ments could be mustered, I think, to show that the object and verb are interdependent, contrary to the original assumption.)

Rather than accept these consequences of the definition of dependency, I prefer to alter the definition slightly. The new definition is like the first in referring to slots, so that X is a modifier of Y if and only if it fills a slot in the structure of Y, but it also requires that this slot be an optional one. A slot is optional for Y if Y belongs to a class of words for which the slot is optional — i.e. if there is an entry in the paradigm either for the word X itself, or for the class to which X belongs, which allows the slot to occur, but does not require it. Take objects, for example. These can now be shown to be modifiers of the verb, since the entry for verbs in general allows an object to occur, but does not require it. Thus even if a verb is obligatorily transitive, like take, the object slot still counts as an ‘optional’ slot by our definition, and the object therefore counts as a modifier of the verb. Now consider adjectives. These are easily shown to be modifiers of their head-noun, since the slot they fill is an optional one; but the noun can not be taken as a modifier of the adjective in spite of the fact that it is referred to in the entry for some adjectives (but possibly not for all adjectives), since the references to it do not allow it, as required by the definition. They say nothing about the presence or absence of a noun; all they do is refer to the position or semantic features of a noun, whose presence is determined by other considerations. Similarly satisfactory results emerge if we apply the new definition to all the constructions listed in (7) of my earlier paper, which I wrongly claimed to be produced by the old definition.

References

---(1979), Panhellenic: mimeo.

Dyirbal ergativity: counter-rejoinder to Dixon

JEFFREY HEATH

Abstract

Replies are made to factual and theoretical points raised by Dixon in his rejoinder (1979) to my paper criticizing the ergative-syntax analysis of Dyirbal (Heath 1976). I move from relatively picayune issues to larger ones, and conclude by calling for answers to remaining factual questions. The most significant new point concerns hints in Dixon’s rejoinder that previous descriptions of the functions of Dyirbal antipassive rules are seriously incomplete.

1. Minor points

Dixon devotes disproportionate space to criticism of my title (‘Is Dyirbal Ergative?’) and of my glossing conventions for noun markers. Since the sense of my title was explained in the abstract directly under it, and since the glossing conventions were explained in an early footnote, the charge of ‘mislading’ is absurd. Dixon acknowledges that glossing of noun markers is irrelevant to the theoretical issues at hand. Indeed, in his recent ‘Ergativity’ article he simply omits the noun markers from the Dyirbal examples for expository purposes since ‘they do not in any way affect the grammatical points being made’ (1979b: 61). Pedantic discussions of such points serve no purpose except, perhaps, to conceal lack of discussion of more central theoretical and factual issues.

Second, Dixon charges that I misrepresented the facts concerning the -sura rule (which adds this suffix to the verb in one clause to indicate that it has a nuclear NP coreferential to the A but not O of a preceding transitive clause). He insists that in addition to this coreferential network it is necessary that the two clauses involve an immediate temporal sequencing relationship. In reply, I would point out first that the temporal immediacy condition is extraneous to the central theoretical issues involving the -sura


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construction, which have to do with describing the coreferential relationships which trigger it and with the relationship of the -\textit{apu} rule to the -\textit{apu} (antipassive) and EQUI rules. Secondly, contrary to what Dixon thinks, I accepted his claim that temporal immediacy is involved in -\textit{apu} clause (1972) suggesting that temporal immediacy is also typical (though perhaps not so obligatory) with other kinds of coreference-chain constructions (e.g. those with various combinations of EQUI and antipassive). Therefore, Dixon’s charge that I altered the glosses for -\textit{apu} constructions to make it appear that no temporal immediacy involved is preposterous. It is true that I find some incompleteness in the data; it is not entirely clear what Dyirbal does with underlying configurations satisfying the -\textit{apu} coreferentiality requirements when the activities in the two verbs are separated by a time gap or are simultaneous; the only regular alternative to the -\textit{apu} rule in the second clause is anticipatory antipassive (A-ANTI) in the first clause and this rule seems to be used quite sparingly. However, this is a matter of descriptive detail and of little theoretical significance.

Derivational suffix -\textit{apu} marking multiplicity of events, or of S or O, seems to have taken on new significance for Dixon. I accepted all of Dixon’s factual claims about this element (despite limited exemplification) and agreed that its patterning differentiated S/O (i.e. absolutive) from other categories including A (ergative). Along with the particle \textit{wara} (see below), -\textit{apu} was the only feature of Dyirbal syntax for which I agreed without reservation that ‘ergative’ patterning was present. I pointed out, though, that one or two elements of this type, both of limited text frequency, were of little typological interest and that similar ‘ergative’ elements occur in many languages with otherwise ‘accusative’ syntax.

