

Author's Response

On the complexity of simplification

by PETER TRUDGILL

In the paper under discussion here, I attempted to shed light on complicated processes connected with sociolinguistic typology by dealing with the main factors which I hypothesise are involved, not as the continua that they in reality constitute, but as bipolar dyads. This was a methodological decision made on the basis of the fact that this was a programmatic paper attempting in a relatively small space to provide as straightforward an outline as possible of what is in fact a seriously complex issue.

For example, in this paper, as in a number of others (e.g., Trudgill 1989, 1996a, 1998), I proposed that there are three societal factors that are likely to be relevant to the study of sociolinguistic typology. These factors are community size (small versus large), social network structure (tight versus loose), and contact (low versus high). All three of these parameters admit of very considerable degrees of more or less, but in fact I have treated them in an either-or fashion. That is, I have attempted to simplify by taking communities with relatively extreme values as exemplars, rather than giving “an exact numerical interpretation”, as Pericliev points out.

As far as sociolinguistic factors are concerned, I have also distinguished between two different types of contact, long-term contact involving child bilingualism, and short-term contact involving adult acquisition. Again, any actual real-life situation is likely to involve larger or smaller proportions of child versus adult learning, a further complication which I have for the time being avoided. I have also made no attempt to answer the question: how long is “long-term”? Rice also correctly points out that TYPES of contact may vary considerably (including, fascinatingly, contact which leads to passive rather than active bilingualism), something which I have also ignored.

Table 1. *Six possible combinations of three societal factors*

	1	2	3	4	5	6
Size:	Small	Small	Small	Small	Large	Large
Network:	Tight	Tight	Loose	Loose	Loose	Loose
Contact:	Low	High	Low	High	Low	High

From a linguistic perspective, in my paper I also discuss small versus large phoneme inventories. Here again I have attempted to simplify the exercise in order to reduce it to manageable proportions by concentrating on extreme exemplars with “very large” (e.g., !Xū with 95 consonants, plus vowels) and “very small” (Hawai’ian, with 13 phonemes) inventories. (Incidentally, my concern is with total inventories, not just with consonants as Pericliev suggests.)

I have also, as Kabak correctly points out, not taken account of the systematicity of phonological systems, though I agree that this is important. On the other hand, I do not find helpful Kabak’s assertion that the disappearance of some phonemes from Polynesian “can be attributed to the loss of voicing contrast among stops” and that “this can explain, in a systematic way, why all five voiced stops disappear”. I do not see the explanatory power of this – it tells us HOW the reduction occurred, not WHY.

As the commentators indicate, these decisions of mine are all simplifications, and represent considerable distortions. I would argue, however, that my ignoring of these complications is a necessary simplification, at this stage of the enterprise, in order to attempt to come to grips with the complexities involved.

I am extremely fortunate to have had my programmatic paper discussed by a series of such distinguished scholars with vast amounts of empirical data from a number of different language families at their disposal.¹ But note that there is one respect in which I do attempt to deal head on with complexity in a way that a number of the commentators do not. If all three of the societal factors I have treated as crucial were independent of one another, there would actually be eight possible combinations. However, it would seem that the combination of LARGE community size and TIGHT social networks is impossible. If this is the case, then there are six possible combinations (Table 1).

1. I am very grateful indeed to the commentators for their erudition and very considerable industry. Note that since the paper under discussion here is about segmental phonology, I have not dealt in this response with Bakker’s remarks on grammatical complexity. For my views on this, I would refer him and other readers to papers cited in Kusters (2003) such as Trudgill (1989, 1992, 1995, 1996b, 1998).

It is true that in my publications, in order to simplify a very complex set of processes, I have dealt with only two of these six. I have once again taken the two most polar opposites: small size + tight network + low contact as opposed to large size + loose network + high contact, that is, categories 1 and 6.

