I have recently enjoyed reading a book by Professor Herbert J. Izzo called *Tuscan and Etruscan*. I am not altogether happy about the title, I cannot accept several of the arguments advanced by the author, and I am going to suggest that he could reasonably come to conclusions different from those he has arrived at. But first I should like to say that I think that *Tuscan and Etruscan* is a work that deserves to be read by all who are interested in the spoken language of Italy: it contains material relevant to consideration of a curious phenomenon of Tuscan phonology, and the author’s method and emphasis raise questions which take us well beyond the linguistic problem with which he is concerned.

I

Italianists who are familiar with Florentine and Sienese pronunciations of the expressions which we write as *poco* or *la casa* or *una Coca-cola* will have come across the essential fact underlying a controversy which has now lasted for generations; the rest is mainly speculation. This fact is that over a considerable part of Tuscany (but not all of it) there is fricativization of /k/ in intervocalic position. In various places within this

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1 A lecture delivered in the John Rylands University Library of Manchester on Wednesday, the 12th of March 1975. I wish to thank Prof. Giulio Lepschy for his helpful comments on the lecture; this printed version is dedicated to him as a further contribution to a friendly discussion.


3 The fricativization occurs only in single intervocalic consonants. Outsiders, misled by the spelling of Italian, sometimes expect it where it does not occur, e.g. in *a casa* < *ad casam*, where the spelling of Italian does not show Tuscan doubling [kk] following the assimilation of *d*. Maps showing the distribution of the phenomenon are to be found in Izzo’s book, as well as in R. A. Hall, “A note on *gorgia toscana*” in *Italica*, xxvi (1949), 64-71; W. von Wartburg, *La fragmentación lingüística de la Romania* (Madrid, 1952); and G. Devoto and
region and at various times over the last four hundred years both [x] and [h] have been recorded, while in some localities the affected consonant has been eliminated. This phenomenon has frequently been called aspiration (a habit which Professor Izzo deplores and considers responsible for a great deal of confusion), as well as fricativization, spirantization or gorgia toscana. It is accompanied, over part of the area concerned, by fricativization of /p/ and /t/. Are these pronunciations to be attributed to the influence of an Etruscan linguistic substratum? Professor Izzo notes (p. 112) that of the ninety-nine publications in which he has discovered some treatment of this question, an affirmative answer is found in fifty-seven, neutrality in twenty, and doubt or denial in twenty-two. He ranges himself unequivocally with those who deny substratum influence.

The question is an appallingly difficult one.

If we want to analyse Latin, we can find in the copious literature in that language abundant material for the study of its grammar and vocabulary. We can read the testimony of ancient

G. Giacomelli, I dialetti delle regioni d'Italia (Florence, 1972), where the relevant map is the work of Luciano Giannelli and represents his research on the distribution. A very useful article for the understanding of the phenomenon is that of A. Castellani, "Precisazioni sulla gorgia toscana" in Boletim de filologia, xix (1961), 242-61.

Professor Izzo’s objection, of course, is to those who persist in using "aspiration" in the sense of fricativization, e.g. in speaking of the voiceless velar fricative [x]. He writes that "Castellani, in the first chapter of Nuovi testi fiorentini (1952), is the first Italian linguist (and, until Contini 1961, the only one) to refer consistently to the Tuscan spirants as spiranti. Castellani reserves aspirazione for real aspiration, which he has observed in the 'double' and post-consonantal voiceless stops of Florentine but, curiously, of no other dialect, either Tuscan or non-Tuscan..." (p. 81). However, in view of traditional usage, the employment of aspirazione by Italian writers is quite understandable.

grammarians. And when we consider the problems posed by the evolution of spoken Latin, we can learn a good deal from the modern languages that have evolved from it. But we know of no living language that has evolved from Etruscan, and we have no Etruscan literature. We have a large number of inscriptions, but they are nearly all very short. Many of them are funerary and repeat the same few words. As a result of the paucity of material in the language, we are unsure of the meaning of much of what we do have, and in many instances we are uncertain of the grammatical function of the Etruscan words that we read. Luckily, the Etruscans left us a number of model alphabets, but these disconcertingly show that the alphabet varied according to locality. Unfortunately, we do not know to what extent local variations in alphabet corresponded to local variations in dialect, and scholars have only a very limited amount of material to use as evidence of the changes that took place in Etruscan over the centuries during which it was spoken in Italy. Moreover, our comparative ignorance in the linguistic field is matched by historical and archaeological uncertainty. As Professor Banti put it in the opening sentences of a recent book,

Etruria, the region of ancient Italy that was inhabited by the Etruscans, corresponds neither to modern Tuscany nor to the Etruria of Roman times.

We have no properly clear picture of just where the boundaries of Etruria were until 27 B.C., when the Emperor Augustus reorganized Italy for administrative purposes. The northern boundaries were then placed at the river Magra and the Apennines; those to the east and to the south at the Tiber for almost all its course. But the Etruria of 27 B.C. was the seventh region of Roman Italy, and not the Etruria of Etruscan times . . .

It will be seen, therefore, that there is bound to be a great deal of doubt surrounding any discussion of the distribution of Etruscan speakers, as well as room for dispute concerning the sounds which the symbols of the Etruscan alphabet are likely to have represented.

But even on the Italian side all is not plain sailing. We do not know as much as we should like to about the history of

Italian pronunciation (particularly before the sixteenth century, when we have the first useful treatises on the subject), with the result that even the antiquity and distribution of the Tuscan sounds to which we have referred are themselves matters of opinion.

II

I suppose that one’s own reaction to the *gorgia toscana* is likely to be conditioned by one’s own linguistic habits in the languages one already knows. To some it has seemed so charming that they have suggested its adoption by non-Tuscan speakers of Italian. Petrocchi, in his *Grammatica della lingua italiana* of 1887, prescribed it (as Professor Izzo reminds us) in the following words:

> Le lingue più belle anno tutte delle lettere aspirate, che sono una gentile sfumatura, un dolce passaggio di suoni tenui tra i forti, che fanno armonia. L’italiano à il C. [C is Petrocchi’s symbol; he called it *chi*.] Insigni filologi parlarono in favore del *chi* aspirato, che oltre alla sua espressione delicata, salva da molti equivoci. Come tu impari l’aspirazione nel tedesco, nel greco, nello spagnolo, ecc., procura di imparare anche la nostra, se non l’èi per natura.

