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The contribution of second language acquisition research

Over the last 25 years, second language acquisition research has made considerable progress in more than one respect. Still, we are very far from proving a solid basis to foreign language teaching, or from a general theory of second language acquisition. What is even more distressing, however, is the fact that our status within the various linguistic disciplines is still very low. It is argued this has not so very much to do with low empirical or theoretical standards in our field - in this regard, SLA research is fully competitive - but with a particular perspective on the acquisition process: the learner's utterances are seen as deviation from a certain target instead as genuine manifestations of his or her underlying language capacity. They are analysed in terms of what they not are, rather than in terms of what they are. There are aspects of our work under which such a "target deviation perspective" is very sensible. But it will not help us to make a substantial and independent contribution to a deeper understanding of structure and function of the human language faculty. Therefore, whatever our findings are, they will remain of limited interest to other linguists until we consider learner varieties to be a normal, in fact, the typical manifestation of this unique capacity with which nature has endowed us.

I've discovered that it is not difficult at all to learn French; where we say "cup", they say "tasse"; and so is it with the other words, too.

Unknown learner

1. Introduction

As one looks back into the development of our field in the last 25 years, one cannot but be impressed how much progress has been made in many important respects. Perhaps the clearest case is second language acquisition by everyday interaction, a subarea about which hardly anything beyond anecdotal evidence was known in the early seventies. In classroom acquisition, research could build on a certain stock of knowledge; but in the course of these years, this stock was enormously enlarged in almost all domains of language, from phonology to syntax, from the lexicon to communicative behaviour. On the more theoretical side, many insights from general linguistics have made their way into our field, and although not everyone may share in this line of thought, there is no doubt that it has considerably

changed and sharpened our perception. Methodologically, we normally meet, and sometimes - for example in clean statistical analysis - surpass, the standards of empirical research in other fields devoted to the investigation of language. It is not accidental that there are more and more good textbooks, and these are more and more comprehensive. It may be an exaggeration, but if so, it is only a mild one, if we say that a new discipline has been established. There is reason to be proud.

This is a good feeling. But since, as Immanuel Kant put it, the human mind suffers from the peculiar fate of being permanently haunted by questions which it cannot answer properly, it might, once in a while, also be haunted by questions such as, for example, the following ones:

(a) Has, as a result of all of these achievements, our work provided a solid basis to foreign language teaching?

(b) How close have we come to a general theory of second language acquisition?

(c) What is the status of SLA research within the various linguistic disciplines?

Our endeavour is research: we want to discover the principles according to which people who already master a language acquire another language; this is a theoretical, not a practical aim. But concerns of foreign language teaching were at the very beginning of our field, and therefore, the first question appears to be a very legitimate one. I am afraid, there is little doubt about the answer. Alerted by this insight, one might ask whether we at least made substantial progress in this direction. This depends on what is understood by "substantial"; but on the whole, I am not convinced that the answer is positive, either. In general, foreign language teachers are very interested in SLA research; in fact, a great deal of SLA work is carried out by researchers who have or had practical teaching experience. But does this fact have more than occasional and declamatory repercussions in the everyday practice of instruction? Among the many theories propagated in our field over the last 25 years, Monitor Theory is probably closest to concrete application, and in fact, it has found considerable resonance in the world of education. To which extent did Monitor Theory really change the preparation of course material, or the way in which this material is presented and processed in the classroom? I suspect the answer is not very flattering to our discipline.

But as was already said, our aim is not primarily a practical one: we want to discover the underlying principles of SLA; what we are aiming for is a theory of SLA, based on solid empirical findings. Is there any such theory in sight? I am sure that some might now get up and raise their finger and say "Yes, MY theory". They should sit down again and think for a moment about the many acquisitional phenomena that must be accounted for - from vocabulary learning to pronunciation, from syntax to interactive behaviour. The truth is simply that, while many theories have been advocated, not one of those has even remotely been accepted by the scientific community, and for good reasons. At best some specific phenomena within the wide field of SLA, for example some selected syntactical or morphological constructions, have found a general and reliable explanation, and even this is highly arguable.

Turning now to the third question, I am afraid we must simply face the fact that among the various disciplines that investigate the manifold manifestations of the human language capacity, the study of how people acquire a second language does not rank very high. This is hardly ever explicitly stated; there is some politeness in academia. But the facts leave little doubt. Second language researchers often cite work from theoretical linguistics or from

psycholinguistics; the opposite is hardly ever the case. We like to invite people from other linguistic disciplines to our conferences; this is normally not matched by invitations in the other direction. Our findings, our theoretical considerations are normally not considered to be crucial arguments in other domains of language studies; in that regard, the impact of first language acquisition research is different. There are exceptions, of course, and they are gratefully noted; but they are rare; on the whole, second language researchers are bottom dwellers in the language sciences.

