Explaining rigidity*

Abstract

I argue that both the 'rigid' and 'non-rigid' or 'descriptive' interpretations of the reference of nominals have an explanation in the structure or linguistic form of these nominals, hence are conditioned broadly syntactically. This seems desirable under assumptions of a 'transparent' and compositional syntax-semantics mapping. Two ways in which nominal reference is conditioned on syntactic complexity are analyzed. The first is based on a traditional DP (and N-to-D movement) analysis and its plausible semantic effects. The second appeals to namehood as a property of atomic (unstructured) lexical concepts as such. It explains rigidity as the trivial effect of syntactic atomicity, in which case rigidity has nothing specifically to do with either names or grammatical categories. I defend the second proposal, on both theoretical and cross-linguistic grounds. I argue specifically that the basis of human reference is the reference of conceptual atoms, which are as such lexically unspecified for name, noun, count or mass properties. The discussion traces out the effects of this proposal for a long tradition of philosophical argument, where name-reference is standardly explained in externalist (causal) or semantic, rather than internalist and syntactic, terms.

1. Introduction

All natural language expressions may be intuitively described as pairings of sounds with meanings. As far as the meaning of nominals is concerned, it is useful to distinguish at least terminologically between their *conceptual content* and the way in which they *refer* in the context in which they occur: their *mode of reference*, as I shall

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say. Rigid and non-rigid (or 'flexible' or 'descriptive') reference are two such modes; the former is usually associated with names, the latter with definite descriptions.[1] By way of example, *the brightest celestial object regularly seen near the western horizon after sunset* might be Mars. All we need to assume for this to be so is a major cosmic reshuffling. In that circumstance, it would still be true that *Hesperus* would be the very planet it was before, namely Venus, even though Hesperus, in our world, *is* the brightest celestial object regularly seen near the western horizon after sunset. This suggests that while names refer in a way that their referent survives even a major cosmic reshuffling, descriptions do not, being more flexible in their meaning.

Kripke (1980) captured this observation by saying that a name *always* designates the same individual, even when we use it speaking of counterfactual situations. The falsehood of descriptive conditions that a speaker associates with a name's referent is irrelevant for the meaning of the name. In the world of the Olympics, *the greatest of all times* may always be someone else. To say that Maurice Green *might not have been Maurice Green*, but Carl Lewis, on the other hand, is to say something as absurd as that *I might be Maurice Green*, or that *Bush might be Kerry*. Given such examples, the phenomenon of rigidity seems real enough.

Still, come to think of it, might Bush be Kerry? All we need to assume is a deception campaign of major proportions. Nor do I have, in fact, a problem phantasizing me to be Maurice Green, which I factually do when saying *If I was Maurice Green, the world's fastest man would be a philosopher.* But what do these

[1] Although many philosophers since Putnam (1975) have argued that rigidity is a property not specific to names, as it can be found with natural kinds as well, a point to be kept in mind in what follows. (Among natural kinds, not only the familiar *water* or *gold* should be included here, but also garden-variety nouns like *man* or *happiness*: the *metaphysical* status of kind terms, i.e. the question of whether they pick out a *natural* kind or not, should not matter linguistically for an analysis of the rigidity of the reference of kind terms.)

observations show? Do they show that in a world where Kerry is really Bush, the name *Kerry* would have referred to Bush? I do not think there is a clear answer to that question. It entirely depends on how we define the technical term reference. In one rather obvious and plain sense, the answer is yes (it's the same sense in which Hesperus *is* Phosphorus). But in another it is not, for there is still a thing, *this thing that called itself Kerry*, which we then realized, by means of a non-trivial insight, to be in fact *that other thing, which calls itself Bush*. What I want to note here is that, for us, the only way to make sense of such an assertion as the one that *Kerry is Bush* (as we might find it in a newspaper reporting a startling discovery) is to *resort to a definite description*. That is, when we said that *Bush is Kerry* (or one person be another person) was an absurdity, the names in that context worked as rigid designators. When we imagined, on the other hand, as we just did, the rather peculiar fantasy in which Bush is Kerry, it seems clear that our underlying mental representations had the form of definite descriptions, despite an identical surface form of the expression we used. In order not to get confused in the future, I will from now speak of names *only* if a rigid designation is involved, and else of descriptions.[2]

The peculiar scenario we are contemplating does then *not* show that names are really descriptions; on the contrary, it shows that in certain circumstances and given certain communicative purposes, nothing will do *except for* a description. Hence it shows, if we take that fact together with our first observations concerning rigidity, that names and descriptions are strongly non-equivalent linguistic forms, and that a

[2] Hans den Besten (p.c.) notes that an example that can be used to make the same point is (i)-(ii), below:

(i) Hamlet is John.
(ii) John is Hamlet.

In (i), we might be looking at *the person playing Hamlet* on stage and wonder: which person is it, getting (i) as an answer. In this case, *Hamlet* is read descriptively, *John* rigidly. In (ii) it's the subject that is read rigidly, not the nominal that is part of the predicate: roughly (ii) means *John is the person playing Hamlet*.

surface name (word) can function as both. They have each their separate point in language use, and are not equivalent in any sense of the term.

