

# THOUGHT EXPERIMENTS IN LINGUISTICS

Sarah G. Thomason

## I. INTRODUCTION

LINGUISTS usually do not think about thought experiments much, although they do use them—sometimes in ways that partly resemble physicists' use of thought experiments, more often in quite different ways.<sup>1</sup> In general, linguists' thought experiments are most likely to be similar to those of the physicists when the theory in question involves proposed universals of language structure, language learning, language change, or language use.

By contrast, when linguists want to test hypotheses about the structure of a particular language, their methodology crucially involves thought experiments in a more literal sense: real experiments carried out by thinking. This peculiar kind of experiment is important in linguistics because (unlike most scientists' data) our primary data, and therefore the evidence for our theories, is inside people's heads, in the elaborate interlocking multileveled linguistic structures that people learn as infants. In discussing the role of thought experiments in linguistic methodology, I will start with the more familiar kind, and then argue that the notion of a thought experiment should be elastic enough to include linguists' experimentation by introspection.

## II. THOUGHT EXPERIMENTS AS A STAGE-SETTING DEVICE

First, then, let us consider some thought experiments that are more or less similar to those in other disciplines. Here it has to be emphasized that using a thought experiment to argue for or against a theoretical claim will not, by itself, win an argument in linguistics; logical contradictions, for instance, typically will not arise. In fact, the result of this sort of linguistic thought experiment is likely to be theory-dependent. The thought experiment will therefore not be a test of a hypothesis, but rather a stage-setting device that suggests tests that the linguist can carry out. In other words, the major role of the thought experiment is to clarify the theoretical issue, or to make it vivid, as a first step in an argument. Often the thought experiment serves to get the audience's agreement in advance about what

would count as supporting evidence for the theory, even if that exact kind of evidence is not going to be forthcoming. The second step—and it is a necessary one if the argument is to be successful—is a demonstration that some real-world situation is sufficiently similar to the result of the thought experiment that other linguists will accept that situation as supporting evidence.

Some interesting examples can be found in the area of language learning, especially children's learning of their native language. This is a topic that is subject to non-thought experimentation only to a limited extent, partly because children of the appropriate ages can't fill out questionnaires, but partly for ethical reasons. The ethical limitations on child-language experiments become clear when one considers the first linguistic experiment recorded in history: Herodotus, writing in the 5th century B.C. (Book II, 2), reports that the Egyptian king Psammetichus wanted to know what race of men was the world's oldest. He had two infants sequestered at birth in a herdsman's cottage, and the person who cared for them was not allowed to utter a single word in their presence. When they were two years old, the children were heard saying the word *bekos*—which, on inquiry, turned out to be the word for "bread" in Phrygian (an Indo-European language, now long extinct, which at that time was spoken in Asia Minor). From this experiment Psammetichus concluded that the Phrygians were the world's most ancient race.

Now, treating children like that is not a procedure that would be looked on with favor nowadays, and modern linguists would not expect to discover the world's original language by Psammetichus' experiment. However, there are questions about child language that *could* be answered by controlling what a child hears from birth on—that is, controlling the kind and amount of linguistic input s/he gets for the task of language acquisition. Some of these questions have to do with the issue of innateness, one of the most important questions in linguistic theory: What sorts of inherited mental structures must be posited to account for the fact that human infants learn their first language with spectacular success in the first few years of their lives?

As a first example, consider the proposal by some sociobiologists that certain specific cultural predispositions, including linguistic ones, might be innate—that is, not just the *ability* to learn human language and other aspects of culture, but genetic predispositions to learn aspects of particular cultures (contrast the standard evolutionary biologists' view that culture is acquired solely by learning from other humans, not by genetic inheritance). A linguist might challenge this proposal by the following thought experiment: Take an infant born to parents of completely homogeneous monolingual linguistic background going back, say, ten generations; remove the child at birth and place him/her with adoptive parents whose own language, and that of the entire surrounding community, is (as far as

linguists can tell) completely unrelated to the language of the child's biological parents and ancestors. Linguists will predict that that child will learn the language of his/her adoptive community as fast and as easily as s/he would have learned the language of the biological parents. A sociobiologist who believes in genetically programmed culture-specific traits would of course be unconvinced by this thought experiment; perhaps the child will not in fact learn the adoptive parents' language quite as fast, and perhaps we simply don't have sufficiently sophisticated measurement techniques to detect the difference.