Not so Georges Millardet, who wrote in his *Linguistique et dialectologie romanes* (1923):

> M. Grammont a écrit (R. ling. rom., lx. 438), “une langue qui gaspille du souffle à des aspirations, des efforts à des exclamations, dernier reste du cri animal, est à un degré de civilisation inférieure. Le français qui est sans aucune espèce de contestation possible la plus affinée de toutes les langues du monde, ne connaît pas d’aspirations et n’a pas non plus d’exclamations . . .” Voilà pourquoi l’h aspirée par laquelle le peuple et la petite bourgeoisie de Toscane remplacent les k initiaux en position syntactique *questa hasa hosta troppo*, ne risque pas de devenir la prononciation correcte dans une langue phoniquement aussi polie et délicate que l’italien.

I remember that, when I first came aware of the *gorgia toscana*, it did not seem either offensive or strange to me, although I was used to a kind of Italian from which it was entirely absent. This may well have been due to my own linguistic substratum. In Welsh, the word for “bread” is *bara*; “and” is *a*; and “cheese” is *caws* (with initial [k]). But “bread and cheese” is *bara a chaws*. And the *ch* represents a voiceless velar fricative [x]. Words starting with *p* [p] and *t* [t] are similarly treated,
pen becoming phen, where ph represents [f], and tân changing to thân, in which th stands for [θ]. The contexts in which fricativization of /p/, /t/ and /k/ occurs are summarized in Welsh grammars under the heading Aspirate (or, more rarely, Spirant) Mutation.¹ I have often wondered during the last twenty-five years, as I have perused, or listened to, numerous contributions to the Etruscan controversy, why this close and obvious parallel is not examined either by those who present the phenomenon as if it were extremely strange and therefore by implication non-Indo-European and probably Etruscan (for whom see Izzo’s interesting section on “the argument from oddness”), or by those who, in order to refute that view, mention linguistic developments which have far less affinity with the sounds under discussion. I have come sadly to the conclusion that some of those who write on the problem of substrata in the Romance languages may be doing so in substantial ignorance of the group of languages which could cast light on the most widespread of the substrata that may have affected the Romance languages: the Celtic.² I am not, of course, arguing here that the particular

¹ See S. J. Williams, Elfennau Gramadeg Cymraeg (Cardiff, 1960), pp. 231-6, and, for Middle Welsh, D. Simon Evans, Gramadeg Cymraeg Canol (Cardiff, 1951), pp. 9-13. An interesting book devoted entirely to the mutational system is T. J. Morgan, Y Treigliadau a'u Cystrawen (Cardiff, 1952). For comparison with the other Celtic languages, see H. Lewis and H. Pedersen, A Concise Comparative Celtic Grammar (Göttingen, 1961).

² Contini (op. cit. pp. 266-7) does get as far as mentioning Celtic fricativization, but only in a dismissive reference. He writes:

D'altronde, omesso quanto nell’obiezione spetta all’insignificanza della variante combinatoria, sul piano dell’evoluzione fonetica generale, se la figura dell’assimilazione è triviale, la sua realizzazione al contrario è quanto mai peregrina. Non sembrano citabili altri casi di spirantizzazione delle sorde verificatisi in condizioni identiche. Lasciamo stare l’etrusco, la cui differmità è stata segnalata abbastanza. Ma la mutazione consonantica del germanico s’inquadra in una rotazione universale delle occlusive, è legata alla posizione dell’accento e soprattutto non spetta alle sole intervocaliche. La spirantizzazione celtica, certamente assai più vicina, tocca secondo le regioni alle semplici o viceversa alle geminate ed è pertanto in rapporto con la riduzione di queste ultime. La coppia di sorda e sonora, diciamo di k e g, è fondamentalmente intatta in toscano; ma dov’essa viene travolta, scompare (in quanto tale) la sonora, non la sorda; e i fonologi adducono il dato ben noto delle lingue senza g, dall’olandese al ceco (che è poi la punta più occidentale di situazioni biancorusse e piccolorusse). Ergo: anche la
mutation from insular Celtic that I have used as a parallel was in continental Celtic in similar contexts at the time of the romanization of northern Italy; in fact, it is probably a later phenomenon than some other mutations which I shall have reason to mention.

But, incidentally, I should add at this point that we are in a stronger position in studying Celtic than we are in trying to make out the meaning and phonology of the little Etruscan that we have, and that, before we come to any "general" conclusions about substratum influence in the Romance languages, it would be advisable to consider areas where the evidence is considerable, not just areas where it is slight. We might then be spared such statements as the following from the publisher's blurb to Professor Izzo's book (for which I trust he is not responsible):

This study will interest Romance linguists, especially historians of the Italian language; but it will also interest historical linguists in general, for by disproving one of the most plausible and best-documented cases of alleged substratum influence, it casts doubt on many other cases where such influence has been claimed with little evidence.

In the days when I was a schoolboy, "best" was still the superlative of "well", and the alleged Etruscan substratum influence is certainly not well documented, except in the unhappy sense that many people have expressed an opinion on it on the basis of slender evidence. Moreover, if I understand Professor Izzo's book aright, he does not find the Etruscan hypothesis very plausible. It is certainly not one which should lead us to prejudge any other substratum theory or lead us to any "general" conclusion.

III

The first chapter of Professor Izzo's book begins with a useful historical survey of works which contain information on the aspects of Italian pronunciation under discussion. I am
glad to note that he has made good use of the important contributions made in the sixteenth century to our knowledge of Tuscan pronunciation by Rhys and Bartoli. I should not, however, be as ready as he is to believe that [6] and [כ] did not then exist; I consider it possible that, owing to their more limited distribution or simply to the fact that they made less impression on witnesses than [x], they may have existed without mention. ¹ I also do not think we are justified in drawing any conclusions about the age of [x]: the fact that it was not mentioned before the sixteenth century may well be due simply to the fact that before then we had no systematic treatise on Italian pronunciation. Rhys makes clear in 1569 that by that time it was clearly to be heard in those cities where he thought the best Tuscan was spoken ("Densa aspiratio familiaris admodum pistoriensibus, ac Senensibus est, omnium autem familiarissima Florentinis"), and that suggests to me that by that time it was widespread.² But I do not, of course, wish to argue from that that the phenomenon is, or was, necessarily stationary.