Dixon’s comment in his recent reply deals only with whether -\textit{apu} is or is not ‘important’ as evidence for ergative syntax, and states that he views it as just as significant as other syntactic processes. If this is the case, it is curious that Dixon devoted less than two pages to -\textit{apu} in his enormous grammar (1972), placing it in a residual ‘Morphology’ chapter distinct from his ‘Syntax’ and ‘Deep Syntax’ chapters. In his recent 80 page paper on ‘Ergativity’ (1979b), derivational elements of this type are ignored while extensive treatment is given to several other typologically critical phenomena. The closest he comes is a short footnote on noun incorporation, which in many languages shows incorporation of S or O (but not A) NPs, but he quickly dismisses this as ‘... a matter of word formation — surely a phenomenon of a quite different type from those discussed here’ (1979: 118fn.). If Dixon is serious in claiming that -\textit{apu} is important evidence for syntactic ergativity in Dyirbal, it will be necessary as a minimum to investigate the behaviour of derivational multiple (and distributive) alls of this type in other languages; when this is done it will soon become apparent that many otherwise syntactically accusative languages have ergative patterning in this particular feature. I think we would be well advised to follow Dixon’s own advice and distinguish sharply between derivational and word-formation processes and larger scale syntactic processes like antipassive and EQUI.

Similarly, Dixon now claims that the particle \textit{wara} ‘wrong’ is ‘important’ evidence, and berates me for the brevity of my treatment of it. In fact, Dixon thought it so important that he dealt with it in seven lines on one page in his grammar (1972) in a section with the unpretentious subheading ‘other particles’ in the ‘Syntax’ chapter. Although the syntax of this element is probably more interesting than that of -\textit{apu}, since the former is less highly grammaticalized, it is hardly comparable in typological significance to the major syntactic processes of the language, and many other languages lack elements which can be directly compared to \textit{wara}.

As for the -\textit{apu} particles, Dixon states that he has more than one example of a semantically passive form (from a transitive verb) in his large corpus. The fact remains that the active type is so predominant that the antipassive suffix -\textit{apu} is often omitted, with the resulting particle (formally ambiguous as to active or passive sense) ordinarily interpreted as active. It is therefore perfectly appropriate to refer to the passive type as ‘marginal’. Dixon’s suggestion that ‘Heath’s theory can’t account for it too well’ is particularly silly since even in 1972 Dixon was forced to resort to a functional explanation for the behaviour of this participle formation. Dixon was hardly comfortable with this since his overall framework was almost purely that of abstract syntax rather than surface-structure functionalism; on the other hand it fits perfectly with the framework I advocated. I would also point out that Dixon has yet to produce a passive example of the similar participle with -\textit{apu} suffix, which appears (from presently available data) to require S or A (not O) head and appears never to show antipassive -\textit{apu} suffix. I suggest (as a hypothesis which can be easily tested) that passive -\textit{apu} particles, while ‘productive’ in the sense that they can be elicited from several transitive stems, are uncommon in spontaneous speech.

Dixon’s comment on my tabular representation of types of surface coreference-chains is that it is ‘virtually identical’ to his own tabular display. In the first place, a tabular display is simply a convenient summary of facts, and it is quite obvious that the theoretical frameworks used by the two of us to come to grips intellectually with those facts which we agree on are considerably distinct. In the second place, the charge is simply false. Dixon’s display showed the operation of the antipassive (-\textit{apu}) rule and of the -\textit{apu} rule; mine showed these plus EQUI, which has
been strangely downplayed from the beginning by Dixon. Additionally, I included in the display some surface types not shown in Dixon’s, and expressed uncertainty about surface forms corresponding to one configuration (two transitive clauses with the same A and O NPs but with case roles reversed from one clause to the next).

In the same section, Dixon charges that I failed ‘... to make the important distinction between A-A coreference involving a pronoun, and that involving a noun’ (Dixon 1979a: 1010). The difference, it turns out, is simply that while both nouns and pronouns can occur in a surface configuration in which EQUI is applied but the -pay and -pora rules fail to apply, this construction is more common with pronouns. Dixon interprets this now as due to the different status of A case-marking for pronouns (nominative) and nouns (ergative).

