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Preface

Towards Language Faculty Science: Remarks on the papers collected in Hoji 2013

(※ In this Preface, Chapters 1, 2, 3, etc. of this volume will be referred to as Papers 1, 2, 3, etc., respectively.)

Paper 1: Hoji, Hajime (1995) "Demonstrative Binding and Principle B," *NELS* 25, pp.255-271.

Paper 2: Hoji, Hajime (1998) "Null Object and Sloppy Identity in Japanese," *Linguistic Inquiry* 29-1, pp.127-152.

Paper 3: Hoji, Hajime (1997) "Sloppy Identity and Formal Dependency," *WCCFL* 15, pp.209-223.

Paper 4: Hoji, Hajime (1997) "Sloppy Identity and Principle B," in H. Bennis, P. Pica, & J. Rooryck, eds., *Atomism and Binding*, Foris Publications, pp.205-235.

Paper 5: Hoji, Hajime (1998) "Formal Dependency, Organization of Grammar, and Japanese Demonstratives," *Japanese/Korean Linguistics*, vol.7, pp.649-677, CSLI Publications.

Paper 6: Hoji, Hajime (2003) "Surface and Deep Anaphora, Sloppy Identity, and Experiments in Syntax," in A. Barss, ed., *Anaphora: A Reference Guide*. Blackwell, Cambridge, pp.172-236.

Paper 7: Hoji, Hajime (2003) "Falsifiability and Repeatability in Generative Grammar: A Case Study of Anaphora and Scope Dependency in Japanese," *Lingua*, vol.113, No.4-6, pp.377-446.

1. Introduction

In this Preface, I briefly address what I consider to be the main contribution(s)

of each of the papers collected in this volume. I also address how the papers contained in this volume are related to my earlier works in Hoji 1985 and Hoji 1990 as well as to my recent work in Hoji 2015. The Preface is intended to provide some background information about the papers in this volume. Remarks will also be made on some methodological issues that arise in the course of the discussion.

2. Hoji 1985

The papers in this volume were published during 1995-2003. Papers 1-6 draw heavily from Hoji 1990, which is in turn a continuation of Hoji 1985. In retrospect, Hoji 1985 tried to identify the informant intuitions that are necessarily based on the satisfaction of a c-command condition. I was concerned mainly with the (un)availability of bound variable construal and scope dependency in Japanese that seem to be sensitive to (i.e., seem to require the satisfaction of) a c-command condition.[1] By making reference to the (un)availability of the dependency interpretations in question, I argued for a particular view of the phrase structure of Japanese that it is strictly binary-branching.[2]

My main concern in Hoji 1985 was to express/describe some "phenomena" in Japanese in the terms of the theory I adopted at the time and to consider what could be said about the theory on the basis of my "findings" in Japanese. The empirical generalizations put forth (or adopted) in Hoji 1985 are, however, often far from being robust.

It seems to me to be reasonable to say that my research subsequent to Hoji 1985 started out as an attempt to overcome a major shortcoming of Hoji 1985, namely that the empirical generalizations put forth (or adopted) there are often far from being robust, despite the fact that in much of the subsequent generative research they have been accepted and comprise one of the basic sets

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[1] Strictly speaking, Hoji 1985, unlike Hoji 2015, is not quite committed to aspiring to identify informant intuitions that are necessarily based on the satisfaction of a c-command condition *at LF*. That is reflected by how Hoji 1985 formulates the conditions that are intended to regulate the availability of the relevant dependency interpretation and also by the fact that it addresses Binding Condition D effects presumably as a reflection of properties of the Computational System of the language faculty. This is clearly related to Hoji 1985 being compatibility-seeking research; see the discussion below about the difference between testability-seeking and compatibility-seeking research.

[2] Hoji 1985: Chapter 1, footnote 15 states that "[i]t is not immediately clear at this point how this hypothesis relates to Kayne's (1981, 1984; Introduction) hypothesis that binary branching is the only permissible branching in any language. Insofar as Kayne's hypothesis is independently supported, evidence that supports the binary branching hypothesis in Japanese lends support for his hypothesis." If we adopt Chomsky's (1993) model of the Computational System, where *Merge* is the only structure-building operation, combining *two* items to form *one*, as I do in Hoji 2015, binary branching is its immediate consequence.

of generalizations in Japanese, along with the proposed/assumed structural analyses for the sentence patterns in question.[3]

Focusing on BVA, it has become clear to me over the years that we could replicate the robust informant judgments as schematized in (1) (and other related paradigms) only if we used certain types of expressions for A and B in BVA(A, B).[4, 5]

(1) a. A-ga ... [... B ...]-o ... V-T
with BVA(A, B)
b. *[... B ...]-ga ... A-o ... V-T
with BVA(A, B)
c. [... B ...]-o ... A-ga ... V-T
with BVA(A, B)
d. A-o ... [... B ...]-ga ... V-T
with BVA(A, B)
e. [... B ...]-ga ... A-o ... V-T
B is referential.

The *empirical* thesis I pursued over the years can be summarized roughly as follows: In order to obtain robust informant judgments in line with the patterns indicated in (1), it is necessary to use the "right types of expressions" for A and B of BVA(A, B), and the use of the "wrong types of expressions" results in not-very-robust informant judgments.[6]

In Hoji 1985, I tried to establish paradigms instantiating the generalization in (1), and a similar generalization for DR, with a number of different expressions for A of BVA(A, B) and also for A of DR(A, B).[7] All

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[3] The relevant issues have been extensively discussed in a series of works by A. Ueyama (including Ueyama 1998) and by J.-R. Hayashishita (including Hayashishita 2004); see Paper 7 for a review.

[4] *BVA* is an abbreviation of *bound variable anaphora*, but it is not meant to be a theoretical notion. Although the term BVA comes from "**b**ound **v**ariable **a**naphora," the former should not be equated with the latter. The anaphoric relation that may hold between *some boy* and *his*, for example, is not considered to be an instance of BVA(A, B), but the one that may hold between *even John* and *his* is. BVA(A, B) seems to be a most effective probe if B is singular-denoting and A is not, and that is why I have been focusing on this type of BVA(A, B) in my works including Paper 7 and Hoji 2015. See the Glossary provided at http://www.gges.org/hojiCUP/.

[5] This has been discussed extensively in Ueyama 1998, Hoji et al. 1999, Paper 7, and Hoji 2015. Ueyama 1998 proposes that there are different sources of BVA (and coreference as well) and that it is by focusing on the BVA of a particular source that we can obtain informant judgments in accordance with (1).

[6] Ueyama's (1998) theory of anaphoric relations and her analysis of the OSV order in Japanese provide a theoretical account of this.

[7] What is meant by *DR(A, B)* is a wide-scope distributive reading where *A* takes scope over *B*; see Paper 7: Sections 2.3.3-2.3.6.

the papers contained in this volume are concerned with how we can establish the robust empirical generalizations under discussion, directly (when they deal with BVA(A, B)) or indirectly (when they deal with the sloppy-identity reading).

The evidence presented in Hoji 1985 for the binary-branching thesis for Japanese was based on the distribution of BVA, DR, and coreference. The distribution in question was identified in the terms of, i.e., by adopting, theories that make reference *solely* to c-command. At that time, there had been theories of anaphoric and/or scope dependency based on precedence (combined with some structural relation, such as command, c-command, etc.). If the relevance of precedence had been accepted (along with the relevance of c-command) in the description of the distribution of BVA, DR, and coreference, Hoji's (1985) arguments for the binary-branching thesis for Japanese would not have been possible. It is in this sense that the arguments in Hoji 1985 for the binary-branching thesis for Japanese were circular, as pointed out by Fritz Newmeyer (p.c. 1985).[8]

Reinhart 1983: Chapter 7 suggests that what formally underlies the BVA(A, B) also underlies the sloppy-identity reading and that the availability of BVA(A, B) and that of the sloppy-identity reading are both constrained not only by the c-command condition but also by the local disjointness condition, widely known as Principle B of the Binding Theory. The discussion of the paradigms of the sloppy-identity reading in English in Reinhart 1983: Chapter 7, among other works, prompted me to see whether we could clearly observe the effects of the structural conditions, i.e., the c-command condition and the local disjointness condition, on the availability of BVA and the sloppy-identity reading in Japanese. In Hoji 1990, I continued my attempt in Hoji 1985 to establish the crucial relevance of c-command for BVA(A, B) while at the same time searching for the best choices of A and B for BVA(A, B) and the best choices of the relevant expressions in the sloppy-identity reading context. My concern was how to demonstrate that BVA(A, B) and the sloppy-identity reading (of the "right type") are regulated by the same *structural* and *lexical* conditions. The relevant considerations were directly related to the properties of various sentence patterns and how those sentence patterns should be formally

[8] The circularity issue arises most clearly in Hoji 1985 when we consider its arguments for the binary-branching thesis in Japanese based on the distribution of coreference. But the issue also arises when we turn to BVA/DR. The "reconstruction cases" for BVA/DR, discussed in Hoji 1985, indicate that it *can* arise based on LF c-command not based on precedence. It does not, however, show that BVA/DR in question *must* always be based on LF c-command. It is, in principle, possible for BVA/DR to arise based on either LF c-command *or* precedence, and in the "reconstruction cases," it must be based on LF c-command. I cannot provide further discussion of the relevant issues here because that would take us too far afield. I would, however, like to note that the circularity in question arises ultimately because, as pointed out in Hoji 2015, facts and hypotheses are inseparable in language faculty science; see Section 5 below.

represented.[9]

3. Papers 1-6

Papers 1-6 were written in the context briefly described above. The work in Japanese generative syntax around 1985 was concerned with how one might be able to express some "phenomena" in Japanese in the terms of the theory or theories being pursued at the time and what one might be able to say about the theory or theories on the basis of one's "findings" in Japanese. In the 1980s and the early 1990s, a large portion of the generative research was concerned with "binding," and researchers in Japanese generative syntax tried to see if there might be some "binding"-related "phenomena" in Japanese that could be expressed in the terms of the theory or theories being pursued at the time and if they could suggest something interesting about the theory or theories.[10]

The Binding Theory (as discussed in Chomsky 1981, among other places) was formulated so as to regulate the hypothesized co-indexation relation between two nominal expressions, on the basis of their hypothesized structural relation, by making crucial reference to the hypothesized features of [+/– anaphor] and [+/– pronominal]. One of the three binding conditions/principles applies to anaphors ([+anaphor, –pronominal]) and another to pronouns ([–anaphor, +pronominal]). When efforts were expended to explore how the Binding Theory might apply to Japanese and what theoretical contributions we might be able to make based on Japanese, the correctness was generally assumed of the conception of the Binding Theory in terms of [+/– anaphor] and [+/– pronominal] features and in terms of co-indexation. It was assumed, more in particular, that Japanese has expressions to which the Binding Theory is indeed applicable under its standard conception.