On the other hand, I have been at pains to argue that it is important in principle to deal with all three of the societal factors of size, network, and contact IN COMBINATION, even if I have sometimes used descriptions such as “small” as a kind of shorthand for “small, tightly knit, isolated”. The commentators have, with their deeply impressive erudition on different language families and skilful usage of databases, been able to go some way to meeting my caveat that “in the absence of a large-scale database of evidence” any conclusions can only be suggestive. However, they have not necessarily always focussed on the three factors in combination, as I believe has to be done. The problem is perhaps that databases can tell us nothing about network structure; and that it needs detailed historical and anthropological work, of the sort Rice reports on, to learn about the extent and nature of contact. Bakker, for example, focusses on only one factor, viz. contact, while Pericliev concentrates mainly on size. In particular, social network structure is little mentioned in these commentaries, even though I take it to be vital. (I do not, by the way, include “dominance” as an important factor as Kabak erroneously states – this was in fact a suggestion of Haudricourt (1961), which I reject.)

It does not appear to me that ignoring the importance of the fact that these factors need to be considered IN COMBINATION can be a fruitful approach because, as Rice very ably illustrates, types of combination of these factors can very readily be found other than the two I deal with. The communities Rice cites, for example, are typically SMALL in size but HIGH in contact, and would therefore probably fall into category 2 above. There can therefore be no reason at all to expect to find a simple correlation between the numbers of speakers of a language and the number of phonemes in that language.

In fact, if we look hard enough we will probably be able to find representatives of all six possible combinations. We can, for example, certainly find examples of category 3 – small, isolated, low contact communities with LOOSE network structures. Rural Newfoundland, as outlined by Paddock (1975), and West Falkland, as discussed in Trudgill (1986) and Sudbury (2000), are both cases where focussing, in the sense of Le Page & Tabouret-Keller (1985), has not taken place and where dialects differ from hamlet to hamlet or family to family because of loose social network structures. An example of category 4 might be the minority Vlach-speaking “language islands” (Berend & Mattheier (eds.) 1994) of the southern Balkans, including Greece (Winnifrith 1987). And a case of category 5 might be 18th-century Japanese. We can expect categories 2, 3, and 4 to be, in some as yet to be investigated way, intermediate between categories 1 and 6.

I agree with my commentators that it is obvious that the nature of the systems in contact will be vital. Bakker correctly says that “compromise between the languages spoken by the groups in contact” is important, while Kabak also correctly states that “the acquisition of phonemes is crucially determined by the extent to which the phonological systems involved in the contact situation differ from one another”. On the other hand, it should be pointed out that Bakker’s claims that pidginisation cannot be held responsible for small phoneme inventories is the exact opposite of the claim made by Sebba (1997), who says that pidgins typically demonstrate loss of non-shared and/or marked articulation types and thus smaller numbers of segments. And remember in any case that my claim was that adult language contact would lead, other things being equal, to MEDIUM-sized inventories, not small ones.

There is nevertheless significant counterevidence to my proposal amongst the commentaries which I will need to consider in future work, plus a certain amount of modest support, especially from Rice’s paper, as for example in the case of Tsuut’ina. Rice’s portrayal of the complex Athapaskan consonantal systems with a number of marked articulation types also supports my contention that “though changes are less likely to occur in [small, tightly-knit] communities, these changes may also tend to be of a more marked type, because of the ability of tightly-networked societies to, as it were, force such changes through; and the languages of such communities may therefore be more likely to have more marked forms and structures”.

There are, however, amongst the commentaries, no real counter-theories, with the honourable exception of Hajek, who proposes that areal diffusion can account for the development of small phoneme systems. “Contact can trigger substantial inventory reduction and the rise of very small systems”, he suggests. Bakker also cites “areal patterns” as an “explanatory” factor. However, neither of these writers gives any indication of what mechanism or mechanisms might be involved in this areal diffusion. It is true, as I said in my paper, that Labov (1994) maintains that in dialect contact situations, mergers tend to spread at the expense of contrasts, something which surely does take place through areal diffusion. But we cannot generalise from this to language contact, which in situations where there is low or no mutual intelligibility is a very different process (Trudgill 2004). So I believe that we need to know about possible mechanisms before we can evaluate Hajek’s claim, in the face of Nichols’ data-based suggestion (1992: 193) that it can be concluded that “contact among languages fosters complexity, or, put differently, diversity among neighbouring languages fosters complexity in each of the languages”; Hajek’s own observation, based on Elbert (1965), that “there is no doubt that language contact through close proximity with non-Polynesian languages has led to phoneme borrowing, through intensive and longstanding childhood bilingualism, as pre-

dicted by Trudgill"; and Bakker's observation that "the phoneme inventory of Michif [...] is the sum of the inventories of [French and Cree]".

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