficient, condition for category neutrality, then the case has not been made for category neutrality in Salish (or Wakashan). This brings up an important methodological point vis-à-vis the type of evidence adduced for category neutrality: the apparent absence of evidence FOR category distinctions has been used as evidence AGAINST category distinctions. However: the ABSENCE OF EVIDENCE IS NOT NECESSARILY EVIDENCE FOR ABSENCE.

So what do we do now? We go back to St’át’imcets, and continue to look for evidence that would falsify our initial hypothesis of category neutrality. And, in fact, we find it.

<sup>18</sup> When the predicate is verbal, extraction morphology is obligatorily absent on a nominal argument. When both the predicate and argument are nominal, there is variability in extraction morphology. Rigsby (1986:284) shows reversible noun-noun examples where neither order has the extraction morpheme. Our consultants prefer the extraction morphology to be present in 23, but not in 24. In spite of this variation, the generalization holds that nonnominal arguments always require extraction morphology.

<sup>19</sup> Recent work by Forbes (2012) shows that Gitksan has an adjective-verb distinction as well; however, the evidence here is more subtle, so we set it aside.

<sup>20</sup> Beck (2002:207–8) points out several languages where one of these properties holds, but not the other.

Here are two arguments for a noun-verb distinction in St'át'imcets, both first elucidated by Demirdache and Matthewson (1995). These arguments are important in the context of the long-running controversy over category distinctions in Salish, since they are purely syntactic, and thus escape the charge that the noun-verb distinction is only morphological, as claimed by van Eijk and Hess (1986).

The first piece of evidence is that only nouns can head relative clauses, and more generally, only nouns can be modified in argument positions. This is shown in the head-final relative clauses in 33–35: only 33, with a noun in the final position, is grammatical.

- (33) ʔáçχ-ən=łkan [na=šáq<sup>w</sup>=a                    **špúza?**]  
 see-DIR=1SG.SBJ [ABSENT.DET=fly=EXIS **bird**]  
 'I saw a flying bird.'
- (34) \*ʔáçχ-ən=łkan [na=šáq<sup>w</sup>=a                    **k<sup>w</sup>ik<sup>w</sup>š**]  
 see-DIR-1SG.SBJ [ABSENT.DET=fly=EXIS **small**]  
 'I saw a flying small (thing).'
- (35) \*ʔáçχ-ən=łkan [na=k<sup>w</sup>ik<sup>w</sup>š=a                    **ša<sup>q</sup>**]  
 see-DIR-1SG.SBJ [ABSENT.DET=small=EXIS **fly**]  
 'I saw a small flying (thing).'

The second argument is that only nouns can head 'complex nominal predicates'. The modifiers in such predicate strings must be individual-level (either nouns or adjectives, but never verbs; Davis et al. 1997); the rightmost (head) element must be a noun.

- (36) [k<sup>w</sup>ik<sup>w</sup>š **špúza?**] [na=ʔáçχ-ən-án=a]  
 [small **bird**] [ABSENT.DET=see-DIR-1SG.ERG=EXIS]  
 'A small bird was what I saw.'
- (37) \*[špúza? k<sup>w</sup>ik<sup>w</sup>š] [na=ʔáçχ-ən-án=a]  
 [bird **small**] [ABSENT.DET=see-DIR-1SG.ERG=EXIS]  
 \*'A "bird small" was what I saw.'
- (38) \*[špúza? **ša<sup>q</sup>**] [na=ʔáçχ-ən-án=a]  
 [bird **fly**] [ABSENT.DET=see-DIR-1SG.ERG=EXIS]  
 \*'A "bird fly" was what I saw.'

We conclude from the evidence presented here that nouns and verbs are syntactically distinct in St'át'imcets: see Matthewson & Davis 1995, Burton & Davis 1996, Davis et al. 1997, Davis & Matthewson 1999, and Davis 2003, 2011 for additional evidence for this conclusion. Convincing syntactic arguments for a noun-verb distinction have now also been adduced for many other Salish and Wakashan languages: see among others N. Mattina 1996 on Okanagan, Davis et al. 1997 on Shuswap, Wojdak 2001 on Nuuchahnulth, Beck 2002 on Lushootseed and Bella Coola, and Montler 2003 on Straits Salish. The last of these is particularly significant because the Lummi dialect of Northern Straits Salish was the language on which Jelinek and Demers (1994) and Jelinek (1995) based their famous but now discredited claims of category neutrality. It is fair to say that for the last decade there has been a consensus among linguists working on Salishan and Wakashan languages that a noun-verb distinction must be recognized at both the morphological and syntactic levels.<sup>21</sup>

**4.2. METHODOLOGICAL ISSUES.** Why did it take so long for basic category distinctions in Pacific Northwest languages to be established? Because of faulty methodology. We see two related problems.

<sup>21</sup> Although the case is more difficult to establish, strong syntactic arguments can also be made for the category of adjective in St'át'imcets and other Salishan and Wakashan languages (see Davis 2011 and references therein).

First, the empirical argument from predicate-argument flexibility was based on the conjunction of two superficial syntactic properties (the absence of a predicational copula, and the presence of headless relative clauses), neither of which is a sufficient argument for category neutrality. Accurate conclusions were reached only once attention was paid to the goal of obtaining necessary and sufficient evidence.

Second, the hypothesis that Salish and Wakashan languages lack category distinctions was accepted too hastily. In particular, the failure to systematically examine a range of syntactic constructions, including relative clauses and complex nominal predicates, led to a premature conclusion of category neutrality. As a consequence, the robust syntactic noun/verb asymmetries that do exist were not discovered until C-linguists entered the arena.<sup>22</sup>

We should mention that the methodology advocated here—which involves actively seeking categorial asymmetries in order to falsify a null hypothesis of category neutrality—has come under fire from D-linguists as embodying a form of ‘methodological opportunism’; C-linguists are accused of ‘choos[ing] the constructions that make the theoretical point you want to make’ (Croft & van Lier 2012:59). In response to this criticism, aimed at her own work on lexical category distinctions in Chamorro, Chung (2012b:138) responds that ‘[a] more constructive—and accurate—way of framing Croft and van Lier’s observation is to say that theories make predictions that can be tested empirically’. We concur.

**4.3. HAS PACIFIC NORTHWEST CATEGORY NEUTRALITY BEEN LAID TO REST?** Unfortunately, no. The following passage is from E&L.

A feeling for what a language without a noun-verb distinction is like comes from Straits Salish. Here, on the analysis by Jelinek (1995), all major-class lexical items simply function as predicates, of the type ‘run,’ ‘be\_big,’ or ‘be\_a\_man.’ They then slot into various clausal roles, such as argument (‘the one such that he runs’), predicate (‘run[s]’), and modifier (‘the running [one]’), according to the syntactic slots they are placed in. The single open syntactic class of predicate includes words for events, entities, and qualities. (E&L:434)

See also Ambridge et al. 2014:e57, who also cite Salish in support of category neutrality. It is depressing that such a thoroughly discredited view of Salish syntax should once again find its way into print, with no acknowledgment of the twenty years of intensive work on lexical category distinctions that has taken place since Jelinek’s original claims.<sup>23</sup> In fact, no one working on Salish holds to category neutrality these days: Eloise Jelinek herself publicly recanted her previous views in 2002.<sup>24</sup>

**5. CASE STUDY 3: DETERMINERS.** Our next three case studies concern semantic variation. The first addresses the issue of definiteness, a subpart of a larger research question concerning the possible parameters of semantic variation in determiner systems. Our empirical domain here consists of three Salish languages: St’át’imcets, Skwxwú7mesh,

<sup>22</sup> As noted above, a morphological distinction between noun and verb in Salish was established earlier; see van Eijk & Hess 1986. Though at the time it was widely believed that the observed morphological differences did not reflect distinctions between syntactic categories, Davis (2011) points out that van Eijk and Hess’s principal morphological diagnostic, based on the distribution of the nominalizer *s-*, actually has a crucial syntactic component.

<sup>23</sup> Our dismay is deepened by an elementary mistake in language classification in Evans and Levinson’s response to Tallerman (2009). To quote: ‘We mentioned the Wakashan language Straits Salish as an example of a language plausibly claimed to lack a noun/verb distinction’ (E&L:481). Of course, Straits Salish is a SALISH language (or, to be precise, a group of two closely related languages).

<sup>24</sup> At the 37th International Conference on Salish and Neighboring Languages, held at Western Washington University in August, 2002.

and Nsyílxcen (Okanagan). The first two of these have rich determiner systems (with seven and eight members respectively); Nsyílxcen possesses only one determiner, *ʔiʔ*.