2. The methodology of transparency

This is precisely what we would expect. A name like *Green* seems structurally simple or atomic, while a description *the greatest of all times* is, if anything, syntactically complex and compositionally interpreted (as an existential quantification, I will assume). That should make for a difference in how they are interpreted, and argues again for the implausibility of the suggestion that names (as such) are 'semantically equivalent to descriptions', as it is sometimes said. While that is *logically* possible, it is a form-function mismatch of such giant proportions that we should adopt it only as an option of last resort. More generally, absent good evidence to the contrary, something of the atomic linguistic form

X

should not be able to mean what the non-atomic

the X

does, or that 'names are really descriptions'. On the same grounds, a proper semantic representation of

X

should not be

ιX,

where the iota-operator, as used by Chierchia (1998; and see also Gaertner 2004), is a mapping from a set or the extension of a predicate to an individual, turning a predicate into an expression with a referential import. Iota is effectively defined as equivalent to the definite determiner *the*: the X= ιX (see Chierchia 1998:346). But in the absence of 'the', it is not clear from what in the linguistic form iota should follow. Moreover,

given compositionality, the meaning of 'the X' *presupposes* that of 'X'; hence 'X' itself should not have the form 'ιX', given that 'ιX' means what 'the X' does.

As a final example to which I will return in the end, we should wonder why the meaning of the English word

dog

should be given by appeal to the meaning of the morphologically *more* complex word

dogs,

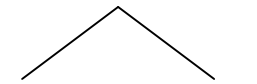
which denotes a set of individuals. Again it seems that the meaning of 'dog+s' *presupposes* that of *dog*, and for this simple reason the meaning of the former should not be invoked in the explanation of the meaning of the latter.[3] If we take linguistic form seriously as a hint towards underlying semantic complexity, this suggestion is a natural one. But it means no less than that the meaning of *dog* should *not* be thought of a set of individuals, or depends on the existence of such a set. On the contrary, to identify a set of individual *dogs*, we need the concept of a *dog*.[4] I return to this issue in the end; let us, for now, simply insist on calling the meaning of *dog* a 'concept' and on assuming that a concept is not the same thing as (or cannot be explained by appeal to) a set of individuals.

The constraint on linguistic theory-building I am suggesting through the illustrations above has a strong empirical bite: it is an instance of Larson and Segal's (1995:78) demand for a 'transparent' syntax-semantics mapping. Transparency (the exact opposite of which is Jackendoff's (2002) 'parallel architecture', on which

[3] Consider Chierchia's (1998:345) following sentence: 'Singular common count nouns like ***dog*** are (characteristic functions) true of individual **dogs**.' (My boldface.) Isn't it at least somewhat strange that to explain the meaning of an item X, one must exploit the meaning of another item, Y, that *contains* X as a proper part?

[4] That of course is merely the oldest of points: as Plato noted, it is not explanatory to explain the content of a concept, *C*, by appeal to things that *fall under* it, things that are *Cs*.

syntax and semantics are two independent generative systems linked by arbitrary 'correspondence rules'), is the strongest hypothesis on the architecture of the language faculty, and thus should be what we begin with, abandoning it only if forced. On this picture, semantic complexity should track syntactic complexity (be 'purely interpretive', as Larson and Segal put it). Consider for illustration the Neo-Davidsonian semantic 'translation' of *John loves Mary*, which, on inspection, involves, apart from an existential quantifier, two logical conjunctions: '∃e (kill(e) & Agent(Brutus, e) & Patient(Caesar, e)'. Transparency forces us to ask is where the extra elements in this semantic representation come from, which are nowhere there, as far as it seems, in the linguistic form. We surely cannot just pretend they are there, as *covert* formatives, in the 'underlying form' of that sentence! Neo-Davidsonians have gladly taken up the challenge, arguing that it is the syntactic form in this instance that bears part the semantic burden: concatenation, or binary branching, translates as predicate conjunction (Pietroski 2002). In essence, the idea is that *kill Caesar*,



means: 'there is an e, of which both *killing* and *Caesar* hold as predicates'.

For the case of names, transparency would mean that if we formally have an atom, X, in a syntactic representation, we should have, formally, an atom in a corresponding semantic representation. A definite description like 'the X', in being syntactically complex, must then be regarded as *posterior* to what exists prior and independent of syntactic processes, namely the lexicon. Getting an expression of the form 'the X' requires a syntactic derivation (the projection of a DP), and idioms aside there is no such thing in the lexicon. If names *are* in the lexicon, as they intuitively seem to be, they *cannot* be structurally complex in the way that descriptions are.

What then could some such philosophical claim as that names are really descriptions mean? It could be a claim about how a name should be formally *represented* in the notation of some formal language designed for purposes of semantic representation. But that would not be particularly interesting, as it would leave entirely open *why* a particular linguistic item should be mapped to that semantic representation, rather than another one. If we wish, not only to *represent* the meanings of expressions, but to *explain* why they have the meanings they do under the constraint of transparency, we should be pointing to the *linguistic form* of the expression, and say it has *that* interpretation because of *that* linguistic form. On this conception, a claim that names are really descriptions is an empirical *structural* claim about the syntax of the expression in question. As such, few philosophers who spoke of the 'semantic equivalence of names and definite descriptions' have defended it, assuming a much looser syntax-semantics interface, on which one syntactic object can be mapped to whatever our semantic intuitions suggest.

