Originalism About Word Types

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Abstract

According to Originalism, word types are non-eternal continuants which are individuated by their causal-historical lineage and have a unique possible time of origination. This view collides with the intuition that individual words can be added to the lexicon of a language at different times, and generates other problematic consequences. The paper shows that such undesired results can be accommodated without abandoning Originalism.

According to Originalism (ORG) (Sainsbury and Tye 2012: 58–63; Sainsbury 2015), word types are non-eternal continuants individuated by their causal-historical origin. Rather than on the basis of their intrinsic features, words should be typed by considering the baptism-like circumstances in which they originated, and the chain of linguistic transmission responsible for their propagation through the community of their users. Structural-functional attributes such as spelling, phonological form, syntactic category, or semantic features fail to individuate word types. Since words evolve, they may change in any of these respects while nonetheless preserving their type-identity. For ORG, the correct view is that a collection C of word tokens clusters under a type W if the spatiotemporal distribution of C makes up a consistent lineage of deference-based W-reproducing events which lead back to the singular event in which the first token of W was produced. Importantly, word types stand in a one-to-one correspondence with originating event tokens: each word type is paired with a unique originating event token, and no two distinct event tokens can originate the same word type.

As a restricted thesis about the individuation of names, ORG appears plausible, since it offers a nice way to track the well-established distinction between generic and specific names

popularized by Kaplan (1990). As an unrestricted thesis about word individuation, however, ORG looks much more contentious. One crucial problem is the following. Suppose, with ORG, that if W is a word type of a language L, there is an originating event token E, occurring at a time t_E, which introduces W in L. Suppose also, in line with ORG's requirement that word types stand in a one-to-one correspondence with originating event tokens, that no individual word type other than W can be introduced in L through E, and that no event token other than E can yield the origination of W. Then assume that the parameters fixing numerical identity for event tokens include time of occurrence. On this premise, E cannot be conceived of as occurring at any time t_x different from t_E (and, for the sake of our argument, distant enough from t_E to neutralize effects of modal tolerance). In fact, if the time of the originating event at hand changed into t_x, the occurring event token would be some pseudo-E numerically distinct from E. The consequence is that we should take it to be impossible for W to be introduced in L at any time different from t_E. For if the introduction of W in L occurred at t_x, its originating event token would be pseudo-E rather than E, and since word types stand in a one-to-one correspondence with originating event tokens, W would fail to be introduced in L: some pseudo-W would be introduced in L in W's place.

ORG is thus committed to the claim that words need to be typed according to their time of introduction in a language. Paralleling Kripke (1980), Salmon (1981), and Forbes (1985), this implies what we might call Essentialism About the Time of Origin of Words (ETW). According to ETW, if W is a word type and L is a language, there is a unique originating event token E such that in order for W to be introduced in L, W must be introduced in L at t_E (or: if W is a word type and L is a language, then there is a unique originating event token E such that failure to introduce W in L at t_E implies failure to introduce W in L *toto coelo*). However, we seem to share the intuition that individual word types can originate at different times. For instance, we regard as truisms the following two statements: (a) the word 'pasteurize' might have been introduced in English n years after 1881 if Pasteur's germ theory and the process of heating liquids to eliminate pathogenic

microbes had been popularized n years later than they actually were; (b) had that happened, the introduced word type would have been the same word 'pasteurize' we are familiar with. Should we then abandon ETW and, because of ETW, the whole theoretical agenda of ORG?

Sainsbury and Tye (2012: 178) consider a version of the objection and sketch two replies. (Sainsbury and Tye discuss concepts, but their line of argument naturally extends to words.) Let us call them Alpha and Beta. Alpha maintains that there is a flaw in the case pressed by the objection. More precisely, from "the principle that events that occur at different times are distinct", it is impossible to conclude that "an event could not have occurred at any time distinct from the time at which it actually occurred". Beta submits that while it can be accepted that the introduction of some pseudo-W displaying the exact same linguistic properties as W could occur at times different from t_E, it does not follow that the actual introduction of W could have occurred at a time different from t_E. Thus, "there is likely to be controversy about whether [word]-introductions could have occurred at times other than their actual times". Putative thinking of an individual word type W being introduced in a language L at two times t_1 and t_2 should be reinterpreted as thinking of two numerically distinct types W_1 and W_2 matching in observable linguistic properties and each introduced in L at its proprietary time t_1 or t_2.