By the time I started preparing Hoji 1990, I had been convinced that there are no expressions in Japanese whose distribution is subject to the binding condition that regulates the expressions with the [+anaphor] feature, even if we allowed the subcategories of anaphors (distinguishing so-called local anaphors and non-local anaphors), and assumed that the binding condition in question applies only to the former. As to pronouns, I addressed in Hoji 1990: Ch. 6 the issue of how the child can learn that a given expression has the [+ pronominal] feature (and for that matter, the [+ anaphor] feature, as well). In part based on the considerations discussed there and in part based on the absence of clear effects in Japanese of the binding condition regulating the expressions with the

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[9] See Paper 7: Section 2.1 for a brief review.

[10] It seems to me to be safe to say that the work of that type was (still may well be) considered "theoretical," even when it did not deduce definite and testable predictions from hypotheses stated in terms of a small number of theoretical primitives.

[+ pronominal] feature, I came to pursue the hypothesis that Japanese does not have expressions that have the [+ pronominal] feature.[11]

In Paper 1, I observed that BVA can arise at least in two distinct ways, and introduced the notions of *Arg-binding* and *Dem-binding*, arguing that the former is subject to the local disjointness condition of Binding Principle B, but the latter is not. I extended the idea to English, looking into cases where we seem to detect local disjointness effects even when the intended bindee is not a pronominal. On the basis of such observations, it is proposed in Paper 1 that so-called Binding Principle B regulates any category (as the dependent term) that does not have the formal [+anaphor] feature rather than regulating categories that have the formal [+pronominal] feature.

Turning to the sloppy-identity reading, I came to realize, while preparing Hoji 1990, that it was not an easy matter to demonstrate that the availability of the sloppy-identity reading in Japanese is subject to the c-command condition. More specifically, I came to understand that many "constructions" in Japanese allow a sloppy-identity reading, irrespective of the satisfaction of the c-command condition. In such "constructions," the sloppy-identity reading obtains even when what corresponds to the "sloppy pronoun" is an inherently "referential" expression, such as a proper noun or an *a-NP*.[12] Such sloppy-identity readings are therefore most likely not based on an LF c-command-based dependency relation. This line of reasoning is supported by the independent observation that such "ellipsis constructions" do not exhibit island effects either, in sharp contrast to the other type of "ellipsis constructions" where the sloppy-identity reading seemed subject to (i) the c-command condition and (ii) the lexical condition that the "sloppy pronoun" must be a *so-NP*.[13]

In the meantime, Otani and Whitman 1991 (O&W) appeared, in which the sloppy-identity reading in Japanese is discussed. I had been convinced by then, on the basis of the research reported in Hoji 1990, that what was considered in O&W as the sloppy-identity reading in Japanese is NOT regulated by syntactic conditions such as the c-command condition and that its (un)availability is subject to various pragmatic factors, and hence, cannot be

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[11] The hypothesis is in line with the widely shared "intuition" that Japanese does not have what deserves to be called "pronouns." Mikami 1953, reprinted as Mikami 1972a,: 52, for example, states that Japanese does not yet have personal pronouns like English *it* and that the *so* paradigm is closest to becoming a personal pronoun like English *it*. See Hoji 1991: note 8, where Kuroda 1965, Fiengo and Haruna 1987, and Kitagawa 1981 are also cited.

[12] For general discussion of the Japanese demonstratives, see Hoji et al. 2003 and the references there.

[13] The clustering of differences between the two types of "ellipsis constructions," which is the main empirical issue in Paper 6, is discussed in Hoji 1990: Chapter 5, Fukaya and Hoji 1999, Fukaya 2007, among other works. Many of the empirical observations reported in the papers collected in this volume are based on Hoji 1990.

regarded as constituting evidence for or against hypotheses about the grammar, if we adopt a categorical conception of grammar; see Hoji 2015: Chapter 3 for relevant discussion. The main goal of Paper 2 was to point that out.

As noted in Paper 2: Section 1, "[t]he main purpose of [Paper 2] is to demonstrate that the NOC [=Null Object Construction] in Japanese cannot be analyzed on a par with VPE [=VP Ellipsis] in English." At the end of its Section 2, Paper 2 states, "Having thus shown that both of the empirical bases for the VPE analysis of the NOC are invalid, I take it to be established that the NOC in Japanese cannot be analyzed as an instance of VPE in disguise, contrary to O&W." Since that was the main purpose of the paper, I could have stopped there. At the time of writing Paper 2, however, I did not have a clear understanding of the significance of the **Schema-based prediction* in the terms of Hoji 2015 (see the Glossary provided at http://www.gges.org/hojiCUP/ and Section 6.2 below), and I thought it was necessary to say something minimally coherent about the source of what Paper 2 calls "the sloppy-like reading" and the property of the "null object" that underlies the "sloppy-like reading."[14]

Among the outstanding issues remaining in Papers 1 and 2 are: (i) whether we can show a clear correlation in Japanese between the effects of the c-command condition and those of the local disjointness condition, (ii) whether it is possible to make *definite* and testable predictions with regard to the availability of the sloppy-identity reading and have them supported by experimental results in a reproducible manner, and (iii) what formal mechanism underlies the local disjointness effects.

Papers 3-5 were written during the same time period. They address the issues just noted. Because of the page limit imposed on each of these papers, what could have been placed in one single and long paper were divided into the three papers, which resulted in some degree of redundancy among the papers, with regard to the discussion of the background issues and the general claims therein. An attempt is made in Paper 6 to synthesize the empirical results in Papers 3-5 as well as those in Paper 2.

One unsatisfactory aspect of the work reported in Paper 2 is that, while the paper successfully demonstrates something is possible, in contradistinction to the claim made in O&W, the paper does not demonstrate that something is clearly impossible due to the formal aspects of the grammar. In Papers 3-5 and also in Paper 6, I tried to make what would later be called **Schema-based predictions* in Hoji 2015. The desire to obtain robust informant judgments (about what is predicted to be impossible) led me to consider increasingly more involved sentence patterns and interpretive possibilities.

Papers 3 and 4, for example, discuss the Mix-reading patterns in "comparative ellipsis" in Japanese. I had clear (enough) judgments on the

[14] As is evident from the discussion in Paper 2: Section 3.3, there is nothing special about the "null object" being an "object" in terms of its formal property. Whatever properties it might have, they should be shared by "null arguments" in general.

relevant sentences myself, and so did my colleagues. But I wanted to substantiate our judgments, so to speak, by making further predictions and by obtaining informant judgments in accordance with those new predictions; see the second paragraph in (4) in Section 0 below. What was crucially investigated in Papers 3 and 4 was how the availability of the sloppy-identity reading is restricted by the LF c-command relation, the locality, and the lexical choices. The thesis pursued there was that the sloppy-identity reading of a certain type is necessarily based on Formal Dependency (FD) and hence its availability is subject to these restrictions. It is in this context that Papers 3 and 4 (and also Paper 6) consider the Mix-reading paradigm. The ultimate test in this regard involves the local disjointness effects in the Mix-reading paradigm.[15]

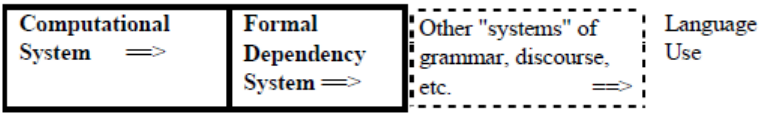
Paper 3 is a continuation of Paper 2 and, to a somewhat lesser degree, of Paper 1 as well. Its main points are:

(2) a. Principle B is a condition on Formal Dependency, rather than on co-indexation.

b. There are at least two types of sloppy identity readings and only one of them is based on Formal Dependency.

c. The so-called "interface between the Computational System and language use" contains the Formal Dependency System, as schematized in (i).

(i)



d. Local disjointness effects that have been attributed to Binding Condition B must be understood as arising from different sources, reflecting different components in the above diagram.

e. *Kare* can be marked [+Dep] in the terms of Hoji 2015.

