Phonology between theory and data

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1. Introduction

One interesting aspect of the field of phonology at the beginning of the 21st century, is that it is becoming more and more interdisciplinary. A rich tradition of approximately 100 years of theoretical research into synchronic sound systems in natural language is combined with experimental methods, corpus research, insights from historical study and several other disciplines. Our most important task is to integrate all the new types of data that are nowadays being unearthed into the carefully designed theories of our predecessors. We should be sufficiently conservative – our theories have proven themselves right when checked against a large number of data – and at the same time we should not be too reactionary – more data are available to us now than ever before, and it is unlikely that all of our theories will stand their test.

There is also a canonical core of insights, ideas and concepts that belong to the theoretical baggage of any serious modern student of phonology: it seems incontestable that everybody needs to know about phonemic analysis and minimal pairs, everybody is aware of the fact that consonants and vowels are grouped into higher ordered, (sub)syllabic constituents, but that there is also evidence that vowels and consonants themselves are composed of smaller units.

Like any healthy discipline, phonology has grown through a constant interaction between data and theory. Phonological evidence played an important role in the turn from structuralism to generativism and, more recently, in the debates between rule-based and usage-based approaches to grammar. Also fields like quantitative sociolinguistics and experimental psycholinguistics seem to build more on phonological insights – albeit maybe sometimes slightly outdated ones from the point of view of the phonologists – than on the results of other fields of grammar, such as syntax. Conversely, the theory is becoming more and more informed by the data unearthed in sociolinguistic and psycholinguistic analysis.

Phonology has also been at the forefront of various kinds of grammatical theories in the past decades that were subsequently/later applied in morphology, syntax or semantics, such as the development of three-dimensional representational structures and the study of constraint interaction. This aspect is at times ignored in some theories that seem to assume that natural language is all about syntax and semantics, with phonology as some kind of additional tag-on. As a matter of fact, very heated debates have been conducted between various approaches that could all be called 'syntacticocentric' (Jackendoff 2002). What this merely means is that phonological theory has in the past decades become somewhat distanced from syntactic theory. This is in itself a regrettable development, but one that can still be repaired, as long as many serious programs in the world still offer syntax alongside phonology. In an interesting paper, Prince (2007:223) has pointed out that there are two main conceptions of a linguistic theory:

> Let us distinguish heuristically between 'Theories of Data' (TOD), which produce analyses when set to work on collections of facts, and 'Free-Standing Theories' (FST), which are sufficiently endowed with structure that many predictions and properties can be determined from examination of the theory alone.

Theories of the former type aim primarily to greater empirical coverage, and tend to stay close to the data. They can be seen as 'summaries' of those data: convenient ways to compress a lot of data (Prince actually compares them to WinZip). Theories of the latter type can be studied 'in their own right': one can just take the theory, set it to work and see what its predictions are (and these predictions can then be tested against empirical reality).

Prince's (2007) sympathy clearly is with the FST; his main interest is in creating formal objects and studying their logical consequences. It seems that the majority of phonologists do not follow this

line of thinking, and work on TOD instead. Both types of activities are necessary. Everybody would probably agree that the optimal theory is an FST and a TOD at the same time: a theory that explains all relevant data, and at the same time has a clear and understood logical structure that can be studied in its own right. Seen this way, there is no obvious answer to what is the right way to go: one can only try to do her/his best, keeping record of insights of previous theories, while at the same time developing daring new theoretical frameworks that can be properly studied and understood.

In this short paper, I will give a brief overview of where we stand with respect to both of these two aspects. There are lots of developments on both sides: I discuss the relevance of new types of data for phonologists in section 2, and some of the more important theoretical discussions in section 3. The main problem with the field now may be that the different parts are growing apart. Phonology is a relatively small field – a few hundred people in the world will call themselves professional phonologists – but with a large task. To be able to understand the reality underlying the sound patterns of human language, we will need to collect many more data and to design better theories; but we need to do so, somehow in a cumulative way, building on the work of our predecessors and our contemporaries. The danger is that for instance experimentalists and theoreticians will consider each others' work irrelevant. I consider this issue largely one of organisation of the field, as I explain in the last section of this short paper, section 4.

2. Phonological data

It is worth pointing this out, since it does not seem obvious to everybody: For several decades, phonological data was seen as incontestable. A researcher working on a language would derive them from her own intuition of from a fieldwork session in which the intuitions of an informant played a major role.