This is possible, certainly, and in any case we could not carry out such an experiment, as described. But there are fairly close analogues to the hypothesized situation in real life—for instance in the case of Vietnamese orphans adopted into monolingual Midwestern American communities. The backgrounds of such children cannot be checked for ancestral linguistic homogeneity, and it is quite likely that someone spoke some Vietnamese to them before they were brought to the United States. Moreover, as far as I know, no one has actually carried out the quite feasible experimental study necessary to prove that the children actually do learn English as fast as their native-born American contemporaries with English-speaking ancestry do. Nevertheless, all the available informal evidence indicates that these children grow up from the start as perfectly ordinary native speakers of English, so—given the total absence of any evidence to the contrary—a linguist will find the thought experiment, together with the partial real-life analogues, convincing as an argument against that particular sociobiological position.

Another example from language learning illustrates what I believe to be the most common function of this type of thought experiment in linguistics: The thought experiment shows what sort of evidence would be conclusive; the linguist admits that such evidence cannot be gathered by direct experimentation or observation; but, the argument continues, other evidence is (or will be) available that points in the direction indicated by the thought experiment. In this example, Pinker (1984) is addressing the question of how a child succeeds in acquiring the syntactic categories that are universal, or nearly so, in the world's languages—among them nouns and verbs. That is, granted that the child's innate equipment includes knowledge, in some sense, of how syntax is organized in human language, how does s/he figure out which bits of the linguistic input are to be analyzed as nouns and which bits are to be analyzed as verbs? The cues cannot, for instance, lie in the ordering of the elements in a sentence, because any given child can learn any of the world's languages, and languages differ widely in basic sentential word order—e.g. SVO (Subject—Verb—Object), SOV, and VSO.

Pinker argues for a process involving what he calls the "semantic bootstrapping hypothesis," whose starting point is the suggestion that perhaps, in parent-infant discourse, parents in fact tend to use nouns to refer to people and physical objects, and verbs to refer to actions and changes of

state. Since the child gets the semantic cues for these words from the external context of discourse, s/he can then infer the syntactic categorization of "nounhood" from the semantic cues, and similarly for "verbhood." That is, the purely grammatical relations are inferred from the witnessed events. Later, the linguistic input will provide the child with more and more examples of less "nouny" nouns—for example, nominalizations of verbs, such as *running* in a sentence like *Running is fun*—and of less "verby" verbs, such as *is* or statives like *resemble*. But by that time the child can fit these semantically opaque nouns and verbs into the syntactic skeleton s/he has already constructed for the semantically obvious nouns and verbs, because the two sets will have similar formal properties and syntactic behaviors; so syntactic categorization will proceed by extrapolation—or bootstrapping—from the easy cases to the more difficult ones.

Now, Pinker claims that the semantic bootstrapping hypothesis is in principle testable: All you have to do is control the linguistic input the child receives so that s/he hears only sentences which are syntactically ordinary but in which nouns never refer to people or objects, and verbs never refer to actions. That is, the child hears only sentences like *Running resembles taking naps*, or *Happiness seems elusive*. (Pinker remarks that you might also need to posit a genetically engineered child whose language learning does not get short-circuited because of boredom with abstract discourse.) If the semantic bootstrapping hypothesis is correct, such a child should evince no syntactic learning, since the necessary semantic cues will not occur in the language s/he hears.

There are (at least!) two reasons why this thought experiment cannot be carried out in real life. First, there's the ethical reason. But besides that, it would be impossible to get the child's caretakers to restrict their utterances to the necessary sentence types (though one could conceivably program a computer to do the job, in the not too distant future). Still, indirect evidence can be gathered to test the hypothesis, and the thought experiment points the way to some tests.

One obvious question is whether parents, in talking to their infants, generally do use nouns to refer to people and physical objects, and verbs to refer to actions. Pinker's survey of published parent-child discourse suggests that they do, though systematic data collection with this question in mind would be necessary to prove the point. Second, when children first produce syntactically analyzable utterances—that is, starting at the two-word stage—do their first nouns refer to people and objects, and their first verbs to actions? Pinker points out that the semantic bootstrapping hypothesis would be in deep trouble if they did not. And so on: One can devise a test for noun and verb categorization, keeping in mind both the hypothesis and the thought experiment that suggests what would *not* promote syntactic category learning.