(2a) is already suggested in Paper 1, but without empirical evidence. Paper 3's empirical evidence in support of (2a) draws from Heim 1992; see Paper 3: (8), (10), and footnote 4. The first half of (2b) is already demonstrated in Paper 2, where it is shown that the so-called Null Object Construction in Japanese (NOC) is not akin to English VP Ellipsis (VPE), with regard to the availability of the sloppy-identity reading. In Paper 2, the NOC

[15] From the perspective of Hoji 2015, the relevant discussion in these papers, including Paper 6, is not satisfactory for the following (related) reasons: (i) it is not clear how our predictions are deduced from universal and language-particular hypotheses; (ii) it is not clear how we can design an experiment, including the Main-Experiment and its Sub-Experiments in the terms of Hoji 2015, so as to obtain results precisely in accordance with our predictions in a multiple-non-researcher informant experiment.

was contrasted with "comparative ellipsis" in Japanese, which seems to "share properties" with English VPE. Paper 3 discusses the *soo su* ('do so') construction in Japanese and shows that it "behaves like" the NOC (and "comparative deletion," which is called the Non-CM-comparative in Paper 6, as opposed to "comparative ellipsis," which is called the CM-comparative in Paper 6). Paper 3 also elaborates on the main point in (2d), which is made in Paper 1, without specific reference to (2c-i).

Multiple sources of the sloppy-identity reading and those of local-disjointness effects, as discussed in Paper 3, should be understood along with multiple sources of BVA and those of DR. I wanted to obtain the most robust generalizations with regard to these "phenomena" in Japanese. I had come to believe that we would have the best chance to do so if we focused on the informant intuitions that are crucially based on the (LF) c-command relation. What led to this belief includes my own experience over the years as a researcher-informant and the conceptual/theoretical reason addressed in Reinhart 1983. I thus tried to identify informant intuitions that are crucially based on the formal relation that is based on (LF) c-command.

The observation that led to (2e) was significant because it had been claimed since the early 1980s (see Paper 1: Section 1 for some early references) that *kare* cannot be used as a bound variable. The validity of the empirical basis for (2e) as discussed in Paper 3, which involves the Mix-reading paradigm, however, has yet to be experimentally demonstrated, especially in a multiple-non-researcher-informant experiment in the terms of Hoji 2015.[16] It is, however, interesting to note that Hoji et al. 1999 points out that it is possible for some speakers to have *kare* as B of BVA(A, B) even in the "reconstruction context," hence where it must be based on LF c-command.[17] The relevant observation thus suggests that *kare* can be β of FD(α, β) at least for some speakers.

Paper 4 focuses on what formal relation Binding Principle B regulates. The evidence adduced there in support of (2a) involves the local-disjointness effects in the Mix-reading paradigm, as compared to the absence of the local-disjointness effects in the same "local context" in other "ellipsis constructions." As in the case of Papers 2 and 3, Paper 4 makes crucial use of "comparative ellipsis" in Japanese.

Paper 4 also suggests an account of why *it recommended it* in English

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[16] That is actually true of most of the empirical generalizations put forth in the papers collected in Hoji 2013; see Hoji 2015 for what we should aspire to attain in our experiments, in accordance with the methodological proposal made there.

[17] It is already noted in Hoji 1991 that *kare* can be B of BVA(A, B) if A is *dono N* 'which N', for example; see Paper 4: footnote 11. (Such instances of BVA(A, B) need not be based on LF c-command but it can be crucially based on a precedence relation, as extensively discussed in Ueyama 1998.) But, while preparing Hoji 1991, I was not aware that some speakers allow *kare* to be B of BVA(A, B) that must be based on LF c-command.

clearly does not allow coreference while its Japanese counterpart readily allows it. This is closely related to (2d). I had been concerned with this issue for some time, starting with Hoji 1990. Some related discussion can also be found in Paper 1. As the exposition in Paper 1 may suggest, I came to be concerned with (2d) initially based on my observation that Japanese lacks local disjointness effects for coreference as observed in English (as in *it recommended it*, for example). It remains a challenge how to account for the clear effects of local disjointness for coreference in English and their absence in Japanese in such a way that the account makes predictions beyond local disjointness; see the second paragraph in (4) in Section 0 below. But the results of multiple-non-researcher-informant experiments in English and in Japanese seem to provide striking confirmation for the difference between the two languages in this regard. Hoji 2015 does not discuss the issue although the relevant experimental results are included at the website accompanying Hoji 2015, in the form of "raw data" for English and in the form of result charts for Japanese.

Paper 5 addresses the lexical condition on FD as well as its two structural conditions (the c-command condition and the anti-locality condition). The three conditions are empirically illustrated together, and in a fairly systematic manner, for the first time in Paper 5. Paper 5 considers only BVA. It addresses (2a), but not (2b); it also elaborates on (2c) and (2d).

Paper 6 was an attempt to grapple with the issues that had remained in Papers 2-5. On the basis of the research that had resulted in Papers 2-5 (as well as Paper 1), I came to realize that the testability and reproducibility we had been able to attain was not nearly as robust as we had hoped. Thus the main methodological concern of Paper 6 was how we could pursue rigorous testability, not compatibility, in dealing with the sloppy-identity reading. Following Hankamer & Sag 1976, Paper 6 recognizes two types of "ellipsis constructions" in Japanese and English, i.e., surface anaphora and deep anaphora, and classifies various "constructions" into one of these two types, on the basis of the operational tests that are designed in accordance with the hypothesized structural and lexical properties of FD, which itself is a hypothesized formal object; see below. The main methodological goal of Paper 6 is to illustrate how we can try to tease apart grammatical and non-grammatical contributions to our linguistic intuitions.

4. Paper 7: A transition to language faculty science

The research during the period between Paper 7 (published in 2003) and Hoji 2015 led me to realize (3):

(3) a. If we want to pursue rigorous testability, we should be engaged in a study of the language faculty rather than language or languages.[18]

[18] Chomsky often uses 'language' meaning the language faculty. What is tacitly included

b. In language faculty science, so-called linguistic phenomena are not the object of our investigation; rather, they are probes in our investigation of the properties of the language faculty.[19]
c. If we take the language faculty as our object of inquiry, we must be an internalist.
d. If we are an internalist, we should be concerned with making and testing predictions about *individuals*.

Paper 7 summarizes the empirical findings in Papers 1-6, focusing on BVA (and to a somewhat lesser degree on DR) but not addressing the sloppy-identity reading. It discusses various correlations of informant judgments regarding the availability of BVA(A, B) and DR(A, B), drawing in part from works by A. Ueyama and J.-R. Hayashishita; see footnote 3. The paper also addresses local disjointness effects. It points out that local disjointness effects with BVA are not as robust as Paper 1 suggests even if we focus on the kind of BVA that is claimed in Paper 1 to be based on FD (Arg-binding in the terms of Paper 1), and proposes a means to attain a more robust experimental result.

5. Hoji 2015

5.1. Introduction

As noted above, the concerns addressed in the papers collected in this volume and subsequent research have led to the methodological proposal in Hoji 2015, which explores how we can aspire to accumulate knowledge about the language faculty in line with Feynman's statement "The test of all knowledge is experiment." The two pillars of the proposed methodology for language faculty science are the internalist approach advocated by Chomsky and what Feynman calls the "Guess-Compute-Compare" method. Taking the internalist approach, the book is concerned with the *I-language* of an *individual* speaker. Adopting the Guess-Compute-Compare method, it aims at deducing *definite* predictions and comparing them with experimental results.

It is hypothesized, in Chomsky 1986 among many other places, that the *language faculty* in its initial state is uniform across the members of the species and that, in its steady state, where its non-trivial "growth" has stopped, it varies in accordance with one's linguistic experience, within the limit imposed by the genetic endowment. Given this, it follows that our hypotheses about the language faculty must be of two types: one is about its initial state and the other

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in (3a) is the suggestion that we should be more explicit about our commitment to the study of the language faculty and that we should avoid using 'language' when we mean the language faculty.

[19] During the writing of the papers contained in this volume, I considered my research as being about linguistic "phenomena" such as BVA, DR, the sloppy-identity reading, etc., rather than about properties of the language faculty.

is about its steady state.[20]

The initial state of the language faculty is uniform across the members of the species; hence, we refer to hypotheses about it as *universal hypotheses*. The steady state of the language faculty varies based on one's linguistic experience, as just noted, but it is hypothesized that most, if not all, of the properties of its initial state remain unchanged in the steady state. Hypotheses about the steady state of the language faculty of an individual speaker must therefore consist of universal hypotheses and hypotheses about the particular consequences of the linguistic maturation that the individual has undergone. If we grossly oversimplify and assume, as is done in Hoji 2015 without addressing the issue, that the speakers of a particular language have undergone the same linguistic maturation, we can call the latter type of hypotheses *language-particular hypotheses*.[21]

The object of inquiry in language faculty science is the language faculty. The language faculty, however, is not directly observable. Moreover, the language faculty as an independent module of the mind is itself a hypothesized concept/object. Language faculty science thus aspires to find out about a hypothesized object by putting forth hypotheses about it. As remarked in Hoji 2015: 5, this makes language faculty science "an extreme case of a theory-laden research program even at its very early stage of development."

The major challenge we face is, therefore, how to attain rigorous testability when dealing with something that is not directly observable. Among the crucial issues is what can be regarded as being revealing about the properties of the language faculty. It should be something that we can *deduce* as a *definite* prediction and that we can identify in our experiments as being *definite*. Otherwise we would not be able to compare the predictions with the experimental results in a *definitive* manner.