Summarizing, if we translate as a rigid designator, the syntax should mirror this too. If we say that *all* surface names are really descriptions (which I haven't said), we say that they *all* contain what all descriptions contain, namely a determiner attached to a noun. This is a syntactic claim and should also be justified on syntactic grounds. Else the only reason we can give for so analyzing the expression is the need to make a certain semantics come out right; and that will leave us in the dark concerning why that semantics should be the one it is. My guiding intuition will be that when something is *inflexible* or *rigid* in its denotation, the reason is that it is, in its form, maximally simple or atomic too.[5] Only something that has *parts* can have

[5] I won't take this to mean that names cannot be internally complex. Obviously, that they are not is not true e.g. of surnames of some Irish and Scottish chiefs of clans:

(i) the O'Donoghue of the Glens

variable parts, and hence *change* (or have a different referent in different worlds). Hence we *need* a variable in the first place, and hence an operator-variable (quantificational) structure, assuming there is no such thing as a variable (trace) in natural language without a transformation that creates it, and an operator that binds it.[6]

I will assume the interpretation of definite descriptions as quantificational DPs, on which the definite determiner denotes a generalized quantifier (see further (16) and (19) below and discussion thereof). In *the king of Moldavia*, *the* expresses a second-order property of the property of being the king of Moldavia: the fact that it applies to exactly one individual.[7] I see no intuitive justification, however, in the claim that definite descriptions so interpreted are thereby not 'referential'. Indeed it

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(ii) the MacNab

What I will deny is only that nominals denoting rigidly (names) can have the status of phrases that are interpreted systematically in line with a standard compositional semantics for complex NPs. Complex names can be like idioms in this respect, which also are somewhere in between phrases and atomic lexical expressions. Note in this connection that the 'saxon genitive' in Dutch is found only after names or namelike entities, as in (iii)-(iv), though not after common nouns that are not namelike, as in (v)-(vi):

(iii) mijn's Opa's fiets
'my grandfather's bicycle'

(iv) Gerard's fiets
'Gerard's bicycle'

(v) *mijn buurman's fiets
'my neighbour's bicycle'

(vi) *het schip's kapitein
'the ship's captain'

However, it is possible after complex names, as in (vii)

(vii) Burgermeester Foortman's voorstellen
'Mayor Foortman's suggestions'

This indicates that complex names indeed are names rather than complex phrases of a standard sort, with a systematic and compositional semantics. Thanks to Ben Shaer and Hans den Besten for discussion of this issue.

[6] I am assuming that this constraint falls out from the fact that in quantification, the variable *forms* in the first place through a process of movement to an A'-position, and could not exist without the latter.

[7] It is the function f from entities to truth values such that there is exactly one x such that f(x)=1.

seems entirely unintuitive to say, of a person talking about the present king of France (or our person above talking about *the person that called itself Kerry*), that it is not talking about anything. Of course she is talking about the king of France (not, certainly, the king of Egypt, or Jacques Chirac). Her speech act is a *referential* one, even though, unluckily, the world it happens to be *placed* in is not such as to provide that act with a referent (throughout, I will assume this distinction between nominal *reference* and *having a referent*). Whether we refer with a term in a language is an *a priori* matter: we can tell, from the linguistic form alone, that a person uttering *the king of Moldovia is sick* is engaged in a referential speech act; whether she picks out any existing object in the world we can only find out about *a posteriori*, and this is not a matter for semantic theory to deal with.

3. Names are predicates

There is a by now familiar observation that all names can take determiners in essentially the same way as nouns, hence can behave as predicates, if this is what nouns are (1a-g):

(1a) The early Russell

(1b) The Russell of 1902

(1c) Yesterday's Tyson was a disappointment

(1d) This Tyson is a sad memory of his former self

(1e) All of Green was invested in this run

(1f) Tysons are rare in the history of sport

(1g) Much Tyson remains to be re-discovered.

Note in particular the possibility of mass-quantifying a name, as in (1g). They also incorporate into nouns which they modify, again manifesting their predicative status (2a-b):

(2a) Russell lovers abhor Wittgenstein.

(2b) Every Napoleon admirer owns a hat.

Tyler Burge in the early seventies concluded on the basis of examples similar to (1) that all names *are* really nouns, or predicates (which is what Quine held as well, or Russell, at a time; see also Elugardo (2002)). Burge in turn accounted for their rigidity, which he assumed as well, through an element that he posited *externally* to them, call it the 'rigidifier'. That was meant to be a demonstrative determiner, which for some reason does not show up overtly. In other words, rigidity is not a feature of names, it is a feature of attached demonstratives, the only 'logically proper names', as Russell put it. In short, the underlying structure of an overtly bare occurrence of *Napoleon* is $[_{DP}$ *this* $[_{NP}$ *Napoleon*$]$.

