

GENERATIVE GRAMMAR AND THEORIES OF LANGUAGE CHANGE
(Methodological and historical aspects)

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0.1. "Historical linguistics and general linguistics have had a long divorce this century, though overtures of rapprochement have been more frequent recently" — states professor Collinge in the Preface to his collection of articles (Collinge 1970). It seems that for this rapprochement the theory of generative grammar (henceforth: GG) may be held responsible to a much greater degree than any other current theory of language structure. There are, perhaps, three reasons for this revival:

a. GG has proved to be, to say the least, one of the most stimulating linguistic theories of the last decade, and since the early sixties some of its proponents (e.g. Halle 1962; Klima 1964; Closs 1965) have naturally turned their attention to historical linguistics in order to:

1. find new empirical data which might have bearing on the problems of language universals (cf. G. Lakoff 1965: 88 - 90 on *agress; Gruber 1967: 135 on be=by¹; see also Postal 1968; R. Lakoff 1968; Kiparsky 1968),
- or 2. develop a theory of change which would incorporate the findings of GG (e.g. King 1969) and would in turn enrich the theory itself (e.g. Kiparsky 1968).

In this article we shall refer to all approaches subsumed under a. above as the **GENERATIVE (or GG) THEORY OF CHANGE**.

b. In the late sixties a number of issues pertinent to the theory of change were raised by linguists interested primarily in dialectological studies. They have tried to formulate a unified theory of language variation which would account for language change of any type (e.g. evolution, completed change,

¹ Gruber wrongly assumes that *be-* in: He *befogged* the issue comes from OE *beon* "be". Actually, it goes back to an OE adv. and prep. *bi*, *big* "by" with the original meaning "around". The fact would even better fit Gruber's model.

change in progress). Those, as they will be called here, VARIATION THEORIES of change contain the most outspoken criticism of generative theory though often use similar terminology and type of argument in their attempts at formulating a general theory of language.

c. Historical linguists who have always kept a check on the newest developments in general linguistics were ready to accept at least some machinery of GG theory of language and offer some proposals (e.g. Birnbaum 1970; Collinge 1970; Katičić 1970) which may somewhat overoptimistically be called GENETIC RELATIONSHIP MODIFICATIONS. These linguists were first to notice some obvious similarities² between nineteenth century language thought (with its interest primarily in language history) and some hypotheses of GG (a theory which had started as a strictly a-historical discipline). Collinge's attitude towards the "transformationalist's chivalrous interest in historians" seems to be typical of what historical linguists expect from GG: "formal theory seems likely to restore to historical linguistics some of its lost dynamic and digging power. And diachrony may conversely be able to offer the generativist a separate but analogous model — that of a 'universal grammar of language change'...". (Collinge 1970: 188 - 189)

0.2. In what follows we try to show why the GG theory of change has proved to be much less successful than it could have been (in §§ 2.0 - 2.3) and why variation theories of change fail to offer a plausible alternative to numerous GG proposals (§§ 3.0 - 3.1).

Section one (§§ 1.0 - 1.3) presents a set of heuristic questions formal and explicit answers to which could form "pure theory of change". Since these questions are only the sum total of all those that either had or could have been asked by the proponents of the three "schools" listed above, their value (and the value of the whole section) is purely referential.

Genetic relationship modifications have been given a cursory treatment only. The three monographs quoted under c. above were utilized, among others, as a touchstone and a source of empirical data which had not or could not have been accounted for within other approaches. We do not provide any examples ourselves for two reasons. First, because in a work of this type they would have to come from various languages and be of a rather anecdotal character; second, because the work is intended to form an introduction to a descriptive analysis of Old English lexicon. In any case, we think such examples outside any theory to be of a limited value.

² The obvious similarities go back to Herder and Humboldt who still had an impact on nineteenth century language thought (both comparativists and neogrammarians, see: J. Viertel 1966: 109 - 132; H. Muellor 1966: 97 - 108, and K. Percival, MS). For other equally obvious though largely superficial similarities see: O. Jespersen 1924: 30 - 35.

1. SOME PROBLEMS FOR A THEORY OF CHANGE

1.0. We shall assume here that no theory of change is possible without previous groundwork in the theory of language. In other words, general linguistics should minimally provide the historical linguist with an answer to the question: "What is language?" which will usually be reinterpreted in the light of a theory of change as: "What in particular changes if language changes?"

The theory of change proper, as evident from numerous works in historical language study, deals with three related sets of problems — RATIONAL, GENERAL, and HISTORICAL — solutions to which depend both on answers offered by general linguistics, and on hypotheses made within the theory of change itself. A sketch of those options that we present below does not aim to be exhaustive, but it seems to be a sufficient characterization of what is meant by "theory of change" by the representatives of generative, variation and genetic relationship theories.

1.1. *Rational* problems (i.e. Why do languages change of necessity?)

Differences between the possible approaches to the problem result from different views upon: (1.1.1.) "What is change?" The concept of change is unfortunately not unambiguous in English and, consequently, many controversies result simply from different definitions (seldom explicitly provided) of this key notion of the theory. Thus "change" may refer to:

- a. fact of difference between two states of an object in time and/or social and geographical space (e.g. $L : : L_1$);
- b. process of differentiation (from an L-state to an L_1 -state);
- c. fact of difference between an L-state and a non-L-state of some object 0;
- d. process of differentiation (from an L-state to a non-L-state);
- e. directed differentiation process from L to non-L states (= "drift").

It should be added here that position d. logically entails (cf. Ajdukiewicz 1948) acceptance of the continuity postulate (i.e. "gradual change", cf. p. 66) while a., b., c. are neutral with respect to it (i.e. may be both "abrupt" and "gradual"). Change in the sense of e. would be trivial for any linguistic theory and Sapir's drift is a directed process of the b. type. The condition: "in time and/or social, and/or geographical space" is valid in all cases.

- Two other much discussed issues belong to this set of problems. These are:
- (1.1.2.) the "actuation riddle" — i.e. which factors may be taken to be "responsible" for the actuation of change,
 - (1.1.3.) the beginning versus spread controversy — i.e. is it valid to speak about the outset of a change as different from its propagation? The controversy arises in the b. and d. definitions of change and it is related to the query as to whether there are stages we observe (or posit) between states L and L_1 of the object.

1.2. *General* problems concern restrictions on the set of possible changes and conditions for change.

Differences between particular theories depend here on answers they provide to the following questions:

(1.2.1.) What are restrictions on language change and where are they to be looked for:

- a. in universal principles?
- b. in the organization of particular linguistic structure?
- c. in the organization of the speaking communities (dialects, styles)?
- d. in factors external to a given language (conquests, borrowings)?
- e. in combinations of a.-d. above?

1.3. *Historical* problems. (How to account for concrete changes that have taken place?)

This type of problem seems to be largely independent of other solutions. It deals with the format of grammars and conclusions from 1.1. and 1.2. On the whole the approaches may differ in:

(1.3.1.) methodological solutions:

- a. purely synchronic grammars for every L-state provided with some contrastive device,
- b. synchronic grammars "on historical principles",
- c. genetic grammars (phylogeny follows ontogenetic development),
- d. "dynamic" grammars (e.g. Bailey 1971),
- e. many stage comparative grammars (e.g. Closs 1965),
- f. "drift" grammars (e.g. R. Lakoff 1970).

(1.3.2.) importance particular linguists ascribe to the effects of concrete changes upon language structure, and/or speech community, and/or upon other linguistic or extralinguistic changes.

2.0. GENERATIVE THEORY

In GG theory language change is viewed as change in the speaker's competence — in the system of rules underlying his ability to speak the language. Thus, language change is equated with grammar change, and the notion of change vacillates between our definitions b. (cf. Halle 1962; Chomsky and Halle 1968) and a. (cf. Closs 1965; Jakobson 1970; McLaughlin 1970). In both cases change is definite (from L to L₁ and not L : : non-L) and abrupt. The direction of change is determined by the hypothesis concerning its beginning, which is furthermore distinguished from its gradual spread throughout a speech community (cf. King 1969: 117 - 119). That is, language change has its origin in the process of transmitting a grammar from one generation to another, and its direction is towards greater simplicity in terms of linguistic structure as a whole. There are generally two types of change distinguished: PRIMARY

CHANGE, i.e., a change in the rule component of grammar which involves rule addition, rule loss, rule reordering and rule simplification, with the proviso that only rule addition is possible in adult's grammar, children being responsible for other modifications; and RESTRUCTURING, that is, change in the underlying representation of which only children are capable. Since it leads to optimal grammars with respect to input data, change of this type may also be viewed as grammar simplification.

The actuation problem (1.1.2) has received little attention from GG theory. Postal (1968: 283), for example, says that the causes of change "without language contact lie in the general tendency of human cultural products to undergo 'nonfunctional' stylistic change", but the value of this explanation is meagre in the light of these "nonfunctional stylistic" changes being similar to changes affecting "automobiles (which) add fins one year and remove them the next". Besides, one may doubt whether within the GG framework one is entitled, albeit figuratively, to refer to language as "cultural product" (ergon). In consequence, the problem of drift was allotted no place in the framework, and the Jakobsonian teleological criterion (Jakobson 1927) has been found in grammar simplification alone, thus making the notion the key construct of the theory.

Unlike in the variation theory, speakers are unaware of change or its causes, since they "are, in general, unaware of the contents of their grammar" (Chomsky and Halle 1968: 250).

Restrictions on possible changes (1.2.1) come from universal principles of either formal (simplicity) or substantive character (naturalness or rule plausibility). Furthermore:

"Linguistic change is normally subject to the constraint that it must not result in the destruction of mutual intelligibility between the innovators (...) and the rest of the speech community (...). This restriction clearly affects the content of the rules to be added (...) the number of rules to be added must also be restricted, for very serious effects on intelligibility can result from the simultaneous addition of even two or three otherwise innocuous rules.

It may be somewhat less obvious that the requirement to preserve intelligibility also restricts the place in the order where rules may be added. All other things being equal, a rule will affect intelligibility less if it is added at a lower point in the order than if it is added higher up. (...) the type of change most likely to survive is the one involving the addition of a single, simple rule at the end of certain natural subdivisions of the grammar". (Halle 1962: 346)

As to what we have called the historical problem (1.3.1) generative theory does not preclude any solution. Hence, the purely synchronic approach was accepted by, e.g., R. Nagucka (1968) in her work on ME syntax, or K. H. Wagner (1969) in his phonological and morphological description of OE; the synchronic multi-stage contrastive investigations were those of E. Closs

(1965, 1972), E. Klima (1964), R. Jakobson (1970) and Ch. Jones (1972). Halle's original article (1962: 347) assumed rather the traditional Jesperssonian synchronic-on-historical-principles approach which was to be valid for at least the phonetic/phonological component of GG:

"The synchronic order of the rules will reflect the relative chronology of their appearance in the language. Moreover, under this condition, the proposed simplicity criterion can be used as a tool for inferring the history of the language".

Another interesting suggestion comes from J. Piaget (1968: 81) who clearly distinguishes synchronic, diachronic, AND generative descriptions of language change, the latter being independent of the two in the sense of our 1.3.1.-c (genetic description).

Our 1.3.2. problem concerning effects is either ill-formed for the theory at hand or posits further questions for which no solution has been offered. It is ill-formed if its solutions are to be arrived at from some criteria coming from the outside of GG language theory (e.g. effects of changes upon the community, upon communicative efficacy or drift of structures and the like). If effects are to be found with the help of the evaluation measure, the theory has to offer some serious restrictions upon the notion of "possible simplifications" of a formal and substantive character. The latter seem to be more important; the more so that the only one proposed by Halle (i.e. rule plausibility) does not seem to be free from imperfection (see: E. Bach and R. T. Harms 1970). The situation has been found irritating by P. Kiparsky (1968; 1971) who has, however, decided to reinterpret the question of effects as one concerning rule justification, i.e. instead of the posited problem (in 1.3.2) of how a particular change may bear upon language (whatever that means), Kiparsky proposes to deal with the question: how a given change adds up to justification of a given rule or rules (cf. Kiparsky 1968: 50 - 53).

2.1. The generative theory of change as presented above (§ 2.0.) appears uncontroversial and basically traditional in most of the solutions offered. That this impression would be mistaken becomes obvious as soon as one analyses critically the implications of all the hypotheses of such a theory of change. We would like to claim here that those implications would force the theory of change to either presuppose some serious modifications in the theory of language proper or clash with what seem to be basic assumptions of a generative theory of language.

The reader should be warned once again to notice the heuristic character of the present section. We are aware that both GG and its theory of change are being constantly modified. We are also aware of the restrictions upon the validity of our argument which come from our assumption that the theory of change depends on the theory of language. (Unlike Kiparsky we do not believe that at this stage the former may bear upon the latter). Furthermore, it is not of our

immediate concern to criticize the GG theory of language. What we do want to claim is that what King (1969) and Closs (1965) call a generative theory of change is either not "generative" (unless the term is to have some specific meaning, thus the theory is not intersubjectively testable) or not a theory at all (as internally inconsistent with GG theory in general).

