Abduction or Inertia? The logic of syntactic change*

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Two assumptions often considered principles of inquiry in historical generative syntax are that linguistic change is abductive (Andersen 1973) and that syntax is inert (Longobardi 2001). In this paper it is demonstrated that these two notions, if meaningfully interpreted, are not compatible: if we wish to develop a coherent theory of language acquisition and change, we must abandon one or the other. The conclusion reached is that that neither abduction nor inertia is a necessary or useful concept in diachronic syntax. I suggest that we should abandon both, instead treating syntactic change on its own terms.

Two assumptions often considered principles of inquiry in historical generative syntax are that linguistic change is abductive (Andersen 1973) and that syntax is inert (Longobardi 2001). In this paper it is demonstrated that these two notions, if meaningfully interpreted, are not compatible: if we wish to develop a coherent theory of language acquisition and change, we must abandon one or the other, with important consequences for the way we conceptualize syntactic change.

The paper is structured in four sections. In the first I present abduction, as introduced into the linguistic literature by Andersen (1973), and outline problems with the notion. Section 2 does the same for the concept of inertia. In Section 3 I outline how, and why, the two concepts are mutually incompatible. Section 4 concludes.

1. ABDUCTION AND ITS PROBLEMS

The notion of abduction as a form of inference originated with the American semiotician and philosopher Charles Sanders Peirce. Andersen (1973) was the first linguist to incorporate it into a theory of language change. The following is his exposition (highly problematic, as I will demonstrate), which many linguists have followed (e.g. Lightfoot 1979; McMahon 1994; Roberts 2007: 124–5).

Andersen (1973: 774–776) distinguishes three modes of inference: deductive, inductive, and abductive. He proposes that they can be distinguished using Aristotelian syllogisms. Deductive inference, a familiar concept, 'applies a law to a case and predicts a result' (1973: 775), as in the syllogism in (1).

(1)

Case	Socrates is a man			P
Law	All men are mortal			$P \supset Q$
		$\approx$		
Result	Socrates is mortal			$\therefore Q$

Inductive inference, in Andersen's terms, 'proceeds from observed cases and results to establish a law' (1973: 775), as in (2).

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(2)	Case 1	Socrates is a man		P_1
	Result 1	Socrates is mortal		Q_1
	Case 2	Abraham Lincoln is a man		P_2
	Result 2	Abraham Lincoln is mortal		Q_2
	Case 3	Michael Jackson is a man		P_3
	Result 3	Michael Jackson is mortal		Q_3
			≈	
	Law	All men are mortal		$P \supset Q$

Crucially, Andersen claims (1973: 775) that 'these two modes of inference share two important characteristics: first, the conclusion contains nothing which is not given in the two premises; second – and this is a natural corollary – if the premises are true, the conclusion is certain to be true.'

Abductive inference, the mode that is key to Andersen's proposal, 'proceeds from an observed result, invokes a law, and infers that something may be the case' (1973: 775). This is illustrated in (3), which can be seen to be equivalent to the logical fallacy of affirming the consequent.

(3)	Result	Trout swim		Q
	Law	All fish swim		$P \supset Q$
			≈	
	Case	Trout are fish		$\therefore P$

Andersen claims that a) unlike induction and deduction, abduction is fallible, and b) abduction can form hypotheses and 'originate new ideas', while deduction and induction are merely processes of hypothesis *testing* (1973: 775). It is easy to demonstrate that abduction is indeed fallible. If the result 'Trout swim' is replaced by 'Otters swim' in the abductive syllogism (3) above, the conclusion becomes 'Otters are fish'. Despite the correctness of the premises, the conclusion is false, showing that the mode of reasoning involved is flawed. Andersen takes abduction to underlie language acquisition, and such flawed abductions to underlie language change.

There are serious problems with this model, however. Indeed, Deutscher (2002) demonstrates that Andersen (1973) was critically confused about abduction, and concludes (2002: 484) that 'the term "abductive innovation" is neither adequate nor necessary for a typology of linguistic innovations'. To begin with, Deutscher shows that Andersen's claim that deductive and inductive inference are both infallible is clearly false. To see that enumerative induction is also fallible, we might try replacing the word 'mortal' with 'dead' in the inductive syllogism (2) above. The conclusion then is 'All men are dead'; once again false, despite the correctness of the premises. Both enumerative induction and abduction are forms of fallible *ampliative* inference in modern philosophical terminology (Deutscher 2002: 471). This error is Andersen's, not Peirce's, since Peirce explicitly differentiated deductive inference from the fallible inductive and abductive modes (Deutscher 2002: 477). Indeed, the 'problem of induction' has been widely recognized in philosophy since Hume (1910 [1748]).