There is no reason to believe that this led to insurmountable problems in the theories that have been constructed on the basis of these data. Many advances in our understanding of topics as diverse as Semitic consonantal roots (Bat-El 2011), Polish syllable structure (Bethin 2011), consonant harmony in child language (Levelt 2011), tonal distributions in Bantu languages (Downing 2011), etc., have been accomplished on the basis of this methodology and it seems difficult to contest the results of all these decades of research.

A reason for this success possibly was that a lot of linguistic data are relatively straightforward and easy to access by intuition. One does not need to engage in refined empirical methodology in order to discover that *blick* is a possible word of English while *bnick* is not (Chomsky and Halle 1965), or that Kikuyu morphemes can influence the tone of the following morpheme (Goldsmith 1990).

The solidity of the theoretical fortress that has been built this way cannot be overestimated. One only has to concentrate on a single empirical field that has been studied for a long time – like Korean laryngeals (Cho 2011) or French liaison (Côté 2011) – to see how much insight has been gained in the past decades to appreciate how much insight has been gained by the interaction between sophisticated theory and ‘simple’ data.

Yet, it is a reason for concern that serious reflection on the quality of the data has not played a more prominent role in textbooks and handbooks that have been produced in recent years. This may be true in particular for the more wide-ranging typological claims which were made on the basis of rather sparse data or even the absence of data (cf. Hale and Reiss 2008 for a long argument against the latter type of ‘negative’ evidence). Fieldwork data are necessarily scanty and fieldworkers may obviously miss important details – for instance because their relevance was not clear at the moment of doing fieldwork.

Furthermore, in the past few decades, many new types of data have become available to phonologists (see Van Oostendorp 2012 for an overview). This advent of new data has sparked an interesting debate on the validity and usefulness of different types of data (see for instance Hyman 2007 and Coleman 2011). Obviously, this discussion also has implications for our evaluation of the theories that have been built up so far. If one believes, with Coleman, that the only reliable,

'scientific' data are those acquired in a laboratory, one should be ready to discard almost all of phonology (which is indeed what Coleman seems prepared to do.)
Whether or not we follow Coleman in this matter depends among other things on one's view on the ultimate goal of phonology. Traditionally, this is seen as the study of sound patterns in human language; but there does not seem to be only one single privileged road into discovering such patterns. We cannot observe human language directly by sensory information – evidence is always indirect in some way.
However, the new types of evidence can clearly shed new light on aspects of human language – patterns can emerge which would not be visible otherwise. In this section, I wish to concentrate on the implications that some of the new types of evidence have on our understanding of phonology. I will discuss their relation to phonetic data, variation data and corpus data in turn.

2.1. The relation to phonetics

One traditional distinction between phonology and phonetics is that the former is about categorical and the latter about gradient data. We know by now that this picture is too simplistic or, to be more precise, that it conflicts with other distinctions we can draw between the two disciplines, e.g. that phonetics deals with the physiological and physical aspects of speech, whereas phonology is about knowledge of language. There is reason to believe that at least some gradient patterns are also 'known' by speakers (Ernestus 2011). One example of this is Final Devoicing. In a long series of experiments on laryngeal neutralisation in speakers of various languages it has been shown that this common phonological process is often not completely neutralising. Port and Leary (2005), summarizing the results of these experiments, state that this indicates that gradient patterns are part of speakers' knowledge of their language.
Port and Leary even set an additional step and claim that such data prove that 'formal phonology' (by which they mean 'categorical phonology') is on the wrong track. That conclusion might be somewhat hasty, for instance because it ignores the fact that the assumption of categoricality has led to so many successful approximations of real language data. Furthermore, even if some knowledge of language is indeed gradient, not all phonetic effects make it into phonology.
A strong example of this is given by Moreton (2008), who shows, in a literature survey, that the effect of consonantal laryngeal properties on the F0 of an adjacent vowel is approximately of the same strength as the influence of the tone of one vowel on that of a neighbouring one. Yet, the consonant-tone interaction rarely, if ever, makes it into the kind of 'hard', 'categorical' effect that can be called phonological as for instance tone sandhi (a phenomenon that may itself later become morphologized and eventually lexicalized). If we gave up the distinction between phonology and phonetics, we would loose such insights .
One of the major puzzles which phonological research therefore must face is how to reconcile categorical and gradient aspects of phonological knowledge into one coherent model. One important problem here is that a classical feed-forward model is probably not sufficient: we cannot simply take the output of phonology and transform it into something more gradient, as assumed in the *Sound Pattern of English* (Chomsky and Halle 1968). The reason is that the gradient effects may work out differently on some lexical items than on others. The relatively best known instances of this are frequency effects (Frisch 2011): for instance, in English, low-frequency words tend to have more aspiration on foot-initial voiceless plosives than those in high-frequency words. Since this is a gradient effect, it should be due to 'phonetic implementation'. But in a feed-forward model, phonetics cannot see the identity of an individual lexical item anymore.