I have no quarrel with this claim about relative frequency, and some kind of connection with case-marking asymmetries is reasonable (I would add, though, that some pronominal categories distinguish all three of A, S, and O and that some human nouns have an optional accusative form producing a similar tripartite system: Dixon has not discussed the effects, if any, of such morphological variation on the matter at hand). My point is that while Dixon berates me for not stressing this statistical imbalance, he is himself guilty of consistently downplaying the significance of this construction as a whole (where just EQUI applies, deleting the A in the second clause). This construction is conspicuously absent from the tabular display which he says is ‘virtually identical’ to mine, while I present it and attribute considerable significance to it. As for Dixon’s own emphasis on differences between pronouns and nouns in syntactic behaviour, it is possible to cite any number of quotations in which Dixon categorically equates the two syntactically. For example:

And although pronouns in Dyerbal show nominative/accusative inflection, the rule for pronoun coordination is identical to that for noun coordination: there must be an NP common to the two clauses, and it must be in S and O function in each clause. (Dixon 1979b: 64)

The latter claim is simply false, for nouns as well as pronouns. Not only is the A-A type with simple EQUI disregarded, the -por rule is as well (unless the term ‘coordination’ is defined so as to exclude the -por rule).

Referring to items 45–46 in Dixon (1972: 376), reproduced as my example 24 (Heath 1979: 429), Dixon argues that I have mistakenly identified EQUI as the rule responsible for deleting the A of a transitive clause following an intransitive clause with S coreferential to this A. It is interesting that while Dixon has just finished protesting at slight and harmless changes in my glossing of the -pora examples (cf. above), he is here quite capable of altering without explanation glosses which he himself gave in 1972 for a textual passage. In 1972 the gloss for the second (transitive) clause was given as: ‘[He] brought the smaller one down across his thigh’, where the bracketed pronoun represents deleted NP corefere-ntial to the S of the preceding clause. I regarded (and continue to regard) this as a transparent example of EQUI (cross-clause coreferential deletion) involving surface S-A cross-clause NP-pair. Dixon did not previously discuss this example (or others like it), and his own gloss strongly suggests that an anaphoric deletion process (EQUI) has applied. Now, however, he claims that the deletion is purely clause-internal, and cleverly presents the example only in the form of a significantly altered translation as ‘She (O) was brought down (by him) (A)’ (1979b: 1019). He provides no criteria for assigning this deletion to a clause-internal process, and I submit that it would be impossible to do so since we clearly have cross-clause coreference here and since all other surface combinations involving shared NPs in nuclear categories (A-A, A-S-O, S-O-S-O) are better deletion of the second NP. It is quite possible that surface S-O-A EQUI (the only remaining possibility) occurs in cases where the two clauses are separated by a longer-than-normal intonational boundary or pause, but this is because in more tightly-knit sequences of such clauses the -pay antipassive (R-ANTI) regularly operates, intransitivising the second clause so that there is no longer surface S-O-A coreferentiality. Instead there is surface S-O-S-O (specifically, S-O) coreferentiality, and the coreferential NPs are deleted. To demonstrate that clause-internal (rather than anaphoric) deletion is involved in such examples as the present one, it would be necessary first to describe the conditions under which the clause-internal deletion occurs (especially in isolated clauses), and then show that the same conditioning factors are present. I do not see how this can be done with the present example, since the deleted NP is clearly coreferential to a nuclear NP in the preceding clause (i.e. not indefinite, insignificant, or predictable from verb semantics), so an anaphoric (EQUI) analysis is pointed to. It is insufficient simply to show that clause-internal deletion can occur (with unspecified conditioning factors) and then to use this rule to ‘account’ for apparently aberrant textual examples.

Dixon concludes with a stirring self-eulogy, comparing his own high standards of scholarship with lower ones attributed to me. His evidence for this consists primarily in the fact that my Ngandi grammar (1978) contains, in the introduction, a disclaimer of infallibility, while Dixon can find no similarly humble statements in his own works. He contrasts the many years of work he has devoted to Dyerbal with the few weeks I spent on Ngandi, and leaves it to the reader to decide whether this is relevant to the present controversy.
I will permit myself to point out that I was hired by a committee of which Dixon was the chairman and driving force to undertake a depth study of another language, Nungubuya, and optionally 'survey' work on adjoining languages like Ngandi; I was actually discouraged from spending too much time on the other languages for fear that I would not complete the major study of Nungubuya. Under these circumstances Dixon's effort to impugn my standards of scholarship by citing brevity of fieldwork on this one language is incompatible with the angelic self-portrait with which he concludes his reply. In any event, Dixon cites no specific defects, gaps, or contradictions in my Ngandi volume, so the attack is as empty as it is irrelevant.