It is in this light that the reader should view our criticism of the "modifications" implied by the theory of change and of such assumptions of GG theory of language as: the homogeneity hypothesis (§ 2.1.1.), the competence-performance distinction (§ 2.1.2.), the acquisition device model (§ 2.1.3.), the evaluation measure (§ 2.1.4.), the directionality indifference hypothesis (§ 2.1.5.), the notion of rule of grammar (§ 2.1.6.) and the so-far most controversial, universal base hypothesis (§ 2.1.7.).

2.1.1. *Homogeneity hypothesis. Realism vs. conventionalism.* GG viewed as a theory of natural language "(...) is concerned primarily with an ideal speaker-listener in a completely homogeneous speech community, who knows his language perfectly (...)" (Chomsky 1965: 3). To justify this position Chomsky (ibid.) adds: "this seems (...) to have been the position of the founders of modern general linguistics and no cogent reason for modifying it has been offered." And one has to agree with Chomsky with the proviso that GG in addition to its being grounded in tradition aims at formal and explicit characterization of natural language which (for reasons given in Chomsky 1965: 8) could not have been an aim of the previous generation of linguists. This, in turn, should compel GG theorists to be explicit on exactly those points on which traditional linguists were vague. The "realistic" versus "definitional" status of theories may be taken to be one such point.

The homogeneity hypothesis may perfectly well serve as a working hypothesis for a theory of change and it was accepted by e.g. Paul, Saussure, Kruszewski and de Courtenay. Apparently Chomsky's position is identical with that of Baudouin de Courtenay:

"We have as many individual Polish languages as we have brains endowed with the capacity for Polish linguistic thinking processes (...) And yet we usually do not apply the name 'Polish' to those objectively existing individual Polish languages. Polish or mother tongue is a term we apply to a non-existing fictitious average Polish speech a vague idea of which we get from our contacts with other members of the community. Similarly, the linguistic science to arrive at the concept of collective-individual Polish language has to carefully analyse all the presumably similar individual language thinking processes and all the more or less identical ways of manifesting and perceiving those processes (...).

Linguistic history is not a history of individual languages; what we want to present here is an outline of changes which affected the collective, average Polish language, or the collective-individual language thinking processes and the changes in the ways of manifesting those processes with the help of physical means". (Baudouin de Courtenay 1921: 3 - 5)

And yet it is surprising how explicit de Courtenay is with respect to the methodological status of his theory when compared to Chomsky or the proponents of the theory of change (e.g. King 1969 ch. 4 §§ 4.1 - 4.4.). Courtenay is a conventionalist and his theory of change is simply a set of statements which admit of modifications. Chomsky, however, is vague on exactly this issue. The quotation which opens this section suggests a stand like that of de Courtenay's. How then is one to understand Chomsky's statement (1965: 9) that:

"It (a generative grammar) attempts to characterize in the most neutral possible terms the knowledge of the language that provides the basis for actual use of language by a speaker-hearer."

Given that "competences" are explicitly claimed by the theorists of change to accept innovations and mutations (why should ideal speakers do that? — one may ask) this gives rise to serious objections as to the plausibility of the parent-to-child model of change (as presented e.g. in King 1969: 85). These objections as well as the homogeneity hypothesis itself have found their strongest opponents in the representatives of the variation theory of change, and we shall return to the problem in § 3.1.

It is only fair to close this section with a notice that some of the proponents of GG theory have explicitly accepted the definitional character of this model (cf. Kuiper 1973; Postal 1968; Lyons 1968). The issue lies not, as stated by Bailey (1971), in whether the theory describes the competence of a linguist (it surely does not) or a native speaker-listener. This hypothesis, however, plus any methodological assumption a linguist working within the realistic or conventionalistic frameworks would be forced to make, could bear heavily upon the problem of the choice between a syntactically oriented GG (or syntactophonological GG) and a semantically oriented one (or semanto-syntactic GG). The former more easily admits of conventionalist approach than the latter, and the vacillation of Chomsky's 1965 "integrated theory" may be characteristic of the model (cf. H. G. Bohnert 1969).

2.1.2. *Competence — performance.* Related to the homogeneity hypothesis is Chomsky's competence-performance distinction (see: Chomsky 1961; 1965). In most neutral terms the assumption may be stated as follows:

"We thus make a fundamental distinction between COMPETENCE (the speaker-hearer's knowledge of his language) and PERFORMANCE (the actual use of language in concrete situations)". (Chomsky 1965: 4)

The underlying competence should be viewed as a system of generative processes of which native speakers-hearers are generally unaware.

The assumption, if taken to be a restatement of the langue-parole (or code-message) dichotomy (with all the reservations made by Chomsky on various occasions, cf. Chomsky and Halle 1965: 459 note 2) presents the historical linguist with a problem of the origin of change. In other words,

given the well-attested variation in performance, and given the fact that competence does change (cf. § 2.1.1.) one may ask if performance variation may cause (=lead to) a change in competence. The question has been answered in the negative (e.g. King 1969: ch. 4; Postal 1968: 272). Thus GG has rejected the doctrine of lapses (cf. Sturtevant 1917: ch. 11) according to which parole mistakes may lead to a change in langue.⁵

The arguments which have motivated such solution are based primarily on phonological change evidence (cf. King 1969: 66 - 79) though some lexico-semantic modifications may also be accounted for (*goed — type in King, *ibid.*) providing we do not make a theoretically possible distinction between speaking ability and listening ability as suggested by Bailey (1971) who misinterprets Chomsky's (1961: 120 - 121) position: "It (a grammar) is not, however, a model of the speaker or hearer. It neither synthesizes particular sentences, as does the speaker, nor does it recognize the structure of presented sentences, as does the hearer. It is quite neutral as between speaker and hearer in this respect".

Bailey's (1971: 25 - 26) separation of "speaker's competences" from "hearer's competences" is an example of a long series of misconceptions of GG position for which the representatives of the theory may be blamed only in so far as they are often not explicit with respect to which of their postulates are axiomatic in nature⁶ (as noted in § 2.1.1.). Some of the misinterpretations have direct relevance for the theory of change, especially if one starts with the assumption (as practically all representatives of GG theory of change did cf. Halle 1962; Postal 1968: 305; King 1969: ch. 3) that:

"all change can be traced to the situation in which two dialects of a language have become different. (...) To say that dialects have become different is to say that the grammars of those dialects are different (...)". (King 1969: 39)

Even if one is ready to disregard the formal status of such notions as "dialect" ("language stage") there is still a number of further issues: is it, for example, possible that a speaker-hearer (let alone the ideal one) may be equally competent in more than one dialect? Any answer to this question will have to presuppose a notion of competence different from Chomsky's ("completely homogeneous speech-community"). A typical example of such equivocation is Stockwell's statement (Stockwell 1969: 232) which, according to its author, is typical for a GG theory of change:

⁵ Thus, it seems that the assumption that all native speakers have equal competence is axiomatic within the theory just like the speaking — listening indifference of the model while the content of competence and its function among other performance factors are among the working hypotheses subject to empirical verification. See: K. Kuiper 1973, on equal competence, and Bohnert 1969, on the possible dangers resulting from Chomsky's inexplicitness as to the content of idealizations admitted in his grammars.

"(...) (vowel) variation is psychologically discrete: where a native native speaker IS AWARE of variation, it can only be variation measurable in terms of contrastive relevance somewhere within the system of either his own speech or of a type of speech with which he is familiar. This is not as strong a constraint on a theory about the perception of variation as some might wish to place on it, at least for English: the strongest one would be that he can perceive only variants that are phonemic in his own speech. I think such a constraint is too strong, if only because few speakers of any language are monodialectal, and certainly few English speakers. The extent of familiarity with dialects other than one's own may vary a good deal, but (...) we all learn parts of the systems of dialects other than our own, and these are part of our store of norms to match against new phonic experience. (...) It may be doubted that the discrete steps of perceptible variation correspond with features in the system: but I do not see what else might serve to define these limits".

But if all this is to be included into the system, the notion of competence⁶ loses any theoretical significance. Native speakers are generally "aware of" a vast number of other linguistic and extra-linguistic facts so far disregarded by GG accounts of competence. For example: foreign accents, tone and rapidity of speech, style values, school-grammar influence, gestures, and even ideology and fashion would have "contrastive relevance". Such an approach contradicts both the homogeneity hypothesis (§ 2.1.1.) and the axiom that competence is equal for all speakers (cf. note 3). The interest is furthermore shifted from the speaker-listener's competence (in which there are only "parts of the systems of dialects other than our own") to a phonetician's competence who would be capable of reconstructing those parts which the speaker "has not learnt yet". Besides, as soon as "perception" is taken account of facts would force us to modify the assumption at hand so as to postulate a much more comprehensive PERCEPTION competence (Bailey's "hearer's competence") and a less extensive PRODUCTION competence as one can usually perceive much more of dialectal variation than one is able to produce (imitate?).

It is somewhat surprising that in a formalized theory of change one so easily gives up a metatheoretical condition postulated nearly a century ago by Kruszewski (1883: 57 - 58), a condition which has furthermore proved so useful since:

"... the following law does not need proof:

1. the sound system of a given individual is always roughly identical
2. the sound system is roughly identical for all the individuals within a given dialect and period of time

(... Individual variations are linguistically invalid and cannot be regarded exceptions to the law postulated above")

If a historical linguist questions the validity of Kruszewski's "law", it means that he either wants his grammar to be a description of a competence of a special type (quantitatively measured degree of perceptual awareness) or refuses to perceive the difference between constant variation and change that has taken place within $t - t_1$ time limits⁴.

⁴ So that we describe a difference between stage L and L₁ of "the same" object

Two other notions of competence⁵ (both largely independent of Chomsky's definition of the term) may be of interest to historical linguists. One has its source in the writings of generative semanticists who postulate that competence should be extended to encompass some elements of the knowledge of the world. It seems rather obvious that language history will have to include at least some social data. If this is to be done in the way suggested by Weinreich et al. (1968) or in still some other ways remains to be seen. In any case this will be a complication of any formalized theory of change.

The second approach originates in the historical writings of GG and by and large may be subsumed under the already discussed problems of linguistic competence versus definitional postulates. Thus E. Closs (1965: 332, note 6) states:

"Questions have frequently been raised concerning the feasibility of using this notion of grammar in historical analysis, in particular concerning the appeal to intuition. (...)

If we take in its strongest terms the requirement placed on linguistic theory that it should characterize and predict all and only the sentences of the language and also account for the native speaker's competence in producing and understanding the utterances of the language, we might ultimately conclude that a grammar can only be written by a native speaker, not a foreigner, and that grammars of dead languages cannot be written at all. (...)

(...) We may quite legitimately put forward a theory of a dead language, in terms of a grammar which fulfills the requirements of descriptive adequacy and explanatory power. This theory will be based on all observable data, and also on unobservable data when necessary, i.e. when the logical consequences of the model would not match the observable data without this hypothesis".

It seems to the present author that Closs must have misrepresented here the position of her critics (she does not mention any source of the criticism) which allowed her to defend forcefully and elegantly the non-existent issue (of whether foreigners may write GG grammars of e.g. English) while she distorted the key problem, viz. what a GG of a dead language is a grammar of? If such a grammar "fulfills the requirements of descriptive adequacy and explanatory power" — it means that it describes the competence of a native speaker-hearer of a dead language and furthermore that our theory of change is able to bear

given the restrictions concerning the maximum change which preserves the identity of the object itself.

For Chomsky-Halle (1968) synchronic ("on-historical-principles") description of ALTERNANTS the latter problem (i.e. variation : : change completed) simply does not arise. Cf. §2.1.1.

⁵ We disregard here some serious criticism of the competence — performance dichotomy coming from psycholinguistic quarters (cf. Campbell and Wells 1970). Some further definitions of the notions have been offered there which — though they have little bearing on the theory of change — point to the vacillation mentioned above.

upon the acquisition device hypothesis⁶. Both claims are either false or they contradict the GG theory of language. For as soon as we decide to call a "DEAD language" a "LANGUAGE" we simultaneously decide that GG will treat it as any other (living or dead) language in the sense that the intuitions of the native speaker of this language are restricted by identical innate predispositions as the intuitions of a speaker of any other language. Thus, never will a theory of change be explanatory in the sense of Chomsky (1965: 24 - 27). Such a grammar may be descriptively adequate providing we extend the notion "idealized native speaker's competence" to include the notion of the linguist's (qua native speaker of a dead language) competence. For given the data which are usually distorted by scribes of various periods and various dialects and the very slight or completely absent possibility of their verification what we describe is not a competence of any single speaker of any language but rather some fiction which we have largely arbitrarily agreed to call Old (Middle) English or some other dead language. Both of those operations are justifiable providing the analyst states openly that such assumptions have been made (as in de Courtenay 1921: 3 - 21).

