

# THE ANALYSIS OF THAI TONES: AN ARGUMENT

by

Peter Bee

*School of Oriental and African Studies*

In the recent publication by *Artibus Asiae* in honour of G.H. Luce (vol I, 1966), the contribution by R.B. Jones<sup>1</sup> can be looked upon as a challenge. Although it purports to be a review of work on Thai diachronic reconstruction so far, it in fact incorporates comments, analyses and proposals that do not conform to the usual train of thought in Thai dialectology. This in itself deserves praise, for it is never too late to mend, and a shrewd point, well taken, might put us all on to a different track—might conceivably bring about a re-alignment of basic premises or procedural methods, analogies or models, that would benefit scholars by trenchant observations of fact or by climactic deductions.

My purpose is not to present an apologia on behalf of the scholars cited by Dr. Jones in his article. It is for this distinguished band of researchers themselves to accept or refute Dr. Jones' findings on the basis of their extensive field work and analytical prowess. What I propose to do is to accept the findings as data and to examine the internal consistency of Dr. Jones' ideas and their repercussions on theory in general. I must here avow at the outset that my conclusions do not concur with those of Dr. Jones. The upshot of my conclusions is a reluctance to see a breakthrough: I think that the new theory is specious. I must apologise for this word and immediately qualify it thus: by 'specious' I mean that an evident rigour of investigation has led to a false economy of terms. Clarity has been won at the expense of subtlety. Inherent in my use of this word is that the older procedures and analyses are *less* specious: I would go so far as to maintain for them at least a Scottish verdict of 'not proven', seeing that these old ideas have, as it were, been accused of misrepresentation yet, to my mind, emerge as less guilty than the prosecution.

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1) *Comparative Thai Studies: a Critique.*

I take Dr. Jones' inspiration to derive from Occam's razor. He desires to cut out those hypothetical superfluities that tend to multiply in direct proportion to the complexity of the problem and, regrettably, to the number of scholars interested in it. Striking another note, we might say that the critic sets up to be a Copernicus, appealing to simplicity and elegance to justify a theory, in opposition to an old Ptolemaic system of cycles and epicycles whereby the movements of the solar system's members had had to be explained hitherto. He asks why a set of four initial consonant categories have to be hypothesized when only three occur as a set in all Thai dialects. He asks why a hypothetical nine tones at least must be reconstructed when he finds only a maximum of six occurring in any Thai dialect he has investigated. Are we not here in the realm of epicycles? Are we not in the grip of a system of analysis which needs props at every stage of its argument and insists on making out these props to be either real data or legitimate constructs?

Though Dr. Jones does not use those terms, the impact of his *Critique* is strong and invigorating. But how long does this invigoration last? For me, it was the table of initials and tone classes (Table 1 below) that sobered the mind precisely because, in such an encapsulated presentation, Dr. Jones invites us to be as enterprising and open-minded as he has been. For me, then, after arranging and re-arranging initials and tones and sets of one or the other or both in as rigorous an experimental mode as was given to me,<sup>2</sup> I found I did not come to Dr. Jones' conclusion. I found the old arguments still to be cogent and Dr. Jones' presentation to lack the prerequisite that a simple and elegant proof ought to have—the demonstrable logic that the theory is right because it answers *more all at once* than the old-fashioned theories could answer stage by stage.

Here is the key postulate for our discussion laid out in Dr. Jones' table:

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2) I acknowledge the valuable help of Dr. Robert Exell of the Mathematics Department, Chulalongkorn University.

p :: ph	1 2 3
ph :: ph	4 5 6
p :: p	1 5 6
b :: b	1 5 6

Table 1

We have to be persuaded that there are but six numbers (tones) to be entertained in our analysis, and, likewise, but three phonemes: p ph b.<sup>3</sup> On the face of it the table assures us that no further proof is needed. Against this is placed the 'epicyclic' construct of four initials and three tones. A pause for calculation, however, will give the respective products for all possible items produced by pairings to be eighteen as against twelve. Clearly, Dr. Jones does not permit eighteen pairings (syllable types): he restricts the number by sets—overlapping sets—so that the total can be reduced to eleven or twelve distinctive occurrences, depending on the dialect.