Professor Izzo’s examination of the historical evidence merges into an account of the views of those who made early contributions to the Etruscan substratum controversy. It is interesting to learn that the theory goes back to Carl Ludwig Fernow (1808), and was not invented by Heinrich Nissen in 1883, as has not infrequently been stated. It is good, too, to have one’s attention drawn to the utility of Freeman Josselyn’s *Études expérimentales de phonétique italienne* (1901). In this chapter, however, I was not happy with the treatment of the views of twentieth-century scholars, particularly with the rapid summary of dozens of contributions on pages 76-78, e.g. "Bertoldi 1945 (energetically supports the substratum hypothesis); Merlo 1946 (repetition of previous arguments); Bolelli 1946 (supports Merlo’s hypo-

¹ It is very striking that a great linguist like Ascoli, for instance, should have mentioned the fricativization of /k/, but not that of /p/ and /t/, in his classic article on the Italian dialects in the 1880 edition of the *Encyclopædia Britannica* (later revised and republished as "L’Italia dialettale" in the *Archivio glottologico italiano*, viii (1885), 98-128).

thesis)... and so on. What Professor Izzo says here is: "The content of Chapter 3, below, makes analysis of the above works unnecessary here." Certainly, he could hardly be expected to give full accounts of them all, and there is a great deal of repetition in them. But some of them were very persuasive works (for or against) by very able linguists (Devoto, von Wartburg, Rohlfs, Castellani, Hall, to name but a few). And I think that in some cases the arguments they used were more persuasive in the order in which they presented them than are the elements from their arguments which Professor Izzo presents to us (sometimes, I feel, rather in the position of skittles to be knocked down) in his third chapter. Nor do I see the usefulness of his statement that the late Professor Merlo "more than any other linguist deserved to be called a substratomaniacl" (p. 74). If we start trying to determine the comparative gravity of the mental affliction visited upon each one of us by pre-occupation with the substratum, we may find that the task requires even greater equanimity in judgement than assessing the Etruscan theory.

In the second chapter Professor Izzo gives us an account of his own investigation of the present distribution of the fricativization of /p/, /t/ and /k/. The results are clearly summarized in a good map on page 99. Another map in the third chapter (p. 128) enables us to compare the distribution of the *gorgia toscana* with the area of the territory believed to have been occupied by the Etruscans "immediately prior to their Romanization". (But I think Professor Izzo is being rash when he asserts (p. 127) that "... it is reasonably certain that few significant groups of Etruscan speakers can have lived outside the territory indicated on the map"; we simply do not know the linguistic history of the northern Etruscans expelled from, or absorbed into, Gaulish territory.) The maps show that a very large area of "Etruscan territory", from Rome to southern Tuscany, does not now enjoy fricativization of /k/. There is compensation for this at the other end: fricativization goes well above the Arno, north of the territory here presumed to be Etruscan. An interesting feature, emphasized by Professor Izzo in that part of the third chapter concerned with the
geographical aspect, is this: the Etruscan population seems to have been most concentrated in Southern Etruria and thinnest in the north. This suggests that the *gorgia* has flourished where the Etruscans were thinnest and is non-existent where they were thickest. As long ago as 1930 Rohlfs attacked the territorial basis of the Etruscan substratum hypothesis, as Professor Izzo reminds us; he himself has now done so more thoroughly. Of course, we must not jump to the conclusion that this kind of argument is by itself conclusive: there is no reason why a tendency in pronunciation acquired from speakers of Etruscan and transmitted to Tuscan should not provide the basis for a fashion in pronunciation later imitated by people whose ancestors never spoke Etruscan. On the other hand, the facts of the present distribution are bound to raise questions in our minds.

I consider the section on the geographical argument to be the best part of Professor Izzo’s third and final chapter (which is entitled “On the attribution of the *gorgia toscana* to Etruscan substratum influence”). The other sections tackle “the argument from oddness” (where he does a fairly good job of refuting arguments that the *gorgia* is unique, or at least so anti-Romance in character, that we should assume an Etruscan, or non-Indo-European, origin), the evidence for the existence of aspirates in Etruscan, the relationship of Etruscan aspirates to Latin stops, the relationship of “Etruscan aspirates and Tuscan spirants” (I preserve here his terminology) and the chronological aspects of the problem, before giving us his conclusions. The chronological section is, I think, his weakest. He himself begins one paragraph with the words: “Another bit of evidence of the same sort, perhaps even weaker than the preceding but still, I believe, worth mentioning...”. There seems to me a good deal of argument here but comparatively little solid evidence.

In fact, the linguistic sections of the third chapter seem to me a mixture of fair argument and good attempts at discovering new evidence (like the examination of the Greek rendering of Etruscan toponyms) with poor debate and refutation of trivial arguments. If Izzo had here only summarized the sound information he had collected and then concluded that the Etruscan substratum could only be considered an unproven possibility, I think he would
carry most of us with him. But he apparently wants to do more than that: he wants to solve the problem “disproving this hypothesis and providing a definitive conclusion to the debate” (as the blurb has it), and this seems to me to have led him into unconvincing reasoning, polemical attitudes and unprofitable indignation. The Etruscan substratum theory can be “definitively disproved”, I think, only if we can prove beyond doubt that Etruscan was without the sounds we have been discussing (or tendencies which could give rise to such sounds), or if we can establish beyond all doubt that Italian was once spoken throughout the relevant region without any trace of gorgia toscana (or any derivative of Etruscan which could develop into it). Since we can do neither of these things, we can only weigh the various hypotheses and try, with the calmest judgement of which we are capable, to judge which have some degree of probability. Now the desire to settle the matter seems to me to have led Izzo to advance some arguments which I find singularly unconvincing. Take, for instance, his treatment of vocabulary. Presumably, one can expect Tuscan vocabulary to be influenced by Etruscan only through those Etruscan words which passed into Latin and then became Tuscan. Here is the summary of the position given by B. Migliorini:

Il latino ha attinto all’etrusco qualche centinaio di parole, di cui parecchie penetrò profondamente nel lessico e sopravvisse (populus, persona, catena, taberna, ecc.), altre scomparve nell’uso parlato e, se mai, rientrò in italiano come latinismi (spurius, atrium, idus, histrio, mantisa, ecc.).

Probabili relli etruschi sono alcuni nomi toscani di piante (brenti, gigaro, ilatro, napa).¹

Even if we were to accept Izzo’s suggestion that it is strange that the local survivals should not be more numerous (though I do not see why, given that so many Etruscan words gained wider diffusion through becoming Latin words), I cannot follow him in the claim that he then makes. It is that, if we accepted the hypothesis that the gorgia is based on an Etruscan substratum,

Tuscan would constitute the first example of a language fundamentally altered in its phonological system by the influence of a substratum language without having been influenced by the substratum language in its lexicon (p. 175).