As usual, we need to decide on an initial, testable hypothesis that will guide data collection. There are at least two semantic distinctions that can be encoded by determiners in natural language—definiteness and specificity—and either of these would be a reasonable place to start. We choose definiteness, because a hypothesis about definiteness makes clear and (for the most part) widely accepted empirical predictions, to be outlined below. We therefore adopt an initial hypothesis that our three Salish languages possess definite determiners. This initial hypothesis will be falsified: Salish languages LACK definite determiners. Hypothesis-driven research thus reveals diversity. In §5.2 we show that, in contrast, non-hypothesis-driven typological methodologies have failed to detect the ways in which Salish determiners differ in their semantics from those of Indo-European languages.

In order to begin testing our initial hypothesis that St'át'imcets, Skwxwú7mesh, and Nsyílxcen possess definite determiners, we need definitions of terms that are precise enough to make testable empirical predictions. First, we assume that determiners are elements that introduce argument noun phrases and cannot occur on their own (in contrast to demonstratives). This is illustrated in 39 for English.

- (39) a. I ate the cookie.  
 b. \*I ate the.  
 c. I ate that.

There is a long-standing debate on the correct semantic analysis of definite determiners; see Frege 1997 [1892], Russell 1998 [1905], Christophersen 1939, Hawkins 1978, 1991, Prince 1981, 1992, Heim 1982, Kadmon 1990, 1992, Abbott 1999, and Roberts 2003, among many others. We cannot do justice to this debate here, but instead focus on the two main empirical claims typically made for definites. The first is that definite determiners can usually only be used in FAMILIAR contexts: the intended referent should be present in the common ground (shared knowledge) of speaker and hearer. This is illustrated in 40–41.

- (40) [Context: out of the blue; no prior discussion of ghosts]  
 #The ghost wanted to scare me. (novel)  
 (41) I saw a ghost and three unicorns last night. The ghost wanted to scare me. (familiar)

The second claim is that definite determiners place a UNIQUENESS requirement on the referent matching the NP description within a context.<sup>25</sup> In 42, there must be one and only one bear in the context, and that bear must have attacked.

- (42) [Context: one bear]  
 The bear attacked.

In a context where there is more than one referent matching the NP description, using a singular definite is illicit, as shown in 43.

- (43) [Context: two identical cups sitting next to each other]  
 a. #Pass me the cup.  
 b. Pass me the cups.

In 44 with a plural definite, it must be true of ALL of the bears in the context that they attacked.

- (44) [Context: more than one bear]  
 The bears attacked.

<sup>25</sup> Uniqueness is realized as maximality on plural and mass nouns (Link 1998).

Test cases like 45 below show that the uniqueness/maximality requirement cannot be canceled. Here, *the chickens* must refer to all four chickens. This cannot be canceled by adding *but one escaped*.<sup>26</sup>

- (45) I saw four chickens and five geese last night. I killed the chickens, #but one escaped.

Given this characterization of definiteness in terms of familiarity and uniqueness, our initial hypothesis that Salish languages possess definite determiners is falsifiable in the following way. If all determiners in the Salish languages under investigation are systematically felicitous in novel (nonfamiliar) and nonunique contexts, then our initial hypothesis will be falsified.<sup>27</sup>

In terms of methodologies of data collection, corpora can be used to extract information about familiarity: positive evidence of determiners in novel contexts can provide clues about a particular determiner not being definite. However, if a particular determiner were NOT found in novel contexts in corpora, the determiner still need not be definite, since its absence in novel contexts could be an accidental gap. In that case, we would require negative evidence in the form of speaker judgments showing systematic infelicity of the relevant determiner when it is used to introduce novel referents. Furthermore, corpora are inherently incapable of providing evidence as to whether noun phrases give rise to obligatory inferences of uniqueness/maximality. Instead, direct elicitation involving the collection of negative evidence is required; we need to try to explicitly cancel any uniqueness/maximality effects that might arise. This is what C-linguists have done for Salish, as we are about to show.

**5.1. DETERMINER SEMANTICS IN SALISH.** Matthewson (1998), Gillon (2013), and Lyon (2011, 2013) have demonstrated that St'át'imcets, Skwxwú7mesh, and Nsyílxcen, respectively, lack definite determiners. In this section we summarize the types of evidence used by these authors to establish the lack of definiteness.

First, all three languages falsify the familiar-context-only criterion. In St'át'imcets, for example, the determiner *ti...a* can be used in both novel and familiar contexts.<sup>28</sup> In 46, the referent *ti=šmómłáč=a* 'a girl' is introduced for the first time in the story. In 47, a couple of lines later, the same determiner is used on the now familiar noun phrase.

- (46) *húy=łkan ptak<sup>wł</sup>, pták<sup>wł</sup>-min lč?a ti=šmómłáč=a ...*  
 going.to=1SG.SBJ tell.story tell.story-APPL here DET=girl=EXIS  
 'I am going to tell a legend, a legend about a girl ...' (novel)  
 (van Eijk & Williams 1981:19, cited by Matthewson 1998)

- (47) *wá?=k<sup>wu</sup>? ?ílal láti? ti=šmómłáč=a*  
 IPFV=REP cry there DET=girl=EXIS  
 'The girl was crying there.' (familiar)  
 (van Eijk & Williams 1981:19, cited by Matthewson 1998)

Similarly in Skwxwú7mesh, the determiner *ta* can be used in novel contexts (48) as well as familiar ones (49).<sup>29</sup> All of the referents in 48a–c are mentioned here for the first time in the stories, using *ta*. In 49, a basket is introduced by *k<sup>wá</sup>ci hiyí sitn* 'a large bas-

<sup>26</sup> It is beyond the scope of this article to decide whether uniqueness/maximality is presupposed or merely asserted; see the references listed above for discussion. The important empirical property of English definiteness is that the uniqueness/maximality inference cannot be canceled.

<sup>27</sup> 'Systematically' is important, because in certain types of discourse context, definite articles in languages like English can be accepted in novel contexts. See the references cited above.

<sup>28</sup> This is true of almost all St'át'imcets determiners. There is one determiner, *k<sup>wu</sup>*, that is restricted to novel contexts, but none that are restricted to familiar contexts (Matthewson 1998).

<sup>29</sup> This is true of the other determiners as well.

ket', and then the now familiar basket is referred to by *ta=sitn* 'the basket'. The determiner *ta* does not disambiguate between novel and familiar contexts.

- (48) a.  $\text{ʔúyuł-šit-əm-wit}$   $\text{ta=qəqiyás siy'ic}$   $\text{ta=məlášis}^{30}$   
 canoe-APPL-PASS-3PL DET=barrel full DET=molasses  
 'A barrel of molasses was put aboard for them.' (novel)  
 (Kuipers 1967:238, cited in Gillon 2013:84)
- b.  $\text{na cíx}^w$   $(t=)\text{ta=χácu?}$ ,  $\text{wəł na múyuñ-cut}$   $\text{ci=Sinulhkaý}^{31}$   
 PTC arrive OBL=DET=lake and PTC submerge-REFL DET=Sinulhkaý  
 'Sinulhkaý reached a lake, and submerged.' (novel)  
 (Kuipers 1967:231)
- c.  $\text{níł məłh s=əs}$   $\text{mən mí}$   $\text{tx}^w\text{wúwuq}^w\text{i}$ ,  $\text{na cíx}^w\text{-ənt-as}$   
 FOC then NMLZ=3POSS just come downstream PTC reach-TR-3ERG  
 $\text{ta=qiýát}$   $\text{ʔúx}^w\text{umix}^w$   
 DET=again village  
 'Then he came downstream and reached another village.' (novel)  
 (Kuipers 1967:233)
- (49)  $\text{na mí}$   $\text{ʔúys}$   $\text{k}^w\text{əhi}$   $\text{hiyí slánaý}$   $\text{čəm-čəməá?}$ -s-t-as  
 PTC come inside DEM.F big woman REDUP-carry.on.back-CAUS-TR-3ERG  
 $\text{k}^w\text{əci}$   $\text{hiyí sitn}$   
 DEM big basket  
 'A big woman came in, carrying a large basket on her back.' (novel)  
 ...  $\text{s=əs}$   $\text{mən cəx}^w\text{-s-t-as}$   $\text{ta=stáʔux}^w\text{ł}$   $\text{tx}^w\text{núw}$   
 NMLZ=3POSS just throw-CAUS-TR-3ERG DET=child into  
 $\text{t=ta=sitn}^{32}$   
 OBL=DET=basket  
 '... and she threw the children in the basket ...' (familiar)  
 (Kuipers 1967:219–20, cited in Gillon 2013:83)

Finally, in Nsyilxcen, the single determiner *ʔi?* can introduce both familiar and unfamiliar referents. This can be seen in 50–51, where *ʔi?* is used both to introduce the new referent *ʔi? səsiʔsəlx* 'their uncle' (50) and to refer back to the same referent (51).