Since, by hypothesis, the language faculty relates linguistic sounds/signs and meaning, it seems reasonable to consider the most basic form of an experiment in language faculty science to be one in which the informant is asked whether a given sentence is acceptable under a specified interpretation.[22]

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[20] The same conclusion is drawn in Hoji 2015: 24 based on the consideration that we are concerned with *universal* properties of the language faculty, but we must deal with a speaker of a *particular* language. This consideration is a consequence of the conception of the steady state of the language faculty just reviewed, combined with taking the language faculty as our object of inquiry.

[21] Strictly speaking, and more in line with the internalist perspective, it is perhaps appropriate to refer to the latter type of hypotheses as *individual-speaker-particular hypotheses*, i.e., hypotheses about the properties of the steady state of the language faculty of the *individual speaker*. For now, however, I keep to the gross simplification that the properties of the steady state of the language faculty are shared by all the speakers of a "language."

[22] In this connection, I would like to note that the informant's response to how acceptable s/he finds a given sentence under a specified interpretation cannot be understood as being

As remarked in Hoji 2015: 5, however, "One may wonder how we can make definite and categorical predictions about the judgment of an *individual* speaker of a particular language as a reflection of universal properties of the language faculty and how we can attain experimental results in accordance with such predictions." In Hoji 2015, I "provide answers to these and related questions and illustrate them by making reference to actual experiments." Hoji 2015 is thus an attempt to show how we can make language faculty science a rigorous empirical research program despite its inherently theory-laden nature. According to the proposed methodology, we check hard predictions with hard facts and state the hard facts in a theory-neutral way. "Hard" as it is used in "hard predictions" and "hard facts" here is borrowed from Feynman (1999: 198–199):

> In the strong nuclear interaction, we have this theory of colored quarks and gluons, very precise and completely stated, but with very few hard predictions. It's technically very difficult to get a sharp test of the theory, and that's a challenge. I feel passionately that that's a loose thread; while there's no evidence in conflict with the theory, we're not likely to make much progress until we can check hard predictions with hard numbers.

In other words, Hoji 2015 is "an attempt to show how we can deduce *hard predictions* and how we can identify *hard facts* in language faculty science."[23] In summary, Hoji 2015 offers a conceptual articulation of how we deduce *definite* predictions about the judgments of an *individual* speaker on the basis of universal and language-particular hypotheses and how we obtain experimental results *precisely* in accordance with such predictions.[24]

5.2. The key to deducing *definite* and *categorical* predictions

As to what should count as evidence for or against our hypotheses about properties of the language faculty, Hoji 2015 proposes that we should focus on what is predicted to be impossible and check whether we obtain informant judgments in line with such a prediction in a *reproducible* manner. It is argued

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definite unless it is something like "Completely unacceptable" or "Fully acceptable." A response that falls between these two cannot be regarded as *definite* unless it could be understood in terms of a numerical value, such as "76% acceptable," "35% acceptable," etc. It is an elementary observation that it does not seem possible to assign such a numerical value to an individual informant's acceptability judgment on a given sentence.

[23] The quoted remarks above are from Hoji 2015: 5. The readers are referred to Hoji 2015 for details. The Glossary available at http://www.gges.org/hojiCUP/ should also be useful for getting a general idea about the proposal in Hoji 2015.

[24] In pursuit of rigorous testability and reproducibility, the experimental demonstration in the book is supplemented by the accompanying website which provides the details of every Experiment discussed in the book. The URL of the website is: http://www.gges.org/hojiCUP/.

in Hoji 2015: Chapters 2 and 3 that the key to deducing *definite* and *categorical* predictions about the informant judgment is the recognition of the fundamental asymmetry in [P].

[P] The fundamental schematic asymmetry
  a. The **Schema-based prediction*:
     Every example sentence instantiating a **Schema* is unacceptable with the specified interpretation pertaining to two expressions.
  b. The *^{ok}Schema-based prediction*:
     Some example sentences instantiating an *^{ok}Schema* are acceptable at least to some extent with the specified interpretation pertaining to two expressions.

[P-a] is a universal statement but [P-b] is an existential one. [P-a] can be disconfirmed but it cannot be confirmed while [P-b] cannot be disconfirmed but it can be confirmed.

Without recognizing this asymmetry, it would not be possible to deduce *definite* and *categorical* predictions about the informant judgment and expect them to be supported experimentally. According to Hoji 2015, definite and categorical predictions in language faculty science are about the complete unacceptability of example sentences that instantiate a *Schema, in contrast to those instantiating its corresponding ^{ok}Schema.

The combination of a *Schema-based prediction [P-a] and its corresponding ^{ok}Schema-based prediction [P-b] is called a *predicted schematic asymmetry*. When the *Schema-based prediction has survived a rigorous attempt at disconfirmation and the corresponding ^{ok}Schema-based prediction has been confirmed, the reported judgments by the informants on the relevant *Examples and ^{ok}Examples are said to constitute a *confirmed predicted schematic asymmetry*. It is suggested in Hoij 2015 that the confirmed predicted schematic asymmetry is the smallest unit of fact in language faculty science.[25]

5.3. The key to obtaining *definite* and *categorical* experimental results as predicted

As discussed in Hoji 2015: Chapter 4, the key to obtaining *definite* and *categorical* experimental results in accordance with our predictions (in the form of predicted schematic asymmetries) is a clear understanding of the structure of our prediction-deduction, i.e., what universal and language-particular hypotheses give rise to the predictions in question.

The relevant considerations have led to the recognition in Hoji 2015 that

[25] It is not possible to provide a full discussion of Hoji 2015 here. What is crucially missing in this particular exposition includes the need to invoke a dependency interpretation and its conceptual justification; see Hoji 2015: Chapter 5, Section 5.6.

an experiment in language faculty science must consist of a Main-Experiment and its Sub-Experiment(s).[26] We must also clearly understand what the informant's reported judgments mean for the validity of each of the hypotheses that have given rise to the prediction in question. A Main-Experiment tests for each informant the validity of the Main-Hypotheses of a predicted schematic asymmetry. Sub-Experiments test for each informant (i) the validity of Sub-Hypotheses of a predicted schematic asymmetry and (ii) the reliability and effectiveness of the design of the Main-Experiment such as how we convey the intended dependency interpretation[27] to our informants.[28]

In order to effectively assess the validity of the Main-Hypotheses tested in the Main-Experiment, it is necessary to interpret its results by focusing on the informants whose judgments in the Main-Experiment are significant with regard to the validity of its Main-Hypotheses, i.e., those (i) for whom the Sub-Hypotheses of the predicted schematic asymmetry seem valid and (ii) for whom the instructions, including the intended dependency interpretation in question, seem clear and effective.

Our predictions are not about *every* informant who participates in our Experiment. They are about those informants whose judgments are deemed significant for the purpose of testing the Main-Hypothesis/ses in the Main-Experiment. Crucial reference to the results of Sub-Experiments is for the purpose of making the result of the Main-Experiment as significant as possible with respect to the validity of the Main-Hypotheses tested in the Main-Experiment, and that is analogous to enhancing the reliability and the precision of the experimental device in a physical science. What has led us to recognize Main-Hypotheses and Sub-Hypotheses as well as Main-Experiments and Sub-Experiments is the desire to be able to focus on the validity of (a) particular hypothesis/ses among those that give rise to the predicted schematic asymmetry. It stems from our desire to assign maximal significance to our experimental result with respect to such (a) hypothesis/ses. We want our experimental result to be as significant as possible, regardless of whether it turns out to be in line with our *definite* and *categorical* predictions.

The key to obtaining *definite* and *categorical* experimental results is thus ensuring the reliability of the experimental device as much as possible. It is

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[26] See the Glossary for Hoji 2015 available at http://www.gges.org/hojiCUP/ for what is meant by technical terms such as Main-Experiment and Sub-Experiment.

[27] It is pointed out in Hoji 2015: Ch. 3: Section 3.7 that it is not clear how we can assign significance to the informant judgment on simple (un)acceptability without invoking a dependency interpretation (that is hypothesized to be crucially based on an LF c-command relation), contrary to the common understanding since Chomsky 1955/1975, 1957 that judgments concerning simple (un)acceptability constitute more "basic" data than those that involve a semantic interpretation.

[28] The more empirical evidence we have accumulated in Sub-Experiments in support of Sub-Hypotheses, the more significance we can assign to the result of our Main-Experiment with regard to the validity of its Main-Hypothesis/ses.

imperative that we pay close attention to the effectiveness and the precision of the experimental device in language faculty science, just as it is imperative to do so in a physical science. Unlike in a physical science, however, we do not (yet) have a physical experimental device. That renders it impossible to check the reliability of the design, construction and operation of a *physical* experimental device. What then is an experimental device in language faculty science? It seems reasonable to consider that our informants and our instructions are part of our experimental device in language faculty science. Once we recognize this, it follows that we must pay close attention to the reliability and the effectiveness of (a combination of) each of our informants and our instructions.

What Hoji 2015 suggests is as follows: We can consider the result of our Main-Experiment to be revealing about the validity of its Main-Hypotheses only if we focus on the informants for whom the instructions are clear and effective and for whom the Sub-Hypotheses seem valid, judging from the results of the Sub-Experiments. Interpreting the result of the Main-Experiment without reference to those of its Sub-Experiments would be like conducting experiments without taking necessary care and without necessary checks; see the Feynman quote given in (10) in Section 0.

5.4. Pursuing rigorous testability and identifying facts in language faculty science

Through my research subsequent to Hoji 1985, I have come to think that much of the research in the field of generative grammar does not pursue rigorous testability. This seems to me to have resulted in the general absence of a clear sense of what constitutes progress in the field. I had thought for some time that such a state of affairs was due to the lack of intellectual rigor on the part of the practitioners, including myself. Upon reading Feynman's "Cargo Cult Science" several years ago (included in Feynman 1985), however, I came to think that one of the reasons for what one might call the absence of intellectual rigor and integrity in question is that we do not have a means to determine what the facts are. If we are unable to determine what the facts are, it may not be entirely clear how to be honest and how not to fool ourselves; see the Feynman quotes given in (4) and (5).

