REVIEW ARTICLE

On explaining language change. By Roger Lass. (Cambridge studies in linguistics, 27.) Cambridge: University Press, 1980. Pp. xiii, 186. $27.50.

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In the preface of this book, Lass complains—rightly, I think—of linguists' general unconcern with the methodological questions of their discipline. More specifically, he maintains that diachronic linguistics is (in fact, has always been) in a state of crisis, because no 'respectable' type of explanation is adequate to explain linguistic change. I hope that reading L's book will have the effect on every practitioner of diachronic linguistics that the reading of Hume had on Kant, namely 'awakening him from his dogmatic slumber'. In what follows, I shall take the value of L's book for granted, and will concentrate on what I regard as its weaknesses. I want to make it quite clear, however, that the problem raised by L is genuine—because, as will be seen, its solution (which escapes L) requires espousing a non-standard conception of the methodological status of diachronic linguistics.

1. Types of explanation. L distinguishes three main types of explanation, viz. deductive-nomological (D-N), statistical (ST), and teleological (TEL); and he analyses in successive order their capacity to explain linguistic change.

1.1. Deductive-nomological explanation. L assumes (5) that it is legitimate to define a 'non-contextual' concept of explanation, i.e. one independent of any particular subject matter or purpose of explanation; and he identifies his (9) with D-N explanation (for this concept, cf. Itkonen 1978a:4–12). This problematic stipulation determines the nature of L's entire book. Because D-N explanations (qua the only genuine explanations) are based on deterministic laws, and because no such laws exist in diachronic linguistics (for a classic statement of the issue, cf. Meillet 1921:15–16), it follows that linguistic change cannot be (genuinely) explained at all. L repeats this conclusion over and over.

The lack of deterministic laws may be interpreted in more than one way. It may be attributed either to ontology (of language), meaning that such laws simply do not exist, or to methodology of linguistics), meaning that they exist, but linguists just have not been clever enough to find them. The precise import of these different interpretations will be discussed in §1.2 below.

Because diachronic linguistics has no D-N explanations at its disposal, it is unable to predict or postdict) particular changes. Lightfoot 1979 contests this view—claiming that, when the transparency of grammatical derivations has decreased up to a certain point, the occurrence of a therapeutic change, but not its precise manner of implementation, can be predicted. I think that Lass (33, fn. 17; 41) is right to reject Lightfoot's claim: the latter's 'predictions' turn out, by his own admission (345), to be nothing but post-hoc explanations, with emphasis on therapy, of why a change has occurred. Like Lass, Anttila (1972:184) argues for the twofold unpredictability of linguistic change (cf. §1.3 below).

When considering both synchronic and diachronic linguistics, L makes the following remark (125): 'If we define explanation in terms of the D-N paradigm, it is doubtful if any well-formed explanation for an interesting linguistic phenomenon has been adduced in the entire history of the subject.' This statement is perfectly correct; but L fails to point out an interesting contrast between diachronic and synchronic linguistics, so far as the general opinion about the availability of D-N explanations is concerned. Whatever one may think of the future development of diachronic

linguistics, everybody agrees that no serious D-N explanations of linguistic change have yet been offered. By contrast, the majority of linguists take it for granted that (non-variationist) synchronic-grammatical descriptions meet the criteria of D-N explanations. This attitude results from mistaking the all-or-none character of linguistic concepts for that of (synchronically valid) deterministic regularities of linguistic events.

L stipulates (21; 48; 88, fn. 22; 166) that sciences not making use of D-N explanations do not qualify as empirical, which he defines as being able to predict, and to be falsified by, single (spatio-temporal) events. This view has distinctly odd consequences: e.g. that microphysics, biology, and historiography (a term which L, for clarity, prefers to 'history') are not empirical sciences. L refers (p. x) to Ringen 1975 and to Itkonen 1974, 1976 to support his view—first presented in Lass 1976, 'Epilogue'—that linguistics is not an empirical science (cf. also Hammarström 1978); but note that L's concept of empiricality is much narrower than Ringen's or mine. The empiricality of a description equals, for me, its being testable on the basis of (sets of) spatio-temporal events; but there is no reason to view testing as being based on prediction, whether of single events or of relative frequencies in sets of (single) events.[1] Consequently, not only microphysics and biology, but also historiography—including diachronic linguistics—qualify as empirical. It still remains true that synchronic-grammatical analysis is, and must be, an instance of conceptual analysis, and hence non-empirical in character (for details, cf. Itkonen 1978a:155–68). Moreover, a non-empirical type of historiography is also possible—as exemplified by the Lakatos-type philosophy of science, which investigates what scientists should have done according to the contemporary norms of rationality, instead of what they actually did (cf. Lakatos 1971:106–8).