A major problem with this proposal, as Higginbotham (1988) pointed out, is that the *non-restrictive* readings of names, on which their rigidity depends, arises only in the *absence* of a determiner – at least if this determiner is overt. Thus in all of the cases in (1), what we are talking about is not Russell, Green or Tyson *tout court*, but *a specific* Russell, Green or Tyson, hence individuals *falling under some description* that is explicitly or contextually given. By contrast, a non-restrictive reading arises where the determiner is dropped: cf. (2), above, where the name is read non-restrictively, and (3-4):

(3) Happy Tyson won in five rounds.

(4) Tyson was rediscovered.[8]

[8] This generalization is not true for what we may call the 'affective' determiner, which is a different case, and leads to non-restrictive readings:

(i) That Thatcher was a pain for Britain

(ii) This Tyson is a hell of a boxer.

In these cases the noun clearly does not act as a restriction to the determiner/quantifier. Interestingly, the affective reading is impossible with stage-

Burge might reply to this that he posits *covert* rather than overt determiners, but that naturally raises the question why the semantic effect of an overt determiner should be so diametrically opposed to that of a covert one, and seems to make Burge's proposal irrefutable. In any case, if a name was introduced by an empty determiner, then we would expect it not to incorporate into another noun. But just this happens unproblematically, as we saw in (2).

In short, while there seem to be good reasons to stick to Burge's insight that names are predicates, his explanation of rigidity – a paradigmatically externalist one through its appeal to the direct referentiality of demonstratives – does not work.

Now, there *are* cases, where it seems we *may* assume, this time on purely syntactic or cross-linguistic distributional grounds, empty determiner ($[_{DP} \varnothing [_{NP} N]]$-) structures for surface bare NPs. However, as is well-known, this option is severely restricted, namely to mass nouns and plurals, which are certainly *not* interpreted in the way that names are:

(5) Boys met with girls.

(6) Cats eat mice.

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level predications, as in (iii), which is good only when the determiner is read restrictively:

(iii) That Tyson (here) is fighting like he never did before.

On the other hand, a Tyson-stage picked out by a restrictive DP-construction can be the subject of an individual-level predication:

(iv) This Tyson (here) is a genius.

What explains this asymmetry between affective and non-affective determiners? If the stage-level/individual level distinction has a rationale in scope relations between generalized quantifiers associated to nominals in a clause, as Raposo and Uriagerela argue (Uriagereka 2002), then the affective DP in (i) would obligatorily seem to demand a wide-scope position in LF over the event variable; it acts as a *topic*. So, on Raposo and Uriagereka's Neo-Davidsonian semantics, there is a topical event of *Thatcher* here, such that *being a pain* is true of it. On the other hand, in (iii) there is a fighting, which is topical, and a certain Tyson-stage merely takes part in it (it occurs only as part of that event, or in a context: it is a contextual individual). What (iv) then shows is that a Tyson-stage picked out by a DP/NP-construction can also be de-contextualized and become the topic of an individual-level predication.

(7) I drink wine.

(8) I ate lion.

Thus, the interpretation in (5) is indefinite-existential; (6) and (7) have generic readings; and (8) involves an indefinite-existential quantification over a mass noun: it is interpreted as *I ate an indefinite amount of lion-meat*. If that mass-reading is to give way to an individual-specific count-reading, the determiner must become overt – more syntactic structure must be present – as in (9), where an individual lion is eaten:

(9) I ate a lion.

Now, it is widely held, in the tradition of Stowell (1989), Scabolcsi (1994), or Longobardi (1994), that bare NPs are pure predicates, and are cross-linguistically only licensed in argument-positions if introduced by a determiner that turns them into arguments; in languages that lack article/determiners, like Chinese, this task has been argued to be taken over by noun classifiers (Cheng and Sybesma 1999). The more general idea here is that Ds or classifiers have a 'singularizing' or 'individualizing' function (see also Croft 1994): they allow a given nominal space, the denotation of a head-Noun, to be referred to, either under a *mass* or an *individual* presentation. Once the noun is either 'massified' or 'individualized',[9] a *reference* becomes possible to it, and *quantification* does, hence *definite descriptions*. Also Chomsky (1995:292) refers to D as the 'locus of reference' in the grammar; in a similar way, Szabolcsi spoke of a deictic and 'subordinating' function of D, comparable to the relation between T and VP, or C and IP in the human clause.

[9] I follow Cheng and Sybesma (1999:516, 519-520) in their claim, directed against Chierchia (1998), that Chinese *makes* a mass/count distinction, though this is expressed not at the level of the noun but at the level of the classifier, which speaks against the need for a semantic parametrization (see further Longobardi 2001).