- (50)  $\text{cwix}$   $\text{ʔi?}=sqilx^w$   $\text{ʔuł}$   $\text{ʔi?}=stəmtíma?$   $\text{ʔuł}$   $\text{ʔi?}=səsiʔ-səlx$   
 dwelling DET=people CONJ DET=grandmother CONJ DET=uncle-3PL.POSS  
 $\text{ʔuł}$   $\text{tkaʔsasíl}$   $\text{ʔi?}=xəxíwʔxuʔtəm$   
 CONJ two(human) DET=girls  
 'There lived some people, a grandmother, their uncle, and two little girls.' (novel)  
 (A. Mattina & DeSautel 2002:111, cited in Lyon 2011:209)
- (51)  $\text{ʔixí?}$   $\text{ʔuł}$   $\text{wík-nt-m-əlx}$   $\text{ʔi?}=t=səsiʔ-səlx$   
 DEM CONJ seen-DIR-PASS-3PL DET=OBL=uncle-3PL.POSS  
 'Then their uncle saw them.' (familiar)  
 (A. Mattina & DeSautel 2002:113, cited in Lyon 2011:210)

<sup>30</sup> This is from a story about first contact with white sailors. The sailors are giving the Squamish people gifts, and the barrel of molasses is one of the gifts.

<sup>31</sup> Examples 48b,c are from a story about a man who fights a monster (Sinulhkaý). The man has just begun chasing Sinulhkaý when she finds a lake. This lake is not named.

<sup>32</sup> In this story, *Kál'kalilh* (Giant Cannibal Woman) steals children to eat. She is introduced to the story, along with her basket. A few lines into the story, she throws the children into that same basket.



- (57) a. ka-háhlh-a      tí=šnóq<sup>w</sup>əm=a  
 CIRC-show-CIRC DET=sun=EXIS  
 ‘The sun appeared.’  
 b. ka-háhlh-a      tí=nkakúšənt=a  
 CIRC-show-CIRC DET=star=EXIS  
 ‘A star appeared.’

Examples 58–59 show that the singular determiner *ti/ta...a* can be used to refer either to a unique puppy in the discourse context or to one out of five puppies in the discourse context; it does not encode uniqueness.

- (58) [Context: There is one puppy in the room. It is sleeping.]  
 x<sup>w</sup>?áʒ=aš k<sup>w</sup>=á=šu      wenáx<sup>w</sup>-č! wa? ɣ<sup>w</sup>uýt ta=sqóqχ?=a  
 NEG=3SBJV DET=IPFV=2SG.POSS true-mouth IPFV sleep DET=puppy=EXIS  
 ‘Be quiet! The puppy is sleeping.’  
 (59) [Context: There are five puppies in the room. One of them is sleeping.]  
 x<sup>w</sup>?áʒ=as k<sup>w</sup>=á=šu      wənáx<sup>w</sup>-č! wa? ɣ<sup>w</sup>uýt ta=šqóqχ?=a  
 NEG=3SBJV DET=IPFV=2SG.POSS true-mouth IPFV sleep DET=puppy=EXIS  
 ‘Be quiet! A puppy is sleeping.’

And 60 shows that the St’át’imcets plural determiner *?i...a* does not enforce maximality. The default maximality effect of *?i...a*, seen in the second sentence, can be explicitly canceled without contradiction in the third.

- (60) qəm̩p wi=χ<sup>w</sup>?účín ?i=šk<sup>w</sup>əm̩k<sup>w</sup>úk<sup>w</sup>m̩t=a wa? š-ʔáčχ-š-tum  
 ten PL=four DET.PL=child(PL)=EXIS IPFV STAT-see-CAUS-1PL.ERG  
 ‘We are looking after fourteen children.’  
 wa? q̩ʔ-ál̩mən ?i=šk<sup>w</sup>əm̩k<sup>w</sup>úk<sup>w</sup>m̩t=a; x<sup>w</sup>uyšt-wí=maʔ ʔáz-xit  
 IPFV eat-want DET.PL=child(PL)=EXIS let’s-PL=HORT buy-APPL  
 k<sup>w</sup>u=š-q̩áʔ  
 DET=NMLZ-eat  
 ‘[DET.PL] children are hungry. Let’s buy some food.’  
 x<sup>w</sup>?it-ʔúl! x<sup>w</sup>?áy=λu? k<sup>w</sup>=š=tákəm ?i=šk<sup>w</sup>əm̩k<sup>w</sup>úk<sup>w</sup>m̩t=a wa? q̩ʔ-ál̩mən  
 many-too NEG=just DET=NMLZ=all DET.PL=child(PL)=DET IPFV eat-want  
 ‘That’s too much! Not all the children are hungry.’  
*Consultant’s comment: ‘The story sounds okay.’*

These data show that Skwxwú7mesh, Nsyílxcen, and St’át’imcets determiners lack any obligatory uniqueness or maximality effects. Since they lack both features we associate with definiteness, we conclude that these determiners are not definite. This makes them different, semantically, from determiners in languages like English. Our initial hypothesis that Salish languages have definite determiners has been disproven.

**5.2. METHODOLOGICAL ISSUES: THE D-LINGUISTIC APPROACH TO SALISH DETERMINERS.** Let us now see what D-linguistics has to say about determiner semantics in Salish. As a representative of the D-linguistic view, we present an overview of the *World atlas of language structures (WALS)* chapter on definiteness (Dryer 2011a) and show that it seriously misrepresents the situation in the Pacific Northwest with respect to the existence of definite articles.

The categories of language distinguished by Dryer (2011a) are given in 61, with language totals for each category. The language map corresponding to these results can be found at [http://wals.info/feature/37A?tg\\_format=map&v1=c00d&v2=c99f&v3=cd00&v4=dfff&v5=cfff](http://wals.info/feature/37A?tg_format=map&v1=c00d&v2=c99f&v3=cd00&v4=dfff&v5=cfff).

## (61) Categories of language with respect to whether definiteness is present:

Definite word separate from demonstrative	216
Demonstrative word used as definite article	69
Definite affix	92
No definite, but indefinite article	45
No definite or indefinite article	198

The first problem with this categorization is that the categories are ill-defined. Dryer (2011a) uses ‘definite’ as a label for ‘a morpheme which accompanies nouns and which codes definiteness or specificity’. Definite articles are claimed by Dryer to have two functions: an anaphoric use and a nonanaphoric use, where the speaker assumes the hearer will know the referent.<sup>33</sup> Specificity is not defined, but Dryer includes specific INDEFINITES under its purview; thus, a problem for those interested in what determiners mean is that the term ‘definite’ is used for a set of elements that confusingly includes a subset of indefinites. Furthermore, it is not clear whether languages in the last group, ‘No definite or indefinite article’, may possess determiners that encode neither definiteness nor indefiniteness, or whether these are only ‘bare noun’ languages (like Cherokee, the example Dryer gives).<sup>34</sup>

Turning specifically to the Pacific Northwest, we see that the coverage is misleading. Of the twenty-three Salish languages, most of which have decent descriptions and several of whose determiners have received in-depth formal treatment, three are included by Dryer (2011a): *Sḵw̓x̓wú7mesh*, Bella Coola, and Comox. All three of these are claimed to have some form of definite article, whether a separate word (*Sḵw̓x̓wú7mesh*, Bella Coola) or an affix (Comox). Unfortunately, this characterization is certainly inaccurate for *Sḵw̓x̓wú7mesh*, and almost certainly inaccurate for Bella Coola and Comox. As outlined above, *Sḵw̓x̓wú7mesh* lacks any determiner that encodes either familiarity or obligatory uniqueness/maximality, and Matthewson’s (1998) survey of seven Salish languages found none with any marker of definiteness or specificity.

How did these mistakes get made? The source cited by Dryer for *Sḵw̓x̓wú7mesh* is Kuipers 1967, the descriptive grammar of the language. Kuipers does describe the *Sḵw̓x̓wú7mesh* determiner system as being divided along (in)definite lines. However, it is clear that the articles called ‘definite’ by Kuipers do not correspond to definites as they are typically defined. Most semanticists’ definitions of definiteness, as well as Dryer’s own definition, involve the notion of HEARER familiarity with a unique referent. Yet Kuipers writes about *Sḵw̓x̓wú7mesh* that ‘[t]he definite forms are used for objects which are individually identified FOR THE SPEAKER in an independent way’ (1967:137, emphasis added). For example, the determiner *ta* is labeled ‘definite’ by Kuipers, but can be used for a referent that is known only to the speaker, as shown in 62.

(62) t’ámat čəx<sup>w</sup> ta=na wa ʔíp̓is-t-an  
 guess 2SG.SBJ DET=PTC IPFV carry-TR-1SG.SBRD

‘Guess what I have here.’

(Kuipers 1967:138)

As Kuipers himself notes, the ‘definite forms’ in *Sḵw̓x̓wú7mesh* are not equivalent to definite determiners in English.

Perhaps the relevant *Sḵw̓x̓wú7mesh* articles are specific, rather than definite; if they are, that would explain the language’s categorization by Dryer. However, the

<sup>33</sup> The latter use may correspond to what we have called uniqueness (the hearer can uniquely identify the referent), but it is not clear.

