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CONTRASTIVE ANALYSIS, ERROR ANALYSIS, AND TRANSFORMATIONAL GENERATIVE THEORY: SOME METHODOLOGICAL ISSUES IN THE THEORY OF SECOND LANGUAGE LEARNING

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Le rôle de l'analyse contrastive en linguistique appliquée s'est transformé au cours de l'évolution. Les premières études contrastives avaient tout simplement pour but de *prévoir les fautes*. C'est surtout sous l'influence de la grammaire générative transformationnelle que l'objet de l'analyse contrastive est devenu de plus en plus une *explication des fautes*. C'est ainsi que l'analyse contrastive a pris dans une large mesure la forme d'un complément de l'analyse des fautes. L'auteur de cet article voit la raison du rôle incertain de l'analyse contrastive à l'intérieur du cadre de la linguistique appliquée dans un manque de théorie bien définie pour l'apprentissage d'une deuxième langue. Il propose d'adapter la théorie d'apprentissage valable pour la deuxième langue à celle de la grammaire générative transformationnelle, de sorte que le modèle descriptif de la compétence transitoire en cause ne soit pas fondamentalement différent de celui qui vaut pour la compétence dans la théorie générative. Il est démontré comment un modèle d'analyse contrastive qui, en même temps, *prévoit* et *explique* les fautes selon les principes méthodiques mentionnés ici, sera à même d'intégrer les problèmes de l'analyse des fautes dans une solution d'ensemble qui élimine l'opposition.

Die Rolle der kontrastiven Analyse in der angewandten Sprachwissenschaft hat sich im Laufe der Zeit gewandelt. Die ersten kontrastiven Studien wollten einfach *Fehler voraussagen*. Vor allem unter dem Einfluß der generativen Transformationsgrammatik wandelte sich das Ziel der kontrastiven Analyse in die Richtung der *Fehlererklärung*. Dadurch wurde die kontrastive Analyse weitgehend zum Anhang der Fehleranalyse. Der Autor dieses Artikels sieht die Ursache für die unsichere Rolle der kontrastiven Analyse im Rahmen der angewandten Sprachwissenschaft in dem Fehlen einer klaren Theorie des Zweitsprachenerlernens. Er schlägt vor, das Modell für die Theorie des Zweitsprachenerlernens auf die Theorie der generativen Transformationsgrammatik abzustimmen, so daß das Modell zur Beschreibung der jeweiligen Übergangskompetenz grundsätzlich nicht verschieden sein soll vom generativen Modell der Sprachkompetenz. Es wird dargelegt, wie durch das *sowohl Fehler voraussagende als auch Fehler erklärende* Modell der kontrastiven Analyse, das den oben genannten methodischen Prinzipien folgt, die Auseinandersetzung mit der Fehleranalyse in einer integrierten Lösung aufgehoben werden kann.

1

In 1969, Carl James lamented that contrastive studies were in the doldrums and had lost the "bite and enthusiasm" which had pervaded the work of Fries and Lado (James 1969). Among the four ways for revitalizing this study, viz. heuristic investigations to test its predictions, greater collaboration with psychology, integration with other branches of applied linguistics particularly the theory of translation, and reorientation of contrastive studies in the light of Chomsky's transformational generative grammar, James laid particular stress on the last one. He noted that some isolated attempts had been made to base contrastive analysis (CA) on the TG model

(Stockwell, Klima, Dingwall) but found them "only superficially generative." "Restatement of the familiar with the paraphernalia of rewrite rules and tree diagrams is not enough," he very pertinently remarked, "little is gained unless the power of the theory is exploited" (James 1969:84).[1]

How does one go about exploiting the power of the TG theory in the interest of contrastive analysis, particularly in the face of such trenchant remarks as are made by Roger Snook (1971:17), who considers transformational generative models irrelevant to CA? His argument is simple. CA is concerned with language teaching and learning in general, and with the prediction of difficulty and errors in particular. Now, language learning and TL errors "are psychological processes or result from such processes." Hence any theory "which attempts to account for these phenomena should be couched in psychological terms or be relatable in an explicit way to the psycholinguistic data" (Snook, *op. cit.*, p. 19). James then goes on to examine the experiments conducted by Miller, Fodor, and Garrett on the relationship between grammatical and perceptual complexity. As is well known, these experiments had failed to show any correlation between the performance measures of sentence complexity and the derivational history of the sentences. From this failure Fodor and Garrett had concluded "that the problems may be not that our experimental procedures fail to measure perceptual complexity, but rather that it is a mistake to claim psychological reality for the operations whereby grammars generate structural descriptions" (Fodor and Garrett 1966:152).

