

Yupik	Proto-Eskimo	Inupik
p f v m	p v m	p v m
u	u	u
a	a	a
i	e	e
	i	i
c s j	s j n	s j
t ɬ l n	t ɬ l n	t l n
k x g N	k g N	k g N
q X R	q R	q R

Of particular concern in this paper is the Point Barrow dialect of Inupik Eskimo. This dialect we will refer to as Inupiat or Barrow, since that is what it is called in the present day literature and by the speakers of the language. Palatalization does not occur in all dialects but seems to be confined, in the Inupik group, to the dialects of Northern Alaska, probably those north of the Brooks Range. The original question which prompted this paper was whether or not palatalization in these dialects was a rule-governed process. In other words, must we assume the following underlying segments:⁴

p v m
u
a
i
č j L ɬ ñ
t s r l ɬ n
k g N
q R

or can we, by means of a palatalization rule, reduce the list to the following:

p v m
u a i
j
t s r l ɬ n
k g
q R

We would like to claim that palatalization in Barrow is a phonological process through which a coronal segment, in certain environments, becomes a palatal segment after a preceding high front vowel in the underlying form. Expressed in terms of a feature rule, it might be stated as follows:

$$\begin{array}{c} \text{C} \\ [+ \text{coronal}] \end{array} \longrightarrow \begin{array}{c} [+ \text{high}] \end{array} / \begin{array}{c} \text{V} \\ [+ \text{high}] \\ [- \text{back}] \end{array}$$

The problem of rule application is made more interesting by the existence of minimal pairs, for example:

igilua	'his house'	atigitka	'my parkas'
igiLua	'either one'	atigička	'put it on'

igini	'fire'
igiña	'over there'

The existence of these minimal pairs removes the process of palatalization in Barrow from the realm of surface phonetic assimilation and places it in the phonological component proper.⁵

It is interesting to note that palatalization in Barrow is progressive rather than anticipatory. A few examples will demonstrate this clearly:

niRi - lgu - ruaq	niRiLguruaq
eat - again - he	'he ate again'
niRi - tqu - giga	niRičqugiga
eat - him to do - I/it	'I want him to eat'
niRi - nagu	niRiñagu
eat - neg/imp	'don't eat it'
ani - ruq	aniruaq
birth - is	'is born'
aliik -	'tear, rip'
- qati -	piqatiga
together with	'my girlfriend'

That palatalization in Barrow is a phonological process which takes place across morpheme boundaries can be shown by the following examples:

tapit - lugu	tapiLlugu
whole - infinitive	'being whole'
niRi - tqu - giga	niRičqugiga
eat - him to do - I/it	'I want him to eat'

ilisima - Nit - niq - suNa	iLisimaNiñiqsuna
know - neg - hearsay? - I	'I didn't know'

Given this data, we are faced with several alternatives:

- 1) We can posit separate underlying palatals in Barrow. We find, however, that certain post-bases and suffixes have different surface forms depending on whether or not they are preceded by an /i/.

pi - lla - ruNa	piLLaruNa	'I can do it'
tautuk - lla - ruNa	tautullaruNa	'I can see'

This indicates that palatalization applies across morpheme boundaries, and therefore palatalization is a phonological process. Given the exceptions to the process, we are faced with positing an underlying palatal segment as well as a palatal which is the output of a rule.

- 2) We can assume that palatalization is a random process. This alternative we would like to reject as being entirely unsatisfactory.
- 3) Within the framework of concrete generative phonology as set forth by Kiparsky,⁶ we can posit a rule of palatalization:

$$\begin{array}{c} C \\ [+ \text{coronal}] \end{array} \longrightarrow \begin{array}{c} [+ \text{high}] \end{array} / \begin{array}{c} V \\ [+ \text{high}] \\ - \text{back} \end{array} \longrightarrow$$

Then, in the lexicon, all items with an /i/ which does not cause palatalization must be marked with the diacritic [- Palatalization Rule]. Although this solution seems to be a rule-governed one, with the information we have so far, such a solution actually amounts to no more than a formalization of the second alternative.