To give some standard examples from Italian providing evidence for the empty determiner analysis, in (10-11) bare NPs occur in a non-argument position, while in (12-13), where they occur in an argument-position, they are not:

(10) Gianni e [$_{NP}$ amico di Maria]

John is friend of Maria'

(11) [$_{NP}$ Amico di Maria] sembra essere Gianni

friend of Maria seems to be John

(12) *Amico di Maria mi ha telefonato

friend of Maria me has phoned

(13) *Ho incontrato amico di Maria ieri

I have met friend of Maria yesterday

Secondly, if empty categories must be properly governed, then the ungrammaticality of

(14) *Aqua viene giu dalla colline

water comes down from the hill

is naturally explained as a proper government violation. Compare by contrast,

(15) Viene giu acqua dalla colline

comes down water from the hill

where proper government is respected.

Let us assume, then, at this point of our inquiry, that Romance surface bare-NPs are really (at least) DPs, and that when the empty determiner is present, it correlates, as noted, either with an indefinite-existential or with a generic interpretation. As Neale (1991:45) argues (cf. also Longobardi 1994:fn.29), in a description like 'the F is G', the former (indefinitely existential) interpretation is the contribution of the empty determiner position as such, while the definiteness aspect of

the article is contributed by this *particular* determiner's specific lexical content. That is, on a conjunctive analysis of 'the F is G', this expression decomposes into (16-17), where (16) is the default contribution by the determiner position:

(16) ∃ x F x

(17) all x which are F are G.

This throws up the question where the existential quantifier in (16) comes from. Chierchia (1998) assumes it to be type-shifter that transforms a predicate into an argument. I do not assume it to be a type-shifter, because I assume that the predicate is born a predicate and stays a predicate; ∃ merely serves to bind off a given variable that, like any other variable, arises through a syntactic transformation. In other words, the indefinite existential reading does not have to be *created* through some special operation; it simply *is* the semantic contribution of the D-head. We are now facing a paradox. To put it in the briefest possible way:

(18) *The paradox of names*:

NPs describe, DPs refer. Names paradigmatically refer. But they are NPs.

For Longobardi (1994), that, together with other observations particularly concerning adjectives, suggested the natural conclusion that names, when non-descriptively interpreted, simply *are* not NPs, but (heads of) DPs. This makes immediate empirical sense of the fact that names can be syntactically found even in positions where languages like Italian forbid all other bare nouns, such as pre-verbal argument positions. For if the name fills the D-head (has moved to it), no lexical government requirement on empty categories would rule such examples out.[10]

[10] Cases like *Il Gianni mi ha telephonato* may be handled quite straightforwardly, by means of an analysis of *Il* as an expletive determiner, and an associated expletive replacement analysis of that determiner at LF.

More importantly for our present project, the fact that names *lack* the kind of interpretation that bare nouns in argument positions generally receive, can *also* be explained under the movement analysis. The head movement from N to D – by *substitution* for D, not *adjunction* to it, as Longobardi (1994:640) crucially assumes – will plausibly have just this, namely rigidity, as its *semantic reflex*. The reason is that the empty determiner-position is then filled, hence the default-existential reading that we have associated with it is overriden. A descriptive reading of the whole DP is not possible because there *is* no head-noun any more in N that could define a range (i.e. a restriction) for a variable bound by a quantifying operator hosted by the D-head, as in the interpretational schema (19), which Longobardi (1994) identifies as one way in which reference may be configured grammatically in natural languages:

(19) 'Denotational' Interpretation

[D [N]]

D x such that x belongs to the class of Ns

That is, N-to-D *movement* gives rise to a *radically different*, namely *referential* interpretational process. No operator-variable structure (A'-chain) determines interpretation, and quantification is blocked. If there is no quantification, no descriptive head-noun is quantified over; hence the referent cannot change; hence the reference cannot be but rigid. Rigidity is not only explained, but in a paradigmatically internalist fashion, the conclusion we wanted to reach.[11]

[11] Chierchia's (1998:399) misrepresents this argument, assuming the proposal to be that 'the semantic character of proper names somehow endows them with a syntactic feature [+r] (something like "rigidly referential") that needs to be checked by raising it to D.' But while it is true that reference is configured in D (in different ways), rigidity is not featural. It is a consequence of syntactic position; and there is no interpretational or semantic property of 'names' to which I assume the syntactic process is sensitive, or which 'drives' the syntactic process (see the next section). Indeed, as I noted above, rigidity is not a property of names only and can affect all nominals. Rigidity is a side-effect of a grammatical process, not a feature. Chierchia

Consider now the transfer of this very same analysis to Chinese, as suggested recently by Cheng and Sybesma (1999), assuming for the moment their proposal, adopted from Tang (1990), that classifiers project full phrases (ClPs) that are isomorphic to DPs in English, both having NP complements. In Chinese, if a bare NP is to have a definite interpretation, this can happen in two ways: either an overt classifier is inserted, which is the Cantonese option; or no classifier is inserted, and then a Longobardi-style movement of N to an *empty* Classifier position takes place, accounting for the definiteness effect; this is the Mandarin option. (20) is a Mandarin example, (21) a Cantonese one (both examples from Cheng and Sybesma 1999):

(20) Gou jintian tebie tinghua
dog today very obedient
'The dog was very obedient today'

(21) Wufei yam jyun *(wun) tong la
Wufei drink-finish CL soup SFP
'Wufei finished drinking the soup'

