An interview on linguistic variation with...

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Giuseppe Longobardi is *Anniversary* Professor of Linguistics in the Department of Language and Linguistic Science of the University of York. He has worked in theoretical and comparative syntax, with a special interest in the study of the structural correspondence of meaning. The structure of nominal expressions and the syntax of negation and negative quantifiers are two of the most prominent topics in his research. He has created a format for theories of grammatical diversity (Parametric Minimalism), and is currently developing an original comparative method for linguistic reconstruction based on syntactic parameters (Parametric Comparison Method).

From your perspective, what are the relevant levels of abstractness to approach the Faculty of Language? The standard ones (namely "language," "dialect," and "idiolect")? Others?

Idiolect is probably the crucial one and the best identifiable, sometimes called I-language since Chomsky (1986). Larger notions, from dialect to language, but even to language family, to language stock, are distributed on a continuum, which is created by linguists as a practical device on the basis of various degrees of the same concept, that of *historically significant similarity* (non-accidental and non-universal language resemblance, formally grounded, as e.g. in Crisma and Longobardi 2009, Ringe and Eska 2013, in a recursive definition of relatedness based on I-language and primary data generated by it). I believe that this same latter concept grounds all those that go beyond idiolect, e.g. both the idea of two or more people speaking 'the same language' and of 'languages from the same family'. As I said, the difference is just a matter of degree. An interesting challenge for linguistics is precisely quantifying such degrees, but there seem anyway not to be sharp non-arbitrary boundaries.

In these terms, it is perhaps not even necessary for linguistics to resort to previous more complex idealizations, such as 'the ideal speaker/hearer in a homogeneous linguistic community' (Chomsky 1965). I-language alone, the single idiolect, in principle without appeal to the existence of other speakers, could suffice to ground pure 'synchronic' linguistic theory, even when the latter is understood as the answer to the 'logical problem of language acquisition': for, in pure theory and somewhat absurdly, one could imagine a situation of a single learner exposed to (i.e. a single language acquisition device activated by)

machine-produced language stimuli. Such a language acquisition device would be the object of linguistic theory.

Historical linguistics is a more general notion, and would require two (but probably only two) founding concepts: I-language, again, and, indeed, historically significant similarity between at least two I-languages. Notice that, in this sense, somehow paradoxically, general (often called 'synchronic') linguistic theory is but a particular case of historical linguistics, rather than the opposite, namely the subcase in which in principle a linguistic competence is studied in relation to a primary corpus, not necessarily compared to 'other minds'.

What are the main advantages / reasons to study linguistic variation?

First of all, very basically, if language is a mirror of mind (Chomsky 1975), then language variation is a mirror of human history. In addition, culturally transmitted variation is very peculiar (even though not unique) of the genus *Homo* and, as such, it gives us further insight in the structure of the human mind/brain and its plasticity. Syntactic variation, then, is very saliently structured and discrete, so as to formally provide an ideal domain to try to apply certain hypothetical-deductive models, popular and successful in the physical sciences, to an innovative, though now conceivable, 'Galilean' approach to cultural history.

Why should syntactic history be so promising for this enterprise? I think we should examine some of the defining features of the success of the natural sciences.

One thing is that the natural science revolution in the 17th and 18th centuries strongly assumed, and then proved, that nature is regular, universal, simple. In other words, natural laws were shown to exist, to hold anytime and anywhere (e.g. the mechanics of the sublunar world was unified with that of the celestial one), and also to be ultimately remarkably general, with elaborate deductive inference from simple axioms (cf. Nobel laureate J.B. Perrin's 1912 famous aphorism: '...la tâche de la science est de remplacer du visible compliqué par de l'invisible simple').

Now, language has been precisely argued to be regular (already since at least the Neogrammarian views on sound change and early structuralist phonology) and universal (since Jakobson's and Greenberg's proposals); now formal syntax has been argued to be even 'simple' in roughly the above sense (e.g. in Chomsky 1981, ch. 1, and within the so-called minimalist framework, Chomsky 1995, Boeckx and Piattelli Palmarini 2005).

So, if formal synchronic linguistics and cognitive science can be pursued along the model of the natural sciences, now we may want to show that history, specifically linguistic and cognitive history, can be so pursued a well.

How do you conceive the relation / tension between linguistic variation and linguistic uniformity throughout the years?