It seems to me that very little is to be gained by further discussion of the issues dealt with in this section. I would have preferred that they had not been given so much prominence in Dixon's reply to my paper. I turn now to two sections on issues which are of somewhat greater theoretical interest.

2. INST-PROM

Here the question is whether to recognize a single transformation (my INST-PROM). Dixon continues to insist that there are two 'really quite different rules' (1979a: 1008), comitative (with underlying intransitive clause) and instrumental (with underlying transitive clause). As these definitions suggest, the two are in completely complementary distribution. Both involve adding the suffix -mal-ihbal to the verb, and both involve the promotion of a nonnuclear NP to surface direct object (O) status. Dixon stresses that there are formal and semantic differences. Formally they differ as follows: for comitative we have underlying S becoming surface A with no demotion of O; for instrumental we have underlying A remaining A but with demotion of underlying O to Dative. It is obvious that these are automatic consequences of the difference in underlying transitivity; demotion of O occurs with transitives because only transitives have an O in the first place by definition, while the shift S → A is obviously limited to intransitives since only intransitives have underlying S. Moreover, my point was that had Dixon preferred an accusative-syntax analysis of the language he could have combined underlying S and A as 'subject', eliminated the S → A part of the rule for intransitive bases, and brought the intransitive and transitive parts of the rule closer together. On formal grounds there is absolutely no reason to consider breaking up INST-PROM into two rules.

As for the semantics, Dixon is forced to acknowledge that the semantic category of the promoted NP may be 'instrumental' in the intransitive as well as transitive case; the only difference is that with intransitives this NP can also be (and usually is) semantically comitative. If Dixon is serious about separating rules involving different semantic case categories, it follows that the 'instrumentive' rule will have to be reformulated so that intransitive as well as transitive bases are involved. If this is done, even if a separate 'comitative' rule is (unnecessarily) proposed, all of the theoretical remarks I made are applicable.

I dwell on this point because it illustrates the way in which a linguist who believes strongly in the ergative-syntax analysis of this language can disregard possible evidence for the reverse (accusative-syntax) interpretation simply by combining or splitting transformations, or by extending the scope of some of them (as in the deletion example discussed earlier). It is clear that the modest semantic differences between the 'comitative' and 'instrumentive' rules are small compared to differences among subtypes of other rules which Dixon regards as unit transformations (such as the -pay antipassive). Without even disagreeing on facts, two linguists with different initial assumptions about syntactic structure could arrive at directly opposing analyses (and typological categorizations) of Dyirbal. I return to this point below in another context.

3. Antipassive rules

Morphologically we have two distinct antipassive rules, both demoting or deleting underlying O and hence changing underlying A to S of surface intransitive clause; they are the -pay rule and the less well known false reflexive (a special use of a 'reflexive' verb form). Originally Dixon said this about the differences between the two:

A false reflexive is very like a -gay construction. It appears to have much the same syntactic purpose — to put an A NP (which is in ergative inflection in the underlying simple sentence) into nominative case, so that it can be incorporated into a topic-chain. (Dixon 1972: 91)

He went on to suggest that the difference was modal (potential versus actual/realized) rather than syntactic:

Citing passages elsewhere in Dixon (1972) in which the false reflexive was described as a clause-internal O-deletion process, I argued that the false reflexive had this as its primary function (as in four of the five published textual examples), though it also could be used as a coreference-indicating process involving two clauses. In the fifth textual example, and in one other possible (i.e. morphologically ambiguous) textual example, it
appeared that the co-reference-indicating function might be a special usage in purposes constructions working on a slightly different basis than the -pay rule, but I expressed uncertainty on this point.

In his recent reply, Dixon is to be congratulated for attempting to provide new data to clarify unresolved factual issues. Unfortunately, while these data are apparently designed to support his original statement that the -pay rule and false reflexive differed only modally (not syntactically), the data presented have left me highly confused. Note that in the passage quoted above Dixon spoke of a single function (syntactic purpose). On the other hand, he now breaks both the false reflexive and -pay rules into no fewer than five 'functions' (1979a: 1009): (i) clause-linking; (ii) clause-internal with O changed to surface Dative; (iii) clause-internal with O deleted; (iv) needed for verb to which it is attached to agree in transitivity with another verb in the same VC [verb complex]; (v) needed to put demonstrative pronoun into surface absolutive case.