I provide some quotations of Feynman's remarks here in hopes that they might give the reader a general idea about the intended points. For a fuller discussion, the readers are referred to Hoji 2015.[29]

(4) "Now it behooves me, of course, to tell you what they're missing. But it would be just about as difficult to explain to the South Sea islanders how they have to arrange things so that they get some wealth in their system. It is not something simple like telling them

[29] I may remove some of the quotations in the final version.

how to improve the shapes of the earphones. But there is *one* feature I notice that is generally missing in cargo cult science. That is the idea that we all hope you have learned in studying science in school—we never say explicitly what this is, but just hope that you catch on by all the examples of scientific investigation. It is interesting, therefore, to bring it out now and speak of it explicitly. It's a kind of scientific integrity, a principle of scientific thought that corresponds to a kind of utter honesty—a kind of leaning over backwards. For example, if you're doing an experiment, you should report everything that you think might make it invalid—not only what you think is right about it: other causes that could possibly explain your results; and things you thought of that you've eliminated by some other experiment, and how they worked—to make sure the other fellow can tell they have been eliminated.

Details that could throw doubt on your interpretation must be given, if you know them. You must do the best you can—if you know anything at all wrong, or possibly wrong—to explain it. If you make a theory, for example, and advertise it, or put it out, then you must also put down all the facts that disagree with it, as well as those that agree with it. There is also a more subtle problem. When you have put a lot of ideas together to make an elaborate theory, you want to make sure, when explaining what it fits, that those things it fits are not just the things that gave you the idea for the theory; but that the finished theory makes something else come out right, in addition.

In summary, the idea is to try to give *all* of the information to help others to judge the value of your contribution; not just the information that leads to judgment in one particular direction or another. (From "Cargo Cult Science," included in Feynman 1985 *Surely You're Joking Mr. Feynman*) (p. 340-341).

(5) "The only way to have real success in science, the field I'm familiar with, is to describe the evidence very carefully without regard to the way you feel it should be. If you have a theory, you must try to explain what's good and what's bad about it equally. In science, you learn a kind of scientific integrity and honesty.

In other fields, such as business, it's different. For example, almost every advertisement you see is obviously designed, in some way or another, to fool the customer: the print that they don't want you to read is small; the statement are written in an obscure way. It is obvious to anybody that the product is not being presented in a scientific and balanced way. Therefore, in the selling business,

there's a lack of integrity." (Feynman 1988: 217-218)

(6) "Another thing I must point out is that you cannot prove a vague theory wrong. If the guess that you make is poorly expressed and rather vague, and the method that you use for figuring out the consequences is a little vague—you are not sure, and you say, "I think everything's right because it's all due to so and so, and such and such do this and that more or less, and I can sort of explain how this works …", then you see that this theory is good, because it cannot be proved wrong! Also if the process of computing the consequences is indefinite, then with a little skill any experimental results can be made to look like the expected consequences." (Feynman 1965/94: 152–153)

(7) "The principle of science, the definition, almost, is the following: *The test of all knowledge is experiment*. Experiment is the *sole judge* of scientific 'truth'." (*The Feynman Lectures on Physics*: 1-1, reproduced in Feynman 1963: 2).

(8) "In general, we look for a new law by the following process. First we guess it. Then we compute the consequences of the guess to see what would be implied if this law that we guessed is right. Then we compare the result of the computation to nature, with experiment or experience, compare it directly with observation, to see if it works. If it disagrees with experiment, it is wrong. In that simple statement is the key to science. It does not make any difference how beautiful your guess is. It does not make any difference how smart you are, who made the guess, or what his name is—if it disagrees with the experiment, it is wrong. That's all there is to it." (Feynman 1965/94: 150)

(9) "It is true that one has to check a little to make sure that it is wrong, because whoever did the experiment may have reported incorrectly, or there may have been some feature in the experiment that was not noticed, some dirt or something; or the man who computed the consequences, even though it may have been the one who made the guesses, could have made some mistake in the analysis. These are obvious remarks, so when I say if it disagrees with experiment it is wrong, I mean after the experiment has been checked, the calculations have been checked, and the thing has been rubbed back and forth a few times to make sure that the consequences are logical consequences from the guess, and that in fact it disagrees with a very carefully checked experiment." (Feynman 1965/94: 150–151)

(10) "Because of the success of science, there is, I think, a kind of

> pseudoscience. Social science is an example of a science which is not a science; they don't do [things] scientifically, they follow the forms—or you gather data, you do so-and-so and so forth but they don't get any laws, they haven't found out anything. They haven't got anywhere yet—maybe someday they will, but it is not very well developed … There's all kinds of myths and pseudoscience all over the place.
>
> I may be quite wrong, maybe they do know all these things, but I don't think I'm wrong. You see, I have the advantage of having found out how hard it is to get to really know something, how careful you have to be about checking the experiments, how easy it is to make mistakes and fool yourself. I know what it means to know something, and therefore I see how they get their information and I can't believe that they know it, they haven't done the work necessary, haven't done the checks necessary, haven't done the care necessary. I have a great suspicion that they don't know, that this stuff is [wrong] and they're intimidating people. I think so. I don't know the world very well but that's what I think." (Feynman 1999: 22-23)

The language faculty is our object of inquiry. But it is what we hypothesize to be underlying our ability to relate linguistic sounds/signs and meaning. The fact that the language faculty, our object of inquiry, is a hypothesized object makes language faculty science an extreme case of a theory-laden research program, even at the earliest stages of its development, as pointed out in Hoji 2015: Chapter 1. One of the concrete proposals in Hoji 2015 concerns how to identify facts in a research program that aims at discovering properties of the language faculty by following Feynman's "Guess-Compute-Compare" method.[30] In other words, Hoji 2015 proposes how we can pursue rigorous testability and reproducibility in language faculty science, despite its highly theory-laden nature.

5.5. Summary

Unless we use certain types of expressions for A and B in BVA(A, B), we cannot expect to obtain robust informant judgments as indicated in (1). This is as expected if there are more than one source of BVA(A, B), and the choice of A and B affects the possibility of the BVA(A, B) of different sources, as discussed in Paper 1 and more in depth in Ueyama 1998. Likewise, we can expect to obtain robust informant judgments about the availability of the sloppy-identity reading, in the form of confirmed predicted schematic asymmetries in the terms of Hoji 2015 only if we focus on a certain type of

[30] As noted in Hoji 2015: Chapter 8: Section 8.2, Feynman's "Guess-Compute-Compare" is for discovering new fundamental laws in physics, not for identifying facts; see also Hoji 2015: Chapter 3: Section 3.8.

sloppy-identity reading. As discussed in Papers 2-6, there is more than one source of the sloppy-identity reading, and the relevant lexical choice affects how the sloppy-identity reading can arise.

It seems reasonable to consider Hoji 1985 as an attempt to identify the informant intuitions that are necessarily based on the satisfaction of the c-command condition. One might suggest that Papers 1-6 were concerned with the nature of the BVA and the sloppy-identity reading that are based on LF c-command, and especially with how to identify the expressions whose use necessarily results in the BVA or the sloppy-identity reading that is based on LF c-command.

One might reasonably regard Papers 1-7 as research which attempts to analyze linguistic phenomena by making recourse to theoretical concepts such as LF c-command, anti-locality, and some lexical property, in terms of which the relevant conditions are formulated. One might then consider the relevant conditions as being on BVA and the sloppy-identity reading, or more strictly, on the type of BVA and on the type of sloppy-identity reading whose availability is contingent upon the satisfaction of those conditions. While working on Hoji 2015, however, I came to realize that I was actually pursuing the possibility that those conditions are on the theoretical/hypothesized formal object, FD, rather than on BVA or the sloppy-identity reading. I was investigating the properties of FD (and ultimately, what underlies FD and other theoretical/hypothesized formal objects). Close examination of BVA and the sloppy-identity reading was for the purpose of finding out about FD (and about the CS). As it has in fact turned out, particular choices of A and B for BVA(A, B) do not necessarily result in robust judgments for every speaker even if they do for most speakers, and there are judgmental fluctuations among speakers and even within a single speaker. If linguistic phenomena were our object of inquiry and if we crucially rely on informant judgments on *specific* Examples in our investigation, it would, therefore, be impossible to deduce *definite* predictions about the *individual* informant's judgments about the phenomena in question *and* expect them to be supported experimentally. Experimental replication in language faculty science must therefore be a highly abstract notion, as discussed in Hoji 2015 and as will be discussed more in depth in Hoji in preparation. A clear articulation of the concept of experimental replication in light of this is therefore of critical importance for the demonstration of the viability of language faculty science as outlined in Hoji 2015.

6. Evaluating the papers collected in this volume in light of Hoji 2015

6.1. Introduction

It is useful to evaluate the papers collected in this volume in light of the methodological proposal advanced in Hoji 2015. For each paper, we can ask

whether and how it makes a *definite* and *categorical* prediction. In the terms of Hoji 2015, we can ask whether it offers a predicted schematic asymmetry, and if it does, what universal and language-particular hypotheses give rise to it.[31] We can also ask whether the prediction is experimentally supported, i.e., whether we obtain a confirmed predicted schematic asymmetry in the terms of Hoji 2015. To put it in somewhat concrete terms, whenever we see an example sentence that is claimed or assumed to be unacceptable (with the specified interpretation), we can ask the questions in (11)-(13).

