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# COMMENTS ON 'SYNTAX SHINDIG' PAPERS

By ANTHONY KROCH
*University of Pennsylvania*
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The 'Syntax Shindig' gave scholars with very different orientations an opportunity to explore in face-to-face discussion their common interest in the historical syntax of English. I am pleased to have been asked by the organizers to comment on some of the papers that arose out of the meeting and have chosen four whose subject matter or methodology touch on my own interests and previous work. I have given a fairly personal, hence one-sided and partial, reaction to these papers in the belief that this way of proceeding well captures the spirit of the meeting itself, where people with different points of view were able to communicate in an open and comfortable way. The exchanges at the Shindig enriched my own understanding and I hope that the brief comments below at least succeed in furthering the dialogues that began there.

## ALLEN, INVESTIGATING THE ORIGINS OF THE 'GROUP GENITIVE' IN ENGLISH

The main point of this paper is to argue that Richard Janda's analysis of the origins of the modern English possessive *'s* clitic (Allen's 'group genitive') as a resumptive possessive pronoun, despite its intuitive appeal, cannot be correct. Janda claims that the possessive clitic arises out of the resumed pronoun in an expression like *my father his hat*. But Allen argues that this resumptive form, while it exists in the history of English, arises only in early modern times. According to Allen, the bracketed forms in expressions of the type 'N [*ys*/*is*/*es*/*his*] N', which she calls the 'separated genitive' are, despite the spacing, only spellings of the genitive ending, and none of them is ever a pronoun. Her primary evidence is the lack of number and gender agreement between the putative pronoun and its antecedent: the form always appears in what would have to be a spelling of the masculine singular *his*. As she mentions, in colloquial Dutch, where the pronominal construction undoubtedly exists, number/gender agreement is obligatory. The same is true for colloquial German, which has the construction with the possessor NP in the dative case: *meinem Vater sein Hut*. It is a bit puzzling, given the Dutch and German data, that this construction did not exist in Old English. There are a few apparent examples that have been cited, but Allen argues that they are all amenable to other analyses. In any event, given the absence of agreement with the separated genitive, the potential examples are unlikely to be the source of the separated construction, even if they are genuine. Allen shows as well that the separated genitive is contemporaneous with the attached inflectional genitive as found on irregular and feminine nouns quite late into Middle English and gives further spelling evidence against interpreting the separated genitive as a pronoun. Thus, the case against Janda's proposal is convincing.

The question then becomes when and why the possessive clitic arose. According to Janda, the reanalysis was a consequence of the loss of case-marking elsewhere in the nominal paradigm, but Allen has no firm view on why the reanalysis should have occurred. She points out, however, that the loss of a distinctive feminine genitive ending would have been a precondition for the change. On the matter of dating, she takes the conservative position that the possessive ending, however spelled, should not be analyzed as a clitic until the earliest appearance of clear instances, which for her are ones in which the ending is attached at the end of an NP with a postnominal PP, as in *the god of Love's*. In such a case, if the ending is interpreted as an inflection, it has attached to the wrong noun. The earliest examples of this type are of the late 14th century. If this date is correct, it rules out Janda's proposal as to the cause of the change, since case marking collapses considerably earlier than this. However, while Allen is correct that such examples are the best evidence for the clitic, it is not obvious that their first appearance gives us a good date. It is possible that the construction arose before, even quite a long time before, the first knockdown examples. This possibility, of course, always exists for new constructions in languages for which we have only textual evidence, and that limited in stylistic range. In this case, the problem is particularly acute because it is very rare to find examples where use of the possessive clitic yields a string distinct from what the genitive ending would give. Apparently, there are less than half a dozen examples in the PPCME's 510,000-word corpus, and I suspect that the construction would also be infrequent in a modern corpus, except in texts where the syntax of the spoken language is allowed to enter freely. Given the rarity of the form, we must accept that any count of occurrences will give us a poor estimate of its date of origin. On the side of an earlier origin are certain early examples cited by Allen, which she treats as grammatically distinct from the full-blown genitive clitic but which are hard to distinguish on structural grounds. These are examples like *our Lorde the Kyngus wille*, in which the possessive ending is attached to an appositive noun phrase. Such examples occur as early as Orm (*c.*1200) and apparently become standard by 1300, nearly 100 years before the first examples that Allen counts as certain. Furthermore, there is the evidence of the spelling itself. For Allen, the spelling has no linguistic significance, but an alternative view might be that the spelling reflects the reanalyzed possessive clitic from the beginning.