2.1.3. *Acquisition device*. The acquisition device hypothesis (with the corollary problems of innate ideas, evaluation measure — see § 2.1.4. — and language learning) may interest the historical linguist on account of the importance attached to the hypothesis by generative theorists of change as well as on account of the weight that children and language learning have traditionally borne in older theories of change (of Humboldt, Bloomfield, de Courtenay and others).

The hypothesis has been stated by Chomsky (1966: 20 - 21) as a specification of the requirements that must be met if something is to qualify as a theory of language in the following way:

(...) "a hypothetical language-acquisition device AD that can provide as 'output' a descriptively adequate grammar G for the language L, on the basis of certain primary linguistic data from L as input; that is, a device represented schematically as:

$$\text{primary linguistic data} \rightarrow \boxed{\text{AD}} \rightarrow G$$

We want the device to be language-independent—that is, capable of learning any human language and only these. (...) Were we able to develop the specifications for a language-acquisition device of this sort, we could realistically claim to be able to provide an explanation for the linguistic intuition — the tacit competence — of the speaker of the language. This explanation would be based on the assumption that the specifications of the device AD provide the basis for language acquisition,

⁶ It is possible that Closs meant by her appeal to explanatory power nothing more than dependence of the theory of change upon the theory of language in general; in this case, no objections can be made to her approach but to Halle's parent-to-child language change model as represented graphically in King (1969: 85) which is highly counterintuitive and which has been unquestioningly accepted by Closs (1965: 311 - 312).

primary linguistic data from some language providing the empirical conditions under which the development of a generative grammar takes place".

It seems that the hypothesis in the form presented above and accepted uncritically by the GG theorists of change (cf. King 1969: 84 - 85) is untenable. Chomsky's shift of attention from the idealized speaker-hearer's competence to a much less abstract "real child's" (or language learner's) competence brings back the issue of confusing the speaker's knowledge of a language and a grammarian's knowledge of that language. The confusion was noticed by G. Harman (cf. Harman 1969: 143 - 151) in his review of *Aspects* and raised again by McCawley (1968: 560). Since the criticisms in the aspects relevant to this article are basically similar we shall quote here only McCawley's opinion for its conciseness and poignancy:

"The flaw of this account of the relationship between language acquisition and linguistic theory is this last point, which in effect likens the child to a linguist who elicits ten notebooks full of data from his informants in New Guinea and doesn't start writing his grammar until he is on the boat back to the United States. Language acquisition involves the child's being continually in the process of constructing and revising his grammar, the appropriate diagram for the process not being that given by Chomsky but one involving feedback:

$$\text{data} \rightarrow \boxed{\text{AD}} \rightarrow G$$

Since language acquisition takes place via some such trial-and-error process rather than by the accumulation of a large body of unanalyzed data, it is not evident that any notion of evaluation measure is needed to explicate learning".

The last paragraph of McCawley's criticism which seems to be fatal for the proposed theory of change (with its reliance on simplicity) will be further discussed in § 2.1.4. But even modification of the hypothesis, by including into it a feedback mechanism, points to a serious inadequacy of the proposed theory of change. For if this theory is to be able to account for a situation in which the child constructs an optimal grammar different from or rather inconsistent with the data provided by his informants with the help of a feedback mechanism, the theory has to give a characterization of possible revisions which a learner may make in his grammars, given a grammar and a datum inconsistent with that grammar. Viewed in the light of a theory of change such characterization will not differ from the characterization of drift⁸ as understood by Sapir

⁷ In a footnote (ibid.) McCawley states:

"I have intentionally omitted the words 'primary linguistic' from before 'data': any data which give the child any information about the meaning of a sentence or a morpheme may play a role in language acquisition, even data which are neither 'primary' nor 'linguistic'. Data must here include not only things that the 'informants' say, but things that the learner himself says and his observations of the results which follow upon saying them".

⁸ The concept of "drift" has heretofore been either replaced with that of "simplification":

(1921: ch. VII), and it might be thought of in terms of substantive constraints on change.

2.1.4. *Simplicity*. The simplicity hypothesis has turned out to be the key concept of all generative theories of change⁹. For example King (1969: 64) states:

"In the preceding chapter (...) four individual categories of change were isolated: rule addition, rule loss, rule reordering and simplification. There do not seem to be other major kinds of change that cannot be reduced to one of these four, and indeed two of the four — rule loss and reordering — can be understood as belonging to simplification in its broadest sense".

Furthermore, if one remembers that according to the theory it is the child that is "responsible" for language change for the reason that:

"The ability to master a language like a native, which the children possess to an extraordinary degree, is almost completely lacking in the adult". (Halle 1962: 344)

and if "...changes in later life are restricted to addition of a few rules in the grammar..." (ibid.) it becomes obvious that the notion of simplification should be given special attention in methodological considerations of GG theory of change. Such is the case, however, only in Halle's article where the notion of simplicity is taken intact from the general theory (cf. Chomsky 1965: 37ff.) and applied to change. Thus even to this article McCawley's criticism does apply:

"It should be noted that no explicit proposal has ever been made for an evaluation measure of the type which Chomsky envisions (a numerical measure of 'cost', in which the various rules and dictionary entries make a specifiable contribution to the 'cost' of the grammar and the grammar with the lowest 'cost' is 'most highly valued'). While proposals have been made for evaluation measures defined on various parts of a grammar, notably phonology, *no proposal has ever been made which would allow the costs of phonological rules, transformations, phrase structure rules, dictionary*

"...nothing about the drift is particularly mystical. Simplification is a fact of language development... It is a universal notion. It is not surprising that some daughter languages should undergo identical but independent simplifications of a rule inherited in common from the parent language" (King 1969: 202), or it has been relegated to concepts from outside the theory of change proper on par with changes caused by rule borrowings (as in Postal 1968: 257).

⁹ It seems that the reliance on simplicity as well as various equivocations of the term may to some extent be explained by a (definitely superficial) similarity of the notion to such traditionally accepted concepts as the principle of least effort, language drift towards economy, or Jespersenian idea of languages becoming "simpler" in time. The proposed GG theory of change will not be able to elaborate traditional notions since the general theory of language differs too much from those of previous scholars in explicitness. R. Lakoff (1970) suggested that the two types of approaches may be complementary if GG theory of change accepts the notion of drift. Her article, however, differs from all others to such a degree that it could not be regarded as belonging to the theory discussed.

entries, and semantic interpretation rules *to be measured in the same units and a total cost assigned to the entire grammar; indeed, even in phonology, where most of the concrete proposals for evaluation measures lie, no one has made any satisfactory proposal for the contribution to cost made by such non-phonological items as syntactic categories and morphemic features like inflectional type*". (McCawley 1968: 560, italics added)

But this should be of vital importance to the theory of change, for how may one claim that English grammar has been simplified between OE and Modern English if the cost of inflection is unknown (as Closs does claim)? McCawley's criticism cannot be overestimated since if correct it is lethal for the theory of change as formulated. Thus if the numerical measure as proposed by Halle (1962: 335 - 342) is proper only for phonology, that is if McCawley is wrong in his suggestion that an IDENTICAL measure be extended into morpho-phonemic and syntactic levels, then the whole argument of Postal (1968) based on historical data (ch.'s 10 - 15) and concerning the non-autonomous status of phonology fails. On the other hand, if Postal is right and if it is impossible (as seems to be the case) to apply the same measure to both phonological and non-phonological components, then Closs, Kiparsky, King and Halle's suggestions that syntactic and phonological changes are governed by the same paradigm is unmotivated, and simplicity should be defined for each level of change separately which would in turn contradict Chomsky's formulation of the hypothesis (1965: 37ff). Of course it is possible that this measure will be defined in some future, but then the claims of all historical linguists working on other types of change than phonological use the term unlawfully. That one of the alternatives holds is evident from the state of the art. While papers and monographs in historical phonology abound (cf. King 1969; Kiparsky 1968, 1971 for bibliographies) no comparable achievement has been noted in historical syntax (cf. Winter 1971: 145 - 156 for a criticism of the situation).

To illustrate other types of confusion resulting from an overburdening of the term 'simplicity' in generative theories of change we may quote Stockwell (1969: 243):

"One's convictions about length in the vowel systems of earlier English are correlated with one's choice of theoretical mirrors for the reflection of relations that are inferred at many removes from citable data. It would seem to follow, therefore, that considerations appropriate to theory formation in more general terms should play a bigger role in historical English phonology: considerations such as simplicity, symmetry, elegance".

And yet Chomsky has always been at pains to elaborate on the misconstrued status of an evaluation procedure for grammars (i.e. theories of languages). Simplicity (cf. Chomsky 1965: 37) was "a notion to be defined within linguistic theory along with 'grammar', 'phoneme', etc."

Similarly King (1969: 176) while discussing comparative reconstruction misuses the term:

(...) "using some version of a 'simplicity metric' we would state rules that economically describe the development of each proto-phoneme into the daughter languages. This simplicity metric would count rules and features in a way that penalizes the reconstruction of proto-forms necessitating ad hoc rules (...).

The grammar resulting from comparative reconstruction (...) is no grammar at all in the sense of generative grammar".

And he does this in spite of Chomsky's warning that "such a measure is not given a priori in some manner and cannot be used to compare different theories of grammar" (Chomsky 1965: 38).

Finally, one may assume that the notion used by GG historical linguists is by definition different from Chomsky's simplicity measure. In this case what the theory lacks is a clear definition of its key concept — a solution equally discouraging for its prospective proponents.

2.1.5. *Directionality*. The directionality (or rather "directionality lack") assumption might be related to three issues important to diachronic language study. One is the autonomy of phonetic changes versus changes conditioned in terms of grammatical-semantic structures. The other is the neogrammarian axiom of regularity ("phonetic laws admit no exceptions")¹⁰. The third one is a well grounded opinion of traditional linguistics that sound changes form the starting point and lead to changes at other levels of linguistic structure (cf. Hockett 1958 § 54; Milewski 1965: 187; Kruszewski 1883: 109).

It is necessary to stress at this point the difference between this section and all previous ones. Directionality is a technical term in GG theory and as such should have no direct influence upon any of the empirical findings of a theory of change. Just in this case, however, a theory of change could supply the justification for one of the possible solutions concerning the direction of derivation¹¹. The theory, however, remains silent on this issue. Thus what we intend to present in this section are various approaches of both generative and traditional historical linguists to the three issues mentioned above which bear on the problems related to the direction of change, "directionality" being here a term independent of its technical meaning (cf. note 11).

¹⁰ For an excellent critical review of the neogrammarian position see Weinreich et al., 1968. There may be some doubts, however, if Kruszewski should be included among the representatives of the school. Accidentally, the title of Kruszewski's 1881 paper on which Weinreich seemed to have based his opinion is:

Über die Lautabwechslung (Kasan: Universitätsbuchdruckerei, 1881); and not as in Weinreich's bibliography (1968: 192): Über die Lautabwandlungen.

¹¹ What is suggested here is not that directionality has empirical consequences, but that the theory of change should not disregard the problem. The discussion of the issue may be found in Chomsky 1969a and in Lakoff's 1971 "On generative semantics". Basically, both represent the identical position that it makes no sense to speak of the direction in which a grammar generates semantic and phonetic representations. Thus, a derivation from a semantic representation (S) to a phonetic representation (P) does not differ from a derivation from a P to an S.

Just as most of the solutions offered by traditional linguistic theories of change were consequences of the assumption that changes in parole lead to a change in langue, so the suggestions of Postal and King (1969) result from their hypothesis that any change (in Postal's case — that sound change) is grammar change (i.e. change in the underlying representation). Thus if phonetics and phonology form an integral part of any grammar the theory has to accept the non-autonomous view of change formulated by Postal (1968: 240) as follows:¹²

"(P-11) — the view that *some* regular phonetic changes take place in environments whose specification requires reference to nonphonetic morphophonemic and/or superficial grammatical structure".

Consequently exceptions to changes are admitted (Postal 1968: 276). We do not think, however, that any of those two solutions are either particularly revolutionary (cf. Postal 1968: 276, footnote 5: "The claim that phonological changes have exceptions is of course somewhat revolutionary") or that they are strictly dependent upon other discoveries of GG. In the XIX c. mentalistic theory of language change as represented by M. Kruszewski and J. Baudouin de Courtenay both hypotheses have been taken for granted. According to de Courtenay what changes is not sound but language thinking capacity. According to Kruszewski (1881: 26) the goal of language study is to formulate general rules which underlie language phenomena. As to the exceptions, both of them not only admit them, but also try to account for them in their theory of alternations¹³. Kruszewski (1883: 40; 85) openly formulates these hypotheses:

"(...) sound laws are simply regularities of coexistence and have come to be unmotivated deposits of long lasting vocal processes".

"Operation of a sound law may be discontinued by unconscious psychological factors".

¹² One may of course say that the situation as presented here is a reversal of what Postal actually did, i.e., that he first found that there were changes which had to be accounted for with the help of non-phonetic components and THEN posited the P-11 hypothesis. This might have been the case, though given the integrated theory of linguistic description, our original statement should not be unjustified. In any case, unlike Winter (1971) we do not want to make an issue of this (as evident from the discussion).