(11) The fundamental schematic asymmetry:[32]
   a. What is the *Schema that the example sentence in question instantiates?
   b. What is the corresponding ^{ok}Schema?

(12) The prediction-deduction:
   What universal and language-particular hypotheses make the *Schema and ^{ok}Schema in (11) a *Schema and an ^{ok}Schema, respectively?

(13) Experimental results:[33]
   a. Does the *Schema-based prediction survive a rigorous attempt at disconfirmation? That is to say, is any sentence that we can construct instantiating the *Schema completely unacceptable (under the specified interpretation), no matter how hard we try to make it acceptable?
   d. Is the ^{ok}Schema-based prediction confirmed? That is to say, can we construct a sentence instantiating the ^{ok}Schema that is more or less acceptable (under the specified interpretation)?

Trying to answer such questions would be a useful exercise for the

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[31] See the Glossary available at http://www.gges.org/hojiCUP/.

[32] I argue in Hoji 2015 that our predictions are not about specific example sentences but about schemata that specific example sentences instantiate as indicated in [P] in section 5.2.

[33] As briefly discussed in Section 5.3 above, an experiment in language faculty science consists of a Main-Experiment and its Sub-Experiments, reflecting the structure of the prediction-deduction and more specifically, how each fundamental schematic asymmetry (see [P] in section 5.2, for example) tested in the Main-Experiment is deduced. The result of the Main-Experiment is to be considered in light of the results of its Sub-Experiments. Reproducibility in language faculty science can be pursued at different levels, including across-Example and across-occasion reproducibility within a single-informant, across-informant reproducibility, and across-language reproducibility. Depending upon the type and the number of the informants, our experiment can be: a single-researcher-informant experiment, a multiple-researcher-informant experiment, a multiple-non-researcher-informant experiment, etc. See the Glossary available at http://www.gges.org/hojiCUP/.

purpose of evaluating a given paper with regard to its potential contribution, and/or its relevance, to language faculty science. Regardless of its relevance to language faculty science, addressing such questions will help us understand what testable predictions are made under what hypotheses and how explicitly each of those hypotheses is formulated[34]

6.2. General remarks

I will now make brief, and not particularly systematic, remarks on the papers collected in this volume from the perspective of Hoji 2015. The discussion is not intended to be self-contained because it is not possible to fully illustrate here the methodology for language faculty science proposed in Hoji 2015. I would like to refer the reader to Hoji 2015. The accompanying website (http://www.gges.org/hojiCUP/) provides some information about Hoji 2015, including the designs and the results of every Experiment discussed in Hoji 2015.

In the terms of Hoji 2015, Hoji 1985 tried to identify as good a probe as possible in discovering the universal properties of the language faculty through the investigation of Japanese, and used the probes thus identified to argue for the thesis that the Japanese phrase structure is strictly binary branching. I was not thinking in those terms when I wrote Hoji 1985. But this now seems to me to be a reasonable interpretation of what I was trying to do in Hoji 1985.

It may be interesting to note that there seems to be a general tendency in the generative tradition that when one works on a language other than English, one addresses generalizations in her/his language in relation to what seem to be analogous generalizations in English. There is nothing inherently wrong with comparing two or more languages. But if the comparison or the claimed analogy is based on shaky empirical grounds, it is unclear what genuine insight we can expect to obtain about what formally underlies the intuitions of the speakers of the different languages in question. One might suggest that loosely "established" "generalizations" in a *number of* languages *can* lead us to an insight into general properties of language. It is unclear, however, how one can pursue rigorous testability in such research if, as argued in Hoji 2015, rigorous testability is closely related to the deduction of definite predictions and experimental testing of the definite predictions.[35]

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[34] As noted at the outset of this Preface, the research reported in the papers collected in this volume is a continuation of Hoji 1985. It would, therefore, be interesting to assess Hoji 1985 from the perspectives of Hoji 2015. Because the research orientation of Hoji 1985 seems to be influenced by that of Saito and Hoji 1983, it would also be interesting to assess Saito and Hoji 1983 from the perspectives of Hoji 2015. I plan to do so when I prepare the Preface to the e-edition of Hoji 1985.

[35] One can argue that testability can be pursued without deducing definite predictions, by focusing on correlations of things and measuring the correlations by means of the significance test. Such a view might be a consequence of (i) not taking the language faculty as the object of inquiry and/or (ii) not aspiring to find out about one's subject

If one "analyzes" a certain linguistic "phenomenon" in Japanese, for example, as being analogous to a phenomenon in English which has been characterized in terms of highly theoretical notions, one's long-term contribution depends in part upon (i) how robust the alleged generalization is and (ii) how the theoretical account/characterization of the generalization is motivated independently of the "phenomenon" under discussion. The parasitic-gap analysis in Hoji 1985: Ch. 2 of what would later be called the A-Scrambling construction in Japanese seems to be a good example of making an analogy of some loosely understood "phenomenon" in a non-English language to a phenomenon in English that is analyzed in highly theoretical terms.[36] If the descriptive generalization in English itself is not as robust as one wishes it to be, and if aspects of the theoretical characterization of the phenomenon are not independently motivated on empirical grounds, the "theoretical characterization" of the loosely understood phenomenon in Japanese is bound not to survive the test of time.[37]

Suppose that one's theoretical characterization of a phenomenon in Japanese were based on a solid empirical and experimental basis, in the form of *confirmed predicted schematic asymmetries* in the terms of Hoji 2015. That would mean that we have a prediction in the form of a *predicted schematic asymmetry* that is deduced from universal and language-particular hypotheses and that we have obtained experimental results precisely in accordance with such a prediction.[38] That would in turn mean that we now have an empirical basis that is almost entirely independent of a particular conception of grammar; see Hoji 2015: Chapter 3: note 33 and the discussion in the text thereabout. If the theoretical characterization of the phenomenon in English changes (over time), we would therefore be in a good position to check the empirical consequences of the theoretical change; hence, we might be able to tell whether or not the change in question is *progressive* in the terms of Lakatos 1970.

Rigorous testability can be pursued only if our hypotheses give rise to predictions that are definite and categorical. Likewise, in order to pursue rigorous reproducibility (among informants as well as within an informant), we

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matter by what Feynman dubbed as the "Guess-Compute-Compare" method. I cannot discuss the issue further here. See Hoji 2015: Chapters 1 and 2 for some relevant discussion.

[36] The parasitic-gap analysis in Hoji 1985: Chapter 2 in question is a good example of compatibility-seeking research; see Section 7 below for remarks on testability-seeking and compatibility-seeking research.

[37] Ueyama's (2003) presentation of the various properties of the so-called Scrambling construction in Japanese in terms of what need to be *minimally assumed*, contrasts sharply with the kind of research presented in Hoji 1985 just alluded to, and seems to me to represent what one should pursue if one wants to maximize testability and the chances of learning from errors, as addressed in Popper 1963, for example, and discussed in Hoji 2015: Chapter 4: Section 4.1 in reference to language faculty science.

[38] See Section 5 for the terms from Hoji 2015 just mentioned.

must deal with something that is observable and is definite and categorical.[39] This is certainly true if we want to deduce *definite* predictions about an *individual* informant from our hypotheses about the language faculty.

In the terms of Hoji 2015, we can consider Papers 1-7 as an attempt to identify as good a probe as possible in discovering the universal properties of the language faculty through investigation of Japanese. If we take FD as our object of inquiry, we can try to determine what might be a good probe for investigating the properties of FD for a given informant, and for a given experimental set-up. We can consider this as a shift from analyzing linguistic phenomena in terms of theoretical concepts to studying the nature of a theoretical (i.e., hypothesized) object by means of linguistic phenomena. Hoji 2015 articulates a conceptual and methodological basis for how we can do the latter and expect our predictions to be supported empirically, and it also provides experimental demonstration in support of the viability of the proposed methodology.[40]

6.3. Paper 1

From the perspective of Hoji 2015, the testability of one's research on the Binding Theory can be attained only if we can specify how co-indexation is related to the interpretation detectable by the informant insofar as the Binding Theory is formulated in terms of co-indexation.[41] Suppose that, as in its standard conception, the Binding Theory regulates the co-indexation relation between two expressions, rather than the possibility of an anaphoric relation between the two. If it is possible for the two expressions to be used to refer to the same entity/individual without being co-indexed, however, it is necessary to specify what testable consequences we can obtain from the co-indexation between two expressions and the lack thereof. What is typically offered by practitioners is that we can focus on "intended coreference" (as opposed to "accidental coreference"). It is, however, not very clear how we can convey to our (naïve) informants, or expect them to be able to determine, whether a particular interpretation is "intended" or "accidental." We cannot ask our naïve informants "Is this grammatical with the indicated co-indexation?," even though that was a rather common way for a researcher to ask his/her colleagues about their judgments in a theoretical discussion. It seems to me that most works, including Hoji 1985, proceeded with the understanding that either the issue was

[39] See the Glossary available at http://www.gges.org/hojiCUP/ for various notions of reproducibility in language faculty science.

[40] A full discussion of the relevant conceptual issues or the experimental demonstration was not provided in Hoji 2015 due to space considerations. I am preparing a book in which I provide further conceptual arguments and experimental demonstration for the main thesis of Hoji 2015.

[41] Lasnik 1981 and Reinhart 1983: Chapter 6 are among the few works (that I know of) that were explicitly concerned with this issue.

not serious enough or it could/would be resolved as our research progressed.

