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There's more than one way to make sense of one-way implications – and sense they need to be made of

by FRANS PLANK

1. Understandable though Michael Cysouw's worries are over the right interpretation of non-absolute universals as expressed in the logical form of material implications, or of chains of such implications, I don't see how they lead to the conclusion that there can only be (mutual) correlations but no (one-way) dependencies among parameters showing crosslinguistic co-variation. Unless co-variation is random or spurious, and however tricky it is to determine statistically whether or not it is random and to determine linguistically whether or not it is spurious, it is bound to be reined in by dependencies since dependency is what much of grammatical (and also lexical) structure is founded on.

In actual fact, when Cysouw proposes to replace implications by correlations plus markedness evaluations of the correlates, it would seem that through markedness he is effectively reintroducing an asymmetry that is but dependency in disguise. But then the notion of markedness invoked by Cysouw, intended to be applicable in what he takes for the appropriate reinterpretation of ANY implication regardless of the nature of the implicans and the implicatum (as summarized by him in (3)), is a rather indiscriminate one, and one

wholly lacking structural grounding; that's why he needs no actual examples but effectively only frequency figures of crosslinguistic distributions to argue his comprehensive case against one-way implications.

In response I submit that abstracting away from the structural substance of co-varying parameters and from the nature of their relationship in linguistic systems is seriously misguided, even for methodological purposes such as Cysouw's in the present discussion paper: this is precisely what the appropriate linguistic INTERPRETATION of implications, including the conception of the co-variables as dependent or interdependent, depends on.

To make this point three examples will have to suffice, but I believe they are representative of the thousands of implicational universals on record, or at any rate of the hundreds that are convincing as descriptive generalizations. First, they are implications which are undeniably one-way: whether they state that *p* demands or prohibits *q* (i.e., that *p* renders *q* necessary or impossible), or also, less categorically, that *p* encourages or discourages *q* (i.e., that *p* renders *q* more probable or improbable than it would be otherwise) – as biconditionals, with "and vice versa" added, they would be false, excluding one property combination too many. Second, the linguistic stories behind these one-way implications are intriguingly different – and these are stories that should perhaps be revised and embellished, but not hushed up or overwritten by figures.

2. When you scroll your way through *The Universals Archive* at http://ling.uni-konstanz.de/pages/proj/sprachbau.htm (instructed by Plank & Filimonova 2000), filtering out those alleged universals which are non-conditional or otherwise not genuinely implicational (like the many "provided that *p*, *q*" universals on record), you'll find many implications sharing crucial characteristics with (1):

(1) If relational alignment (for purposes such as case marking or agreement/cross-reference) is ergative, then, with more than chance frequency, basic word order is verb-final or verb-initial.

Let's grant – not implausibly to the best of my knowledge, though without statistical tests having been performed on the parameters individually or in combination – that (1) is statistically valid for known languages responsibly sampled for genealogical, areal, and especially structural diversity. Let's also assume, as is our custom, that all known (or knowable) languages are structurally speaking not such a random subset of all languages ever acquired by *homo sapiens loquens* (and perhaps abandoned or otherwise extinguished without a trace) as to render extrapolations about what are humanly possible grammars hopeless, and sampling from known languages futile. There is no question either that the truth of (1) could be one or the other paradox of material implication: there are languages for which the antecedent statement (the implicans) is true, and

there are languages for which the consequent (implicatum) is false. Also, the languages for which the implicans is known to be true do not all come from one family or area, in which case (1) would (or at any rate, could) have been but a statement about properties jointly inherited or borrowed.