At this point, however, not forgetting that we still owe it to Dr. Jones to explain how he does this, I must insert a *caveat*. It is simply that Dr. Jones' singling out of the chief bugbear of comparative dialectology does not coincide with what I myself would choose. The burden of the whole article is the desirability of a plausible 'reading' for the reconstruction of low class consonants in Thai. In other words, how is the starred form of low class consonants to be written down? Now, it must be granted that this *is* a problem but, in my own experience, it rates second to the dilemma of the partition of mid class consonants among the tone sets in different dialects.<sup>4</sup>

- 3) We are speaking here of initial phonemes relevant to sets of tones. Taking the bilabials as characteristic for the phoneme inventory of initials, the full row of articulations would, of course, be p ph b m f w. But for tone, m f w are members of the 'set of all the sets' and hence not critical for tone allocation. Although it looks as if b is a member of the p-set, the possibility of a p occurring with tones 2 and 3 (top row of Table 1) indicates that b should be kept separate.
- 4) This will be discussed more fully towards the end of this article.

What exercises Dr. Jones' mind is the switch of low class consonants in different dialects between phonemic aspirates on the one hand and phonemic non-aspirates on the other. To the 'old' analysis this problem was straightforward: what can we postulate as original common ground if aspiration/non-aspiration was destined to be the distinctive contrast between dialects that it yielded. The allegiance of the mid class, however, was bewildering: why did some dialects group mid with high and some with low? Why, within a certain dialect, did the mid class break up as a set, the members going individually high or low? Now, preference for this problem as against that is a question of emphasis that is more important than one might think. I am reminded of the tale of two tramps who were given a cake. The first tramp asked the second, 'Do you like the ends or the middle?' The second tramp replied, 'I like the middle,' whereupon the first tramp cut the cake in two.

But let us get back to the Table again, the data of which we have agreed to accept. The distribution of the numbers (tones) can obviously be diagrammed thus :

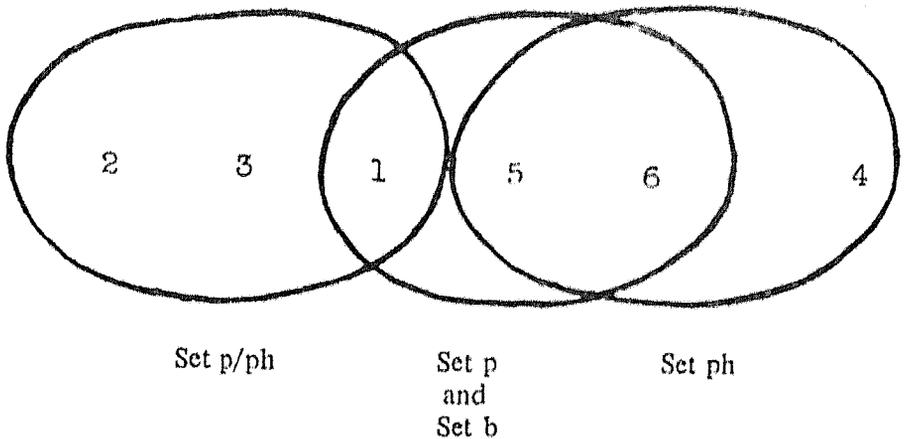


Diagram A

Clearly, with 1 5 6 as members of an overlapping set, we might expect the three remaining numbers to act as markers, showing unequivocally which set we were dealing with. But what *are* they marking? —sets of what? Diagram A has taken the *tones* as the criteria for distribution.