Furthermore, Peirce himself was not terminologically consistent throughout his life: Burks (1946: 301) observes that 'the variation to be found in his views on abduction is ... typical of Peirce's writings'. Specifically, in his early work, Peirce 'felt bound to express his very general notions within the straitjacket of Aristotelian logic' (Deutscher 2002: 471); cf. the syllogisms (1)–(3) above. In his later work, however, Peirce's notion of abduction was much more general: he viewed it as the process by which any creative hypothesis was formed (Deutscher 2002: 474). This later view is given in (4).

(4) Peirce's later notion of abduction: 'The surprising fact, C, is observed; But if A were true, C would be a matter of course; Hence there is reason to suspect that A is true.'

This notion is in no way coextensive with the syllogism in (3): what is 'a matter of course' has nothing to do with laws, cases or results. Andersen (1973), however, mixes up the two notions – hence his claim that induction and deduction are processes used only for testing hypotheses formed by abduction.

This terminological mess has led to mass confusion among historical linguists. Consider the following example of abductive reasoning given in Trask's *Dictionary of historical and comparative linguistics* (2000):

> 'I have heard people saying things like "books" and "trees"; therefore there must be a rule of English that nouns are pluralized by adding *-S*.'

This is not an example of abductive inference but of enumerative induction in its narrowest, Aristotelian sense, as in the schema in (2): jumping from cases and results to infer a law (Deutscher 2002: 481). Although it may be consistent with Peirce's later notion of abduction as given in (4), it is in no way consistent with the schema in (3). Another problematic definition is that given in the glossary to Roberts (2007):

> 'Change caused by the fact that learners only have access to the output of a generative grammar ... and to Universal Grammar ... with no direct access to the grammar itself. The combination of primary linguistic data ... and Universal Grammar may lead the learner to abduce a system which is distinct from that underlying the primary linguistic data by reanalysis ...' (2007: 445)

The notion of abduction used here is essentially as broad as the notion of change itself, since it is entirely possible for deductive and/or enumerative-inductive inference to lead the learner to postulate a different system from that giving rise to the primary linguistic data (PLD). The insight that children have no access to the grammar underlying the language they are trying to acquire is, of course, an important one, and crucially associated with Andersen (1973) as well as Lightfoot (1979), but this was not what Peirce (or Andersen) ever meant by 'abduction'.

Deutscher concludes that the term is misused and indeed useless in linguistics, and that the way linguists use it is out of step with its more general use in philosophy and science, where the term has largely been replaced by the more general notion of 'inference to the best explanation' (cf. e.g. Lipton 2000). He does, however, make one important qualification:

> 'The questions that Peirce raised about the nature of inference still stand at the core of any theory that attempts to explain language learning and change. How are hypotheses for new linguistic rules formed in the mind? How does a language learner decide between alternative rules that can explain the same surface form? What, from the point of view of the language learner, represents the 'best explanation' for his/her input?' (2002: 484).

The notion of abduction may not even be useful in this sense, though. Lass (1997: 335–336) expresses doubt that abduction or indeed any form of inference can be used to model language change, given that in his view language change is not carried out by an agent who can 'act' or 'abduce'. Even assuming that language change can be reduced to language acquisition (which Lass himself (1997: 337) vigorously denies), it is far from obvious that children *choose* to acquire language. There is substantial experimental evidence (e.g. Kisilevsky *et al*. 2003) that the process of language acquisition seems to begin before birth, suggesting that it makes more

sense to view it as an automatic biological-mechanical algorithm in the tradition of cognitive science rather than as the result of any conscious action on the part of the acquirer.

Although a much-mentioned buzzword, then, it is not clear that 'abduction' in fact means anything at all, or that it is of any use in the study of language change.

2. INERTIA AND ITS PROBLEMS

The origins of the notion of inertia as applied to language change lie in work by Keenan (1994, 2002, 2009), who expresses the idea as in (5).