These considerations suggest a gloomy picture. But this picture is as one-sided as the glorious picture which one gets when looking over the undeniable achievements over the last 25 years. The truth is simply that considerable progress has been made; it is just not enough. In the next three sections, I will have a closer look at the present situation and why it is as it is.

2. What can SLA research contribute to the understanding of the human language faculty?

I see another language as distorted English, and then, I try to work it out.

Well-known linguist

2.1 Our status within the linguistic sciences

Why are we at the bottom end? It could be, of course, that this simply results from irrational but firm caste prejudices against the newcomer. To the extent to which this is the case, there is little hope to fight against them. But although prejudices are not completely absent from the academic world, it is perhaps too easy an excuse as to assign our present status to the irrationality of the other inhabitants of this world. If we really want to climb us some steps on the ladder, we better look for more realistic explanations. There is one obvious candidate. It may well be that the empirical and theoretical standards in SLA research do not meet the established criteria of serious scientific work. This is surely false for the empirical side, at least in comparison to what is found in other language sciences. To a native speaker of German, for example, recent work in theoretical linguistics, whatever its theoretical standard may be, is a reliable source of surprise and amusement: it is full of strong and unwarranted statements about the grammaticality or non-grammaticality of specific constructions. And given that German is one of the best-studied language in the world, with abundant descriptive grammars, dictionaries and people around to consult, one wonders whether this is much better for what is said about Warlbiri or Mohawk. No second language researcher would normally dare to make such strong claims with so little evidence. Now, theoretical linguistics is perhaps not the most serious competitor in terms of empirical reliability. But first, its low standards in this regard do not seem to have been harmful to its reputation. And second, does our empirical work score much worse when compared to, for example, typological linguistics, a field with relatively strong empirical ambitions? There are excellent typological studies, no doubt, and just as with theoretical linguistics, we can only benefit from taking them into account. But how well-founded are claims about typological universals of language? Take, for example word order universals, perhaps the best-known case. It is most

impressive to see that an author is able to say something about 500 languages - but this is still a little share of the world's languages, about 10% perhaps. It could be, of course, that these 10% are representative of the world's languages; but who would dare to say so without having had a more than casual look at the other 90%? How much time must such an author have spent on each of the 500 languages - one day, two days? It could be, of course, that there are excellent descriptions of these 500 languages, and it suffices to look up what the word order of some particular language is, just as one might look up whether this language has unaspirated stops or a word for "hell". But how many languages are really well-described? Even in the case of Latin, English, French or German - languages that have really been extensively studied -, it is extremely difficult to say what "the basic word order" is. How reliable are the available grammars of Dyrbal, Twi or Mopan in this regard? Or take a notion such as "aspect", which underlies, for example, the distinction between English *he worked* and *he was working*. This is a notorious problem for the linguist as well as for the learner; German, though historically closely related, has no progressive form, and hence, a German learner of English has normally a hard time to understand its precise meaning. So do linguists: there are endless studies, but no generally accepted analysis. If this is true for one of the most salient constructions in the best-investigated study of the world - what should one think of statements on "imperfective, progressive, non-completive aspect" in, say, Estonian or Gorontalo? One cannot but have the impression that any claim on typological universals must be based on very superficial evidence, and hence should be looked at with appropriate suspicion. This is not to belittle this kind of research, quite to the opposite: how else should one proceed in these difficult issues? But I do believe that, when compared to typological linguistics, SLA research need not hide as concerns its empirical respect standards. This does not mean that the empirical basis of our field is flawless, and every effort should be made to broaden and solidify it. But on the average, we do not fare worse in this regard than other fields of linguistics; in fact, if it comes to quantitative analysis, SLA research ranks relatively high; hence, inadequate empirical standards cannot be the reason for the low ranking of SLA research.

