

COST ACCOUNTING VS. EXPLANATION:

A REPLY TO A REPLY

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Talmy Givón's reply to my paper (Heny [1972]) raises several important questions. His objections amount to the claim that my explanation of the relationship between Shona nouns and adjectives was far too costly--and was in any case quite unnecessary. There seems to be a fundamental difference between Givón and myself over what it is that constitutes a sound linguistic argument, and in my reply I shall simply try to show why I do not believe that Givón's observations diminish in any way the force of my original arguments. In this way, the basic disagreement may be resolved.

I take it as axiomatic that the goal of a theory of language is to provide explanations of linguistic phenomena. An explanation may be effected in a number of ways, but it involves, essentially, the setting up of a hypothesis from which the observed and otherwise aberrant phenomena can be deduced or predicted. An explanation may be simple or complex depending upon the circumstances. In judging between rival explanations relative complexity may be an issue, but it is totally pointless to object, in a vacuum, that an explanation is complex. Likewise, it would be quite wrong to compare an explanatory but complex account of a set of phenomena with a simple description of those phenomena.

In the present case, I know of no systematic theory other than the one set out in Heny [1972] from which it is possible to deduce (fairly precisely) that "nouns" and "adjectives" in Shona will exhibit the relationships discussed in that paper. Although admittedly somewhat programmatic, it is the only existing explanation of those facts, and hence there is no point in remarking that simpler ways exist of describing the facts. Of course there are. I explicitly formulated and rejected one such mere description in my paper: phonological

rules can be used to delete that nasals (and so on--Givón is wrong in thinking that it is only the nasals that are relevant--see Heny [1972]), and these rules must work everywhere except before nouns and adjectives. In any such solution the fact that nouns and adjectives are alike in a number of ways, including concord, has to be regarded (from the point of view of a synchronic grammar) as purely a matter of chance.

It is obviously inappropriate to compare an explanation (such as mine) with any mere description, except under very unusual circumstances: when a synchronic explanation of the facts is in principle to be rejected. We can, however, reject an explanation out of hand only if it is possible to demonstrate:

- (a) that no plausible explanation can be provided, or
- (b) that there is independent evidence showing that the regularity in question must necessarily be regarded as a chance regularity, or
- (c) that there are other (e.g. diachronic) explanations of the phenomena which operate in such a way as to render a synchronic explanation inappropriate.

I believe that Givón's three objections are really addressed directly to these three points (although he does not say so) and that his first objection, under this interpretation is not that my explanation is more complex than certain mere descriptions, but amounts rather to the claim that my account is simply too complex (or costly) to be plausible at all. But when that objection is examined in a little more detail it amounts to very little. The main problem was that I found it necessary to articulate a very unusual little sub-class of nouns which exhibits a number of peculiar semantic and syntactic characteristics. (As a matter of fact I called attention to these facts myself, in considerable detail.) However, any serious account of Shona grammar will have to make provision for a peculiar little class of adjectives--whether they are nouns, or just noun-like. I do not, therefore, believe that Givón has shown any reason for rejecting my explanation on the grounds that it is in principle untenable.

His second objection addressed itself to the possibility that the observed relationship between nouns and adjectives was to be regarded as purely a matter of chance. In particular, Givón observed that other closely related languages failed to display the same regularities, and in Bemba, for example, there was a slight trace of the phenomena, which was clearly to be regarded as superficial and purely accidental in the synchronic grammar. Hence, there was no reason to suppose that the Shona facts deserved more serious explanation. At this point, the implausibility of my theory was again suggested: an account which set up a systematic and deep difference between Shona and Bemba in this one rather minor area, was to be rejected even if it were the only available one. For closely related languages simply do not differ in such a way.

This objection, too, carries little weight. It may--and then again may not--be the case that related languages can differ at a rather deep level. My argument was specifically limited to Shona. Whether or not it applies in some form to Bemba I do not know, but that is irrelevant. In the languages of the world, the behavior, membership and very existence of the class of adjectives seems particularly susceptible to variation. Any theory of language will eventually have to cope with that fact. And we do not at present possess enough data to decide whether it is only at a very superficial level that the variation occurs. In particular, it should be noted that if my explanation of the Shona facts is correct, then it follows that languages (quite closely related languages) do indeed differ in this area at a rather deep level. It is obviously quite wrong to assume that they cannot differ in that way in order to demolish my argument! But in any case, there is strong prima facie evidence that some kind of explanation of the facts is called for.

Givón himself provides some: it seems unlikely that a purely superficial, chance regularity in Shona would at the same time have been maintained in Swahili (see Givón [1972])--and in most of the other southern Bantu languages, and in dozens of others in other Bantu areas (see Guthrie [1967]).

It must therefore be in his last argument, if at all, that Givón succeeds in establishing the fact that a synchronic explanation is not required. He argues that the observed relationships derive solely from the historical origin of the adjective class in Bantu. Since, he claims, morphology "lags behind syntax" there is a "diachronic explanation" of the phenomena and the synchronic grammar of Shona need (should?) not attempt to account for what, again, appears to be no more than an accident when viewed synchronically. In essence the explanation is that the adjective class arose in the first place as a result of certain noun stems being used as modifiers and, presumable, bequeathed a noun-like character to the class as a whole. However, this "explanation" is not part of a systematic theory or hypothesis from which the Shona facts will automatically follow. Givón's ideas about the relationship between syntax and morphology are extremely interesting, and may well throw light on the original word order of Bantu languages (Givón [1971]), but he has not related them to this set of phenomena. Nor is it easy to imagine how that could be done. For one thing, as Givón himself [1972] observed, there are adjective stems which are related to verb stems, while a few others seem to have arisen quite independently. A **theory** of "morphological lag" seems to be in principle unable to account for the fact that stems which were never noun stems adopted a noun-like morphology. This is an unexplained innovation. There is, likewise, no way in which a lag theory can account for the fact that after the original noun stems began to be used solely as modifiers they acquired the ability to co-occur with prefixes of any class--but still only of the shape associated with nouns. Again there is crucial innovation. Past syntax is quite irrelevant.

In any case, Givón's theory is unable to account for the fact that the class of adjectives arose at all, and having arisen persisted as a peculiar aberration (a superficial accident) in a large number of languages. Why this particular accident, why this lag? No matter how good a diachronic explanation of certain aspects of the change, that persistence, which is the persistence of a syntactic class, and not merely of a morphological aberration, calls for an explanation. I conclude,

therefore, that none of Givón's objections have thrown serious doubt on the explanation provided in Heny [1972], and hence that that explanation, however complex it may be, must stand until a better is available. Adjectives in Shona are a special class of noun, and languages, even closely related ones, can differ at a rather deep level.

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