As James points out, the failure was perhaps a foregone conclusion. Chomsky had been quite explicit on the point. A generative grammar, he had said, was not a model for a speaker or a hearer. The linguist's description of how a sentence is derived belongs to the theory of competence, whereas its actual production or interpretation is a matter of language use, or performance. There is no necessary correlation between the two. All Chomsky had claimed was that a theory of competence was part of the theory of performance in the sense that every ideal hearer-speaker was regarded as having the grammar at his disposal. However, the theory did not include an account of how the grammar was used. It did not incorporate a model of the mechanism underlying the speaker-hearer's verbal performance. That belonged to the theory

1 More recent attempts to exploit the power of the TG theory have not yielded any more satisfactory results for the same reason. Instead of trying to find-in the methodology of TGG a solution to their basic difficulty, the exponents of a TG version of CA have been trying to latch themselves on to the various theoretical constructs proposed by TG grammarians from time to time only to find a few years later, as the parent grammarian moves on, that they have been left holding an empty construct. Thus Kreszowski based his hypothesis that any set of translation equivalents are derived from a common deep structure on Lakoff's 1968 definition of deep structure which has since been shown to be untenable. Bouton (1976) has made the interesting demonstration that the deep structure conditions and the Universal Base Hypothesis are mutually incompatible when applied to the problem of translational equivalence in CA, which further illustrates the dangers lurking in simple-minded applications of TG constructs to CA.

of performance which was primarily a psycholinguistic and sociolinguistic study. Questions of language learning and aberrant TL performance, says Snook, belong to the theory of performance while TGG is a theory of competence. Obviously, then, a TG grammar cannot be relevant to these problems, and hence to CA.

Several things are wrong with Snook's argument not the least of which is his selection of convenient quotations from Chomsky and Fodor and Garrett. It is true that a generative grammar is a model of competence and does not include matters of performance, but it is also true that, according to Chomsky, competence is a necessary component of any performance model. To this extent, psychological reality is attributed to competence by Chomsky. But beyond claiming that a knowledge of the rules of grammar is a prerequisite for meaningful performance in a language, the theory does not say much on this issue. For instance, it does not claim that the relation between competence and performance is such that the rules of grammar which constitute competence represent directly psychological processes operating in 'real time' in the production or perception of the sentences of the language. The Miller and Fodor and Garrett experiments were based on too strong an assumption, which was never made by Chomsky. The assumption was that each PS and T rule was psychologically real, i.e. that competence and performance were directly related and a performance model was isomorphic with a competence model. This was too simple-minded an assumption. No one who had understood the high degree of abstraction and sophistication that Chomsky saw in the processes of language could ever attribute this assumption to Chomsky. Fodor and Garrett realized this and were very careful to state explicitly that "...in showing that predicted complexity order fails to obtain, one has not shown that the grammar is disconfirmed . . . one would best interpret negative data as showing that an acceptable theory of the relation between competence and performance models will have to represent that relation as abstract . . ." (Fodor and Garrett 1966:152). The mistake perhaps lies in assuming that an abstract relationship between competence and performance is incompatible with a part-whole relationship, i.e. with a model which assumes, as Chomsky does, that a competence grammar must part of the performance model. It is not at all clear that there is such an incompatibility. The complexity, or abstractness, of this relationship may equally well be due to the complicated psychological mechanisms which connect linguistic competence with behaviour, and it is to these that the psycholinguists need to direct their attention. Sutherland's comments on the Fodor and Garrett paper (Sutherland 1966:161) are very pertinent in this connection:

> The task of psycholinguistics is not to confirm Chomsky's account of linguistic competence by undertaking experiments. To the extent that Chomsky has succeeded in axiomatizing grammar, his account does not stand in need of such confirmation. The task of psycholinguistics is to my mind . . . to find out what are the mechanisms which underlie linguistic competence.