The situation is more ambivalent on the theoretical side, in particular since there is less agreement on what "high theoretical standards" are. I cannot see, however, that the concepts and theories that underly present-day SLA research are less well defined, less clear, less consistent than those of normal descriptive linguistics. In fact, the key concepts are more or less the same. There are good reasons to argue that notions such as "passive", "tense", "case role" are not particularly well defined; but this is in no way specific to SLA research. In typological linguistics, it is often not very clear what, for example, "accusative" or "subjunctive" means in a particular language, say in Guugu Yimidhirr as compared to Eipo. But this has never been detrimental to the reputation of these linguistic disciplines, and therefore, it should not be detrimental to SLA research. So, as compared to the vast majority of work done on language and languages, there is no reason to hide because of low theoretical standards. But can we live up to the scientific level of, for example, Montagovian formal semantics or Chomskyan generative grammar? Opinions may be greatly at variance here. Personally, I believe that our work, on the average, is indeed considerably beneath the level of logical semantics, with its rigidly defined concepts and systems; but in this regard, logical semantics is quite exceptional within linguistics, only comparable to the mathematical study of formal languages and maybe some areas of computational linguistics. The immediate comparison should perhaps be to research in the tradition of generative grammar, especially since ideas from this field have also played a considerable role in recent SLA research. Many feel that work in this tradition is theoretically way ahead of what we are doing. I am inclined to share in this view, although the case is perhaps less clear than it might be. In its initial phases, generative grammars were relatively rigidly defined, their formal

properties were clear, and it was comparatively easy to test the consequences of particular theoretical assumptions. Over the years, and in particular with the increasing move from specific rules to more general principles, on the one hand, and from language-specific description towards universal properties of grammar, on the other, theoretical as well as empirical statements have become increasingly fuzzy. It is not at all clear what notions such as “subject, small pro, theta role, weak feature”, to mention but a few, really mean, and whether they are used in a consistent way by different authors, or by the same author in different publications. But for the sake of the argument, let us assume that generative grammar in its most recent, minimalist version is theoretically far ahead of SLA research. Would it help us climbing up some steps in the rank order, if we rigidly adopt this, or some other, theoretically more advanced, framework?

No one really knows; but I do not believe it. First, numerous attempts in the past ten years have been made in this direction; but one cannot say that they have found strong repercussions in other areas of linguistics; no theoretical linguist has ever changed his or her views because of some findings from SLA research; at best, he or she would say that such findings corroborate these views; but even this is hardly ever found. Second, development in theoretical linguistics is fast, and as soon as some version has found its way into empirical work on language acquisition, it is outdated in its own field; work on “parameter-setting” is a good example. Third, it is hard to apply this framework to some of the central acquisitional phenomena, say vocabulary learning or problems with the use of tense forms. Theoretical linguistics in this sense is confined to some highly selective morphological and syntactical properties. But the fourth and main reason is this: I do not believe that our low ranking is fundamentally connected to our empirical or theoretical standards. As was said above, these could surely be improved, but on the average, they are not worse than in other fields of language studies. The main reason is that we have nothing interesting to say to people in these fields.

Why should the analysis of the odd productions of the second language learner, this distorted, flawed, ridiculous, chaotic mimicking of “real language”, be able to tell us something new, something principled, something fundamental about function and structure of a particular language, about the human languages in general, about the very nature of the human language faculty? No matter how much we improve on our theories and our empirical work - it will not change very much, so long as we do not demonstrate that we are able to make an independent, a genuine, a substantial contribution to the study of the human language faculty.

There is no real reason why the investigation of an activity so common as the acquisition of a second language should not be able to make such a contribution. The fact that it hasn't, or at least that it is not seen this way in the academic world, results to my mind from a particular way of looking at language acquisition. This view, to be discussed in the next section, is a consequence, first, of the fact that our field has its primary origin in practical problems of language teaching and, second, of a particular perspective on the object of linguistic investigation in general. These two points are closely interconnected to each other.

2.2. Two views on SLA research

As so many other disciplines, in fact, as any scientific endeavour, the study of second language acquisition has its origin in practical concerns - in problems of second language teaching. This origin has naturally led to a particular view on SLA. Two assumptions are constitutive of this view:

A. There is a well-defined target of the acquisition process - the language to be learned. This "target language", as any "real language", is a clearly fixed entity, a structurally and functionally balanced system, mastered by those who have learned it in childhood, and more or less correctly described in grammars and dictionaries.

B. Learners miss this target at varying degrees and in varying respects - they make errors in production as well as in comprehension, because they lack the appropriate knowledge or skills.

I shall call this view the **TARGET DEVIATION PERSPECTIVE**. It is the teacher's task to erase or at least to minimise the deviations; it is the researcher's task to investigate which "errors" occur when and for which reasons. As a consequence, the learner's performance in production or comprehension is not so very much studied in its own right, as a manifestation of his or her learner capacity, but in relation to a set norm; not in terms of what the learner does but in terms of what he or she fails to do. The learner's utterances at some time during the process of acquisition are considered to be more or less successful attempts to reproduce the structural properties of target language utterances. The learner tries to do what the mature speaker does, but does it less well.