¹³ Out of his three groups of alternations (1967: 29 - 39) only one type may be accounted for in strictly phonetic terms, namely one of the type of the Germanic s:z alternation in the initial, intervocalic or prenasal position. Two other types, namely the war:gewe-son, and Haus: Häuser, Häuslein — types do admit exceptions, the last one being also morphologically conditioned. Here is Kruszewski's formulation of a rule which accounts for the latter type of exceptions:

$$N_{\text{stem}} + \begin{bmatrix} a \\ o \\ u \\ au \end{bmatrix} \rightarrow N_{\text{stem}} \begin{bmatrix} ä \\ ö \\ ü \\ äü \end{bmatrix} \quad \text{+ } \{-or, -lein\}$$

As to our third issue (i.e. does sound change lead to changes at other levels?) Kruszewski (1883: 109) again seems to be much more explicit than the representatives of the GG theory of change:

"Morphological changes which depend on perintegration of morphological units are possible without any stimulus on the part of a phonetic change".

"It seems to be impossible for a sound change to entail a semantic change; for sound does not exist as an independent unit and thus may have no psychological content in itself". (Kruszewski 1883: 83)

In GG theory we may only guess that some changes (like those described in Closs 1965) will affect the syntactic component of a grammar and that others will affect only the phonological component (as evident from P-II). But without an overall simplicity measure we are unable to state their interdependence as was usually done (in a rather informal way) in traditional descriptions of language change¹⁴. Thus in their attacks against structural theories of mainly autonomous phonology, the GG theory of change often becomes as atomistic as pre-neogrammarian theories of change¹⁵. Such a situation may of course result from a limited aim of the generativists who do not want to study language history but look for the best possible underlying representations of synchronic forms: This possibility will be discussed in § 2.2. In this case, however, the chapters of Postal's and King's monographs which have in their titles such words as "theory" and "change" should be treated as empty promises.

2.1.6. *Rule. Rule ordering.* It may be safely assumed that the notion of rule will uncontroversially remain in any formalized theory of change. The only controversy which may remain concerns the rule ordering hypothesis put forward by Halle (1962: 347) which states:

"that the synchronic order of the rules will reflect the relative chronology of their appearance in the language. (...) under this condition the proposed simplicity

¹⁴ Thus, e.g. Fisiak (1968: 76) discusses a well attested English loss of surface gender distinctions which he relates to phonetic/phonological changes:

-a
-e
-o → /o/ → /ø/
-u
-em (dat. pl.) -en (dat. pl.) /ø/

He states that the phonetic/phonological changes caused the morphological-syntactic modification. It may be doubted if within the present GG theory of change such facts could be presented, neither have we any principled basis for rejecting this view or for postulating two different grammars for the two processes. It should be obvious that we do not doubt that some grammar which would account for the same output is possible.

¹⁵ This may be taken as a slight oversimplification. We do not want to suggest that any theory of change exists as a fait accompli, but only that given the set of existing hypotheses and a number of monographs all of which admit to be generative and happen to be atomistic we want to avoid the weak points of a future GG theory of change.

criterion can be used as a tool for inferring the history of the language, for it allows us to reconstruct various stages of a language even in the absence of external evidence such as is provided by written records or by borrowings in or from other languages".

In this Halle again echoes Kruszewski's (1881: 26) insights:

1. necessary and possible is the study which aims at stating rules underlying language phenomena;
2. such study is a much more useful tool for discovering the relationship between languages of the PIE family as well as — to a degree — for reconstructing both the PIE proto-language, and proto-languages of individual groups".

The possible differences lie in the fact that Kruszewski wants his postulates to justify a synchronic study of "living dialects" while Halle's hypothesis has been taken as a guideline for a theory of change (though we would agree here with Kiparsky who sees the hypothesis as an attempt at justification of rule orderings in synchronic grammars). Furthermore, since Kruszewski's theory was basically definitional (in the sense that no native speaker was necessary to have a valid description of some proto-language) and also less formal, there would be no problem for his theory in case some external evidence forced upon the investigator an ordering different from the one which could have been postulated on the basis of the simplicity criterion alone. For Kruszewski external evidence would be decisive. In spite of the fact that the cases for insertion into a historical GG of a rule "out of order" are relatively few, the possibility of attaching more importance to simplicity than to external evidences has not been excluded from the GG theory of change, as has been neatly demonstrated by W. Winter (1971: 151 - 156).

2.1.7. *Universal base hypothesis.* The universal base hypothesis will be mentioned here in passing for two reasons. One is a belief held by e.g. Closs (1965) and Bach (1971: 161 - 162) which is often offered as a justification of why "there has been practically no work done on the history of syntax as compared to studies of synchronic syntax". According to Bach "the main problem is that we are simply so ignorant of the universal syntactic characteristics of languages that we can't get to first base in doing any kind of comparative work". The other is a similar hope of some historical linguists that if we accept the hypothesis historical studies would somehow become more promising. A typical statement of this kind would be:

"What we expect from TG in connection with diachronic studies is to show whether the base rules were the same for all stages of the development of a given language, and, if so, to show how the same deep structure of the sentence received different surface structures due to the changes occurring in performance (...)" (Lipińska: 1972: 215)

It has been proved, as reported by Bach himself (1971: 1 - 15), that our theories of language (both those of Chomsky, and generative semanticists) are

too powerful and that the question about the set of rules common to all languages has for the time being no empirical content, and thus the universal set of base rules is impossible. Hence, we must assume that Bach's statement quoted above did not refer to the hypothesis as such but to the need for substantive universals (of the type of Bach's substantive restrictions on transformations), as in his work quoted above. That they are a necessary precondition for a theory of change has been said before. But they are not enough. We must either have some additional restrictions on possible changes (a characterization of language family or language group) possibly as in R. Lakoff (1970) or we must know how to make principled comparisons of languages (i.e. we need a theory of linguistic contrast).

The other suggestion (putting aside the confusion as to whether changes in performance may cause changes of the abstract surface structure) has been invalidated by what Bach had reported on Peters's and Ritchie's findings (cf. above). One may also add that such hopes are vain because:

a. such notions as language or language stage are not given a priori but are defined by generative grammars; so that for diachronic linguistics PIE — West Germanic — Proto-OE — OE — ME — Modern English may be one language or a set of languages each; and because

b. the set of base rules is not defined with respect to time.

2.2. In section 2.1. (§§ 2.1.1 - 2.1.7) we have assumed a situation in which a theory of change is to supplement a linguistic theory. Counterfactually, we have propounded that the hypotheses of the linguistic theory at hand (i.e. GG) are operationally well-founded, i.e. generally approved of by competent opinion. Furthermore, we stipulated that the theory of change be in no position (except for § 2.1.5) to bear on the modification of the hypotheses in question. With the exception of P. Kiparsky (1968, 1971; cf. § 2.2.2 below) this seemed to have been the position of the originators of the generative theory of change (i.e. of Halle 1962, Klima 1964, Closs 1965 and King 1969). Consequently, we tried to draw the reader's attention to a set of hypotheses (§§ 2.1.1 - 2.1.7) which would be crucial if the theory of change is thus restricted.

Actually the situation has never been so straightforward. Never was the GG theory of language brought to a standstill, nor were all of its hypotheses operationally well-founded. Thus, never could the proponents of the theory of change be sure of the full import of their proposals within the general theory. This section, therefore, will be devoted to adjusting the picture to reality¹⁶. In other words, in sections 2.2.1 - 2.2.2 we shall try to show how a particular linguist or a group of linguists arrived at particular solutions, how they see their assumptions within GG, and to what logical consequences each particular approach may lead.

¹⁶ If the reader has inadvertently taken section 2.1. to be a downright criticism of the theory, the present section may be viewed by him as its defense.

2.2.1. Given the restrictions mentioned above (which were accepted by Halle, Klima, Closs and King) as well as those coming from the general philosophical outlook of the proponents of the theory (viz. mentalism and anti-conventionalism are of particular interest here) one may reconstruct two methodological positions (cf. § 2.0) of the proponents of the theory of change.

2.2.1.1. Group A (Halle 1962; Chomsky and Halle 1968; Postal 1960; to some extent Klima 1964) would be interested in the competence of a contemporary speaker-hearer of English; in other words, in what is known as a synchronic description of the language. It is a trivial fact that we speak the way we do because "our ancestors spoke in some definite and not other way and some of their language thinking capacity has been bestowed upon us as the so called language tradition" (J. Baudouin de Courtenay 1921: 10). Furthermore, some of the synchronic alternants, notably phonetic/phonological ones, seem to be evidences that speakers have preserved reflexes of the processes which were operative in grammars characteristic of their ancestors centuries ago¹⁷. It is possible that there is a number of rules present in synchronic grammars which were introduced into the language by consecutive generations of speakers and thus given the simplicity metric and the ordering hypothesis one has an additional justification for the order of rules postulated in synchronic grammars (and/or for their shape — cf. 2.2.2.). But at the same time one must allow for the fact that "in synchronic grammars one finds numerous rules that cannot be traced directly to any sound change" because of mutations or restructuring (Chomsky and Halle 1968: 251). It follows logically from this approach that even if there is some external evidence that a given set of rules must have looked different in grammars of our ancestors (as noticed, for instance, by P. Wolfe with respect to the Great Vowel Shift in her 1971 Linguistic Institute lecture, or by W. Winter 1971) one is not forced to modify the underlying representations for respective formatives in synchronic grammars unless they contradict the postulated simplicity measure and plausibility condition (cf. Chomsky and Halle 1968 § 1.3: 256 - 259 for a similar argument).

This position which may be termed synchronic GG "on-historical-principles" seems to be plausible for synchronic phonetic/phonological investigations. That it may lead to a theory of change, however, is most doubtful. As noted earlier, the notion of an ideal speaker-listener in a completely homogenous speech community who adds rules to his grammar has the savour of a paradox. And to get out of it one is either to give up the aims of one's field of study (cf. Chomsky 1968: 24, 81 "Linguistics, so characterized, is simply the subfield of

¹⁷ "In our discussion of the history of the English vowel system, we shall encounter changes such as the trisyllabic laxing rule ..., which has been carried over intact from the eleventh century into the contemporary language..." (Chomsky and Halle 1968: 251).

psychology that deals with these aspects of mind" ... "for language, after all, has no existence apart from its mental representation"), or to become interested in what linguists do to arrive at their theories¹⁸ (i.e. in discovery procedures) as evident in King's (1969: 66) argument:

"Note that 'addition of a rule' does not mean that the adult looks around for a rule, finds one, and tacks it on to the end of his grammar in the way that a computer programmer might add to a previously written program an instruction to carry out an additional operation¹⁹. The statement 'Changes in adult grammars are typically limited to... addition of at most a few rules' is shorthand for the more complicated formulation: 'Of the few ways a speaker's competence in his language changes once he has reached linguistic adulthood, one of the more common is MOST SIMPLY ACCOUNTED FOR IN THE LINGUIST'S MODEL OF THIS COMPETENCE as the addition of one or at most a few rules to the set of rules comprising a given component of the grammar. This rule acts on the previously produced output of the grammar and may modify it' (??? M. N.). Such a statement is neutral with respect to the internal makeup of the speaker's competence — the mass of brain cells, nerves, and so on, that account for speech in neural and physiological senses"²⁰.

A further weakness of this approach, if it is to be seriously regarded as a source of the theory of change, is that it often turns on the metaphorical use of the term "language". Thus Halle (1962: 342, 347) calls "Pig-Latin" language alongside proto-Indo-European and Germanic while it seems rather obvious that the two latter should not be termed so even at the presystemic stage of argumentation (let alone as outputs of PIE grammars; cf. Wierzbicka 1965: 80 - 82; King 1969: 176) while the former is "language" only in the technical generative sense of the term i.e., may be generated by a system of rules but so may any arbitrary recursively enumerable set.

On the other hand this approach offers to synchronic grammars a solution to the problem of alternating forms i.e. identical underlying forms for alternating surface realizations particularly at the phonetic-phonological level, but

¹⁸ An unhappy alternative given the aversion of the GG theorists to anything concerning the ways linguists arrive at the theory (i.e. analytic procedures; cf. Chomsky 1964: 7).

¹⁹ It might be instructive at this point to quote Postal's analogy between a language change and a change in computer programme (cf. Postal 1968: 270 - 273):

"...Assume now that that part of the computer program consisting of the four ordered instructions is changed by the addition of a new instruction ... Thus the new set of instructions consists of old ... followed by (the new ones).

I maintain that the above description is analogous in significant respect to the rule addition type of the so-called sound change".

It is true that Postal does not mention the programmer, but neither does he mention the linguist which makes his reader ponder over the unknown power which added the instructions and ordered them.

²⁰ It is rather obvious that we are neither able to say what has happened in terms of cognitive psychology. Thus, if a theory of change were to bear upon general linguistic theory the latter could not be in the least related to psycholinguistics.

also, as evident from works of Klima (1964) and Bever and Langendoen (1970), at the syntactic level (stylistic alternations of surface relative pronouns). Thus if the synchronic-on-historical-principles type of analysis is beneficial in that it demands that linguists working within the GG framework be acquainted with the facts from the history of a particular language it offers no plausible hypotheses which being in accord with the previously established general assumptions of GG theory would offer solutions to problems mentioned in 1.0.