2.2 Variation data

Another simplification of classical models concerns variation. Most classical generative and structuralist models make the idealization that languages are uniform and grammars are deterministic: once we know what the underlying form of a word is, we know how it will be

pronounced – always in the same way. Sociolinguists already pointed out in the 1970s that it would be relatively easy to refine the model by adding a stochastic component (Labov 1969, Cedergren and Sankoff 1974). This idea has been taken up in various forms in current theories; most notably in the form of stochastic constraint ranking in Optimality Theory (Boersma and Hayes 2001).
One ontological problem arises in the combination of generative theory (including OT) and sociolinguistics: the former is based on the idea that language is a property of the human mind (or brain), whereas the latter sees language as a property of a speaking community (Weinreich *et al.* 1968; Labov often refers to this as 'the central dogma of sociolinguistics').
An obvious way to reconcile the two approaches is by assuming that the social structure of the community is reflected in the individual mind. This seems to be the point of view taken by Exemplar Theory (e.g. Bybee 2001, Pierrehumbert 2001): the richness of variation in the outside world is directly reflected in the individual mind. The individual human hears and observes everything that is spoken around her and furthermore stores this information. In such a model there thus is little principled distinction between the observer's inside and the outside world. Under such a model, then, there is only variation and no predetermined pattern.
Most researchers within the exemplar-based paradigm nowadays seem to agree that some kind of abstraction be at work, so that individual exemplars group together in categories. Furthermore, in order to describe productive phonological processes, one needs to introduce principles of 'analogy'. In the absence of a fully articulated theory of analogy, we may assume that it has the properties of a formal theory.
If this reasoning is right, we reach a similar kind of puzzle as that in the previous section: we have evidence that two qualitatively different factors are involved in the explanation of sound patterns. One possible reason for this is that language actually is a multidimensional object, that is rooted in human cognition but at the same time also in interaction. Possibly the two faces of linguistic structure can be related to this – in as far as language is used for human cognition, it is categorical and invariant; in as far as it is the result of interaction, it is gradient and variable. To some extent, these two aspects of language must interface within the human mind, in ways that are currently unknown, since human thought itself is obviously influenced by cultural factors.
That phonology plays a role in thinking may seem less self-evident to some readers than that it plays a role in talking. However, psychologists have suggested that the fact that we can give concepts an abstract (sound) shape, may be a crucial component of human success (Burton-Roberts 2011), because we can give a phonological shape to a proposition we can as it were look at such a proposition in an objective way in our own thought.
This would imply that the phonological shape of a word serves as its cognitive 'address' – it is a reflex of how concepts are stored in an abstract lexicon. One could thus tentatively assume that two types of cognition are involved in shaping phonologies. On the one hand, there is an abstract cognitive system, which is potentially even 'substance-free', i.e. remote from concrete sound shapes. On the other hand, there is sociolinguistic knowledge, which seems to usually involve rather precise details of the pronunciation of individual sounds. One could imagine that also two memories are involved, one dealing with abstract concepts, and the other with social situation. The latter may be responsible for the gradient and variation data we find. As a matter of fact, it might be a form of ordinary long-term memory in which everything is stored in detail. However, every time a linguistic signal is stored, it might be tagged with the abstract phonological shape it is supposed to represent – just like every time somebody produces a word, the implementation may refer to the previous instances that the person heard the word.