1.2. Statistical explanation. L rejects the use of ST explanations in diachronic linguistics because he thinks they are non-causal and non-explanatory. This view is far from uncontroversial. Durkheim ([1895] 1938:124), while insisting on the inevitable statisticalness of sociological 'laws', had already claimed that the sole purpose of sociology is the discovery of social causation.

Let us consider an artificially simple case, where instances of input A, acting upon some system, produce instances of output B, and where a sample of 100 days is (counterfactually) assumed to be representative of the entire population. This example is adequate to our purposes insofar as the typical linguistic variables are qualitative in nature: it is in general not the case that a gradual increase (or decrease) in A co-varies with a similar change in B; and the dichotomous variable with the two values 'presence' vs. 'absence' is the simplest example of a qualitative variable.[2] Now (apart from the cases of perfect correlation and of non-correlation between A and B) the three basic possibilities are represented by Contingency Tables 1–3. The following alternatives are

	A	Ā	
B	40	0	40
B̄	10	50	60
	50	50	100

Table 1.

	A	Ā	
B	50	10	60
B̄	0	40	40
	50	50	100

Table 2.

	A	Ā	
B	40	10	50
B̄	10	40	50
	50	50	100

Table 3.

then open to us:

(i) We may accept the statistical regularities (or 'laws') exhibited by the contingency tables to be irreducible.

(ii) We may regard them as resulting from our ignorance, i.e. as reducible to unknown deterministic regularities.

[1] It is of crucial importance to realize that, in accordance with the mutual irreducibility of 'is' and 'ought', norms (or rules) of language cannot be (exhaustively) reduced to (sets of) spatio-temporal speech events.

[2] 'Increase in A' does not mean increase in the number of units exemplifying any of the values of A. Thus we may speak of increase in a quantitative variable like a temperature, but not in a qualitative variable like a phoneme; cf. Itkonen 1980.

(iii) We may drop the issue of deterministic vs. statistical regularities, and undertake a particularistic, NON-NOMIC explanation of why an instance of B occurred or failed to occur when an instance of A was or was not present. It still remains true that connections between great numbers of instances of A and B must, as a matter of practical necessity, be handled statistically.

Regarding (i): If statisticalness is taken as a matter of ontology, then A in Table 1 is the necessary, but not sufficient, cause of B: i.e., B never occurs unless caused by A, but in 20% of the cases A fails to cause B. In Table 2, in turn, A is the sufficient, but not necessary, cause of B: i.e., A always causes B, but in 17% of the cases B occurs spontaneously, i.e. without being caused by anything else. (To the extent that it is possible for B to occur spontaneously in the presence of A, then A is the sufficient condition, but not cause, of B.) In Table 3, finally, A, though still the only cause of B, is neither necessary nor sufficient for B to occur. —To deny the existence of statistical causation just because it seems 'mysterious' would amount to imposing the categories of our everyday thinking, a-priori, on (physical or human) reality.

It is generally (though not universally) assumed today that the laws of microphysics are irreducibly statistical. It is more problematic whether a similar assumption can be made concerning the 'laws' of human sciences. Not surprisingly, L's position on this issue is somewhat ambivalent. He first (26–7) denies, on general grounds, that there can be any statistical laws of language; but then he admits (28) that at least some such laws exist, namely Labov-type variable rules, and he insists that these are 'exceedingly mysterious ... distinctly odd'. (Of course, such 'laws', because of their restricted validity, are not comparable to the laws of microphysics.)

Regarding (ii): If statisticalness is taken as a matter of methodology, or of ignorance, then one must attempt a more diligent analysis of the data, to transform the previously given contingency tables into, e.g., Tables 1′–3′.

