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Toward A Scientific Study of the Language Faculty: A proposal and implications

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- This is a somewhat non-technical elaboration of the proposal put forth in my book manuscript *A Foundation of Generative Grammar as an Empirical Science* (henceforth Hoji 2009) and its general significance and implications; it is meant to be an introduction to Hoji 2009.
- It addresses: (i) the general method in science (that I find reasonable and that in fact seems to be a common view/understanding in (mature) science(s)), (ii) the background and the proposal, and (iii) the significance and implications of the proposal.
- The concrete illustration of the proposal is not provided here.

1. *Generative grammar* as the study of the language faculty[2]

- What is the goal of generative grammar?

Chomsky's remarks in *Third Texas Conference on Problems of Linguistic Analysis in English* May 9-12, 1958, published in 1962, seem to point directly to what he had in mind at least around 1958, in my view more directly than what we find in his writings in the 1950s and 1960s and the subsequent years. (The emphases in (1) and (2) are by HH.)

(1) (p. 167)
Hill: If I took some of your statements literally, I would say that you are not studying language at all, but some form of psychology, the intuitions of native speakers.
Chomsky: That is studying language.
Long: I agree with Chomsky and Harris here. Language goes on in the brain, not merely in the throat.
Chomsky: How language fits into the throat is a matter which is quite interesting. *I claim, however, that study of the native speaker's reactions is what all linguists are studying.*[3]

(2) (p. 168)
Chomsky: I don't think such a test eliminates intuition; I think we want our tests to converge on intuition. If you want to eliminate intuition, then I think my absurd procedure is perfectly satisfactory.
Hill: Linguistic intuition is itself a system, almost a complete grammar. If it is good enough, why bother with any other grammar?
Chomsky: *Because I am interested in explaining intuition. If you cannot accept this as the purpose of linguistic study, I am lost. I would like to get a theory which will predict intuitions.*

Minimally, the language faculty must relate "sounds" (and signs in a sign language) and "meanings." A fundamental hypothesis in *generative grammar* is the existence of the Computational System at the center of the language faculty. Since Chomsky 1993, the Computational System is understood in generative research to be an algorithm whose input is a set of items taken from the mental Lexicon of the speaker of a language and whose output is a pair of mental representations—one underlying 'sounds/signs' and the other 'meaning'.

(3) The Model of the Computational System:

[1] One of the differences between the 9/9/2009 version and the 12/4/2009 version is that *confirmed schematic asymmetry* is used in place of *repeatable phenomenon*.

[2] What is contrasted here is "*generative grammar* as the study of language(s)."

[3] There seems to be a typo here. But I am reproducing what is in the volume because what is intended seems clear enough.

Numeration μ => [CS] => LF(μ)
⇓
PF(μ)

Numeration: a set of items taken from the mental Lexicon
LF(μ): an LF representation based on μ
PF(μ): a PF representation based on μ

Following the common practice in the generative tradition since the mid 1970s, let us call the former a *PF* (representation) and the latter an *LF* (representation). The LF and the PF representations in (3) are meant to be abstract representations that underlie a sequence of sounds and its 'interpretation', respectively. Our hypotheses about the Computational System are thus meant to be about what underlies the language users' intuitions about the relation between "sounds" (and signs in a sign language) and "meanings." The main goal of *generative grammar* can therefore be understood as demonstrating the existence of such an algorithm by discovering its properties. Construed in this way, it is not language as an external 'object' but the *language faculty* that constitutes the object of inquiry in generative grammar.

The above characterization of the goal of *generative grammar* is very much in line with Chomsky 1965.

(4) Chomsky 1965
   a. [L]inguistic theory is mentalistic, since it is concerned with discovering a mental reality underlying behavior. (p. 4)
   b. Mentalistic linguistics is simply theoretical linguistics that uses performance as data (along with other data, for example, the data provided by introspection) for determination of competence, the latter being taken as the primary object of its investigation. (p. 193 in a note appended to (4a))

CONCLUSION AT THIS POINT:

- The object of inquiry in generative grammar is what underlies the language users' intuitions about the relation between "sounds" (and signs in a sign language) and "meanings."

  - What does this mean for the use of the researcher's own introspective judgments in hypothesis formation and hypothesis testing? This question raises a number of interesting methodological issues, concerning *repeatability*, cross-linguistic research, etc., and how one answers the question may tell us a great deal about one's research 'orientation'.

2. The Main idea

I would like to explore some consequences of adopting the following general scientific method (as Richard Feynman puts it) for research concerned with the properties of the language faculty.

(5) The general scientific method:
[Guess] — [Computing Consequences] — [Compare with Experiment]

YouTube video: http://www.youtube.com/watch?v=CL0SB4JkXzY
Feynman Part 1 (about 3' 15" from the beginning of the tape)[4]

"I am going to discuss how we look for a new law. In general, we look for a new law by the following process. First we guess it [Writing on the blackboard "Guess."]. Then we compute the consequences of the guess to see what would be implied if this law that we guessed is right. [Writing on the blackboard "Computing Consequences."] Then we compare the result of the computation to nature, with experiment or experience, compare it directly with observation, to see if it works. [Writing on the blackboard "Compare, Experiment."] Compare it directly to observations to see if it works. If it disagrees with experiment, it's wrong. In that simple statement is the key to science. It doesn't make any difference how beautiful your guess is, how smart you are, who made the guess, or what his name is. If it disagrees with the experiment, it's wrong. That's all there is to it."[5]

Feynman continues the above passage by adding "obvious remarks," stating, "When I say if it disagrees with

[4] See Appendix III.

[5] The passage that corresponds to the above is in: Feynman, Richard. 1994. *The Character of Physical Law*. New York: The Modern Library. (p. 150) (The book was originally published in hardcover by BBC in 1965 and in paperback by MIT Press in 1967.)

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."

More in particular, I will propose that the language faculty can be studied with the general scientific method as schematized in (5) and make concrete suggestions as to how.

In order to proceed with our investigation of the language faculty with the general scientific method in (5), we must ensure the following, at least to a minimally satisfactory degree.

(6) a. It is possible to compute the consequences of the "guess."
b. It is possible to determine whether or not the consequences of the "guess" agree with the observations and/or the experimental results.

(6a) and (6b) can in turn be ensured only if (7) and (8) hold, respectively.

(7) The "guess" is part of, or is related to, a larger deductive system.

(8) The consequences of the "guess" are related, ultimately, to something 'observable/measurable'.

Furthermore, we should like to ensure (9) as best as we can.

(9) Disagreement between the consequences of the "guess" on the one hand and the observations and/or the experimental results on the other could lead us to learn something about the language faculty.

The proposal put forth in Hoji 2009 is an attempt to ensure these.

3. Proposal

3.1. The main proposal

The main proposal in Hoji 2009 contains the thesis in (10).

(10) If we want to discover the properties of the Computational System that is hypothesized to be at the center of the language faculty, what I call a *confirmed schematic asymmetry* should be considered as the *minimal unit of 'facts'* for such research; see also (58) below.

A PREVIEW
What is meant by a *confirmed schematic asymmetry*:
A *confirmed schematic asymmetry* consists of a **Schema*-based prediction that has survived a rigorous test of disconfirmation and the corresponding ^{ok}*Schema*-based predictions that have been confirmed and hence it must consist of a **Schema* and the corresponding ^{ok}*Schemas*, and their corresponding **Examples* and ^{ok}*Examples*, and the informant judgments on those examples.

(11) A **Schema*-based prediction:
The informant judgment on α under interpretation $\gamma(a, b)$ is *always* β=0 (i.e., totally unacceptable) for *any* **Example* of a **Schema*.

(12) An ^{ok}*Schema*-based prediction:
The informant judgment on α under interpretation $\gamma(a, b)$ is β=1 (i.e., fully acceptable) for *some* ^{ok}*Example* of an ^{ok}*Schema*.