The compound statement in (1) is a material implication, not a strict implication (or entailment): it is no mere tautology, saying the same thing twice in different words. Crucially, the implicans and the implicatum in (1) are in fact so different as to be INTRINSICALLY unrelated – or, if you will, there is at best a rather distant systematic relationship insofar as alignment, especially for the purposes given in parentheses in (1), and basic order are both to do with the overt marking of noun phrases in grammatical core relations. However, how the single core relation in intransitive clauses compares to the two in transitive clauses, which is what alignment is about, has no STRUCTURAL bearing on the basic ordering of subject, object, and verb in clauses of all types, nor vice versa. (LOGICALLY, the two parameters are independent anyhow, as they have to be for implications to have any empirical force to begin with.)

Being intrinsically so independent, there is no way to construe the two parameters in (1) as being DIACHRONICALLY related, in the trivial sense of successive generations of members of the same speech communities being able to reanalyse one parameter (or its value) as the other – like reanalysing duals as plurals or adfixes as infixes, to anticipate my other examples. Subjects, objects, and verbs can certainly be reordered from one generation to the next, and basic orders before and after the change may conform to different alignment patterns; thus, SVO & VS, rarely attested, is unambiguously ergative (though not unambiguously verb-medial, owing to the verb-initiality of the intransitive clause), and it may conceivably follow upon a stage with VSO & VS, accusatively aligned when reference is made to the post-verbal position and ergatively aligned when reference is made to clause-final position (but unambiguously verb-marginal). Nonetheless, ergative alignment as such, for whatever purposes of overt relational coding, cannot meaningfully be said to result from a verb-marginal basic order as such reanalysed.

Further, there is no plausible structural sense in which ergative relational alignment, the implicans in (1), could be said to be marked RELATIVE TO verb-marginal basic order, the implicatum, even though it is crosslinguistically less frequent. If anything, the markedness of ergative relational alignment could be evaluated relative to alternative relational alignments (accusative, neutral, active/inactive, tripartite, double-oblique), and the markedness of verb-marginal basic orders relative to verb-medial basic order: ergative alignment is marked – in the sense of being crosslinguistically less frequent for most, if not all, morphosyntactic purposes – at least relative to accusative alignment; verb-marginal basic orders collectively, and also verb-final order individually, are unmarked – in the same frequency sense – relative to verb-medial basic order. Thus, the

value of the implicans parameter in (1) is marked, and that of the implicatum parameter unmarked.

(1) is a decidedly one-way implication: adding "and vice versa" would make it false by any statistical standards. Verb-marginal basic order does co-occur with relational alignments other than ergative in a sufficiently large number of known languages to rule out this value of the former parameter as a predictor of this value of the latter parameter. However, the implicational dependency only holds between these particular VALUES of the parameters involved: ergative for alignment and verb-marginality for basic order. The PARAMETERS themselves are not equally asymmetric, insofar as the implicans and the implicatum can be reversed by contraposition under negation:

(1′) If basic word order is verb-medial, then, with more than chance frequency, relational alignment is not ergative.

Overtly least asymmetric is of course another logical transformation ($p \supset q \equiv \sim(p \wedge \sim q)$):

(1″) With more than chance frequency, ergative relational alignment does not co-occur with basic word order other than verb-marginal.

Of these three logically equivalent ways of stating this statistically valid constraint on possible co-occurrences of alignment patterns and basic orders, none would seem to be linguistically more basic, giving one or the other parameter priority as a predictor for the other. Assuming, for example, that unmarked values for a parameter are more basic than marked values for purposes of stating implications would only lead to contradiction in the case at hand. With reference to parameters rather than their values, the dependency captured by (1/1′/1″), thus, is mutual rather than one-way.

Since it is unlikely that he really wants to see all conditionals strengthened to biconditionals, it is perhaps cases such as this that Cysouw has in mind when he argues against one-way implications and for interdependency as the only possible way of (testable) co-variation. (From the footnote allusions to the inflectional domain of the categorial elaboration of and syncretisms in person marking systems in his paper I don't see what exactly the relevant co-variations are supposed to be like.) But, as shown presently, what is illustrated by (1/1′/1″) is not the only possible linguistic interpretation of a relationship between parameters or their values in implicational statements of crosslinguistic co-variation.