Although Dixon cites at least one textual example in his sample for each of these functions with both rules, his data confirm my view that function (iii) (deletion of O, clause-internal) is associated largely with the false reflexive. Nevertheless, the fact that several examples of this function with the -pay rule are cited comes as something of a shock. Except for a minor subtype of the -pay rule with imperatives, Dixon did not describe clause-internal O-deleting -pay antipassives anywhere in his 1972 grammar, nor anywhere else (to my knowledge) in his voluminous writings on the language. In his recent attack on Janice Jake (cf. the end of his 1979a reply to my paper) he has categorically denied once again that a -pay clause can occur in isolation (i.e. he repeated his assertion that such clauses occur only noninitially in absolutive-chains, aside from the imperative examples). I had accepted without question Dixon's factual discussion of the -pay rule, though I had found reasons to question the facts involving virtually all other transformations. Although Dixon gives no examples in his paper (1979a), and I found none in the published texts, the revelation that the -pay rule can occur clause-internally will considerably alter our understanding of the role of this rule in the grammar. If Dixon's new position is confirmed by scrutiny of the relevant textual examples when these are made public, it will of course be necessary for me to re-evaluate my presentation of the two antipassive rules, showing more functional overlap than I had previously recognized. I will be interested in seeing whether the clause-internal -pay-clauses are easily distinguishable from clause-linking (coreference-indicating), or whether some ambiguity is permitted. I want to closely study the coding procedures used by Dixon to identify the 'function' of particular textual examples, and will want to examine the possibility of lumping or splitting some of the five functions recognized by Dixon. Of course, since some types are not yet represented by a single published example, I cannot properly undertake these tasks now.

I will note, however, that function (v) involving demonstrative morphology is, in my view, simply a special case of function (i). Contrary to some statements made recently by Dixon, no demonstrative category (proximate, unmarked, or distant) lacks ergative case forms and A NPs can freely take such demonstratives. Although exemplification of the relevant cases of antipassive is almost nonexistent, from the available descriptions it seems that we have a double-predicate construction of the type '(X is this, (X) hit me' in which regular clause-linking antipassive (presumably mostly -pay) applies in the second clause (Heath 1979).

Dixon grumbles about this analysis but it is perfectly reasonable, involves a demonstrative-predicate construction found independent in the language, and is roughly similar to double-predicate constructions with demonstratives found in other Australian languages.

As for function (iv), I confess that I have no idea what this is unless it is just a subtype of function (i) (clause-linking). If antipassives are used to create surface transitivity agreement, I presume we must have an intransitive and one conjoined transitive clause (perhaps in either order), with the transitive one then intrasubject by antipassive. If, however, the underlying A of the transitive clause is coreferential to the underlying S of the intransitive clause, antipassive will occur through function (i) in the usual way — obligatorily (I would say, almost obligatorily) if the transitive clause is second and optionally (by A-ANTH) if it proceeds the intransitive. If my guess is wrong and Dixon is here referring to some new usage of the antipassives in which this particular underlying coreferentiality is not found, then a Pandora's Box has been opened and all of us (starting with Dixon) will have to go back to the drawing boards.

It should be apparent from these remarks that massive clarifications (and exemplification) are in order immediately. It is astonishing that with all of the published material on Dyirbal, and with all of the attention lavished on the -pay rule, it is only now that such significant new data are presented (or hinted at) by Dixon — and in the guise of defending his 1972 analysis! If I have previously misunderstood the relationship between the -pay and false reflexive rules, it is not because I challenged Dixon's presentation but precisely because I accepted his descriptions of the -pay rule at face value and confined my revisions to the area of theoretical interpretation. I do not believe at this point that new data along the lines now suggested by Dixon will pull the rug out from under my general understanding of the language; I suspect that the 'functions' of the antipassive rules are reducible to those I recognized previously so that the
major changes involve the distribution of the two antipassive rules over this range of functions. I would add that I have strongly supported the recognition of disjunctive functions (and structural descriptions) for Dyirbal transformations and have generally opposed analyses claiming a single function (and structural description) for each rule. I thus recognized several -nay rules, at least two false reflexive rules, etc. (meaning disjunctive structural descriptions for the ‘same’ mechanical change). Even after combining Dixon’s instrumentive and comitative into INST-PROM, I went on to distinguish clause-internal from cross-clause functions. Consequently, I see no reason to think that additional information concerning antipassives will force major revisions in my overall approach to the language.

4. Evaluation of data

I am delighted to hear that Dixon is ‘hoping eventually’ to publish an adequate text collection, and I am merely sorry that a definite and quotable collection has not been available during the eight years of controversy since Dixon (1972). It is already clear that the classification of textual examples (e.g. involving purposive constructions) is complicated and that Dixon and I disagree on classificatory procedures; only a large published text collection will permit more thorough evaluation of the differences.