The situation is not all that bleak, however, for even with our limited knowledge of the Eskimo language, we can isolate groups of semantically related words and morphemes with the common property that the /i/ involved does not trigger palatalization (e.g. pronouns: ilaa 'he', ilvič 'you', ilifsik 'you', etc.). Given some system in the exceptions to the rule, we conclude that a concrete solution to the problem is viable. It presents a workable, rule-governed solution to the problem and is an adequate description of the facts. Unfortunately, it explains nothing and for that reason is an unsatisfying answer.

Because of this lack of explanatory force, we extend our search to the literature and to what accounts exist of other contemporary dialects, especially those which are mutually intelligible with the dialect spoken by our informant.

Our first interesting discovery is that the Barrow word /ini/, 'place or dwelling place' is related to a word noted by Swadesh⁷ as

/ene/, 'lodging'. We then discover that a large number of Barrow words in which /n/ does not palatalize are related to 'place' and 'being':

inillaiN	'could be there'
inmiut	'the inhabitants thereof'
iniq	'it exists'

We conjecture that all of these words containing /in-/, which relate to place and being, are derived from an historical /en/ that is related to /ene/.

If the initial /i/ in /iniq/ 'to be' is derived from the historical vowel /e/, then another group of semantically related counter-examples may also be similarly derived:

ittuq	'to be located'
nakittamaruak	'standing'
tiNittaun	'was drifted away' (was dislocated from)

The above words are related semantically to /iniq/ and they are formed from /ittuq/, which may be a regularly inflected form of /iniq/.⁸ It seems that all the various forms of 'be' share the common property of being unable to trigger palatalization.

The problem of the variable 'i' or 'schwa', as it is referred to in the literature, becomes evident in the plural formation of a relatively small class of nouns:

<u>singular</u>		<u>plural</u>	
	<u>normal</u>		
aiviq		avRiich	'walrus'
iRRi(q)		iRRich	'mountain'
	<u>but</u>		
siun		siutit	'ear'
suppun		supputit	'gun'

The /i/ in the plural form of the items in the second group fails to trigger palatalization of the following /t/. This seems to be peculiar to this particular group of noun plurals until we consider the following form:

supputinik	'with his guns'
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Obviously the /i/ of this and related items fails to trigger palatalization. Interestingly enough, Schultz-Lorentzen points out supportive evidence for our assumption of an underlying 'schwa' in a comment about plural formation in the West Greenland dialect:

"Where the stem ends in a 't', this is regarded as a 'te' and the affix is joined to this, 'aputilik' (from 'aput', snow)."9

This form, /aput/, mentioned above, is equivalent to the Barrow form /apun/, 'snow', and this alternation of /t/ and /n/ is a well-attested fact of Inupik.¹⁰

In addition to the systematically different behavior of the historical 'schwa' with respect to palatalization, there is evidence of additional systematic behavior of this segment in that the surface form /i/ of this segment alternates with a surface form /a/. Swadesh notes that those forms of Inupik which have 'schwa' word-finally have corresponding final /a/ in Yupik (Western Eskimo):

tumi	'road'	Inupik
tuma	'road'	Yupik

Most of the Barrow lexical items which exhibit this 'ti/n' alternation have included in them an agentive marker /-ti/, 'that which, or one who does X'. This agentive marker exhibits still another alternation, /-ti/ with /-ta-/, the latter occurring in the environment of a vowel:

aNuniaqti	'hunter'
aNuniaqtausuktuNa	'I want to be the hunter'
aNuniaqti - u - suk - tuNa	
hunter - stative - want - I	
mikuqti	'seamstress'
mikuqtausuktuNa	'I want to be a seamstress'
mikuqti - u - suk - tuNa	
seamstress - stative - want - I	
suppun	'gun'
supputauruq	'he's being a gun' (in a play)
supputi - u - ruq	
gun - stative - he	

kigun	'tooth'
kigutaa	'his tooth'
kiguti - a	
tooth - his/sg.	

but:

kuki	'cook'
kukiusuktuNa	'I want to be a cook'
kuki - u - suk - tuNa	
cook - stative - want - I	

Given this evidence, we now have historical and cross-dialectal information indicating a source for the /i/ in Barrow which fails to trigger palatalization. Within the Barrow dialect itself we have an explanation for an otherwise mysterious alternation between /i/ and /a/. The major question now is whether or not this explanation can or must be included in a synchronic description of the language.