Obviously, we can comfortably accept an early date for the possessive clitic only if we can explain why the first entirely convincing instances appear so much later. One possible explanation is that the texts reflect the conservatism of the written language. If the clitic was resisted in writing as an undesirable innovation from speech, experience with sociolinguistic variation predicts that it should at first appear in texts only where its surface difference from the old genitive ending was minimal; that is, on NPs without postmodification. When an author attached the clitic to an appositive, the usage might occasionally have escaped editorial censorship because the misattachment of the genitive ending to a noun coreferential with the head noun of the phrase would not be salient to an editor whose own speech countenanced the possessive clitic. If further evidence for such an interpretation can be found, Janda's causal proposal will be worth another look even if his views on the source of the clitic form must be rejected.

ANDERSON, ON VARIABILITY IN OLD ENGLISH SYNTAX, AND SOME CONSEQUENCES THEREOF

The syntax of the Old English clause is complex, but work by generative grammarians over the last decade has brought a considerable amount of order to this complexity. Anderson's paper reflects well the current status of our knowledge of Old English syntax and contains several interesting proposals of its own. Grasping the relationship between this work and other recent studies, however, is made difficult by the author's decision to present his analysis in a notation that will be unfamiliar to many readers and by his further decision not to highlight the many points of agreement between his and other analyses. In these comments, I will focus on the relationship between Anderson's paper and other recent work on Old English syntax and on the empirical issues at stake. I will not attempt a critique of the notational scheme, which would require an extensive technical discussion outside the scope of historical syntax proper.

The first matter that deserves emphasis are the substantial points of agreement that specialists in Old English syntax have achieved. Thus, it is now generally acknowledged, after the work of Kemenade (1987), that the dominant pattern in Old English clauses is verb-second (Anderson's 'prime' structure) in main clauses, and verb-final (Anderson's 'non-prime' structure) in subordinate clauses. In addition, however, work by Pintzuk (1991, 1993) has shown that subordinate clauses may exhibit Infl-medial structure (Anderson's 'subject prime' structure) and that the frequency of this structure increases over time within the Old English period. There is also an option for verb-final main clauses, which Anderson and many others treat as marginal, wrongly in my opinion (see Kroch and Taylor 1995). The variety of clause types found in Old English is sufficient to demonstrate that the language is, as Anderson says, 'syntactically mixed'; and Pintzuk's work shows that this mixture is diachronically unstable. It is these results, along with similar findings in work on other constructions and languages (Kroch 1989, Fontana 1993, Santorini 1993, Taylor 1994), that form the basis for the hypothesis that syntactic change advances diachronically through grammar competition.

For Anderson the number of different 'systems' in Old English is somewhat greater than for other researchers. He claims, contra Kemenade and Pintzuk, that the subject prime structure is not a variant of the general verb-second pattern but a separate option. His reason for this is the existence of examples in which more than one constituent precedes a non-final verb, an order which is, on the surface, incompatible with verb-second syntax. Anderson's examples are interesting but not conclusive, because he does not systematically rule out the obvious alternative possibilities: that these sentences exhibit verb-final structure combined with the postposing of some constituents, or that parenthetical elements in preverbal position are obscuring a verb-second structure. Thus, the following superficially verb-third sentences, similar to some of Anderson's examples, are natural in German despite its rigidly verb-second main clause syntax:

(1) Nichtsdestotrotz, wir müssen weiter.
nevertheless we must (go) further

(2) Diese Gäste dagegen /übrigens/ auf jeden Fall können
these guests however /by the way/ in any case can
wir nicht mehr einladen.
we not more invite

More work is required before we can determine whether the range of exceptions to verb-second word order is significantly greater in Old English verb-medial clauses than in the modern verb-second languages. The necessary comparison must be between the patterns in the Old English texts and those in relevant written documents from other verb-second languages. It must not be based on summary grammatical descriptions of the modern languages, because these almost always abstract away from usage factors, like the use of parentheticals and discourse particles, that are felt to be irrelevant to grammatical structure.