2.2.1.2. Group B linguists, (notably Closs 1965, 1972; McLaughlin 1970; Jacobson 1970; Nagucka 1968; Wagner 1969; and possibly Klima 1964) who are interested primarily in language history, were ready to explicitly or implicitly accept what shall be called here the synchronic-contrastive approach and thus to follow Bloomfield's (1933) suggestion that: "all historical study of language is based upon the comparison of two or more sets of descriptive data" with the all-important reinterpretation of "sets of descriptive data" as "grammars" (i.e. "ordered sets of statements that describe the data" — to use Halle's words).

This approach may be characterized as follows: Given the apparatus and general theoretical assumptions of GG plus a set of data and admitting of such inferences from the data as result from the intuition of the historical linguist, one posits a hypothetical competence for which a given grammar is to account (in case of a one-period grammar, e.g. Nagucka's Chaucer's grammar) or a set of competences whose descriptions will constitute grammars of consecutive language-stages which are then to be contrasted either informally (as in Closs 1965, 1972) or with the help of extension rules (as in Klima 1964 and Jacobson 1970) which would correlate the grammars pairwise.

In the case of one-period grammars the aims are usually set judiciously low, as in Nagucka (1968: 11):

"This study does not claim to be a complete grammar (...). It is meant to be an empirical test of the theory rather than an attempt to look for new aspects of it". (or in the Preface:) "The purpose of the monograph is to show the practical application of the transformational theory in historical linguistic analysis (...)"

The multi-stage approach sets its aims much higher. We have already mentioned Closs's attempts at formulating a theory of change to which she wants to offer an elaboration of such concepts as innovations and mutations (= King's "additions" and "restructurings"). But at the same time the approach suffers from two types of serious defects. The more important one results from the fact that we have not the slightest idea what to contrast with what²¹, the condi-

²¹ At this stage of analysis historical study crucially depends on the results obtained in contrastive language study (cf. Nickel 1971).

The empirical issue is what additional restrictions hold if two dead hypothetical "languages" are contrasted and what if one contrasts a living language with a dead one.

tions under which our contrasts are valid, nor when the comparisons become uninteresting. The other defect concerns substantial issues. Namely, do we have any answer to the question: "Is there any particular place in our grammar (if this is an OE grammar, for instance) which would account for its 'Englishness' independently of the intuition of the investigator?"

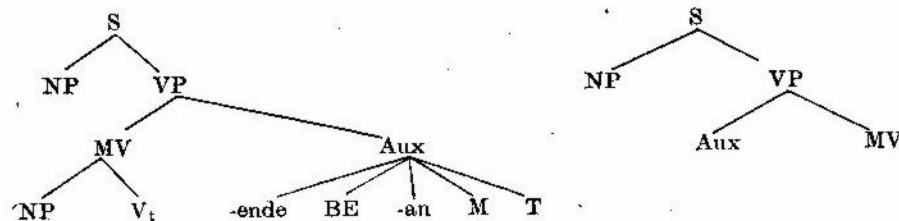
Closs seems to suggest (1965) that the basic structure of OE sentences is nearly identical with that of Mod. E (cf. 2.1.7). As a result her OE grammar is even observationally inadequate for, as noted by K. H. Wagner (1969: 46 - 47) it would generate such deviant sentences as:²²

* He sceal sellan ðæm cyninge ðæt land. (instead of the correct one: He sceal ðæm cyninge ðæt land sellan).

But then, if Wagner is right in his claim that "the regularities of OE syntax are much closer to those of German than those of Mod. E." should we not be forced to reject in historical studies at least the notions of innovations and mutations and restrict our analysis to one-period grammars; the definition of "Englishness" (or Germanness) to be looked for in some meta-theoretical contrastive device, which seems to be independently motivated by the fact that native speakers do know whether someone speaks English or not. Whether such a device should incorporate extralinguistic notions is an empirical issue. It would be unwise to preclude at this point any answer

But it seems to be rather well established by the proponents of the theory of contrast that all theoretically interesting contrasts occur at levels other than the transformational subcomponent (as wrongly assumed in Closs 1965). This has been suggested by T. P. Krzeszowski (personal communication; cf. also: Krzeszowski 1973). His CGD (contrastive generative device) could account much better for the contrasts presented informally in Closs 1965. The necessity of such a device for synchronic studies seems obvious.

²² Additionally, the contrastive multi-stage approach, especially that of Closs 1965, in its struggle to posit underlying structures which would be easy to compare (and often forced by an overall stage-independent simplicity criterion to arrive at particular solutions) may be accused of admitting of "revolutionary" changes at some period of language development. And this in turn contradicts the mutual intelligibility constraint as posited by Halle (1962). Thus, compare, for example, the underlying structures for OE and ME as in Closs 1965:



Is it possible that some element (in this case: do and gin) may suddenly appear out of nowhere to completely remake the base? (See also: Goldman 1970).

to the question. Our aim here was only to show how weak the contrastive multi-stage position is as a basis for a theory of change²³.

Furthermore, group B linguists are generally not clear as to whether they intend to apply the theory (as Nagucka explicitly states) or to explicate it. Hence Closs's demand that a theory of a dead language had explanatory power (cf. 2.1.2). Therefore both the one-period, and multi-stage approaches have obvious troubles when it comes to "conclusions", i.e. contributions a particular investigation claims to offer to the field of study. Even the weakest claim that GG as a theory of language may successfully be applied to historical analysis is empty given the excessive power of the theory (cf. Bach 1971). If Bach is right that: "The mathematical study of transformational grammars has led to the result that they are too powerful to qualify as theories of natural language. (...) they fail to distinguish natural languages from arbitrary recursively enumerable sets", so it is of very limited interest that such grammars may generate a set of sentences referred to as Chaucer's English or Alfredian prose. And the stronger the claim, the more untenable it is. Thus, what may it mean that:

"the abstract, hypothetical representation of the language in question, i.e. deep structures generated by the base rules, hardly differs from that postulated for Modern English". (Nagucka 1968: 112)

or that:

"in the history of English the difference between e.g., Middle English and Modern English is minimal in the abstract deep structure and quite considerable in surface structure". (Fisiak 1971: 112)

The status of statements like the two quoted above in a theory which stipulates that all (even totally unrelated) languages are similar in their deep structures is at best tautological.

Conclusions like these are usually modelled upon those arrived at by linguists of group A (and notably Chomsky²⁴) which must be viewed, however, as a

²³ This has not gone unheeded. H. Andersen, for example, in his 1972 *Language* article criticizes GG theorists for confusing what he terms "diachronic correspondence" with phonetic change:

"Phonetic changes are observable in any speech community at any time. Diachronic correspondences are not; they are the linguist's way of defining relations between equivalent units at different times. ... A diachronic correspondence can be used to summarize a phonetic change.... Since Kiparsky's classification of correspondences between different stages of a phonological system is a typology of diachronic correspondences, it has no value as a statement about the possible types of phonetic change. A priori, it would seem hazardous to claim that the class of possible diachronic correspondences equals the class of possible phonetic changes... But this speculation is pointless, for Kiparsky is not dealing with phonetic change at all ... (12 - 13).

²⁴ Cf. Chomsky (1966: 90) "the underlying forms are extremely resistant to historical change" and Chomsky (1968: 69) "dialects that are superficially quite remote ...

totally illegal procedure on two counts. First, it makes no difference for Chomsky if he arrives at his universalistic hypothesis on the basis of some intuition he gets from interlanguage or intralanguage informal comparisons. What is important to him and what he tries to successfully justify is that there is "a vast central core of rules and processes common to many languages" (1968: 69). Second, Chomsky studies synchronic English grammar, and states synchronic Modern English rules. If it is simply a fact that the underlying processes and representation which he is forced to posit on the basis of synchronic facts (plus some of the assumptions of a theoretical nature: viz. evaluation measure) are not different from those of earlier periods, the only thing it may suggest to linguists interested in e.g., Middle English is that it is possible for a Middle English grammar to reflect some Old English or West Germanic rules and representations, but no conclusions as to the shape of Mod. English rules are allowed. What the value of those former conclusions is, and whether this will lead to an infinitive regress position²⁵ remains to be seen if enough work on one-period hypothetical grammars of "dead" languages is done.

2.2.2. As evident from the bulk of his writings (cf. especially, Kiparsky 1968, 1970, 1971) Paul Kiparsky has been aware of most of the restrictions upon the theory of change mentioned above (2.2., 2.2.1). Consequently, he has offered some proposals which make his theory of change radically different from all the previously mentioned approaches. He has rejected the assumption (as formulated in 1.0 and 2.2. above) that the theory of change should be appended to a general linguistic theory without its being able to weigh upon modifications the linguistic theory is to undergo.

"On this view (...) the facts of linguistic change assume a new relevance as empirical evidence on the nature of language. We must be prepared to allow them to bear on even purely synchronic questions and, for example to let the fact that some phonological change is explainable by one linguistic theory but not by another carry weight in the choice between these two theories. The application of linguistic change to linguistic theory now becomes at least as important as the converse process"²⁶. (Kiparsky 1968: 188)

share a vast central core of common rules and processes and differ very slightly in underlying structures, which seem to remain invariant through long historical eras."

²⁵ Or rather, whether linguists will be forced to accept the PIE representation as the ultimate representation beyond which no historical work is possible, but also whose underlying representation is impossible. Such solution would be close to that offered by Birnbaum (1970: 26) who postulates typological deep structures.

²⁶ Compare this with King (1969: 102):

"Let us begin by asking the question: Does historical evidence decide which of two synchronic grammars is higher valued? The answer is a flat No".

To remain consistent with his aims Kiparsky (1968: 188) is even ready to sacrifice the goals of linguistic theory as set by Chomsky:

"We could not draw this conclusion if we regarded a grammar simply as a theory of the sentences of a language, and a linguistic theory as a theory of grammars. For this position would entail that linguistic change is no concern of linguistic theory, although it might of course be a pleasant bonus if linguistic theory could be usefully applied to questions of linguistic change. But it would not cause us to demand of a linguistic theory that it must (in conjunction with a theory of linguistic change) provide an explanation of the linguistic regularities of diachrony..."²⁷

What kind of inferences can be made from the facts of linguistic change that would bear on the nature of grammar? It could offer justification for assuming the psychological reality of generative grammars. The justification would be much more convincing and straightforward than the one offered by psycholinguistic experiments which, given the existing experimental techniques, are unable to clearly separate the contributions of competence from the facts of performance (Kiparsky 1968: 174). If historical change affects a given class of elements in a uniform way or if blocks of rules collapsed by braces undergo a systematic change this may serve as a powerful argument for including the class into our grammar and for the fact that our notation is not a spurious one²⁸. Thus: "In general, any sound change corresponds to a possible phonological rule in grammar, although the converse is not true (...)" (Kiparsky 1970: 308). The notion of change would also decide upon groupings of rules which represent natural processes (those which can jointly produce a change). Next, it should be remembered that the notion of change has been crucial in all generative arguments against the postulated level of autonomous phonology (Postal 1968; Kiparsky 1968: 183 - 185) and as suggested by Kiparsky it may be equally relevant to investigations of linguistic universals (Kiparsky 1968: 185 - 189). Kiparsky admits that "the description of a particular langu-

²⁷ Kiparsky's remarks in the two last sentences are in total agreement with the tenor of the present article. The sentence which immediately follows the quoted passage, however, and the reference to the notion "competence" in particular, are characteristic of the vacillations discussed in 2.1.2. in addition to its being incomprehensible unless it predicts a new linguistics; the sentence is:

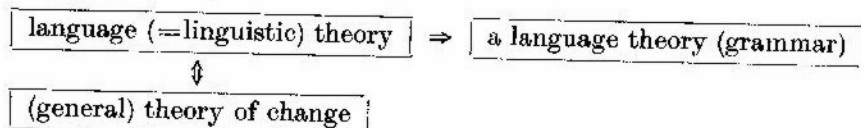
"It is a very different matter if we regard a grammar as a theory of linguistic competence, and the field of linguistics as the study of universal grammar". (1968: 188)

There has so far been proposed no "theory of competence" which would not at the same time be "a theory of sentences" (of their structure and/or meaning which may be identical); nor does a "linguistic study of universal grammar" which would not be "a theory of grammars" seem possible.

²⁸ "One answer, then, to the question concerning the empirical basis for the notational conventions of linguistic theory is that these conventions are an essential part of any attempt to characterize what is a possible linguistic change and what is not a possible linguistic change".

age is not to be based on the history of that language but on its synchronic structure" and yet "the general properties of linguistic change are part of the evidence on which our theory of language can be based, and the description of a particular language is carried out with reference to a general theory of language. Hence historical facts can become relevant to description via the theories of grammar and language change" (Kiparsky 1970: 314 - 315).