Let us start with Alpha. Alpha's task is to convince us that in order to pursue ORG, one does not need to accept ETW. But the argument is unconvincing. It is hard not to derive, from "the principle that events that occur at different times are distinct", the conclusion that "an event could not have occurred at any time distinct from the time at which it actually occurred", as long as it is accepted that time of occurrence individuates event tokens. If you believe that events that occur at different times are numerically distinct, speaking of event tokens as things that can occur at different times is simply not an option. In particular, if time of occurrence is definitional of event token-identity, and it is necessary for any two events occurring at different times to be individuated as distinct tokens, then no word-originating event can preserve its token-identity once

the variable of its time of occurrence is manipulated. The derivation of ETW follows straightforwardly.

One might try to circumvent the problem by stressing that Sainsbury (2015) usually writes of originating "acts" as opposed to originating "events". Now, times are indeed essential to event-token identity. But acts have an additional intentional component which makes them suitable to be realized by different physical event tokens. So act-token identity is liberal with respect to event-token identity. But if act-token identity is liberal with respect to event-token identity and word originations are properly understood as acts, we may refrain from buying into the notion that word originations could not have occurred at any time distinct from the time at which they occurred. However, my worry is that in order to make sense of the intentional ingredient that is supposed to guarantee act-token identity despite variation in the underlying event-token, we would have to contravene ORG. Suppose that A and B are two originating act tokens occurring at different times, and we want to determine under what intentional conditions A and B can be considered the same. The natural answer would be that A and B engage in the act-identity relation if the target of the intention at hand is the introduction of a lexical element bearing the same structural-functional properties across the two cases (otherwise the intentions themselves would diverge). But this would mean reverting to the individuation criteria based on intrinsic linguistic attributes that ORG was trying to supersede. Besides, the very notion of "originating act" seems rather ill-suited to describe cases of lexical innovation in which a new word is introduced in a language without any recognizable intention to do so (e.g., lapses, accidental blends, and the like). The upshot is again that if you want to buy ORG, ETW is probably going to be part of the package. It should be stressed that ETW makes perfect sense within the overall makeup of ORG. In fact, from the uncontroversial premise that originating events occurring at different times in duplicate worlds are bound to initiate materially distinct stages of linguistic transmission, and the ORG premise that word types are non-eternal continuants constituted by the sum of their historical stages, the

conclusion that no individual word type introduction could "have occurred at any time distinct from the time at which it actually occurred" follows naturally.

The task of Beta, by contrast, is to show that even if ETW proved definitional of ORG, the theory would not be *ipso facto* untenable. Beta's line of argument is much more promising. In particular, Beta encourages us to consider, I believe correctly, that ETW's requirement of a one-to-one correspondence between word types and originating event tokens threatens ORG only if one has positive motivations to resist the idea that word types might differ *solo numero*. To understand the point, suppose that a word type W bears a certain set S of structural-functional properties (again: spelling, phonological form, syntactic category, semantic features, and so forth). Suppose furthermore that W can be introduced in L only through the event token E, at t_E. Now, from the requirement that the introduction of W in L can only occur at t_E, it does not follow that it is impossible for word types bearing the exact same set S of intrinsic attributes as W to be added to L at a time other than t_E. In principle (e.g., abstracting from structural constraints of integrability in the lexical system of the target language), duplicate word types exhibiting the same structural-functional properties of W can be introduced in L at any time other than t_E. What ORG is asking us is simply to consider such twin types as numerically distinct from W.

Which leads to the question: why exactly is it so problematic to entertain the ETW-based notion that type-distinctness and linguistic indiscernibility may coexist in the realm of word types? Or: why do we feel that there is something irresistibly wrong in the idea that word tokens displaying the same set of structural-functional attributes may nonetheless cluster under different types? The reason is probably that, by embracing ETW, ORG does the following: (a) it commits to a number of attestable word types which exceeds the number of attestable word types one would be committed to if word types were individuated solely on the basis of structural-functional considerations; (b) it licenses the overgeneration described by (a) while affording no observable advantages in the explanation of linguistic behaviour (after all, why should one view two word

tokens A and B as instances of different types, if A and B are to all appearances and even functionally indistinguishable?).