At this point in Cheng and Sybesma's story, something surprising happens, however. They assume that the meaning of D, hence of Cl, is essentially equivalent to that of Chierchia's (1998) iota-operator that we have encountered in the beginning: both shift semantic type from predicate to individual. In other words, they assume that D or Cl do in the syntax what iota does in the semantics: they create referential arguments, and yield the definite interpretation. This is the basis for the proposal of Chierchia (1998) (taken over in Gaertner 2004), that D and iota can and do *compete* with one

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(ibid.) asks why grammar 'should project syntactically what is already taken care of by something we surely need anyhow, namely the way in which syntactic categories are mapped onto their meaning?' But how exactly a given lexical item such as *Russell* or *Tyson*, as we have seen, are 'mapped to their meaning' is an open issue, and at least in part a consequence of the syntactic context in which they appear!

another, OT-style. If a D is present, the application of iota is blocked. This, of course, is only plausible once a 'global' organization of human grammar is assumed, in which something in the syntax can compete with something in the semantics, to which it has no access in the 'T'-model at work in the Principles and Parameters framework and in Minimalism. On the latter model, iota is not there in a syntactic derivation, being not there even in any human lexicon, hence two derivations, one of which contains a lexical determiner, and the other one of which does not, cannot compete with one another, or rule one another out; if so, the explanation of the definiteness effect in Cantonese that because classifiers are inserted iota cannot be used and be applied to a bare NP, is dubious. But in fact, and independent of the grammar model we assume, it seems iota is not *needed*, since classifiers in Cantonese simply do the work that iota does; and in the case of Mandarin, the explanation of the definiteness effect in bare NPs, that because classifiers are not present, iota can and must operate, it is unclear how iota can be triggered by something like the *absence* of a syntactic formative. Moreover, iota seems here *again* unneeded, since for Mandarin, the authors precisely assume that the head-noun moves into the head-Cl position. But that *is* the derivation of the definiteness effect in the Longobardi-style account that the authors assume, attributing to both D and Cl an 'individualizing' and 'type-shifting' function (pp. 520, 524); hence the iota-operator in the Mandarin case does again no more than duplicate an action that has taken place in the syntax already. While the authors claim (p. 522) that N-to-Cl movement is a 'necessary step' for iota to operate, it makes the operation of iota redundant.[12] This is support for my initial doubts about iota as a technical

[12] Cheng/Sybesma concretely suggest (p. 522) that there are two options: either iota changes a predicate type to the entity/argument type, and then there is a type-mismatch of the item in N and the type it should have when being in N. Therefore N-to-Cl movement takes place. But this to motivate movement semantically, which is a non-standard account of movement, given that movement is usually assumed to be

device in the context of a desire for a transparent syntax-semantics mapping. Iota follows from nothing in the syntactic form, and in the explanations above it seems it need not enter at all.

4. Syntax makes names

Consider now *proper names* interpretations of nouns in Chinese, which occur in demonstrative-classifier combinations, yielding restrictive or predicative readings in such contexts:

(22) Nei-ge Hufei zhen bu xianghua

that-Cl Hufei truly not decent

'that Hufei is really not reasonable!'

This again suggests that names – although movable to D or CL – are like all other nouns in being base-generated in N, and undergo head-raising from there, like other definite bare nouns. Once again, however, the authors (p. 523) follow Chierchia (1998) in suggesting that this movement, if it happens, is *semantically conditioned*, by a 'type-mismatch' between the name generated in N, namely <e>, the type of individuals, and the type of entities that should be in N, namely <e, t>. In other words, movement is necessary, to shift the type.

This account implies that it is an intrinsic property of names that they *are* names; it is that property that is meant to drive the syntactic derivation. But this seems theoretically questionable. A name is plainly an N, hence if it is not a predicate, which nouns by their nature are, why is it base-generated in N in the first place? And isn't the idea that a *semantic* fact might set the syntactic machine into motion, a bit like the idea of telling one's body to get sick, if one doesn't want to give a talk, say? As we all

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driven by the need to check morphological features. *Or*, they suggest, N moves to Cl because otherwise iota cannot operate; but this is again to motivate movement semantically. And it is precisely because Cl does the type-shifting work that iota is supposed to do, iota seems unneeded.

know, this usually won't work. Syntactic processes do not run for a reason, and they do not run just because a nice semantic result would ensue if they did. So much autonomy of syntax, I think, we should uphold.

