CONTACT, CHANGE AND THE PASSAGE FROM ALLOMORPHY TO SUPPLETION*

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ABSTRACT

'Non-native' words that do not follow the phonological rules posited strictly on the basis of the native elements of a language often pose tremendous analytic dilemma. They are generally excluded from analyses: rarely are they considered as modifying the phonological system of the borrowing language. Turkish has borrowed immensely from non-harmonic languages, but it is still analyzed as respecting its former harmonic constraints. I will show that even if the question is well acknowledged in the literature, problems arise at many levels of analysis because of specific views that are generally shared on the architecture of the language faculty. I will first claim that the synchronic data suggest that we must suppose at least co-existing allomorphic forms in the lexicon (suppletion) because of the reorganization of the phonological system that occurred following borrowing. I claim that the so-called 'harmonic' allomorphy is at least morphologically conditioned and that the allomorphs are stored. But I will also show that even an analysis in terms of suppletion is not enough to handle the data: a word-based morphology is necessary to properly take care of the facts; models based on morphemes and lexemes can not do so.

Key words: Phonology, Morphology, Allomorphy, Suppletion, Turkish, Language change

1. INTRODUCTION

'Non-native' words that do not follow the phonological rules posited strictly on the basis of the native elements of a language often pose tremendous analytic dilemma. They are generally rejected from analyses (or given a different analysis) and this, using the devices that are available in the theory or model with which linguists are developing their analyses. That is to say: they are rarely perceived as modifying the phonological system of the borrowing language. The purpose of this paper is to explore many of the related problems that we face when we take certain (mainstream) theoretical positions on what should or should not be considered phonological processes in a given language.

In this paper I will discuss the case of Turkish because of the alleged simplicity of its phonological and morphological processes and because Turkish borrowed immensely from non-harmonic languages[1]. But Turkish is still analyzed as respecting its former harmonic constraints.

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[1] "On possède peu de données quantifiées sur l'ampleur de l'épuration. Il est généralement admis que les mots turks représentaient entre un cinquième et un quart du stock lexical du turk ottoman, dans la langue écrite au début du XXe siècle — parfois sensiblement plus chez certains militants nationalistes, qui entendent parler « pour le

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I will show that even if the question is well acknowledged in the literature, problems arise at many levels of analysis because of specific views that we generally share on the architecture of the language faculty.

I will first claim that the synchronic data suggest that we must assume at least coexisting allomorphic forms in the lexicon, that is, suppletion, to handle the results of the reorganization of the phonological system that occurred following borrowing.[2] This means that surface allomorphy is no longer the result of phonological processes applying to deep invariant representations, as is proposed in all works dealing with vowel harmony. The fact that a restructuring of the linguistic system took place must be accepted, but the dominant perspective on the interface between phonology and morphology does not allow us to take this position. I argue that what is generally perceived as 'harmonic' allomorphy is stored and so is what Paster (2006) (and others) calls 'phonologically conditioned suppletive allomorphy'.

The second step will consist in showing that even a suppletive analysis is not sufficient to handle the data. Diachronic scenarios will be explored and I will argue that we have no choice but to accept the fact that full words (at least some of them), whether morphologically simple or complex, are stored and are not necessarily reinvented each time by taking as inputs the stock of morphemes (coexisting supplementary allomorphs) present in our lexicon. Complex words are

peuple ». La linguiste Kâmile İmer avance des chiffres. Selon elle, la part de mots turks dans la langue de la presse s'élevait à 35% en 1931, trois ans après la réforme de l'alphabet ; en 1933, à l'heure où la « mobilisation linguistique » (dil seferberliği) battait son plein, elle atteignait déjà 44 % ; et en 1946, après quinze ans d'intenses efforts non seulement pour fabriquer le turc-pur mais encore et surtout pour en imposer l'usage au sein de la classe lettrée et, plus largement, dans la population, elle s'élevait à 57 %" (Szurek 2013: 9). The actual number of 'foreign words' in Turkish ("The only language ever to approach English in its wealth of vocabulary, it attained a remarkable degree of expressiveness and grandeur" Lewis 2000:xx) is the subject of a big debate, as we see in this quote. It is tainted with so much emotions and ideology that it is hard to get a neutral perspective on the subject. The given numbers are often a good indication of the political orientation the people giving them are following (see Lewis 1999 for good examples). The ways the statistics are made are never clear. If it is counted as dictionary entries, etymologically 'foreign words' are already computed this way (origin being part of the entry) for the officially (ideologically?) accepted ones. On the other side, some dictionaries are devoted entirely to 'non-native Turkish words', permitting this way the reification of this division, its status of presupposition becoming this way a societal fact. If it is counted as tokens, I am pretty sure we get a totally different picture. When we are listening to Turkish, even when we do not understand it, we slowly get accustomed to the *şey* 'thing' and *yani* 'it means/I mean' that punctuate every sentence (two 'Arabic' words; note that the second one is 'disharmonic'). A lot of the everyday life goods (clothes, kitchen instruments, accessories, etc.) are referred to with 'foreign words'. The vocabulary of religion, of science, of art, etc. is almost totally foreign; and this, even after a reform that was intended to clear the vocabulary of 'foreign' (Arabic and Persian not Western) influence and this, even if native speakers do not know where the words they use are coming from (see Zimmer 1969's study about "the degree of awareness of a number of different morpheme structure conditions in Turkish displayed by native speakers of that language" Zimmer 1969: 309). The disharmonic part of this 'foreign' vocabulary is very important, but numbers are not available (but see Becker & al. 2011 for related topics: "Many of the lexical trends that identified in our quantitative lexicon analysis are ultimately traceable to extensive lexical borrowing from Arabic, to much the same degree that many of the lexical trends found in English phonotactics [...] are ultimately traceable to lexical borrowing from French centuries ago" (Becker et al. 2011: 62).

[2] I use the term 'Allomorphy' for any phenomena that involves *one* underlying representation and multiple surface allomorphs. I use 'Suppletion' for any phenomenon involving multiple underlying representations for the same unit. Terminology can be confusing and (because it is) theory-dependent: "some authors use the term 'allomorphy' to cover any case of variation in a morpheme's surface form (e.g. Bye 2007); others use the term 'suppletion' to refer to multiple underlying forms (cf. e.g. Spencer 1991, Embick 2010); and yet others use these terms differently again, restricting e.g. 'suppletion' to non-affixal allomorphy (e.g. Harbour 2007). Here, we eschew the term 'suppletion' and use 'allomorphy' to refer only to differences arising from the existence of multiple underlying exponents" (Bonet & Harbour 2012: 199). In what follows, I take the side of Bye for 'allomorphy' and the side of Spencer and Embick for 'suppletion'.