	C		$\bar{C}$	
	A	$\bar{A}$	A	$\bar{A}$
B	40	0	0	0
$\bar{B}$	0	40	10	10

TABLE 1′.

	D		$\bar{D}$	
	A	$\bar{A}$	A	$\bar{A}$
B	10	10	40	0
$\bar{B}$	0	0	0	40

TABLE 2′.

		A		$\bar{A}$	
		C	$\bar{C}$	C	$\bar{C}$
D	B	0	0	0	10
	$\bar{B}$	0	0	0	0
$\bar{D}$	B	40	0	0	0
	$\bar{B}$	0	10	10	30

TABLE 3′.

In Table 1′, we uncover a new necessary cause C—which, together with A, constitutes the sufficient (and necessary) cause of B. In Table 2′, we uncover a new sufficient (but not necessary) cause D, which may operate simultaneously with A, thus occasionally giving rise to a causal over-determination of B. In Table 3′, we uncover, on the one hand, a new cause C—constituting, together with A, a sufficient cause of B—and, on the other, a new sufficient cause D, the two sufficient causes being such as never to operate simultaneously. A and C are not necessary tout court; i.e., they are necessary components of a sufficient, but not necessary cause.

In the human sciences, one never actually succeeds in establishing any of the latter three contingency tables or their equivalents. The normal situation is, rather, the 'weak reciprocal implication' of Table 3, to use the term of Boudon (1974:28–9). For a practicing sociologist who investigates social causation, the choice between alternatives (i) and (ii) is therefore purely 'metaphysical' (cf. Blalock 1964:15–18). However, choosing (ii) may be a useful methodological principle, because it enables one to acquire a comprehensive view of the data and to come up with maximally precise statistical laws. In fact, even those who are convinced that alternative (ii) is false may gain by acting as if it were true (cf. Runciman 1969:11). L's position on this issue is, again, far from clear. He briefly accepts alternative (ii) as true (29–30), then considers it as a mere methodological principle (31–2), and later seems to give it up altogether (101–2).

Regarding (iii): Several current trends in social theory maintain the impossibility of D-N explanations, and the irrelevance of ST explanations (cf. Dray [1963] 1974:69–70, Diesing 1972:163–4, Harré & Second 1972:131). This is the alternative which L rather uncertainly chooses, arguing (109) that we should somehow develop a new, non-predictive and (in his terminology) non-empirical mode of explanation (cf. §2 below).

Social scientists never accept an observed correlation as causal unless they can postulate an intelligible, psychological mechanism—such that a (more or less complex) third term either intervenes between the (observed) terms of the correlation, or functions as their common cause. Hence the concept of (statistical) causation is inseparable from that of (psychological) MECHANISM. With this observation, we may now proceed to evaluate L's views of ST explanations as non-causal and non-explanatory.

Denying the existence of statistical causation altogether is nothing but prejudice (cf. the reference to microphysics above). But L also argues (166–7), more specifically, that Labov-type variable rules can be given no causal interpretation. This view rests on a mistakenly narrow concept of 'cause' as one which allows only 'triggering conditions' to qualify. It goes without saying, however, that 'standing conditions' like sex, social class, or grammatical context must also be accepted as potential causes (cf. Mackie 1974:34–7). It can in fact be shown that variable rules should be reinterpreted as instances of causal models like those customarily used in sociology (cf. Itkonen 1980).

If the concept of explanation is restricted to the D-N type, it is a tautology that ST explanations are not ('truly') explanatory. However, in discussing the concepts of 'ease', 'optimization', and 'naturalness/markedness', L adduces more specific arguments to support his view that the ST type explains nothing. An ST explanation of the change [nb] > [mb] which refers to the 'ease of articulation' is 'merely a semi-formalized paraphrase of an observed distribution' (21). Similarly, considering the changes CVC > CV and CVC > CVCV as instances of the 'optimization of the syllabic structure', as Schane 1972 does, rests on the fallacy of 'making the goal out of a mere distribution' (34). Chomsky & Halle's concepts (1968) of 'unmarked' and 'natural' are interchangeable with 'common' (and hence with each other); thus their theory of markedness reduces to the 'blinding tautology that nature tends towards the natural' (43).