Furthermore, *rigorous* testability can be pursued only if we can identify, independently of binding-theoretic considerations, what expressions in the language in question have the [+anaphor] feature or the [+pronominal] feature. It is also imperative that we try to motivate the structural properties of a particular language under discussion that the binding conditions make crucial reference to, again independently of binding-theoretic considerations.

The testable claim made in Paper 1 (a *Schema-based prediction in the terms of Hoji 2015) can be stated as in (14).

(14) *QP-ga β-cm V (under BVA(QP, β))
where QP is not of the form *dono N*

I have subsequently learned, however, that Examples of the form in (14) are not always unacceptable for many informants, including myself, even if we focus on the binder-bindee pair (i.e., the choice of the QP and β for BVA(QP, β)) that results, for a given informant, in confirmed predicted schematic asymmetries as indicated in (1). This is acknowledged in Paper 7 Section 4.1: footnote 67, where it is suggested that we can obtain a confirmed predicted schematic asymmetry, in the terms of Hoji 2015, if we employ a different *Schema than (14). The judgments reported there, however, have yet to be replicated in a multiple-non-researcher informant experiment by the experimental method proposed in Hoji 2015.[42]

6.4. Paper 2

In accordance with the methodology proposed in Hoji 2015 for language faculty science, the confirmation of an ^{ok}*Schema-based prediction* alone does not constitute a fact in language faculty science. An ^{ok}Schema can be considered as being part of a fact in language faculty science only if it is combined with the corresponding **Schema*; see Section 0 for a brief discussion of the proposal in Hoji 2015.

If we do not have a confirmed (predicted) schematic asymmetry, we do not (yet) have a fact to explain, according to Hoji 2015. The inclusion of Sections 3 and 4 in Paper 2 was due to the lack of a clear understanding of this point, and especially the significance of the *Schema-based prediction. I might venture to say that it was also due to the fact that I was still a linguist then, not a language faculty scientist. When a linguist demonstrates that an alleged generalization is not valid by showing that there are acceptable *Examples instantiating the *Schema that is part of the alleged generalization, s/he often encounters a reaction like the following: "Okay. You have shown that the

[42] The difference between English and Japanese regarding local disjointness effects of coreference, as addressed in Paper 1, on the other hand, receives striking confirmation from the results of multiple-non-researcher-informant experiments in English and in Japanese, as noted toward the end of Section 3.

generalization turns out to be not valid. But there is a contrast, at least to some degree and at least for some speakers, between the *Examples and the corresponding ^ok^Examples that originally motivated the generalization. You have shown that the generalization has exceptions and you have concluded, on the basis of that, that the hypotheses that accounted for the generalization should not be accepted as they stand. But what is your alternative account of the contrast that the generalization in question points to?" The linguist tends to feel compelled to respond. The inclusion of Sections 3 and 4 in Paper 2 seems to me to be largely due to this tendency of a linguist in a context like that.

Papers 1-6 occasionally contain discussion of English paradigms. They often serve as a basis for discussion of the Japanese paradigms.[43] One might suggest that the inclusion of the discussion of English paradigms in Papers 1-6 were prompted by the absence of the rigorous testability-seeking research orientation and the lack of the strong internalist commitment; see the remarks in Section 0 above regarding the "comparative research" as practiced in Hoji 1985. That seems to me to be a basically accurate characterization of the research orientation pursued in Papers 1-6. But the remark at the end of Paper 2: Section 1, "…in what follows I will refer mostly to O&W [which deals with Japanese] rather than to Huang 1988, 1991, only because I cannot evaluate the relevant data in Chinese in the way I have been able to evaluate the relevant data in Japanese," seems to indicate that I already had the internalist inclination although I was not as committed to it as I am now and I did not know at that point how to try to pursue rigorous testability in research that deals with language or the language faculty.

6.5. Papers 3 and 4

Papers 3 and 4 address the sloppy-identity reading and the Mix-reading pattern in "ellipsis constructions" in relation to the points listed in (2).

From the perspective of Hoji 2015, I should note that there is inherent difficulty in designing an experiment dealing with the sloppy-identity reading. Even if we deal with the simple cases of the sloppy-identity reading—e.g., cases that do not involve the Mix-reading pattern—it will be significantly more difficult to design an experiment dealing with the sloppy-identity reading than one dealing with BVA. The reason has to do with the fact that our experiment necessarily consists of a Main-Experiment and its Sub-Experiments; see Sections 0, 0, and 0 above. Our Experiments test a predicted schematic asymmetry. A predicted schematic asymmetry is given rise to by a set of hypotheses; see Section 0. Crucial among the hypotheses is one that specifies how the phonetic sequence in question is "represented" in the mind of the informant; more technically put, what LF representation(s) the phonetic

[43] English paradigms were sometimes discussed in light discussion of Japanese paradigms, such as the Dem-binding discussion in Paper 1 and the discussion of some of the local disjointness effects in Paper 6.

sequence corresponds to. Such specification is directly related to what condition(s) is/are or is/are not satisfied in the LF representation in question.

The Experiment on the sloppy-identity reading must involve at least two sentences at a time, and the notion of parallelism is crucially invoked. It is therefore necessary to have hypotheses not only about the LF representations corresponding to each of those sentences but also about how the two LF representations are related, as it pertains to the notion of parallelism. This makes it qualitatively more difficult to design an effective experiment dealing with the sloppy identity reading than one dealing with BVA (or any other dependency interpretation) in a single sentence.[44]

The Mix-reading pattern, discussed in Paper 3 and Paper 4, poses an additional problem because of the complication of the relevant judgments and also because of the lack of hypotheses about what leads to the (un)availability of the Mix-reading pattern that can serve as a basis for designing an effective experiment which consists of a Main-Hypothesis and Sub-Hypotheses.

In order to be able to use the sloppy-identity reading as a good probe into the properties of the CS, we must therefore have a minimal articulation as to how we can design an experiment dealing with the sloppy-identity reading that consists of a Main-Experiment and its Sub-Experiments, and how the result of the Main-Experiment is to be interpreted on the basis of the results of its Sub-Experiments. It is hoped that the articulation provided in Hoji 2015 will serve as a good basis for our future research in this domain and help us obtain confirmed predicted schematic asymmetries involving the sloppy-identity reading.

6.6. Paper 5

The main concern of Paper 5 is the three conditions on FD, two structural and one lexical. In the terms of Hoji 2015, Paper 5 used BVA as a probe for testing the hypotheses in question. As discussed in Hoji 2015 and also in Paper 7, the choice of LG (e.g., α and β in BVA(α, β)) affects the effectiveness of the probe for a given informant. What is predicted is not about the individual informant's judgments on specific Examples instantiating a particular Schema. Rather, it is about the correlations of the individual informant's judgments "across" the three conditions and "across" different LGs (and in some cases "across" different SGs).[45] The significance of the correlation of

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[44] When dealing with a single sentence, the researcher can try to minimize the effects of the lexical choices and check sentences in which non-sense words are used in the parts of the Schemata that are not crucial for the testing of the hypotheses in question. We can obtain clear judgments despite (and sometimes because of) that. If we dealt with the sloppy-identity reading, that would be very difficult (if not impossible) to do. That is because, as noted, the sloppy-identity reading is crucially related to the notion of parallelism holding between two sentences and it is difficult to determine the parallelism in question without having some pragmatic context specified.

[45] See the Glossary available at http://www.gges.org/hojiCUP/ for "LG" (=Lexical Group)

judgments is addressed in Paper 7. However, in Paper 5, it is not articulated how we can obtain correlations of judgments in the terms of confirmed predicted schematic asymmetries.

Hoji 2015 offers experimental demonstration of the correlation of judgments between the lexical condition and the LF c-command condition on FD. Hoji in preparation tries to address the correlation of judgments across more dimensions, including the anti-locality condition on FD, as well as different LGs and different SGs, and provides further support for the claim made in Paper 5.

6.7. Paper 6

Paper 6 tries to deal with something that has definite and categorical properties by focusing on the sloppy-identity reading in surface anaphora, rather than in deep anaphora. FD is hypothesized as a formal object underlying the sloppy-identity reading observed only in surface anaphora.

In Paper 6 a number of operational tests were applied to various "ellipsis constructions." It seems safe to say that the main concerns of Paper 6 were with the logical issue of testability. The relevant hypotheses were tested in a *single-researcher-informant experiment* (with myself being the informant) and in *multiple-researcher-informant experiments* of a rather limited scale, in the terms of Hoji 2015. Once one tries to design an experiment to test the empirical predictions made in Paper 6 with regard to the Mix-reading pattern, one quickly understands that it would be quite a challenge to design a Main-Experiment and its Sub-Experiments and obtain a confirmed predicted schematic asymmetry in the Main-Experiment in a multiple-informant experiment. This stems from the difficulty in articulating what universal and language-particular hypotheses lead to definite and testable predictions about the *individual* informant's judgments about the Mix-reading pattern. When I started conducting on-line experiments in 2004, in an attempt to replicate robust judgments among informants, I decided not to deal with the sloppy-identity reading because of the additional complications that such an attempt would invoke.