(13) An ^{ok}*Schema*-based prediction, an alternative formulation:
The informant judgment on α under interpretation $\gamma(a, b)$ is $0<\beta$ (i.e., not totally unacceptable) for *some* ^{ok}*Example* α of ^{ok}*Schema* σ.

In regard to (8), the consequences of the "guess" are to be related, ultimately, to whether or not we are dealing with a *confirmed schematic asymmetry*.

3.2. Some specific aspects of the proposal

I would like to try to go over the following aspects of the proposal.

(14) a. There is an asymmetry between a **Schema*-based prediction and an ^{ok}*Schema*-based prediction in terms of the significance of their failure (to be borne out).
b. The informant intuition is more directly revealing about the properties of the Computational System if it is on the (un)acceptability of a sentence *under an interpretation involving two expressions* than if it is on "simple" (un)acceptability of sentences.

3.3. The *Evaluation-of-Predicted-Schematic-Asymmetry* (*EPSA*) method

The proposed method of evaluating predicted schematic asymmetries, which capitalizes on (14), among other things, is called, i.e., we are now calling it, the method of *Evaluation of Predicted Schematic Asymmetry* (*EPSA*). Hoji 2009 is intended to provide conceptual justification for the *EPSA* method and some concrete illustration of it.

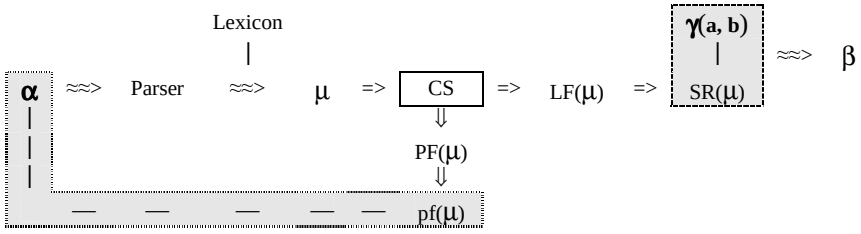
3.3.1. The model of judgment making

➢ For (14a), let us turn to **the model of judgment making** in (15).

The essential aspects of (15) are an immediate consequence of adopting the thesis that the Computational System (see (3)) is at the center of the language faculty.

A Hypothetical Dialogue (I have created) between a typical "generative grammarian" (G) and a naïve outsider (N):
N: So, you are interested in what underlies the language users' intuitions about the relation between "sounds" (and signs in a sign language) and "meanings."
G: Yes.
N: I suppose you construct, or at least test, your hypotheses on the basis of the language users' intuitions.
G: Yes.
N: You should then have some idea about what goes on when the language users or your informants judge a sentence.
G: Well, ...
N: What is then your hypothesis about what the informant does when s/he judges a sentence?
G: ...

(15) The Model of Judgment Making by the Informant on **the acceptability of sentence α with interpretation γ(a, b)**[6, 7] (due to A. Ueyama):



a. α: presented sentence
b. μ: numeration
c. γ(a, b): the interpretation intended to be included in the 'meaning' of α involving expressions *a* and *b*
d. LF(μ): the LF representation that obtains on the basis of μ
e. SR(μ): the information that obtains on the basis of LF(μ)
f. PF(μ): the PF representation that obtains on the basis of μ
g. pf(μ): the surface phonetic string that obtains on the basis of PF(μ)
h. β: the informant judgment on the acceptability of α under γ(a, b)

3.3.2. The informant judgment (I)

[This subsection is included in order to show what attempt(s) have been made and *failed* for providing a semi-formal characterization of the relation between the informant judgment β and the difficulty in parsing and the unnaturalness of the interpretation of the entire sentence in question. What is noted in this subsection is

[6] A numeration is an input to the CS and its output representations are LF and PF, and that is indicated by "==>" in (15). The two arrows before and after *CS* in (15) thus represent the 'is the input of' and the 'yields as an output' relations, respectively. Similarly, what is meant by the arrow between *LF* and *SR* is that *SR* obtains based on *LF*. What is intended by" ≈≈>," on the other hand, is not an input/output relation and are used more loosely, as indicated in (i).

(i) a. α (Presented Sentence) ≈≈> Parser: ... is part of the input to ...
b. Parser ≈≈> Numeration: ... contributes to the formation of ...
c. SR ≈≈> Judgment: ... serves as *a* basis for ...

[7] Hoji 2009: Appendix compares (15) with the model of judgment making suggested in Schütze 1996. Schütze, Carson. 1996. *The Empirical Base of Linguistics: Grammaticality Judgments and Linguistic Methodology*, University of Chicago Press.

definitely an improvement over what is stated in Hoji 2009 as "the model of quantifying the informant judgment," which, I hate to say, is presented in a very misleading and incoherent way.]

The informant judgment β ranges between 0 and 1, with the former corresponding to 'complete unacceptability' while the latter corresponding to 'full acceptability'. β is based on [G], [P], and [I], as indicated below.

(16) β = [G] divided by F, where:
F is a function of [P] and [I] and
$1 \leq F$.

(17) a. [G] is 1 if and only if (i) PF(μ) obtains[8] and (ii) SR(μ) compatible with γ(a, b) obtains; otherwise, [G] is 0.
b. [P] ($0 \leq$ [P]) represents the degree of difficulty the informant 'feels' in 'obtaining' or trying to 'obtain' μ, as it is reflected in β.
c. [I] ($0 \leq$ [I]) represents the degree of unnaturalness the informant 'feels' about SR(μ) compatible with γ(a, b), as it is reflected in β.

The following should be the case in regard to how the values of [P] and [I] affect F.

(18) a. If [P] and [I] are both "0," then F=1 so that β ends up being "1."
b. The larger [P] and/or [I] become(s), the larger F becomes, and the *sufficiently* large value of F makes β *sufficiently* close to "0" but never "0."

Notice that if [G] is "1," β is never "0" although it can be quite close to "0," given that we do not have any limit to how large [P] and [I] can be, as in (17b) and (17c). This formulation of β thus allows us to distinguish between (19a) and (19b), at least, theoretically, although it is most likely not possible to make the distinction when actually observing or feeling the informant judgments.

(19) a. total unacceptability due to [G]=0
b. what appears to be total unacceptability even when [G]=1

In the case of (19a), β is indeed 0. In the case of (19b), on the other hand, β may be infinitely close to 0 but is not 0.

3.3.3. The informant judgment (II)

It seems that the characterization of the relation between the informant judgment β and the difficulty in parsing and the unnaturalness of the interpretation of the entire sentence in question can remain at the level as indicated below, to avoid unwanted and unwarranted implications.

If we characterize [G] as in (20), *making reference to the informant*, we can restate (21a) as (21b).

(20) [G] represents whether or not the informant has found a numeration μ corresponding to the presented sentence α such that the numeration μ results in pf(μ) non-distinct from α and SR(μ) compatible with the interpretation γ(a, b).

(21) a. (=(17a))
[G] is 1 if and only if (i) PF(μ) obtains and (ii) SR(μ) compatible with γ(a, b) obtains; otherwise, [G] is 0.
b. [G] is 1 if and only if the informant has found a numeration μ corresponding to the presented sentence α such that the numeration μ results in pf(μ) non-distinct from α and SR(μ) compatible with the interpretation γ(a, b); otherwise, [G] is 0.

Making reference to [G], we can characterize/state the informant judgment β as in (22).

(22) The informant judgment β:
a. $0 \leqq \beta \leqq 1$, where β=0 corresponds to total unacceptability and β=1 full acceptability.
b. [P] represents the degree of difficulty the informant 'feels' in 'obtaining' or trying to 'obtain' μ, as it is reflected in β.
c. [I] represents the degree of unnaturalness the informant 'feels' about SR(μ) compatible with γ(a, b), as it is reflected in β.
d. If [G]=0, β=0.

[8] It is assumed that the informant's *string sensitivity* ensures that PF(μ) and α are identical. The *string sensitivity* of the informant has to be ensured by preliminary experiments, for example.

e. If [G]=1, β may be 1 but may be lower than 1 due to [P] and/or [I].