It is also important that textual patterns be contrasted with materials obtained in elicitation sessions, and perhaps Dixon is the only person in a real position to undertake this. I am disappointed that virtually no information on syntactic differences between material in spontaneous texts and in short elicited utterances is yet available. We need such information for analysing -nura participles, relationships among alternative output types for various referential configurations (including surface A-A coreference with EQU1), and many other phenomena in which discourse-functional factors are involved.

I am therefore disturbed by the fashion in which Dixon disparaged surface syntactic structures which he ‘... could not get informants to replicate’ (1979a: 1004). From my own fieldwork I can cite a number of morphological and/or syntactic configurations which were used by informants in spontaneous texts (i.e. in something approaching natural discourse) but which the same informants refused to repeat in isolated sentences. I have strong suspicions that similar variation occurs in Dyirbal. In those areas in which alternative surface structures are possible and in which delicate patterns of usage differentiation are suspected, it is essential that we know not merely gross aggregate statistics, but also the direction and approximate extent of change from free-flowing textual usage to usage in isolated elicited examples. Sentences which informants will approve of when uttered by the linguist are of virtually no interest in this regard, and it is unfortunate that so many of the published Dyirbal examples are of this type.

5. Accusative versus ergative syntax

I have shown, above and in Heath (1979), that decisions on how to classify Dyirbal as a whole in the ergative/accusative syntactic typology require arbitrary choices of criteria. Dixon’s decision to classify Dyirbal as maximally ergative in syntax really involves two distinct (and separable) arguments: (i) that S/O behaves as a unit in contrast to A in the formulation of transformations, pointing to underlying identity of S and O (represented in tree diagrams); and (ii) that transformations operate to produce surface coreferentiality of S/O NPs in conjunct clauses. I can think of only one way to test the first claim, and that is to formulate the various rules in transformational terms (with careful recognition of alternative formal possibilities for the same rule) and then examine the clustering of S with A, O, and other nominal categories. I did this for all relevant transformations, dealing with structural descriptions as well as structural changes (i.e. not handling structural descriptions by means of separate constraints). I found virtually no specific association between S and O to the exclusion of A, except for -nay (multiple/distributive suffix) and waru (‘wrong’). If my objections to Dixon’s analysis of certain purposive examples are laid aside, there was also an underlying well-formedness constraint for purposives requiring that the NP in the matrix clause coreferential to a NP in the lower clause be the S/O, but I felt that those textual examples were probable counterexamples. Overall, the evidence for ergative syntax on these counts is negligible, and directly comparable ergative features of these types are found in many accusative languages.

As to the surface-target interpretation, I showed that it ‘accounts’ only for certain instances of antipassive and certain instances of INST-PROM. It does not account for the operation of the -nura rule, EQU1, or other important syntactic processes, and does not account for all instances of antipassive or INST-PROM. It is a teleological argument which is not readily derivable from the raw facts. My own interpretation of Dyirbal syntax was also teleological, but had the advantage that it accounted for a much wider range of data (notably the interaction of EQU1 and -nura with
the antipassive rules, and special uses of INST-PROM and other rules in purposes due to the nonapplicability of -pura there. Moreover, the functions identified were straightforward (chiefly the disambiguation by syntactic means of nominal reference in clause transitions) and universal rather than posited solely on the basis of Dyirbal itself. Even if it is claimed that this functional analysis is merely equally good as the ergative-syntax interpretation, it follows that the latter is analytically arbitrary as I argued. My own position, of course, is that the functional analysis is superior to the other analysis since it accounts for a broader range of data in a more straightforward and direct way.

Although I did not propose an 'accusative' analysis of Dyirbal syntax, I did point out that any linguist who favoured such an analysis could find as much 'evidence' for it as Dixon found for the ergative analysis. I have already emphasized this point vis-a-vis INST-PROM, showing that treating it (or at least its clause-internal manifestations) as a unit rule permits recognition of a 'subject' category (A/S). In addition, there is the crucial fact that Dyirbal has several rules converting A into S or vice versa, and several converting O into nonnuclear cases (Dative, Instrumental) or vice versa, but no productive rules involving conversions crossing these sets (e.g. S - O or O - A) even though many other languages have them (causative formation, etc.). It is obvious that this could be used, if we wished it, to support a strong typological characterization of Dyirbal as 'based on' the syntactic identity of underlying S and A; among other things this would greatly simplify the formal statement of rules. A similar identity (or at least affinity) involving O and certain nonnuclear categories could also be considered. I do not support this for the simple reason that I do not think that S/A identity explains the particular combination of rules found in Dyirbal; these rules function in a coordinated and effective way to signal coreferential (and to some extent noncoreferential) relationships involving all nuclear (and some underlying nonnuclear) NPs in adjacent clauses, and it is a serious error to think that placing Dyirbal on an ergative/accusative typology (whether categorical or continuous) will provide insight into the structure of the syntax. Indeed, I see no nonarbitrary way thus to categorize the language in the first place; different initial classificatory procedures, all apparently reasonable, can lead to radically opposed classifications. It would have been useful to have a detailed reply from Dixon on this point, which is far more central to the controversy than my choice of title, the number of months I spent on Njandi, or other extraneous matters which take up much of the space in Dixon's reply.