As to good typological practice, when it is hard to deny that two intrinsically independent variables nonetheless co-vary as expressed through an implication, I'd join Cysouw in recommending to take statistical advice. Perhaps one was too naively persuaded by mere frequencies that verb-marginal basic

order is significantly more frequent in languages with ergative alignment than in languages in general (1), or equivalently, that non-ergative alignments are significantly more frequent in languages with verb-medial basic order than in languages in general (1′).

But, unless too discouraged by Fisher's Exact or whatever other statistics, I'd also feel challenged to discover missing links which might indirectly connect parameters that cannot be related directly. This could be any structural parameters equally concrete, and individually perhaps intrinsically as unrelated and functionally ostensibly as unsupportive of one another, as those in (1/1′/1″). (To hazard a hunch, for the example case they might be to do with morphological typology, with agglutination, as opposed to flexion, also tending to imply verb-marginality.) Or the unificatory responsibility could also lie with a more abstract grammatical principle, with repercussions in concrete structural domains ostensibly as distinct as alignment and basic order. The connection sought could conceivably also fall out from diachronic scenarios, with ergativity and verb-marginality, or non-ergativity and verb-mediality, as coincidentally or necessarily simultaneous stages in the development of alignment as such and of basic order as such.

When unsuccessful in the forging of links between heterogeneous parameters, one would have to concede, for the time being, that it is by chance rather than structural necessity that their values happen to be dependent one on the other (but not vice versa). This, to be honest, is the situation in which I seem to find myself as regards (1/1′/1″).

Structural coincidences may have good non-structural reasons, though. The historical contingencies of speech communities forming and dissolving and interacting, giving to and taking from neighbours in situ and when on the move, may still be holding clues as to how one structural habit has come to be practised especially in such communities as also practise another. See Nichols (1992) for ways of exploring such contingencies, as affecting alignment and basic order among other co-variables.

3. The logical form of (2) is equally that of a material implication, but, apart from being valid (to the best of my knowledge), that is about all it shares with (1).

(2) If there is a dual, then, with far more than chance frequency, there is also a plural.

Like (1), (2) is a statistical rather than an absolute truth, although the counterexamples on record are far fewer. Taking (2) for a universal claim about words rather than about languages would increase the counterexamples: there are languages that have a plural as well as a dual but where a subset of nouns,

designating natural pairs (ears, door posts, divine dualities, and such), inflect for dual but not plural.

Unlike in (1), where they are intrinsically independent, the implicans and implicatum in (2) are terms of a single inflectional category, number, and there are plausible explications of the paradigmatic relationship between them – dual conceived of as a kind of a plural, limited to any sets of twos or only to pairs (less plausibly, plural could be conceived of as an extended dual; but then the extensions here would be by ones rather than twos) – which motivate seeing one of them as marked RELATIVE TO THE OTHER. (Markedness may have to be seen as reversed, however, for nouns for persons and things typically occurring as pairs rather than as individuals.) This markedness evaluation coincides with, or rather: is reflected by, crosslinguistic frequency – which is no surprise here, since, differing from (1), the terms of the frequency comparison figure as the implicans and implicatum of one implication.

Finding a crosslinguistic distribution of the two number terms such that most (if not quite all known) languages which have a dual also have a plural while many languages with a plural lack a dual, no one (I believe) would hesitate to conclude that this is a case of co-variation where one parameter (having/lacking a dual) is dependent on the other (having/lacking a plural), but not vice versa, with the dependent and independent parameters referring to the marked and unmarked terms of the morphological opposition of number. As before, however, we're strictly speaking dealing with a one-way dependency of VALUES of these parameters: HAVING a dual is dependent on HAVING a plural. Like any implication, the truth of (2) remains unchanged by contraposition under negation:

(2′) If there is no plural, then, with far more than chance frequency, there will be no dual either.