In purely scientific terms, I have never seen much of a conflict there, beyond the long noticed tension between the two main goals of linguists stated in Chomsky (1964), descriptive adequacy and explanatory adequacy: wherever that tension is not resolved, that is simply a sign that insufficient progress has been made by linguists in that specific area, mostly an empirical rather than conceptual problem. In the actual practice, it is instead the case that language scholars often pursue and

privilege one or the other accordingly to their own tastes or scientific 'biases' (which may turn out to be very positive heuristically, by the way, no less than so-called metaphysical research programs (Popper 1959)). In my personal view, it is very important that formal grammatical theories, while still struggling to identify 'the right level of abstraction' for language representation, are more and more concerned with variation. Levinson and Evans (2010) have recently noted that many terms which one could say describe a sound approach to language variation begin in d-: so D-linguistics would be oriented toward 'data', 'diachrony', 'diversity', perhaps even 'Darwinism', standing here for evolutionary models. I regard focus on these aspects as very important, indeed necessary for linguistic theories, but it seems to me that one of the crucial d-words is missing from the list, and that is 'deduction'. All aspects of variation should be considered also and fundamentally in terms of deeply deductive models, precisely in order to test the response of the study of man to the approaches which produced the big revolution in the *philosophia naturalis* in the XVII century.

In your opinion, what are the contributions of dialectology (both traditional and present-day studies) to the study of language?

As I hinted before, there is probably no obvious qualitative difference between the concept of 'different languages' and 'different dialects'. If by different dialects we mean comparison of very close and similar I-languages, then its usefulness is that found in any comparative enterprise: it is always safer to start comparing similar entities, because it is easier to single out the minimal differences, combining which it is then possible, perhaps, to understand superficially greater contrasts.

What are the relevant sources to obtain evidence to study language and its variation (speakers' own competence, corpora, experiments, non-linguistic disciplines, etc.)? Is any of them potentially more relevant than the others?

I remember Massimo Piattelli Palmarini giving a series of highly instructive lectures, years ago, about the methodology of science (and what is not always understood about it in linguistics and the human sciences). One of the main points was that in the advanced sciences it is taken for granted that any source of evidence should in principle be accepted if enlightening for the understanding of a certain problem, without delimiting the evidence *a priori*. In this sense, I think that linguists should take data from every suggestive domain, manipulating and ranking them, of course, with a grain of wisdom: e.g. it would probably be less reasonable to take the evidence of an isolated fact from one potential source (say, diachronic corpus evidence or some individual processing experiment, or a single construction in an otherwise poorly studied language) and combine it with very intelligibly structured data from another rich type of source (e.g. a complex analysis based on detailed speakers' judgments from several well analyzed languages) and assign them the same probative value.

Your recent research has developed a new comparative method that adopts ideas from Genetics, Mathematics, and Computer Science (as well as theoretical formal linguistics). Can you briefly explain how using those non-linguistic disciplines can contribute to our understanding of language

variation? What do you think is the position of standard theoretical approaches to language in such a broader research scenario?

Traditional approaches to the study of language have been, with few exceptions which do not change the general picture, non-quantitative: such approaches have been very successful, allowing for discoveries of quite important generalizations from Grimm's Law to parametric differences, to cite two. But the more we try to describe new aspects of language the more we are likely to bump into so complex arrays of facts that being able to treat them mathematically and computationally is necessary. Of course, I don't believe at all that quantitative methods are all we can use for studying language nor that a quantitative method of any sort can provide us with insights about language if we do not have a qualitatively sophisticated theory thereof. With these *caveats* clear in mind, we can however proceed to explore if certain aspects of language, like many of the natural world, can become an object of inquiry approachable with both tools which characterized the scientific revolution of the XVII century: abstraction from everyday opinion, idealization, deduction, on one side (the qualitative 'revolution'), and precise measurability of complex entities (so important e.g. in Lavoisier's establishment of modern chemistry (Koyré 1961)), on the other. Our recent comparative method (Longobardi & Guardiano 2009) tries to test these tools on, and to contribute to, historical investigation of languages and the populations speaking them. Genetics is an important model for this enterprise, because over the past 50 years it has undergone three developments which all concern us here: 1) it has constantly increased the accuracy and abstractness (in the sense of remoteness from everyday observation) of its entities, from genetic markers to DNA sequences to whole genomes; 2) has used this ever growing *theoretical* knowledge as a tool for *historical* knowledge, with the development of population genetics and molecular anthropology; 3) has endowed itself with elaborate statistical and computational tools to represent the history and structure of biological populations.

In all these senses, we have a lot to learn from genetics and the associated mathematics, well beyond the already exciting possible comparison of our empirical results in historical linguistics with theirs.

Syntax, in linguistics, is the domain which has perhaps undergone some of the most apparent theoretical change and progress. In fact, in syntax, although great advance in the diachronic application of formal linguistic theory has been made from Lightfoot's (1979) seminal work on, few really historical discoveries can be pointed out, in the sense of actual growth of our factual knowledge of the past. Perhaps it is not accidental that the salient attempt of this kind I have in mind, the syntactic dating of *Beowulf* developing out of Pintzuk and Kroch's (1989) approach, is precisely the product of the most impressive revolution in the use of quantitative and computational methods in historical linguistics we have recently witnessed, namely *corpora* syntax.