(5) 'Things stay as they are unless acted upon by an outside force or DECAY'
(Keenan 2002: 327; emphasis his)

Longobardi (2001) goes a step further, proposing what he terms the 'Inertial Theory' of syntactic change. He claims this has empirically testable consequences, and might turn out to be 'empirically false or only partly correct' (2001: 278). The cornerstones of the theory are set out in (6)–(8).

(6) 'syntactic change should not arise, unless it can be shown to be *caused* – that is, to be a well-motivated consequence of other types of change (phonological and semantic changes, including the disappearance of whole lexical items) or, recursively, of other syntactic changes'
(7) 'linguistic change proper ... may only originate as an interface phenomenon'
(8) '*syntax*, by itself, is diachronically completely inert'
(Longobardi 2001: 277–278; emphases his)

The idea of inertia has received widespread attention in the literature on diachronic syntax; it is discussed by, among others, Lightfoot (2002: 130), Hróarsdóttir (2002, 2003), Ferraresi & Goldbach (2003), Ingham (2006: 257), Roberts (2007: 232), Jäger (2008), Waltereit & Detges (2008), Biberauer & Roberts (2009: 74), Reintges (2009), Axel & Weiß (2010), Sundquist (2010), Breitbarth *et al*. (2010) and Meisel (in press).

It seems desirable, following the view of Lightfoot (1979, 2002) that there are no principles of history and that 'there is no theory of change to be had independent of theories of grammar and acquisition' (2002: 127), to reduce the Inertial Theory to properties of the faculty of language and to acquisition. However, it turns out that such a reduction is impossible. This is so because when working on syntactic change we have to assume (9) and (10).

(9) Acquirers do not have access to the grammar of the 'target' language.
(10) Experience plays a direct role in the acquisition of syntax.

These assumptions are uncontroversial: acquirers are not telepathic, and the acquisition process makes use of evidence. As noted in Section 1, the observation in (9) was an important part of Andersen (1973). Now in order for the Inertial Theory to work, (11) would also have to hold:

(11) The acquisition of syntax is a deterministic process.

The intended meaning of (11) is that, for any temporally ordered set of sentences (PLD), any and all learners exposed to it will converge on the same grammar (a one-to-one or many-to-one mapping; cf. Lightfoot 2006: 89): there is no '"imperfect" learning or "spontaneous" innovation' (Longobardi 2001: 278). Clearly (11) is necessary for any version of the Inertial Theory, since imperfect learning and spontaneous innovation cannot be said to be caused by

other types of change or by interface phenomena: the falsity of (11) entails the falsity of (6)–(8). It must be noted, however, that many algorithms for syntactic acquisition (e.g. Gibson & Wexler 1994, Yang 2002) do not assume that the acquisition of syntax is deterministic in this way. While the unambiguous triggers model of Fodor (1998) and cue-based models such as that of Lightfoot (1999, 2006) are compatible with determinism, the Trigger Learning Algorithm of Gibson & Wexler (1994), upon which the diachronic model of Niyogi & Berwick (1995) is based, relies on the existence of local maxima to explain change: the learning algorithm contains a 'roll of the dice' which may lead learners irretrievably astray in a certain proportion of cases. Similarly, the probabilistic component of the model developed by Yang (2002) may lead to the acquirer assigning different weights to certain hypotheses than the individuals from whose competence the PLD is generated. It is also not the case that (11) is coextensive with the Inertial Theory, since (11) makes no predictions about any relation between grammars diachronically, unlike the statements in (6)–(8).

Assuming (9), (10) and (11), the following scenario illustrates how the Inertial Theory must be false: imagine a child whose parents' grammar requires V-to-C movement in *wh*-questions. Now let us suppose that the parents never needed or wanted to ask direct questions in the presence of the child (for whatever reason), and therefore that the PLD includes no relevant examples. The child therefore fails to acquire V-to-C movement in *wh*-questions in her grammar. The scenario may be unlikely to occur, but, crucially, cannot be argued to be impossible for any principled reason.