A set diagram that shows the distribution of the initial phonemes as well as the tones will be different. It can be drawn only after expanding the Table to account for all possible occurrences and then plotting the distribution that is found:

ph can take tones	1	2	3	4	5	6
p can take tones	1	2	3		5	6
b can take tones	1				5	6

The possibility presents itself, then, that ph is a member of the set of all sets. Tones 2 and 3 occur in a set, the other members of which, viz. 1 5 6, are gathered together in a sub-set characterised by their ability to take initial b as well as the other initials.

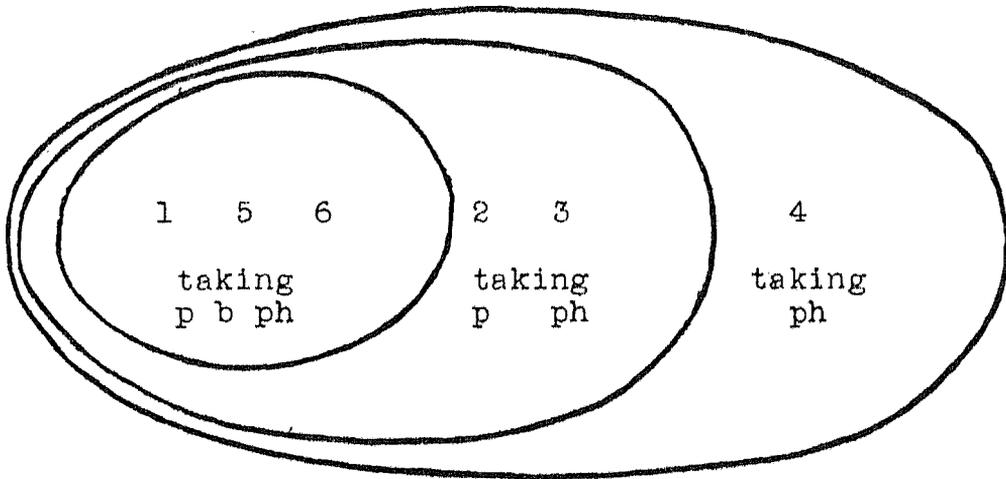


Diagram B

Apart from 4 appearing as a *unique tone*, produced only in the environment of one initial, ph, the diagram permits us to coin the terms *maximally tonal*—i.e. ph takes all six tones—and *maximally initiated*—i.e. tones 1 5 6 take all three phonemes.

We must present the count in a more refined way, however, since what looks like a set containing both p and ph has, in fact, a necessary choice: one or the other but not both. It means that some dialects have p as sole member and some have ph. Let us call the former dialects D1 and the latter D2. The count now shows:



Tone 4 was certainly reserved for outright, unequivocally ph-initiated syllables and no others. Could it be, perhaps, that Tone 4 is going to be one of a set of tones in complementary distribution—three allotones of one toneme, one unique for ph-syllables, one for p and one for b-syllables? The likelihood of complementary distribution within such a tone-set encourages us to look for other unique occurrences, one each for rows p and b. Is bl going to be unique, for instance? We can find out only by considering the shaded rectangle on Table D0.

A conflation of this same portion for D1 and D2 does yield something like a set p/ph proposed by Dr. Jones. Between D1 and D2 the sequence of numbers 123 seems to jump back and forth from row p in D1 to row ph in D2. Now, if this jump were complete—a clean switch across from one row to the other without any remainder—then there would be nothing to sway our choice in reconstructing \*D0. But let us examine the switch again:

D1	1	2	3		D2	1	2	3
p	x	x	x		p	x		
ph					ph	x	x	x

Table 2

Clearly, something in D2 seems out of line with any explanation in terms of a complete switch across. Item pl refuses to jump. Another thing about item pl, by the way, is that it dooms any hypothesis of complementary distribution of unique tones. For the initial b Tone 1 is far from unique. Moreover, 2 and 3 seem to be a strange sort of 'unique pair'—unique for different initials in different dialects (now for p and now for ph). The point is that these two tones can be interpreted as marking a set (for tonal behaviour) irrespective of what happens about initial aspiration or the lack of it.