There are three reasons which make the target deviation perspective so natural and attractive, in fact, almost self-evident. The first of those was already mentioned: it is the natural perspective of the language teacher: language teaching is a normative process, and the teacher is responsible for moving the student as closely to some norm as possible. And if the student misses this norm in one or the other way, this must be changed. Nothing could be more sensible.

Second, it is also the natural perspective of all of those who had to learn a second language in the classroom - and that means, of practically every language researcher. It is very difficult to get rid of the perspective which the teacher's red ink burned into our mind: there is language to be learned, it is very well defined, and you missed it. I believe that this normative experience has also deeply shaped the way in which linguists usually perceive the object of their efforts - a point to which I will return in the next section.

Third, the target deviation perspective provides the researcher with a simple and clear design for empirical work. There is a yardstick against which the learner's production and comprehension can be measured: the target language, or actually what grammar books and dictionaries say about it. What is measured are the differences between what the learner does and what the set norm demands. As a rule, therefore, the research design is a - much subtler and often highly refined - elaboration of the 'red ink method': errors are marked, counted, and statistically analysed. One may count, for example, how often Spanish and French learners of English omit the subject pronoun in classroom tests, and if there is a significant difference, then - everything else being equal -, this may be attributed to the influence of the first language. Alternatively, one might also look at the individual error and try to analyse how it came about, that is, quantitative analysis and hypothesis testing can be replaced or complemented by more qualitative approaches. All of these methods are well-established in the sciences, there are certain standards in their application, and when these standards are met - and usually, they are met in SLA research -, there is not the least methodological objection. But this analysis, no matter how well it is done, does not inform us about what the human language faculty does; it tells us to which extent and perhaps why it differs sometimes from a certain norm. At the very best, it tells us where and why our species-specific capacity to learn and to process languages does not work under particular circumstances; but it does not tell us much about its structural and functional properties. And therefore, people who want to

understand this faculty and its specific manifestations do not find these results relevant to their concern.

The alternative to the target deviation perspective is to understand the learner's performance at a given time as an immediate manifestation of his or her capacity to speak and to understand: form and function of these utterances are governed by principles, and these principles are those characteristic of the human language faculty. Early attempts in this direction are reflected in notions such as "interlanguage" (Selinker), "approximate systems" (Nemser), and related ones. But these notions are still based on the assumption that there is "the real thing" - the target language and, similarly, the source language -, and there are systems in-between, or systems that only miss the "real thing" just a bit. The view which I have in mind - **THE LEARNER VARIETY PERSPECTIVE** - is somewhat more radical. It goes back to early attempts to analyse the language of adult foreign workers in Germany (Klein and Dittmar 1979); much the same idea is found in Bley-Vroman (1983). The learner variety perspective can be characterised by three key assumptions (Klein and Perdue 1997: 307s):

A. During the acquisitional process, the learner passes through a series of **LEARNER VARIETIES**. Both the internal organisation of each variety at a given time as well as the transition from one variety to the next are essentially systematic in nature.

B. There is a limited set of organisational principles of different kinds which are present in **ALL** learner varieties. The actual structure of an utterance in a learner variety is determined by a particular interaction of these principles. The kind of interaction may vary, depending on various factors, such as the learner's source language. With successive input analysis, the interaction changes over time. For example, picking up some component of noun morphology from the input may cause the learner to modify the weight of other factors to mark argument status. From this perspective, learning a new feature is not adding a new piece to a puzzle which the learner has to put together. Rather, it entails a sometimes minimal, sometimes substantial reorganisation of the whole variety, where the balance of the various factors successively approaches the balance characteristic of the target language.

C. Under this perspective, learner varieties are not imperfect imitations of a "real language" - the target language - but systems in their own right, error-free by definition, and characterised by a particular lexical repertoire and by a particular interaction of organisational principles. Fully developed languages, such as English, German, French, are special cases of learner varieties. They represent a relatively stable state of language acquisition - that state where the learner stops learning because there is no difference between his variety and the input - the variety of his social environment.

In other words, the process of language acquisition, and of SLA in particular, is not to be characterised in terms of errors and deviations, but in terms of the two-fold systematicity which it exhibits: the inherent systematicity of a learner variety at a given time, and the way in which such a learner variety evolves into another one. If we want to understand the acquisitional process, we must try to uncover this two-fold systematicity, rather than look at how and why a learner misses the target.