What is stated in (22) suffices for the purpose of the discussion in Hoji 2009.

3.3.4. A **Schema*-based prediction and an *^{ok}Schema*-based prediction

(23) (Cf. (11).)
A **Schema*-based prediction:
The informant judgment on α under interpretation γ(*a, b*) is *always* β=0 for *any *Example* of a **Schema*.

(24) (Cf. (12).)
An *^{ok}Schema*-based prediction:
The informant judgment on α under interpretation γ(*a, b*) is β=1 for *some ^{ok}Example* of an *^{ok}Schema*.

(25) (Cf. (13).)
An *^{ok}Schema*-based prediction, an alternative formulation:[9]
The informant judgment on α under interpretation γ(*a, b*) is 0<β for *some ^{ok}Example* α of *^{ok}Schema* σ.

- the informant's *resourcefulness*
- *single-informant experiments* vs. *multiple-informant experiments*

(26) Confirmability and disconfirmability

	Confirmation	Disconfirmation
^{ok}Schema-based predictions	possible	impossible
**Schema*-based predictions	impossible	possible

REVIEW:
A **Schema*-based prediction is β=0 because it is predicted that [G]=0 for any **Example* α of any **Schema* and that should result in β=0. The judgment that α is not totally unacceptable under γ(a, b) (even if not fully acceptable) would therefore disconfirm a **Schema*-based prediction. Notice that such a judgment should mean that, corresponding to α, there *is* SR(μ) compatible with γ(a, b). This in turn should mean [G]=1; if the value of β is lower than 1, that must be due to [P] and/or [I]. While the *marginal acceptability* would disconfirm a **Schema*-based prediction, as just noted, it would be compatible with an *^{ok}Schema*-based prediction in (24)/(25) since 0<β≤1 means [G]=1.

3.3.5. The claim

✓ **Claim**: A *confirmed schematic asymmetry* is **the most basic *empirical* unit of 'facts'** in research concerned with the properties of the Computational System.

We suggest that the informant judgments must obtain as indicated in (27) in order for a *confirmed schematic asymmetry* to obtain.

(27)

	the judgments *necessary* for a *confirmed schematic asymmetry* to obtain
**Examples*	β=0
Corresponding *^{ok}Examples*	0<β≤1

More accurately put, we suggest that the informant judgments must obtain as indicated in (28) in order for a *confirmed schematic asymmetry* to obtain; see Ueyama 2009.

(28)

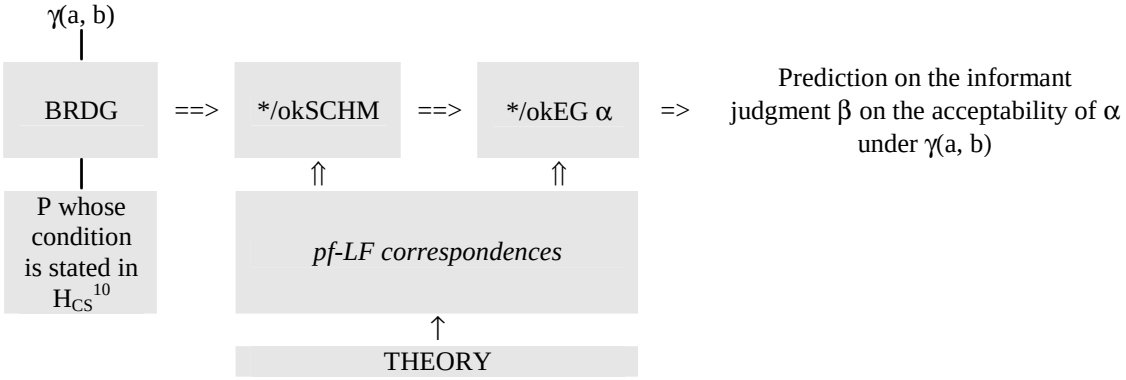
	the "representative values" (RV) *necessary* for a *confirmed schematic asymmetry* to obtain
**Schema (s)*	RV=0
Corresponding *^{ok}Schemas*	0<RV≤1

3.3.6. The model of prediction making

[9] Other formulations of an *^{ok}Schema*-based prediction are also considered in Hoji 2009.

➢ For (14b), let us turn to **the model of prediction making** in (29).

(29) The model of prediction making:



a. BRDG: the *bridging statement* that relates γ(a, b) and P as its necessary condition.[11]
b. P: the property at LF mentioned in BRDG[12]
c. H_{CS}: the hypothesis about the Computational System that states the condition for P. (Universal)
d. */okSCHM: *Schema or okSchema
e. */okEG: *Example (in the case of **Schema*) or okExample (in the case of ^{ok}*Schema*)
f. H_{Lex}: the hypothesis about an item in the mental Lexicon.[13] (Language specific)
g. *pf-LF correspondences*: (the general patterns of) the pf-LF correspondences (assumed by the researcher)
h. THEORY: the hypotheses adopted elsewhere in the theory (i.e., other than the H_{CS} under discussion), including those about the Computational System and those about items of the mental Lexicon of the speakers of the language in question.

Question: When a **Schema*-based prediction on sentence α under γ(a, b) has survived a rigorous test of disconfirmation, how could we ensure that the total unacceptability of the **Examples* is indeed due to the properties of the H_{CS} (and/or H_{LEX}), not due to some parsing difficulty of some magnitude?

Answer: By making sure that the same surface forms as the **Examples* are not totally unacceptable if they are considered *without* γ(a, b).

- If a **Schema*-based prediction were on simple unacceptability of sentence α, we would not have a similar means to ensure that the total unacceptability of the **Examples* is indeed due to the properties of the H_{CS} (and/or H_{LEX}) under discussion.[14] This is the conceptual basis for (14b), repeated here.

(14) b. The informant intuition is more directly revealing about the properties of the Computational System if it is on the (un)acceptability of a sentence *under an interpretation involving two expressions* than if it is on "simple" (un)acceptability of sentences.

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[10] It is necessary to add here "and/or H_{LEX}"; see (29f). The model in (29), however, does not make reference to H_{LEX} because H_{LEX} can also affect *pf-LF correspondences* and its inclusion would complicate the presentation of the model here.

[11] It is argued in Hoji 2009: chapter 3, section 4.1.1 that the *bridging statement* must be of the form in (i) as long as we are dealing with the informant judgment on the acceptability of sentences under interpretation γ(*a*, *b*); see (15c).

(i) A certain linguistic intuition such as the sense of the availability of interpretation γ(*a*, *b*) arises only if (a) certain condition(s) is/are met *at LF*.

It is also noted there that the *bridging statement* is not testable unless the hierarchical relation 'gets converted' to a precedence relation because the informant cannot detect hierarchical relations among elements in the presented sentence. The crucial assumption here is that the "information" detectable at LF does not include precedence relations. We thus need to 'convert' the *bridging statement* to a statement of the form in (ii).

(ii) A certain linguistic intuition such as the sense of the availability of interpretation γ(*a*, *b*) arises only if *a* and *b* appear in a *linearly arranged schema* of a particular form.

And we can make a statement of the form in (ii) empirically testable only if we commit ourselves to particular *pf-LF correspondences*. This is a point that is of utmost importance if we want to make our proposal empirically testable.

[12] *FD*, discussed in Hoji 2009, is an instance of *P* here.

[13] This is not included in the above chart.

[14] This consideration may not apply if we are dealing with sufficiently simple sentences/examples. But the degree of complication of our experiments can increase rapidly as we start investigating predictions on the basis of interaction of a number of hypotheses.

I thus suggest (30).