In this version, LACKING a plural appears dependent on LACKING a dual. As always with material implications, there is a wholly asymmetric equivalent, too:

(2″) With more than chance frequency it will not be the case that there is a dual and no plural.

Still, unlike with (1/1′), one of the asymmetric renderings seems more basic here, namely (2). In terms of inflectional systems, the ultimate starting point is having no inflection for number, and beyond that, diversity is a question of the elaboration of this category from two terms (most commonly singular and plural) to richer, hierarchically structured systems of oppositions. This is also the perspective of the grammar acquirer, recreating or creating an inflectional system from scratch (even if that system sometimes ends up less rich than that

of the preceding generation of speakers). The relationship expressed through (2) can then naturally be conceived as one of LICENSING: a dual (the implicans term) needs to be licensed by a plural (the implicatum term) in the same inflectional system, with the plural itself not needing to be licensed by another non-singular number; or in other words, with the license specified in the prepositional phrase: no dual without a plural. On such grounds, the parameter referring to the marked term (dual) and its positive value (having it) – what needs to be specially licensed – will be given priority as predictor, severely limiting a choice as to the values of the other parameter, referring to the unmarked term (plural).

The bases of such one-way dependencies, as best expressed in the form of an implication, and more transparently through one version (2) than another (2′), in line with an interpretation in terms of licensing, are structural rather than statistical. Getting one-way dependencies wouldn't be such a natural result for equipollent oppositions, for example. (Greenberg 1978 bears rereading for this point, among others.) Surely, implication (1) could also be reformulated in terms of licensing: ergative alignment needs to be licensed by verb-marginal basic order; but so can (1′), and neither version finds immediate intuitive support in the essence of intrinsically independent parameters such as these or their values.

There could conceivably be a direct diachronic relation between the terms of one inflectional category; but the dependency of the (marked) dual on the (unmarked) plural, argued for on structural grounds, would be getting it wrong diachronically: duals, the parameter in the implicans in (2), are known to have sometimes been reanalysed as plurals, the implicatum parameter; but reanalyses the other way round are practically unheard of (or were, until page 57 of this very issue of *LT*: atypically, Koryak, Alutor, and Kerek appear to have reanalysed their old plurals as duals; Fortescue 2003.). Otherwise dual and plural are diachronically to some extent independent, in the sense that there are historical sources of plural inflections other than dual forms (e.g., number words, pronouns themselves inflected for number, reduplication, paucals or non-number categories semantically reanalysed) and that duals are apparently never (that is, exceedingly rarely: see above, p. 57) created from plurals (but most commonly from the numeral 'two' or also the quantifier 'both' getting grammaticalized, less frequently from non-number categories reanalysed). (See further Plank & Schellinger 2000 on diachronic vs. achronic interpretations of dual implications.)

Construing diachronic dependencies in a rather different sense as in (3) could seem but a trivial reformulation of the implication as given in (2):

(3) a. A dual cannot be innovated unless a plural already exists or is innovated simultaneously;

b. a plural cannot be lost as long as a dual exists or is not lost simultaneously.

On the other hand, considering that dual and also plural are inflectional terms subject to change and in particular to innovation and loss rather than being universally present at any and all times, (3a/b) might give a more appropriate developmental angle on their interaction. Again, in this view the two terms interact asymmetrically at each developmental step, with unmarked plural as license (or its absence as hindrance) for innovations of marked duals and with marked dual as hindrance (or its absence as license) of losses of unmarked plurals.

And there are lots of implicational universals on record which can be interpreted as stating licenses on innovations or losses – like those where implicans and implicatum define the structural extension, or diachronically speaking the steps in the gradual spread or retrenchment, of a marker or a process (e.g., If labial consonants are palatalized, then dental ones are also palatalized, and if dental, then also velar – with palatalization spreading from back to front targets). Although not such an obvious candidate, owing to inherently independent parameters not yielding a natural licensing reading, (1/1′/1″) could also be reformulated along such diachronic lines. Since it is after all only through innovations and losses that crosslinguistic diversity comes about, innovations and losses are what ought to be sampled, not languages. (Which is another point of Joseph Greenberg's (Bell 1978: 146).) And whoever wants to perform statistics would then be well advised to perform them on (samples of) innovations and losses too.