D2, then, has 12 items marked x for occurrences as against D1 with 11. The likelihood is that D2 resembles \*D0 more closely than does D1. This is because it is preferable to assume reduction by

assimilation--all other things being equal--as a more likely process to have occurred than dissimilation in favour of a particular p1 occurrence. We mean the likelihood is that the phoneme p in \*D0 took tone 1. Later this was overlaid in course of the change ph → p in D1 and resulted in the 'adding' of two sets now both pronounced with an initial p. No matter how many identical sets we add, the total will always remain one.

Since we shall now take D2 as equivalent to \*D0, we must add another maximally initiated tone to the tones 5 and 6 noted above. There will now be tones 1 5 and 6, all maximally initiated. Keeping this point in mind, therefore, a possible analysis for D2 -- \*D0 would show :

Maximally initiated	1	5	6	(taking p b ph)
Unique	2	3	4	(taking ph only)

Table 3

Although such an analysis at first sight looks fair and informative, it will not satisfy any linguist; certainly not Dr. Jones--this is not the table he proposed--nor does it resemble any other format for the analysis of tones in Thai dialectology that has ever been offered. The objection to taking 2 3 4 as a set is that it fails to predict why tones 2 and 3 'jumped' or switched up to row p later in some dialects (D1) whilst Tone 4 never did in any.

Central to the whole issue, of course, is the question whether there was a conditioning of tones 'subtended' by differences of initial phonemes or whether, on the whole, phonematic tone and phonemic initial were independent of each other. Dr. Jones certainly seems to think that the six tones as phonemes pre-existed any splits or assimilations that the initial phonemes might undergo. More than that, he implies that phonemes that came to alternate between one dialect and another (p/ph in particular) were marked off as different from original p and ph precisely by their ability to occur in one tone class (1 2 3) as against another two classes (4 5 6 and 1 5 6).

In the D0 table of tones and initials, the numbers 2 and 3 stand out as remarkable. They are what I shall call variators. This means that they permit of a change in initial phoneme (p or ph exclusive) whilst still remaining, as a pair, in unique distribution. This remains true whichever preference one has, whether it be D1 or D2, for the dialect which is nearer to \*D0. (Dr. Jones' reconstruction of \*ph for the set p/ph seems to favour D2, the same choice that we have made too.) More remarkable than that, they can tolerate p as one of their variations but can never associate with b. All the evidence from the tables leads us to think that for tonal groupings *original* p and b were indistinguishable. Indeed, there is no unique tone for original p and b at all. We can put it another way and say: whereas tones 2 and 3 were primary for the recognition of a word and the articulation of the initial phoneme only secondary (i.e. it could be either p or ph and still not run the risk of homophony with other 'original' p or ph words where articulation *was* a distinctive feature of the first order), this tonal primacy in distinctiveness would be endangered or lost if ever the initial b were allowed to co-occur! Also remember that 2 and 3 *do* occur with initial m! Surely it cannot be that b controls the distribution of tones 2 and 3 in a negative way (it alone repels them), whilst ph controls tone 4 in positive way (it monopolizes it)? If tone 4 is limited to co-occurrence with ph, are tones 2 and 3 limited to non-co-occurrence with b? If so, why?

Another very similar rule of non-co-occurrence will have to be found for initial ph with tone 1 in Dialects like D1. Here it is a case not merely of either-or type variation but also of loss. D1 has one less x marked for occurrence than D2. Why should ph commence to repel association with tone 1 in the development of these particular dialects? Why should words with tone 1 beginning with old ph now be homophonous with words beginning with old p? The linguist answers assimilation. But why assimilation of only *some* ph initials? The linguist answers that only those in the environment of tones 1 2 3 were allowed to assimilate. This led to a phoneme 'overlap' in the case both tone and initial for item p1.