2

Those contrastive analysts who have adopted the TG framework as their model have so far predicted the areas of difficulty on the basis of a comparison of models of competence, i.e. the linguistic grammars of the languages involved. If any of these predictions turn out to be correct, it suggests that, as James (1969:65) points out, "some areas of performance and competence are systematically identical." It has been repeatedly said (see, e.g. Dušková 1969, James 1971, Nickel 1971, and elsewhere) that CA is not committed to the view that all errors made by the foreign language learner are caused by interference from the source language and that therefore not all errors are predicted by CA.[2] Among the errors not predicted by CA will be some which are due to purely performance factors. These will include, e.g., Selinker's "transfer of training strategy", strategies of second language learning, and strategies of second language communication (Selinker 1972). This indicates that while some errors may be predictable from a comparison of competence grammars, there will be many errors not so predictable since they are due to performance factors. This confirms the view that a performance model cannot be coterminous with a competence model: it must include the competence model and exceed it. As James (1969) points out, what still remains to be discovered is the precise nature of the relationship between performance and competence. The question facing psycholinguistics is "whether, and if so, how the native speaker's modelled competence is projected onto his performance" (James 1969:85). I think enough evidence is now available to answer the whether-question in the affirmative. The issue is a crucial one in the theory of TGG and has been posed in the form of the claim that the rules and systems which are said by the linguist to constitute the grammar of a language are also a model of the native speaker's competence, i.e. are psychologically real (Katz 1964). Recently this claim has been made more precise by distinguishing the 'performancist' position from the 'Platonic' and the 'competencist' ones (Katz 1977). The performancist position is the one that the Fodor and Garrett experiments failed to confirm. Following the same position in an attenuated form, Fodor, Fodor, and Garrett (1975) discovered the unreality of semantic representations. The competencist position is the one that is indicated in the quotations from Fodor and Garrett and Sutherland above, viz. that "failure to predict experimental results such as complexity orderings does not count against representations at any grammatical level" and that "grammars are to be judged not by their success in predicting such results but

2 In a recent paper, Nöth (1979) classifies errors into monosystematic, diasystematic, and non-linguistic varieties. A similar but more comprehensive analysis is attempted in Sah (1971). Among other things, Nöth, who regards errors as providing a discovery procedure for the linguistic theory, makes the relevant point that though Chomsky's idealization "seems to have no place for error analysis" and treats errors as "part of the study of performance," Chomsky's theoretical position is often not in accordance with his own procedure of analysis. Chomsky very frequently gives "examples of mistaken English sentences in order to explain central concepts of his linguistic theory."

by the internal evidence in favour of the structural descriptions they generate" (Katz 1977:561). This does not amount to making grammar an abstract science like mathematics, which is the Platonist position. The competencist position still makes a claim to psychological reality of the grammar but in the weaker sense in which "it represents an idealization of the knowledge that speakers of the language have about its grammatical structure" (Katz 1977:564). It is this Chomskian sense of "grammar as competence" that makes the claim of the psychological reality of grammars equivalent to the claim that competence is neither the whole of performance nor totally absent from it but forms one of its essential components. Evidence has been forthcoming from such diverse fields as studies in aphasia, language acquisition, bilingualism, second language learning, etc. which shows not only that the distinction between competence and performance may be a correct one to make but also that the actual relationship between them may be the one that is implied in the competencist position. Studies in aphasia (e.g. Weigl and Bierwisch 1970) suggest that a theory which treats of aphasic disorders as loss of performance abilities rather than as loss of competence is better consistent with the facts. Competence, once lost, is unrecoverable; but performance may be impaired for reasons other than the loss of competence and revived later, all of which suggests a role for competence as well as performance. Evidence from CA also suggests that competence may be an indispensable factor in performance. The evidence is provided by what Pit Corder (1967) calls the systematic nature of some errors. He justifiably uses these as a basis for setting up the notion of "transitional competence." Transitional competence contains elements from both L1 and L2 competences and accounts for a significant number of errors. It also helps us to distinguish between systematic and non-systematic errors, thus providing a more coherent account of errors than has been provided so far.

The central importance of the kind of evidence Pit Corder and Selinker provide for a transitional competence lies in providing indirect verification of the Chomskian claim that competence is not only relevant but basic to any "reasonable model of language use" (Chomsky 1965:9). The crucial defect in Snook's argument is therefore the following: it forecloses the basic issue by precluding the use of evidence from contrastive studies in verification of the postulated competence-performance relationship. The basic issue surely is whether competence is essential to performance, and not, as Snook makes it appear, that a TG grammar is a model of linguistic competence. That it certainly is. What Chomsky had said was that "the generative grammar does not, *in itself*, prescribe the character or functioning of a perceptual model or model of speech production" (Chomsky 1965:9; italics mine). This quotation cannot be used to justify Snook's conclusions. From the fact that, *in itself*, a TG grammar is not a model of performance one cannot argue that *therefore* it is irrelevant to performance, but this is precisely what Snook does. And in doing so, he also blocks the avenues for any further exploration of the areas of applied linguistics, such as CA, where evidence relating to the postulated relevance of competence to performance may be found.