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stored and these complex words serve as models to generate in a productive way forms that have not been used so far by the speaker. In other words, I will claim that not only a morphology with suppletive allomorphs but a word-based one is necessary to properly handle the facts I will present. Models based on morphemes (the dominant framework) and lexemes (Anderson 1992, Aronoff 1994 and Stump 2001, for example) can not do so.

2. THE PHONOLOGY-MORPHOLOGY INTERFACE

I will begin the discussion with this uncontroversial statement:
A deduction in which the premises are false cannot be true. This applies to any form of argumentation. The biggest challenge we face is knowing which premise is true and which one is not.

If two (or more) grammatical models are compatible with the data, it is in fact very difficult to choose between them, especially if the predictions they offer are very similar. When this situation occurs, certain criteria must be proposed for the choice. Existing ones generally rely on notions such as simplicity, elegance, and so on. But that still does not tell us if the premises are correct.

English –s plural formation is a basic example often used in textbooks to show the difference between what is processed phonologically and what is processed morphologically. The question it raises is: what is the domain of this productive rule?

(1)

Sg.	Pl.
cat	*cats*
dog	*dogz*
house	*housez*

There are, mainly, three possible answers to the question of which domain is responsible for the allomorphy in (1):

1) The purely phonological approach: *-z* is the underlying form and phonology takes care of the surface allomorphy when the morpheme is added to a stem;
2) The purely exemplarist / morphological / analogical approach: what is given on the surface is what speakers store and general cognitive processes can explain how we produce new forms from what is stored. In the given case, we store singular and plural forms as wholes and apply analogically the same operation for the new plural words we want to create: after *t*, the plural is in *-s*; after *g*, the plural is *-z*, etc.;
3) Both 1 and 2 are correct and do not need to be considered as excluding each other.

The answer for the formation of the *-z* plural is pretty straightforward, i.e., not many linguists would say that it is not phonological. But it offers a good starting point for what I want to explore here.

How can we decide which solution (1-3) is right? All predict the same data. For example, in a Wug Test designed to explore the productivity of this rule, someone whose answer is 1 will see evidence that phonology is active and is extended to new cases, while someone whose answer is 2 will see evidence that analogical formation based on stored examples is an active process and is extended to new cases.

The biggest difference between 1 and 2 has to do with the issue of storage: should allomorphs be considered only as phonologically conditioned (*-z* + phonological rules); or should we

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accept that allomorphy can be stored, that is -s / -z / -ez are all input forms. Most debates on the phonology-morphology interface in fact revolve around this kind of question, although it is not always explicitly stipulated[3].

As I have said, few linguists would claim that the allomorphy described in (2) is not phonologically driven. This position is supported by universal phonetic laws that can be extrapolated from processes such as assimilation, coarticulation, neutralization, etc.: a voiced segment often takes the features of a voiceless one and this is, from a physiological point of view, very natural.

But this explanation does not logically forbid the third answer: the rule may be active, but the surface outputs of the rule may be stored as well. Nothing in the data will give us reasons for choosing between answer 1 and 3. This is, in my opinion, the starting point for Booij (2009).

In this paper, Booij examines diachronic phenomena and concludes that we must assume that speakers store morphemes with all their surface phonetic details if we want to explain how certain types of lexical levelling take place. If they were not stored, we could not explain how the results of dead phonological rules could induce lexical levelling. The scenario goes as in (2):

(2) Stage 1: Underlying representation + phonological rule = surface allomorphy
Stage 2: The phonological rule disappears from the grammar
Stage 3: The results of the dead phonological rule (allomorphy) are still there and induce lexical levelling
→ Booij concludes that we must assume that the forms that are stored in Stage 1 are stored in their surface (phonetic) form even when the rule is productive, i.e., stored with the outputs of the rule, to explain that these surface outputs become inputs for lexical levelling when in Stage 3 the original rule responsible for these outputs (allomorphy) no longer exists.

I came up with a very similar deduction independently. But Booij's argument is not the knockout argument that he thinks it is. The main problem is that Booij's scenario can be accounted for by reanalysis, a solution that does not imply that speakers have stored the surface forms at the synchronic level: lexical levelling can have taken place for the next generation based on the opacity of the encountered forms. In any case, children must start from fully specified surface forms to create their underlying phonemic representations, to develop generalizations about surface forms, and so on. They need to store surface forms with all the phonetic details. But this does not mean that once generalizations are made, the storage will not be in terms of abstract representations.

Booij defends his claim as follows:

[3] Just look at how little exists in the phonological literature for 1) isolating languages and for 2) agglutinative languages where not much allomorphy exists. For agglutinative languages, literature increases proportionally as exceptions to the phonological rules that are posited increase. The debate generally revolves around questions like: do we accept (weak) suppletion; how much can we stretch the power of our rules; how much diacritical marking can we accept, and so on. "[…] consider the continuum below:
(8) a. systematic across the entire language
b. systematic but with some exceptions (a regular rule with some exceptions)
c. systematic but only within a circumscribed environment (a 'minor' rule)
d. systematic but only within an arbitrarily listed set of cases (a 'minor' rule for a diacritically marked class)
e. wholly unsystematic.
The extremes of this continuum are uncontroversial: (8a) is the domain of phonology, (8e) is the domain of the lexicon and so of allomorphy" (Bonet & Harbour 2012: 202).

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> "It is quite clear that adult speakers also continuously subject the outputs of their language system to reanalysis, that is, there is a continuous inspection of output forms. The possibility of inspection and reanalysis presupposes that these output forms are stored: have a certain degree of permanence in memory" (Booij 2009: 491).

This is a statement that not all linguists will accept and Booij's argument crucially depends on it. And we are back to where we started: How do we know if the premises used in our deduction are correct?

As I said, Booij's scenario is compatible with reanalysis as conceived by the proponents of the point of view expressed in the answer 1 above (the phonological account which stipulates that any surface difference is to be accounted for by phonological processes). (3) is the scenario they will propose, and it is as compatible with the data (i.e. allomorphy becoming suppletion through the lost of a phonological rule) as Booij's account:

(3) Stage 1: Allomorphy is phonologically driven
Stage 2: The rule disappears from the grammar
Stage 3: The underlying form is opaque for the learner in the absence of the rule
Stage 4: On the basis of surface forms, the next generation 'creates' a grammar where allomorphy will not be phonologically conditioned. The various allomorphs are now stored. It is true that to come up with this grammar, children will need full words to begin with. But once their grammar is fully developed, they will stop storing regular cases. Their lexicon will still resemble the bloomfieldian lexicon (see Pinker 1991 for a good example of this perspective)
→ The only premise that we must add to our set of premises is that allomorphy is not always phonologically driven, a move that has (usually) been forbidden in structuralism and generative grammar, but that is not as much nowadays (even if linguists tend to favour invariant underlying morphemes; but if the rules necessary to relate an underlying form to the surface ones are too complex and psychologically unrealistic, some underlying allomorphy will be assumed). To explain the type of lexical levelling (or more generally, analogy) described in Booij's examples, we need suppletion in the input forms, *not more*.