Syntactic change has clearly occurred in the above scenario. Is this change 'caused', in the terminology of (6)? The answer is unclear: if there is a cause, it is clearly whatever motivated the fluctuation in the trigger experience. But this is not necessarily a well-motivated consequence of other types of change. Here it is essentially chance that has 'caused' the change; even assuming determinism in acquisition as in (11), there is simply no guarantee that the PLD will contain relevant examples. The claim that syntactic change does not arise unless caused, as phrased in (6), then, makes predictions that are too strong. The scenario is even more of a problem for (7), since there can be no question that the change in this scenario might have originated as an 'interface phenomenon': no semantic or morphophonological change preceded it. Finally, if (6) and (7) are false of this scenario, then (8) is also false: syntax is not 'diachronically completely inert'. While the assumption in (11), that acquisition is deterministic, *may* be tenable, then, the Inertial Theory as proposed by Longobardi (2001) is not.

There may be a deeper problem even with the application of the 'pretheoretical' notion of inertia. (5), Keenan's claim that things stay as they are unless acted on by an outside force or decay, applies not only in linguistics, as he stresses; in fact, the formulation is very similar to that of Newton's (1687) first law of motion. The problem is that, if the Chomskyan view that traditional conceptions of 'language' are incoherent is accepted (as, implicitly, by Keenan 2002 and Longobardi 2001, and those in the generative community making use of the notion), there is no clear sense in which a language is diachronically a 'thing'. Under this view, I-language is the object of study, and each generation has to acquire a grammar anew, as Lightfoot (1979, 2002: 117, 2006) has consistently argued. In other words, if the I-language thesis is accepted and there is no meaningful entity above the level of the individually instantiated grammar, there is no reason we'd *expect* inertia to hold in language change.

Inertia, and the Inertial Theory of Longobardi (2001), therefore suffer from a number of crippling problems, suggesting that, like abduction, the concept may be of little use in the study of language change.

3. THE INCOMPATIBILITY OF ABDUCTION AND INERTIA

As we have seen, the notions of abduction and inertia are highly problematic in their own right. In this section I demonstrate that the two notions are mutually incompatible: no theory

of abduction with empirical content is compatible with any theory of inertia with empirical content, as the assumptions that underlie them are conflicting.

In particular, the assumption of determinism in (11), and therefore also the Inertial Theory, cannot easily be reconciled with abduction or inference to the best explanation. Broadly speaking, abduction, and inference to the best explanation, emphasize the creativity of acquirers, while determinism denies the possibility of such creativity. Recall that (11) is intended to mean that, for any temporally ordered set of sentences (PLD), any and all learners exposed to it will converge on the same grammar. But if learning takes place by abduction or inference to the best explanation, how can this be guaranteed? As emphasized by Deutscher (2002), Peirce's late notion of abduction as in (4) and inference to the best explanation are forms of ampliative inference, and as such are, *definitionally*, not deductively valid; see, for example, the definition in the *Routledge Encyclopedia of Philosophy* (Craig 1998). So if learning takes place by abduction or inference to the best explanation, unless all people can be guaranteed to abduce in precisely the same way (cf. Lass 1997: 335) it cannot be deterministic, and syntax therefore cannot be inert.

One might try to save the compatibility of abduction and inertia in the following way: one could argue that abduction/inference to the best explanation applied regularly in language acquisition, in that, given a set of conditions *R*, the criteria for the best explanation would always apply in the same way and the same explanation would be adduced by each acquirer. This would equate to adopting the idea, mentioned with some scepticism by Lass (1997: 335–6), that all people could be guaranteed to abduce in a uniform manner. Notationally this would look something like the following:

(12) $P \supset Q$
$R \supset (Q \supset P)$
Q
R
_____
$\therefore P$

In other words, there exists a law $P \supset Q$ (as in the syllogistic schematization of (3) in Section 1), say 'If x is a man, then x is mortal' or 'If x is the head of a head-final phrase, then the phrase that is x's complement is head-final' (cf. Holmberg 2000, Biberauer, Holmberg & Roberts 2007, 2008 for discussion of this putative linguistic universal, which is in fact certainly more complex than this if it holds at all). But in a certain set of circumstances, represented by the boundary conditions *R*, it is legitimate to flip the rule around and to infer that 'If x is mortal, then x is a man' or 'If the phrase that is x's complement is head-final, then x is the head of a head-final phrase'. (Biberauer, Holmberg and Roberts (2007, 2008) demonstrate that the latter is *not* a universal.) Then if *R* obtains, and if *Q* obtains (i.e. if x is mortal or if the phrase that is x's complement is head-final), then we infer *P*, that x is a man or that x is the head of a head-final phrase. While of course this system isn't guaranteed to produce 'truth', it is fully compatible with the assumption of determinism in (11). Assuming that $R \supset (Q \supset P)$ is shared by all acquirers, whenever *R* and *Q* are both the case, *P* will also be.