3

If the notion of competence is to be exploited fully in CA, it is necessary not only that the goal of a precise definition of the competence-performance relationship be constantly kept in view, but also that we go beyond the notion of "differential competence" (see, e.g. Corder 1967; Nemser 1971) to the notion of "transitional competence." Carl James remarks that "contrastive study will be pedagogically of value proportionately to the identification of competence and performance values, that is, to the degree that its statement of differential competences can predict probable obstacles to acceptable performance in the target language" (James 1969:85). The limitation of such an approach is exemplified by James himself. A sentence "John is a naughty boy" is derived in English from the conjoining of two sentences "John is a boy" and "John is naughty". In another language, a form isomorphic with "John is a naughty boy" may be derived from "John boys" and "John naughties" by a different transformation. In a case like this, a contrastive study based on competence must predict that "this speaker will have difficulty in arriving at "John is a naughty boy" in English, whereas we attest that his performance is perfectly acceptable." The conclusion James draws from this example is that "the scope of performance" must be extended beyond L1 performance to "what we might call 'contact' performance." To me, it seems that the example actually calls for an extension of the scope of competence from "differential competence" to "transitional competence". My reason is as follows: the prediction is based on a study of the differential competence of English and the other hypothetical language and it turns out to be incorrect. If instead of basing the prediction on differential competence we had examined the transitional competence of the learner as reflected in his errors, then, irrespective of whether this particular error was actually in the corpus or not, the prediction would be subject to our finding in the learner's competence a (semi-) stable system of rules generating such an error. The question of how the learner arrived at such a system would not be very relevant to the study of transitional competence since, as Nemser (1975:104) points out, this transitional "approximative system" is structurally independent of the base and target languages. It would be reasonable to assume that an equivalence process of some sort was at work and that both L1 and L2 competences were involved. If in the present case absence of the relevant kind of error showed that the correct equivalence had been arrived at, the error would not be generated (i.e. predicted). The notion of transitional competence thus helps to eliminate the large number of "misses" which are bound to occur when a comparison of competence grammars is used to predict difficulties for an organism which is completely missing from the picture. The notion of transitional competence brings the organism back into its rightful place in the picture and makes use of its performance data to delimit further, and much more accurately, the areas of difficulty. There is no circularity involved here, as those familiar with Chomsky's central argument in *Syntactic Structures* will appreciate (see Sah 1973).

While contrastive explanations are now accepted as the final output of EA, there is no parallel acceptance of performance data (error and non-error) as input to CA. Particularly, non-error data, the significance of which has been recognized by some error analysts (see, e.g. Jain 1974), have no role to play in CA, not even in aposteriori CA where at least the error data are taken into account. Although it has been pointed out by several linguists (e.g. Hamp 1968, Stockwell 1968, James 1971), who believe that CA and EA are not really to be viewed as alternatives, that they are actually prognostic and diagnostic varieties of CA, **what has actually been demonstrated is that CA is a subcomponent of EA and not that EA is a variety of CA**. If the sole function of CA is to provide aposteriori explanation of errors, as the exponents of EA would have us believe, then it is no more than an explanatory appendage to EA, and an ad hoc one at that, as it is expected to explain, if it can, the residue of errors left over from other explanations.

Dissatisfied with this minor role for CA and unable at the same time to see any more meaningful role for it in the field of applied linguistics, some linguists have tried to enlarge the explanatory function of CA in the direction of theoretical linguistics. Beginning with statements like "The justification for contrastive analysis is to be found in its explanatory power" and "Explanatory power should be the ultimate goal of all contrastive linguistics", Van Buren (1974:279) goes on to conclude that "if a contrastive study fails to explain anything about the nature of the language data, it scarcely seems worth the time and labour expended on it." The goal of contrastive studies, according to him, is to "contribute to our knowledge of language structure and of the relations which obtain between different language systems," while the "chain of connections between contrastive linguistic theory and what happens in the classroom" is said to be an indirect and a complex one. This view of "contrastive analysis" is perhaps sufficiently different from CA as it is understood in applied linguistics to justify its being called by the title of "contrastive linguistics". As a tool of applied linguistics, CA is immediately concerned with "what happens in the classroom" inasmuch as it helps the teacher-linguist to construct pedagogic grammars from scientific grammars. If the predictive power of CA is taken away, its value to applied linguistics is considerably reduced, though its value for theoretical linguistics may proportionately increase. The original inspiration behind CA was its use as a device for predicting certain kinds of fairly widespread errors in the learning of a second or foreign language. This is a practical goal, as applied linguistics is a practical study (Corder 1973:137). Explanation, on the other hand, is a theoretical goal better served by a linguist engaged in the pursuit of linguistic universals. Contrastive study is only one of the various techniques in such a linguist's repertoire of techniques and its status is that of a discovery procedure. While no one will deny the importance of the feedback practice provides to theory (nothing is as theoretically provocative, says Fishman, complementing Kurt Lewin's famous remark about the usefulness of a good theory, as effective practice CA cannot be viewed afresh solely as a discovery procedure employed in the service of theoretical