(30) The **Minimum Paradigm Requirement**: *when working under* (15), a paradigm must minimally consist of examples of the following three types
   a. a **Example* such that at least one of the conditions (structural or lexical) for γ(a, b) is *not* satisfied in any of the LF representations that could correspond to it
   b. an $^{ok}Example_1$ such that it minimally differs from (30a) and the structural and lexical condition(s) for γ(a, b) is/are satisfied in an LF representation that could correspond to it
   c. an $^{ok}Example_2$ such that it is identical to (30a) in terms of the surface string but with an interpretation that does not include γ(a, b)

3.4. Learning from errors

What crucially underlies the contention (made in Hoji 2009) that "an alleged generalization that does not form a *confirmed schematic asymmetry* has not (yet) attained the status of *data in generative grammar*" is the desire to accumulate hypotheses about the Computational System that are empirically testable and to proceed in accordance with the general heuristic in (31).

(31) (Hoji 2009: Chapter 5, (19))
A general research heuristic:
We should maximize our chances of learning something about the properties of the Computational System from the disconfirmation of our predictions.

(31) is very much along the lines of what Popper (1963) provides as his own summary of the theses that he has put forth. I only copy the first 4 of the 17 points that Popper (1963: 965-966) gives, "re-stating all the controversial things I have been saying in a number of theses which I shall try to put in as challenging a form as I can."[15]

(32) (Popper's (1963: 965) (1)-(4))
   a. All scientific knowledge is hypothetical or conjectural.
   b. The growth of knowledge, and especially of scientific knowledge, consists in learning from our mistakes.
   c. What may be called the method of science consists in learning from our mistakes systematically; first, by daring to make mistakes—that is, by boldly proposing new theories; and second, by searching systematically for the mistakes we have made, that is, by the critical discussion and the critical examination of our theories.
   d. Among the most important arguments which are used in this critical discussion are arguments from experimental tests.

The insistence on emphasizing the significance of **Schema*-based prediction and the postulation of the model of prediction making in (29) have been prompted by the consideration in (31); see (14a). So is the insistence on relying on the informant judgments on the *(un)acceptability of sentence α under interpretation γ(a, b)*, see (14b).[16]

3.5. Empirical illustration

Hoji 2009 provides some Japanese sentences that have been used in our preliminary experiments, and they are intended to serve as a basis of illustration of (14) (and perhaps other notions such as *single-informant experiment*, *multiple-informant experiment*, *structural/contextual resourcefulness*, etc.)

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[15] See Popper 1963: 965-966. (Popper, Karl. 1963. "Science: Problems, Aims, Responsibilities," *Federation Proceedings (Baltimore), Federations of American Societies of Experimental Biology* Vol. 22, Issue 4: 961-972.) Feynman's basic points/theses (as in his 1974 Caltech address and in his 1964 Cornell Lectures) seem strikingly similar to Popper's. A similar point is already made in Poincaré 1902 *LA SCICENCE ET L'HYPOTHESE*. The page reference below is to its 1952 English translation *Science and Hypotheses* (Dover Publications).
"… The physicist who has just given up one of his hypotheses should, on the contrary, rejoice, for he found an unexpected opportunity of discovery. His hypothesis, I imagine, had not been lightly adopted. It took into account all the known factors which seem capable of intervention in the phenomenon. If it is not verified, it is because there is something unexpected and extraordinary about it, because we are on the point of finding something unknown and new. Has the hypothesis thus rejected been sterile? Far from it. It may be even said that it has rendered more service than a true hypothesis. Not only has it been the occasion of a decisive experiment, but if this experiment had been made by chance, without the hypothesis, no conclusion could have been seen; and only one fact the more would have been catalogued, without deducing from it the remotest consequence." (Poincaré 1952: chap. 9, 150-151)

[16] Hoji 2009: chapter 5, section 2 contains further discussion.

3.6. Summary

The proposal in Hoji 2009 can be understood as being driven by the desire to discover the properties of the language faculty with the scientific method schematized in (5), along with the research heuristic in (31). As indicated above, (5) and (31) are not novel ideas by any means in regard to scientific practice. The question is, really, whether we can (aspire to) discover the properties of the language faculty by 'following' (5) and (31). I suggest in Hoji 2009 that we can, but only if we start with something that we can manage at our initial stage of investigation, i.e., only if we build our hypotheses on *confirmed schematic asymmetry*, very much along the lines of Nakaya's (1958: 17) "Science has its intrinsic limitation; it is a discipline where we extract phenomena that are reproducible in a broad sense and investigate/understand them statistically."[17] I thus maintain that if we want to discover the properties of the Computational System that is hypothesized to be at the center of the language faculty, what I call a *confirmed schematic asymmetry* should be considered as a *minimal empirical unit of 'facts'* for such research. The method articulated in line with this is now called the *Evaluation of Predicted Schematic Asymmetry* (*EPSA*) *method* and Hoji 2009 is an attempt to provide its conceptual justification and some concrete illustration.

4. General significance and some implications

4.1. The major consequences/implications

The research concerned with properties of the Computational System is, or at least, can be, regarded as part of an attempt to understand what characterizes the human being, as opposed to other beings, organic and inorganic. We can understand that what underlies my proposal is the sense that the proposed method is a way, and the only way that I know of at the moment, to understand by the general method in (5) and with the heuristic in (31) the properties of the human being that are, at least for now, beyond reach by a theory that successfully deals with much of the universe. As to the general significance of the proposed methodology, I would like to think/suggest that, if successful, the research being pursued here will show (33) and (34).

(33) The core properties of the language faculty can be investigated scientifically in line with (5), repeated here. (Or to put "conversely," we investigate the properties of the language faculty that can be investigated scientifically in line with (5); cf. Nakaya's remark quoted in section 3.6)

(5) The general scientific method:
Guess — Computing Consequences — Compare with Experiment

(34) a. The empirical merit of particular linguistic theorizing can, and hence, in my view, should, be determined by experiments.
b. The interpretation of the experimental results does not require statistics of much sophistication, in regard to the most crucial criterion in hypothesis evaluation.
c. *In principle*, anyone can be a judge on the validity of hypotheses about properties of the Computational System, at least to the extent that s/he can see for her/himself whether the relevant **Schema*-based prediction survives a rigorous attempt of disconfirmation.

4.2. Implications for cross-linguistic empirical research

The proposed methodology has implications also for cross-linguistic research insofar as it is concerned with properties of the Computational System. Although it is not entirely clear how *repeatability* could be measured in the context of cross-linguistic empirical research, it seems useful to consider the issue in light of the thesis in (35) and pursue a thesis like (36).

(35) *Across-speaker repeatability* can be meaningfully addressed only if *within-speaker repeatability* (*across-occasion* and *across-example repeatability*) obtains.

(36) A cross-linguistic empirical claim can be meaningfully addressed only if *within-speaker repeatability* obtains in regard to the issue/phenomenon in the language(s) under discussion, and it would in fact be well to have also achieved some degree of *across-speaker repeatability*.

[17] This is my translation of the relevant passage of Nakaya, Ukichirou 1958 *Method in Science* (Kagaku no houhou). To the extent that "investigating/understanding statistically" is part of what counts as science, which seems to be a fairly common view among certain researchers—it may indeed be part of the 'definition of science today' and some people seem to think that we must mimic it in linguistics as well. Hoji 2009, on the other hand, advocates the possibility that a scientific study of the Computational System can be pursued without necessarily requiring statistics of much sophistication although we do need "basic statistics" (such as the averaging of "scores" among the informants) to measure the reproducibility of the result of an experiment.

In other words, it seems rather senseless to address a cross-linguistic empirical claim without having obtained *within-speaker repeatability* and *across-speaker repeatability* in each of the languages under discussion. This seems rather common-sensical. But the point is perhaps worth making in light of the fact that a cross-linguistic study seems to often make crucial reference to an alleged generalization that falls *far short* of being a *confirmed schematic asymmetry*, as in the case of the alleged generalizations discussed in Hoji 2009 and elsewhere regarding *zibunzisin* and *otagai* in Japanese, for example.

Convincing the others (presumably the other practitioners in the field if not those outside the field), I believe, is part of science. Obtaining *repeatability* is a necessary condition for convincing the others. And that makes it imperative that we develop a reliable experimental methodology to test the validity of one's hypotheses and especially a reliable method of evaluating the result of an experiment. But such methods have a function beyond convincing the others. It also has the function of making us feel *willing to be convinced by others*.