Let's look at one other example of this general type of linguistic thought

experiment, from a different area of linguistics, to highlight further what I take to be linguists' main reason for using the device. This example comes from historical linguistics, the study of language change—an area which, like language learning, is hard to study by direct experimentation, though not for the same reasons. In most areas of historical linguistics, the main difficulty with experiments is the time required for conducting one: There is no analogue of the biologist's beloved *drosophila*; the only relevant generations are human ones, so we have to wait several hundred years to see the final outcome of a particular complex set of linguistic changes.

One controversial question in historical linguistics is this: To what extent can languages in contact influence each other? In particular, can speakers of one language borrow so many words and/or so many structures from another language that the borrowing language has to be considered a "mixed language" in a strong sense of that term? If this is possible, there could be serious implications for the theory and methodology of historical linguistics, because of the crucial importance of what we call the Comparative Method. This method is used both to establish that two or more languages are "genetically related" (in our standard biological metaphor) and to reconstruct portions of their undocumented, prehistoric parent language. One major theoretical assumption underlying the Comparative Method is that any given language is a changed later form of its single parent language; and since a mixed language would have (in effect) at least two parents, it could pose a problem for the application of the Comparative Method.

In the controversy over mixed languages, one area of disagreement has to do with defining the term: How much borrowing makes a language mixed? A rather common proposed criterion is that to be mixed a language would have to have derived exactly half of its structures from one language and half from another. But since no one has proposed a way of counting structures—you can't easily compare (for instance) a single distinctive speech sound to a single syntactic pattern—it's hard to see how this criterion could be applied in any useful way. Framing the question in these terms tends to get the discussion bogged down in rather trivial disputes, of the yes-it-is-no-it-isn't variety. And when a particular language is proposed as a candidate for mixed status, the unresolved definitional issue makes it difficult to judge the case. But if we take seriously the claim that an *unmixed* language is a changed later form of its single parent language, it is easy to devise a thought experiment to specify a mixed language that will fit anyone's set of criteria.

Basically, the phrase "changed later form of a single parent language" implies the following historical process: continuous transmission of an entire language—words, sounds, grammatical structures—from generation to generation, with relatively minor changes between any two generations. So, for the thought experiment, suppose that you, as a native speaker of English, replace all your English vocabulary items with Russian ones, while keeping

all your English grammatical structures. Once you do that, you are no longer speaking English, because the vocabulary is not English; and you are not speaking Russian, either, because the grammar is not Russian. Surely the language you are now speaking is a genuine mixed language. That is, it surely is not a changed later form of any single parent language, evolved gradually through continuous whole-language transmission.

So far so good: Everyone involved in this particular controversy will agree that this would indeed count as a mixed language. But, though the thought experiment does that job well, it does not resolve the main issue, because people who believe that mixed languages do not exist will simply say that there are no real-world analogues to the hypothesized English-Russian mixture—and that there could not be any, because there are no known historical linguistic processes by which such a mixture could come about. So, to make a convincing argument, it is necessary to go on to the second step and provide an example as close as possible to that of the thought experiment. It turns out that there are a few such languages. The closest match in structure to the hypothetical English-Russian case is a Tanzanian language called Ma'a, which has mostly Cushitic vocabulary and a grammar that is almost entirely Bantu (primarily from the Bantu language Pare, and secondarily from the Bantu language Shambaa).

Notice that the success of the argument does not depend on our being able to say just how a language like this arose. If there is no known historical process that could have had such a result, that shows only that there are historical linguistic processes that we do not yet know about. Certainly nobody would seriously suggest a deliberate, instantaneous replacement of native-language vocabulary by foreign vocabulary, as specified in the thought experiment; and in fact Ma'a in its present form seems to have developed gradually over a 300-year period, not instantaneously. Moreover, it turns out that Ma'a did not arise through borrowing of Cushitic vocabulary by Bantu speakers, as the thought experiment might lead us to expect. Instead, it arose in the opposite way, through borrowing of Bantu grammar by Cushitic speakers who kept their own vocabulary (Thomason 1983). So the process hypothesized in the thought experiment is not at all close to what actually happened in the history of Ma'a. Nevertheless, the thought experiment has served its purpose by identifying the necessary type of mixture in such a way that the evidence of an actual case will be immediately convincing: The Ma'a case matches the result of the thought experiment, even if that result was achieved by quite different means.