linguistics, which it must be if explanation is regarded as its sole *raison d'être*. If CA is to retain its basic relevance to language learning and teaching, it must also retain its traditional link with applied linguistics.[3] And this implies that it must predict and not merely explain.[4]

A possible line of argument here is that there is essentially no conflict between prediction and explanation and that they actually go together. This line of argument usually ends up by regarding explanation as the stronger goal and concluding that if this stronger goal is achieved, the subordinate goal of prediction is automatically achieved. That this is not so has now been amply demonstrated in discussions on the methodology of social sciences (see, e.g. Isaac Sheffler 1960, Kaplan 1964:346 ff., Scriven 1964:173). Scriven points out that explanation and prediction are logically quite different: it is possible to have explanation without prediction and *vice versa*. Kaplan confirms this. Scriven goes on to account for this logical possibility by pointing out that in some cases explanation is easier than prediction while in other cases it is more difficult. "On balance," says Scriven, "explanation in psychology is easier than prediction" (Scriven *op. cit.*). I think this also applies to linguistics ("a sub-field of cognitive psychology" according to Chomsky) and more specially to psycholinguistics. The import of this statement of course cannot be realized fully so long as difficulty is held to be synonymous with difference, and predictions are based on the differences between competence grammars. It is only the notion of transitional competence which is capable of bringing out its full import. Not until we find out more about the ways in which elements of the two competences fuse and interpenetrate in the learner's transitional competence can we make predictions which will be less free in inviting the ridicule of the critics of CA. This implies that the contemporary exponents of CA must also be less free with their predictions than their predecessors unwisely tended to be.

3 See, e.g. Bouton (1976), who states that it is usual for the purpose of CA to be stated completely in terms of its contribution to language teaching. He quotes Sciarone (1970): "The *raison d'être* of CA is the realization that in learning a second language one is confronted with interference from the native language."

As an example of the practising teacher's touching faith in the pedagogical value of CA, see Ray (1976).

4 It is in the light of these remarks that I view with some concern Noth's attempts (see Fn. 2) to see in error analysis also a discovery procedure for linguistic theory. These attempts by applied linguists to jump on the theoretical bandwagon cannot but bode ill for the future of applied linguistics and particularly for its value for the problem of language learning and teaching. It is puzzling to find these linguists totally unimpressed by Chomsky's expression of scepticism about the value of theoretical linguistics to problems of language teaching (Chomsky 1966), or perhaps they feel that though linguistics may have nothing to contribute to language teaching theory, the contribution in the other direction is so substantial that linguistics is poorer without it. Whatever the reason, it would be a sad day for applied linguistics when its exponents are forced to seek legitimacy for their pursuits in the goals of theoretical linguistics.

This can be made possible if we impose on our predictions the condition of explanatory power. But this condition is nothing new: it is already implicit in the notion of (transitional) competence. Just as any competence grammar must meet the three levels of adequacy requirements imposed on it by linguistic theory in the case of the native speaker-hearer, so any grammar of transitional competence must meet these requirements in the case of the second language learner. Not only must it describe exhaustively the sets of limited data with which it is concerned (observational adequacy) but it must do so in a manner both explicit and predictive (descriptive adequacy; see, e.g. Corder 1973:86-8). In addition, the theory of CA, like a linguistic theory, must incorporate an evaluation metric for the grammars of the interlanguage. A grammar meeting the constraints of explanatory adequacy would reflect some universal truths about the way languages are acquired and the ways in which they interact in a contact/learning situation.