4. In the case of (4), the identical logical form of a material implication abbreviates yet a third genre of story.

(4) If there are infixes, then, with far more than chance frequency, there are also adfixes.

For all I know, (4) may even be an absolute rather than only a statistical truth. But for once let's share Cysouw's (Neo-)Platonism and assume that what can exist does exist, and will be found out sooner or later: to be sure, infixes without adfixes in the same language would be no contradiction in terms nor in grammatical structure. There are certainly WORDS or WORD-FORMS with infixes but no adfixes in languages that have adfixes in other words or word-forms.

The parameters in the implicans and implicatum of (4) are intrinsically closely related: infixes and adfixes (comprising suffixes, prefixes, and circumfixes) are but different linear realizations of affixes, occurring inside or outside of stems.

Infixes are structurally marked relative to adfixes: they make stems discontinuous, and discontinuous constituents in general are more difficult to process and store than continuous constituents, as when affixes remain outside stems. Mirroring this asymmetry, the crosslinguistic frequency of infixes is much lower than that of adfixes collectively and also of suffixes and prefixes individually. (Circumfixes also cause discontinuity and are rare.) Should the cell (–adfixes, +infixes) in one's tetrachoric table remain empty, with the cell (+adfixes, +infixes) not exactly overpopulated either, that would not be a zero to be dismissed as meaningless, notwithstanding the recommendations from Cysouw's statistics of deviations from expectations for parameters in isolation: there IS an important one-way dependency of infixes on adfixes here.

Implication (4) is one-way insofar as adding "and vice versa" would render it massively false. But the familiar other logical transformations might again seem to reveal symmetry at least among parameters:

(4′) If there are no adfixes, then, with far more than chance frequency, there are no infixes either.

(4″) With far more than chance frequency, it will not be the case that there are infixes and no adfixes.

Unlike for plural and especially dual, it is not structurally so obvious what would be the more basic values for these parameters, having or lacking. Yet there is a sense in which the value "having" for the parameter "adfixes" can be said to license the value "having" for the parameter "infixes", with having infixes thus as a predictor for the choice between having and lacking adfixes, and not vice versa, and with (4) therefore as the more transparent formulation than (4′) or (4″) – and this sense is diachronic in the most direct manner conceivable.

Unlike in (1) and (2), in (4), '$\supset$' really means '$<$'. Of course, the meaning of '$<$' itself needs interpretation in terms of kinds of reanalysis of forms in constructions and of ensuing restructurings of their representations in lexicon and grammar. Now, infixes diachronically DERIVE FROM adfixes – that is, ONLY from affixes, and ONLY by two mechanisms of change: usually through phonological metathesis, optimizing syllable structures (Gabelentz 1891: 330); rarely through "entrapment", with an outer adfix reanalysed as part of the stem (Ultan 1975). On the plausible assumption that not all adfixes will get metathesized or become stem-parts on all their occurrences, wherever infixes have been created they will always co-occur with adfixes, without whom infixes could not have come into existence. Occasional claims that infixes may have non-adfixal origins are not well supported; a "floating" feature realized as a stem-internal segment, hence akin to an infix, is attested at least once. (See Moravcsik 2000 for a concise survey of generalizations about infixes.)

Thus interpreted, implication (4) is not a universal limiting variation across languages at any and all times, thereby constraining change insofar as languages must not change so as to violate a timeless law, or at any rate not without subsequent changes remedying the offence. Rather, (4) is a law of change, claiming that particular target structures (infixes, the implicans parameter) can only result from particular mechanisms of change operating on particular sources (adfixes, the implicatum parameter), thereby imposing limits on how languages can differ, since they can only be what they could become. Lots of implicational universals on record are really laws about what can be reanalysed as what. Among them are the perhaps most famous ones of, or inspired by, Greenberg (1963): adpositions result from verbs or head nouns being grammaticalized, hence the harmonic ordering of their noun phrase complements with object and attributive noun phrases – which is what they were in the source constructions.