### III. THOUGHT EXPERIMENTS AS TESTS OF HYPOTHESES

Let us turn now to the other kind of linguistic thought experiment—the kind that involves introspection, by the linguist or by an informant (a

native speaker of some language the linguist is investigating), about the appropriateness of a particular linguistic form or construction. Thought experiments of this type are actual tests of hypotheses about language structure. If, for instance, my tentative structural analysis predicts that a particular sentence should be grammatical in a given language, then getting a native-speaker judgment about its grammaticality will test the validity of my analysis. Notice that I am not talking about data collection *per se*—that is, about answers to such questions as “How do you say ‘potatoes’ in your language?”—but rather about introspective judgments as to whether one would use a particular pronunciation or sentence, and if so, when. Answers to questions of this sort can provide crucial evidence for or against a theory—sometimes a theory about language in general, but more often a hypothesis about the structure of a particular language, usually with theoretical implications about language in general.

First, here is an example of a thought experiment that tests a general claim in historical linguistics. Picard recently made the following assertion about sound change: A change from [p] to [č], he said, is “simply impossible—there can be absolutely no question about that” (1984:427). When I read that claim it sounded wrong to me, because I knew of a pair of related languages (not the ones Picard was discussing) in which [p] in one language corresponded to [č] in the other, in a large number of words—a situation that could only have arisen through sound change. So I performed an introspective thought experiment to see if I could make some phonetic sense out of a change [p] > [č], by pronouncing combinations of [p] with various following sounds to provide a context for the change. (Of course, the languages that made me suspicious of the general claim could have undergone a change in the opposite direction, from [č] > [p], or both sounds could have come from some quite different third sound; but I started with [p] > [č] because of the claim I wanted to test.) When I concentrated on phonetic contexts of the type Picard was discussing, with [p] before a vowel [i] (or, better, before [y]), that is, the phonetic sequences [pi] and [py], I found that I could shift quite easily from a phonetic sequence like [pɣa] to [pča]; and from there it would be easy to explain a simplification to [ča], so that, if the intermediate steps were undocumented, the whole change process would look in retrospect like [pɣa] > [ča]. (The phonetic details of the process I was imagining need not be described here; a full discussion appears in Thomason 1986.) With a similar phonetic argument I could also explain a change [pi] > [či].

This bit of introspection satisfied me, but it may well not have been convincing to others as a demonstration of the phonetic reasonableness of a change from [p] to [č]. Though I was sure of my phonetic ground, my argument was still hypothetical as a historical claim, and in general linguists are not inclined to accept theoretical arguments without some concrete supporting evidence. In other words, the phonetic reasoning

would have little practical value unless actual examples of such changes could be found. So I took the second step that is typical of the more general thought experiments discussed above and searched for real-world examples. This search turned up a sizable number of clear examples, some of them documented changes, in various parts of the world—notably in Europe, in some changes from Latin to several different Romance languages, and in southern Africa. The examples showed all the intermediate steps I had thought of, and also some that I had not thought of.

Now, the story of this thought experiment is certainly similar in some respects to the thought experiments described in the previous section. First I posited an imaginary historical situation, with an imaginary process of change; then I presented real-world analogues of the hypothesized situation. As in the other historical example, the argument that a particular type of change *could* happen was not completely convincing until it was shown that such a change *had* in fact happened. But there are also some obvious differences between this example and the others discussed so far. The most important difference is that in this one I constructed the imaginary process of change through direct experimentation with my own pronunciations of various sound sequences, until I found a plausible sequence of events for a historical process [p] > [č]. This procedure could conceivably have constituted the entire argument, because the hypothesized historical stages corresponded to actual pronunciations; they were not, as in the case of the transplanted infant or the language-deprived child or the mixing of English and Russian, hypothetical events. Nevertheless, real historical examples were needed to make a solid case. Why?

The answer to this question is that introspective judgments about language are notoriously subject to the experimenter effect: Knowing what the theory predicts tends to sway linguists' and informants' judgments about what is and isn't acceptable in their speech. So, for instance, my effort to find a phonetic route by which [p] could change to [č] could well have influenced my pronunciations of the [py] and [pi] sequences, or my interpretation of those pronunciations, or both. It is a truism in phonetics that as soon as you start thinking about how you pronounce something you do not pronounce it that way any more. In practice, things are not quite as bad as this saying suggests—it is possible for an experienced phonetician to apply limited controls to introspective judgments about what s/he says—but all the available experimental evidence shows that speakers (including linguists) often have beliefs about what they say that simply do not correspond to what they actually do say. This means that, if a linguistic point is disputed, the introspective judgments of the opponents in the controversy are worthless as evidence for either side. And that is why I needed real historical examples to support my argument that [p] > [č] is a possible sound change: I was using myself as an experimental subject, and my evidence was suspect.