What's the problem with this solution? Well, in one important sense there is no problem at all: it's a deductively valid inference. Crucially, however, if we apply this logic then we have reduced this instance of 'inference to the best explanation' to deductive inference. But since inference to the best explanation is definitionally *ampliative*, what we've argued is actually that if language acquisition works in this way it is *not* a case of ampliative inference, inference to the best explanation or abduction, but rather a deductive process. It then becomes redundant and meaningless to refer to the acquisition process as 'abductive'. By

formulating abductive inference in such a way as to be compatible with the determinism of (11), we have in fact done away with the notion of abduction entirely.

Alternatively, we could abandon the notion of determinism in (11), and with it any notion of 'inertia', whether that of Keenan (1994, 2002, 2009) as in (5) or that of Longobardi. (2001) as in (6)–(8). But (11) is an ontological claim, and if we deny it, we are making the opposite claim: that there are random 'rolls of the dice' involved in language acquisition; cf. Bresnan & Deo's (2001) 'Fallacy of Reified Ignorance' and the discussion in Hale (2007). This amounts to abandoning any hope of coming up with a fully causal explanation for acquisition phenomena, and to abandoning Popper's Principle of Causality: 'the simple rule that we are not to abandon the search for universal laws and for a coherent theoretical system, nor ever give up our attempts to explain causally any kind of event we can describe' (Popper 1968: 67; cf. the discussion in Lass 1980: 101–103). This does not necessarily mean that insight into the patterns behind the data is impossible in linguistics, as Lass (1980) argues; it could conceivably be the case that this deductive-nomological mode of explanation is inappropriate to language acquisition. However, even if (11) turns out to be empirically untenable (and I know of no evidence falsifying it for syntax), there is still a case to be made that something like it should be adopted on methodological grounds, as is done in e.g. microeconomic theory, where the many contingent factors affecting individuals' decisions are abstracted away from and uniformity is assumed. Such a methodological decision is implicitly taken in general linguistic theory, where it is normally assumed that whatever constitutes the human capacity for language is biologically invariant across the species, despite the fact that other organs (e.g. the eye) are known to vary between individuals in subtle ways. In other words, perhaps it's methodologically more responsible to argue, as Einstein (1971 [1926]) put it in a letter to Max Born regarding quantum mechanics, that '*He* does not throw dice'.

4. Conclusion

In Section 1 I argued, following Deutscher (2002), that the notion of 'abduction' in linguistics is confused, misused, and out of step with philosophy and other sciences. It may well be the case that abduction (or inference to the best explanation) is not relevant to language acquisition at all (Lass 1997). In Section 2 I argued that the notion of 'inertia', insofar as it has content as a hypothesis, e.g. the Inertial Theory of Longobardi 2001, cannot hold if we take even a broadly Chomskyan world-view, since it presupposes a relation between grammars diachronically that cannot be guaranteed to obtain. Even a weaker claim, the assumption of determinism in acquisition (11), is far from uncontroversial (cf. also Walkden 2010). In Section 3 I argued that no version of inertia with empirical content is compatible with any version of abduction (or inference to the best explanation) with empirical content, since the assumption that language acquisition proceeds deterministically excludes the possibility that the type of inference involved is ampliative.

My conclusions for syntactic change are: The notions and buzzwords surrounding 'abduction' and 'inertia' are unhelpful, and it is a logical necessity to abandon at least one of them. In my view, we should stop talking about abduction, we should stop talking about inertia, and we should start taking seriously the task of coming up with syntactic learning algorithms that are compatible with what we know about language acquisition and language change. Borrowing terms and metaphors from philosophy and other sciences may be useful on occasion, but in the case of 'abduction' and 'inertia' it has led only to confusion and time-wasting. Sometimes it's better to talk about language on its own terms.

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