My objections to this are three. First of all, to know that "Tuscan would constitute the first example of a language fundamentally altered in its phonological system by the influence of a substratum language without having been influenced by the substratum language in its lexicon", one would have to know all the world's languages and their substratum languages, however long since dead. Secondly, is it not exaggerating just a little to refer to Tuscan as "fundamentally altered in its phonological system" by the gorgia toscana? Not all Tuscany has the gorgia, and where it does occur, it is of phonetic, but not phonemic, significance. Thirdly, since there are Etruscan words in Tuscan, Tuscan has been influenced in its vocabulary. I regret passages of this kind, for they disfigure a useful book, just as I regret what seems to me the exaggeration of the reference to "the Ascolian substratists, who think that pronunciation remains practically unchanged over the millennia even where a group gives up one language for another" (p. 31). If the phrase "Ascolian substratists" refers to specific persons who have made such a declaration of faith, we had better know who they are. If the phrase refers to all who have learned something from Ascoli's teaching on substratum, it is untrue. These lapses are, I suppose, the result of over-eagerness, the desire to come now to a "definitive conclusion to the debate". But in the Etruscan question, proof is something we cannot just insist upon; our research must first tell us more about Etruscan and the Etruscans. And our ignorance in that field is so appalling that our readiness to come to definite conclusions is quite astonishing.

But I should not like to end my remarks on this book with words on its defects. I found it stimulating and instructive. And I should like to conclude by drawing attention to one of its positive aspects, Professor Izzo's own suggestion of a possible explanation of the gorgia:

The gorgia sounds can be considered the result of a weakening of the articulation of occlusives. This change does not require the postulation of an intermediate aspirate stage. Tuscan Italian distinguishes single from double
consonants, and in the realization of this contrast there is a difference in force of articulation, the double consonants being tenser than the single. Like a number of other languages using similar contrasts, Tuscan has, in part of the system, changed the contrast between lax and tense occlusive to a contrast between spirant and occlusive. This development appeared first in the change of [-c-] to [-š-] in the fourteenth century. Later [-k-] became [-x-] and still later [-t-, -p-] became [-θ-, -φ-]; and there seems to be a more recent and less marked tendency to spirantization of /-b-, -d-, -g-/. But all stops retain their occlusion when preceded by other consonants. The Tuscan development roughly parallels, at a distance of centuries, sound changes of the western Romance languages, where intervocalic voiceless single stops became voiced spirants (Latin ripa, French rive), but double stops and stops after other consonants remained stops (Latin mittere, Spanish meter; Latin quantu, Spanish cuanto). The Tuscan developments require no assumption of exotic influence to account for "non-Romance" sound changes. And, whereas the substratum hypothesis assumes that the gorgia sounds must go back to the Etruscans and therefore be 2000 years old (in the Romance speech of Tuscany), it appears rather that they are part of a relatively recent phonological shift that is still in progress (pp. 175-6).

I cannot here accept the remarks pertaining to chronology; I certainly do not think we are entitled to assume that the gorgia sounds are necessarily recent. But I think the phonetic explanation is worthy of serious consideration. I shall revert to it in due course, but not in order to draw the same conclusion. Moreover, I think the point about the spirantization of /b/, /d/, /g/, is worth emphasizing; but again, I do not believe we have enough evidence to warrant our coming to any conclusions in connection with the chronology of such fricativization.

IV

I have already hinted that I am not altogether happy with the title of Dr. Izzo's book. It is a book about the gorgia toscana and related matters (including the archaeological background). But I wondered at the end if it was right to assume that this was the same as the whole history of the relationship of Tuscan to Etruscan (if, indeed, it has anything to do with it). So I asked myself: if this is not all that story, how would you investigate the possible influence of a substratum language on a modern dialect or group of dialects, like the Tuscan?

I think I should begin in a different way. I should start with an examination of the modern dialects and dialect boundaries. Here, so far as Tuscan is concerned, I do not think
I should find myself at variance with the traditional classifications of the Italian dialects. Next, I should classify the dialects which border on the Tuscan dialects. I should then list the linguistic features on which I based my classification. In other words, I should ask myself: what makes the Tuscan dialects so different from their neighbours (or the neighbouring ones so different from the Tuscan) that we put them in different categories? After that, I should ask myself to consider what we know of the possible linguistic substrata, not only of Tuscany, but of the neighbouring dialects. Finally, I should try to relate our findings on the distinctive features of the dialects to our knowledge of the possible linguistic substrata, not only of Tuscany, but also of the neighbouring dialects, paying particular attention to areas of transition.¹ It would be a long and arduous undertaking, but not, I suspect, without profit. Here I shall limit my remarks to consideration of a small aspect of it, in order to make some observations on the phenomena which Professor Izzo chose as the subject of his book.

V

If we go south and east from the area occupied by varieties of Tuscan, we shall find ourselves examining dialects in which scholars have suspected Osco-Umbrian substratum influence; with them I am not here concerned.

If we go north, we shall find ourselves in the territory of the Gallo-Italian dialects. One of their most obvious characteristics, which they share with other kinds of Western Romance, is the voicing of Latin voiceless intervocalic consonants. The student of Celtic is reminded of the system of initial mutations in the Brittonic branches of Celtic. Welsh grammarians, for instance, divide their mutations into "soft", aspirate, and nasal. The system of "soft mutations" can be summarized thus:

The mutable consonants are: p b m
          t d l l
          c g rh

¹ For classification of the Italian dialects, see the useful handbook by C. Grassi, Elementi di dialettologia italiana (Turin, 1967). On the problems and methods of Italian dialectology, we now have the important first volume of M. Cortelazzo's Avviamento critico allo studio della dialettologia italiana (Pisa, 1969).
A PROBLEM OF LINGUISTIC SUBSTRATUM

SOFT MUTATION

(1) \([-\text{voice}] \rightarrow [+\text{voice}]\)
thus \(p \rightarrow b\)
\(t \rightarrow d\)
\(c \rightarrow g\)
\(rh \rightarrow r\)

(2) \([+\text{voice}] [+\text{fricative}]\)
but (i) \([+\text{velar}] [\emptyset] \text{(zero)}\)
(ii) \([+\text{nasal}] [-\text{nasal}]\)
(iii) \([+\text{bi-labial}] [+\text{labio-dental}]\)
thus \(b \rightarrow v\)
\(d \rightarrow \delta \text{ (dd)}\)
\(g \rightarrow \gamma \rightarrow [l]\)
\(m \rightarrow \tilde{v} \rightarrow v \text{ (f)}\)

This system, of course, goes back to a period when the vowel at the end of the word preceding the mutation still existed, and the mutation was then intervocalic. Such intervocalic mutation has also occurred internally in the development of Welsh from Common Celtic. The parallelism with Western Romance has been carefully examined by A. Martinet in his stimulating article, *Celtic Lenition and Western Romance Consonants*.² Although Martinet is careful throughout not to reject the possibility that Celtic lenition and Western Romance consonantal development resulted from parallel evolution determined by structural analogy, the evidence he adduces makes it clear, in my view, that the parallel features are so numerous and so close and the geographical distribution so nearly co-extensive, that it would strain credulity a good deal less to ascribe the Western Romance changes to Celtic substratum influence than it would to assume coincidental development throughout the series he examines. Since Martinet’s article is readily available and full of interesting

¹ I have here quoted from the tables on Welsh phonology prepared for his pupils by Dr. Emrys Evans, Reader in Celtic Studies at Manchester; the information on Irish on page 132 is another extract from the same source. I am grateful to Dr. Evans for allowing me to do this and for his readiness to discuss Celtic problems with me.