<sup>34</sup> If the latter, then there is no category for languages whose articles encode distinctions other than (in)definiteness—a serious problem.

S<sub>k</sub>w<sub>x</sub>wú7mesh ‘definite’ determiners also cannot be said to be obligatorily specific. This can be seen in 63: the noun phrase *ta=súq<sup>wi</sup>?* ‘the/a fish’ can take either wide or narrow scope. The narrow-scope reading does not involve a specific fish; it simply asks whether the hearer bought any fish at all.

- (63) nú čəx<sup>w</sup> síʔ-an ta=súq<sup>wi</sup>?  
 Q 2SG.SBJ buy-TR DET=fish  
 ‘Did you buy any/the fish?’ (wide OR narrow scope) (Gillon 2013:8)

Dryer also mischaracterizes Bella Coola. He cites Davis & Saunders 1997:86 for the claim that this language possesses a definite article, but Davis and Saunders actually provide an analysis of the Bella Coola deictic system as encoding distance from the speaker, gender, number, and a demonstrative/nondemonstrative contrast. The last does not correspond to an (in)definiteness distinction; Davis and Saunders do not claim that the demonstratives are necessarily familiar, unique, or specific. Instead, the demonstratives are used primarily for individuals who are visible to the speaker, and ‘occur[] correctly with a gesture of pointing’ (Davis & Saunders 1997:87). The assumption that an element that is demonstrative in this sense is necessarily definite may or may not hold for Indo-European languages (see Prince 1981), but it certainly does not hold for Salish, as shown by Matthewson (1998), among others. Nor does the other grammar of Bella Coola (Nater 1984) provide evidence for a definite determiner. Nater states that there are two sets of determiners in Bella Coola: those that are usually translated as ‘a’ and those that are usually translated as ‘the’; however, he neither makes any claim nor provides any evidence that the latter are definite in the sense of familiar or unique.

With respect to Comox, Dryer (2011a) cites Hagège 1981:134 on the claim that this language has a definite affix. Hagège actually states that the Comox system is NOT constructed according to a definite/indefinite opposition, although he writes that the articles he glosses as ‘anaphoric, distal’ are often used as indefinites, while the articles glossed as ‘anaphoric, proximal’ are often used as definites (no examples of sentences in context are given to support this claim).<sup>35</sup> Watanabe (2003) makes no mention whatsoever of a definiteness distinction in this language, writing merely that ‘[t]here are four principal determiners in Sliammon: *tə* “referential (non-feminine)”, *ʔə* “referential (feminine)”, *k<sup>w</sup>ə* “non referential”, *ʃə* “remote” ’ (Watanabe 2003:79). The evidence for a definite determiner in Comox is, at best, negligible.

The problem here extends beyond the fact that sources such as Kuipers 1967 or Davis & Saunders 1997 were over- or misinterpreted: it extends to choices about which sources to look at, and which to ignore. Thus, while Hagège 1981 is generally known by Salishanists to be unreliable (see Kroeber’s (1989) polite but damning review), there is a growing body of accurate, targeted research on determiners in Salish, which could have been consulted more profitably. Gillon (2013) provides detailed discussion of S<sub>k</sub>w<sub>x</sub>wú7mesh determiners, and argues that they encode neither definiteness nor specificity. Matthewson (1998, 1999) does the same for St’át’imcets, and N. Mattina (2006) and Lyon (2011, 2013) do the same for Nsyílxcen. N. Mattina writes (2006:127) that in Nsyílxcen/Okanagan, ‘the absence of a definite/indefinite contrast among determiners is apparent’. An indefinite, nonspecific usage of Nsyílxcen *ʔiʔ* is illustrated in 64.<sup>36</sup>

<sup>35</sup> ‘quoique le système ne soit pas construit sur l’opposition entre défini et indéfini, les anaphoriques lointains équivalent souvent à l’article indéfini de langues comme le français ou l’anglais, du fait des implications mêmes de la notion d’éloignement; symétriquement, les anaphoriques proches équivalent souvent à l’article défini’ (Hagège 1981:134).

<sup>36</sup> A. Mattina (1973) states that the Nsyílxcen determiner *ʔiʔ* ‘marks a definite complement’. Hypothesis-driven testing by Lyon has revealed that this is incorrect, at least under a standard definition of definiteness.

(64) ʔiɬ-n                      ʔiʔ=qáqx<sup>w</sup>əlx yaʕyáʕt sǰəlǰǰált  
 eat(-DIR)-1SG.ERG DET=fish      all      day  
 ‘I eat (a) fish every day.’

(Lyon 2011:217)

Identical problems also arise with Dryer 2011b, the *WALS* chapter on indefinite articles. The overarching problem is that semantics cannot be done on the basis of descriptive secondary sources, whose data collection methods are not hypothesis-driven in the relevant respects and do not include negative data, and, of course, large-scale typological surveys based on such descriptive grammars can only be as accurate and complete as the original data and generalizations on which they are based. For subtle semantic contrasts such as definiteness and specificity, it is almost never the case that the original descriptions are explicit or comprehensive enough to serve as the foundation for accurate crosslinguistic comparison.

**5.3. SUMMARY.** Our goal in this case study was to investigate the question of whether all languages possess definite determiners. Our C-linguistic methodology led us to adopt the initial, easily falsifiable, hypothesis that Salish languages have definite determiners. This hypothesis made a set of predictions about familiarity and uniqueness; upon being tested, these predictions were not upheld. We found that *Skwxwú7mesh*, *St’át’incets*, and *Nsyílxcen* determiners lack the core semantic properties of definites: they are not restricted to familiar referents, and they do not enforce uniqueness or maximality. The three Salish languages discussed here were obviously not forced into a universalist mold.

Once again, a look at the facts easily debunks the myth of the C-linguist who ‘use[s] off-the-shelf categories arising from specific grammatical traditions, and foist[s] them on all languages’ (L&E:2739). On the contrary, it is D-linguistic methodology (exemplified here by the definiteness chapter in *WALS*) that wrongly classifies Salish languages as possessing definite or specific determiners. Hypothesis-driven research leads to the finding that determiners in Salish have different semantics from those of languages like English. Further hypothesis-driven testing, which unfortunately we do not have space to report on here, has established the subtle, language-internal distinctions that Salish determiners DO make.<sup>37</sup>

A further positive spin-off from hypothesis-driven research on Salish determiners is that the information gained about diversity in determiner semantics impacts our understanding of more familiar languages. For example, the *Skwxwú7mesh* and *St’át’incets* facts suggest that determiners can encode deictic information, like demonstratives do, without enforcing familiarity. This runs counter to a common assumption that deictic features are a sufficient condition for analyzing a determiner as definite (see for example Etxeberria & Giannakidou 2013). The uncoupling of deictic features from definiteness is something we would not have detected if we had looked only at Standard Average European languages; we also would not have discovered it if we had accepted the incorrect claim that Salish languages possess definite articles.

**6. CASE STUDY 4: QUANTIFIERS.** The empirical question addressed in this case study is: ‘Do all languages have noun phrases that must be analyzed as generalized quantifiers—so-called “essentially quantificational noun phrases”?’ Obviously, this question is highly theory-dependent: it could not even be asked without a formal (set-theoretic)

<sup>37</sup> For example, *St’át’incets* determiners further encode ‘assertion of existence’ (Matthewson 1998), and Gillon (2013) argues that all determiners encode domain restriction. These are in addition to the deictic distinctions made by *Skwxwú7mesh* and *St’át’incets* determiners; see van Eijk 1997, Gillon 2009a, Davis 2010a.

account of the meaning of quantificational expressions, in the tradition of Montague (1973).

Our initial hypothesis—originally proposed by Barwise and Cooper (1981)—is that all languages do have essentially quantificational noun phrases. The result after hypothesis testing on St'át'imcets is that NOT all languages have noun phrases that must be analyzed as generalized quantifiers (GQs).

St'át'imcets has several lexical items that receive quantificational translations in English, and that appear at first (and second and third) glance to be very similar to their translational equivalents. Some examples are listed in 65.

(65)	tákəm	'all'
	x <sup>w</sup> ʔit	'many'
	k <sup>w</sup> ík <sup>w</sup> əna?	'few'
	šáq <sup>w</sup> uł	'half'

The question here is whether these elements form noun phrases that can ONLY receive an analysis as GQs (rather than as referential or set-denoting expressions, for example).<sup>38</sup> Without going into analytical details (see Szabolcsi 2011 for an overview), we can outline some predictions about how we expect essentially quantificational noun phrases to behave. First, they should be able to give rise to scopal ambiguities, as illustrated in 66.

(66) A child broke every cup.