The conclusion is that we are still struggling to explain Stage 2: how a rule can disappear and how it happens. The advocates of intergenerational reanalysis have the advantage here: they can elegantly explain the disappearance of a rule. Booij's claim that reanalysis can take place for any adult speaker and that the speaker continually updates her representations have the status of hypotheses only, even if they seem realistic. We are still in the position to decide between two analyses that are both compatible with the data.

As can be inferred from what has been said so far, I agree with Booij's claims. The only problem is that many linguists will not take for granted his assumption about language change as occurring in adults' ('daily', 'constant') reanalysis (and his argument strongly depends on this assumption). Even more if these linguists hold in their premises that the acquisition of a language is about setting the parameters provided by UG. [4] The scenario I came up with, which is extreme-

[4] This is another premise that has a lot of weight in generative linguists' discussions. I will not develop more on this subject in what remains since it does not affect what I want to say. What I have to say, however, might have an effect on the properties of UG we want to propose.

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ly close in spirit to Booij's scenario, involves clear cases of reanalysis occurring in the post-acquisition period.

My data comes from Turkish. It is said that Turkish has a phonological process, vowel harmony (VH), which produces much of the surface allomorphy found in the language. Intensive borrowing got rid of VH. The main question is: Has VH been completely abolished from the system or has it been abolished only for certain parts? Clements & Sezer (1982) argue that it no longer exists for roots but that it still occurs in affixation. The problem is that some suffixes are disharmonic as well. But as always, we can use any diacritics provided by the theory with which we work and go on with the proposed rules.

3. TURKISH VOWEL HARMONY (TVH)

These are the vowel sequences that are permitted in an *ideal* Turkish word (morphologically simple or complex) according to the traditional analyses (following Göksel & Kerslake 2005): [5]

(4) *ɨ* followed by *a* or itself
a followed by *ɨ* or itself
e followed by *i* or itself
i followed by *e* or itself
ü followed by *e* or itself
u followed by *a* or itself
ö followed by *e* or *ü*
o followed by *a* or *u*
Note: *ɨ* = *ɯ*
ü = *y*
ö = *ø*

The 'phonological' rules involved are 'Fronting and Rounding Harmony'. An extra addendum is added that states that *o* and *ö* cannot appear in non-initial syllables, which explains why *o* and *ö* are not followed by 'something and itself' (but by 'something and something else').

There are two types of suffixes in Turkish (but see the exceptions presented below): type I and type A.

1) The vowel of the I-type suffix is a high vowel. Fronting and rounding harmony determines the form it will take relatively to the last vowel of the word to which it is suffixed to:

(5) *-i* 'ACC.' *-di* 'PAST' *-miş* 'PAST'

[5] See also other reference grammars such as Lewis (2000) and Kornfilt (1997) for similar accounts. In Royer-Artuso ([2013] 2015), I offer a review of the literature on TVH and this, from traditional account to generative ones. You can also find a precedent in Kabak & Vogel (2011). I prefer to concentrate on the reference grammar analysis because of its formal simplicity. In any case, the reference grammar analysis is basically what is found elsewhere. The existing analyses are in fact mainly devoted to the problem of non harmonic words and affixes that do not fit into the constraints given in (4). In my paper, I show that none of them succeed, including Kabak & Vogel's. Kabak & Vogel reached the conclusion that prespecification is needed for exceptions to TVH, which roughly means that we cannot give an account of their behaviour. For reasons of space, I cannot give a complete picture of the existing literature.

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k~~ı~~z 'girl'	*k~~ı~~zı*	*k~~ı~~zdı*	*k~~ı~~zmı~~ş~~*
kas 'muscle'	*kas~~ı~~*	*kast~~ı~~*	*kasm~~ı~~ş*
diz 'knee'	*dizi*	*dizdi*	*dizmiş*
el 'hand'	*eli*	*eldi*	*elmiş*
mum 'candle'	*mumu*	*mumdu*	*mummuş*
yüz 'face'	*yüzü*	*yüzdü*	*yüzmüş*
göl 'lake'	*gölü*	*göldü*	*gölmüş*
kol 'arm'	*kolu*	*koldu*	*kolmuş*

2) The vowel of the A-type suffix is unrounded and non-high. The frontness property of the last vowel of the word to which it is suffixed to determine the form it will take:

(6) *-a* 'DAT.' *-lar* 'PL.' *-dan* 'ABL.'

k~~ı~~z	*k~~ı~~za*	*k~~ı~~zlar*	*k~~ı~~zdan*
kas	*kasa*	*kaslar*	*kastan*
diz	*dize*	*dizler*	*dizden*
el	*ele*	*eller*	*elden*
mum	*muma*	*mumlar*	*mumdan*
yüz	*yüze*	*yüzler*	*yüzden*
kol	*kola*	*kollar*	*koldan*
göl	*göle*	*göller*	*gölden*

There are many exceptions to TVH due to 1) the large number of loans that have entered the language; 2) compounds and their lexicalization; 3) some exceptional suffixes in loanwords; 4) some foreign suffixes and prefixes that were borrowed; 5) certain native suffixes that do not vary (do not participate in VH); and 6) acronym formation (this image may not be complete).

These exceptions (if they are even mentioned) are the main problems that anyone attempting to provide a consistent analysis of TVH will face. These data are usually removed from the analysis by putting on them diacritics of some sort, generally of the [+foreign] or [-native] type.

These are the actual 'restrictions' on vowel sequences in Turkish after borrowing:

(7) Any vowel from the set {Turkish Vowel} can be followed by any vowel from the set {Turkish Vowel}

As Kabak puts it, "constraints on vowel co-occurrences in nearly all roots of native origin are almost the same as those in suffixes in Turkish" (Kabak 2011: 2839), but "any combination of vowel seems to be legitimate as long as the donor language permits it"(Ibid.: 2844). This should already indicate that there might be something wrong with the traditional (phonological) analyses of that language. We know that phonological processes tend to be transferred from L1 to L2 and that loans are generally adapted according to L1 phonology (see for example Flege 1981, Van Coetsem 1989, Strange 1995, Major 2001, Escudero 2006 and all the papers in Cala-

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brese & Wetzels 2009 and in Hansen Edwards & Zampini 2008). This means that one of the means we have to see if our proposed phonological rules really exist is to examine the phenomenon of transfer:

> "Borrowing provides evidences for the phonologist who seeks psychological verification of his theory in order to confirm him in the correct solutions. The main motivation for the analysis of borrowing is the possibility that the phonological properties of a language largely determine both the phonological shape and the phonological realization of a loanword. Thus, by analyzing occurring borrowed forms and/or conducting the necessary test on foreign sound perception, various aspects of speaker's internalized phonology can be determined" (Yavaş 1978: 34; see also Singh 1988).