An important addition made by Anderson to the inventory of clause types, here agreeing with a recent paper by Pintzuk (1996), is that, at least in the Old English of Ælfric and later writers, there is evidence for structural VO word order; that is, we sometimes find VO ordering of the main verb and its complements in cases where postposition of the complements is unlikely or impossible and where the verb-second effect is controlled for by the presence of an auxiliary verb. The existence of these clauses is evidence that Old English exhibited a second sort of grammar competition (between OV and VO structure) in addition to the Infl-medial/Infl-final competition mentioned above. Hence, the beginnings of the transition to the VO structure of modern English apparently predate Middle English, the period to which the change is usually assigned. This redating may not yet be accepted among specialists, but the evidence for it is strong. Anderson gives the relevant examples, including one with a postverbal pronoun object, which could not have been postposed by syntactic processes otherwise known to exist in the Germanic languages.

Like many students of Old English, but contra Pintzuk (1991), Anderson does not distinguish structurally between verb-second sentences with ordinary topics and those with 'affective' elements (negative, interrogative, subjunctive, etc.) in topic position. Failure to make this distinction, however, leads to difficulty in accounting for the placement of weak or 'clitic' pronouns. Anderson addresses the difficulty by proposing a pre-verbal niche for these pronouns in the non-affective case, but he gives no explanation for why weak pronouns in sentences with affective topics should occur after the verb. Pintzuk's solution to this problem is to say that the verb appears higher in the clause (in the COMP position) in sentences with affective topics than it does in sentences with ordinary topics (where it appears in the INFL position). In a variant of Pintzuk's analysis that we have proposed (Kroch and Taylor 1995), this difference in verb position allows weak pronouns in all clauses, both main and subordinate, to be placed in the same position; that is, immediately to the right of COMP. Since this is the position that standard generative analyses assign to weak pronouns in German, our proposal potentially gives a unified analysis of weak pronouns in West Germanic.

One attractive feature of Anderson's discussion is his separation of the notion 'syntactic subject' from the notion 'morphosyntactic subject'. The former occupies the canonical subject position immediately to the left of the tensed verb, while the latter manifests nominative case and agreement with the verb. Only in sentences with morphosyntactic subjects in the canonical subject position do the two notions pick out the same constituent. This distinction seems a valuable way of distinguishing the function of subjects as default topics from their morphological properties; and, as Anderson points out, it is helpful in characterizing expletive subjects like the *there* of existential sentences. Interestingly, this idea of Anderson's is proposed independently in Chomsky's most recent work (1995), where it appears as a bifurcation in the way that the so-called 'EPP' (Extended Projection Principle) feature, on the one hand, and the agreement and case features, on the other, are manipulated. The convergence between Anderson's and Chomsky's formulations is quite striking and indicates the extent to which, in current syntactic theorizing, notational differences hide conceptual similarities.

Anderson makes two ambitious proposals that depend on his notational scheme and that at once deserve and resist careful evaluation. First, he gives a solution to the problem of the absence of Main Verb > Object > Auxiliary Verb order in Old English and, second, he proposes a link between the rise of syntactic auxiliaries of the modern sort in the history of English and the loss of verb-second word order. In both cases, the proposals are difficult to judge because his exposition does not make clear what his scheme allows and what it does not. With regard to the first problem, Anderson tells us that his schema (24) excludes the unwanted word order in a principled way, in contrast to the stipulation needed by Pintzuk to block it. But his solution is of interest only if no rule schema allowing the unwanted order is formulable within his notation. Otherwise, the schema is itself stipulative and, crucially, cannot explain why the unwanted order is absent, not only in the West Germanic languages, but also in all other Infl-final languages. With regard to the purported relationship between the rise of the auxiliary and the loss of the verb-second constraint, Anderson's discussion is simply opaque. He appears to be claiming that true syntactic auxiliaries, by their nature, can take only subjects as topics while full verbs can take any sort of topic. It is, however, entirely unclear why this restriction should hold or how it follows from his notation. Indeed, he remarks that auxiliaries (actually, only auxiliaries) can take affective topics in modern English. So, the proposed restriction has to be overridden in the affective case and it is not clear how it can be formulated with the required limitation without resorting to a brute force solution.