While these are only theoretical considerations, there are empirical ones as well. It is demonstrably only a matter of *convention* when a common noun can be used as a name, and when not. That is to say, it has nothing to do with the workings of the language system *as such*. Thus it is an arbitrary convention in German, which could have been otherwise, that the noun *Wolf* (meaning wolf) can be used as a personal proper name, while the noun *Hund* (dog) cannot. That the operations of the syntax are not only independent of such facts but ignore them is shown by the fact that it can override them. Thus if convention dictated the semantic interpretation determined by a linguistic form, then (23)

(23) Dog came in

would be ill-formed, in the way that **dog came in* is. But it in fact *is* well-formed under the interpretation where a person called 'dog' came in, hence the nominal acts as a proper name rather than as a descriptive condition. In fact the example *forces* that interpretation, as no other is available. This would be explained under the assumption that proper names move to Cl/D: for it would be because of that move in (23) that the N loses its descriptive content and refers rigidly.[13] In other words, we haven't got a proper name here, base-generated in N, that waits to be promoted on the ground that it is, semantically, a proper name, and a type-mismatch occurs. No, it is because of the promotion to D that it *is*, or rather *becomes*, a proper name. Proper names are the

[13] We should and need not expect that it will lose its descriptive content *entirely* – as has happened with German *wolf*, used as a proper name – since grammatical form determines linguistic interpretation only modulo convention: knowledge about the latter may interfere with grammatically conditioned facts and create interaction effects.

creatures of the syntax, which despite all conventions that there may be, can rule what to make a proper name and what not.[14]

In short, if we ask why it should be that some nouns *can* move to D, while some others cannot, I would suggest that the computational system of language has no access to such information as whether a noun is used as a proper name. This matter is a matter only of how interface representations are used by outside interpretive systems. Borer (2004) reaches a similar conclusion, offering the example (24), from Hebrew:

(24) Ze'eb radap axrey ha.yeled

'Ze'ev chased after the boy.'

(not: a wolf chased after the boy, although *Ze'eb* denotes both 'wolf' and is a proper name).

Similarly, consider (25) (taken from Cheng and Sybesma, p. 523), from Cantonese, where bare nouns in sentence-initial position yield a proper name interpretation,

(25) Sin-saang mou lei

teacher not-have come

' Teacher/*the teacher did not come.'

or (26), from Mandarin, where the same is true:

(26) Linju bu lai le

neighbour not come SFP

'Neighbour won't come any more.'

Cheng and Sybesma here comment that 'some common nouns' (like *neighbour*, *teacher*) 'can' also refer rigidly to individuals, whereas the right conclusion seems to be that, modulo convention, *all* can. Very paradoxically, but interestingly in the light

[14] The conventionality of the name-noun distinction is supported by eponyms, i.e. names that have become nouns historically (e.g. *Caesar* in Latin).

of my previous discussion, they also suggest that N-to-Cl movement in such cases is 'without the ι operator' (p. 523), which is effectively to concede, given that we unquestionably have a normal noun here in the head-N position, that the iota operator is unneeded for the referentiality, confirming my earlier point that it is a descriptive device that simply denotes the result of a process that takes place in the syntax through a transformation already. I see my above reasoning confirmed here that the Noun-movement is *not* driven by 'the nature of proper names instead of the ι operator' (ibid.), since there *are* no proper names in the numeration underlying the derivation of (25), and iota seems redundant for reasons I have argued for above.

5. The conceptual content of names

We have now committed us to the view that modulo convention, there are no names in the lexicon strictly speaking. Without relevant transformations taking place in the syntax, there are no names but only nouns. Since we are not appealing to beliefs, world knowledge, pragmatics, or reference at all to explain rigidity, our explanation is internalist. Or rather: what is purely internalist is the explanation of the *mode of reference* that names have. For as I said in the very beginning, names are characterized by two things: their pure conceptual content, on the one hand, and their mode of reference, on the other. What remains to be explained, if this distinctive mode of reference is explained (and if we accepted the explanation, as I myself ultimately will not, despite its attractiveness), is the pure conceptual content that names have. Perhaps the explanation of *that* might force an externalist ingredient in our explanation of namehood.

I do not think that this is the case. Note that the mode of reference as well as the actual referent in an act of language use of a name may *change*, while the *meaning* or conceptul content of the name does not thereby change, too. Specifically, as

illustrated in (1), we can use the name *Tyson* to refer to different individual Tysons on different occasions, call those Tyson-stages, or even to Tyson-masses. But we can do so only if we *have* the lexical item *Tyson* with its idiosyncratic sound and meaning in the first place. Different Tyson-stages are *different*, and Tyson-masses are different from individual Tyson-stages. Thus there *must* be a sense in which the latter's *lexical* meaning is *independent* of both its actual referent on an occasion and the mode of reference of the referential act into which it enters. We are forced to say that as a *lexical* item, both the referent of *Tyson* and its mode of reference are *not yet settled*: they depend on a particular syntactic configuration into which they enter. For example, if in the course of the derivation, *Tyson* gets plural morphology and surfaces as a bare NP, it is bound to be interpreted either as kind referring (as in (1f)), or indefinitely existentially, as in *While being in Vegas, I met Tysons all over the place*.