² A. Martinet, "Celtic Lenition and Western Romance Consonants" in *Language*, xxviii (1952), 192-217. A later version of the article, translated into French, is to be found in the same author’s *Économie des changements phonétiques* (Bern, 1955), pp. 257-96.
material, I shall not try to summarize it here, particularly since only part of the system is of interest from my point of view in this article. I should, however, like to draw attention to the following conclusions of his, which I should like the reader to consider in relation to Professor Izzo's independently evolved phonological explanation for the fricativization that occurred in Tuscan intervocalic consonants:

Our conclusion is that in both Celtic and Western Romance, genuinely consonantal phonemes went through a phonological process in which we distinguish the following successive steps:

1. Under the pressure of geminates, whose articulation tended to relax, intervocalic single consonants were weakened to some extent, so that in contexts of great aperture every consonantal phoneme developed a clearly distinct positional variant. This variant was not restricted to word-medial position but appeared wherever the phonetic conditions of its appearance were present in the utterance.

2. The geminates having been reduced to single strongly articulated consonants, the unweakened allophones of every consonantal unit became phonemically identified with the reflex of former geminates, and no longer with that of intervocalic single consonants. This phonemic reshuffling must have determined, at least in the case of liquids and nasals, a phonetic identification of the reflexes of former geminates and those of the corresponding unweakened single consonants, whereby the latter received an energetic articulation. When -olo- became -Olo-, then os lo and -oslo (with unweakened l) became -os Lo- and -osLo-; whereas -o lo- and -olo- (with weakened l) did not change. In other words, an Indo-European or Latin word beginning with l must have been pronounced with strong L after a pause or after a word ending in a consonant, but with weak l after a word ending in a vowel. Word initial was thus a position of neutralization for the opposition L/l, since the use of [L] or [l] was there determined by the context.

3. In the next stage, Insular Celtic on the one hand and Western Romance on the other are found to diverge. In Celtic, widespread syncope and apocope completely modified most of the contexts, so that the use of L and l became distinctive also in word-initial position and acquired morphological significance. In Western Romance the phonemic stability of word initials was restored by the analogical extension of one and the same phoneme to all syntactic situations.

I believe that both Martinet and Izzo have independently put their finger on the essential point in similar sets of phenomena: a system of contrastive strong and weak consonants. It seems to me reasonable to consider that the Western Romance system could have arisen from a tendency in Common Celtic, which later manifested itself in the development of lenition in

1 A. Martinet, op. cit. p. 212.
the insular Celtic languages. But I think that the best description of what probably occurred is to be found, not in quotation from either Izzo or Martinet, but in Professor Jackson's account of lenition:

This is the mutation whereby intervocally, whether in the interior of a word or initially when preceded in a close speech-group by a proclitic ending in British in a vowel, and in general terms internally between vowels and sonants, the British single voiceless stops \( p, t, c \) became voiced to \( b, d, g \), and the single voiced stops \( b, d, g \), as well as \( m \), became the spirants \( \delta, \tilde{d}, \tilde{z}, \) and \( \mu \). The \( \mu \) subsequently gave \( v \), while \( \tilde{z} \) went on later to nil in W. (in certain cases \( j \)) and \( \chi \) in CB. With Late Brit. \( l, r, n = L, R, N \) the only result of lenition was a weakening of articulation; the non-lenited \( L \) at a later date developed into \( \lambda \) in W., and the non-lenited \( R \) into \( \delta \) in W. with traces of something similar in C. and B.

There is no need to enter here into the causes of lenition, except to say that it evidently arose from a loosening and slackening in the articulation of the CC consonants in certain positions...

The close similarity of the phenomenon of lenition in Goedelic and Brittonic suggests that it is based upon a similarity in the consonant systems of the two groups going back to the Common Celtic period. According to Pedersen (e.g. VKG. i. 242) Common Celtic possessed an aspirate \([t']\) etc. where there is now not lenition, and mere \([t]\) etc. where there is now lenition. The recent work of the Abbé Falc'hun on Breton phonology suggests, however, to the present writer a somewhat different but analogous situation in Common Celtic. Falc'hun has shown that Breton has a system by which a series of consonants with long articulation is opposed to another series with short articulation, of approximately only half the length of the others; and it appears that the consonants which are, historically speaking, lenited are of the weak variety and those which are non-lenited are of the strong variety. Thus, for example, the lenited \( b \) in \( e \) baz, "his cough", from paz (W. ei bas, from pas), and the lenited \( l \) in \( e \) leur, "his floor", from leur (W. ei lawr, from llawr), have approximately only half the articulatory duration of the non-lenited \( p \) in paz or the non-lenited \( b \) in bac'h, "hook" (W. bach) and the non-lenited \( l \) in leur. Similarly, internal \(-b-,\) which is the lenition of \(-p-,\) and internal single intervocal \(-l-,\) have in Breton approximately half the duration of internal originally geminate \( p \) arising from Pr.B. \( b+h \) as in glepa, "wettest" (MW. gulyppaf), and of internal or final geminate \( l l \) as in toullou, "holes" (W. tyllau). These distinctions are mostly ignored in the written Breton language, though they appear clearly on the kymograph and are obvious to native speakers when their attention is drawn to them.

One may propose, therefore, that the Common Celtic language may have had some such system whereby consonants in absolute initial (a position liable to have special articulatory force), internal geminates of whatever origin (e.g. \(-mm- \prec -sm-)\), and internal consonants in certain homorganic and other groups such as the \( l \) in \(-lt-\) or the \( t \) in \(-xt-\), were comparatively long sounds, and consonants initially after proclitics ending in vowels, or internally when single between vowels or in combination with certain other consonants, as e.g. \(-rb-,\) were comparatively short sounds; the duration of the former group being approximately double that of
the latter group. In absolute initial, however, they were probably not as long as full geminates, since true geminate \(pp\), \(tt\), \(cc\), whether internal or arising through the spirant mutation later gave \(f\), \(th\), and \(ch\), whereas initial non-lenited \(p\), \(t\), and \(c\) remained. We may speak of them for present purposes as half-long, writing e.g. \(m(m)\)-, \(b(b)\)-, though in Breton they seem to have fallen together later with the fully long geminates in \(mm\)-, \(pp\)-, etc.; in any case they were long enough to resist the subsequent weakening which attacked the short consonants and which we call lenition. The Common Celtic use of e.g. \(-m-, -b-\) after a proclitic ending in a vowel was not of itself lenition, as is shown by the fact that internally -\(mm\)-, -\(bb\)-, etc., were incapable of lenition; it was simply that the secondary CC. strengthening of IE. consonants in absolute initial to e.g. \(m(m)\)-, \(b(b)\)- did not occur in this quasi-initial position, any more than it did with single intervocal IE. \(-m-, -b-, etc., within the word.