Because of the /a/ - /i/ alternation, our previous diacritic solution no longer seems quite so uncomplicated. Not only must we posit a feature [- Palatalization Rule], but we must add a rule as follows:

Rule N i → a / ____V

and we must mark a large number of lexical items and morphemes either [- Rule N] or [+ Rule N]. In positing our rule and its exceptions, we have complicated our lexicon, not to mention the comparative grammar, to a great extent.

II. Abstract Analysis in Synchronic Description: The Implications from Barrow

In the current controversy over abstract versus concrete phonology, the simplicity metric has been played down by those who favor a concrete synchronic solution. Crothers suggests that an abstract segment complicates the acquisition model in a way which a concrete description does not.

"An abstract analysis, considered as the end product of the acquisition process, must be based upon a preliminary organization of the data which is obviously much closer to a concrete analysis. Why posit an additional step in the acquisition process."¹¹

If we consider only the local Barrow dialect, this may well be true. We are faced, however, with a dialect situation in which dialects with and without palatalization are mutual^{ly} intelligible. Whereas in Barrow the merging of the 'schwa' with the /i/ has been accompanied by palatalization of coronals after /i/ but not after 'schwa', thereby keeping some distinction between the two underlying segments, the same is not true in the more easterly dialects. In these dialects a surface distinction is maintained in the vowel quality and no palatalization occurs.

Barrow Eskimo is one of a closely related group of dialects with a common, relatively recent, history. The question arises, then, as to where to draw the line in a synchronic description. There are dialectal variations within small groups, larger communities and even larger regions. Should a description account for only the smaller geographical region, or should it be able to account for variations from region to region (which, incidentally, must be taken into account by the speakers of any given dialect in order for the dialects to be mutually intelligible)? In other words should the principle of maximal dialect coverage apply in a synchronic description of language?

One of Kiparsky's points in his urging for a concrete synchronic description is that his strong alternation condition accounts for certain features of phonological change, and according to Chen (classroom presentation), these changes are more easily accounted for by the changing of a rule feature than by changing the underlying representation. We believe, however, that palatalization in Barrow exemplifies a language change in the process of spreading. Jenness in 1927 indicated that palatalization was confined to the Barrow dialect among those of the Inupik group. Swadesh (1952), however, indicates palatal phonemes as underlying in the Yupik group. Language learning materials written for the purpose of teaching Eskimo in Northern Alaska indicate that palatalization is today a common phenomenon in all the northern Alaska dialects. If a language change in process can be detected by an extending isogloss, the phenomenon of palatalization can perhaps be represented as extending from the Yupik dialects north, and the isogloss representing this phenomenon may continue to move in an easterly direction. The fact that the Barrow dialect is closer to the Eastern dialects which do not palatalize than to the Western dialects (where palatalization is a stable process) cannot be adequately represented by a concrete description positing rule features in the lexicon. This correlation is best expressed by positing some underlyingly distinct segment such as 'schwa' in Barrow. If the language acquisition model must be taken into account --- and this we will discuss below --- then the positing of the abstract underlying segment will account for the ability of the Barrow speaker to generalize from his abstract knowledge of his own language the underlying similarity of different surface forms in the different dialects.

From the evidence available, it seems that the neutralization of the 'schwa' in Northern Alaska is a relatively recent event and that, because of the relationship of Barrow to other dialects, we are not justified in positing one underlying segment /i/ with rules and rule features to explain the exceptions. One of the many questions raised by the case in point is: at what point is a language change to be completed, so that a complete neutralization can be said to have occurred in history, which need not be reflected in a synchronic description?