6.8. Paper 7

Paper 7 is a methodological paper. The methodological nature is much more transparent in Paper 7 than in Paper 6. For an empirical illustration of the viability of its methodological proposal, Paper 7 critically examines the empirical arguments that have been adduced in support of Hoji's (1985) binary-branching thesis for Japanese. Paper 7 adopts Ueyama's (1998, 2003) analysis of the so-called Scrambling, i.e., OSV, in Japanese. The paper, however, discusses only a portion of the empirical consequences discussed in Ueyama 1998, 2003. For example, it does not address 'multiple-scrambling'

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and "SG" (Schema Group).

and 'long-distance scrambling' in any depth; but see Paper 7: footnotes 30 and 84 for brief remarks. As noted in the preceding pages, by the time of preparing Paper 7, I had come to the realization that we must focus on the *Schema-based prediction to pursue rigorous testability and reproducibility. Ueyama's analysis of OSV in Japanese, like other analyses in the field, however, does not give rise to a *Schema-based prediction if we limit our discussion, as we do in Paper 7, to the simplex OSV, i.e., the OSV without involving an embedded clause or 'multiple scrambling'. In order to make a *Schema-based prediction with regard to the simplex OSV, I addressed "resumption" in Paper 7.[46]

The page limit allowed Paper 7 to address only a portion of the empirical materials that I had worked on prior to its writing. When I turned to experimental demonstrations of the validity of various hypotheses in Hoji 2015, I had to reduce the empirical coverage even further, because it would take a great deal of time to design and conduct an experiment to test just one predicted schematic asymmetry, and it would take a great deal of space to present its full discussion, including how the predicted schematic asymmetry has been deduced and how the experimental result is interpreted in accordance with the structure of the prediction-deduction. An attempt is made in Hoji 2015, fairly successfully, to replicate the judgments reported in Paper 7 (i) on the basic paradigms in (1), except for (1d), (ii) on the *a*-NP vs. *so*-NP distinction, and (iii) on the 'split antecedence' paradigm. But the validity of a number of empirical claims made in Paper 7 has yet to be tested experimentally in accordance with the methodology for language faculty science proposed in Hoji 2015. The claims made about the 'resumption'-related paradigm is one such case. The local-disjointness-related paradigm is another.

As noted in Section 0, the research during the period between Paper 7 and Hoji 2015 led me to realize (3), repeated here.

(3) a. If we want to pursue rigorous testability, we should be engaged in a study of the language faculty rather than language or languages.[47]
b. In language faculty science, so-called linguistic phenomena are not the object of our investigation; rather, they are probes in our investigation of the properties of the language faculty.[48]
c. If we take the language faculty as our object of inquiry, we must be an internalist.
d. If we are an internalist, we should be concerned with making and testing predictions about *individuals*.

After Paper 7, I started my attempt to obtain reproducible experimental results in accordance with the various empirical generalizations presented in

[46] "Resumption" in Japanese is discussed in Ueyama 1998: Appendix A.2 and Appendix B.1, Ueyama 2003 and Kataoka 2006: Chapter 3, Section 3.3.3.

[47] See footnote 18 appended to the original (3a).

[48] See footnote 19 appended to the original (3b).

Paper 7, initially in a multiple-researcher-informant experiment of a rather limited scale and then in a multiple-non-researcher-informant experiment. Although I was at one point working with the average of the responses by a group of informants, as in Hoji 2006a, 2006b, and 2010, the subsequent recognition of (3c, d) led me to focus on the reported judgments by *individual* informants, eventually leading me to the methodological proposal in Hoji 2015.

Clearly, Paper 7 paved the way to Hoji 2015 in terms of my conceptual understanding of what to pursue in language faculty science as an exact science and also in terms of what would serve as an empirical and experimental illustration of the methodology for language faculty science. The differences between Paper 7 and Hoji 2015 in terms of their articulation of various conceptual and methodological issues are, however, quite substantive. It would take us too far afield to try to present an assessment of the methodological contribution of Paper 7 from the perspective of Hoji 2015.[49] I would like to try to do so on a separate occasion.

6.9. Summary

The following seems to me to be a fair assessment of the empirical claims made in Papers 1-7, from the perspective of Hoji 2015. When a *Schema-based prediction in the terms of Hoji 2015 is addressed and when it is claimed in Papers 1-7 to be disconfirmed, the disconfirmation of such a *Schema-based prediction is replicated, quite clearly and remarkably, in a multiple-non-researcher informant experiment.[50] When Papers 1-7 make their own *Schema-based predictions and predicted schematic asymmetries including such *Schema-based predictions, we obtain results in a multiple-non-researcher-informant experiment that are quite close to confirmed predicted schematic asymmetries for some of those predicted schematic asymmetries. For some other predicted schematic asymmetries made in Papers 1-7, we are not yet in a position to be able to design an experiment to test the validity of the hypotheses that give rise to the predicted schematic asymmetries. Yet for some other predicted schematic asymmetries, the result of a multiple-non-researcher-informant experiment disconfirms their *Schema-based predictions although the experimental results are much closer to the predictions than in the case of the *Schema-based predictions that are claimed to have been disconfirmed in Papers 1-7.

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[49] For example, the term *falsifiability* is one of the key notions in Paper 7, but it is not among the vocabulary addressing the conceptual issues in Hoji 2015. That is a consequence of the conceptual articulation of the methodology for language faculty science as laid out in Hoji 2015.

[50] In Hoji 2015, not many such *Schema-based predictions are discussed although a number of experiments have in fact been conducted to test such *Schema-based predictions.

7. Summary

During 1985-2015, the concern and the focus of my research have slowly shifted, eventually leading to Hoji 2015. During the research that led to the papers collected in this volume, I came to be increasingly concerned with methodological issues, as indicated by the titles of Papers 6 and 7. The shift can perhaps be characterized as being from compatibility-seeking to testability-seeking research.[51]

One might point out that compatibility-seeking research and testability-seeking research are not mutually exclusive. One can *test* the degree of compatibility. The difference between the two I intend here has to do with whether one deduces *definite* predictions and aspires to obtain *definite* experimental results in accordance with such *definite* predictions. What is referred to here as compatibility-seeking research does not aspire to do so. It *typically* proceeds based on rather loose compatibility among various observations, a collection of which is regarded as constituting a generalization, and on a rather loose sense of compatibility between such "generalizations" and the theory under discussion (which in turn is often rather loosely formulated). It *typically* addresses "predictions" that have not been deduced from hypotheses in a rigorous fashion, and the formulation of their hypotheses is *typically* independent of whether the hypotheses lead to *definite* and *testable* predictions.

The difference between testability-seeking research and compatibility-seeking research can also be understood in relation to what is typically considered as supporting evidence (for hypotheses) in each type of research. Testability-seeking research tries very hard to look for ways in which its hypotheses can be shown to be invalid. What constitutes evidence in support of its hypotheses under the testability-seeking research is the definite prediction made under the hypotheses having survived a rigorous attempt at disconfirmation. In order for a given hypothesis to have the chance to receive empirical support, it must be possible for the hypothesis to give rise to a definite prediction, in conjunction with other hypotheses. Therefore, when a hypothesis is put forth under testability-seeking research, one of the first questions to be considered is how it can be put to rigorous empirical test, i.e., how its validity can be tested experimentally, and how the hypothesis can be invalidated. Under this approach, the formulation of hypotheses and even the choice of the specific research topic are severely limited by the desire to seek testability and the desire to deduce definite predictions from hypotheses.

Compatibility-seeking research, on the other hand, does not make

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[51] One might even characterize the shift as being from linguistics to language faculty science. Such a characterization, though perhaps implied by the title of Hoji 2015, may require substantial discussion about the relation between language faculty science as outlined in Hoji 2015 and Chomsky's generative enterprise. The relevant discussion cannot be pursued here because it would take us too far afield; see Hoji 2015: Chapter 1 for brief discussion.

(rigorous) attempt at disconfirmation of the predictions made under its hypotheses. Instead, it typically seeks *confirming evidence* for its hypotheses. What constitutes confirming evidence depends in part upon how rigorously one carries out one's research. But it may be the confirmation of an [ok]Schema-based prediction in the terms of Hoji 2015. Or it may be the identification of some pairs of Examples that seem to exhibit a contrast *in the direction of* what is suggested, though not necessarily deduced, by the hypotheses in question, often despite the fact that it can be easily shown that the contrast in question does not constitute a confirmed predicted schematic asymmetry in the terms of Hoji 2015. The compatibility-seeking research thus seems to lead us "into the swamp of scholasticism—of clever questions and answers which have a tendency of multiplying endlessly; a swamp from which there is no escape once we have slipped in; a swamp over which the paralyzing vapours of the publication explosion hold an eternal sway."[52] There are a number of issues that deserve serious discussion in relation to this, but I cannot pursue the discussion further here.[53]

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[52] What is quoted is from p. 977 of "Replies to My Critics" contained in Volume 2 of *The Philosophy of Karl Popper*, edited by Paul Authur Schilpp, 1974, The Open Court Publishing, La Salle, Illinois, pp. 961-1197. This remark is part of: "Criticism, as we shall see more clearly later, is the lifeblood of all rational thought. But we should criticize any theory always in its best and strongest form; if possible, repairing tacitly all its major mistakes and concentrating on the great, leading, and simplifying ideas. Otherwise we shall be led into the swamp of scholasticism—of clever questions and answers which have a tendency of multiplying endlessly; a swamp from which there is no escape once we have slipped in; a swamp over which the paralyzing vapours of the publication explosion hold an eternal sway." If a given work does not offer a rigorously testable prediction and is built on an (often massive) accumulation of what falls (far) short of a confirmed predicted schematic asymmetry in the terms of Hoji 2015, criticism of such a work seems virtually impossible without it becoming part of the swamp itself.

[53] Some of the issues may provide a starting point of a good case study of the sociological analysis of "scientific" communities in Kuhn 1962.

had the chance to present my ongoing research.

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[54] The formatting has not been made consistent among the references below.

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