Surface scope: There is a child *x*, and for every cup *y*, *x* broke *y*. (only one child)

Inverse scope: For every cup *y*, there is a child who broke *y*. (can be different children)

Second, essentially quantificational noun phrases allow proportional readings. The English element *many*, for example, has a reading where 'Many Ns V' requires a large proportion of Ns to V. This reading is facilitated by a partitive structure, as in 67. If twenty-five out of thirty students passed the test, the sentence is fine; it is decidedly more questionable if twenty-five out of one hundred did.

(67) [Context: 25 out of 30/#100 students in the class passed the test.]

Many of the students passed the test.

*Many* also has a cardinal reading—facilitated in 68 by a *there*-insertion structure—on which it suffices for there to be a large number of Ns. This reading does not require a quantificational analysis; see Milsark 1974, Partee 1988, Szabolcsi 2011, among many others.

(68) [Context: There are 40,000 students at UBC. Yesterday there was a protest rally and 2,000 students turned up.]

There were many students at the rally yesterday.

Scopal ambiguity and proportional readings are each necessary conditions for an essentially quantificational noun phrase, and, together, they constitute sufficient evidence. That is, a noun phrase that participates in scopal interactions and displays proportional readings must be analyzed as a GQ.<sup>39</sup> Our initial hypothesis that St'át'imcets possesses

<sup>38</sup> Following Partee (1990), we are interested in noun phrases that MUST (rather than merely CAN) be analyzed as generalized quantifiers, because it is technically possible to give any noun phrase—including, for example, a proper name—a GQ analysis (cf. Montague 1973).

<sup>39</sup> On its own, scopal behavior is not a sufficient condition, since elements other than GQs (e.g. negation, modals, intensional verbs) participate in scopal ambiguities. Matthewson (1998) assumed (following much prior literature) that proportional readings alone were a sufficient condition for generalized quantifierhood. We see immediately below that this is not correct.

essentially quantificational noun phrases is therefore falsifiable by means of evidence that St'át'imcets noun phrases lack these properties.

**6.1. TESTING QUANTIFIERS IN ST'ÁT'IMCETS.** Matthewson 1998 demonstrates that St'át'imcets DP-internal quantifiers such as *x<sup>w</sup>ʔit* 'many' and *k<sup>w</sup>ik<sup>w</sup>ənaʔ* 'few' have proportional readings. In fact, they ONLY have proportional readings. This is shown in 69 for *x<sup>w</sup>ʔit* 'many': the sentence is acceptable only when a large proportion of the policemen went home.

- (69) ʔúχ<sup>w</sup>al' [ʔi=x<sup>w</sup>ʔit=a plíšmən]  
 go.home [DET=**many**=EXIS policeman]  
 'Many of the policemen went home.' (Matthewson 1998:304)  
 ACCEPTED in a context where twenty-five out of thirty policeman go home.  
 REJECTED in a context where twenty-five out of one hundred policemen go home.

Based on data such as in 69 and the assumption that proportional readings are a sufficient condition for essentially quantificational noun phrases, Matthewson (1998) concludes that St'át'imcets possesses necessarily quantificational NPs.

However, this is not the end of the story. Davis (2010b, 2013) subsequently showed that St'át'imcets quantified phrases do not participate in scopal interactions. Take a look at 70.

- (70) [Context: Four children are meant to read four books over the summer holidays.]  
 [šáq<sup>w</sup>uʔ ʔi=šk<sup>w</sup>úk<sup>w</sup>miʔt=a] paq<sup>w</sup>al' ikšt-mín-itaš [tákəm  
 [half PL.DET=child=EXIS] read-RED-3PL.ERG [all  
 ʔi=púk<sup>w</sup>=a]  
 PL.DET=book=EXIS]  
 'Half of the children read all of the books.'

There are two potential scopal readings of 70, given in 71.

- (71) Reading 1: There is a set of two children, such that those two children read all of the books. (one set of half the children)  
 Reading 2: For every book x, there is a set of two children who read x. (can be different children for each book)

Davis (2010b) shows that in St'át'imcets, NEITHER of these two potential scopal readings exists. This is established by means of scenarios that support one or the other reading; the elicitation techniques include a felicity judgment task responding to visual cues consisting of pictures of children and the books they read. Consider first the situation in 72. In this scenario, 70 should be good on an inverse-scope reading, since for each book, it is true that half of the children read it.

- (72) A reads            B reads            C reads            D reads  
           books 1, 2        books 2, 3        books 3, 4        books 1, 4

However, Davis reports that consultants reject 70 in this context; one commented: 'No, because all of them read something'. This shows that the inverse-scope reading is unavailable.

Now consider the scenario in 73. Davis (2010b) observes that given the scenario in 73, 70 should be false on a wide-scope reading for the subject (since it is not true that there are two children who read every book). However, consultants accept the sentence in this context. One commented: 'Good, because all the books were read'.

(73) A reads	B reads	C reads	D reads
books 1, 2	books 3, 4	—	—

The generalization about 70 is that it is judged good in all situations where exactly two of the children between them read a total of four titles (and bad otherwise). This is a CUMULATIVE reading (Scha 1981). Davis shows for a range of other quantifiers—in fact, for every potential strong quantifier in the language—that systematically, ONLY cumulative readings are available.<sup>40</sup> Importantly, we do not need a generalized-quantifier analysis to generate cumulative readings. These scope tests therefore suggest that, contrary to Matthewson's (1998) proposal, St'át'imcets LACKS essentially quantificational noun phrases.

The results of hypothesis-driven testing on quantification reveal that St'át'imcets quantifying expressions possess one of the criterial properties of GQs (proportional readings), but lack another (scope interactions). We have shown that St'át'imcets noun phrases differ from their counterparts in languages like English, and thus, diversity has been detected.

The study of St'át'imcets quantifiers also has interesting theoretical consequences. Under the assumption that both of the properties we tested are necessary conditions for essentially quantificational NPs, we have to conclude not only that St'át'imcets lacks essentially quantificational NPs, but also that proportional readings alone are not a sufficient condition for such NPs. That is, we can no longer assume that proportional readings and the ability to participate in scope interactions go hand in hand.<sup>41</sup>

**6.2. METHODOLOGICAL ISSUES.** The approach taken here to the investigation of St'át'imcets quantifying expressions was firmly C-linguistic: we assumed an initial hypothesis, based on an explicit, formal theory of quantification, and our empirical testing was driven by this hypothesis. Furthermore, our initial hypothesis in this case was that St'át'imcets would behave like English. It is therefore important to point out that St'át'imcets was not forced into an ethnocentric or universalist mold. On the contrary, we established some subtle, language-internal properties that crucially differentiate St'át'imcets from languages like English. And St'át'imcets actually impacts our understanding of more familiar languages, because it shows that—unlike what it might appear from study of English—proportionality does not necessarily entail GQ-hood.

Not only did the C-linguistic approach to quantification not PREVENT us from discovering diversity, but the precise way in which St'át'imcets quantifiers differ from English ones also was discovered only because of hypothesis-driven testing. We very much doubt that the data in 69–73 would have appeared in even the best descriptive grammar. Consequently, this kind of information is lacking from any typological study based on descriptive sources. We see again that a C-linguistic approach—even when it involves an initial hypothesis of uniformity—is well suited to uncovering diversity.

In fact, our work on Salish is just one example of the crosslinguistic diversity in the domain of quantification that has emerged over the past few decades. Much of the work

<sup>40</sup> English also allows cumulative readings with quantifiers that take plural restrictions, including *half* and *all*. This is unsurprising, since cumulativity is a general property of plural predicates (see e.g. Link 1998); the point is that English also allows scopal readings, which St'át'imcets lacks.

<sup>41</sup> See also Krifka 1999 for this point. Davis (2010b) derives proportional readings within a non-GQ analysis using a choice function that picks out a subset of the set denoted by a plural DP. The result is defined only if the required proportion holds between the subset and the original set.

describing diversity in this area arose as a direct or indirect response to Barwise and Cooper's (1981) hypothesis that all languages possess generalized quantifiers: see the crosslinguistic collections in Bach et al. 1995, Matthewson 2008, and Keenan & Paperno 2011, and see also Benjamin Bruening's 'Scope fieldwork project' (<http://udel.edu/~bruening/scopeproject/scopeproject.html>). Rather than it being an embarrassment that Barwise and Cooper's universal has not stood the test of time (as implied in E&L:431), it is a benefit that they advanced a strong hypothesis with predictions, which other researchers could test. And it is primarily formal linguists who are uncovering and documenting diversity in the area of quantification. Thus, it is hypothesis-driven, C-linguistic research that accurately and convincingly establishes that languages vary in the semantics of their quantifier words.<sup>42</sup>

**7. CASE STUDY 5: MODALITY.** We are concerned with two empirical questions in this case study. First, 'Do modals in Pacific Northwest languages encode distinctions of modality type?'; and second, 'Do modals in Pacific Northwest languages encode distinctions of modal force?'. Before outlining our initial hypotheses, we explicate the terms 'modality type' and 'modal force' with respect to English.