The point might appear rather obscure if one only thinks about interaction among the native speakers of one's own language(s). Suppose one is evaluating someone else's work that deals with a language that one does not speak as one's native language. One can never be sure about the reliability of the generalizations presented in such work. What does one do then? Some typical reactions come to mind, as indicated below.

(37) a. Some people may simply assume that the presented generalizations are valid, i.e., they may simply assume that they are *confirmed schematic asymmetries* in the terms of the preceding discussion.
b. Some people may do so only if the alleged generalizations would support what they are pursuing; and this seems to be a rather typical practice in the field as far as I can tell.
c. Others may think like the following: "Well, maybe valid; but maybe not. So I will take them as valid only if I detect something analogous in my own language, and until then I leave them in the category of 'Maybe'."

Now, we would have a rather different attitude if the alleged generalization were presented along with the relevant experiment(s) and its/their result(s)—which would presumably include the **Schema(s)*, the *^{ok}Schemas*, and **Examples*, and the *^{ok}Examples*. We would in that case be much more willing to accept the proposed generalizations as valid, insofar as they form a *confirmed schematic asymmetry*.

Accepting such a *confirmed schematic asymmetry* as being established in regard to another language might in fact help us with our research on our own language(s) since we would in that case have good reason to believe that, unless there is reason otherwise, the same generalization should hold in our own language(s), provided that the generalization is based on a universal statement. Much of cross-linguistic research, however, seems to proceed without being seriously concerned with whether an alleged generalization constitutes a *confirmed schematic asymmetry*.[18]

4.3. Freeing ourselves (and the field) from *English-centricity* and *authoritarianism*

I should like to suggest that, given (34), the field may (finally) become free from (38).

(38) a. English-centricity (and other related "guidelines" that are seemingly accepted by many practitioners)
b. Authoritarianism

And I take this to be a significant consequence of adopting the proposed methodology.

I would like to further speculate that *science* will become accessible to anyone, in principle, to the extent that anyone can participate in a scientific experiment concerning the language faculty and appreciate the significance of its results without a special talent or training in mathematics. Together with (38), this has the *potential* of having non-trivial implications of fostering less reliance on authority in general. Under the proposed methodology, the only authority one would pay heed to is the result of an experiment; nothing else matters, *ultimately*, very much like what Richard Feynman advocated in his 1964 "Messenger Lectures" at Cornell University; see the Feynman remarks quoted at the beginning of the handout (right after (5)), at least at the current stage of our research.

Remarks such as (39) and (40), which can be found in abundance, represent Chomsky's stance on authority and it is very much in accord with what underlies the research methodology advocated here.

(39) "Compare mathematics and the political sciences—it's quite striking. In mathematics, in physics, people are concerned with what you say, not with your certification. But in order to speak about social reality, you must have the proper credentials, particularly if you depart from the accepted framework of thinking. Generally speaking, it seems fair to say that the richer the intellectual substance of a field, the less there is a concern for credentials, and the greater is the concern for

[18] And we sometimes, if not often, observe practice in the field (including some introductory textbooks) where an alleged generalization continues to be adopted despite a demonstration in published works that it clearly fails to qualify as a *confirmed schematic asymmetry*.

content."
(Chomsky 1979: 7 (*Language and Responsibility*))

(40) "Now, if you ask, "What media can I turn to to get the right answers?" First of all, I wouldn't tell you that because I don't think there's an answer. The right answers are what *you* decide are the right answers. Maybe everything I'm telling you is wrong. Okay? Could perfectly well be; I am not God. But that's something for *you* to figure out. I mean I can tell you what *I* think happens to be more or less right. But there isn't any reason why you should pay any attention to it."
(Noam Chomsky, in Manufacturing Consent: Noam Chomsky and the Media (1992)[19])

Once we turn our attention to linguistics, however, Chomsky has been regarded as a, if not the, authority. It is remarkable that his influence in the field has remained as such over half a century; cf. the changes of Einstein's "status" in physics over the years. Hideki Yukawa's remark in (41) is suggestive in relation to what seems to be at issue.

(41) Because it was not possible to determine the validity of these hypotheses by means of comparison with empirical facts, the atomism doctrine and the continuity doctrine coexisted in opposition for a long period of time, and furthermore, because atoms themselves were invisible and were a product of imagination, it was possible for the proponents of atomism to put forth various models of atoms—some similar or dissimilar to others, just as there are similarities and differences among myths of various ethnic and racial groups. What affected the rise and fall of these numerous hypotheses were more or less personal and sociological factors such as the extent of faith in the character and wisdom of their advocates and the followers, their presentational and rhetorical skills, and religious authority.
(Yukawa 1976: pp. 12-13)[20]

Suppose that someday (a version of) the methodology advocated in Hoji 2009 becomes the norm of the field. Anyone can place on-line his/her hypotheses (necessarily with some relevant generalizations), along with the prediction(s) (both **Schema*-based predictions and ^{ok}*Schema*-based predictions), the experimental specifications (including preliminary experiments) (including the **Schemas* and the corresponding ^{ok}*Schemas*) and perhaps actual experiments (hence including actual **Examples* and ^{ok}*Examples*) and their results. And they can have a reasonable expectation to be taken seriously irrespective of his/her credentials as long as their **Schema*-based prediction(s) has/have survived a rigorous test of disconfirmation and the corresponding ^{ok}*Schema*-based predictions have been confirmed.

Perhaps, it is not unreasonable to think that we are pursuing the methodology along the lines of Hoji 2009, which is Popperian in its essentials, because we do not want to be judged by our credentials. That is to say, we do not want to be part of the "game" where our work is judged by whether we have the blessing of the authority (e.g., cited by so and so, published in such and such journals or from such and such publishers, etc.), whether the majority of the subfield you belong to agrees with you, etc. We want our work to be judged by criteria that go beyond such considerations, and the *EPSA* method is an attempt to establish such a criterion for the field that I believe has not yet become a science. The *confirmed schematic asymmetries* that will be accumulated in accordance with the *EPSA* method, I would like to think, will form an empirical foundation for a theory of the language faculty (not of language(s)), very much like the data accumulated by Tycho Brahe served as the empirical foundation of Kepler's work, eventually leading to Newtonian theory and the accompanying scientific revolution (and subsequent revolutions in physics).

5. A Summary in the form of questions

(42) In regard to research concerned with the properties of the language faculty:
- a. What are our "guesses," i.e., hypotheses, *about*?
- b. How could we obtain (i.e., *compute*/*deduce*) *consequences* from the hypotheses in question?
- c. How can such consequences be tested and what criteria could we have for determining whether the computed *consequences* agree or disagree with experiments?
- d. If our hypotheses are about the properties of the language faculty, what should such hypotheses look like, what primitive concepts and relations are likely to be included in the theory in terms of which

[19] The video can be downloaded at:
http://video.google.com/videoplay?docid=-5631882395226827730
The subject matter is not linguistics but it is highly recommended.

[20] This is my translation of the relevant passage. Yukawa, Hideki "An outline of theoretical physics" in *Me-ni mienai mono* 'Invisible things' (first published in 1946); the page references are to the 1976 Koodansya gakuzyutsu bunko.

such hypotheses are stated?

e. How can they be tested in an experiment, what kind of experiments are possible and/or suitable to test the validity of such hypotheses?
f. How are we to interpret the results of our experiments?
g. When is our prediction disconfirmed or when are we willing to accept that it has been disconfirmed?
h. When is our prediction confirmed or when are we justified to say that it has been confirmed?
i What are the implications of disconfirmation of a prediction?
j. How could we proceed in the event that our prediction fails to be borne out?

Questions like the following also come to mind.