In the enterprise you are asking me about, I would like to explore and stress the benefit that both qualitatively sophisticated theory and quantitative applications of it may reserve for linguistic history and, perhaps, a science of human history, more generally. A science of human history is in its infancy, but some reflections on the features it should have are not impossible (see e.g. the last chapter of Diamond 1998). I think this would be very important: paraphrasing but

reversing a famous thinker (Marx 1888), I would like to say that so far philosophers and human scientists have hoped or tried to change human history (often meeting with disastrous outcomes, from Plato's misadventures in Syracuse on); now for the first time we should seriously begin to understand it. The modern cognitive sciences can be a good vantage point for a breakthrough if theoretical and historical concerns are conjugated, and qualitative and quantitative sophistication is pursued. More than higher renowned thinkers, a pre-scientific precursor of this research program, in my personal view, is the pre-Enlightenment Neapolitan philosopher Giambattista Vico (1746), who envisaged an imaginative though then unthinkable 'New Science' of human intellect and history at the same time.

Your LanGeLin project addresses Charles Darwin's question "whether the cultural transmission and differentiation of languages over the period of human history matches the biological transmission and differentiation of the genetic characters which define the populations of the world". This goal touches on three prima facie independent domains: culture, biology, and language. What is the precise position of language in such a 'triangle'? Is it equally determined by culture as it is by biology? Is the relationship asymmetric?

The LanGeLin project plans to use syntax, rather than vocabulary and lexical meanings, as a tool to reconstruct linguistic history. It is a plausible guess that as syntax should be free from the arbitrariness of lexical meanings, it should also be less directly influenced by the variability of culture. Of course, if we find that genetic diversity and syntactic diversity are measurably correlated with each other in a significant way (Longobardi et al. 2015), without there being direct causation between the two variables, then it is possible that differences in other cultural features (say, laws, culinary traditions etc.) may also interestingly correlate with language diversity. I think this is likely to be the case, but to demonstrate it, it will be necessary to reduce some of these domains to measurable entities, exactly as we are trying to do with syntax.

As for the relationship of language with culture and biology, it goes without saying that we expect it to be somewhat asymmetric: language variability is environmental, the biology of languages appears heavily invariant across the species.

Some recent studies argue that it is diversity what truly characterizes human language, often implying that the universal nature of language is wrong (or that some allegedly specific trait, such as recursion, is not present in all languages). Is this scenario a residue of the fact that the I-language / E-language distinction has not been understood? Is it something else?

I completely agree that diversity is very characteristic of human language, and that is my main motivation for studying it. But I think that a good deal of the debate about universals, if it is really a debate, is little more than some folklore offered by us linguists to non-linguistic audiences. The question of what is universal is a matter of detailed empirical argument and goes much beyond recursion/non-recursion, not to speak of oversimplifications like universals vs. no universals. For example, I don't think one can properly claim that there are natural

languages which do not have recursion at the abstract level of the general rule Merge (a schema which combines two syntactic objects into a third one, which can in turn be combined with another one, and so on): more appropriately, the question could be if it is universal to have recursion of sentential constituents, basically to have subordinate clauses. Other issues, even more interesting, perhaps, and sensible, are tied to more fine-grained, though no less general, concepts of syntactic theory: e.g. are there languages allowing super-raising, or assigning higher scope to wh-phrases already fronted to a peripheral sentence position, or allowing reflexives to have ambiguous antecedency outside their minimal clause, just to cite some. However, these potential candidates for universality are not so popular as targets in the literature trying to state the no-universals thesis. Is that the consequence of their being really unfalsified as potential universals, or just to the fact that the so-called debate lacks in analytical depth? Perhaps, it will turn out that they are not universal conditions on language, but I still have to see detailed arguments, which would be very interesting for the theory of language diversity, against their apparent invariance. This I would be ready to call a real scientific debate about universals.

But there is another point to be stressed, one which is often ignored, in a very misleading way for non-linguistic audiences, namely that there is much more agreement on the existence of many implicational universals, even among linguists who have argued against various absolute universals: therefore, implicitly or under different names, a rich form of universal grammar is *de facto* more widely acknowledged.