English modal auxiliaries allow varying interpretations, which we refer to as different MODALITY TYPES. An epistemic interpretation for *must* (based on knowledge or evidence) is illustrated in 74a, and a deontic interpretation for *must* (based on rules or laws) is given in 74b. Deontic interpretations are a subtype of CIRCUMSTANTIAL modality (see e.g. Kratzer 1991, Portner 2009).

- (74) a. Sue **must** be in her office (given that her door is open). (epistemic)  
 b. Sue **must** be in her office (from 9 to 5, because she is the receptionist).  
 (circumstantial)

The fact that the same word, *must*, is used for both epistemic and circumstantial interpretations indicates that the distinction between these two modality types is not lexically encoded. The same is true of most English modal auxiliaries (Kratzer 1981, 1991, among many others).

While English modal auxiliaries do not typically encode modality-type distinctions, they do lexically encode MODAL FORCE. The necessity modal claim using *must* in 75a is stronger than the possibility claim using *may/might* in 75b; the speaker is more certain in 75a than in 75b.

- (75) a. She **must** be in her office. (necessity)  
 b. She **may/might** be in her office. (possibility)

The information we have just summarized in a few lines is not a simple surface-obvious taxonomy, but rather is based on a precise and formal analysis of modality rooted in possible-worlds semantics, as pioneered by Kratzer (1981) and worked on during the intervening decades by many researchers. This approach to modality makes empirically falsifiable predictions about which distinctions of modal force or modality type will and will not be found in natural languages. (For reasons of space we do not go into formal details here.) Another important point is that we assume a compositional semantic theory in which semantic values are provided in the first instance by lexical entries of indi-

<sup>42</sup> Everett (2005) makes claims about Pirahã quantification that are partially similar to those made here for St'át'imcets. Matthewson (2012) shows that Everett has not provided the required empirical evidence for Pirahã, and makes suggestions about the hypothesis-driven testing that needs to be done to establish the facts in this language. See also Nevins et al. 2009 for arguments against Everett's proposals about quantifiers.

vidual elements. This becomes relevant when we compare our findings to those of studies with alternative methodologies.

**7.1. MODALITY DISTINCTIONS IN THE PACIFIC NORTHWEST.** Our two subquestions in this case study each require a separate initial hypothesis. The most easily falsifiable hypothesis in each case is that the relevant distinction is encoded. Take, for example, modality type. Suppose we observe a certain modal being used in contexts that strongly favor an epistemic meaning. We then make the initial hypothesis that this modal is strictly an epistemic modal. This initial hypothesis will be falsified if the modal also appears in nonepistemic contexts. Our two initial hypotheses are summarized in 76.

- (76) a. Pacific Northwest modals lexically encode modality type.  
 b. Pacific Northwest modals lexically encode modal force.

Notice that 76a predicts that Pacific Northwest modals will be unlike those of English, while 76b predicts that they will pattern similarly to those of English. Testing these hypotheses leads to the discovery of diversity on both counts: while no modal system is entirely uniform, the general pattern in the Pacific Northwest languages we have looked at is for modality type to be lexically encoded, and modal force to be left up to context (the exact inverse of the English system).

We illustrate a typical Pacific Northwest pattern using St'át'imcets. Consider 77. This sentence, containing the modal *ka*, is accepted in epistemic contexts, where the speaker is reasoning based on some evidence or knowledge that Philomena must or might be in her house.

- (77) wá?=**ka** š-ʔal l=ti=čítx<sup>w</sup>-š=a š=Philomena  
 be=**EPIS** STAT-stop in=**DET**=house-3**POSS**=**EXIS** NMLZ=Philomena  
 'Philomena must/might be in her house.'

Our initial hypothesis in 76a predicts that the modal *ka* cannot also be used in circumstantial contexts. This prediction is upheld: 77 cannot be interpreted as a statement about the rules concerning Philomena's whereabouts, and more generally, *ka* is rejected in circumstantial situations. For example, 78 can be interpreted only epistemically, and cannot be understood to mean, for example, that Henry SHOULD knock. As argued by Rullmann and colleagues (2008), *ka* has ONLY epistemic interpretations.

- (78) níʔ=**ka** k<sup>w</sup>=š=Henry wa? pəŋ<sup>w</sup>pəŋ<sup>w</sup>-č-ám  
 FOC=**EPIS** DET=**NMLZ**=Henry IPFV knock(**REDUP**)-opening-MID  
 'That'll be Henry knocking.' (Rullmann et al. 2008:321)

In contrast to *ka*, the modal *ka* has only circumstantial interpretations, as shown in 79.

- (79) lán=ʔkax<sup>w</sup>=**ka** ʔáčχ-ən ti=k<sup>w</sup>támč-šw=a  
 already=**2SG.SBJ**=**CIRC** see-**DIR** DET=**husband-2SG.POSS**=**EXIS**  
 'You must/can/may see your husband now.' (Rullmann et al. 2008:329–30)

What about modal force? Our initial hypothesis is that *ka* will encode a particular modal force. However, the two possible translations of the modal in 77 already suggest that this initial hypothesis might be false: *ka* appears to allow both necessity and possibility interpretations. In order to fully test this issue, we have to test 77 in a range of discourse contexts supporting either necessity or possibility interpretations (i.e. where the speaker is more or less sure about Philomena's whereabouts), and we also have to test sentences like 80, which would be predicted to be contradictory if *ka* were an unambiguous necessity modal (cf. English #*Elvis must have left, but he mustn't have left*).

- (80) ʔák=**ka**=tu? k=Elvis, ʔu? x<sup>w</sup>?áz=**ka**=ʔu? k<sup>w</sup>=š=ʔak=š  
 go=**EPIS**=then DET=Elvis but NEG=**EPIS**=just DET=**NMLZ**=go=**3POSS**  
 'Maybe Elvis left, but maybe he didn't.' (Rullmann et al. 2008:345)

The results of modal force testing on *ka* reveal that this modal is indeed felicitous in contexts supporting any level of modal force; see Rullmann et al. 2008 for data and discussion. For *ka*, then, the initial hypothesis in 76a is upheld, but that in 76b is falsified.

Further testing, which we do not have space to report on here, reveals that, in fact, the entire modal system of St'át'imcets is organized in the opposite way to that of English: St'át'imcets modals ALL lexically encode modality type, but leave modal force up to context. For full discussion, see Matthewson et al. 2007, Rullmann et al. 2008, and Davis et al. 2009. The differences between the English and St'át'imcets systems are represented in simplified form in Tables 1 and 2.<sup>43</sup>

	NECESSITY	POSSIBILITY
CIRCUMSTANTIAL	must	may
EPISTEMIC		

TABLE 1. The English modal system.

	NECESSITY	POSSIBILITY
CIRCUMSTANTIAL	ka	
EPISTEMIC	<i>ka</i>	

TABLE 2. The St'át'imcets modal system.

A third, 'mix-and-match' type of modal system is exemplified by Gitksan, which encodes modal force only for nonepistemics (circumstantials). Within the circumstantials, it encodes subtypes of modality (Peterson 2010, Matthewson 2013a).

		POSSIBILITY	(WEAK) NECESSITY
CIRCUMSTANTIAL	PLAIN	daʔaqtɬx <sup>w</sup>	sgi
	DEONTIC	ano:q	
EPISTEMIC	PLAIN	ima(?a)	
	REPORTATIVE	gat	

TABLE 3. The Gitksan modal system.

In summary, we see that hypothesis-driven research in the Pacific Northwest has led to the discovery of significant diversity in the distinctions that modal elements lexically encode. The findings reported on here were discovered through targeted fieldwork, designed to test the predictions of formal hypotheses about the meanings of modals. The testing of initial falsifiable hypotheses yielded data that would not otherwise have been detected, and led to the discovery of diversity.

In the next section we again compare our C-linguistic approach to the D-linguistic strategy exemplified in *WALS*.

**7.2. METHODOLOGICAL ISSUES: *WALS* ON MODALS.** *WALS*'s chapter on modals (van der Auwera & Ammann 2011, henceforth vdA&A) engages similar questions to those addressed by the formal research discussed above. However, the methodology used in vdA&A differs from that used by researchers working in the tradition of formal semantics. The generalizations drawn by the authors are based almost exclusively on secondary sources, of which the vast majority are descriptive grammars, and therefore do not involve

<sup>43</sup> Similar formal investigations into modality type and modal force have been conducted for a range of unrelated languages, and a small formal typology is beginning to emerge. It turns out that some languages encode both modality type AND modal force; examples of this include Javanese (Vander Klok 2012) and Blackfoot (Reis Silva 2009).

hypothesis-driven testing of modal semantics. In this section we present an overview of vdA&A's results and compare them to those obtained by hypothesis-driven testing.