(43) a. Can the data in generative grammar be categorical, i.e., clear-cut, to begin with?
b. Are the data in generative grammar necessarily probabilistic and hence require a statistic analysis in a crucial way?
c. Why do we need to conduct an experiment and why could we not test our hypotheses by observing how people use the language?[21]

6. Appendix I: Further remarks on methodology

6.1. Chomsky on methodology

In answering a question about [his] "method of investigation", Chomsky (1980[22]: 190) states as in (44).

(44) As for my own methods of investigation, I do not really have any. The only method of investigation is to look hard at a serious problem and try to get some ideas as to what might be the explanation for it, meanwhile keeping an open mind about all sorts of other possibilities. Well, that is not a method. It is just being reasonable, and so far as I know, that is the only way to deal with any problem, whether it is a problem in your work as a quantum physicist or whatever.

There are certain fields like psychology where people do carry out extensive study of methods of investigation. There are other fields like physics where you do not study methods of investigation. So at MIT the physics department does not have a course in experimental methods, but many psychology departments spend a lot of time on what they call methodology. Well, there is a lesson there, but I won't draw it. (Chomsky 1988: 190)

The lesson that Chomsky did not draw seems to be something like (45).

(45) If you are doing real science, you don't talk about methods. After all, they don't talk about methods in physics, perhaps the most advanced area of science.

Well, there is a lesson to draw here, and I would like to draw it.

(46) In physics, at least in the field as a whole, there has been a long, in fact very long, tradition of empirically testing hypotheses, establishing a fairly reliable means to determine the plausibility of the hypotheses. Even so-called theoretical physics could not have a meaningful existence if it were totally unrelated to empirical observations/generalizations.

For hypothesis forming, there is no method. We know that; well, at least, most people seem to agree on that. But for hypothesis testing, the situation is totally different. I do not mean to endorse the practice in psychology that Chomsky seems to have alluded to in (44). To the extent that, or if, much of psychology is concerned with behavior rather than the nature/representation/mechanisms of the mind, something is seriously wrong with their emphasis on "methods," I would agree. But that does not mean that we need not be concerned with methods of *testing* our hypotheses.

As addressed in some depth in Hoji 2009, this is related to the nature of data in the empirical inquiry that we are engaged in.[23]

- A crucial question: What should count as data in research that is concerned with the properties of the

[21] This is related to a much more general question of how a study of the Computational System is related to other types of studies of language and languages. Our answers depend upon what kind of hypotheses we put forth, how we are to test their empirical consequences, and what we would take to be a disconfirmation of our predictions.

[22] Chomsky, Noam. 1988. *Language and Problems of Knowledge: The Managua Lectures*, MIT Press.

[23] One might be tempted to infer from (44) that Chomsky refuses to draw a line between testable hypotheses and not-testable hypotheses and points out that a serious astrologer perhaps did what is suggested in (44). (We should bear in mind, incidentally, that it is not the case, after all, that rigorous works by astrologers never resulted in the accumulation of data that would later prove to be crucial in the development of a theory that would yield testable predictions.)

language faculty, and more narrowly, with the properties of the Computational System that is hypothesized to be at the center of the language faculty?

That is one of the main concerns in Hoji 2009 and the *EPSA* method proposed there includes a specific answer to this question.

One might wonder whether the view expressed in (44) can be reasonably regarded as representing Chomsky (consistent) position. We do not seem to find Chomsky's remarks on methodology that is (clearly) contrary to (44); on the other hand, we do find Chomsky's remarks or those attributed to him that are consistent with (44). Schütze 1996, for example, states as in (47).[24]

(47) Chomsky (personal communication) believes that research practice in linguistics ought to follow that in the natural sciences, where (in contrast to the social sciences) "almost no one devotes attention to 'methodology'." Obviously, I disagree.
(Schütze 1996: 210, footnote 1)

Thus, (44) does seem to express Chomsky's position.

6.2. Chomsky on history and philosophy of science

Consider also (48), taken from Chomsky 1979.[25]

(48) I should also mention work on history and philosophy of science, which has begun to furnish a richer and more exact understanding of the manner in which ideas develop and take root in the natural sciences. This work—for example, that of Thomas Kuhn or Imre Lakatos—has gone well beyond the often artificial models of verification and falsification, which were prevalent for a long time and which exercised a dubious influence on the "soft sciences," as the latter did not rest on the foundations of a healthy intellectual tradition that could guide their development. It is useful, in my opinion, for people working in these fields to become familiar with ways in which the natural sciences have been able to progress; in particular, to recognize how, at critical moments of their development, they have been guided by radical idealization, a concern for depth of insight and explanatory power rather than by a concern to accommodate "all the facts"—a notion that approaches meaninglessness—even at times disregarding apparent counterexamples in the hope (which at times has proven justified only after many years or even centuries) that subsequent insights would explain them. These are useful lessons that have been obscured in much of the discussion about epistemology and the philosophy of science. (Chomsky 1979: 73)

It seems to me that what is meant by "the often artificial models of verification and falsification" is the models (put forth by philosophers of science) in which verification and falsification of a scientific hypothesis is to be done on the basis of some concrete observational facts, or something like that (that, I understand, is the basic tenet of "logical positivism"). I assume that what is meant by the "soft sciences" are inquires beyond the 'natural sciences' (such as social sciences, and perhaps linguistics being included here, as it is practiced in much of the field, (including much of the generative research, I hate to say)).

Let us now turn to the following portion of (48).

(49) (A portion of (48).)
[A]t critical moments of their development, [the natural sciences] have been guided by radical idealization, a concern for depth of insight and explanatory power rather than by a concern to accommodate "all the facts"—a notion that approaches meaninglessness—even at times disregarding apparent counterexamples in the hope (which at times has proven justified only after many years or even centuries) that subsequent insights would explain them.

It might not be an overstatement that how one understands (49) in the context of generative grammar, and more in particular in the context of assessing one's own research in generative grammar, may 'define' one's orientation as a researcher dealing with 'language-related issues'.

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[24] Schütze, Carson. 1996. *The Empirical Base of Linguistics: Grammaticality Judgments and Linguistic Methodology*, University of Chicago Press. (47) is given as a footnote appended to the text remark in (i).

(i) ... I would echo Greenbaum's (1977c) recommendation that every linguistics department should offer a course in experimental linguistics. In addition to reasons internal to our own field, this would give a student a leg up in joining the blossoming interdisciplinary enterprise of cognitive science. It would also seem to be a natural outgrowth of Chomsky's own suggestion that linguistics be viewed as a branch of cognitive psychology. Somehow, the focus on cognitive issues has not yet been accompanied by adoption of the scientific standards and concern with methodology of that discipline. (Schütze 1996: 210)

[25] Chomsky, Noam. 1979. *Language and Responsibility: Based on conversations with Mitsou Ronat*, Panthen Books, New York.

One might take (49) to mean that we should not be too concerned with, or impressed by, empirical observations since, after all, empirical observations alone never determine the fate of a scientific theory, anyway. In this connection, it is perhaps worth focusing on the following portion of (49).

(50) (A portion of (49).)
"[E]ven at times disregarding apparent counterexamples in the hope (which at times *has proven justified only after many years or even centuries*) that subsequent insights would explain them." (the emphasis by HH)

We should consider what, in principle, would be required for something like this to happen/take place, i.e., what would be required in order for "apparent counterexamples being disregarded for many years or even centuries and getting explained by subsequent insights, thus justifying the disregarding of them (for many years or even centuries)." In the context of generative grammar (the research concerned with the properties of the language faculty, it seems appropriate to raise questions like the following.

- Do we have such "counterexamples" in generative grammar over the past half century?
- What *could* be (the form of) such a "counterexample" in research concerned with the language faculty?
- What would be a necessary requirement for something to be such a recalcitrant counterexample in a given research program?