Within the Generative Enterprise, the research stemming from the Principles and Parameters framework has proven very fruitful to study both variation and uniformity. However, this trend has been subject to much criticism, on both theoretical and empirical grounds. In your opinion, what is the status of "Parameter Theory" nowadays?
The main question here is the setting of language diversity and parameter theory (I do not see a more realistic attempt at capturing diversity, with all its open issues) within the minimalist program in generative syntax. Apparently, some linguists understood the minimalist program mainly as a strive for simplicity and axiomatization of the theory of grammar, which it certainly is, as one of its ultimate consequences. In this sense, any addition to, and complication of, universal grammar (and parameters are one) may appear scarcely minimalist. Of course, one should also wonder if non-parametric variation (say, of a more arbitrary type) would be more appealing. The real problem here is the very fact of grammatical diversity, not any theory of it. But, independently of this point, I regard as the central issue of minimalism the question of finding explanations for the current state of the language faculty, basically for why it developed the way it did (evolutionary adequacy, Longobardi 2003). As such, we are still desperately far away from any convincing theory, but one way of proceeding I have started to suggest in Longobardi (2005) is the following: let us try to identify similarity of format for parameters from various domains and let us classify parameters into few recurrent formal schemata (e.g. does feature F overtly or covertly attract category X?). This would decompose hundreds of parameters into few schemata and a number of primitive features, to which the schemata may apply. One

consequence is that the number of entities to be attributed to the initial state of mind will decrease immediately and substantially, and then that it will be necessary or desirable to find evolutionary explanations for a limited number of entities (schemata). Once one instance of a schema was introduced into language, it could have been replicated for other instances of F and X. This is a very unspecific research program, but a promising one for the future. This way parameter theory would become perfectly compatible with minimalism and would be a central component of language as, indeed, both a mirror of mind and a mirror of history.

What are the challenges that we will have to address in the following decades when it comes to study language and its variation?
The central challenge concerns the right level of analysis between the two objectives of achieving wide typological scope (a wide amount of diverse languages, such as the hundreds used e.g. in WALS) and satisfactory depth of study for each language (ideally, Chomsky's 1964 classical descriptive adequacy). Ultimately, we would wish to achieve both to the highest possible degree: I think that this latter point should be uncontroversial, unless one wants, indeed, to arbitrarily predefine the empirical domain or the depth of axiomatization that linguistic theory should attain. Reality is still very far from this golden standard, though.

So, some intermediate strategy must be envisaged. Not many decades ago, one phrase, 'the third way' (between Western systems and socialist economies) became very popular with some political movements in Southern Europe. The attempt to find a third way in that sense ended up as a failure, probably for extrinsic historical reasons which went well beyond the strategy and decision capacity of those politicians. But what I suspect is that third ways are intrinsically hard to achieve in any complex system, because every system including scientific research programs are internally interconnected and difficult to mix with other systems and strategies. In addition, in our case we have the problem that under a realistic philosophical approach to language structures we do not wish to give up any serious bit of descriptive adequacy, whenever it looks attainable: i.e. in principle we do not want to lose generalizations which we regard as plausibly true about the mind of speakers for the sake of adding more unanalyzed evidence from other languages.

Therefore, I am more confident in the research strategy of having parametric syntax evolve slowly and progressively toward applicability to a larger number of different languages.

Of course, the enterprise risks to be titanic, something, in theory, like writing generative grammars for all attested languages. For this reason, I have been proposing and following, for the past 10 years, what I call Modularized Global Parametrization (Longobardi 2003), a strategy which consists of pursuing the maximum of practically attainable typological exhaustiveness for detailed and sometimes deductively intricate parametric hypotheses, at the cost of focusing on small compact modules of grammar. This latter cost is what tries to control for the enormous work that combining scope and depth would require. I really think that this is the only way to go for a theory of linguistic diversity, even for the sake of

building learnability models of some plausibility, i.e. for addressing classical explanatory adequacy in a realistic fashion.

Another problem there is the 'supposed objectivity' of descriptive structures. I had a quick but, as on other occasions, stimulating conversation on this with Martin Haspelmath, recently. It may appear that a surface level of analysis (say, in terms of Dixon's 1997 concept of Basic Linguistic Theory) is more uncontroversial than complex hypotheses on mental structures generating surface strings: but my position is that this is a dubious conclusion, because in a sense we must suppose that there is one theory which is the correct one for a language or a mind, and the best approximation to it should be the simplest system of axioms accounting for the largest amount of data; of course, especially due to the conflicting requirements (simplicity and predictive scope), there may be, and normally there is, a lot of uncertainty as to the 'best' theory. But, is the notion surface description more objective? Perhaps quite the opposite, since there is no definite criterion to decide what is an observational fact, at what shallow level of analysis we should stop looking for further generalizations in order not to risk going beyond Basic Linguistic Theory. I am not saying that these problems cannot be addressed, but they seem less simple than just pursuing the usual search for the best generalization in science, with all its inevitable issues, consequences, but also rewards (in terms of subtle predictions and explanations).

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