2.3 Corpus data

Even if it seems that some of the available evidence could be accounted for in terms of a 'different' kind of phonology, one could still wonder what kinds of evidence we have for the structure of 'cognitive' phonology.
Our empirical scope has considerably grown over the last decades to consist of more than only

judgments of native speakers.
Actually, 'judgments' do not play as strong a role in phonology as they do, for instance, in syntax: phonologists rarely ask their informants whether or not string *xyz* is a well-formed word of the language. However, if we take into account standard tests like minimal pairs, the phonological consequences of morphological concatenation, etc., a lot of data are dependent on the judgment of one informant. One obvious problem with this is that judgments do not necessarily provide us with a direct window into the human language faculty. There will always be noise – the informant does not understand the question, or understand it too well and is guided by her own theoretical biases; she may hesitate in admitting she knows a taboo word, or be too tired to think properly.
An obvious way to circumvent this problem is to test at least controversial data on a reliable sample of informants (who are preferably naive in the sense that they are not aware of any theoretical claim that is tested) and apply statistical methods to the results. This is nowadays often taken into consideration, although it is not always possible to do so.
Furthermore, one should be careful to base oneself on samples of individual judgments collected among possibly large groups of informants when studying phonological systems. The problem is that the average of judgments of a large group of people may itself not be a possible system. As an illustration of the issue, consider the question about which properties (e.g. phonemes) a given language has. Based on fieldwork, we find the following (Roman letters are names of informants, Greek letters names of phonemes):

(1)

A: α, β, γ	E: α, ζ, γ
B: α, ε, δ	F: η, ε, β, γ
C: α, ζ, δ	G: η, ζ, γ
D: α, β, γ	H: η, ε, γ

If we now suppose that the shared language of these informants has the sounds which the majority of the informants has, we come to the following:

(2) Language: α, γ

This is however not a possible dialect of the language, for instance it has two segments rather than three of four. The 'collective' system, thus, is different from that of any individual speaker. Similarly, there are several types of covariation that are difficult to detect. The problem mentioned here usually does not arise, because people pre-process data such as these, and observe that all systems have three phonemes. However, it is very difficult for computers to make such observations, or to 'know' to what extent these are random or systematic facts; notice that the observation which is necessary here is in any case an observation about systems. If we do not record who said what, but just record the individual sounds (as we would do, for instance, in a blind corpus-based study), we would just cumulate all the data in (1) into:

(3) α, α, α, α, α, β, β, β, γ, γ, γ, γ, γ, γ, γ, δ, δ, ε, ε, ε, ζ, ζ, ζ,

The possible structure of a phonological inventory for an inventory can never be recovered from this. It seems more meaningful, then, to do statistics on the basis of whole inventories rather than individual data points.

3. Theory in crisis?

The past two decades have seen the flowering of one theory that has practically dominated the field: Optimality Theory, as proposed in Prince and Smolensky (1993). In particular in the 1990s, this framework served as a kind of *lingua franca* of phonology. Alternative frameworks definitely existed, such as Government Phonology (Kaye, Lowenstamm and Vergnaud 1987) or Articulatory

Phonology (Browman and Goldstein 1992), both of which were proposed at more or less the same time as OT, and acquired some followers – but for a long time, most articles in journals and most conference presentations contained 'tableaux' with shaded cells and pointed fingers.
There are no numbers to justify this claim, but it seems that, at this time, the presence of Optimality Theory is diminishing. There is less debate about some of the problems surrounding the theory (the arbitrariness of constraint ranking, opacity of some phonological processes, the issue of what constitutes a possible (input)form for an OT analysis). It also seems to me that the reason for this is not so much that other theories are replacing OT – or in any case, there does not seem to be any alternative at present has the same sociological power as OT had fifteen years ago. This is not necessarily a problem, obviously – theoretical diversity might actually be a positive state of affairs. Like many other things in life, theories might benefit from competition. One of the pleasant aspects of phonology is that there is relatively speaking quite some interaction between proponents of different theories. The two major phonology conferences in Europe – the Manchester Phonology Meeting and the Old-World Conference in Phonology – both host talks from a large varieties of views on what phonology is, and the wider conferences on formal linguistics (like *NELS, WCFFL* and *GLOW*) also tend to have broader (albeit smaller) sessions on phonology than on syntax.
At the same time, there is also a reason to worry that Optimality Theory is not being replaced (nor is it keeping its predominant role on phonology conferences): there seems some worrisome turning away from any kind of explicit theory on the part of phonologists. There is a lot of admirable experimental work going on, but it often is not clearly embedded in a very explicit theoretical framework. Frameworks such as Laboratory Phonology or Exemplar Theory so far do not seem to have reached a sufficient level of formalisation, but more importantly they do not appear to even aim to achieve it.
To be honest, I find this a reason for concern. But before I go into this, I will summarize some of the reasons why people are turning away from theories, in my opinion.