The similarities between the two situations – the first language learner learning his native language and the linguist's model of his terminal competence, and the (first or) second language learner and the linguist's model of his transitional competence – thus run all along the line. In both cases the grammar models the competence. Taking up for comparison only the first language learner's terminal competence and the second language learner's transitional competence, we find that the function served by linguistic theory in the former is served by the theory of SLL (Second Language Learning) in the latter. The input to the former is the primary linguistic data. The linguistic theory, with its set of postulated universals, produces as output the grammar of the language concerned. If the output of this grammar does not match that of the native speaker, the theory is not validated and requires modification and revision till a match is achieved. In the case of the applied linguist's model of a transitional competence, the input consists of performance data (both error and non-error) of the learner. **The theory of SLL itself comprises, *inter alia*, the competence grammars of L1 and L2, the mechanisms of comparison, the postulated universals (developmental, implicational, and others) of language acquisition**, etc. The CA sub-component of this theory operates to compare the two grammars in the light of the input data and, together with the other components of the theory, produces as output the grammar of the interlanguage. Among the output of this grammar are predictions of error. If the predictions turn out to be incorrect, the theory of SLL must be modified. This may mean revising anything from the grammars of L1 and/or L2, the mechanisms of comparison, to the postulated universals of language acquisition and interaction. One implication of a model of SLL theory of this kind is that grammars of interlanguage may provide a check even on the grammars of competence. This however is not the paramount consideration for us. The paramount consideration for us is that **in a model of this kind it becomes possible to employ both prediction and explanation in the service of the theory in a mutually reinforcing manner without either getting bogged down with the other.** Faulty predictions imply imperfections in the theory, but correct predictions do not nec-

essarily mean that the grammar is the best one possible: the theory does not allow for any decision procedures. Correct predictions only imply that the grammar may be a correct one, but no more than that. This is so because the theory imposes certain internal criteria on the grammars in order to ensure that the best grammar is more than a correct grammar, i.e. that explanation does not remain confined to prediction. However, it also means that as explanations become more refined, predictions become correct over a wider range, and of course that they become more meaningful. In other words, they are no longer simple predictions but explanatory ones.

4

To sum up, we have now before us four models of CA, if CA is interpreted in the wider sense in which Stockwell (1968) and Hamp (1968) understand it. The first model is what I call the **Simple Prediction Model.** This model takes the competence grammars of the two languages as input and produces as output the likely errors of the learner. We can diagram this model as follows:

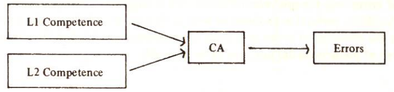

Fig. 1: The Simple Prediction Model

This is the original Lado-Fries model which was taken over *in toto* by the earliest TG theorists of CA (Stockwell *et al* 1965, Dingwall 1969, P. Schachter 1967, Whitman 1970). All that these studies do is to replace the units and structures of the Lado-Fries model by the PS and T rules of TG grammar. The advantages gained from this substitution were only those associated with the deep-surface distinction in syntax (Nickel 1971:4), although in view of the uncertainty attached to the precise status of the deep structure (e.g. whether it was universal or not) and the indecision about what exactly in this formulation concerned the contrastive analyst most (transformations alone or the structural descriptions as well: the question was of course linked to the previous one), it cannot be said that even this distinction was fully exploited. In more recent years, this approach has been developed further by Dirven (1976). Dirven is not satisfied with the deep structure oriented analysis and proposes a redefinition of CA in terms of a conceptual approach. But although Dirven argues convincingly for the enlargement of the scope of CA to include an analysis

of the conceptual and perceptual strategies employed by the native speakers, he is either still working within the framework of the Simple Prediction Model or has opted out of CA altogether. In either case, the basic methodological insights of TG grammar remain totally unexploited in the service of applied linguistics and there is no change in the conventional ways of looking at CA and EA. The assumption regarding prediction of errors on the basis of grammatical comparison of languages remains unquestioned: only the scope of grammar is redefined and its heuristics expanded. The basic objection raised to this model that it fails to predict *all and only* the actually occurring errors (see Ritchie 1967, Whitman and Jackson 1972) remains unanswered, although TGG itself aimed at allowing only grammars which generated *all and only* the grammatical sentences of a language and would therefore not lack the methodology requisite for this purpose. As I see it, and will become clear in the sequel, what permitted TGG to constrain grammars to generate *all and only* the grammatical sentences was basically its non-linear nature, whereas the refurbished CA remains basically a linear model.

The second model (Fig. 2) is a model of error analysis in the sense of Stockwell and Hamp, who regard EA as the inductive variety of CA. This conception of EA as a variety of CA arose as a result of dissatisfaction with the first model most clearly reflected in the remarks of James cited at the beginning of this paper. So long as the prediction of errors was the sole *raison d'être* of CA in language teaching and learning, it was decidedly preferable to dispense with it altogether and choose the path of EA, since the coverage of errors was more comprehensive in EA and the risk of non-fulfilment of predictions did not have to be incurred:

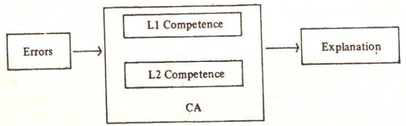

Fig. 2: The Error Analysis Model

If some errors were due to contrastive reasons, this could always be discovered after other errors had been accounted for. A comparison of the relevant areas of the grammars could then be undertaken to see if a contrastive linguistic explanation was available for them. It was quite unnecessary to compare whole grammars of the languages when only a limited number of errors were to be accounted for in this way.