ST explanations may certainly be used in such unhappy ways; but if L wants to say that unhappy uses are inherent in ST explanations, he is mistaken. In sociology it is current practice to postulate unobservable statistico-causal mechanisms which produce logically-independent observable effects; and there is no reason why one cannot, in principle, do so in linguistics too. L notes (25, fn. 9) that John Anderson has made him aware of this objection; but he replies that the unobservable causes are postulated, in the 'virtus dormitiva' fashion, so as to match one-to-one their observable effects. Again, this may be true of Chomsky & Halle's theory of markedness, but there is no reason why it should be true in general. (What I have just said must not be taken to imply that, in these and similar cases, ST explanations are the best that one can come up with; cf. §2 below).

Assimilation is one causal mechanism that operates statistically (or better, non-deterministically). As might be expected, L rejects explanations based on assimilation because they are non-predictive. For instance, Ohala 1974 explains the change [sl] > [ʃl] in some Norwegian dialects by dividing it into two sub-processes, first the articulatory assimilation [sl] > [sl̥] and then the acoustic assimilation [sl̥] > [sʃl] > [ʃl]. L admits (40–44) that this may have happened, but he finds the explanation unsatisfactory because it is inevitably post-hoc; i.e., it fails to show why this change had to happen, instead of the change [sl] > [sl̥] or the non-change [sl] > [sl]. It is clear that nothing within the limits of the possible could satisfy L.

It may be good to add that, as is evident from the preceding discussion, L is not opposed to the idea of investigating 'constraints on POSSIBLE changes'. What he denies is that such constraints are sufficient to explain the occurrence of any ACTUAL changes. In this he is, of course, correct. But it does not follow that actual changes cannot be explained at all.

1.3. TELEOLOGICAL EXPLANATION. A distinction is often made between TEL and functional explanations. In the former case, animal or human behavior is explained by a not-yet-existent state of affairs, or 'goal', toward which it is directed; in the latter case, an on-going process of an organism is explained

the contribution it makes to the organism's well-being or survival. L sub-
nes both cases under a single concept.

'he above-mentioned distinction must not be confused with that between teleology of purpose l of function—which, when applied to human behavior, aims at capturing the differences between els of consciousness: 'purpose' is conscious, whereas 'function' is unconscious. This dichotomy iich occurs in somewhat different terminology in Kaplan 1964:363–5), has been espoused by dersen (1973:789) and, following him, by Vincent (1978:413); these writers claim that linguistic inge exhibits teleology of function, but not of purpose. L interprets the distinction rather more ngly; he identifies 'purpose' with conscious purpose, and 'function' with biological function. takes it as self-evident (82) that linguistic change is not consciously-purposive action, and refore addresses himself to the question of whether it is amenable to TEL explanations as used)iology. (He considers this type of explanation to be acceptable, though inferior to D-N expla- ion; 88, fn. 22.) Biology is a natural science, and the natural sciences are based on laws; but re are no laws of linguistic change (cf. §§1.1, 1.2 above); hence L easily concludes that linguistic inge is not amenable to TEL explanation.

, commits a serious mistake in assuming that those linguists who employ TEL explanations, : Anttila 1972 and Campbell 1975, must share his 'biologicalist' conception of teleology. It is ; mistake which allows him to criticize existing TEL explanations of linguistic change as 'na- alistic' and 'mechanistic' (135). This criticism is all the more surprising in view of L's earlier –78) criticism of the non-predictive and non-nomic use that Anttila and Campbell, among others, ke of TEL explanations.

:onsider the well-known case of the Greek future:

PRESENT	FUTURE
trép-ō	*trép-s-ō*
mén-ō	*men-é-ō* < **men-és-ō*
lú-ō	*lū́-s-ō* (?< **lú-ō* < **lū́-ō* < *lū́-s-ō*)

mpbell offers the standard explanation, according to which the sound change which drops the ervocalic *s* was prevented from operating (or was canceled afterward) in those cases where its come would have been (or was) the formal identity of the present and the future. Hence the AL of this ANALOGICAL resistance to the sound change $s > \emptyset$ / V_V was to keep the present and future formally separate. L objects to this analysis on two grounds (68–70):

a) Campbell uses the illegitimate 'how else' strategy in arguing that, although the existing non- erministic (TEL) explanations may ultimately be reducible to D-N explanations, they MUST be d as long as the latter are not available.