It is the subsequent weakening just referred to which is the phenomenon known as lenition. What seems to have happened is that at a certain stage yet to be determined the CC. short consonants, being mostly intervocal, underwent a loosening or weakening of articulation which resulted in the voiceless stops \(p\), \(t\), \(c\) becoming voiced to \(b\), \(d\), \(g\); the voiced stops \(b\), \(d\), \(g\) becoming the spirants \(\theta\), \(\delta\), \(\gamma\); \(m\) giving \(\mu\); and tense \(L\), \(N\), \(R\) developing into ordinary \(l\), \(n\), \(r\). The long consonants, however, whether intervocal or in absolute initial, were energetic enough to resist this loosening and remained unaffected at first; though later and as a quite separate evolution -\(pp\)-, -\(tt\)-, and -\(cc\) became \(f\), \(th\), \(ch\), and -\(bb\)-, -\(dd\)-, -\(gg\)- were simplified. The half-long consonants in initial position have lasted to the present day in Breton, being now fully long, but in Welsh they were subsequently shortened.¹

¹ K. Jackson, *Language and History in Early Britain. A Chronological Survey of the Brittonic Languages First to Twelfth Century A.D.* (Edinburgh, 1956), pp. 543-6. Professor Jackson’s book first appeared in 1954, and he had time only to add a note concerning Martinet’s article (p. 710). I accept his statement that some of what Martinet says needs modification in the light of a wider knowledge of Celtic philology; but none of this fundamentally affects the parallelism in the Celtic and Western Romance consonantal systems as presented by Martinet. As Professor Jackson puts it:

> It is interesting to see that Martinet envisages tentatively a system of strong and weak consonants, including the stops, similar in many respects to the one outlined above; most of the differences are comparatively unimportant, though Martinet does not allow for all the contingencies. We both regard initial non-lenited consonants as having fallen together with the original geminates, while differing considerably over details. In the matter of the \(l\), \(r\), and \(n\) sounds I assume that in CC. they were all one degree stronger in articulation than Martinet does (giving, e.g., \(LL:L\) where he has \(ll = L:\!l\)), for reasons which he did not take into account.

As for our knowledge of continental Celtic, a useful recent survey is that by É. Bachellery, “Le celtique continental” in *Études celtiques*, XIII, i (1972), pp. 29-60. A very valuable and thorough study of continental material is to be found in D. Ellis Evans, *Gaulish personal names: a study of some continental Celtic formations* (Oxford, 1967). For the work on Breton to which Jackson refers, see F. Falc’hun, *Le système consonantique du Breton* (Rennes, 1951).
VI

Why have I drawn attention to the voicing of intervocalic plosives in Western Romance and the possibility of Celtic influence? What has that to do with the problem of the fricativization of /p/, /t/ and /k/, which has been ascribed to the influence of an Etruscan substratum?

I draw attention to it for two reasons. The first is that voicing of intervocalic consonants was not neatly contained by the Spezia-Rimini line; it spilled over into Tuscany, which I consider a transitional area in this respect. The second is this: if we compare the Brittonic and Goidelic branches of Celtic, we find that in certain situations where lenition of /p/, /t/ and /k/ gave voicing in the former (as we can see in Welsh), the latter had fricativization (which we can study in Irish). And even in Welsh, whereas in certain contexts we had the result [b], [d], [g], we had in others [f], [θ] and [x], though this (the development I used simply as an example of a parallel on pp. 115-6) may be a later mutation in Welsh than the “soft mutation” (i.e. voicing). Let us look briefly at each of these two points.

VII

In 1965 R. G. Urciolo wrote a book entitled The Intervocalic Plosives in Tuscan (-P-T-C). In my review of that work I summarized the problem thus:

The development from Latin to Italian of intervocalic plosives has for long presented linguistic historians with a most difficult problem: why should the voiceless consonants of Latin have given such apparently inconsistent results in Italian, where voiced and voiceless continuators exist side by side (fuoco, but luogo; cantato, but strada; ape, but povero, etc.)? Meyer-Lübke’s suggestion that the consonant remained voiceless after a tonic vowel, but was voiced if it occurred before it, was contradicted by too many examples to win acceptance. Nor did Ascoli have much luck with his explanation—that the divergent developments of the consonants should be explained either by the nature of the neighbouring vowel (e.g. that a favoured voicing, as in ago) or by differences in the history of individual words (e.g. that luogo derived from an accusative, while fuoco might come from a nominative *focs). The development of linguistic geography, of course, provided plenty of evidence to show that northern Italy preferred voiced forms while southern Italy stuck to voiceless ones. But this did not solve the problem, since the Spezia-Rimini line did not provide a clear frontier; Tuscany, and consequently the literary language, presented a mixed
bag. Pieri collected a great deal of evidence in support of the view that the
indigenous Tuscan forms were voiceless and that the voiced forms in Tuscany
were due to northern influences or borrowings from Gallo-Romance dialects.
Merlo and Bottighioni came to a very different conclusion: that the genuine
Tuscan development was a voiced consonant (*ago, *lago, *luogo, *spada, etc.) and
that words with voiceless intervocalic plosives were Latinisms. Pieri's view is
today widely accepted. It won the adherence of Bartoli. It has also been
consistently held by G. Rohlfs and is incorporated in his extremely influential
Historische Grammatik der Italienischen Sprache (Berne 1949-54). Rohlfs points
out that, if Merlo was right in maintaining that "il continuatore toscano della
sorda latina . . . è una sonora", then words like *dico, *amico, *nipote, *capo, would
have to be considered Latinisms, as well as all verb endings which included
voiceless consonants (*sapete, *potuto, etc.). The writer of the present book,
R. G. Urciolo, a pupil of Merlo's, makes a determined effort to show that Pieri
was wrong. He discusses a very large number of examples, taken from widely
varying sources (texts of various dates, the AIS, his own knowledge of Italian
dialects, etc.). It should be said at once that his book has several disconcerting
features. 1

I then criticized some of Urciolo's procedures and suggested that
some of his examples were of doubtful validity. But I went on
to say this:

Yet, when one has considered all the faults of this book, one must still admit, I
think, that there are enough valid examples in it to suggest that the nature and
number of words in Tuscan which have voiced consonants are such that they
cannot all be northern borrowings.