In syntax, too, introspective judgments on tricky points are subject to the

experimenter effect. In this domain the methodological difficulties involved in overcoming the problem are much more serious than in phonology, because in syntax introspection is probably the major means of hypothesis testing, and nonintrospective experiments, with proper controls, are harder to carry out here than in phonology. Labov (1975) discusses several examples of cases in which the analyst's introspective judgment seems to depend on the analyst's theory. According to a particular theory of Chomsky's, for instance, the English sentence *We received plans to kill me* should be grammatical, while the sentence *We received plans to kill each other* should be ungrammatical. And, in fact, Chomsky's own intuitions agree with these predictions. Labov tried these sentences out on non-linguist subjects and found that their judgments about the grammaticality of the two sentences were just the reverse of Chomsky's. Only linguistic graduate students who were familiar with, and in agreement with, Chomsky's theory turned out to agree with Chomsky's judgment on the two sentences.

Labov's conclusion is that syntacticians need methodological safeguards to protect them from the consequences of the experimenter effect, and he proposes that properly controlled nonintrospective experiments be carried out, using subjects who are not familiar with the theoretical issues involved, whenever native speakers of a language disagree in their introspective judgments of grammaticality. Labov defines a good experiment as the use by a subject of "his linguistic competence without reflection or introspection, applying the rules of his grammar to the interpretation of sentences in a natural context" (1975:125). Such experiments can be devised if the linguist is ingenious; but few syntacticians bother with them. Replacing introspection with real experiments of this sort would be immensely time-consuming, and therefore impractical for most syntactic research—and also unnecessary for most points on which all native speakers agree.

The problems inherent in testing hypotheses by means of introspection can be even worse when the linguist must rely on the judgment of native-speaking informants for a language that the linguist does not speak. An analysis of the structure of a previously undescribed language proceeds roughly in the following way: First the linguist elicits data from an informant—basic vocabulary items, paradigms, various sentence types. Then, fairly early in the process, the linguist begins to form tentative hypotheses about the language's structure, based on the material s/he has collected so far. S/he then tests these hypotheses by asking such questions as, "Can you say this?" and "When would you use this construction?" The value of the answers to these questions depends to a great extent on the linguistic sophistication of the informant, which in turn depends on the informant's ability to think abstractly about language. (This ability often correlates with the informant's level of formal schooling, because grammar lessons—either in the native language or in some other language—encourage people to think abstractly about language.)

So, for instance, suppose that you are trying to figure out how to count

things in Flathead, an Indian language spoken in Montana. You have discovered that the word for "two" is *'esel*. When you count blankets, you just add the numeral to the word *sičem* "blanket": *'esel sičem* "two blankets"; similarly for other things, e.g. *sq'elt* "backpack": *'esel sq'elt* "two backpacks." But when you ask for a translation of "two Indians" ("Indian" = *sqelix*"), you get *česel sqelix*", and you also get a [č]- prefix on other numerals, e.g. on *'upen* "ten": *č'upen sqelix*" "ten Indians." The same pattern is found with other nouns denoting people, such as *šmen'* "enemy": *č'upen šmen'* "ten enemies" (but *'upen sq'elt* "ten backpacks"). Your initial hypothesis is that a prefix *č* is added to any numeral for counting people. To test this hypothesis, you ask your informant whether it is every possible, in any context, to say *'esel sqelix*" for "two Indians" or *'upen šmen'* for "ten enemies," i.e. without the *č*- prefix on the numeral. It is quite likely that your informant will think a bit and then say no. It may therefore be a long time before you discover by accident, when it happens to turn up in a sentence you've elicited for some other purpose, that it is indeed possible to use the unprefixated numerals for counting people—provided that the "personness" of the item counted is marked elsewhere in the sentence, specifically on a verb that governs the noun as an object.

The point, in the present discussion, is that an informant who lacks talent for experimentation through introspection may not be able to think of an example of a desired construction type even if it is perfectly grammatical and occurs rather frequently. My colleague Terrence Kaufman, who has much more field-work experience than I have, has observed that it is a rare informant who will volunteer a linguistic form without being asked for it directly, unless it is a fairly common form (personal communication, 1986).