The goal of vdA&A is to document 'to what extent languages have identical markers for situational and epistemic modality'. 'Situational' modality is 'distinguished from epistemic modality'—in other words, it is what the formal literature calls 'circumstantial'. Thus, the chapter deals with whether languages lexically encode modality type. Three types of language are distinguished, listed in 81. The language map corresponding to these results can be found at [http://wals.info/feature/76A?tg\\_format=map&v1=cd00&v2=cf6f&v3=cfff](http://wals.info/feature/76A?tg_format=map&v1=cd00&v2=cf6f&v3=cfff).

(81) Categories of language with respect to whether modality type is lexically encoded<sup>44</sup>

**HIGH OVERLAP:** The language has markers that can code both situational and epistemic modality, both for possibility and necessity.

Number of languages: 36

**SOME OVERLAP:** The language has markers that can code both situational and epistemic modality, but only for possibility or for necessity.

Number of languages: 66

**NO OVERLAP:** The language has no markers that can code both situational and epistemic modality.

Number of languages: 105

English is in the 'high overlap' category, since English modal auxiliaries can be used for both situational and epistemic modality, as shown in 74 above. St'at'imcets would be in the 'no overlap' category, since, as shown above, it uses different lexical items for situational versus epistemic modality. The category of 'some overlap' consists of languages that can use the same modal for both situational and epistemic modality (like English does), but only for possibility, while for necessity modals, they have distinct elements for situational versus epistemic. (Or vice versa: they have distinct situational versus epistemic modals for possibility, and merge the two for necessity.) VdA&A gives Hungarian as an example.

At first glance, these results seem to replicate part of what was found in the previous subsection—that languages may or may not lexically encode modality type—and seem even to be superior to the formal research results, because vdA&A's results are based on the study of 207 languages, rather than on the small handful discussed above. However, the methodology employed has a number of flaws, which have led to incomplete and sometimes inaccurate results.

The first set of problems has to do with the definitions of the modal categories. For a start, the categories are too vaguely defined. The 'high overlap' category is defined as follows: 'THERE ARE markers that can express both situational and epistemic modality' (emphasis added). According to this definition, a language would fall into the high-overlap category if even one or two items allowed overlap between modality types. This means that languages in which all modals allow overlap are grouped together with languages that allow overlap in just one or two cases.

The categories are also too broadly defined. VdA&A's three-way categorization misses significant differences among languages whose lexical items do not allow over-

<sup>44</sup> vdA&A differentiates the categories by colored dots (red, pink, and white respectively); we have given them names that correspond to how vdA&A characterizes the categories. 'Overlap' refers to the ability of a single lexical item to be used for both modality types.

lap between situational and epistemic modality. There are at least three distinct subtypes within the ‘no overlap’ category, listed in 82 and presented in schematic form in Tables 4–6. Combining these all into one category overlooks diversity in terms of the ways modal systems are organized.

- (82) **NO OVERLAP-1:** Languages that also have no overlap between necessity and possibility (e.g. Blackfoot and Javanese; see n. 43)
- NO OVERLAP-2:** Languages that have overlap between necessity and possibility, only for epistemics (or only for situationals) (e.g. Gitksan; see Table 3)
- NO OVERLAP-3:** Languages that have overlap between necessity and possibility for both situationals and epistemics (e.g. St’át’imcets; see Table 2)

	NECESSITY	POSSIBILITY
SITUATIONAL	A	B
EPISTEMIC	C	D

TABLE 4. No overlap-1 languages.

	NECESSITY	POSSIBILITY
SITUATIONAL	A	B
EPISTEMIC	C	

TABLE 5. No overlap-2 languages.

	NECESSITY	POSSIBILITY
SITUATIONAL	A	
EPISTEMIC	B	

TABLE 6. No overlap-3 languages.

These problems would be unlikely to have arisen in a typology based on a formal analysis. For example, modal force simply cannot be ignored in a formal analysis—the force a modal has must be explicitly specified in that modal’s lexical entry. So the distinction between modals that are underspecified for force and those that are not (e.g. the difference between modals A/B in Table 5 and modal A in Table 6) would be immediately obvious. Similarly, as formal typologies are forced to consider lexical entries for individual modals (rather than eyeballing the modal system of an entire language), they are much more likely to notice systematic gaps, or subgroupings like those exemplified in Tables 4–6. We see this attention to detail in works on modality that are both empirically and theoretically grounded, such as Nauze 2008 or Vander Klok 2013.<sup>45</sup> Work such as this has an additional advantage: it makes predictions about which modal denotations—and therefore which systems—cannot appear. For example, Nauze proposes the following generalization (2008:222):

Modal elements can only have more than one meaning along a unique axis of the semantic space: they either vary on the horizontal axis and thus are polyfunctional in the original sense of expressing different types of modality or they vary on the vertical axis and can express possibility and necessity, but they cannot vary on both axes.

This proposal rules out a modal system whose elements are underspecified for both modality type and modal force; under *vdA&A*’s categorization, a system of this type would merely be classified as ‘high overlap’.

<sup>45</sup> Nonformal works such as Palmer 2001 also provide useful detail, of course.

Finally, the same problems we saw above with the *WALS* chapters on determiner semantics also arise with research into modals that relies primarily on descriptive grammars. Descriptive grammars are (naturally) not typically based on targeted fieldwork that tests concrete hypotheses about modal semantics. In order to find out whether modals in a language allow both situational and epistemic readings, negative data is essential: if an element is to be analyzed as a purely epistemic modal, for example, it is only half of the story to show that it can be used in epistemic contexts. We also require evidence that the item is impossible in situational contexts. As an example of the shakiness of the empirical ground here, witness vdA&A's categorization of Tlingit (a Pacific Northwest language from the Na-Dene stock) as having 'no overlap' in its modal domain. This classification is PROBABLY largely correct, as shown in careful detail by Cable (2012). However, the sources quoted in vdA&A on Tlingit—one page in each of Swanton 1911, Naish 1979, and Story 1979—are not sufficiently detailed to support any such claim about overlap in modality type. Swanton (1911:193) gives two examples of a modal translated as 'probably' (with no discourse contexts and no negative evidence); Naish (1979:51) lists one example each of two modals described as 'dubitative' (again with neither contexts nor negative evidence), and Story (1979:156) presents no data at all. In none of the places quoted in vdA&A is there any discussion of circumstantial modality. Luckily, as mentioned above, vdA&A's categorization of Tlingit happens to be largely correct, but see Matthewson 2013b for many examples where factually incorrect conclusions have been drawn in vdA&A.

We would like to clarify again that the issue here is not with the descriptive grammars themselves, which may be excellent. The problem arises when data and generalizations are used for purposes for which descriptive grammars are inherently inadequate. In order to establish the range of meaning of a particular modal element, hypothesis-driven semantic fieldwork is required. If that fieldwork has not been carried out, then there is a serious danger of misinterpretation and inaccuracy.<sup>46</sup>

Can the *WALS* modality chapter be fixed? Only if formal, hypothesis-driven research is done on individual languages first. Typology can only be as good as its primary sources.<sup>47</sup>

**8. CONCLUSION.** Levinson and Evans (p. 2739) say that '[w]hat now needs doing is to launch a major new research effort for world-wide linguistic typology'. We agree. However, this research effort needs to be based on accurate information, and we have argued that that such information can only be discovered by first conducting in-depth, formal, hypothesis-driven fieldwork on as many individual languages as possible.

<sup>46</sup> Given these issues, it is troubling that Levinson and Evans regard *WALS* as part of an exciting new explosion in methodologies; they write (L&E:2747):

are we ... going to view our discipline as based on cumulative, explicit data and the development of increasingly sophisticated tools to exploit it? Where linguists have made these investments, as with the *WALS* database, the explosion of new research based on new questions has been spectacular.

But *WALS* is in fact thoroughly traditional in its approach. To an overwhelming extent, it uses the grammatical terms and concepts of Bloomfieldian descriptive linguistics, which have been around for nearly a century.

<sup>47</sup> The accuracy and usefulness of *WALS* could also be increased by adopting a collaborative and self-improving format (akin to Wikipedia). One or two researchers can never hope to control accurate information about 200+ languages. Therefore, it would make sense to allow researchers who are out in the field to have direct influence on the content. (The 'comment' function available on *WALS* almost never leads, in our experience, to revisions.)

In fact, there is already a more Wikipedia-like alternative to *WALS*, the Syntactic Structures of the World's Languages (<http://sswl.railsplayground.net/>), run by a group of C-linguists. This new forum is still in its infancy, but it has the potential to serve as a more useful large-scale typological resource for syntax.

This arguably sets the bar much higher for crosslinguistic research than does the methodology favored by typologists who draw generalizations based on inspection of existing descriptive grammars. As we have shown in our case studies, however, traditional grammars seldom contain any detailed, specific information about syntax and semantics, let alone the kind of hypothesis-driven research that is necessary to answer most meaningful questions in these areas. That is not their purpose.