b) Campbell's explanation is post-hoc.

legarding (a): Contrary to Botha (1971:125–7), to whom L refers, there is nothing wrong with 'how else' strategy: it merely amounts to the truism that a theory which might be true is better n a false theory, or no theory at all. In fact, it might even be said that a false theory is better n no theory at all: as Lakatos says (1970:121), '"Falsification" in the sense of naïve falsifi- ionism (corroborated counter-evidence) is not a sufficient condition for eliminating a specific ory: in spite of hundreds of known anomalies, we do not regard it as falsified (that is, eliminated) il we have a better one.' It would be irrational to follow Lass and to eliminate our only theory. iss is inconsistent enough to embrace the 'how else' strategy in a different context, namely in motto of Chap. 5: 'Well this is I think all very crude analogical [sic!] talking, but, if we can't c better, we have to talk the way we can.')

legarding (b): L's criticism of the post-hoc nature of Campbell's explanation is the same as the : he directed against Ohala (whom he mistakenly regards, 64, as not making use of the 'ease articulation', and hence as non-teleological). Whether one attaches any importance to this type criticism depends entirely on whether one happens to share L's aprioristic predilection for D- explanations. Two things must, however, be pointed out. First, to insist seriously on D-N)lanations would mean wiping out more than 2000 years of historiography, because not a single torical explanation fulfilling the criteria of D-N explanations has ever been offered (cf. Donagan 66] 1976:189). Yet, from Herodotus onward, historians have been quite happy, in default of D-

N explanations, to use their own 'how else' strategy. Second, and more important, it is impossible to understand why, in the first place, L undertakes to criticize existing TEL explanations for being post-hoc: if there are no laws of linguistic change, then truthful explanations in this field MUST be non-predictive or post-hoc, and the post-hoc character of their explanations should be to the credit of Anttila and Campbell.

L claims (78–9) that TEL explanations contain a 'logical' defect, namely the fallacy of affirming the consequent, which then takes the following form: 'If there is the goal X, behavior Y occurs; behavior Y occurs; therefore there is the goal X.' This is not, however, a TEL explanation, but a hypothetico-inductive inference from observable behavior to its unobservable goal. Once the existence of X is established (on the basis of evidence not limited to Y), X is used to explain Y. Precisely the same mode of inference occurs as the preliminary stage of any causal deterministic explanation with unobservable causes and observable effects (cf. Itkonen 1978a:12–14). It is important to realize that, in situations like this, the 'fallacy of affirming the consequent', far from being a fallacy, is the only legitimate mode of inference. (It is true, of course, that goals may be postulated in a fallacious 'virtus dormitiva' fashion; but so may unobservable causes.)

L distinguishes three basic modes of linguistic TEL explanation, viz. 'preservation of contrast' (65–70), 'minimization of allomorphy' (71–5), and 'avoidance of homonymy' (75–80); and he identifies the second with Anttila's principle (1972:98–101) of 'one-meaning/one-form'. He fails to see, however, that all three are in fact exemplifications of the last-mentioned principle. Both in preservation of contrast and in avoidance of homonymy, the 'one-meaning/one-form' situation is maintained by preventing the 'two-meanings/one-form' situation; and in minimization of allomorphy, the 'one-meaning/one-form' situation is restored by eliminating the 'one-meaning/two-forms' situation (cf. Itkonen 1978b).

L repeatedly (70, 74, 85–6) criticizes TEL explanations for their (apparent) unfalsifiability. It is true that the assumption of the generally purposive nature of social (including linguistic) behavior is not an empirical hypothesis, but rather a methodological principle or 'program for inquiry' (cf. Kaplan 1964:365–6). Yet non-nomic TEL explanations of a single subject can usually be ranked according to more or less intersubjective criteria, which means that the inferior ones have been (empirically) 'falsified', i.e. rejected. Therefore it is not right to say that TEL explanations are unfalsifiable—though, to be sure, their falsification is based on the lack of internal coherence and of comprehensiveness, rather than on the non-occurrence of predicted events (cf. Diesing, 230–34).