Clements and Sezer have already proposed in 1982 that roots do not harmonize anymore in Turkish because of the immense quantity of words that have been borrowed, but that the suffixes still are harmonizing. I entirely agree with them, but I think they did not go far enough. I claim that TVH is a morphological analysis under the guise of a phonological one. This will be the subject of the next section.

Here is the image we end up with: a Turkish phonology very easy to describe and formalize, but with many exceptions,[6] and this, not only for roots, but for the affixes as well. Exceptions are specified as [-native] or [-harmonic] (see Kabak 2011: 2837 for a presentation of this diacritic), lexically, morphemically and / or as being governed by a different phonology (a coexisting co-phonology). And this, even if native speakers do not know the origin of what is present in their lexicon and do not harmonize loans. Even if they do, it is usually not completely and not in the expected direction, e.g., *minibüs* from French → *minübüs* **minibis*. We even have many cases where 'harmonic' words in the donor's language are 'de-harmonized', e.g., *halal* 'permitted' from Arabic → *helal*. And this is without taking into account the disharmonic 'native' words, e.g., *elma* 'apple', *anne* 'mother' and suffixes, e.g. the invariant *-(I)yor* 'Pr.', *-im mi*? 'should I X', *-gil* 'from the family X', *-(I)mtrak* 'that looks like X', *-ken* 'while doing X', etc.

As a reviewer mentioned, "the idea that vowel harmony is no longer a part of the phonology of Turkish implies (and even entails) things about how speakers would produce and/or make judgments about existing and novel words". The results of the experiments presented in Altan (2011) and in Sofu (2001) are very interesting in this context.

Some native Turkish speakers were tested in an experiment where they had to learn an invented 'language' (it was actually closer to learning an invented 'list of words' than a language). There were three situations: 1) only harmonic words; 2) only disharmonic words; and 3) mixed lists of words. Altan found that the context (harmonic / disharmonic / mixed) had an influence on the type of errors and on the production efficiency of these learners: 1) those in the harmonic situation tended to harmonize; 2) those in disharmonic situation did not; and 3) those who were in the mixed situation harmonized and disharmonized in their 'speech errors' at random. Having already commented on the mixed character of the Turkish lexicon, it is easy to speculate that Turkish speakers will react in the same way as the subjects in the mixed situation reacted to the word lists. And if we remember the discussion about loans and remember that the subjects are native

[6] I will not list all the lexical exceptions here because, as can be inferred from footnote 2, it would be like listing the 'non-native' forms in a language like English.

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Turkish speakers, it is very problematic that they were not influenced by their supposed harmonic phonotactics in the context of this experiment.

We can also look at the following cases presented in Sofu (2001): 1) *çok yoğlu (= çok yağlı)* 'very greasy'; 2) *fuur-una (= fuar-ina)* 'its fair' from french *foire*; 3) *banim başımin belasi (= benim)* 'my head's curse'; 4) *gölmekten ölür (= gülmekten ölür)* 'laughing to death'; 5) *herkas hata (= herkes hata yapabilir)* 'everybody can do mistakes'. The first two present something like TVH (a process that goes from left to right). The problem is that the sequences of vowels are already correct /o a/, /u a/ (in the *fuuruna/fuarina* case, the resulting 'harmony' occurs after *fuar* becomes *fuur* and from there suffixation takes its input). The last three are following a sort of anticipatory assimilation (not THV), something very common in every language, even those that do not share the 'harmonic' character of Turkish. Note that the fifth one even creates a disharmonic word (*herkas*).

These data are problematic if we posit VH. As the results of experiments on Turkish speakers presented in Becker & al. (2011), the authors suggest that "speakers' ability to project trends from their lexicon onto novel items is a well-established observation" (Becker & al. 2011: 63). Their experiment does not concern VH, but their conclusions are appropriate for the present discussion. They suggest that only universal constraints are transferred. If VH is not such a constraint (something like a facilitatory process), we might as well just say that it is about arbitrary results of some antecedent stage of the language, i.e. *morphologization*.

Another fact militates against a harmonic analysis: phonological processes sometimes go beyond the domain in which they usually apply. I think of processes like affrication, consonant assimilation, etc., that sometimes go beyond the domain of the word and apply to the next word if its first 'phoneme' allows it. I suppose that TVH, described as a spreading process, should often apply to the first vowel of the next word in fast tempo speech, slips of the tongue and other contexts of the same type. But I have never observed anything that resembles this and has never heard of a mention of it (Skousen 1972 says it takes place in Finnish VH).

4. A MORPHOLOGICAL THEORY UNDER THE GUISE OF A PHONOLOGICAL ONE

The analysis of Turkish as a harmonic language is, as I have said, not a phonological analysis (as it seems to be) but a morphological one (at least for the synchronic state of the language). Three core concepts are at the center of the traditional and contemporary analyses of Turkish (at least for what concerns us here):

(8) 1) Agglutination;
2) Vowel Harmony (VH); and
3) Regular stress

VH and Stress are seen as giving word boundaries for morphologically simple and complex words (note that in Example 9, bold syllables represent stressed syllables):

(9) *ma**kas*** 'scissor'/*makas-**lar*** 'scissor + Pl.'/*makas-lar-**dan*** 'scissors + Abl.'
*göz**lük*** 'glasses'/*gözlük-**ler*** 'glasses + Pl.'/*gözlük-ler-**den***'glasses + Pl. + Abl.'

The invariant morphemes are concatenated and then phonological processes are applied to

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the outputs of the morphological component. So we can specify exceptions to these rules one by one. But it is not necessary for suffixation, because of its relative regularity, i.e., its (almost) complete lack of exceptions. Only certain affixes must be specified, e.g., *–(I)yor* 'Pr.', the invariant *–im mɨ* 'Should I X', *-istan* 'country of X', *-matik* 'machine for X', *-i* 'player of instrument X', *-o* 'diminutive', *-kolik* 'addict to X', etc.:

(10) *yap* 'doing' + *iyor* 'Pr.' → *yapɨyor* 'He is doing'
yap + *im mɨ* 'Should I ?' → *yapim mɨ* 'Should I do?'