I also drew attention to some interesting remarks on the subject
by Arrigo Castellani, which I should like to quote here:

Mi sia permesso di citare quello che scrivevo a conclusione d'un lungo capitolò
dedicato a *p, *t, *k intervocaliche, e di riflesso a *s, in un mio corso di grammatica
storica italiana del 1951-2 (Roma, Facoltà di Magistero, pp. 135-6): "La teoria
del prestito non basta per ispiegare la sonorizzazione toscana (e umbromarchi-
antichi]: e tali voci non posson certo esser venute dall'Italia settentrionale (dove
si diceva *oca, *poco, *recare). Inoltre le parole toscane con sonora intervocalica in
luogo di sorda (supratutto con *g in luogo di *k) sono troppe perché si possa
ammettere che si siano introdotte isolatamente, per via di prestito.— Credo che,
nella maggior parte dei casi si debba pensare ad una sonorizzazione imitativa.
La pronuncia colla sonora dev'essere stata ritenuta, per un certo tempo, la più
elegante. Dal quarto secolo in poi l'Italia centrale superiore è esposta a continui
influssi settentrionali. La capitale dell'Impero è prima a Milano, poi a Ravenna.
La sede del regno ostrogoto continua ad esser Ravenna. Infine, nei secc. VII e

1 In Archivum linguisticum, XVII, ii (1965), pp. 200-2.
VIII la Toscana è in istretto contatto colla Val Padana, culla del dominio longobardo. Sembra probabile che, in questa situazione, il modo di parlare dei Settentrionali, che doveva colpire soprattutto per la sonorizzazione delle occlusive, sia stato imitato in vasti settori della popolazione. Come conseguenza s'è avuta l'adozione della sonora intervocalica invece della sorda in un certo numero di parole (dopo varie oscillazioni, alcune delle quali per esempio frucare e frugare, lacrima e lagrima, son rimaste fino ai nostri giorni)".¹

It may well be that northern tendencies in pronunciation, themselves due to a Celtic substratum, were extended southwards in the manner envisaged by Professor Castellani. We may thus have in Italy, though for different reasons, an extension of the kind Martinet noted in Spain:

Yet we cannot help observing that the geographical distribution of the phenomenon largely coincides with at least some of the sections of Western Europe where Celtic languages must have been spoken around 300 B.C.: northwestern Italy together with the Romansh domain, Gaul, large sections of central and northern Iberia, and the British Isles. On the whole, the contrastive pattern would seem to be found mostly where Celtic speakers either preserved their language or gave it up in favor of Latin. We may of course have to reckon with extensions beyond the original Celtic-speaking domain, for instance in Catalonia and the lower half of the Ebro Valley—not to speak of southern Spain and Portugal, to which the Reconquest eventually brought Northern forms of speech.²

But it may also even be true that the Celtic substratum itself extended a little further south than we usually believe. (M. Lejeune's valuable Lepontica, which contains so much material of interest to Celtic scholars, contains one very interesting inscription from Todi. Although he points out that it has obvious affinities with other material from the region of Novara and suggests that it pertained to people who happened to have come from that area, the fact that it is from Umbria is bound to give us furiously to think.)³ But, however these tendencies came to Tuscany, they were undoubtedly there. On the other hand, certain other obvious features of the Gallo-Romance dialects are completely absent. It is, as I said, a transitional area.

¹ A. Castellani, "Il nesso sì in Italiano" in Studi linguistici italiani, I (1960), 67-68.
² A. Martinet, op. cit. p. 214.
It was Rudolf Thurneysen who proposed the use of the word "lenition" to describe certain consonantal changes characteristic of all insular Celtic languages, and it is useful at this point to recall his definition: "Lenition . . . is the term used to describe a mutation of consonants which normally originated in a reduction of the energy employed in their articulation. It affected not only medial, but also such initial consonants as were closely associated with the preceding word. . . . It is earlier than the loss of vowels in final and interior syllables . . . for it presupposes the continued existence of these vowels."¹ I have already given the rules for soft mutation in Welsh. It will be remembered that part of that table gave the changes affecting /p/, /t/, /k/ as follows:

\[ p \rightarrow b \]
\[ t \rightarrow d \]
\[ k \rightarrow g \]

In Irish we have:

\[ p^- \rightarrow /p/ \rightarrow /f/ \]
\[ t^- \rightarrow /t/ \rightarrow /\theta/ \rightarrow /h/ \]
\[ c^- \rightarrow /k/ \rightarrow /x/ \]

The changes we have tabulated can also be seen in medial position by examining a list of Welsh and Irish words from common Celtic roots, e.g. if we take a couple of random samples from G. Dottin's *La langue gauloise*, we find:

*bracem* acc., farine de choix (Pline, XVIII, 62), dont on fait la cervoise (glose chez Ducange); *irl. braich*, gall. *brag* "malt", bret. *bragez* "germe de blé", v. fr. *brais*.

*litana*, "(silva) vasta" (Tite Live, XXIII, 24, 7); v. gall. *litan* [later written *llydan*] *irl. lethan*, bret. *ledan* "large".²

I have also illustrated the fact that fricativization takes place in certain contexts in Brittonic, as well as in Goidelic (pp. 115–6). I do not think that I need further labour the point that the kind of change we have in the fricativization of /p/, /t/, /k/ in Tuscan is as close a parallel to one result of Celtic lenition as the Western Romance voicing of /p/, /t/, /k/ is to another.


IX

Our ignorance of the exact distribution of Celtic speakers, Etruscan speakers and Celticized Ligurians is daunting. So is our ignorance of the linguistic relations between the regions of northern Italy after romanization and in the period when Tuscan was formed. This should not altogether prevent us from offering possible explanations of the development or spread of linguistic phenomena on the basis of the evidence we have, as Professor Castellani did in the interesting passage I have quoted. But it does mean that any hypothesis can only be of the most tentative kind.