More importantly, it is necessary for us to ask what the usefulness is of a highly restricted and purely synchronic analysis. Given Kiparsky's conditions on synchronic description, which might be considered as giving a literal interpretation to the term 'synchrony', the only appropriate way of looking at the model is as a time-lock. In this view, however, any claim for predicting directionality of change, reversibility, stability, etc., involves change over time and therefore does not belong in a synchronic description.

Furthermore, since all language is in a state of flux, the adequacy of a static, time-locked description is questionable, for what we end up with, as we did in the case of Barrow, is a descriptive model with no explanatory power. If limited in this way, should a theory which seeks purely synchronic descriptions even try to predict the facts through rules, or should it merely describe and set up classes and exceptions? As for a synchronic description as a reflection of the language acquisition model, even the process of language acquisition is a sort of diachronic model and cannot be captured adequately by a time-locked synchronic analysis. Such an interpretation of Kiparsky is obviously taking his strong alternation condition and its consequences to an absurd extreme. Nonetheless, it helps to make the point that the theory should accommodate the facts rather than requiring that they be altered or diffused to accommodate the theory.

In current phonological theory, there is often confusion and overlapping of the terms diachronic analysis, synchronic analysis and language acquisition model. We offer here some comments regarding the relationship of these three concepts and their place in the phonological theory.

A diachronic study may be considered to be a description or analysis of a language from point A in history to point B, showing a continuum of change. A synchronic description may be of the time-locked variety mentioned above. But we claim that a synchronic description may also be viewed as a description of the resultant state of these diachronic processes at a given point in time, one which utilizes the previous development to explain relevant extant phenomena in the language. The definition given here is opposed to a

purely descriptive model or to a model which purports to reflect only the conscious knowledge of the speaker.

A synchronic model reflects how synchronic descriptions end up being what they are, and whether it should perform this function or not is a matter of theoretical formality which, like other attempts at formalization, can be carried to an absurd extreme in the face of fact. One function of a synchronic description is to describe and explain the phenomena of a language at a given point in time in a lucid and meaningful way. Among other things, it should give some analysis of the processes that are going on in the language, and in accomplishing this it should take into account whatever is relevant to the current processes and should include some sort of prediction as to the direction of change in the future. It may be that we are actually referring to a restricted form of diachrony, in which case we are claiming only that the uses of a strictly synchronic study are much more limited than is normally considered to be the case. It remains a fact that most interesting 'synchronic' descriptions make use of change over time either in formulation or evaluation, and this is a viable form of analysis.

We understand that a too loosely defined conceptualization of synchrony is unsatisfying and conducive to abuse. (It is necessary, of course, that the elements of a synchronic study be well-motivated, especially until it is better understood exactly how much information is actually available to the speaker of a language.) Sociolinguistic evidence, for example, indicates that speakers are aware of dialect variations which are not their own, as would obviously have to be the case when dialects are mutually intelligible, and the synchronic model should be able to accommodate the existence of such information.

A language acquisition model, given the above-mentioned distinctions for diachrony and synchrony, represents a third and distinct model. While it is certainly not the case that language acquisition mirrors the historical development of a language, a language acquisition model, particularly in phonology, is at least in part crucially based on the notion of an increasing complexity of segments and rules over a period of time. This notion does not necessarily reflect the state of the language itself. If at some future time it becomes known exactly what a language acquisition model involves (which assumes defining psychological reality), then one might be able to restrict the synchronic model in such a way that it reflects the language acquisition model. It is very likely, however, that if at some time an adequate language acquisition model is developed or defined, it will include a substantial amount of non-generative material, that is, language that is learned in ways other than by the application of rules and generalizations.