But we forget important details in proceeding this way. When account has been taken of what has been said so far, the distributions given in (4') (on acceptable Turkish vowel sequences and related harmonic rules and constraints) is in fact a surprising description of the system (Example 4 is repeated for convenience):

(4') *ɨ* followed by *a* or itself
a followed by *ɨ* or itself
e followed by *i* or itself
i followed by *e* or itself
ü followed by *e* or itself
u followed by *a* or itself
ö followed by *e* or *ü*
o followed by *a* or *u*
Rule: Fronting and Rounding Harmony
Constraint: **o* and *ö* cannot appear in a non-initial syllable

The constraint '*o* and *ö* cannot appear in a non-initial syllable', for example (which will be the center of much of what remains for its apparent simplicity but especially for the problems it really poses for the analyses), *disappeared* with borrowing (e.g. *horoz* 'coq' from Greek, *aspiratör* 'vacuum cleaner' from French, etc.; the 'affectionate' suffix *–oş,* the 'diminutive' suffix *–o*, etc.; let alone the problem a *native* suffix like *-(i)yor* poses for this account). But the phonological system did not take account of this fact, and this, not only for roots, but also for suffixation. A word like:

(11) *göl* 'lake'

when suffixed with *-lar* 'Pl.' should surface as in (12) according to the harmonic rules:

(12) **göllör* 'lakes'

but surfaces as in (13):

(13) *göller* 'lakes'

the form predicted by the extinct phonology (the harmonic rules + the constraint on *o* and *ö*). This fact clearly indicates that we must propose an analysis in which some sort of suppletion is involved (i.e., the morphological choice of allomorphs coexisting in the mental lexicon), that is,

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surface *-ler* and *-lar* are not conditioned phonologically but there from the beginning in the lexicon. The invariant morpheme approach cannot handle these types of data.

5. FROM PHONOLOGICAL EXCEPTION TO LANGUAGE CHANGE

All the proposed models of TVH that we find in the literature are in fact trying to cope with the complexity introduced into Turkish's phonological system by the large number of loans from languages that do not share its (proposed) phonological restrictions. Co-phonology is the only device that is conceptually close enough to this assessment: [7] Turkish is now a mixed language and we have to take this fact into account in our descriptions. But co-phonology is not very different in spirit from the [+foreign] diacritic, at least in the case of languages such as Turkish, where the speakers do not know what languages were the sources of borrowing. The use of all these types of devices gives an intriguing image of native Turkish speakers: these speakers are somehow 'code-switching' with languages they do not know. Or, even more: they speak their own language with a foreign accent. Turkish being a mixed language, we must treat it in this way in our description: the phonological module does not work in the same way as the 'original'.

The problems facing descriptions of mixed language always remind me of the following paradox: If we have a boat and from time to time we change one of its pieces, do we have the same boat at the end of the day? It appears that all proposed models of TVH are an affirmative answer to this question.

At the meta-theoretical level, the conclusion of this discussion can be more or less illustrated in the following way: grammars are generally organized in a way that more or less reflects the stages of language acquisition. Beginning with phonology, we get to morphology and then syntax (the necessary chapter between phonology and morphology is handled by dictionaries). When we open a Turkish grammar, one of the first things we learn is that words must respect Vowel Harmony. Then comes a question: for the child acquiring this language, will mastering this type of phonology not become a severe handicap for the following task of learning this language's lexicon?

The only conclusion I can think of is that when a speaker develops the ability to produce sequences she could not produce before, we can only suggest that her phonological module or component has changed in some way. Often bi or multilinguals will be the source of this phenomenon, and the fact that they can insert L2 'foreign' words into L1 without transforming them makes these words good candidates for adoption by speech community.

The problem is that the dominant perspective on the interaction between phonology and morphology (allomorphy) gives no other option to describe the synchronic state of Turkish (see the introduction of Thomason & Kauffmann 1988 for a good discussion of the reasons linguists do not want to see languages having two or more 'parents').

6. A WORD-BASED (NOT MORPHEME- OR LEXEME-BASED) APPROACH TO TVH

[7] Kabak & Vogel (2011) show that co-phonology does not work for Turkish since many words would have to be considered participating in more than one co-phonology.

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The conclusions in the previous section are the ones that are reached in Royer-Artuso ([2013] 2015), a paper dealing exclusively with TVH and the problems of its status as a synchronic phonological process. The discussion was agnostic about how to handle the data.

In following works, I proposed that a word-based account that did not give a place to the notion of (invariant) morpheme was the only available solution to deal with the Turkish data. To do this, I couched my analyses in Whole Word Morphology, a framework developed by Alan Ford and Rajendra Singh (and others following them) in numerous publications (Ford and Singh 1991, Ford et al.). The model has a phonological companion theory, Generative Phonotactics. Let me summarize briefly these two theories and show how they work together.

6.1 Generative Phonotactics

The phonological theory, Generative Phonotactics (Singh 1987, Desrochers 1994), states that phonology is automatic: to be considered phonological, a process must apply whenever its context of application is found. This context is purely phonological. The first step consists in finding the phonemic inventory and the phonotactic constraints of the language under study and from there on, understand which repair mechanisms are involved in the phonological component of the language.

Let us suppose that Turkish was really a harmonic language. The phonotactic constraints would be the following (in (14) I translate the restrictions on vowel sequences given in (4) in phonotactic constraints):

(14) *ɨ* followed by *a* or itself, or: *~~ɨ~~Ci, *~~ɨ~~Ce, *~~ɨ~~Co, *~~ɨ~~Cu, *~~ɨ~~Cö, *~~ɨ~~Cü
a followed by *ɨ* or itself, or: *aCi, *aCe, *aCo, *aCu, *aCö, *aCü
e followed by *i* or itself, or: *eCa, *eC~~i~~, *eCo, *eCu, *eCö, *eCü
i followed by *e* or itself, or: *iCa, *iC~~i~~, *aCo, *aCu, *aCö, *aCü
ü followed by *e* or itself, or: *üCi, *üC~~i~~, *üCo, *üCu, *üCö, *üCa
u followed by *a* or itself, or: *uCi, *uCe, *uCo, *uC~~i~~, *uCö, *uCü
ö followed by *e* or *ü*, or: *öCi, *öCa, *öCo, *öCu, *öCö, *öC~~i~~
o followed by *a* or *u*, or: *oCi, *oCe, *oCo, *oC~~i~~, *oCö, *oCü

Repair mechanisms would then ensure that none of these constraints will be violated. This would replicate the traditional harmony rules.