Dependency of the target on the source, one-way, does not necessarily mean diachronic irreversibility. In the case at hand, infixes can again become adfixes through re-externalization. There remains an asymmetry, though, but that is only captured when the story behind the implication is spelled out in more detail.

Adfixes typically result from clitics getting morphologically bound to their hosts; existing adfixes or adfix sequences or stem-parts reanalysed are alternative sources, or they can also be borrowed. Their sequencing relative to stems and to each other, although it tends to continue to reflect their syntactic ordering prior to univerbation (and what exactly accounts for the greater ease of the univerbation of enclitics than of proclitics is still somewhat controversial), is the responsibility of morphology. Infixes only arise through entrapment or, far more commonly, through the metathesis of affixes (often containing sonorant consonantal segments) and (non-sonorant) consonants at the edges of stems, in order to improve syllable structures. An explanatory account for this part of the story would have to give reasons why optimizing syllable structures should at some particular point be ranked above the integrity of continuous stems as a priority for wellformed word-forms. At this developmental stage, at any rate, infixes retain the morphologically status of adfixes, phonologically re-ordered. Faced with an affix whose overt position deviates from its morphological one, one possibility for learners is to reanalyse it as an infix also underlyingly, thereby complicating morphological representations (with continuingly phonological conditions, though, insofar as such infixes will remain edge-bound). The other, and apparently more common, possibility is once more to rerank morphological wellformedness conditions above phonological ones, with morphological adfixes as an automatic result again surfacing outside stems. (See Crowhurst 1998 for a recent case study from Toba Batak.)

The real asymmetry behind '$<$' replacing '$\supset$' in the case of (4) thus rests (i) on the more solidly morphological standing of adfixes relative to infixes and (ii) on adfixes having diachronic sources other than infixes while infixes can only be gotten from adfixes. To be sure, unlike part (i), part (ii) could be tested statistically, though one's sample would have to have a diachronic dimension; but I would trust no statistics that told me otherwise.

5. Needless to add in conclusion that, after almost four centuries of tacit or explicit approbation (the first two, less familiar, portrayed in Plank 2001), I'd be reluctant to give up one-way material implications as a tool for expressing generalizations about crosslinguistic co-variation, or to reinterpret all of them as two-way interactions as recommended by Cysouw (2003) on the supposed strength of statistics. Nor am I persuaded that doing typology I mostly ought to be doing applied statistics and next to no grammar.

No one doing grammar needs convincing that grammars are rife with asymmetry. Implicational universals abbreviate possibly long grammatical stories about crosslinguistically recurring asymmetries of various kinds. Good implications – especially when they seem valid at a glance or after testing, and their counterexamples have good excuses – are those which make sense as guides in the acquisition and restructuring of grammars. Their guidance can be that of timeless laws constraining what can depend on what else in possible grammars, regardless of linguistic experiences on the part of a learner or borrower other than perhaps those triggering concomitant choices, or that of laws of change, constraining reanalyses of particular primary linguistic data encountered by grammar acquirers dependent on other choices made in their grammars.

There IS a problem when such laws turn out to admit of exceptions. And any ONE exception, however statistically insignificant, suffices to raise questions. Sometimes linguistic and population historians have answers that the typologist will forever be lacking. Sometimes the questions need rephrasing by typologists themselves, refining their implications and making better sense of what these imply for grammars and their acquisition.

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Universität Konstanz

Correspondence address: Sprachwissenschaft, Universität Konstanz, D-78457 Konstanz, Germany; e-mail: frans.plank@uni-konstanz.de

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