If CA was to survive, a different *raison d'être* had to be found for it. This was found in explanation. Now it was possible to admit frankly that CA did not claim to account for all possible kinds of errors (e.g. James 1971), and that, anyway, that was not the main purpose of CA. Its main value lay in the explanations it provided for the errors. This amounted to accepting a status subordinate to that of EA as "subcomponent of the more encompassing field of error analysis" (J. Schachter 1974: 206). But this fall in the status of CA was accompanied by an acknowledgement of the importance of its role since, without CA, EA was a mere taxonomy. This model, then, took errors as the input, and with a comparison of the competence grammars of the languages involved, sought to provide explanations for the errors. This is still a linear model since now there is no way of verifying if the explanations are correct. In the Simple Prediction Model, there is at least the possibility of the predictions turning out to be incorrect. Now that constraint is also gone and is not replaced by any built-in constraint of the theory. As a result, the danger of ad hoc explanations becomes all the greater. To this extent, this model is even weaker than the first one.

In practice too this model turns out to be weaker than the Simple Prediction Model, as has been pointed out by J. Schachter (1974). The weakness arises from the fact that this model bases itself on error data alone and consequently fails to take account of the avoidance phenomenon. Schachter correctly argues that "if a student finds a particular construction in the target language difficult it is very likely that he will try to avoid producing it" (J. Schachter 1974:213). Unlike the Simple Prediction Model, the EA model (or the CA aposteriori model as Schachter calls it) is not neutral between comprehension and production but is based on production only. As a result, at least in those parts of language where avoidance is possible (it is not possible in phonology), the coverage of errors is confined to those occurring in production only while errors of comprehension are left undetected.

The third model, which I call the Simple Explanation Model (Fig. 3) is the one I have criticized above as being a discovery procedure in theoretical linguistics rather than a serious model of CA. I find it advocated most clearly and strongly in Van Buren (1974) and suggested in Dirven (1976). Explanation here completely supplants the goal of prediction. It is almost identical with the Simple Prediction Model, and equally linear, with the difference lying in the output. As I have already discussed this view earlier, I shall here do no more than presenting the model in a diagrammatic form:

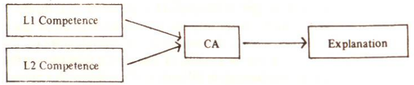

Fig. 3: The Simple Explanation Model

The last model, which I call the Explanatory Prediction Model, is favoured, implicitly or explicitly, in most recent discussions of error analysis (e.g. Corder 1967, 1971, 1973; Selinker 1972; James 1971; Hamp 1968) and contrastive analysis (Nemser 1975), but a clear and comparative statement of the methodological issues involved is not yet available. The input in this model (see Fig. 4) are the learner's performance data, both error and non-error. This makes it different from the Error Analysis Model and also precludes the distortions arising from the production bias of that model. A further difference is that the output in this model, as in the classical CA or Simple Prediction Model, is in the form of predictions about potential performance rather than explanations. At the same time, however, it is different from the classical CA model in the role it assigns to contrastive study. The role of CA in this model is neither to provide direct explanations for the errors nor to make immediate predictions about potential error performance but to contribute to the production of a model of the learner's transitional competence. The specific role of the theory of CA in the overall framework of a theory of SLL is to provide the necessary theoretical wherewithal whereby the L1 and L2 competence systems can be seen to contribute to the learner's transitional competence. The other components of the SLL theory take care of non-error performance and error performance not due to contrastive reasons. Language acquisition universals help us decide on the areas where no errors are to be expected while *et cetera* provides scope for cognitive processes of the kind, now being widely studied, that account for the non-contrastive variety of systematic errors:[5]

5 It should be noted that this model also incorporates what Nemser (1975) and his followers (e.g. Varga 1977) have been arguing for. Predictions of error based on contrastive analysis alone, they argue, are likely to be proved false more often than predictions which also take account of developmental factors, the learner's creative strategies, and his occasional compulsion to meet the communicative needs. Nemser proposes the hypothesis that learners in the process of language learning develop transitional "approximative" systems, structurally independent of their base and target languages. These approximative systems are actually Corder's transitional competence divested of its psychological garb. What is proposed here is that the theory of SLL must accomodate CA as well as these other factors that Nemser and Corder have emphasized since the learner's transitional competence (or approximative system) draws upon all these resources and does not confine itself to CA alone. It also naturally follows that the particular contrastive analysis that is relevant is not the one made by the theoretical linguist but the one that results from the learner oriented approach of the applied linguist.