To make it clear that I offer mine with hesitation, I shall put it in the form of a question following the statement of a few facts. The Celtic tendency to lenition has had more than one result in the Celtic languages themselves; we can see that by examining matching sets in the insular dialects. One of those results has a close parallel in the voicing of intervocalic consonants in Western Romance, and it has been suggested that that development may be the result of Celtic substratum influence. There are examples of it in Tuscany, which in this respect is a transitional area. In part of Tuscany, there is another consonantal phenomenon identical with another result of Celtic lenition: fricativization of /p/, /t/, /k/. In connection with this, it is worth bearing in mind that Celtic philologists do not now maintain that all continental

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Celtic was of the p-Celtic type (as we used to call the kind of Celtic to which the Brittonic languages belonged), but stress the likelihood that there were in different places on the continent widely varying forms of Celtic.¹

Is it not just possible that the *gorgia toscana* may be the result of a tendency to lenition which originated in a Celtic substratum operating in a transitional area (which happens to be the area in which Celtic and Etruscan influences meet)?²

I am always prepared, of course, to admit the possibility that what we have in the *gorgia toscana* may be the result of spontaneous phonetic change. What I am not prepared to admit (and what seems to me very unwise to assume) at this stage in our investigation, is that the matter can be regarded as settled.

¹ The present state of knowledge and theory on this matter is summarized by É. Bachellery, “Le celtique continental” in *Études celtiques*, XIII, i (1972), pp. 29-60.

² If we consider the possibility that fricativization of /p/, /t/ and /k/ resulted from a substratum influence, not from spontaneous phonetic change, then it is certainly interesting that the fricativization should have occurred in an area in at least part of which people once spoke a language in which there were, according to Etruscologists, aspirated occlusives. Indeed, according to the consonantal system envisaged by Mauro Cristofani, aspiration in Etruscan was very important, since the difference between surd occlusive and aspirate occlusives was of phonemic significance. (He, like other Etruscologists, believes that Etruscan did not have the voiced occlusives /b/, /d/ and /g/.) But it seems to me that the fricativization we have in the *gorgia toscana*, which is always intervocalic, is (if due to substratum influence at all) more likely to have had its origin in the tendency which led to Celtic lenition, which was also originally intervocalic, than in Etruscan aspiration of occlusives, which occurred in phonetic contexts different from those of the *gorgia*. This difference has already been emphasized by G. Rohlfs, who, when discussing the use in Etruscan of the letters believed to have indicated aspirated occlusives, wrote:


It is worth adding that the fricativization of /b/, /d/, and /g/ is hardly likely to be a totally unconnected phenomenon. Incidentally, I am inclined on the whole to accept Cristofani's view that Etruscan had aspirated stops, since his account of the
Etruscan consonants seems to me more soundly based than Professor Izzo's ingenious attempt at casting doubt on the existence of Etruscan aspirated stops (TuRscan and Etruscan, pp. 135-56). But I would agree with Izzo that in the past Etruscologists have been too ready to assume that they know the value of the symbols used for Etruscan consonants, and I believe that there is still uncertainty here. Nevertheless, Cristofani seems to me to treat the subject much better than his predecessors (cf. A. Trombetti in La lingua etrusca (Florence, 1928)). Cristofani's treatment of the relevant sounds is as follows:

IL SISTEMA CONSONANTICO.

L'adozione dei segni dell'alfabeto greco relativi alle occlusive sorde e alle sorde aspirate fa presumere che in etrusco esistessero le tre seguenti coppie correlate:

- p : ph
- k : kh
- t : th.

Il sistema consonantico greco possiede, come è noto, una serie di occlusive (ossia di consonanti la cui articolazione prevede la chiusura del canale respiratorio) con tre punti di articolazione (labbra, denti, velo del palato). Fra i suoni graficamente notati TT, jS e (p, k, y e x) esiste un rapporto di opposizione definita plurilaterale, che genera fasci di correlazione a tre membri, i quali prendono parte alle correlazioni di sonorità (β, γ, δ) e aspirazione (φ, χ, θ):

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   π   θ   ι
  / \ / \ / \
 β  ω  η  χ  δ  φ
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L'inutilizzazione dei segni relativi alle sonore nell'alfabeto etrusco significa che unica correlazione esistente nella lingua è quella dell'aspirazione con una serie di aspirate marcate (ph, kh, th) e di non aspirate non marcate (p, k, t).

Altrove (ved. p. 9 ss.) abbiamo parlato per i grafemi relativi alle velari, di un sistema semplificato con K per l'Etruria settentrionale, e di un sistema più complesso, in uso nel VII e VI secolo a. C. nell'alfabeto dell'area centrale e meridionale, che prevede l'uso di K difronte ad a, di C (gamma) difronte a e/i, di Q difronte a u.

La scrittura settentrionale potrebbe pertanto definirsi fonologica, nel senso che nota con K il solo fonema /k/ esistente nella lingua, mentre la scrittura dell'Etruria centrale e meridionale può definirsi fonetica nel senso che K, C e Q vengono usati a seconda che precedano la vocale media, palatale o velare.

I due membri delle coppie correlate sono fra loro in rapporto di opposizione; i fenomeni p > ph, t > th, k > kh, sono possibili solo se si ricorre a motivazioni interne all'economia della lingua.

Analisi particolarmente disinvolte dell'etrusco hanno spesso portato a trascurare l'importanza di queste opposizioni fondamentali e a ricondurre a una stessa radice alcune parole in cui occorreva, invece, i due membri delle coppie correlate. Come la correlazione di aspirazione in greco
distingue i due significati di τρέπω e τρέφω, così dobbiamo supporre che ogni variazione di significante, se non è motivata, porta a una variazione di significato anche in etrusco.

In etrusco, infatti, si assiste a una neutralizzazione delle opposizioni segnalate solo in posizioni di contatto interno con liquida o nasale.

'Αλκμήνα > Alxumena
'Ηρακλῆς > Hercle, Herxle
Ancus > Anxe

In alcuni casi l’arcifonema può verificarsi in posizione iniziale:

Περσεως > Perse/Perse
*p urθce > φυrθce
Κάλχας > Xalxas

Il fenomeno qui segnalato non investe soltanto le opposizioni fra aspirata e non aspirata, ma include anche le spiranti.

Pupluna > Fufluna
θυρθα > θυφθα
θαρνα > θα fna

Il passaggio p > ph > f ha alcune volte indotto a pensare che in etrusco ci fosse un fenomeno di «rotazione delle consonanti» riconducibile ad analoghi fenomeni delle lingue indoeuropee o preindoeuropee, ma, come si è visto, questi mutamenti si spiegano soltanto nell’ambito dell’economia interna della lingua. I grafemi p, φ, t, θ, k, χ, o f diventano, ognuno nell’ambito della sede di articolazione che designano, espressioni grafiche che non realizzano fonemi differenti, ma piuttosto una variante grafica di uno stesso arcifonema.

(M. Cristofani, Introduzione allo studio dell’etrusco (Florence, 1973), pp. 48-52.)