- "Counterexamples" to an alleged *confirmed schematic asymmetry* do not even remotely compare with (apparent) counterexamples as alluded to in Chomsky's remarks above.[26]

Cf. Lakatos' scientific research programs.[27]

(51) Two illustrations of the hard core, auxiliary hypotheses, *progressive and degenerating problemshift*:
a. the anomalous orbit of Uranus and the subsequent discovery of Neptune
b. the anomalous perihelion precession of Mercury

It may be a useful exercise to raise the following questions in relation to the type of research advocated in Hoji 2009 (and, for that matter, in relation to one's own research—including the graduate students') and we might also ask whether the answers to each of those questions would hold only "at critical moments."

(52) a. Is it guided by "radical idealization"?
b. Is it guided by a concern to accommodate "all the facts"?
c. Is it guided by "a concern for depth of insight and explanatory power"?
d. Does it at times disregard apparent counterexamples?

Notice that the content of the crucial notions in (52c) are quite obscure and can be very subjective, as addressed in Postal 2004; chapter 12 though from a somewhat different perspective than what is being pursued here.[28] If someone proposed or endorsed something like (53), that should be reason for concern, in my view; cf. Feynman's remark on the general method in science.

(53) "[A] concern for depth of insight and explanatory power" should guide our research more than a concern for whether our predictions are borne out.

As long as we articulate our hypothesis rigorously enough, for example, by following the *EPSA* method—we should be able to make definite and testable predictions. By carefully designing and conducting experiments to test the predictions, we should be able to tell, fairly objectively, whether or not the predictions are borne out and hope to be able to evaluate the validity of hypotheses in question about the Computational System. While the validity of hypotheses can thus be checked on the basis of the results of experiments, there does not seem to be an objective criterion for the determination of how insightful a given hypothesis or theory is or how much explanatory power it may have, etc. The problem becomes quite acute if we do not have a way to test the empirical consequences of the hypotheses under discussion.[29, 30]

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[26] Lexical hypotheses about *zibunzisin* and *otagai* and alleged generalization regarding the "locality requirement" on a numeral-classifier sequence in Japanese come to mind, for example.

[27] Hoji 2009: chapter 5 states its methodological proposal in the terms of Lakatos' *scientific research program*.

[28] Postal, Paul. 2004. *Skeptical Linguistic Essays*. Oxford University Press.

[29] It goes without saying that "simplicity" is valued in any field where an explanation is sought. Such is clearly the case where "simplicity" can be "measured" in a (fairly) transparent way because the proposed account/explanation is stated in mathematical terms, for example. The remarks in the text are not intended to deny that. Neither are they intended to deny that one may have a strong sense in regard to "simplicity," "elegance," etc. of a theory (or hypotheses) in question even when

6.3. Chomsky on Peirce

Chomsky (1979) responds to Mitsou Ronat's remark in (54), as in (55).

(54) Abduction is, I believe, a form of inference which doe not depend solely on a priori principles (like deduction), nor solely on experimental observation (like induction). But that aspect of Peirce is very little known in France.

(55) Or here in the United States either. Peirce argues that to account for the growth of knowledge, one must assume that "man's mind had a natural adaptation to imagining correct theories of some kinds," some principle of "abduction" which "puts a limit on admissible hypothesis," a kind of "instinct," developed in the course of evolution. (Chomsky 1979: 71)

If what is intended in (55) were that we must have some intuition about "correct theories," and that we must rely on such intuition in deciding on "correct theories" as the most crucial criterion, such a view, when applied to generative grammar, would amount to a declaration that generative grammar cannot be an empirical science, as far as I can tell. It is possible, if not likely, that Chomsky is comparing, in his remarks above, generative grammar on the one hand and structural linguistics and some behaviorist/empiricist-oriented research activities on the other.[31] I would nevertheless like to think that his remarks above and the brief discussion I have provided above is in fact quite suggestive as to what Chomsky considers (his) generative grammar to be.

Conspicuously missing in Chomsky's remarks above (and remarks here and there in Chomsky 1995: chap. 1, as well) are concerns about how to test predictions that our hypotheses make. I am inclined to think that "depth of insight and explanatory power" can be meaningfully addressed only when our research activities are accompanied by concerns with how to test our hypotheses (and the accompanying research practices).

The part that contains (54) and (55) in Chomsky 1979 *Language and Responsibility: Based on conversations with Mitsou Ronat*, Panthen Books, New York, 70-71 is reproduced below.

> M.R.: To what degree can your discoveries about language and your definitions of fields of knowledge lead to the emergence of new philosophic questions? To which philosophy do you feel closest?
>
> N.C: In relation to the questions we have just been discussing [which has to do with how we acquire our knowledge in general --HH], the philosopher to whom I feel closest and whom I'm almost paraphrasing is Charles Sanders Peirce. He proposed an interesting outline, very far from complete, of what he called "abduction" …
>
> M.R.: Abduction is, I believe, a form of inference which does not depend solely on a priori principles (like deduction), nor solely on experimental observations (like induction). But that aspect of Peirce is very little known in France.
>
> N.C.: Or here in the United States either. Peirce argues that to account for the growth of knowledge, one must assume that "man's mind had a natural adaptation to imagining correct theories of some kinds," some principle of "abduction" which "puts a limit on admissible hypothesis," a kind of "instinct," developed in the course of evolution.

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the proposal is not stated in mathematical terms. The point of the text remarks is that one's sense of "simplicity," "elegance," and the like concerning a theoretical statement should not be the ultimate deciding factor in assessing its merit. This is in line with Feynman's remark quoted earlier (see section 2) "It doesn't make any difference how beautiful your guess is, how smart you are, who made the guess, or what his name is. If it disagrees with the experiment, it's wrong. That's all there is to it." See also Feynman's (1965/1994: 165) remark.

[30] If two (or more) competing "theories" had *exactly the same empirical consequences*, which I am inclined to doubt would actually happen (at least in regard to research concerned with the properties of the language faculty), the "theories" are notational variants. One might consider what new phenomena/issues each "theory" leads us to investigate so as to see which "theory" would be more appealing. If no differences can be found, which I highly doubt, the choice among the "theories" might just be a matter of taste, based on one's sense of "simplicity," "elegance," etc. If one is a lousy theorist, one has a terrible sense about that; if one is an ok theorist, one's sense may be okay; and so on and so forth. But since I doubt very much that two or more "theories" actually turn out to be genuine "notational variants," one perhaps do not have to worry about that. (Of course, we use our hunches (as a theorist) when choosing a particular hypothesis or approach over the other alternatives. But the ultimate fate or merit of those hunches will be determined by the results of experiments on the predictions that are made under what we have chosen to pursue.)

[31] It may be the case that what Chomsky had in mind here was not just the structuralist/behaviorist approach in general but it was directed also toward generative semantics.

While the aspects of C. Peirce's concern that interest me most include not only how a hypothesis in science can be formed (I guess by abduction (and also by induction, too, I would suspect, insofar as adduction is possible only if there is a generalization of some sort, without which we could not be able to identify a puzzle, to begin with) but also how it should be put to empirical tests (on the basis of deduction (as well as induction, I suspect)), Chomsky, in the above passage, seems to be talking about how we acquire our knowledge in general. It is, however, not clear that Chomsky does not find it necessary for us to put our (scientific) hypotheses to empirical tests. I would like to think that he does. It might, however, be the case, indeed, that he is not concerned as much as we are with putting our hypotheses to empirical test, and that might not be a particularly unwarranted guess, on the basis of his remarks such as those quoted above as well as on the basis of how he has proceeded with his research especially since the mid 1980s.[32]

6.4. Feynman on methodology

- Richard Feynman's remarks on methodology; cf. Chomsky's remarks on methodology in generative grammar
  - ✧ "Scientific Honesty"

Richard Feynman's 1974 Caltech Commencement Address "Cargo Cult Science" contains the following:[33]

> 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 give all of the information to help others to judge the value of your contribution; not just the information that leads to judgement in one particular direction or another.
>
> ...
>
> We've learned from experience that the truth will come out. Other experimenters will repeat your experiment and find out whether you were wrong or right. Nature's phenomena will agree or they'll disagree with your theory. And, although you may gain some temporary fame and excitement, you will not gain a good reputation as a scientist if you haven't tried to be very careful in this kind of work. And it's this type of integrity, this kind of care not to fool yourself, that is missing to a large extent in much of the research in cargo cult science.
>
> A great deal of their difficulty is, of course, the difficulty of the subject and the inapplicability of the scientific method to the subject. Nevertheless, it should be remarked that this is not the only difficulty. That's why the planes don't land—but they don't land.