## HUDSON, THE RISE OF AUXILIARY *do*: VERB-NON-RAISING OR CATEGORY-STRENGTHENING?

In 1953, Alvar Ellegård (1953) published an extensive grammatical and statistical study of the origin and rise in the use of English auxiliary *do* over the period from the 13th to the 17th centuries. This paradigm diachronic study, based on several thousand examples and exhibiting a thoroughly modern understanding of the quantitative study of linguistic variation and change, has been the basis for a large number of subsequent discussions of this important and, from a comparative perspective quite peculiar, evolution in the morpho-syntax of English. In 1985, Ian Roberts published an article applying generative syntactic theory to the evolution of the English auxiliary, concluding that modern English differs from Middle English in a way similar to how it differs from French; namely, in failing to exhibit a transformational movement of tensed main verbs to the auxiliary position (so-called 'V to I raising'). The loss of this movement forces the appearance of *do* in those cases where the auxiliary position must be filled for independent grammatical reasons, most obviously in negative sentences and in questions. In 1989, I published a reanalysis of Ellegård's statistics, showing that the quantitative evolution in the use of *do* from the beginning of the fifteenth to the middle of the sixteenth century supported Roberts' analysis. The article also presented evidence for a regularity in the evolution of usage frequencies in texts during syntactic change which I have since called the 'Constant Rate Effect' (CRE). The CRE states that in all linguistic contexts in which an innovative form is in competition with a conservative one, the use of the innovative form increases over time at the same rate, when this increase is measured on the logistic scale. The CRE has been confirmed in several quantitative studies of syntactic change, some of which are reported in the 1989 article and others in subsequent publications (Santorini 1993, Pintzuk 1995, among others). Hudson's paper, while accepting the reality of the CRE, attempts to show that the facts do not support Roberts' analysis, as I had claimed. However, he fails to understand the mathematical model that underlies the quantitative result, and this failure vitiates his criticism. Although I differ with Hudson's paper on many points, I will focus these remarks on his misunderstanding of the mathematical model that underlies the CRE, as this misunderstanding is crucial for anyone who cares to compare our analyses.

According to Hudson, Ellegård's data show that the use of *do* spreads from one context to another over time. He specifically rejects my interpretation of the same data, according to which *do* is present in all contexts from the beginning of the change and differs from context to context only in frequency. In taking this line, Hudson agrees with everyone who has looked at the data in a qualitative way, including me when I started to investigate the time course of syntactic changes in 1982. Indeed, I began my studies thinking that the temporal evolution of syntactic changes would pose just the sort of challenge to generative theory that Hudson attempts to mount. What changed my mind was the discovery of CRE. The interpretation of frequency curves as indicating spread from context to context is attractive because of their S-shape. Due to this shape, the period when the use of an innovative form increases most rapidly is always later in less favoring contexts than in more favoring ones. Hence, if we consider the period of most rapid increase as the crucial one for dating a change in a context, the different contexts will necessarily differ in date, and we must say that the change spreads from context to context. But there are problems with the assumption that the period of rapid change is crucial in this way. First, the innovative form always appears in a given context at an appreciable frequency and for an appreciable length of time before what Hudson calls its 'growth spurt'. Why then should we use the growth spurt to date when a context adopts the innovative form? Second, for some changes, there is clear evidence that the innovative form occurs initially, not in the contexts which most favor its use and for which the growth spurt occurs first, but in all contexts attested with sufficient frequency to make measurement possible. This is the case, as Ellegård points out, for *do*, which shows up in affirmative declarative sentences as soon as in any other context, even though its frequency is lowest in just this type of sentence. Hudson, from his reading of Denison (1993), is aware of this fact but he does not appreciate its importance. In what sense can we say that *do* spreads from questions to negative sentences to affirmative declaratives, if there is no period at which it occurs in questions but not in the other contexts?