As we have seen, there are many exceptions to these constraints. In fact, we have seen that no sequence of vowels is prohibited anymore in Turkish. This would exclude instantaneously a phonological analysis of 'harmonic processes' in Turkish. In the model, this means that the 'surface alternations' must be analysed as morphologically driven.

6.2 Whole Word Morphology

Whole Word Morphology proposes that morphology is no more than "the study of formal relationships amongst words" (Ford & al. 1997: 4) as presented in (15):

(15) $/X/_a \leftrightarrow /X'/_b$

where:

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a) X and X' are words
b) a and b are morphological categories
c) ↔ indicates an equivalence relation (a bidirectional implication)
d) X' is a semantic function of X
e) ' indicates a formal difference between the two poles of the morphological operation
f) ' can be null if a ≠ b

Let us take the six morphological paradigms offered in (5) and (6). Since we have seen that phonology is not involved in the surface alternations, we must treat these alternations as being handled by the morphological component, that is, by word formation strategies. Example (16) gives the word formation strategies for the suffix *-di*:

(16)[8] /X~~i~~(C)/$_N$ ↔ /X~~i~~(C)d~~i~~/$_{N\ (Past\ 'It\ was\ a\ N')}$
/Xa(C)/$_N$ ↔ /Xa(C)d~~i~~/$_{N\ (Past)}$
/Xi(C)/$_N$ ↔ /Xi(C)di/$_{N\ (Past)}$
/Xe(C)/$_N$ ↔ /Xe(C)di/$_{N\ (Past)}$
/Xu(C)/$_N$ ↔ /Xu(C)du/$_{N\ (Past)}$
/Xo(C)/$_N$ ↔ /Xo(C)du/$_{N\ (Past)}$
/Xü(C)/$_N$ ↔ /Xü(C)dü/$_{N\ (Past)}$
/Xö(C)/$_N$ ↔ /Xö(C)dü/$_{N\ (Past)}$

Note that *d* → *t* when preceded by a voiceless consonant (*bulut* 'cloud' → *buluttu* 'it was a cloud'). This is clearly without exception and corresponds to a phonotactic constraint in Turkish: sequences of voiced and voiceless consonants are forbidden in this language, and must therefore be repaired if they are to appear (here, following word formation processes). The suffix *-ci* 'someone who deals with X', in (17), will serve as an example:

(17)

Native words	Non-native words
yalan 'lie' *yalanc~~i~~*	*akordeon* 'accordeon' *akordeoncu*
çanta 'bag' *çantac~~i~~*	*gazete* 'journal' *gazeteci*
But:	
ip 'rope' *ipçi*	*kasap* 'kind of meat' *kasapç~~i~~*
at 'horse' *atç~~i~~*	*şikayet* 'complaint' *şikayetçi*

Note : *c* = $\widehat{dʒ}$

[8] Or in a more general and condensed manner:

/ X [V] (C) /$_{N.}$ ↔ / X [V] (C) d [V] /$_{N.\ Past}$
[α ant.] [α ant.] [α ant]
[β lab.] [β lab.] [β lab.]
[+high]

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$ç = \widehat{tʃ}$

Note that the native / non-native distinction is not relevant here: there is no alternative for the speaker, it is totally automatic, which is what we would expect from real phonological processes.

With this approach we also get rid of the need to stipulate the extra constraint taking care of the vowels *o* and *ö* that we saw did not fit the facts. As we have seen, no constraint *σCo or *σCö exists synchronously in Turkish. Example (18) gives the word formation strategies for the dative suffix -a:

(18)[9] /X~~ɨ~~(C)/$_{N}$ ↔ /X~~ɨ~~(C)a/$_{N\ (Dat)}$
/Xa(C)/$_{N}$ ↔ /Xa(C)a/$_{N\ (Dat)}$
/Xi(C)/$_{N}$ ↔ /Xi(C)e/$_{N\ (Dat)}$
/Xe(C)/$_{N}$ ↔ /Xe(C)e/$_{N\ (Dat)}$
/Xu(C)/$_{N}$ ↔ /Xu(C)a/$_{N\ (Dat)}$
/Xü(C)/$_{N}$ ↔ /Xü(C)e/$_{N\ (Dat)}$
/Xö(C)/$_{N}$ ↔ /Xö(C)e/$_{N\ (Dat)}$
/Xo(C)/$_{N}$ ↔ /Xo(C)a/$_{N\ (Dat)}$[10]

Nothing more is needed to account for the (remnants of) 'harmonic' behaviour of Turkish vowels in suffixation and especially for the particularity of vowels *o* and *ö*.

7. FROM ALLOMORPHY TO SUPPLETION: THE NECESSITY OF WORD-BASED APPROACHES

But some colleagues objected that I did not need to get rid of the notion of morpheme and of traditional morphemic approaches to morphology and that the word formation strategies I gave were still compatible with morphemic models that give a place to suppletion. In fact, models of this kind exist: suppletion in these models is assumed if one can show that the original phonological processes responsible for surface morphemes alternations are no longer productive synchronically.

The remaining part of the paper is an attempt to show that even these types of models cannot handle the data without going into theoretical and / or logical dead ends.

What I have described so far is relatively similar to what Anderson describes in the following quotation:

> "It is hardly a novel suggestion that most if not all cases of morphologically or lexically determined variation can be traced historically to the effects of originally phonological rules" (Anderson 1992: 340).

[9] I give the word-formation strategies in details for convenience. But see the preceding footnote for an example of a condensed formalisation.

[10] Note that the last two strategies (for the words ending in *o* and *ö*) are the important ones for the following discussion.

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In a sense, (weak) suppletion *is* allomorphy when one can show that certain phonological rule(s) have disappeared from the grammar. Anderson proposes that phonologically-driven patterns are morphologized or lexicalized by the next generation when rules become too opaque to find a phonological motivation for the encountered alternations (Ibid.). I want to show that a piece is missing in this analysis and that this is where the morpheme-based (or lexeme-based in Anderson's case) ontology really shows its weakness. [11]

Assuming some sort of suppletion works when synchrony *only* is involved, that is, as a *descriptive* method. But we must account for the change in the system for the first individuals who have gone from the phonologically-driven rules (if there was such a system) to the non-phonologically driven ones. This can only be accounted for if we assume that full morphologically complex forms were stored as such in their lexicons. Anderson proposes reanalysis and it is often this way that linguists treat the transition from one state of the system to another, that is, change.