The result for typology based on descriptive grammatical sources is that some important areas of potential diversity in syntax and semantics are simply ignored, while others are misanalyzed or misinterpreted. The first category includes our case study on variation in condition C effects, which has no counterpart at all in the nonformal typological literature. Our examination of the noun-verb distinction in Salish falls into the second category, where evidence based on surface distribution has led to a long-standing misanalysis of category neutrality, still prevalent in the typological literature. And our three case studies in semantics all reveal serious misinterpretations of the data, due to reliance on insufficiently rigorous and theoretically informed grammatical description.

Fortunately, in recent decades C-linguists have been making substantial progress toward a more reliable, accurate, and well-grounded crosslinguistic typology. Largely, this is due to the emergence of a generation of theoretically informed, methodologically sophisticated fieldworkers working on less-studied languages, who draw equal inspiration from Boas and Chomsky, and who are very conscious that they stand on the shoulders of previous generations of excellent descriptive linguists while deepening and extending the empirical foundations they have inherited.

In our own area of language expertise, the Pacific Northwest, there has been a lively, at times heated, but ultimately very productive conversation between Bloomfieldian descriptivists and generative linguists. These two groups differ in theoretical outlook but are united in their desire to produce as detailed and accurate a picture of the grammars of Pacific Northwest languages as possible, with the looming specter of language extinction a strong motivating factor. The result has been real progress in several areas that had remained unresolved for nearly a century, since Boas first set the agenda for work in the area. A notable example is the current consensus regarding the existence of lexical category distinctions in Salish and Wakashan, following a debate sharpened by the theoretically informed category-neutral hypothesis put forward by Jelinek and Demers (1994) and Jelinek (1995). The latter led directly to testable predictions, subsequently falsified by evidence made available by C-linguistic methodology.

It is important to point out that the Pacific Northwest is by no means unique in this respect. Indeed, in almost any area of the world where minority languages are spoken, we can point to similar positive interactions between descriptivists and theoreticians—so much so, that it is increasingly hard to tell the difference between the two. This is exactly as it should be, but runs counter to the divisive distinction between D-linguists and C-linguists drawn by Levinson and Evans and their caricature of C-linguists as arm-chair theoreticians interested only in their own intuitions about obscure aspects of English syntax.

What, then, should we make of the charges leveled in L&E that C-linguists choose ‘off-the-shelf categories arising from specific grammatical traditions, and foist them on all languages’, a move that ‘typologists and descriptivists deplore ... not only because it does procrustean violence to the basic data, but also because it pre-empts the discovery of new categories and new patterns’ (L&E:2739)? As we have pointed out above, this reflects a misunderstanding of scientific procedure. An initial hypothesis—even one that is based on a well-understood phenomenon in a familiar European language—does

not force an unfamiliar language into a eurocentric mold. An initial hypothesis is a heuristic, designed to generate falsifiable predictions; as we have shown, this procedure can and often does lead to the discovery of crosslinguistic variation, as in our case studies of binding condition C, determiners, quantification, and modals.

Evans and Levinson's complaints about the methodology of generative linguistics are equally groundless. Of course, the caricature they and others draw of C-linguists as relying exclusively on their own linguistic intuitions has never been true of fieldworkers who work with languages that they do not speak natively. But it is equally false to claim that C-linguists in the field rely exclusively on grammatical intuitions, and even more so to claim that they employ them naively. With regard to the first point, C-linguists use any source of data that is reliable and useful: this includes all kinds of naturalistic data, from casual conversation to traditional narratives, as well as many different types of controlled elicitation procedure, including but not limited to those based on grammatical intuitions. As for the second point, only beginning students in fieldwork classes ask for linguistic judgments in isolation, and they quickly discover that the results are unilluminating. In fact, C-linguists have been in the forefront of developing enhanced field methodologies, often employing experimental techniques first developed for use in the study of language acquisition, including various ways of avoiding metalanguage biases by presenting visual stimuli, and providing enhanced context in order to maximize naturalness while still allowing for controlled elicitation. Nevertheless, it is true that syntactic and semantic judgments remain a key component of the inventory of field techniques employed by C-linguists, and unapologetically so, given the fact that they alone are capable of providing the negative evidence that has proven decisive in answering many research questions in the field. The crucial need for negative evidence is illustrated by every one of the five cases discussed here.

One might imagine that, given their criticism of the methodological practices of C-linguists, Evans and Levinson would offer some kind of innovative alternative in E&L, consonant with their claim that 'radical changes in data ... are upon us' (L&E: 2733). They offer no such alternative. Instead, they fall back on the use of corpora—the most traditional of all field methodologies, dating back to the text-grammar-dictionary model established by Boas. Of course, increased computational resources mean that corpora are now easier to compile into databases and to search, but merely increasing the amount of data available and the ease with which it can be accessed does not make up for the well-known inadequacies of the corpora themselves—in particular, the absence of negative evidence, the existence of accidental gaps, and the underrepresentation of rarely attested yet fully grammatical structures. And this is only in terms of their usefulness for syntax; for semantics, they are even less valuable, since they contain no systematic information about possible meanings at all.

For the understudied, endangered languages that are a priority in crosslinguistic research, corpus-based linguistics is even more problematic, given the relatively small amount of textual data available and the limited time available to collect and transcribe more. Here, targeted elicitation of specific structures is critical; it is no good waiting till the relevant utterances turn up in texts, because in all likelihood they never will, and certainly not in the systematic way necessary for syntactic and semantic investigation.

So much for radical changes in the methodology of data collection. What about the theoretical claims that accompany them? As far as we can see, these are either thoroughly revisionist (Boasian relativism, Bloomfieldian descriptivism) or too programmatic to offer any concrete predictions (as with e.g. the claim that 'the funneling properties of coevolutionary selection' can account for relativization strategies; see

L&E:2745). In spite of their use of Darwinian rhetoric, Evans and Levinson offer no falsifiable theory that can serve to generate empirical predictions.

As syntactic and semantic fieldworkers working on languages that face imminent extinction, our task is an urgent empirical one, and we must choose theories that yield the most extensive and reliable data in the fastest and most direct way possible. The hypothesis-driven methodology of C-linguistics has proven consistently superior in this respect. Furthermore, over the last thirty years, C-linguistics has provided exactly the incremental, cumulative progress that is correctly characterized in E&L as the hallmark of a mature science. Within this enterprise, there is now a shared body of theoretical assumptions, knowledge, and methodology that constitutes the toolkit of working syntactic or semantic fieldworkers, irrespective of their specific theoretical adherence.<sup>48</sup>

For example, though no one now believes in the precise details of the original binding conditions of Chomsky (1981), every C-linguist working on anaphora understands the phenomena that they were intended to explain, the problems inherent in the original explanations, and the various solutions proposed since. The fact that the original conditions—and many since—have turned out to be empirically inadequate does not mean that the research questions that they were designed to answer are not worth addressing. On the contrary, the study of anaphora represents one of the richest and most interesting intellectual traditions in generative grammar. And crucially, it has also generated a body of data that provides the fieldworker investigating a less-studied language with a set of testable initial hypotheses, which, as we have seen in our case study on condition C effects, are eminently falsifiable.

How, then, do we go about providing a ‘scientific typology’? The long way. It is at the level of fieldwork on individual languages that genuine progress must be made, by following the scientific method from the outset. As we have shown, reliance on secondary sources designed for other purposes (i.e. traditional descriptive grammars) cannot provide the empirical foundation for a scientific typology in the areas of syntax and semantics. Instead, theoretically informed, targeted elicitation needs to be carried out language by language, family by family, and area by area. Only then will it be possible to ask about the scope and limits of crosslinguistic diversity, on the basis of primary research with ‘its feet more firmly on the empirical ground’ (L&E:2747).

We were partly moved to write this article because, as C-linguists whose careers have been devoted to exploring linguistic diversity, we could recognize neither ourselves nor the languages we work on in the distorted mirror held up to us by E&L. We know we are not alone, and that the conclusions we draw from languages of the Pacific Northwest could be amplified by parallel case studies from anywhere C-linguistic fieldwork is being undertaken. At a time when much of the world’s linguistic heritage is in jeopardy, it seems rash to us to abandon a research paradigm that in a relatively short time has proven so successful in uncovering crosslinguistic diversity. On the contrary, we see C-linguistic methodology as an irreplaceable component of syntactic and semantic fieldwork, whose role is likely to increase rather than diminish as the task of linguistic documentation for minority languages becomes ever more urgent.

<sup>48</sup> Unlike Evans and Levinson, we see no intellectual reason to divide generative models into D-linguistic (lexical-functional grammar, role and reference grammar) and C-linguistic (‘Chomskyan’); the distinction is purely sociological. All generative grammarians share a commitment to formally explicit, predictive models, and as we have shown, C-linguists are just as interested in linguistic diversity as D-linguists.

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[henry.davis@ubc.ca]  
[carrie.gillon@asu.edu]  
[lisa.matthewson@ubc.ca]

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