Given these theoretical considerations, we believe that, in regards to the phenomenon of palatalization in Barrow, we are entirely justified in positing an underlying 'schwa' in our 'synchronic' description -- an abstract segment which is related to the historical 'schwa' of Proto-Eskimo. We can then claim that the surface /i/ derived from the underlying 'schwa' does not cause palatalization, while the true underlying /i/ does. We assume, therefore, that palatalization is a rule-governed process, and that the adult speaker, even if by analogy, is able to generalize from those forms which he learns through experience, which segments will cause palatalization and which will not.

Our primary reason for making this claim is the comparative evidence in present day dialects which are mutually intelligible with Barrow. Somewhere one has to take into account the complication of the comparative grammar and the trade-off between an appropriate theoretically restrained account of a single dialect and the cross-linguistic and even universal account of which it is a part.

III. Meta Rules (Or Does It?)

Much recent phonological theory has focussed on the inability of the existing generative framework to incorporate notions of naturalness as evaluative criteria in a satisfactory manner. An adequate framework should, ideally, incorporate all and only human linguistic processes and/or possibilities, and it is this set which it is intended will be reflected in the notion of naturalness. The generative phonological framework, however, is too powerful, in the sense that any logically possible occurrence may be accounted for without respect to its likelihood of occurrence. In an attempt to restrain the power of the generative device, a simplicity metric and markedness theory have been incorporated into the general theory to rule out impossible phenomena (static or dynamic) on the one hand, and to mark some as less likely to occur than others. It has also been the case in phonological theory until recently, that cross-linguistic processes have only been described and have not been predicted. In an attempt to remedy this situation, and to incorporate notions of naturalness into universal phonological theory, the notion of metarule has recently been proposed and entertained. The metarule as it has been conceived, is a formalization of those constraints which actually occur in language. That is, we assume that the number of ways in which languages can differ from one another is not infinite, but rather is quite limited, and is expressible in terms of a small number of substantive universal constraints. These statements, if properly formulated, will not only describe the facts in a given body of data, but will predict the pattern of distribution in any other naturally occurring language as well. More explicitly, they will predict 'the directionality, schedule of change, and the progressive domain of sound change',¹² thus reflecting the natural (and ultimately

phonetic) asymmetry of sound change. Naturalness, then, is part and parcel of the notion of metarule as it pertains to phonological processes. How the definition of what is natural in phonology is arrived at is not obvious at this stage of theoretical development. It is clear that in the long run part of the motivation is phonetic, despite the inherent abstractness of phonological rules. However, superimposed upon the continuum of phonetic plausibility are other patterns which are cross-linguistic and perhaps universal. Thus the notion of naturalness is also defined in terms of frequency of occurrence in actual languages and in terms of probabilistic claims (ability to predict such frequency of occurrence). It should be pointed out that the notion of continuum is essential here, since it is necessary to the predictive claims made for metarules (directionality, progressive domain, etc.), and since frequency of occurrence is not generally an all or nothing proposition with respect to any phonologically plausible process.

Phonetic theory is not at present fully developed enough to provide decisive motivation for any particularly interesting proposed metarule. The other explanatory criterion, probability, is an empirical claim whose validity cannot, a priori, be determined. Therefore, at present, much of the definition of what is natural rests with the notion of frequency of occurrence, which is merely a descriptive criterion, and a pragmatic one as well. This has some serious consequences. The accumulation of numerical statistics verifying a certain percentage of cases which correspond to the prediction made by a proposed metarule, relative to a percentage of cases which do not, is useful as such, namely as a statistical count. The problem arises when the conclusion is drawn, that the higher percentage can from then on be marked [+ natural], while the other group must be marked [- natural]. This attribution of binarity to the notion of naturalness defeats the purpose of the metarule itself. What is the point of trying to make a predictive statement about what a language can and cannot do if the end result is merely that X is more likely to happen than not, or that X is more likely to be observed than Y? The present combination of simplicity metric and marking conventions is adequate for that purpose and makes no unsupported claims about language universals.