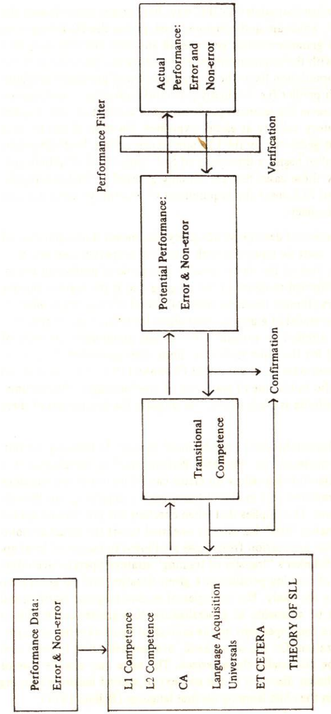

Fig. 4: The Explanatory Prediction Model

The theory of SLL, when formulated in this way, has certain parallels with the linguistic theory of TGG, which are quite obvious. First, just as the TG theory fixes as its goal production of grammars which generate all and only the sentences of a language in accordance with the requirements of explanatory adequacy, so does the theory of SLL in this formulation fix as the goal production of grammars of transitional competence which predict (i.e. explicitly generate) both error as well as non-error forms likely to occur in the learner's performance in accordance with parallel requirements of explanatory validity as earlier explained. An error is defined as a deviation from the forms generated by the L2 competence system. Such deviations are predicted on a controlled basis by the theory of SLL. Only if the deviations predicted cover all and only those cases that are actually attested in the learner's performance can a theory of SLL meet the requirement of descriptive adequacy and aspire for explanatory adequacy.

Secondly, the requirement of descriptive adequacy also means that a grammar of transitional competence must be equipped with recursive properties like any such grammar of competence. Just as the native speaker is capable of producing and interpreting any new, wellformed sentence in the language, so is the learner capable of committing new errors (though not new *types* of errors) of production and perception. In both cases the model of grammar must allow for infinite generation by a finite set of rules. Any attempt to provide a finite state grammatical account of learner's errors must fail for the same reasons as finite state grammars for natural languages. Further, the methodological point that Chomsky (1957:23) makes about grammars of languages also holds true of grammars of interlanguages: "the assumption that languages are infinite is made in order to simplify the description of these languages."

One important qualification however must be made explicit. In labelling the output of "Transitional Competence" as "Potential Performance" in the absence of a better description, the intention is to allow for errors caused by purely performance factors. These are not predicted by a model of transitional competence but they do occur in actual performance. This implies that when checking the predictions against actual data, the "performance filter" has already operated to sift the errors attributable to factors of universal application from those attributed to factors of local application. For instance, Selinker's "transfer of training" strategy operates according to the specific features of training peculiar to a given situation and no generalizations of universal validity are likely. His strategies of second language learning and communication seem to be amenable to generalization to a greater extent but as conceived by Selinker are too dependent on the individual's past experience to permit universal characterization. On the other hand, overgeneralization follows concrete ways which may be universally characterized. The same may also be true of simplification, which Selinker discusses in the context of second language learning but which also operates in the child learning his first language (Bellugi 1971).

The central feature of this model, which sets it apart from the others, is represented by the returning arrows. They indicate that the validity of a particular theory of second language learning, and of the models of transitional competence which it produces, depends on our observational verification of the performance predicted by the model, in particular, the error performance. If the performance is verified, the grammars as well as the theory meet the requirement of observational adequacy; if further the grammars are also generative, both also meet the requirement of descriptive adequacy and we can raise the question of explanatory adequacy in a meaningful sense.

Even the fact that it is possible to raise these questions in the context of SLL and CA must be considered adequate justification for the Explanatory Prediction Model at present. But when we consider that this model actually incorporates the other two serious models, those of CA and EA, thus bringing the CA vs. EA controversy to an integrated solution, its claims for serious consideration by applied linguists become indisputable. It is perhaps not unreasonable to conclude that it was some such solution that Schachter had in mind when she argued that "only by a combination of approaches, say CA apriori predictions, error analysis, and comprehension testing, will we begin to amass some reasonably unassailable information on what the second language learning process is all about" (J. Schachter 1974:213).

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