The notion of reanalysis, from my point of view, is often used as another addendum to the dominant 'ontology'. [12] It can in fact be considered as another protective belt to the hard core of the morphemic theory: change, in this analysis, can only occur through a 'wrong' analysis of the data by new native speakers faced with extreme opacity and therefore unable to determine the phonological rules causing allomorphy. If we accept only invariant morphemes, new rules will be necessary for the new generation; if we accept suppletion, we say that the new generation has developed representations that differ from those of the preceding one.

This perspective has the following logical implication, which seems necessary unless the theoretical foundations collapse: speakers, once they have developed their grammar, keep on with it until they disappear. Change can only occur in diachrony, in the period of formulation of grammar by the new generation, and this new generation will also keep on with its grammar until the end, and so on.

To give an example of the problems associated with this type of account, let us return to the phonotactic restriction concerning *o* and *ö* discussed above. As we have seen, there used to be a constraint forbidding *o* and *ö* to appear in a non-initial syllable, which has disappeared through borrowing. This constraint is proposed to explain why a suffix of type A does not surface as -(C)o(C) or -(C)ö(C) (*göl* 'lake' + *lar* 'Pl. **göllör*, which should be the expected form according to VH). This constraint is in fact proposed not to have to assume suppletion.

Let us suppose that such a constraint really existed and suppose that the speakers keep on with their grammars at all costs. Once words that did not respect this constraint were borrowed and entered their lexicon (because such a moment must be assumed), what did happened? Did they treat them only as exceptions or did a reorganization of their grammar take place? I assume,

[11] Note that Anderson is a proponent of a non-morphemic approach to morphology but he still advocates a view where units smaller then the word exist. The main problems that are discussed by critics of the notion of morpheme are semantically non-compositional examples under the saussurean notion of sign, e.g. Aronoff (2007). The problem is that semantically non-compositional morphologically complex words and reanalysis are also both compatible with morpheme-based models accepting suppletion in a bloomfieldian lexicon, the "list of basic irregularities" (Bloomfield 1933: 274), e.g. Pinker (1991). No argument that I am aware of justify the idea that some words must *necessarily* be stored in their entirety, not only for morphologically simple words and exceptions but for morphologically *regular* complex words *as well*, i.e. for the ones that follow the productive word-formation processes of the language. I see the real contribution of this paper as being a justification that some need to be so and that these words are what speakers use to construct the morphological part of their grammar.

[12] And this could be said about any process of grammaticalization as well, but I will not develop further on the subject here. Note that I am not *against* the idea of reanalysis. I am only against it when it is used only to protect the general framework that underlies our analyses (to be fair: often without awareness of doing so).

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if we want to be realistic: 1) that a reorganization occurred (in the same way that a L2 learner must change phonologically to stop the interferences of her L1 phonology); and 2) that phonology has ceased at this time to be involved when new words were formed that respected the old allomorphic paradigms.

The only possibility is that these speakers' morphology and / or lexicon was now responsible for the paradigmatic asymmetries, i.e., in their new synchronic system: complex forms needed to be listed in one way or another to prevent unwanted outputs from *new* inputs, i.e., not encountered yet in their morphologically complex surface forms. Another way to put this is to say that they were needed as models for analogical processes. What follows is what to me is the scenario we must necessarily assume.

At the moment when a rule disappears, there is no suppletive form in the lexicon to take the former role of phonology in the creation of allomorphs:

(19) no constraint *σCo or *σCö and *one* underlying form *–lar* 'Pl.'

Given this distribution of labour, we would thus expect a complete reorganization of paradigms based on the underlying (invariant) allomorph:

(20) *güllar*, *iplar*, *göllar*, etc.

But this is not what happens: the remnants of allomorphic forms continue to be the inputs to word-formation strategies:

(21) no constraint *σCo or *σCö and *one* underlying form *-lar*

But:

(22) *güller*, *ipler*, *kollar*, etc.

This shows that, even before the rule disappeared, full complex words were stored. If not, how could one possibly remember the surface forms provided by the extinct phonology? Another way to put this is to say that once the phonological rule disappears, each exception (the new suppletive forms) must instantly enter the lexicon. But this presupposes that the morphologically complex forms are already accessible in their entirety for this massive dumping to take place.

This type of argument is in many respects very similar to that of Booij. The main difference has to do with the locus of change: in the Turkish case, it was borrowing that changed the grammar.

8. LOANS AND PHONOLOGY

The fact that a word is a loan is, from a synchronic point of view, only relevant for two types of speakers: (i) The etymologist or the speaker with some background in etymology; and (ii) The bilingual who masters (at least minimally) the language from which the loan is borrowed.

When loans follow the phonological rules of the language, they do not create problems of analysis. When they do not, they become extremely problematic. A literature on loans that are not adapted to the phonology of L1 does not really exist. They are generally considered only as

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'exceptional'. Discussions that are most closely associated with this topic are those that deal with code-switching. But for code-switching to take place, speakers must somehow master L2.

The current situation for Turkish native speakers is that almost none of them master Arabic and Persian, the two languages from which the majority of loans were borrowed. For them, any new word they learn is a loan from other speakers of their language, whether it is etymologically native or not, and more importantly, whether it is harmonic or not.

The situation was different when these loans entered the Turkish lexicon: some speakers were bi- or trilingual. But not from birth: they were learning these languages as L2.

The prediction in this type of situation is that L1 phonology (Turkish) will influence the way these words will be pronounced (transfer of L1 phonology). We would expect that, at first, everything in their L2 will show up harmonized. But this would impede on communication because VH would create forms that do not agree with the minimal pair requirements of the donor languages. The L1 speakers therefore had to develop a phonology sufficiently flexible to meet the requirements of these L2 / L3 languages.

What all this means is that we should not look at *language* to explain change. We should look at individual *speakers*. Some speakers change in their post language acquisition period. According to the model we use, we can say that a rule, a process or a constraint disappears from their grammar because the speaker needs to keep up with forms that are incompatible with her L1 phonology.

This scenario implies that Booij is right to say that representations and by implication, rules or constraints can change, and the system reanalyzed over the course of a lifetime. We do not need new generations for this process to happen. It also means that, according to the argument presented above, morphologically complex words must necessarily be stored for the type of change I have described to take place, even for the regular cases. We do not need a dichotomy between regular and irregular patterns and only store the latter because they are irregular, unpredictable, non-compositional, etc. We must accept that speakers store morphologically complex *regular* words as well.