### 6. Conflict or Harmony?

Although in his final crescendo Dixon makes a sweeping reaffirmation of his original theory and dismisses my own contribution, in his introductory remarks (somewhat less charged with rhetorical fervour) he is more kind, and says in effect that his theory and mine are simply operating at different levels of analysis and are thus not necessarily in conflict.

In assessing this remark we must first decide whether Dixon's analysis of Dyirbal is, in the technical sense, simply a typological classification or a theoretical interpretation. By the latter I mean, roughly, a collection of interrelated claims about causal (including reciprocally causal) relationships linking linguistic features to other linguistic features and/or to external factors such as linguistic 'functions'. Dixon's analysis is, in fact, a combination of typology and theory; to the extent that it is a typology it is difficult to criticize, but to the extent that it is a theory it is open to criticism.

My criticisms of the typological features are simply that the criteria actually used in assigning Dyirbal to the 'ergative syntax' category are partly hidden, as I showed in the preceding section. However, once these criteria are made explicit (and disregarding certain relatively small matters of factual detail), it would be quite possible for Dixon to propose a rigorous typology in which Dyirbal would have to be assigned to the ergative pole. Because this is a matter of definitions and criteria, it cannot be directly attacked, except by claiming that the typology is less useful or meaningful than other typologies.

However, to the extent that Dixon's analysis makes causal claims it is directly incompatible with the functional theory I proposed. For example, Dixon's view is that Dyirbal speakers operate antipassive rules in most cases as a device for achieving surface S/O coreference, and the term 'function' or 'purpose' is used repeatedly in this context. Consider this recent quotation:

> Pseudization (antipassivation) is largely motivated by need to place a deep O(A) NP in derived S function, in order to meet certain syntactic requirements (and/or preference of discourse structuring . . .) (Dixon 1979: 119).

My interpretation is that passives and antipassives have a wide range of distinct functions within a language or across languages, and that the -pura antipassive in Dyirbal is primarily a way of indicating coreferential relationships; it forms part of a more complex, functionally integrated system with EQUI and -pura such that each rule is complementary to the others. I argue that this complementarity is not accidental, and that the
evolutionary processes which produced the attested system were crucially affected at every stage by such functional considerations. I place little or no emphasis on the fact that surface coreferenceentiality of S/O NPs results from distinct underlying structures in some cases; I regard this as a byproduct of more important form-function interrelationships, and certainly do not believe that surface (or shallow-structure) targets are the motivating forces behind antipassive and passive rules. Now it may be that Dixon secretly agrees with some parts of my theory; he might, if pressed, acknowledge that my analysis of EQUI is not entirely without merit. I cannot say this for sure since Dixon’s reply simply does not discuss such issues.

It is important to emphasize that the present debate concerning Dyirbal is a local manifestation of the more general ongoing debate between proponents of essentially ‘pure’ syntax and proponents of discourse-based functional syntax. Dixon correctly associated his framework with ‘…a more traditional view of syntax’ (1979a: 1004) than mine, and we can connect Dixon more directly with the classical generative grammar of Chomsky and his associates, and me more directly with functional syntacticians such as Kuno and Halliday. Dixon differs somewhat from Chomsky in stressing the ergative/accusative typology, but they have similar basic assumptions about the role of universals versus language-specific features, levels of syntactic structure, and so forth. I differ somewhat from Kuno in spending less energy showing that discourse factors affect the operation of particular transformations, and more on describing broad patterns of functional interaction and complementarity involving several transformations and morphosyntactic elements with emphasis on reference-disambiguating functions. But broadly speaking the Dixon-Heath disagreement is the same as the Chomsky-Kuno debate, and it would be a serious error to think that Dixon and I simply misunderstand each other or that the issues involved are only of interest to regional specialists.