I do not believe that it is uninteresting or unimportant to investigate errors and deviations, quite to the opposite. Such an investigation tells us a lot about the problems of the learner, their causes and how they can possibly be avoided. No reasonable person can take this to be irrelevant. But it will never lead us to a real understanding of how the human faculty works when exposed to new input - that is, it will never lead us to a real understanding how language acquisition, and second language acquisition in particular, functions. This is the reason why the answer to the second question from section 1 is negative. Nor will it tell us something substantial about the nature of the human language faculty itself. This, I believe, is the reason why our work ranks low within the linguistic sciences. Maybe we do find out something, maybe our findings are even reliable and of practical importance - but they contribute little to the general aim of linguistics.

But are we able to make such a contribution without leaving our field proper? After all, we are concerned with learners and what they do with their language faculty - that is, we are concerned with learner varieties, very elementary ones and very advanced ones; as soon as the target language is reached, our job is done. But can the investigation of learner varieties constitute a substantial, a fundamental contribution to the investigation of the human language faculty? I believe the answer is yes, in fact, I believe that learner varieties are the normal manifestation of this capacity, and “real languages” are just a special case, defined only on social and normative rather than on structural grounds. This is surely not the common way in which language researchers or the person on the street would see this; therefore, it needs some explanation.

2.3. The “real language hoax”, or: Learner varieties are the normal case

There are many ways to look at language, and linguistic thought in the 20th century in particular is anything but uniform. Thus, the two dominant currents in this century, classical structuralism and generative grammar, have defined the primary object of their efforts in somewhat different ways. The following two famous passages, though somewhat simplifying the position of their authors, illustrate the point:

La linguistique a pour unique et véritable object la langue envisagée en elle-même et pour elle-même. (Saussure 1916: 317)

Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shift of attention and interest, and errors (randomly or characteristic) in applying his knowledge of the language in actual performance. (Chomsky 1965: 3)

In Saussure’s view, the object to be investigated is a social entity, a “fait social” - a system which is defined by inherent structural relations between its elements; there is nothing specific to the individual speaker in this system. In Chomsky’s view, the object of investigation is an individual entity - the knowledge which the ideal speaker has of his or her language; there is also a social dimension, but it is not felt to be of primary importance. In actual fact, however, the difference is much smaller than it looks like. Under both views, the object of investigation is an ideal entity. Neither Saussure nor Chomsky would deny that there is social variation, that is, that speakers speak in different ways; but they abstract away from this variation. Saussure would surely not say that “la langue” exists anywhere and

anyhow except by virtue of the fact that its speakers know it; where should it exist? Any “fait social”, religious convictions, values, norms, they all are nothing but belief systems in the head of people. Similarly, the knowledge of the ideal speaker must be knowledge of something - the language of some community, *une langue*, and this knowledge must not be incomplete, or imperfect, or false. It must be the perfect reproduction of some external language.

Saussure, Chomsky, all of us are used to take perfect mastery of a language to be the crucial case, and the linguistic knowledge of a perfect speaker - a speaker who masters a “real language” up to perfection - to be the primary object of the linguist’s efforts. But what does it mean that a speaker masters a language perfectly well, what must his or her knowledge be like in order to qualify as native? Our common *façon de parler* in these matters somehow implies that there are such entities as “real, fully-fledged languages”, such as English, Latin or Kilivila, and speakers ‘know’ them to a higher or lesser degree. But this is a myth. Neither is there a structurally well-defined “external language”, a point that has been repeatedly made by sociolinguists as well as by theoretical linguists. Nor is, a fortiori, the perfect internal representation of such a structurally well-defined and stable entity the normal case. The fact that it is a myth is clear to everybody who ever tried to answer the most frequent question posed to the linguist (‘How many languages do you speak?’) or the second-most frequent question (‘How many languages are there on earth?’). I always say ‘five thousand’ (to the second question), and I have found that the only person who is not happy with this answer is myself - because I know that there is no clearly-shaped and well-defined entity such as ‘a language’, let alone five thousand. The honest scholar feels obliged to explain that there are no clear borderlines, that there are many dialects, registers, that it is arbitrary whether we count Frisian and Dutch, Dutch and Standard German, Standard German and Swiss German as variants of one and the same language or not, that ‘a language’ is a dialect with an army and a navy, etc etc. No layman wants to hear this, and understandably so. Most linguists don’t want to hear it, either, and this is not understandable.