2. HOW TO CONSTRUCT A NON-NOMIC METHODOLOGY. Not until pp. 98–100 does L reveal the structure of his over-all argument. In his three first chapters, dealing with the three basic types of explanation, L plays the devil's advocate and assumes the role of a positivist, i.e. one who believes that the human sciences must simply adopt the methods of, and hence be assimilated to, the natural sciences (cf. Radnitzky 1970:72–92). In the last two chapters, he assumes his true role of an anti-positivist and insists on the qualitative difference between human and non-human nature. The difficulty is, however, that L is unable consistently to maintain this 'dialectical' mode of argumentation. First, while presumably acting as an advocate of positivism, he is already quite obviously convinced of its untenability (cf. §§1.1, 1.2 above). Second, he erroneously considers existing TEL explanations of linguistic change as exemplifications of the positivist attitude (cf. §1.3). Third, the rationale of his 'dialectical' strategy breaks down because, as we shall see, he is unable in the last two chapters to add anything constructive to what he has already achieved.

L starts (98) from Sampson's view (1976:963) that D-N explanation is the 'only method of adding to the total of human knowledge'. He shows (102) that, if this view is taken literally, it is invalid, because philosophy (including the philosophy of science, analysing the concept of D-N explanation) obviously adds to the 'total of human knowledge', yet does not use D-N explanations.[3] And if the view is taken less than literally, it is outdated, because the development of physics and (in particular) of biology has shown that the D-N paradigm is just a survival from the days of Newtonian physics.

After discussing the complexity and randomness of the subject matter of biology, L proceeds to show (114–42) that human language is even more intractable. Here I find it difficult to detect a continuous and cogent line of argument, perhaps because unsystematicity simply cannot be discussed in a very systematic way. In any case, L reaches some sweeping conclusions about the nature of language (129): it is, in Popper's simplistic terminology, 'a World 3 artefact, ... manifested by an infinitely complex and delicate World 1—World 2 interaction'; language is also like a ' "play" in the sense of Huizinga's study (1944) of what he calls the "Spielelement der Kultur"' (135); finally, language is 'emergent' (141) and even 'mysterious' (143).

Having argued that the subject matter of (diachronic) linguistics is utterly different from that of physics, L faces the uncomfortable task of showing what kind of non-predictive or non-nomic framework is adequate to it. He admits (100, 109) the urgency of this task, but disclaims any obligation to carry it out himself: 'making positive suggestions is not the critic's business' (144). Nevertheless, he ventures in his last chapter to offer some half-hearted remarks on the topic; these are clearly inconsistent with what he has said before. But even these self-contradictions are instructive, because their solution suggests itself rather easily; and this is also the solution to the important problem that L has raised. I shall concentrate on two of his self-contradictions, and will refer to them as (i) and (ii). In each case, A and B will stand for the conflicting claims, and C for the solution—which is by no means a 'synthesis', but a well-founded choice of one alternative over the other.

Case (i) has the following elements:

(A) L assumes (82) that linguistic change is not purposive; later (168), he repeats, partly in opposition to Itkonen 1978b, that 'language change is not something that people "do"'—or, we may add, refrain from doing.

(B) L assumes (31) that the non-deterministic intentions of speakers are involved in linguistic change, and he accepts (131) the metaphysical principle of free will; earlier (3) he has stated that the 'problem of intention (or crudely, "free will")' is the 'basic problem in any historical explanation involving human beings'.

(C) It is an elementary conceptual point that there can be no intentions without actions (or 'doings'), and that free will is manifested in acting (or in forbearing to act); therefore, if linguistic change involves intention and free will, it must be one or another sort of action. Moreover, intention is inseparable from, if not identical with, purpose; therefore, to the extent that linguistic change involves intention, it must be purposive. Consequently, L must reject either A or B. Because all his anti-positivist or 'humanistic' attitude is based on the acceptance of B, he must reject A.

Two additional remarks are in order. First, it is true that linguistic change (both qua innovation and qua acceptance) is not action on a par with speaking, or using an instrument. Rather, it is like modifying an instrument while (or just before) using it. Second, even though people do X for a reason (or purpose), there need be no reason why they do X rather than Y. The choice between two equally good alternatives (here it is between two innovations, between innovation and non-innovation, or between acceptance and non-acceptance) may be random. To this extent, L's reference (135–6) to 'play' seems justified.