Furthermore, this is an acceptable conclusion only if the 'unnatural' group is actually nonoccurring in natural language. Perhaps more seriously, the notion of frequency of occurrence as the determining factor of naturalness is misleading, since it implies falsely that all occurrences of a predicted phenomenon do have the same motivation, that is, that they are exactly the same. This method implies that it is actually the motivation of a process which provides the definition of naturalness, when in actuality a phenomenon may reflect several different processes which produce the same output.

In his paper, "Predictive Power in Phonological Description"¹³ Chen discusses a proposed metarule for palatalization which predicts that the direction of spread for palatalization in any given language will occur along a continuum from a back point of articulation forward, and that this directionality is reflected both in the history of language and in the extension of domain. This rule, in its basic form is expressed as:¹⁴

$$\begin{array}{ccc} \text{Rule 5.} & \begin{array}{c} C \\ \boxed{\alpha \text{back}} \end{array} & \longrightarrow \text{palatalization} / \begin{array}{c} V \\ \boxed{\alpha \text{back} \\ \beta \text{high}} \end{array} \\ & \text{language universal constraint} & \begin{array}{c} \alpha \geq m \\ \beta \geq n \end{array} \end{array}$$

(Language specific values are assigned for m and n).

Thus we expect, for example, that if one finds palatalization of coronal segments in a given language, one will also find palatalization of velars, even if only in residual form. Given the evidence presented above for palatalization in Eskimo, it seems that in Barrow we have a genuine counter-example to the proposed metarule. Neither this dialect, nor any others in either Eastern or Western Eskimo that we are aware of, shown any indication of active, incipient or residual palatalization of velar segments, although, as we have seen, coronal palatalization is an extensive and systematic process in some dialects. Other dialects, such as Greenlandic, show evidence of related processes (assibilation of /t/ to /ts/, for example¹⁵) in coronal segments.

If we accept as fact the claim that palatalization in Barrow is restricted to coronal segments, then what else is necessary to establish that this is indeed a true counter-example to Chen's proposed metarule? It seems that the following should be demonstrated to further substantiate an apparent violation as a true counter-example: It should be shown that the phenomenon is a true process, that it is neither superficial nor random, and ideally, that its systematic occurrence is explainable and predictable. The presentation above should demonstrate that all of the above are properties of Barrow palatalization.

The metarule as stated by Chen excludes consonants articulated back of the velar area, for example uvulars, from the back to front continuum. This is not trivial, because its exclusion makes the claim not one of directionality, but of arbitrary extension. In fact, however, uvulars are extremely resistant to palatalization, as are labials, and given the increasing number of observed languages which palatalize only coronal segments, one is inclined to think that the progressive domain is actually outward from the point of palatal articulation. While this solution is not as neat as one would hope, in that it does not predict whether a language will select coronals

or velars for initial palatalization, it may prove to be the only appropriate statement until such time as a more precise, perhaps phonetic motivation for selecting one or the other can be determined.

The motivation for developing a theory of metarules as a part of phonological theory, then, is that present theoretical frameworks are essentially descriptivist, and are incapable of according theoretical status to predictable patterns of phonological change. Or, as it is expressed by Chen:

What is needed is . . . "to impose certain language universal constraints as part of a finite set of higher order metarules. These metarules, together with their constraints, circumscribe the outer limits of language-like processes as distinct from all other random, unnatural processes."¹⁶

That is to say that metarules provide an explicit definition of naturalness. The question, given this claim, is: What is the function and status of a solid counter-example in terms of the theory? In terms of the above definition of metarules, a counter-example is a priori random, unnatural and unlanguage-like. This is a strong claim and one which we find appropriate to the notion of language-universal metatheory. This explicit definition of what a metarule should be, however, should be reflected not only in the way a violation of that rule is regarded, but should also be considered in the formulation of the rule itself. A language-specific rule or claim, under any circumstances, permits only two options with respect to an alleged counter-example. The counter-example must either be absorbed and explained, or one must admit to a flaw in the claim or rule. With metatheory, this accountability problem is intensified. Given the claim of the metatheory that it provides the definition of what is linguistic and what is not, a rule which purports to be a part of that claim can admit to no counter-examples. If a true counter-example occurs, then the rule it counters, while it still may be a cross-linguistic rule, is no longer part of the metatheory.