3.1. Stipulativeness of theories

One reason one often hears is that existing theories are overly stipulative. They may be formal, but they consist of a lot of axioms of which the ontological status is unclear at best. Optimality Theory, for instance, is accused of containing many *ad hoc* constraints. Standing in the tradition of generative grammar, Prince and Smolensky (1993) argued that there is a universal and innate set of constraints, but this is nowadays considered to be unsatisfactory by many scholars. And even repairing this by assuming that the constraints are not innate, but somehow derived from phonetics, is considered unsatisfactory by several people – possibly because the OT architecture itself would still be innate.
There is no need to specifically defend Optimality Theory here. We can observe that indeed many formal phonological theories contain a lot of *ad hoc* stipulations. We also grant for this reason they are very difficult to falsify, in many cases.
The alternative that is usually proposed is a form of reductionism: the explanations for linguistic phenomena should come from independent other factors – for instance, general cognitive mechanisms, articulatory or perceptual ease, or aspects of social interaction.
Obviously, reducing explanations to independently necessary stipulations is good scientific practice. Yet, there are two issues which is worth pointing out. First, it is not necessary to only reduce individual facts to such factors. It may be more efficient to take existing theoretical generalisations, and show how they may be reduced. Importantly, a reduction will only work if we can reduce a theory to another *theory* – a clear set of principles, making clear predictions. For this, it would be necessary to make a choice about what the 'independent factors' are, and what the theory is behind them. I know of very few examples of such theories, except perhaps on smaller subdomains of the traditional domain of phonology. In any case, in the absence of a complete reduction, it is not unwise to continue working on a formal theory.
Secondly, and relatedly, the amount of data we have is very large. It is impossible to compare

corpus data directly to the results of phonetic experiments, or brain scans to the results of extensive fieldwork, as they have nothing in common. In order to be able to say something comprehensive about all these different types of data, we need to refer to a level which is more abstract than all of these data. This level of abstraction is called a theory.
To be clear, none of this means that we should accept theories to be replete with redundancies and with *ad hoc* formal niceties. It means we should constantly work on refining our theories in our hopes that theories of cognition, phonetics or social interaction acquire at some point enough predictive power.

3.2. The lack of predictive power of theories

Another issue that has been put forward against phonological theories is that they fail to make predictions. My impression is that the issue is most often raised against generative phonology as a branch of generative linguistics.
In particular, it has been observed that the quest for universals has failed. The generative enterprise is based on the assumption that the language faculty is richly endowed with language-specific innate structure. The main argument for this has been the existence of universals: features that occur in one language after the other and for which there cannot be a historical explanation. In an important paper about this topic, Hyman (2008) has presented a survey of known phonological universals. The list may be disappointing to some; in particular, when read in a critical way, one might say that Hyman (2008) confirms a much earlier statement by Maddieson (1984):

> *The issue of language universals is difficult because there appear to be so few absolute universals. In the domain of phonology, we cannot move much beyond the statement that all languages utilize consonants and vowels. Perhaps the most specific statements we can make would be to say that all languages have plain stop consonants and low vowels.*

Hyman (2008) shows that the statements can be refined somewhat, and that we can also find a few more interesting universals – in particular if we allow for implicational universals and for the mentioning of contrast (e.g. 'a vowel system can be contrastive for vowels only if there are output nasal consonants'). What emerges from Hyman's paper is that even the few universals we can agree on are not trivial and raise the question why they exist at all.
It is not very difficult to imagine a language without any phonology at all. Such a language would not consist of a fixed inventory of segments and/or there would not be any sequential constraints on ordering such segments in strings. As a matter of fact, humans do use artificial languages of such a type: programming languages are a well-known example: all 'words' in a language like C++ are unanalyzable units and they do not adapt themselves to their context. The string `bnick()` is a possible function name in C++ just as much as `blick()` is (see also Kaye 1989). However, even though such languages are easily imaginable, natural language is never of this type. They all have a small (and well organized) set of sounds and restrictions on how these can be put together into words.
This raises a number of questions: why do all languages have this feature which seems unnecessary? And why do they have this particular way of organizing things ('horizontally' as it were) and not others? Phonological theory has been quite successful in raising the issue, even if there are no solutions available yet.
The ultimate question probably is how it is possible that there has been a discipline of phonology at all. Apparently, over the past century we have been able to uncover a large number of patterns, diachronic and synchronic, categorial and gradient, universal and more specific. Where do these patterns come from?