2.2.2.1 One may be a bit confused by Kiparsky's use of some terms. In the passage quoted above, in addition to such well-known notion as "a general theory of language" he uses such notions as "the general properties of linguistic change" (= "a theory of language change") and "the description of a particular language". The phrase "the theory of grammar" (or theories) is probably equivalent to what is known as "the theory of a language" (i.e. grammar), (though - cf. Lakoff 1965 - it may be equivalent to "linguistic theory", a possibility we disregard in what follows). But what is the status of the notion "the description of a particular language" then? All this hair-splitting would be unnecessary were we able to answer the question (crucial to Kiparsky as evident from his 1968 article) concerning the place which the theory of change would occupy in linguistic considerations. Is the relation as presented in this diagram?



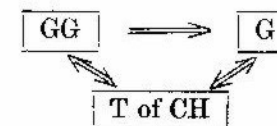
If the diagram is a true representation of Kiparsky's position (as seems to be the case in the 1970 article), then the subject matter of such a theory of change would be dangerously close to what Chomsky terms "analytic procedures" (that the possibility is not a figment of the author's imagination may be seen from Labov 1971) since it should not be excluded a priori that some inventive generative grammarian is as likely to arrive at an identical language theory without any historical investigation (as did for example Chomsky 1957 and Halle 1959). That this is not an invalid suggestion for Kiparsky comes from his preoccupation (in the 1968 article) with "what a practicing linguist knows", a concept totally irrelevant for Chomsky (cf. Chomsky, in Hook 1969) and seldom dealt with by other GG theorists. Here is Kiparsky's position:

"The processes of normal language learning being unconscious, we have absolutely no ideas about the form of grammars, though we have clear ideas about the form of sentences which grammars account for. It is true that the practicing linguist soon acquires ideas about the form of grammars and such concepts as generality. But these ideas are somehow the result of his work on languages, and we would like to know what the ideas are based on". (Chomsky 1968: 172)

But if the notion of change is only to replace the intuition of the linguist qua native speaker, the theory of change has no independent justification. Fur-

thermore it is not change that Kiparsky means (process of differentiation of two definite states $L-L_1$ in time) but variation (process of differentiation of two indefinite states L -non- L in social and geographical space and possibly in time) and this may lead not to a generative theory of change but to a theory of variation.

Anything that was said above points only to Kiparsky's vacillation as to the place of the theory of change within the GG framework and is of course completely non-sequitur with respect to his claim as to the justification of grammars with the help of the theory of change. But in this case our diagram would have to be modified:



The problem of justification is an empirical issue, but since the justification of the notion of "the possible phonological rule" (or "the possible rule" in general) will anyway have to be done independently of characterizing the possible sound change (or "the possible change" in general), given that changes form a proper subset of rules, one may safely assume that even without the theory of change rules of grammars will be justified providing we analyse a sufficient number of languages (both natural and hypothetical including "dead" ones) and restrict the power of the theory.

And this seems to be a position consistent with that of Kiparsky in his recent article (Kiparsky 1971) where he seems to join group A linguists (cf. 2.2.1.1 above):

"(...) the present state of linguistics is such that the synchronic theory is often rather indeterminate in exactly the respects that would be most relevant for historical linguistics. For this reason much progress in historical linguistics depends on sharpening the synchronic theory so that it will provide basis for diachronic explanation. It is interesting that most work that has so far been carried out along these lines points to one general conclusion. This is that the function of the 'evaluation measure' in linguistic theory is carried out by a series of *substantive* conditions in addition to (...) the *formal* condition of simplicity". (Kiparsky 1971: 578 - 579)

The above quotation may serve as a not too optimistic conclusion as to the status of a theory of change in the present day generative linguistic theory of language.

2.3. *Genetic relationship modifications.* Kiparsky's approach is often similar to those of Collinge and Birnbaum. Thus, Collinge also comes to the conclusion that GG is vague at just the points which "a historian might use". His book (Collinge 1970) is a mine of examples from classical languages and when (in chapter 8) he comes to discussing particular issues as suggested in the "Closs-Halle paradigm" (Collinge takes account of the GG theory as it was developed

up to 1967) each objection is richly documented. The objections may be summed up as follows:

a. "diachronic simplicity and T-simplicity are very different animals" (Collinge 1967: 163)

"... 'existing grammar' is no sound basis for calculations of simplicity and/or obtrusiveness when it is that postulated for the proto-language, precisely because that postulation itself rests on no separate foundation". (Collinge 1967: 165)

b. the parent-child model is highly implausible (Collinge 1967: 167 - 168)

c. like Winter (1971), Collinge finds that "Halle cites cases where neither T-grammar nor traditional history has solved the problem, but no case where T-grammar has and diachronists have not" (Collinge 1967: 169) (next, he discusses in terms of Halle's binary features a case of simultaneous historical processes which may be regarded as a quasi-counterexample to the paradigm)

d. on pp. 173 - 175 Collinge questions the psychological validity of the postulated underlying structures reflecting historical surface representations.

On the other hand Collinge notices "an implicit kinship (of GG models) with historians" (Collinge 1967: 156 - 159, 176 - 182) and believes that GG is a plausible framework within which historical investigation of a dead language may be conducted (cf. the quotation on p. 14 above).

Birnbaum (1970) comes close to Kiparsky in that he proposes that historical linguistics could influence questions of general linguistic theory. On the basis of his own work on Slavic and Balcanic languages he concludes that both the modifications proposed by the generative semanticists and in Chomsky's recent ("Deep structure, surface structure, and semantic interpretation") article are unacceptable. Historical studies could profit from GG theory on the condition that the concept of a multi-layered deep structure be elaborated within the model. The three layers he introduces are: a language specific "infrastructure", the typological deep structure, and the "profound structure" of "greatest depth and absolute generality, i.e. universality encompassing all (natural) languages". It is the typological deep structure which proves most fruitful in Birnbaum's work on linguistic typology, if appropriately extended to include an abstract metalinguistic model (like the one proposed by Uspenskij) which would match such structures with the transformationally established 'infrastructures'."

Birnbaum has, furthermore, violently opposed the "directionality lack hypothesis" as expressed in Chomsky's article (1969a: 63 - 66) referred to above. He states:

"This, as it were, total cancellation of direction (in a competence model of language) seems to cast some serious doubt upon the whole concept of language as a 'creative' faculty of man (approximating Humboldt's *energoia*), expressible and formalizable

in terms of various sets of generative rules, rather than merely a structure assumed to exist so to speak independently, i.e., as something given beforehand (comparable to Humboldt's *ergon*)". (Birnbaum 1970: 65)

Next, he notices the parallelism between the method of internal reconstruction and some recent proposals in GG (i.e., what we called here the synchronic-on-historical-principles approach, cf. § 2.2.1.1). His conclusions are not unlike those presented in the present article:

"... it is important, I believe, to voice a word of warning as regards this parallelism. Although M. Halle in particular has claimed such a parallelism... to be non-accidental in character, serious reservations remain as to this non-accidental, or rather motivated, nature of it. Whatever the case, this parallelism should not be cited as a major factor to justify the specific ordering of generative phonological rules. The order of such rules must rather be determined by the criteria of economy and maximal simplicity, while still meeting the adequacy requirement of linguistic description". (Birnbaum 1970: 113)

Little needs to be added here. Although some of Birnbaum's criticism is valid, it is clear from his discussion of directionality²⁹ that he has envisioned a version of GG different from all those proposed so far. It has been said earlier that if any language theory is to be mentalistic, historical linguistics in such a theory should not bear upon any particular theoretical proposals. In light of this, other suggestions of his must be treated as empirical issues to be solved by a future theory of change if it is to be like the multistage approach (as suggested by Birnbaum 1970) in particular. The present author, however, doubts if this will be the case.

On the whole, Collinge and Birnbaum may be said to have noticed many weak points in GG models offered so far, yet their preoccupation with how historians might best "use" the model has considerably weakened their arguments. This utilitarian attitude forced them to disregard the question of how far reaching consequences some of their proposals would have for the linguistic theory in general.

3. VARIATION THEORIES

3.0. The foundations for what we term here variation theories of language change are to be sought in dialectological studies and in Weinreich's contribution to the field in particular (cf. Weinreich 1953; 1954; Weinreich, Labov

²⁹ Chomsky's article could not have presented any "cancellation of direction" since nowhere (as stated by G. Lakoff in his follow up "Generative semantics", not even in Chafe's theory as Birnbaum claims) has directionality been an issue within the theory itself. One is though free to discuss at the pre-theoretical level what is going on when a speaker "produces" a sentence, whether meaning "comes first", sound next, or structure next, or the other way round. What Birnbaum calls "ergon", seems to be the theory itself, which as a body of statements actually does remind us of a product.

and Herzog 1968). Most of the assumptions which we intend to discuss below have been presented in Weinreich et al. (1968), where the term "theory" does actually appear and which may be taken as one of the "most imaginative studies of change"³⁰.

In the words of the co-authors of the "Empirical foundations for a theory of language change" the goals of the theory are as follows:

"... we want a theory of language change to deal with nothing less than the manner in which the linguistic structure of a complex community is transformed in the course of time so that, in some sense, both the language and the community remain the same, but the language acquires a different form". (Weinreich 1968: 102)

Before we try to explicate this position by confronting it with our set of questions of § 1.0. we have to stress the uniqueness of the theory³¹ with respect to the notion of language. In this theory, language is viewed as "an object possessing orderly heterogeneity" (Weinreich 1968: 100) and, furthermore, "nativelike command of heterogeneous structures is not a matter of multidialectalism or 'mere' performance, but is part of unilingual linguistic competence" (1968: 101). A linguistic theory which would account for such "competence" will have to be aware of social and geographical facts³².

3.0.1. Variation theory which forms only a "part of a larger theoretical inquiry into linguistic evolution as a whole" claims (1968: 103) to be ready to account for changes under all possible definitions suggested in § 1.1. (def. a. - e.). Actually, it identifies variation processes with variation facts and with spacio-geographical processes of differentiation in time calling all of these "changes in progress". The remaining, "completed changes" also fall within the range of the theory.

The actuation riddle³³ (cf. § 1.1.2.) may be solved by assuming that:

"Linguistic change begins when the generalization of a particular alternation in a given subgroup of the speech community assumes direction and takes on the character of orderly differentiation." (Weinreich 1968: 187)

³⁰ As it occupies exactly 50% of the Proceedings of the 1966 University of Texas Symposium it would be more proper to refer to the work as a monograph.

The reference to this work as "the most imaginative study" comes from the Editors of the Proceedings.

³¹ The authors are well aware of the originality of the heterogeneity approach as well as of the predecessors of some of its basic insights (the Prague School, Meillet, Fries and Pike, and the American dialectologists); the uniqueness pertains to some of their attempts at formalization, and some individual solutions (functional distinctness and linguistic variable in particular).

³² "Linguistic and social factors are closely interrelated in the development of language change. Explanations which are confined to one or the other aspect, no matter how well constructed will fail to account for the rich body of regularities that can be observed in empirical studies of language behavior". (Weinreich 1968: 188)

³³ The term has actually been borrowed from this theory; in Weinreich's work, but not in the present article, it is additionally associated with the disappointment with the solutions of the riddle offered by Paul, de Saussure, Bloomfield, GG, and others.

or, in other words:

"... when one of the many features characteristic of speech variation spreads throughout a specific subgroup of the speech community. This linguistic feature then assumes a certain social significance — symbolizing the social values associated with that group. Because the linguistic change is embedded in the linguistic structure, it is gradually generalized to other elements of the system... Eventually, the completion of the change and the shift of the variable to the status of a constant is accompanied by the loss of whatever social significance the feature possessed". (Weinreich 1968: 186 - 187)

This lengthy quotation will facilitate our presentation of the position of the theory with respect to other issues. As to the general problem, the theory assumes that the restrictions on the set of possible changes are built in the heterogeneous language system as functions of extralinguistic and/or internal linguistic variables, so that individual changes are restricted as much by purely linguistic factors (often of a "purely synchronic character" as e.g. the general constraint that "mergers expand at the expense of distinctions" (Weinreich 1968: 134 - 137, 154) as by social factors; since "the level of social awareness is a major property of linguistic change" (Weinreich 1968: 178 - 183). The theory would not, however, preclude any other type of constraints (cf. § 1.2.1.) which could be derived from "a set of validated universals" of either Greenberg's (1963) quantitative or Jakobson's and Chomsky's "implicational" types³⁴ provided the latter be appropriately limited so as not to be "so broad that we are unlikely to find cases of changing languages which are approaching a possible violation" (139). Then, restrictions may be sought in "long-term trends" on the assumption that we "can formulate a better *system* of trends", i.e. a "schedule" of possible trends characterized independently of particular languages or groups of languages (cf. Greenberg 1963: 140 - 141). Generally, the problem of constraints should be viewed within a broad context which could encompass three closely interrelated issues, namely transition (i.e. intervening stages), embedding (linguistic and extralinguistic concomitants), and evaluation (linguistic and extralinguistic effects of changes).