7. Some queries

Instead of sniping at critics with nit-picking remarks and declarations of infallibility, Dixon should publish a comprehensive statement of his present position on matters of fact and interpretation relevant to the understanding of Dyirbal syntax. For every one of the major transformations there are important unanswered questions; I had previously thought that at least all the essential facts on the -pay antipassive were published but it now appears that even that was incorrect. It is absolutely essential that the contradictory remarks by Dixon on some transformations be resolved and that gaps in the data be filled. Dixon has every right to issue further statements in favour of his polemical position, and I am even prepared to endure further attacks on my Ngandu grammar, but the one thing Dixon must do is provide a comprehensive description of relevant facts as he sees them. I conclude here with specific suggestions as to which issues need clarification.

1. As Dixon himself has indicated (1979a: 1009), a more extensive analysis of antipassives (-pay and false reflexive) is in order (see my section 3, above). We need to know what coding procedure was used to identify instances of each of the five ‘functions’ of these rules, and whether some surface structures are ambiguous as between clause-internal and complex functions. In particular, we need more information (and examples) concerning clause-internal uses of the -pay rule, and clause-linking uses of the false reflexive (particularly in purposives). Can the false reflexive be used, as one (and possibly two) textual examples suggest, to indicate coreferentiality involving the A (not O) of transitive matrix clauses in purposive constructions?

2. How can we reconcile Dixon’s statement that -para functions to bridge two distinct absolutive-chains with his separate account of three-clause constructions showing that a -para clause is skipped over by absolutive-chain (coreference-chain) rules (see Heath 1979)?

3. It is necessary to present a substantial list of textual purposive constructions in which surface coreferentiality involving the A of the matrix clause is present or at least can be suspected. Readers can then draw their own conclusions about them; Dixon’s declaration that all apparent examples of this type have distinct interpretations is insufficient since criteria are not given for this analysis. Most of the relevant examples are syntactically ambiguous and it is important that a number of examples be published so we can determine (on semantic and other nonsyntactic grounds) what is going on.

4. As indicated earlier, analysis of the factors at work in clause-internal NP-deletion (affecting underlying A or O, in the latter event triggering antipassive rules) is needed.

5. It is essential that we have statistical evidence concerning frequency of alternative output types particularly for coreference-chains of the type X saw Y and (X) heard Z with coreferential A NPs but noncoreferential O NPs. We can get (i) just EQUI deleting coreferential A; (ii) -para (-pay plus -para) in the first clause; (iii) anticipatory -pay (A-ANTI) in the first clause and retrospective -pay (R-ANTI) in the second. Dixon seems to be referring to this matter in complaining that I lay stress on configurations found ‘only once or twice’ in the data and that I
deemphasize "... some of the most frequent constructions-types (of which I have thousands of examples)" (Dixon 1979a: 1004). I would like to see just how many examples Dixon really has of these three output types in his texts, and additional information concerning what happens when speakers shift into more highly self-monitored speech in elicitation sessions. I do not think that Dixon has thousands of textual examples of configurations (ii) and (iii), since in the published texts there is just one example of (ii) and none of (iii) (indeed there is only one example of A-ANTI). My suspicion is that the relative frequency of (ii) and especially (iii) increases as speakers move into self-monitored speech, but this is purely conjectural at this point.

6. Also using textual samples, how does anja (topicalizing or focus particle) really pattern? Is it used, for example, in role-switching configurations like 'X hit Y and Y hit X (back)'? If so, to which NP in the second clause is it characteristically preposed? What, explicitly, are the procedures used by Dixon to decide that anja can apply to S/O NPs but not to A NPs? (Normally Dixon states that anja directly precedes the focused constituent, but in the case of S/O NPs he allows noncontiguous anja (at the beginning of the sentence) to focus on the S/O NP.)

7. Does Dixon object, factually or theoretically, to the interpretation I gave of EQUI, which is significantly different from the interpretation implied in Dixon (1972) and explicitly stated by other linguists who believe in the ergative-syntax analysis of the language?

8. How do conditional constructions operate, for a variety of underlying referential-network types?

9. Finally, a correction of the erroneous statements about Dyirbal demonstratives in Dixon (1979b) is in order.

Dixon states here and there that he is happy to 'let the reader decide' this and that. Unfortunately, such phrases occur chiefly as cute rhetorical devices in some of the more combative paragraphs. Readers should be permitted to decide not merely whether the brevity of my Ngandi fieldwork disqualifies me from commenting on Dyirbal; rather they should be permitted critically to review all of the theoretically significant data and should be exposed to alternative theoretical viewpoints and alternative analyses of specific passages.

References