The most important point, however, is a mathematical one. Given the S-shape of curves of change, we cannot use raw frequency data to give us a single parameter for the rate of increase in the use of the innovative form. This is because at the beginning and end of a linguistic change, the rate of increase is low, while in the middle of a change it is very high. In order to associate such a nonlinear evolution with a single rate, it is necessary to model it with some nonlinear mathematical function. I chose the logistic, as it is the simplest appropriate function and the one commonly used to model cases of competition in other fields. My decision to model the data in this way could be challenged, but Hudson does not do so. He accepts the statistical results that depend on the logistic model and tries to explain them in a new way. But accepting the results should commit him to the mathematical reasoning that underlies them. In this case, the crucial point is that a logistic curve of frequency is transformed into a straight line when plotted on an appropriate scale. This is why such a curve can have its rate of change given as a single number. The number is just the slope of the straight-line logistic transform that corresponds to the original frequency curve. The rates that are held to be equal under the CRE are the slopes of the logistic transforms that are calculated from the frequency curves of the different linguistic contexts of a given syntactic change.

It is important to understand that the straight-line transform of the logistic function runs from $-\infty$ to $+\infty$ in both the x and y dimensions, which, in the modeling of language change, represent time and (the logistic transform of) frequency, respectively. For this reason, acceptance of the logistic model is incompatible with the use of frequency data to date a change. As the reader will realize, when two straight lines that run from $-\infty$ to $+\infty$ are parallel, they can be exactly superimposed either by sliding one of them along the x-axis or by sliding it along the y-axis. Hence, the model is neutral between an interpretation under which one context precedes another in time in its adoption of the innovative form and one under which the first context only uses the innovative form more often than the other. The first interpretation corresponds to seeing the lines corresponding to the two contexts as displaced along the x-axis and the second to seeing them as displaced along the y-axis. In other words, once we adopt the logistic model, we can only choose between the two opposed interpretations if we have information in addition to the relative frequencies of the innovative form in the different contexts and the location in time of the growth spurts of the different contexts. As mentioned above, in the present case, Ellegård gives good evidence that *do* appeared simultaneously in all contexts; and this evidence, in conjunction with the CRE effect, leads to the conclusion that there is no spread of *do* from context to context over time.

Hudson claims, following Warner (1993), that the spread of *do* reflects a gradual strengthening of the linguistic category 'Auxiliary Verb' in English and that the frequency ranking of contexts reflects how strong the Aux category must be in order for *do* to be used in each context. He recognizes that the CRE poses a challenge to this interpretation. Why, under his interpretation, should the rate of increase in the use of *do* be the same in different contexts when these contexts are differentially sensitive to the strength of the Aux category, as measured by the date of their growth spurt? I found no plausible answer to questions like this in my investigations, and this failure led me to the analysis in my paper. Hudson's answer is partly that *do* is spreading through the same lexicon of verbs and through the same population of speakers no matter what its syntactic context and that these facts are sufficient to explain the CRE. But they are not. There is no reason why the use of *do* should rise at the same rate in questions as in negative sentences just because the same verbs are used in the two sentence types or because the change moves through the same population of speakers (same sociolinguistic groups, not individuals, of course, since the growth spurts of the different contexts are more than a generation apart). A functional and diffusionist view like Hudson's is, on the contrary, more compatible with a temporal evolution in which different contexts show different rates of increase, because the functional factors favoring the use of an innovative form would be expected precisely to differ by context. This is what Bailey (1973) thought must happen and what the CRE directly contradicts. While Hudson asserts that the CRE is expected under his scenario, he gives no substantial argument or evidence in support of this assertion. It is worth noting that Ellegård's evidence actually challenges Hudson's clearest empirical claim; namely, that diffusion through the same lexicon should lead to the same rate of change. Ellegård points out that certain verbs resisted the use of *do* more than others, which might suggest lexical diffusion at work; however, the verbs which exhibited such resistance were not the same in questions as in negative sentences. Hudson's proposal predicts the opposite.

Hudson's discomfort with the CRE is clear. At one point, he suggests that it is not strictly valid but is the result of some unspecified sort of averaging of the data. At another point, he says that the rise of *do* happens for different reasons in different contexts, for functional (i.e., processing) reasons in 'high *do*' contexts and for 'cognitive' reasons in 'low *do*' contexts. Hudson interprets the fact that the CRE holds across these two groups of contexts as indicating rough equality in the strength of functional and cognitive pressures. In other words, it is for him essentially an accident that the CRE should hold, not only within the groupings of contexts that are most similar but also across what he thinks is the major linguistic division in the contexts. In truth, Hudson's interpretation of the history of *do* would be much more attractive if, despite the evidence we have accumulated, the CRE were to turn out to be invalid. Hence, we can hardly take his analysis as a successful explanation of the effect, which I continue to believe should be interpreted as a reflex of grammar competition in the sense of our original work (Kroch 1989, Santorini 1992).