In strong opposition to King, Halle, Bloomfield and a number of other scholars, the theory holds that the distinction between the origin of a change and its propagation is untenable, since it leads to a counterfactual claim "that changes in progress are unobservable" (Greenberg 1963: 129; cf. 124). Instead, the old dichotomy of historical linguistics, namely the opposition of the Stammbaum and wave theories of linguistic differentiation is revived (cf. 1963: 152). It is claimed (e.g. Labov 1971: 426) that wave effects account better for variability in sociolinguistic space.

³⁴ "We recognize front — back symmetry as one of the near universal conditions of linguistic change". (Greenberg 1963: 175)

On the assumption that "continuous variation exists within each dialect as a structural element, ... and that the steady movement of tokens from one categorial class to another is part of the underlying structure" (Labov 1971: 426), the problem of gradual vs. abrupt changes does not arise in this theory, or, in other words, that for the theory in question the problem is ill-formed.

A superficially similar situation arises with respect to our historical problem (cf. § 1.3.); that is, any attempt at answering the question: "How to account for concrete changes that have taken place?" will be a failure, and thus the question is ill-formed like the one discussed above. We said "superficially" since the ability to account for the completed change is openly claimed by the proponents of the theory. Furthermore, an inventive variation theorist could always answer the question by positing a stage at which the "completed change" of a given form was only a co-variation of the innovating and archaic forms. This may pose some problems for the theory to which we return in § 3.1., and the quotation below is a proof that the "inventive theorist" was not a straw opponent of ours:

"We can today easily assent to Saussure's argument that Old High German *gesti* 'guests' did not coexist in the consciousness of any speaker with the Modern German counterpart, *Gäste*, with the result that these items have therefore never been linguistically opposed. What is missing in his conception, however, is the possibility of a moment in time when a more archaic *gesti* and a more innovating variant *gesti*, did coexist in the minds of some very real speakers of the language". (Weinreich 1968: 122)

Thus, we arrived at the methodological postulates of the theory as well as some procedural problems posited thereof (cf. Weinreich 1968: 159 - 188). The latter concern the techniques for collecting and quantifying data and some tests of the social awareness of speakers presented with a set of data. The former, however, include some elaboration of the theoretical apparatus:

"The heterogeneous system is ... viewed as a set of subsystems which alternate according to one set of co-occurring rules, while within each of these subsystems we may find individual variables which co-vary but do not strictly co-occur" (1968: 170). Such a set of subsystems (or "a diasystem" in Weinreich 1954) would comprise usually two or three subsystems which the theory stipulates to be functionally distinct (1968: 162). Each system, or "code", "is conceived as a complex of interrelated rules or categories which cannot be mixed randomly with the rules or categories of another code or system" (1968: 166 - 167). The linguistic variable is "a variable element within the system controlled by a single rule". "Such a variable is a heuristic concept, useful in making observations and gathering data" (Labov 1971: 464). All rules may be considered to be of the form³⁵:

³⁵ For an illustration see a rule which accounts for the variability of r-pronunciation in New York City (Labov 1971: 170).

$$A \rightarrow g[B]/X \left[\begin{array}{c} \bar{Z} \\ \bar{Z} \end{array} \right] Y$$

$$g[B]=f(C, D, E)$$

"where B is one or more features of A, and C, D, and E are linguistic or extralinguistic variables. The expression g[B] is the linguistic variable defined by the rule, usually denoted (B)" (Labov 1971: 170).

"Each of these variables will ultimately be defined by functions of independent extralinguistic or internal linguistic variables..." (1971: 170) and all variables will usually be interdependent.

Thus, it is evident that the apparatus of the theory is comparable with that of GG (actually it incorporates the latter) though considerably more elaborate, as it should be to handle sociolinguistic facts. What differs the two approaches most from the purely formal point of view is the weight they attach to methodological (particularly procedural) problems. Compare, for example, Chomsky's statement opening his discussion of "Goals of linguistic theory" with that of Labov (1971):

"I will not be concerned with systems of terminology or methods of investigation (analytic procedures)". (Chomsky 1964: 7)

"Through the convergence of several techniques, we can hope to make further progress towards the common goal of finding right answers to hard questions about language. ... A permanent concern with methodology means living with the deep suspicion that we have made a mistake at some crucial point in the investigation". (Labov 1971: 480 - 481)

3.0.1. *Variation theories versus GG.* Naturally, in addition to some formal differences there is a number of substantive issues which the proponents of variation theories raise against GG theories of language and language change. Some of these (cf. Weinreich et al., 1968: 125) result directly from the heterogeneity assumption, and we shall postpone their treatment until § 3.1. Others have already been noticed by GG grammarians and need not be mentioned here (e.g. the need to limit the power of the theory — Weinreich et al. 1968: 139; cf. Bach 1971; Chomsky 1965, 1968 — by postulating even tighter substantive and formal constraints).

There are still, however, some points which seem to bear on the proposed GG theory of change in a rather direct way. Three of them which have been brought up by Weinreich (1968: 144 - 150) shall be mentioned here³⁶.

$$/r/ \rightarrow g[r]/ \left\{ \begin{array}{l} K \\ \# \end{array} \right.$$

$$g[r]=f(\text{Style, Class, Age})$$

³⁶ Since the distinction between Weinreich's first and second issue has the relation of a principle and an illustration of the principle we shall discuss them together; we depart slightly here from the way Weinreich develops his arguments since we do not want to introduce the notion of language heterogeneity into this section. A similar line of reason-

Given that children do restructure their grammars many times³⁷ and the often quoted evidence (e.g. Chomsky 1965: 200, note 14) as to the basically heterogeneous, often random character of the primary input data which are to bear upon the form of the child's output grammar, the parent-to-child model of linguistic change may at best be regarded counterintuitive. Weinreich goes even further (1968: 146) accusing Halle that his model implies "that change is completed within a generation". Then he raises an empirical issue pertaining to the model (1968: 145):

"Yet there is a mounting body of evidence that the language of each child is continually being restructured during his preadolescent years on the model of his peer group ... (and not) upon the data provided by their parents' speech".

This fact would not have been dangerous in itself, for input data are in any case degenerate and finite in scope, and one may replace the notion of parents with that of a peer group, had not Halle aimed at the psychological reality of the model. Since Halle's model, however, and most of the proponents of GG, aim at a realistic description of competence³⁸ Halle distorts historical evidence by, for instance, introducing a time interval between grammars which seemed to have coexisted in Shakespearean London (1962: 147-148). Had he, on the other hand, stated that his aim was only to contrast two synchronic grammars, from some statement of diachronic correspondences (to use H. Andersen's term) nothing could have been inferred to justify the innovation

ing is that of Andersen (1972). We agree with both Weinreich and Andersen in their conclusions and go beyond their observations when we point to the lack of any criteria as to when "diachronic correspondences" are theoretically interesting or independently justified (in the case of Andersen's criticism), or to reasons for which the parent-child model was accepted (in the case of Weinreich).

³⁷ As evident from psycholinguistic analyses; Weinreich quotes: U. Bellugi's 1967 Harvard University Dissertation, and Miller's and Ervin's 1964 article. It should be made clear, however, that Weinreich does not criticize "the role of children's rule forming behavior in language change" but only the parent-to-child hypothesis in the form it has in Halle 1962; Closs 1965; and King 1969: 84 - 87.

³⁸ A rather vexing problem might be mentioned at this point. We are aware that Bar-Hillel, for example, ascribes to Chomsky and other GG theorists the conventionalistic position (in e.g. his review of Lyons 1968; cf. Bar-Hillel 1970: 369f.) which we would not grant the theory and for the lack of which we criticise the GG theory of change. In case Bar-Hillel is right what remains of some of our criticism in this section and in some subsections of §2.1. is the criticism of the theory for the lack of clarity. And that is very little. But we would like to claim that like Lyons (1968: 154) and Bohnert (1969) we think that Chomsky is a methodological realist, in the sense that he would not agree to calling his "competence" a heuristic fiction at both the pretheoretical and theoretical levels. We base our claim on the fact that Chomsky equates model with theory and refuses to clearly state the idealizational distortions accepted within the model. On his vacillation in *Aspects* (the only place where "idealization" appears cf. Bohnert 1969).

— mutation theory of change. Thus the theory of change fails when faced with historical evidence.

Weinreich's third point concerns the apparatus of both theories (Halle's distinctive feature analysis in particular) and may be stated here as follows: If empirical evidence is to decide on which of the apparatus is better suited to describe what happens when language changes, then:

"This evidence shows that the mechanism of change is not a sudden substitution or addition of higher-level rules, but rather the application of a continuous function to phonological space at a level where continuous values are possible". (Weinreich et al., 1968: 149)

In practice, however, the import of this criticism is lessened by the fact that Weinreich himself uses distinctive features in the description of the New York City vowel system (1968: 172 - 174) though he stresses again that:

"...the distinctive feature apparatus must be replaced by a homogeneous set of dimensions which define locations in phonological space; however, we cannot outline the quantitative basis for such dimensions here and we will therefore retain the binary".

The reader would have a distorted picture of the situation if he takes this criticism as exhaustive or even typical for the controversies between the proponents of the two theories. We have limited ourselves to issues immediately pertaining to the theory of change which should not be abstracted (as it is in this section) from linguistic theory in general, against which most attacks were being directed. The most important issue (i.e. homogeneity — heterogeneity controversy) will be discussed below; for others the reader is directed to Labov (1971) and Bailey (1971)³⁹. Some of the critical remarks found in these works are well founded (particularly those directed against the vagueness of GG on the points relevant to dialectologists and historical linguists). Large part of the criticism, however, seems irrelevant. It has often been invited by the carelessness and frequent lack of understanding of some basic GG assumptions⁴⁰ on the part of those historians and dialectologists who claim to be proponents of GG.

3.0.1.1. One very important issue has been raised by Katičić (1970) whose approach is very close to variation theory of change. His monograph is an attempt at formalization of such notions as linguistic relationship and linguistic

³⁹ We shall limit our quotations from Bailey (1971) to a bare minimum for two reasons; first, because the work, though of considerable importance, has not, to my knowledge, been published yet, and the copy at my disposal is "a very preliminary version" (in its author's words); second, because Bailey, it seems, has taken on the burden of playing the role of the infant terrible of the variation theory comparable to Postal (1968) in GG, quotations from whom were for the same reason scarce.

⁴⁰ Or rather implications of such assumptions; glimpses of those might be seen in our discussion in sections 2.1.1. - 2.1.7.

(historical) comparison; or rather of the notion "linguistic variety" (i.e. "a set of all languages that can be perceived as different"). Similarly to Weinreich, he proposes to treat "the substitution of languages (as) nothing but a particular mapping of the linguistic variety set on time, space and society" (Katičić 1970: 31) but unlike the variation theory (and like GG) he operates with discrete units and rules (instead of functions and variables) and admits the priority of a general theory of language over any theory of language change.

Out of the many valuable suggestions contained in the work we would like to quote one that a generative theory of change might accept. The problem is:

"... whether language change can be defined by linguistic concepts only. The answer to this question is in the negative. We need extralinguistic concepts in order to define language change and genetical relationship in linguistics. Internal change can be RECOGNIZED by restrictions imposed on linguistic diversity but it cannot be said TO BE itself such a restriction of the diversity of languages. ... there may be languages, related genetically, which cannot be recognized as such because the restrictions on their diversity have faded away in the course of change with many replacements so that the observable restrictions are no more conclusive enough". (Katičić 1970: 41)

Since GG theory of language offers no device for dealing with problems of this type, it is up to the theory of change to find a device (or to hand the task over to thus justified variation theory) which could account for just these facts.

3.1. *Heterogeneity and homogeneity hypotheses.* Baudouin de Courtenay has characterized the subject matter of linguistics in the following way:

"All that is constant in language, all the incessantly alive, belongs totally to the domain of the psychic world. And yet, the individual-mental reality of language, i.e. language thinking capacity, is possible only on the condition that there exist other human beings similarly language-endowed and ready to interact; in other words, it is possible only in community, in human collectivity. This individually existing psycho-linguistic world could similarly come into existence only in community, and only owing to mutual influence of individuals upon one another. Finally, it is quite obvious, that human language exists to the extent that it serves in social intercourse. All this points to the fact that a psychological characterization of any language must properly be taken as a *psychological and sociological characterization*, that is, it must demonstrate what currents are formed in the souls of individuals belonging to a given language community under the influence of the inter-individual intercourse". (de Courtenay 1915: 157, translation mine; italics as in original)

We would like to claim here that this position is virtually undistinguishable from what Chomsky (1965: 3) terms "the position of the founders of modern general linguistics", and, furthermore, that it is basically the only legitimate

position for a general linguistic theory no matter if one terms it homo- or heterogeneity position (and the labelling in the case of de Courtenay's statement would be much more difficult than in that of Chomsky's). We intend to show that this position does not lead to any paradoxes concerning either living languages or dead ones if the latter are inferentially believed to be languages at all; as is not the case with respect to the PIE "language". Finally, we would like to make clear that the theory of variation is as implausible a position to base any general theory of language upon, as any theory of change (GG theory included) would be.