- So, after all, according to Feynman, there is methodology in physics.

A crucial question one might raise is how we can "measure" scientific integrity/honesty. Suppose one adopted the view that the informant judgments *must converge* on *every single sentence* used in an experiment.

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[32] The conspicuous absence of "acceptability judgment," "acceptability and grammaticality," etc. in the Indexes in Chomsky's books since the late 1970s, as compared to his books in the 1950s and 1960s, is also suggestive in this regard; cf. Chomsky 1965 (*Aspects*): chapter 1, for example.

[33] See (68b).

One would in that case most likely feel that the use of informant judgments itself is an act of dishonesty because such convergence just cannot seem to be attained. If one adopts a different view, in line with the *EPSA* method, on the other hand, we can in fact expect convergence of informant judgments and the use of informant judgments will no longer have to be regarded as an act of dishonesty.

6.5. *Progressive problemshift* in the terms of Lakatos 1970/1978

(56) "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." (Taken from Richard Feynman's 1974 Caltech Commencement Address; see above.)

Here again, one may justifiably wonder how we know what counts as a "fact" in the context of research concerned with the properties of the Computational System? As noted above, my answer is:

✓ **Claim**: A *confirmed schematic asymmetry* is ***the most basic empirical unit of 'facts'*** in research concerned with the properties of the Computational System.

Feynman also addresses the importance of making new predictions.

(57) "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." (Taken from Richard Feynman's 1974 Caltech Commencement Address; see above.)

This corresponds to Lakatos' (1970/1978) *theoretically and empirically progressive problemshift.* If we fail to do the above, our work remains a description of 'facts'. It is the pursuit for *theoretically and empirically progressive problemshift* that has led to the insistence of having a tight connection between [a *confirmed schematic asymmetry*] and [hypotheses about the Computational System and/or those about lexical items that the Computational System makes reference to]; cf. the model of prediction making in (29). An alleged empirical generalization is thus evaluated not only in terms of (58a) but also in terms of (58b).

(58) a. whether it constitutes a *confirmed schematic asymmetry*
b. whether the hypotheses that are claimed to be responsible for the empirical generalization, now "elevated to" a *confirmed schematic asymmetry*, contribute to making a new prediction concerning a *new confirmed schematic asymmetry*

6.6. The Reinhartian heuristic

The two most general hypotheses put forth in Reinhart 1983 are (59) and (60).

(59) Reinhart 1983: 25, (19):
Sentence-level semantic interpretation rules may operate on two given nodes A and B only if one of these nodes is in the domain of the other (i.e., A is in the domain of B, or B is in the domain of A, or both).

(60) Reinhart 1983: 26, (21):
If a rule assigns node A some kind of prominence over node B, A must be a D-head of the domain which contains B.

We can restate (59) and (60) as in (61) and (62), respectively, taking "sentence-level semantic interpretation rules" as "Computational-System (CS)-based rules or conditions that contribute to or regulate interpretive possibilities," which seems to be a reasonable interpretation, given the discussion in Reinhart 1983.

(61) Reinhart 1983: 25, (19), restated:
CS-based rules or conditions that contribute to or regulate interpretive possibilities can involve A and B only if A c-commands B, or B c-commands A, or both.

(62) Reinhart 1983: 26, (21), restated:
If B is *dependent upon* A in terms of how B gets interpreted, B must be c-commanded by A.

In the context of the present discussion, let us take the statements in (61) and (62) as applying at LF.[34] (61) is a hypothesis about the form of a Computational-System-related hypothesis that pertains to an interpretation involving two elements; (62) is an instance of, or is a somewhat abstract way of expressing, a *bridging statement*.

[34] Although taking (59) and (60) (hence (61) and (62)) as applying at LF is not justified by the textual reading of Reinhart 1983 alone—while the restatement of (59) and (60) as in (61) and (62) can—, that is a reasonable way to understand what is intended in Reinhart 1983 once it is 'translated' into the general framework adopted here regarding the organization of the CS.

We can thus understand Reinhart 1983 as containing a heuristic like (63).

(63) The Reinhartian heuristic:
The relation at LF that underlies γ(a, b) mentioned in a bridging statement must be based on a *c-command* relation between LF(a) and LF(b), where LF(a) and LF(b) are LF objects corresponding to a and b.

The Reinhartian heuristic in (63) has proven to be an extremely useful tool over the years in identifying *confirmed schematic asymmetries*. Given that it makes reference to the structural relation of c-command, which is directly derivable from the only structure-building operation in the Computational System, *Merge*, we should not be surprised if (63) turns out to be more than just a research heuristic.

7. Appendix II: Some remaining issues (and remarks of clarification)

(64) Some further thoughts about the role of *confirmed schematic asymmetries* and (what I take to be) the current stage of our research program:[35]
a. If *generative grammar* is ever to become a research program that deserves to be called an empirical science, it is necessary to establish *confirmed schematic asymmetries*, specifying how to do that. And the first step toward that should be to acknowledge the significance of *confirmed schematic asymmetries*.
b. *Confirmed schematic asymmetries* are like "basic units of facts" in research concerned with the language faculty.
c. It is perhaps safe to assume that our research program is still at a pre-scientific stage. At its (future) scientific stage, we should perhaps be concerned with how to deduce from our theory the number of *confirmed schematic asymmetries* that we will have accumulated by then instead of being (merely) concerned with establishing a *confirmed schematic asymmetry* itself. At that stage, *schematic asymmetries* would likely be of a highly abstract character although they should ultimately be related to *confirmed schematic asymmetries* of the sort that we are trying to establish right now.
d. It is up to the researchers concerned with the discovery of the properties of the language faculty whether the field can eventually attain the status of an empirical science or remains to be a field where seemingly empirical materials and issues are addressed without serious attempts to state explicitly how the consequences of the predictions can be tested and how hypotheses can in principle be refuted.

(65) A lingering/remaining question:
Question: Is physics the right place to turn to as a "model" for a research program that is concerned with the language faculty?

8. Appendix III: Recommended audio-visual materials and readings

(66) YouTube videos:
a. "Feynman on Social Sciences," (1' 52")
http://www.youtube.com/watch?gl=JP&hl=ja&v=_EZcpTTjjXY
b. "Feynman"
Part 1: http://www.youtube.com/watch?v=CL0SB4JkXzY (9' 06")
Find Parts 2-6 from the above.
c. Feynman's "Messenger Lectures" available at: http://research.microsoft.com/apps/tools/tuva/#.
d. The Feynman lectures available at: http://vega.org.uk/video/.

(67) Audio:
Lakatos "Science and pseudo-science"[36]

(68) Papers:
a. Popper, Karl. 1963. "Science: Problems, Aims, Responsibilities," Federation Proceedings (Baltimore), Federations of American Societies of Experimental Biology Vol. 22, Issue 4: 961-972.
b. Richard Feynman. 1974 "Cargo Cult Science," a Caltech Commencement Address.[37] (Reproduced

[35] (a) and (b) have already been made in the preceding discussion.

[36] The audio tape and its written version—which appears in Lakatos 1978 as "Introduction: Science and Pseudoscience." (pp. 1-7)—are available at: http://www.lse.ac.uk/collections/lakatos//Default.htm.

[37] The paper is available on-line. Two of the URLs of the paper are:

(with slight adaptation) in *Surely You're Joking, Mr. Feynman!*)

http://www.columbia.edu/itc/applied/wiggins/Classes/E4903/Fall2003/cargo.pdf
http://www.physics.brocku.ca/etc/cargo_cult_science.php