#### IV. CONCLUSION

It seems to me that this last sort of thought experiment—examining one's own native-speaker linguistic intuitions for evidence about the acceptability of a particular construction in a particular language—is quite far removed from thought experiments of the kinds discussed earlier in this paper. Nevertheless, judgments of appropriateness, like thought experiments that set the stage for the presentation of evidence in support of a theoretical claim, involve the positing of an imaginary situation as an aid in making the issue clear. Typically, in order to make a grammaticality judgment, or even a judgment about how you pronounce something in your own language, you have to try to imagine a context in which the construction or pronunciation might occur. If you cannot, then you conclude that it does not occur in your language.

But the purpose of introspective experimentation on one's own language is quite different from the purpose of the other kind of thought experiment: The introspection is an actual test of a hypothesis, while the stage-setting thought experiment is not itself a test, though it may suggest tests that

the linguist could carry out. The other major difference between the two types of linguistic thought experiment is that stage-setting thought experiments normally constitute only the first step in an argument; the next step is to find real-world examples that fit the thought experiment as closely as possible. The second step does not exist in introspective experiments on one's own language, because the introspective judgment is the real-world evidence—at least when the linguist's or the informant's judgment is not disputed.

Finally, though all linguistic thought experiments are probably subject in some degree to the experimenter effect, this effect becomes a major issue only with introspective thought experiments—not only those which test hypotheses about one's own language, but also those which, as in the [p] > [č] example, test hypotheses about language in general. While this problem may not be peculiar to the field of linguistics, it does constitute a salient difference between linguists' thought experiments and those of other scientists.

#### NOTE

1. I gratefully acknowledge helpful comments by Richmond Thomason and Nuel Belnap on an earlier draft of this paper; but they are of course not responsible for any remaining errors or infelicities.

#### REFERENCES

- Herodotus. *The Persian Wars*.
- Labov, William (1975), Empirical foundations of linguistic theory. In Robert Austerlitz (ed.), *The Scope of American Linguistics* (Lisse: The Peter de Ridder Press), pp. 77-133.
- Picard, Marc (1984), On the Naturalness of Algonquian *l*. *International Journal of American Linguistics*, vol. 50, pp. 424-37.
- Pinker, Steven (1984), *Language Learnability and Language Development* (Cambridge, MA: Harvard University Press).
- Thomason, Sarah G. (1983), Genetic Relationship and the Case of Ma'a (Mbugu). *Studies in African Linguistics*, vol. 14, pp. 195-231.
- Thomason, Sarah G. (1986), On Changes from Palatalized Labials to Apical Affricates. *International Journal of American Linguistics*, vol. 52, pp. 182-86.

the linguist could carry out. The other major difference between the two types of linguistic thought experiment is that stage-setting thought experiments normally constitute only the first step in an argument; the next step is to find real-world examples that fit the thought experiment as closely as possible. The second step does not exist in introspective experiments on one's own language, because the introspective judgment is the real-world evidence—at least when the linguist's or the informant's judgment is not disputed.

Finally, though all linguistic thought experiments are probably subject in some degree to the experimenter effect, this effect becomes a major issue only with introspective thought experiments—not only those which test hypotheses about one's own language, but also those which, as in the [p] > [č] example, test hypotheses about language in general. While this problem may not be peculiar to the field of linguistics, it does constitute a salient difference between linguists' thought experiments and those of other scientists.

#### NOTE

1. I gratefully acknowledge helpful comments by Richmond Thomason and Nuel Belnap on an earlier draft of this paper; but they are of course not responsible for any remaining errors or infelicities.

#### REFERENCES

- Herodotus. *The Persian Wars*.
- Labov, William (1975), Empirical foundations of linguistic theory. In Robert Austerlitz (ed.), *The Scope of American Linguistics* (Lisse: The Peter de Ridder Press), pp. 77-133.
- Picard, Marc (1984), On the Naturalness of Algonquian / *International Journal of American Linguistics*, vol. 50, pp. 424-37.
- Pinker, Steven (1984), *Language Learnability and Language Development* (Cambridge, MA: Harvard University Press).
- Thomason, Sarah G. (1983), Genetic Relationship and the Case of Ma'a (Mbugu). *Studies in African Linguistics*, vol. 14, pp. 195-231.
- Thomason, Sarah G. (1986), On Changes from Palatalized Labials to Apical Affricates. *International Journal of American Linguistics*, vol. 52, pp. 182-86.