3.3 Inability of theories to deal with new data

A final point seems to almost bring us back to the beginning of this short overview: existing theories sometimes seem completely unable to handle new types of data. The theories are categorical, the data are gradient, for example, but above all: the theories are clean and the data are relatively messy. Other theories are therefore necessary, which reflect the messiness of the data in a more precise way.

I feel that Prince's (2007) distinction between *TOD* and *FST* becomes relevant again. It seems clear that there are two conflicting conceptions of what a proper theory should look like. One is that the theory should give a compact but extremely precise description of reality – even if it comes at the cost of having a very complex theory that is actually difficult (or even impossible) to understand. The other is that simplicity and straightforwardness are the main goals – even if this comes at the cost of not covering all data.

It is impossible to decide objectively which of those criteria are best. But we can all agree that the ultimate theory should be one that compromises neither on empirical coverage nor on theoretical elegance: it covers the whole world while it can be written on a T-shirt. The choice on which end we should work, or how we should make the compromise while working on it seems an arbitrary one, and it does not seem to make a lot of sense to 'replace' a theory of one type by a theory of another type.

At the same time, it may not be so clear what one should do instead. If there were a strong, broadly accepted theory, one could ask *TOD*ists to work on its empirical coverage and *FST*'ists to refine its theoretical structure. But since there seems very little 'standard' in current phonological theory, one can only hope that practitioners of different types of theory will see the value of what other people are doing and aim at integration of the various approaches.

4. Conclusion

There are reasons to consider the history of phonology as starting with Ferdinand de Saussure, who died exactly 100 years ago. In these 100 years, a lot of work has been done, and a lot of insights have been gained. It is fair to say that we know much more about sound patterns in human language than people did at the beginning of the 20th Century.

At the same time, many phonologists seem to feel that we have not yet reached the standards of some of the 'hard' sciences. If we accept that this feeling is right, we might try to find out how we can get it at this level. It seems clear (i) that human language is a very different kind of object than the physical world and (ii) the world is a different place now than it was in the 17th Century, the age of the scientific revolution.

For instance, scholarly work is much more professionalized. For all phonologists I know, doing phonology is (part of) their job and not some occupation for leisurely gentlemen. We know that, in many different disciplines, this situation has all kinds of consequences. For instance, modern scholars will tend to be slightly more risk averse. They need to aim at having enough results, enough publications. This means that they have a tendency towards what Kuhn (1962) called 'normal science': solving puzzles, or performing experiments, within a certain framework, rather than undertaking the more risky work of developing new fundamental theories, which may take years before obtaining results. (See Smolin 2006 for an argument that this also causes problems in a more established field such as physics.)

It is also not clear what the large amount of new data, and all the technologies which are available to analyse them, mean for a field in such a relatively young state. Again, the situation in the hard sciences in the 17th Century was different – the instruments to study the relevant data and the data themselves were developed in tandem: one does not get the impression that somebody like Newton was drowned in all kinds of data arriving every day.

Nobody knows what to do, of course. It is important to keep the field organized. We will need phonologists in the lab, phonologists in the field, phonologists behind the computer screen and phonologists at the drawing board designing new phonological theories. In addition, we need a way

in which these people can communicate.
The good news is that the infrastructure of the field is rather good. There are several journals with a broad scope – it is fair to say that *Phonology* is widely seen as the most important journal in the field, and the editorial policy of that journal is rather inclusive. Also our conferences tend to be places where phonologists of different theoretical convictions meet. There are critical voices, but there is no bitterness. A few things might still be missing – like an online paper archive that is independent of a specific framework. But these are minor details. We now just need to think and work very hard, to discover the patterns we are all interested in, and try to see whether we can understand where they stem from.

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