The set of relations that hold between our premises (morphemes, phonological allomorphy, concatenation of morphemes to create words or phrases, etc. and change) creates the need for a theory of change based on reanalysis seen as learner-driven (as opposed to user-driven). This theory is necessary because, according to the premises, only children have access to full forms as input for the 'creation' of their grammar (they then abandon these full forms for abstract minimal redundancy-free representations):

> "It is significant that many of the challenges faced by a constructive account derive from the assumption that a speaker, having identified the parts of a word form, then proceeds to discard the original word. The idea that speakers 'optimize' their mental lexicon by storing only the parts of complex forms in turn reflects more general assumptions about the lexicon being largely 'redundancy free'. An examination of complex morphological systems suggests the need to reconsider these assumptions and the conception of grammatical analysis that underlies them" (Blevins 2006: 569).

I hope that I have been able to show that these premises must be abandoned: the units that are stored are not morphemes, but complete words. This means that 'reanalysis' can take place constantly because speakers *of all ages* have access to complete forms. If it does not take place, it is not for grammatical reasons, but for questions related to norm, frequency, etc. (frequency is

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somehow another way of saying 'norm'). Storage and therefore reanalysis do not stop in early childhood, when children are said to compute the grammar. Storage and reanalysis take place constantly.

9. CONCLUDING REMARKS

I hope that I have been able to show that Whole Word Morphology is a model that can deal adequately with this type of problem, and this, without any remaining paradoxes: we can account straightforwardly for the kind of scenario that was discussed above. Once a loan enters L1 without being adapted according to the processes existing in her L1 phonology, it must be assumed that the speaker (but not necessarily the *language*) is at this time phonologically different.

Few models are suitable for dealing with this type of problem. In the list of available theories, I see only a few who are able to do so:

1) Natural phonology (well presented in Donegan & Stampe 2009; see also Baronian 1999 for an interesting attempt to 'merge' Natural phonology and Generative Phonotactics) is able to do this because it defines phonological processes as innate processes that need to be suppressed. If VH is such a process, it becomes easy to see that, as in Generative Phonotactics, as soon as a word that does not conform to these natural biases is produced 'correctly', the speaker has jumped to another level of phonological 'complexity' (notwithstanding the problem of defining 'complexity');

2) Optimality theory, which is in any case influenced by Generative Phonotactics (see the many references to Singh in Prince & Smolensky 2004, an updated version of their 1993 original proposal). A reordering of the constraints will take place as soon as a borrowed word does not conform to the order of constraints. At least, this should be considered the realistic scenario. See Green (2007) where an Optimality model is developed where the place of morphology is discussed following many ideas developed in the Whole Word Morphology's literature;

3) Usage-based phonological and morphological models (for example Bybee 2003, Blevins 2004 and Booij 2010), where surface forms play a much more important role than in traditional abstract analyses. Many important ideas are developed within this framework. The main problem I see is that the overall organization of the language architecture is not very clear, e.g., the facts to be attributed to the domain of phonology and to the domain of morphology. Another problem with usage-based / exemplar-based models is that it is difficult to see how we can explain L1 → L2 phonological transfer, a fact of great importance in the previous discussion. Nevertheless, my analysis of Turkish would be very similar to a Construction Morphology analysis (Booij 2010) if the latter model did not resort to as many co-phonologies as necessary to take care of the 'exceptions'.

I will end this discussion with a final example that corroborates what I proposed in Section 4. In this section I stated that when a speaker develops the ability to produce sequences that he could not produce before, we can only suggest that its phonological module or component has changed in a certain way. Often bi- or multilinguals will be at the source of this phenomenon and the fact that they can insert 'foreign' L2 words in L1 without transforming them makes these words good candidates for adoption by the speech community:

> "Borrowed words" should be reserved for words that are not so domesticated. "Adapted words" are borrowed words not for speakers but for historical linguists. Undomesticated,

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unadapted words raise questions regarding bilingualism and its transmission as well as regarding the possibility of phonotactic change as a result of language-contact and bilingualism" (Singh 1985: 269) (see also Fries & Pike 1949 for interesting discussions on multilingualism and its influence on phonology).

Some colleagues objected to my handling of exceptions in Turkish by saying that a single exception to a rule should not be sufficient to reject this rule when it manifests itself productively elsewhere. Apart from the slippery slope puzzle that this response can induce (what about two exceptions, and if two, what about three, etc.), this is the whole question of 'What exactly is phonology?' that is involved here. We can of course continue doing our analyses based on invariant morphemic representations and leave to phonology everything that has to do with surface outputs that are not semantically different. But I think the cost is high.

The puzzle these colleagues came out with is the following: in Turkish, there is a phonological rule that devoices consonants in codas. But it has been argued that some native speakers of Turkish pronounce the word *etüd* 'étude' (from French) with a final *d*. Am I to say that devoicing is no longer a phonological rule in Turkish? My answer is twofold:

1) It depends on what we mean by *phonology*. According to what I said earlier about multilinguals, it is clear that for me these speakers do not have phonological rules or constraints related to devoicing. In fact, only those proficient in other languages that have no final devoicing are able to pronounce *etüd* with a final *d*. For monolinguals, this is not even a possibility: they cannot even hear a *d* in a coda when it is pronounced with it: they will hear it with a *t*. Their phonological system treats the inputs minus the impossibilities of its phonology. In other words, multilinguals are doing what they *know* is expected from them; monolinguals do what they *can.* For the former, the 'borrowed' words are 'correctly' represented, but they also have competing representations for what monolinguals produce. For monolinguals, the same 'borrowed words' are 'incorrectly' represented.

But if, in any case, we want to relate the same underlying representations to many different surface representations according to the phonological rules that we proposed, then a form like *etüd* is problematic and we have to reject it as an exception. We still can have recourse to the variable rules of the sociolinguist, to stylistic rules, etc., but this does not yet explain much. In fact, it can be seen as only reifying the traditional approaches, where deep representation are linked to many surface ones using phonological rules (see Sankoff & Labov 1979 for a representative example of this method). I would suggest that multidialecism or multilingualism are probably involved whenever we feel the need to resort to these constructs.

2) It depends on our vision of what a language is. If we adopt an approach where it is the linguistic system that is important to model and where the speakers are considered homogeneous respectively to the rules of this language, we have no choice but to treat a word like *etüd* as an exception to the system. Because it is true that few people are able to pronounce it this way (the percentage of bi or multilingual having Turkish as their L1 is rather low in Turkey).

But if we take the individual speaker as the center of our analyses, then exceptionality is not the exception but the rule, so to speak: each individual is bound to many different circles and this will influence the outcome of her linguistic behaviour. She might be or become multidialectal, multilingual, which will have repercussions on her phonological system, at least as soon as the L1 → L2 transfers will be overridden. This amounts to saying that language contact is much more about speaker contact (something very difficult to model or predict but that we must never-

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theless take into account) than about language systems interacting (if this means something anyway, apart for linguists).

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