If the environment was so critical, therefore, can it not be said certain tones were permissive of certain changes? They did not *cause* change in initial articulation (for, after all, we are postulating initial development and tonal development as two independent processes) but merely allowed change to happen or not happen, as the case might be. Yet two of these 'permissive' tones are very narrowly restricted in occurrence (2 and 3, the unique pair, never occurring with b). The other one, tone 1, permits itself to be repelled in all the D1 dialects by an initial phoneme (ph) with which it had hitherto always associated, whilst maintaining tolerance for initiation with b. A strange kind of permissiveness! On this line of argument it becomes clear that the idea of the environment of six fully phonematic tones exerting selective patterns of behaviour upon initial phonemes is well nigh inadmissible. If there was an assimilation of \*ph to p in certain dialects, then *all* the \*ph set had to go, not just those with certain tones. To have it otherwise hits at the basis of the thesis: that there were originally six tones and three phoneme categories, and that they were mutually independent.

The restricted occurrence of tones 2 3 and 4 also hits at the crucial factor of supposed original independence of initials and tones. Why should they be so restricted? For instance, tone 4 is clearly restricted by the occurrence of ph and not vice-versa. As we have observed, 2 and 3 are restricted by the non-co-occurrence of b and, in D1 only, tone 1 is restricted by the non-co-occurrence of ph. Yet if we are to postulate a Proto-Thai stage antecedent to \*D0 (which we may call \*DP), then the implications of a plausible \*DP table, such as:

*DP	1	2	3	4	5	6
p	x	x	x	(x)	x	x
ph	(x)	x	x	x	x	x
b	x	(x)	(x)	(x)	x	x

Table DP

are *either* :

that certain tones later rid themselves of certain initial phonemes ⊗, others rid themselves of some in some dialects only ⊗, whilst others alternated between the two according to dialect (↑)—a spontaneous reduction of statistical spread supposedly happening independently of any influence from the articulation of the syllable-initial;

*or* :

that there was interdependence—action and reaction mutually—between tones and initials, e.g. tones 2 and 3 ‘allowed’ the p/ph alternation whilst tone 4 suppressed it (this is tone influencing possible change of phonemes); ph repelled tone 1 in D1 dialects and b repelled 2 and 3 in all dialects (this is initial phonemes influencing possible tone);

*or* :

that there was a one-way influence only—that of the initial phonemes over a reduced number of ‘basic’ tones, the appropriation of additional tone phonemes being in direct proportion to the loss of distinctive features in the inventory of initial phonemes. This we can call the theory of compensation.

Here the middle way is not the right one. It does not offer a system so much as an easy way out seized upon as an opportunist’s route from \*DP through \*D0 to D1 and D2. Though just plausible, it is not internally consistent.

The first explanation demands belief in the inherent selectivity of certain tones, this tendency developing even against scope of their own statistical coverage as phonemes. It can be said to be a progressive specialization or concentration on certain initial phonemes by certain tones. Its logical conclusion is the ‘unique’ tone. This is a process not known to most linguists. What *is* known, on the contrary, is the isolation of unique tones because they have not been overlapped (or overlaid) by assimilation. In other words, where one view is of unique tones as the newest end-product of increasingly drastic refinement and narrowing of scope, the other view is of unique

tones as fossils from the past, alive thanks to a lucky escape from the accretions of change and interchange.

The third and last way presents difficulties of reconstruction for drafting a fuller inventory of initial phonemes at the Proto-Thai stage, some of which find no exponents in the present-day dialects. But we do know of a plethora of examples in other languages for this sort of phenomenon. We also know of examples where tones were affected by it (Chinese).