There are five thousand languages on earth. There are 193 countries on earth. This means that there are - on the average - 26 languages per country, with a range between 1 and several hundred. Two semesters of statistical training inform the linguist that this does not necessarily mean that every inhabitant of a country speaks 26 languages (on the average). Multilingualism of a country does transfer to the multilingualism of its inhabitants. But it would be equally silly to conclude that monolingualism is the normal case. THE NORMAL CASE IS THAT A PERSON HAS VARYING KNOWLEDGE OF DIFFERENT LANGUAGES. That is a good way to state the facts for the layman who believes that there are well-defined entities called ‘languages’. But there aren’t. A “real language” is a normative fiction. What really happens is that human beings, equipped with this species-specific mental capacity called human language faculty, manage to copy, with varying degrees of success, the ways in which other people speak. They develop learner varieties - one, two, many. In some cases, they push this process to a degree where their own competence to speak and to understand does not saliently differ from that of their social environment (or, perhaps, a special group within their social environment, like school teachers). Then, we speak of “perfect mastery”. But this perfect mastery is just mastery of a special case of a learner variety. It reflects that case in which neither the learner nor his neighbours notice any difference, or at least no difference they would consider to be of particular SOCIAL importance. It is not the cognitive representation of something - “a real language” - that is fundamentally different from the representation of other learner varieties.

Normally, the speaker’s language faculty also allows him or her to develop and to store many different learner varieties at the same time. All of those are manifestations of the human language faculty; their investigation can not only inform us about the nature of the

acquisition process but also about the nature of the human language faculty itself. They do not enjoy the same social and normative reputation as a “real language”. But this does not mean that “a real language” is more important for an understanding of the human language capacity than other learner varieties that are “less perfect”. Since this perspective is surely not the received one, I will consider it in some more detail.

2.4 Can learner varieties really tell us something about the human language capacity?

Der Sineser hat sich durch seine steife Einsilbigkeit den Weg zu aller weitem Kultur verschlossen; aber die Sprache des Huronen oder Grönländers hat alles in sich, sich zu der Sprache eines Plato oder Voltaire zu erheben. [The Chinese, by his stiff monosyllabicity, has precluded himself from any further culture; but the language of the Greenlander and the Huron has any chance to rise to the language of a Plato or Voltaire] (Adelung, 1806: XXV)

We laugh at Adelung’s odd idea that a language should have a rich morphological structure in order to qualify as a serious language. But one wonders whether the common view to look at “real languages” is so far from a perspective, which sees the learner’s way to express him- or herself as a highly imperfect manifestation of the our innate capacity to learn and to use “a language”, a manifestation that is restricted, flawed, poor in its structural and lexical possibilities, and hence, simply not of particular interest to anyone who wants to understand the nature of the human language capacity. I believe this view is understandable but wrong, and this for at least two reasons. First, even if learner varieties are taken to be imperfect manifestations, then this still does not mean that their study cannot be highly instructive for an understanding of the underlying capacity. To deny this fact would be as ridiculous as the idea, biology should not deal with more elementary forms of life, such as bacteria, molluscs, or the humble *drosophila melanogaster*, and only devote its attention to life in its most advanced, in its most complex manifestations, for example the gentle tiger or the human being. In fact, it is just the study of elementary forms which has advanced biology to its present rank within the sciences; these forms do not show everything that is possible in the evolution of living organisms; but they are more transparent in what their structural properties are.

Second, learner varieties vary considerably; after all, they reflect a transition of simple to very complex forms; hence, they also differ in what in particular they can tell us about the human language faculty. There is no reason to assume that the variety of a very advanced learner who speaks German with a strong accent, distinct traces of English word order, wrong choice of prepositions and without any case marking on nouns is less perfect a manifestation of the human language capacity as Standard German, as spoken by the native speakers of Standard German, or the more educated among them. This learner’s variety of German is just not as the German grammar books want to have it, and as the indigenous population speaks if it speaks as the grammar wants to have it. It is “imperfect” because it deviates from a norm; this norm can be set by a descriptive grammar, or by the habits of some social group. The fact that this variety is “imperfect” has nothing to do with the nature of the human language capacity; after all, German could be like this learner variety (maybe it should).

The case is different for very elementary learner varieties - say the variety of a learner who has just arrived in a country and knows nothing but a few nouns, a verb or two, and some rote

forms. The investigation of such a learner's production may be not very telling; most of the potential with which the human language capacity has endowed us cannot apply. Still - some of its properties may be visible even at this elementary level. One might ask, for example, what happens if the learner tries to put two words together - is this done completely at random, does he or she follow the principles of the first language, or are there some universal constraints? And it becomes more and more instructive, the complexer and richer the learner variety gets. One such case will now be discussed in some more detail.