By way of illustration, suppose that two isolated communities M and N of speakers of Northeastern New England English (NENE) independently add to the lexical system of their dialect the verb 'blurk'. Suppose also that, by a miraculous coincidence, M-introduced 'blurk' and N-introduced 'blurk' are assigned the exact same set of structural-functional properties. M-introduced 'blurk' and N-introduced 'blurk' exhibit the same semantic features, the same syntactic category, the same argument structure, they respond to the same morphological conditions, they manifest the same patterns of stress and tone assignment, they are realized through the same articulatory gestures and written symbols, and so forth. Now ask: how many word types have originated in this scenario? A structural-functional approach, for which word types are "collection[s] of phonetic, semantic, and formal properties, which are accessed by various performance systems for articulation, perception, talking about the world, and so on" (Chomsky 2000: 151), will commit to the conservative solution that M and N have independently introduced in NENE the same word type, since in the proposed scenario there would be no linguistic reason for members of M and N to discriminate between M-introduced 'blurk' tokens and N-introduced 'blurk' tokens. By contrast, ORG will count 'blurk'$_M$ and 'blurk'$_N$ as two types, and insist that the difference in origin between their tokens, albeit irrelevant to the task of explaining the linguistic behaviour of M-based and N-based users of 'blurk', must be taken to yield distinct type-categories.

Here is, then, one way the argument against ORG could run. Because language learners have no access to the history of their language beyond the data presented by their caretakers and peers, facts about the historical origin of a lexical symbol S cannot be relevant to a model of how the lexical system of language users types S, nor to a model of the computations that their internal grammar is allowed to perform upon exposure to tokens of S (Hale and Reiss 2008: 161). Importantly, it seems that it is only by typing lexical vehicles with respect to their narrow

structural-functional profile (which excludes information about historical origin) that we can accomplish non-trivial tasks such as producing scientific psychology (e.g., Aydede 2000). Moreover, the operating principle that word tokens are to be grouped under a single type whenever their linguistic properties license an association to the same mental symbol (or to symbols displaying the same narrow computational profile across users) correlates nicely with the explanatory success of formal grammars. Nothing similar seems forthcoming from ORG. Hence, ORG's notion of numerically distinct types displaying the same linguistic properties should be resisted. One should not have identity in properties amenable to judgments of linguistic role (or scrutiny by an internal grammar) without also having type-sameness, regardless of time of introduction in the language.

At this point, the situation might seem intractable. But notice that there is a simple way out. The key lies in acknowledging that ORG and grammatical typing operate at different levels, pursue fundamentally distinct explanatory goals, and cannot be placed in direct competition with one another. The two frameworks do not make rival statements within a shared conception of what moves are legal in the game of word individuation: they simply play two different games. ORG focuses on historical ancestry and sets out to deliver a *diachronic* lexical taxonomy, whereas the theoretical labour carried out in typing word tokens based on structural-functional equivalence falls in the scope of *synchronic* linguistic analysis (Egré 2015). The anti-ORG argument to the effect that word types are clusters of linguistic properties was based on an observation of how word tokens are organized into type-categories for the purposes of grammatical processing. Yet, upon closer examination, ORG is fully compatible with the tenet that word tokens count (*for* language users, in an *epistemic* sense, *relative* to the make-believe game of grammatical calculus) as tokens of a given type because the they are perceived as projecting such and such linguistic properties (see Cappelen 1999: 99–101). Hence, the seemingly substantive mismatch between the two styles of individuation may call for a simple reduction to a difference in the arbitrary

informational reach adopted by the two typing systems. The different 'word' concepts underlying ORG and structural-functional typing are both coherent and potentially instructive ways of organizing into type-categories a space of word tokens. Their divergence lies at the level of the range of variables that their "typing algorithm" is designed to compute (compare to the notion of proper names bound in semantic vs. cognitive types proposed by McCulloch 1991: 75–76). Structural-functional typing considers just synchronic grammar; ORG adds a further layer of diachronic information to the addressed data. In most cases, coupling the two techniques will yield, rather than an inconsistent system of types, an instance of the multi-layered ontologies familiar to readers of information science. In the 'blurk' case, for instance, grammatical typing will first individuate the synchronic super-type 'blurk'$_{null}$, then ORG will implement diachronic sub-typing into 'blurk'$_M$ and 'blurk'$_N$ based on the difference in origin between the tokens manipulated by M and N.