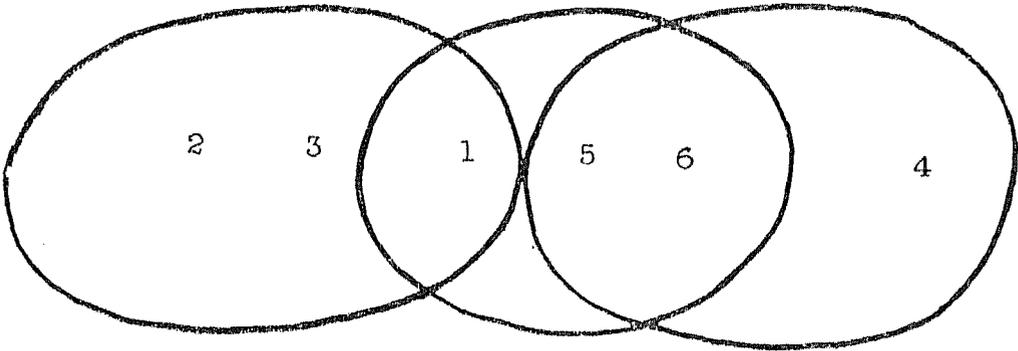
A final blow against the original independence of all six tones is the 'three-ness' of Dr. Jones' distribution table. Granted he demands all six for \*m (and \*f and \*w, of course) and can be understood as having demanded all six for 'ph (his Proto-Thai form), yet he must settle for three, and always and only three, for b as far as the evidence shows. Likewise for h. But if \*ph, an aspirate, can originally take all six, why does the evidence show h as taking only three?

In a paradigm of 4 initials and 3 tones (a total of 12 items) a loss by assimilation of any one initial creates a demand for compensatory distinctive features. The total of items at this stage would be, of course, 9, so the number 3 needs to be added from an alternative source, tones. The original three tones must therefore submit to a demand for a *minimum* of three extra distinguishing features, bringing the total to six truly phonematic tones. The simplest account is to be seen in tabular form:

*DP Original	? goes to ph			Tonal Compensation (...D0)									
	1	2	3		1	2	3		4	5	6		
p	x	x	x	p	x	x	x	p	x	x	x		
ph	x	x	x	ph	x	x	x	ph	x	x	x		
b	x	x	x	b	x	x	x	b	x	x	x		
?	x	x	x										
12 items				9 items				12 items					

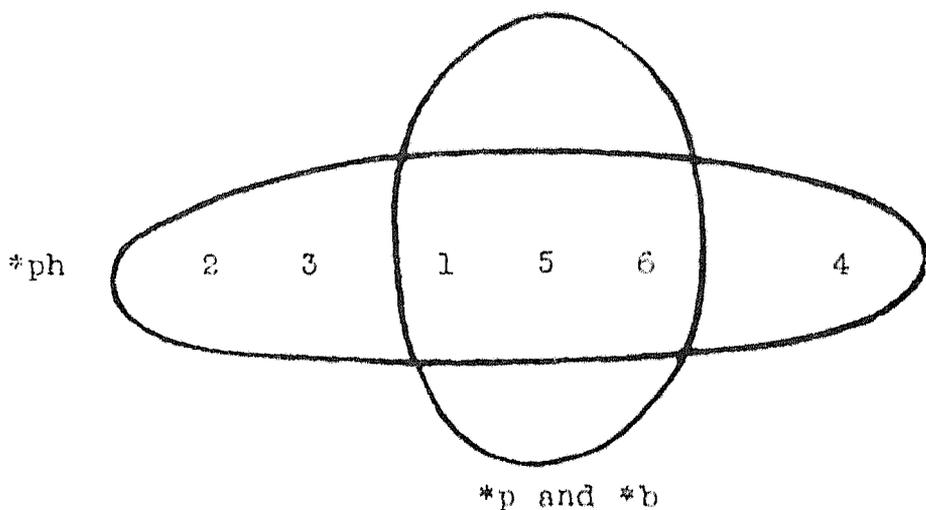
Table 4

There are three kinds of initials, then, in \*D0: there is one set made up of those unvariant originals that have been overlapped or masked by later variants. Such *unvariant* but non-unique forms can be termed set U. The *variants* that have come to be assimilated, i.e. the newly switched, overlapping initials (e.g. ? → ph above) will be called set V. The others that have no problems about identity or distinguishing features at all will be set D. The 'new' tones, after compensation, by their *raison d'être*, must distinguish U from V, but the D set can, in theory, cover any three tones it likes since distinctiveness can be fully assured by initial phonemes.