There are two ways to resolve this problem: one may either admit into the metatheory only those rules and claims which are truly universal and which do indeed define what is linguistic and what is not. Or one may weaken the theoretical claim made by the metatheory, so that it no longer claims to define what is linguistic in terms of what is natural, but merely provides an enumeration of sorts regarding the cross-linguistic likelihood of certain processes occurring vis-a-vis the alternative possibilities. This sort of metatheory would legitimately use primarily a frequency of occurrence statistical count, and would inherently not be a predictive model, but merely a descriptive one. This defeats the proposed motivation for the metatheory, namely that it is able to predict and not merely to describe. Therefore, a well-motivated metatheory must select the

first option, and permit only those predictions which are indeed able to circumscribe linguistic limits.

In conclusion, we would like to propose an alternative way of considering metarules which might prove to be both useful and intuitively satisfying. Rather than viewing the output structure of processes across languages, and trying to collapse all like outputs into one language-universal rule, one could look at the processes themselves, several of which may produce a single output. A meta-rule, in this case, would be a predictive statement about the shape of a single process. Other rules might relate these process-statements one to another, especially if, as may be the case with palatalization, several different processes produce like outputs.

It seems clear to us that palatalization in Barrow is a true counter-example to the palatalization metarule discussed above. It also seems to us, however, that perhaps palatalization in Barrow is a different process from the palatalization referred to in the formulation of that metarule. It seems quite natural that languages should have different motivations for change, and a universal meta-theory should attempt to accomodate such a possibility. Perhaps the notion of motivation can provide a substantive criterion for the determination of naturalness. Perhaps, too, the problem of meta-rules and their counter-examples could be in part resolved by positing metarules which focus on motivated processes themselves rather than on their output structures.

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NOTES

1. Swadesh, 1952.
2. The transcription used in this paper will be primarily standard Eskimo orthography adapted for our typewriter. For reference, some of the nonobvious representations are given below:

Eskimo orthography	This paper	IPA
ᑎ	N	ɲ
ᑎ̃	ñ	ɲ̃
ᑎ̃̃	ḥ	ɲ̃̃
ᑎ̃̃̃	L	ɲ̃̃̃
ᑎ̃̃̃̃	Ḍ	ɲ̃̃̃̃
ᑎ̃̃̃̃̃	q	ɲ̃̃̃̃̃
ᑎ̃̃̃̃̃̃	g	ɲ̃̃̃̃̃̃
ᑎ̃̃̃̃̃̃̃	R	ɲ̃̃̃̃̃̃̃
ch, tch	c, ch	tʃ

3. Swadesh, op cit. We have used /e/ here to represent the /ə/, as in Swadesh's system.
4. There is an alveolar fricative which sometimes appears after /i/ that we have not included in this analysis because it is not apparent to us how it is derived historically or how it fits in, especially comparatively.

e.g.	Proto	Yupik	Barrow	Greenland
'eye	iji	ii	iʒi	ifi

5. We assume the point of view expressed by Schane (1971) that 'only those phonetic differences resulting from morphophonemic rules are phonemic'.
6. Kiparsky, 'How abstract is Phonology'
7. Swadesh, op. cit.
8. ini+tuq = iniq 'be'

ini+tuq	
inituq	
intuq	
ittuq	= 'be located'
9. Jenness, 1927.
10. There is some controversy as to whether this vowel is underlying or epenthesized. For discussion of this, see Underhill, Ms.
11. Crothers, 1971.

12. Chen, 1973.
13. Chen, *ibid.*
14. Chen, *ibid.* The rule as given here is incomplete, but contains the information relevant to this paper.
15. This is a widespread process in many dialects. As further evidence of the schwa, assibilation takes place after a true /i/ but not after a schwa. Swadesh notes this also (1952).
- 16.. Chen, *op. cit.*

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