In a large crosslinguistic and longitudinal research project, we examined how 40 adult learners picked up the language of their social environment by everyday communication (Perdue 1993). Their production was regularly registered and analysed over about 30 months. This production, and the way in which evolves, varies in many respects; but it also shows a number of striking similarities. One of the core findings is the existence of a special language form which we called the "Basic Variety". It was developed and used by all learners, independent of source language and target language; about one third of our learners also fossilised at this level, i.e., they learned more words, but they did not complexify their utterances in other respects, in particular in morphology or syntax. As any form of human language, the Basic Variety has a lexicon, i.e., a repertoire of minimal meaningful expressions, and compositional rules, i.e., rules which allow the speaker to construct more complex expressions from simpler ones. In the Basic Variety, the lexical items mostly stem from the target language. They are uninflected, and although there is occasional variation in form, this variation is not accompanied by functional variation; in other words, there is no functional morphology. By far most lexical items correspond to nouns, verbs and adverbs; closed class items, in particular determiners, subordinating elements, prepositions, are rare, if present at all.

We noted three types of rules according to which these lexical items are combined into larger units:

A. Phrasal; they have to do with the lexical category (noun, verb, etc); if a verb - i.e., the uninflected verb stem - governs two arguments, then it is normally placed between these arguments.

B. Semantical: they relate to semantic properties of the arguments; thus, the argument which exerts the strongest control over the situation, is normally placed first.

C. Pragmatical: they relate to specific pragmatic functions, such as topic-focus structure, the introduction and maintenance of informations etc; in the basic variety, the element which is in focus is regularly in last position.

In the production of a concrete utterance, these organisational principles interact; normally, they go hand in hand; but sometimes, they also get into conflict, and these conflicts turn out to be germs of further elaboration. I shall not work out this here (see Klein and Perdue 1997 for a detailed presentation, and the papers by Bierwisch, Comrie, Schwartz and Meisel in the same issue of "Second Language Research" for a critical discussion).

In the present context, two facts about the Basic Variety are particularly remarkable. First, ALL speakers in our sample use it at some time - in fact, many stop at this level. If this is correct in principle, then the properties of the Basic Variety cannot be derived from the particular languages involved, except for the choice of lexical items - it must somehow reflect principles dictated by the human language faculty. Second, it is highly efficient for communicative purposes. If there is a communicative problem, then it is usually due to a lexical gap, rather than to the absence of a particular morphosyntactical feature. Now, note that there is no functional inflection whatsoever. This means that there is no tense, no aspect, no mood, no agreement, no case marking, no gender assignment; nor are there, for example,

any expletive elements. Still, people tell, for example, very complex stories, just by clever use of some adverbials, some particles with temporal meaning, and pragmatical principles. In other words, a great deal of what we, with Adelung, feel so constitutive for “a real language”, and what constitutes a great deal of classroom acquisition, is simply absent - but it does not seem to matter so much. And after all, there are languages such as Mandarin Chinese which, same traces aside, also lack functional inflection, and it does not seem to harm its speakers.

Two lessons are to be drawn from this. First, it may well be that we totally overrate the role of particular structural properties of “real languages”. Sure, German has this complex system of noun inflection (“I rather decline two beers than a single German noun”, Mark Twain), and Spanish has a complex system of verb forms, and Sanskrit has a complex system of everything. But the existence of such features is in no way a constitutive trait of the human language faculty. Second, one wonders why some manifestations of the human language capacity have these, and other, complexifications, whereas others don’t. Where and why are they necessary, where are they just decorum, faithfully traded down from one generation to the next without any deeper reason, highly esteemed by linguists, utterly detested by second language learners? It is the study of learner varieties, of their internal systematicity and of their systematical development over time which allows us to address and to eventually to answer these questions. It is second language acquisition research which allows us to get a more realistic picture of what is essential and of what is peripheral in the human language capacity.

3. Conclusion

This is just a theory. But I need facts.
From a detective novel

In the introduction, three questions concerning the present state of SLA research were raised: what have we achieved for language teaching, how close are we to a theory of second language acquisition, what is our status within the chorus of disciplines that deal with language? To all of these, the answer was quite skeptical. In section 2, I tried to explain why our work had so little impact on linguistics in general. The reason, I argued, is simply that so far, we have nothing of real interest to contribute to a deeper understanding of human language. We must consider learner varieties as primary manifestations of our innate language faculty, no less important than so-called “real languages”. They are not just bad copies of some target, from which they deviate to varying extent, but as objects in their own right; real languages are just a special case - that case in which a learner variety does not perceivably differ from the way in which the learner’s social environment speaks; as a research object, it is not privileged in any way. In this concluding section, I will briefly return to the two other question.