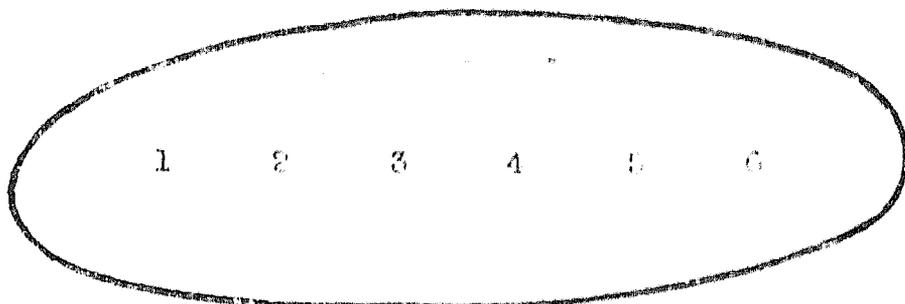


This was our first diagram (Diagram A) for tone distribution: 1 2 3 will be members of set V; 4 5 6 will be members of set U; and 1 5 6 will be the D set which, as we have said, can in theory be redrawn to include within itself any three tones at all. This is the stage at which Dr. Jones began his *Critique*. The argument against him can be presented in the following diagrams:

If U and V at one stage go back to common \*ph (alike for both the *Critique* and this argument), and if Dr. Jones insists that \*ph is an irreducible unit phoneme (unlike the argument), then what prevents the diagram



from stabilizing itself by permitting *\*p* and *\*b* similar coverage to *\*ph*? Why only *three* members of the overlapping set? Why not four, five or finally six, ultimately reaching a proto-stage



*\*p and \*ph and \*b*

which corresponds to our Table DP? I can not find answers to my questions either explicitly or implicitly in the *Critique*.

In the 'old' analysis, U and V would diverge because of reconstructed distinction between their initial phonemes at some earlier stage. The initial categories U V b p would now all be distinctive. There are good grounds for reducing the number of tones by half whilst retaining the same total of clearly distinguished items. But the problem of which tones were nearest to the original

'basic' three is vexed by the chance that the D set might have moved anywhere in the dialectal developements for the overlapping of sets of three. The third paradigm in Table 4 (Tonal Compensation) is, then, naive in the extreme. If the unique tones always stood out as the V set, our troubles would be over. But arguments directed against just such a proposition in Table 3 show that any simple solution does not match the facts—certainly not the data accepted for investigation here. This is the reason for emphasizing the mid class as the overriding difficulty in tonal analysis in dialectology as against the low/high classes (U and V) that form the 'legs' upon which most of our analysis has to stand. We simply cannot account for the vagaries of mid class allegiance. It is all very well to say that tone assimilation is the key to this problem but, after a conspectus of tone charts in many dialects, we must admit that we are not even sure of the favourite assimilations or overlaps (Is 1 5 6 really the favourite set?) let alone the reason *why* they were favoured.

The total number of possibilities for sets of three out of a row of six numbers is 20. With such large combinatorial possibilities for mid class analysis with six present-day tones, would not the tendency be for the researcher to look for lexical, orthographical and general 'etymological' clues to cut down the tedious process of the statistical digestion of raw data? Particularly if the actual combinations occurring seemed fairly limited, whilst the mathematical possibilities in theory amounted to a score.

Is there some co-efficient or moderating principle that keeps the D set from exhausting all 20 combinations? Is this principle connected with different qualities of tone, of initial phoneme, or of both. Is it constantly applied as a kind of law, or did it function only at some critical stage of dialect developement? I hope that Dr. Jones and those eminent scholars he pays tribute to will join me in deeming these questions worthy of attention.