In view of all this, I think it can be safely concluded that the theoretical premises of ORG can be deemed viable despite the undesired complications delineated above. Now one last twist. Suppose that the advocate of ORG is not completely satisfied with the liberal view I have been sketching. She is happy with it because it safeguards ORG from objections coming from proponents of synchronic approaches to word typing. However, she also wants to know whether ORG, besides providing a viable typing system, has better chances than structural-functional typing to match the real, epistemically unrestricted catalogue of word types available in the world. Call this question Q. There are three problems with Q. The first is that Q might be otiose. If you are among those who believe that questions about the ontology of words should be asked relative to the fine-grained array of disambiguated notions in which the all-encompassing concept of 'word' is decomposed by linguistic theory (e.g., Di Sciullo and Williams 1987; see Bromberger 2011), chances are you will find Q too underspecified to be worth asking. The second problem is that, as a policy for word-type counting, ORG might be far from fully fleshed-out. What exactly

counts as a first token of a word type? What about the conventionalization of idioms, compounds, and multi-word expressions? What about conversion cases (e.g., the 13th century verb 'close' recycled into a noun in the 14th century)? Is there a single notion of linguistic transmission available to ORG, even if the one introduced by Kripke (1980) and generalized by Kaplan (1990) seems hard to apply to elements like conjunctions, prepositions, and auxiliaries? Maybe we should refrain from asking Q until we are clear about what ORG has to say about these cases. The third problem is that Q presupposes the existence of a fact of the matter about how many word types there are, which might be controversial if you are a fictionalist à la Yablo (2001), an instrumentalist à la Bromberger (1992), if you believe that quantification over linguistic types should not be deemed ontologically committal (Azzouni 2013), or simply take the appeal of the liberal approach illustrated here to indicate that questions about word typing fall outside the business of ontology. But suppose that (no matter how) we address these problems in a way that allows Q to be posed. In such a case, I see three immediate strategies that the advocate of ORG may wish to adopt in the attempt to corroborate the proposition that ORG should be preferred over structural-functional individuation as a contender for ontological adequacy.

The first (somewhat structuralist move) would be to propose that structural-functional individuation can be incorporated into ORG because synchronic-linguistic typing itself is inherently relative to stages of individual or public language systems. Typing via linguistic sameness can only be operated with respect to the state of a given I-language (or of a given public language) at a specific time. The argument could be reinforced by claiming that, in choosing among rival typing strategies, preference should be given to the one whose statements pick up objectively natural properties, in Lewis's (1983) sense, and that because sharing a causal-historical lineage is more objectively natural than expressing the same grammatical features, there are metaphysical considerations favouring ORG over structural-functional individuation (see Rayo 2013: 44–47).

The second would be to argue that appealing to synchronic linguistics as a device for ontological theorizing introduces hidden complications (e.g., Cappelen and Dever 2001: 291–96), and vindicate ORG as the comparatively less controversial account of word types available on the market. Consider the following Gettier-like expansion of the 'blurk' scenario sketched above. Suppose that a native member of M, Leo, is a competent user of 'blurk'$_M$, and that a native member of N, Uri, is a competent user of 'blurk'$_N$. One day, Uri hits his head against a wall and passes out. Upon regaining consciousness, due to a bizarre turn of events, Uri finds himself among members of M. Sadly, the wall incident has impaired Uri's ability to keep track of his location. Noticing that the people around him address him in NENE, Uri forms the occurrent belief that he is among fellow members of N, and starts chatting with a group of M-natives comprising Leo. At some point in the conversation, Leo produces a token of 'blurk'$_M$. Uri readily interprets Leo's utterance of 'blurk'$_M$ as an utterance of 'blurk'$_N$, and forms the corresponding occurrent belief B that Leo has produced a token of 'blurk'$_N$, i.e., a token of a word type which is available for linguistic deployment because of its introduction in NENE by N. Because 'blurk'$_M$ and 'blurk'$_N$ are structural-functional twins, B does not prevent Uri from parsing Leo's utterance correctly and communication succeeds. Even so, B seems downright false, and Uri's interpretation of Leo's 'blurk' seems only accidentally correct. Overall, ORG appears rather well-equipped for the task of reconciling Uri's true beliefs about the linguistic properties of the 'blurk' form manipulated by Leo and the fact that B does not constitute knowledge, as well as for the task of giving a precise account of the causal conditions under which Uri may be said to "know" the grammatical properties of the word used by Leo.

The third would be to reinforce ETW by looking for evidence of its unbuttoned superiority in synchronic predictive power over approaches to word individuation based solely on grammatical role. This could be done by explicitly pushing the analogy with biological categorization (ORG looks very much like a cladistics for lexical types) and claim that, *ceteris paribus*, it is impossible

to manipulate the time of origin of a word type W without causing W to develop differences in intrinsic properties that would prove detectable if one assumed a grammatical approach to word type individuation. As one might argue, varying the time of introduction of W in the language will impact on the material environment in which W is distributed and transmitted, which in turn will affect the evolution of W's intrinsic properties, and hence its heritable grammatical profile.

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