No scientific endeavour can be pleased with the mere discovery of some facts, interesting as these may be: what we eventually are aiming for is a set of general principles, from which the individual observations can be deduced. What we want is “a theory”. Now, this term can be understood in various ways, and more often than not, it is just a label for a collection of more or less well motivated speculations. In this sense, there are many theories of second language acquisition. If the term is meant to be more than “just a theory”, it is fair to say that we do not have a theory of second language acquisition. Is the learner variety perspective, defended in the preceding section, such a theory, or is it at least close to such a

theory? The answer is “no”, for two reasons. First, it is just a way of looking at a range of linguistic phenomena. It is an approach which eventually leads us to a deeper understanding of the acquisition process and of the human language faculty. But as such, it does not state strong general principles which cover all known evidence and correctly predict new findings, as a serious theory, say the theory of gravity or quantum mechanics, does. Second, it seems a wrong ambition to look for a comprehensive theory of second language acquisition. The phenomena to be covered are simply too divergent. It would be like the quest for “a theory of nature”. Someone who sets out to learn a new language has to acquire all sorts of new knowledge and skills. Suppose you are in a German pub, you just had a beer and you want another beer, then the most straightforward and therefore the best way to express this is “Noch ein Bier, bitte!” You may learn this as a rote form, a particular useful one in this case. But if you want it to be part of your productive competence, if you want to do this in the same way in which a native speaker does it, then you must learn a wealth of things; you must learn

- the sound-meaning coupling of these four words
- you must learn that “noch” does not mean “still” in this case, but something like “another one”
- you must learn to pronounce the long vowel in “Bier” without any diphthongisation, as normally done by speakers with English as a source language
- you must learn to omit some parts of the underlying full expression (it is an elliptical construction)
- you must learn to mark the accusative - something that is simple in this case, because it is identical with the nominative, but more difficult if you happen to order a wine (where you have to say “einen Wein” rather than “ein Wein” in this context)
- you must learn to use “bitte” appropriately (in German, it is used as a first turn, but also as a third turn, in response to “thank you”)

and so on and so on. A great deal of this knowledge concerns entirely peripheral properties of German. There is no deeper reason why /i:/ is not slightly diphthongised in German (except in some northern dialects), or why the final /t/ in “Bier” is usually vocalised, rather than, for example, flapped or retroflex. These are just things you have to learn piece for piece. Such a piecemeal learning may also obey some general rules; but if this should be the case, the underlying principles are surely not the same as those which tell you what the precise range of usages of some lexical item is, or which elements you can omit in an elliptical utterance, or when you should say “bitte” and when you better don’t. If one really wants to understand what happens, and what ought to happen, when people learn a language, all of this must be investigated. But I do not believe that there can be a universal, meaningful theory of the entire processes that happen when someone learns a language. Hence, it seems pointless to strive for “a theory of second language acquisition”. No such theory is possible if it is not to become void of content and hence uninteresting. This does not mean, however, that the aim of our efforts should be just a listing of facts. What is needed, therefore, are **PARTIAL THEORIES**, that is, theories which state the principles behind what happens in particular areas of knowledge acquisition; and we may hope that one day, some of these partial theories converge without losing their empirical content.

When I said that a great deal, if not most, of what has to be learned concerns “peripheral properties”, this does not mean that these properties are unimportant; they are just not essential to the understanding of the human language capacity. Whether the German word “Getränk” covers alcoholic as well as non-alcoholic beverages, whether the voiceless stops in this word are aspirated or not, whether it is “der Getränk, das Getränk, die Getränk” -

all of this does not matter when we want to understand the principles which underly function and structure of human language. But it is utterly important if you want to speak German like a native speaker, or as some norm wants to have it. Then, you must precisely copy these features, funny and idiosyncratic as they may be, because deviation is punished. It should make us think that is just these features which seem most difficult for adult learners, in contrast to children; the age effect in language learning is essentially observed for peripheral properties. In any event - this is the point where the "target deviation perspective" has its legitimate place. Therefore, I do not believe that the two perspectives contrasted in section 2.1 are mutually exclusive. No perfect replication is possible without taking something for the norm; and if we want to understand why some learners miss this norm in certain ways, then we must study their errors; and the better we have understood the reasons of their errors, the better we can systematically intervene in the learning process. But this perspective tells us little about the nature of the human language capacity, and it tells us little about the principles of acquisition. To this end, the learner's production and comprehension must be analysed in their own right: learner varieties must be seen as independent, as normal manifestations of the human language faculty, and we are the ones that study them and uncover the principles that determine their structure and their function.

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