[3] It is fair to add that Sampson 1980 no longer holds his earlier positivist views.

Case (ii) has the following elements:

(A) L requires of the new, non-nomic mode of explaining (or 'explaining') linguistic change that it at least make the phenomenon under study INTELLIGIBLE by showing it 'as part of a pattern' (162). Explanations of this type can 'be criticized in terms of their consonance with the rest of our empirical knowledge, and their internal coherence' (160).

(B) L rejects TEL explanation of linguistic change, mainly because of its 'elementary logical fallacies' (157)—although he has admitted (69, fn. 22) that it 'makes the unintelligible partly intelligible'. Anxious to make some positive suggestion, L enumerates (162–8) five non-causal types of explanation employed in physics or biology (taken from Kaplan 1965): 'taxonomies', 'mathematical generalizations', 'temporal (or interval) laws', 'statistical principles', and 'purposive laws'. Astonishingly, L either says nothing about how such explanations can be used to make linguistic change intelligible, or else even denies (as in the case of purposive laws) that they can be so used.

(C) L mentions only one type of explanation which is able, even in a 'flawed' way (160), to make linguistic change intelligible—namely TEL explanation. Moreover, the 'flaws' that he imputes to TEL explanation—viz. the fallacy of affirming the consequent, the 'how else' strategy, and unfalsifiability—are purely imaginary (cf. §1.3 above). Therefore, if L wants to keep A, he must reject B; and all his humanistic, anti-positivist attitude indicates that he wants to keep A.

Some words of clarification are again called for. The concept on non-nomic explanation for which L (157–62) is groping is PATTERN EXPLANATION, which has been independently developed in several human sciences (cf. Diesing, 157–67 and passim; also Kaplan 1964:327–36, 358–69). The most important type of pattern explanation is TEL ('functional', 'purposive') explanation, where the explanatory coherence obtains between the circumstances (including other actions performed by the agent), the ends, and the means. Behavior amenable to pattern explanation should be characterized not just as purposive, but also as RATIONAL.

This last point requires two comments. First, the rationality assumption is made for reasons of methodological simplicity (supposing that the question as to ontological accuracy must be left open). Rational actions, unlike non-rational ones, require no WHY-explanation (although a HOW-explanation may still be needed). In this sense, the status of rationality in the human sciences is comparable to that of inertia in Newtonian physics (cf. Laudan 1977:184–9). What is true of rational actions is, mutatis mutandis, also true of rule-governed or correct (as opposed to incorrect) actions; but there are no RULES of linguistic change—a point which L (31–2) may not have grasped. Second, it is the great merit of L's book that it forces us to make a choice: either to accept the existing dualism between nomic theory and non-nomic practice, or to eliminate it by constructing a non-nomic theory. To my knowledge, the concept of rationality offers the only systematic basis for constructing a concept of non-nomic explanation. A similar conception of linguistic change as rational behavior has been defended in detail by Coseriu 1958, and long before him by Whitney ([1875] 1979, esp. pp. 143–52). Note that rationality must be understood here as UNCONSCIOUS. The concept of unconscious rationality has also proved necessary in cognitive psychology (cf. Neisser 1967:292–303) and even in psychiatry (cf. Mischel 1974). A single linguistic change is a collective action, where acceptance by the community functions as a 'rationality filter' on innovations; and a long-term linguistic change or DRIFT is a series of such actions, each of which is a reaction to the situation created by the preceding actions.[4]

In sum, I do not think we have to accept the pessimistic conclusions of L's

[4] I have not found it necessary here to distinguish between the two main types of linguistic change, viz. change by adults and change by children. An adult changes (part of) a social language by literally changing his own language, either by making innovations or by accepting innovations made by others. A child changes (part of) a social language by abducing for himself a mental grammar which differs from that of adults. L seems to concentrate on the former type of change. It is important to realize that language acquisition too, qua 'process of information analysis' (Cazden & Brown 1975:306), can be subsumed under the concept of unconscious rationality.

original and thought-provoking book. Lass acts like a man who, not content to claim that the emperor has no clothes, wants actually to strip him naked. My advice would be to give a garment or two to the lightly-dressed monarch.

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