## Koopman, Another look at clitics in Old English

This paper addresses a crucial question for the syntax of Old English and the Germanic languages generally – the status of pronouns. Recent generative analyses have termed Old English pronouns 'clitics' and have appealed to the special syntax of clitics to explain why pronouns appear in positions where full noun phrases ordinarily do not. Unfortunately, the notion 'clitic' does not have an agreed meaning in linguistics. Koopman defines the term by reference to a standard set of diagnostics, based primarily on Kayne's treatment of

French verbal clitics, and he evaluates the behavior of Old English pronouns with respect to this standard. Not surprisingly, he finds that Old English pronouns do not conform to it very well. It is important to recognize, however, that having pronouns that exhibit behavior intermediate between Kaynean clitics and full noun phrases is not a peculiar characteristic of Old English. Rather, it is quite general to the European languages, a fact that has led syntacticians in recent years to propose an additional pronominal category, the so-called 'weak pronoun' (Holmberg 1991, Cardinaletti and Starke 1994). In other words, there are two kinds of non-clitic pronouns: the 'strong' pronouns, which bear stress, are usually emphatic, and appear in the position of full noun phrases, and the 'weak' pronouns, which lack these behaviors. Unlike clitics, which can be considered syntactic heads or even affixes, however, weak pronouns seem to be full phrasal projections whose special syntax is related to some prosodic or structural deficiency. This deficiency leads them, for reasons that are not well understood, to resist stress and localization and to move leftward in the clause to one or more peripheral positions, left-adjoined either to the verb phrase or to the sentence.

From the perspective of the historical syntax of Old English, the adoption of a three-way distinction among pronouns is valuable because it allows us to describe the language without constant recourse to exceptionality. Specifically, we no longer need ask why Old English pronouns have no overt host, as Romance clitics do, since the absence of such a host is general to the class of weak pronouns. The appearance of unstressed pronouns in topic position is also no longer unexpected, since weak pronouns are phrasal projections; and the tendency of pronouns to appear at the left edge of subordinate clauses in Old English reflects straightforwardly their 'weak' status, just as in modern German. Note, however, that this analysis commits us to saying that the tensed verb in a topicalized sentence like (3a) is in a lower position than it is in its German counterpart (3b) since, unlike in German, the verb is to the right of the weak pronoun:

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (3) | a. | Ælc | yfel | he | mæg | don | b. | Alles | Böses | kann | er | tun. |
| | | every | evil | he | may | do | | all | evil | can | he | do |
| | | (WHom 4.62) | | | | | | | | | | |

Adopting a three-way classification of pronouns does not render the patterns that Koopman describes entirely unproblematic. When a pronoun appears in a coordinated structure, with a modifier, or in its base syntactic position as the object of a verb, it is most naturally considered strong; but there is no morphological indication of such status in Old English. Although strong and weak pronouns are often spelled alike, the absence of any difference in the way they are written makes the use of the distinction somewhat dangerous in historical studies. However, statistical evidence can perhaps support the distinction. For example, we would expect most pronouns in running text to be weak; and, therefore, we should not find a high frequency of unambiguous instances of object pronouns in their base position. A trickier matter is the striking ability of Old English pronouns to move leftward out of prepositional phrases. In other West Germanic languages, such movement is restricted to a special class of locative pronouns, but in Old English ordinary personal pronouns exhibit this behavior. It is not clear whether the movement should be treated as obligatory for weak pronouns, so that all cases without movement would involve strong pronouns, or whether the movement, though restricted to weak pronouns, requires an additional trigger. The first alternative entails an important difference between Old English and German pronouns, since the latter, even when weak, do not move out of prepositional phrases in the way that Old English ones do. Statistics, here on the frequency with which Old English prepositional object pronouns move leftward, might help to decide this question as well.

*Department of Linguistics*
*619 Williams Hall*
*University of Pennsylvania*
*Philadelphia*
*PA 19104-6305*
*USA*
*e-mail:* kroch@ling.upenn.edu

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