We do not, however, by any means want to question the basically accurate observations that speakers are aware of a number of sociolinguistic facts, that they can switch styles or be equally proficient in two or more ethnic languages or dialects, or that language-norm awareness ("school grammar" or RP) does exert an enormous influence upon particular languages. What we do not possess is a theory of language⁴¹ which would be able to account for these observations and which could at the same time characterize the notion "human language" to the extent that such characterization explains (= "accounts for") all we feel this notion to carry (= "our intuitions"). Furthermore, we do not think such a theory is possible as it would be overspecified, hence describing nothing in particular. Were it ever to arise, though, it should not result from any preconceived notions concerning "ethnic languages" only. (These notions may somewhat catachrestically be called "intuitions" but they assume the theory of human language as given, attempting at the same time to formulate it.) Weinreich's definition of language as "an object possessing orderly heterogeneity" leaves him with a cluster of problems as soon as he starts dealing with neither "object" (but sets of rules) nor "possession" (but competence, performance, deep and surface structure), i.e. with a set of notions that may hardly be understood outside the criticised theory. A still more ambiguous situation arises in the case of Bailey (1971: 11 - 12) when the theory rejects even the concept of "ethnic language" and is stipulated instead to operate in terms of "lects", "isolects", "basilects", and "acrolects" in order to account for the "panlectal understanding competence" and "speaking competence" (1971: 25 - 26). One may see how far we have got from the notion "human language" as it is felt by an average English speaker (i.e. at the pretheoretical level; as stated above the notion

⁴¹ By general linguistic theory (a term used so often here that it may need clarification) we mean a set of statements (rules, definitions, etc.) which simultaneously account for the three following statements:

- a. "the whole human race possesses but one language",
- b. "groups of individuals, consisting of various numbers of individuals, may possess languages which are not totally identical",
- c. "every man possesses an individual language" (perhaps a trivial fact).

does not appear at the theoretical level at all)⁴². And it is doubtful if by our preoccupation with phonetic/phonological — actually the most variable and least creative level, the notion may ever be elaborated within the theory.

We should not, however, disregard another possibility, i.e. that the heterogeneity hypothesis is logically borne out by the facts pertaining to language change. Thus Weinreich (1968:98) says:

"The present paper is based on the observation that structural theories of language, so fruitful in synchronic investigation, have saddled historical linguistics with a cluster of paradoxes which have not been fully overcome." "...the solution we will argue, lies in the direction of breaking down the identification of structuredness with homogeneity. The key to a rational conception of language change — indeed of language itself is the possibility of describing orderly differentiation in a language serving a community." (1968: 101)

If what Weinreich wants to say here is that variation in a community of speakers of one "ethnic" language is not chaotic but structured objectively (existence of styles and dialects) he is right, but the fact has already been noticed by the structuralists. If he wants to point to a lack of a technical mechanism for dealing with such facts, he is also right (the necessary mathematical sophistication, other than statistical, is a pretty recent invention). But, as evident from the last sentence, when he claims that both of these are sufficient or necessary conditions for a theory of human language, then Weinreich is wrong.

Bailey (1971: 17) has taken the above hypothesis of Weinreich to its logical conclusion. The result is as follows:

"The point of view that will be defended here denies that there are two different kinds of analysis — diachronic and synchronic — since a purely synchronic analysis in abstraction from change is denied. A grammar is as wide as its data. A dynamic grammar that admits only data potentially available to a given, illiterate child may be called ... a 'synchronic' grammar. But it really has to be diachronic, if it is to describe the whole competence of speakers in a ... (given) ... community ..." ⁴³ And in a trivial sense, he is right, for, technically speaking, no one may lawfully say: "It's three o'clock now" and remain consistent with empirical facts. (A linguistic example which "proves" Bailey's point would be such morphological syntagma whose sense depends in a way on "historical" knowledge, e.g. sacrilegious).

The question of paradoxes widely plaguing historical linguistics has somehow become one of the most popular subjects in methodological considera-

⁴² Cf., for example: "my position is that, whatever its level of abstraction may be, the grammar should set up underlying representations and rules that will generate all of the systematic variation in the data at the systematic phonetic level for every lect abstracted form" (Bailey 1971: 13).

⁴³ We shall disregard here all the misunderstandings of GG evident from the passage.

tions. The three most often cited will therefore be mentioned here. Two of them concern the theory of change in particular, namely, the old Zeno Paradox (mentioned in: Weinreich 1954; Bohnert 1969; Bailey 1971; Andersen 1972; Collinge 1970 and Klyčkov 1971) and the not unrelated "Saussure's temporal paradox" (Weinreich et al. 1968; Bailey 1971) both of which will be discussed presently. The third one, which we would like to deal with now, is the "Saussurean homogeneity paradox" formulated by Labov and mentioned in Bailey 1971 and Labov 1971. The paradox has been stated as follows:

"the social aspect of language can be studied by the theorist asking himself questions, while the individual aspect can only be studied by a social survey".

To be rightly named paradox, the statement should contain, or lead to two contradictory sentences which the above statement clearly does not. For, either we have a clear notion of what language is, and in this case the status of linguistic theory is null (the theory is unnecessary), or we want to find what language is and thus, we formulate a theory by any imaginable procedure, be it introspection, social survey, handbook analysis, or the like. Neither Chomsky with respect to his "performance", nor de Saussure, with respect to his "parole" had ever suggested that it be studied "only by a social survey" (if this is what is meant by the "individual aspect" as seems to be the case given such a Saussurean statement as: "In separating LANGUE from PAROLE, we simultaneously separate: 1. what is social from what is individual, ..." (1959: 14). On the contrary, both stressed that the nature of PAROLE (performance) will remain mysterious and chaotic unless we clearly grasp the nature of LANGUE (competence):

(Saussure 1959: 18) "this study (of langue) is purely psychological; the other, secondary has as its object the individual aspect of language, that is, parole, including phonation, it is psychophysical".

(Chomsky 1965: 10) "There seems to be little reason to question the traditional view that investigation of performance will proceed only so far as understanding of underlying competence permits".

Thus, we think we have shown that Labov's statement is not a paradox at all but results from misunderstandings of some crucial points in Chomsky's and Saussure's approaches to language theory.

Zeno's paradox, as reformulated by Weinreich (1954) appears as follows:

"A moving arrow is located at some point at every moment of time; at intermediate moments it is at intermediate positions. Therefore it never moves. Rigidly applied, the typical elements of structural description — 'opposition' and 'function of units with respect to other units of the same system' — have come close to incapacitating structural analysis for the consideration of several partly similar varieties at a time".

First, the analogy is largely ineffective: Zeno's paradox concerning some substantive issues, Weinreich's illustration — formal (terminological) ones. We do not want to enter here into the complicated problems of Zeno's lack

of mathematical sophistication, his logical inconsistencies or semantic equivocations⁴⁴. The problem was discussed extensively by Ajdukiewicz (1948) who has proved rather convincingly that no description of change does logically entail two contradictory sentences. What is more, he has explained the source of the variability theory position with respect to language change:

"If one accepts the continuity postulate⁴⁵, he must assume that the multitude of states we analyze does not have the discrete ("corpuscular") structure but forms a continuum in the sense that there are no two states A and A' minimally different from each other, but that to each A-state one may always find a corresponding A'-state which will differ from the previous one as little as we like". (Ajdukiewicz 1948: 104, transl. mine)

In the light of the above it is easier to follow Bailey's dynamic "lectal" position, but his weak points become obvious. Even if generative theory remains vague as to what it means to say that the English language has changed, it is explicit as to what it means that a grammar G changed into G₁ while no proponent of the variation theory will be able to explain such concepts as grammar, language, or X speaks Y. Both theories work with different concepts of change and hence the clash of both on such statements as "change is not perceptible" is unavoidable since one describes L : non-L variation, the other — L : L₁ difference. It seems that the two are complementary and the superiority of GG lies in its complementing a possible theory of human language, while no clear theory of language is possible with the assumptions made within the variation theory as noted above.

At this point we do not think that any discussion of the "temporal paradox" is necessary. The "paradox" which, as reported in Bailey, was formulated by de Saussure (1959: 78) himself is evidently not a paradox at all:

"If you took language (LANGUE) in time, apart from the mass of speakers — suppose an isolated individual living through several centuries — you perhaps would

⁴⁴ Briefly: a *mathematical* solution depends on the infinite number theory and was intuitionally offered by Aristotle, *logical* inconsistencies are of this type: from the two sentences below, the second does not logically follow from the first:

1. For every human being there is a man who is his father;
2. There is a man who is father of every human being.

Similarly: from the assumption that at every time point *t* there is a position *p* at which the arrow is at *t*, it does not follow that there is a *p* at which the arrow is located at any moment. *Semantic* issues (in the case of Weinreich's formulation) may concern: "to be located", "point", "moment of time", "intermediate moments", "intermediate positions" — such issues were discussed (for a different formulation) by Bergson.

⁴⁵ The continuity postulate stipulates that between *t*₁ at which some object *A* is in an α -state, and *t*₂ at which *A* is in a β -state there are moments at which *A* is neither in α nor in β states but is in the process of change from α to β .

The postulate, however, is not an a priori postulate but an inductive generalization from experience and may be rejected by some branch of science (e.g. modern physics: the quantum theory). See: Ajdukiewicz (1948: 100 - 104).

not establish any change; time would produce no result on it. Conversely, if you considered the mass of speakers apart from time, you would not see the effect of the social forces that operate on LANGUE."

Summing up, we have noted that the heterogeneity hypothesis is untenable as it may lead to no theory of language in general, nor to a grammar of a language. Its proponents are preoccupied mainly with terminological issues and data gathering, though many of their observations concerning variation in languages may be helpful in solving problems unsolvable in other theories (e.g. the actuation riddle, transitional "language stages", spread of a particular change, effects of changes, observation of "drifts-in-being", etc.). Thus, some of the restrictions on possible changes will undoubtedly originate within this approach (or rather they may be sooner arrived at here). We do not think the heterogeneity hypothesis to be necessary or even helpful to arrive at all these conclusions, on the contrary: as the proponents of this theory will have to refer in any case to the notion of the "standard dialect", "ethnic language" (English, German, Polish, etc.) in their contrastive statements it would be much more plausible to claim that in addition to other capacities, all speakers have intuitions as to what "language", and "a language" is. A general theory of language will have to account for this no matter if one approaches it from a homogeneity or heterogeneity postulate.

Conclusions. In the preceding sections we have tried to show why no generative theory of change, nor any plausible alternative exists. Part of the "responsibility" for that we found to rest with the general theory of language. Thus, we postulated some minimal requirements the general theory should meet if the theory of change is to come out with a plausible model. What we need is a clearly defined status of the theory itself (presumably with some "idealizational postulates" unequivocally formulated); or, in other words, an unambiguous notion of "competence" which the theory of change would need to answer the key question: "what is change?" (L : non-L variation, or L : L₁ difference). If, as seems to be the case, the theory of change is to work in terms of grammatical contrasts, we assume that an answer to the above question explains what the grammars we are to contrast are grammars of. Then, principles of "interesting" (non-trivial) comparisons should be provided. My guess is that such principles will have to operate at various levels of "depth" and not, as sanctified in the literature, at the level of only deep structure, or transformational and lexical subcomponents (as in: Nagucka 1968, Closs 1965 and R. Lakoff 1968, respectively). This, in turn, entails a need of formal and substantive constraints to be provided by the general theory to elaborate (and possibly restrict) the notion of simplicity. It seems plausible that the notion of simplicity will have to be defined within a theory of contrast independently of the general theory, as hinted at in Collinge 1970. It is also the theory of contrast (or general theory if it included the latter) that should establish

when two rules, or sets of rules are "same" and when they are "different".

The theory of change, on the other hand, will have to be explicit with respect to its goals. And these also seem to depend on the definition of change and non-trivial comparison. We do not think that Chomsky and Halle's synchronic-on-historical-principles approach needs any theory of change at all. To arrive at their underlying structures and processes, which will have to anyway be independently justified (as not all structures and rules reflect historical processes) they may use any heuristic procedure. There is still the all-important problem of justification of certain synchronic rules and categories which may be thought of as a bonus for which the whole problem of the theory of change has been raised. We have assumed that if such justification could not be provided by psycholinguists (for the reasons stated in Kiparsky 1968) it certainly could be provided by linguists studying grammars of both living AND dead languages. We may assume that the set of rules and categories found in these grammars does define the notion "possible rule" and "possible category" or the more so if one knows how to make non-trivial contrasts of formal, explicit and simple grammars. For the notion of "linguistic universals", as rightly noted by P. Ladefoged (at Buffalo 1971 LI session), are only "linguistic terrestals". What remains undefined within such theory is the concept of variation of grammars. But as this will have to lead us away from, rather than to a theory of human language, we have to admit that we need a complementary approach which possesses an adequate sociological and geographical apparatus to better account for such problems as the actualization, embedding and transition of change among others.

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