Now, what *is* that same Tyson-thing, that pure lexical content that stays the same as the reference and mode of reference changes with contexts of use? Can we characterize the content that *Tyson* has as a purely lexical concept more specifically? Now, it is perfectly obvious to most philosophers that the meaning of that lexical item resides in a *relation* that this word has to *a particular external object*: Mike Tyson. Many philosophers hold that this relation is a *causal* relation and is *all there is* to the meaning of a name. But this analysis won't do. For *that thing*, the material object Mike Tyson, which we just mentioned, was clearly given in our mental apperception as a concrete individual in flesh and blood now, hence *under a description*. And Tyson does not *need* to be so given. It can lose that descriptive feature, while staying the same thing. To push this to an extreme, a religious boxing aficionado could say:

(27) Tyson deceased yesterday and now lives in heaven.[15]

You may find what (27) states to be implausibly the case, either because you do not believe in disembodied phases of existence, or because you think Tyson would not be in heaven if he underwent such phases, but this is irrelevant. We are not talking about what you think or find plausible, but what your concepts and the conceptual intuitions triggered by them allow you to conceive. Note in this regard that it is one of the oldest and most persistent philosophical intuitions that it is not (logically or conceptually) *necessary*, for any person, to have a body (though physically, very likely though with absolute certainty, it is). The point the dualist has always been pressing is, not that we *are* in fact disembodied minds, but that it is not *conceptually necessary* for us to be embodied. If that conceptual truth holds, as I think it does, it is out of the question to *stipulate* that the meaning/conceptual content of the name 'Tyson' or for that matter of the personal pronoun 'I', as uttered by myself, consists in a reference to a material object or body. It is interesting to speculate whether the opposite conclusion, that name-referents are external material bodies, is triggered by an inexplicit and maybe unreflected-upon empiricist and externalist bias in philosophical epistemology, which as such might be argued should not enter the analysis of linguistic meaning as such.[16]

I do not say here that we do *not* use the word 'Tyson' to refer to a certain physical object living in the US, but that we do it, if we do so, *on the basis of* a lexical concept, which is the *same* when we use it to refer to a disembodied Tyson-mind, or a Tyson-mass, hence is not *defined* in terms of any of the particular referents (stages,

[15] Many philosophers would say at this point that 'Tyson' refers in this sentence to what it does refer now (in the actual world). I have no such intuitions, and would say simply that it refers to Tyson the person here (which is not necessarily embodied, as the example shows). I return to this problem below.

[16] I think it is very interesting to note that much of modern semantics was not so much linguistically driven than driven by particular epistemological concerns, but I won't go into that.

masses) it may take on an occasion. The opposing position is that the individual (as opposed to the individual-stage, mass, or disembodied) reference is coded into the very lexical meaning of the word. But this, I hold, is a matter of belief, not meaning.[17] What argues directly for this is that we may *lose* the belief, while the meaning, for all we can tell, stays the same, say when we utter (27).

Note that no human language, as far as I know, morphologically marks the difference that existence makes. Whether some entity really exists, or exists in a

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[17] Collapsing belief and meaning remains a pervasive feature of philosophical discussions on names. Consider Braun and Saul's (2002) 'resistance to substitution in simple sentences' puzzle, that (i) seems true to many, while (ii) seems wrong, and should lead to the falsehood of (i) too, given the identity of Superman and Kent:

(i) Superman leaps more tall buildings than Clark Kent

(ii) Superman leaps more tall buildings than Superman.

It strikes me that the explanation of this puzzle is entirely straightforward, from a linguistic point of view. Thus there is a constraint operative in the human linguistic system according to which if there are two referential expressions in one clause, they are by default interpreted as not referring to the same thing (reference is obviative). The mechanism is a dump one: two nominals, two interpretations. This constraint exerts its influence even if the formative used for both nominals is the same: thus (iii)

(iii) John killed John

strikingly cannot mean that John killed himself, an interpretation for which the system uses a new and special formative, *-self*; if (iii) can be interpreted at all, it is interpreted so there are two different persons both called John. This is even true in a language like Africaans, where there *is* an option for a co-referential interpretation in the quite different case of 'appelative' nominal forms interpreted as second person (data courtesy Hans den Besten, p.c.):

(iv) Oom moet Oom gedra
uncle must uncle behave
'you have to behave yourself'

We see the same influence in our interpretation of (ii), which sounds 'strange' to us, as we would predict, and is not relevantly different in this respect from (iii). Like (iii), (ii) also and explainably puzzles us, being slightly deviant: it makes us look for different referents where the formatives involved indicate there is only one, which is what puzzles us. There *are* examples where this severe constraint can be overriden, as in small clause constructions like (v), in which the two nominals do refer to the same thing:

(v) We called [$_{\text{Small Clause}}$ him John].

But these are rather special cases. In short, the first judgement is unproblematic and expected. The second is explained as an interface effect: the language system interfaces with others, notably human belief systems. Thus, while the former system demands two referents in (iii), the latter comes in by telling us that both referents are the same. This is a contingent truth, the former, that the two referents are distinct, is an analytic one, following from the rules and workings of the linguistic system itself.

particular form, according to current physics or common world knowledge (say, whether water is H_2O or XYZ, or maybe a visual illusion), is a posteriori information that the linguistic system does not know about. This rightly predicts that there is no evidence that the linguistic system computes compositional semantic interpretations for sentences containing *Hamlet* in any different way than it computes such interpretations for expressions containing *Tyson*. As Longobardi (1994:fn. 32, p. 638) notes, wide-scope (de re) and rigidity effects for names for fictions and for actually existing individuals are the same. If it turned out, contrary to received wisdom, that Hamlet actually *was* a historically real entity, and that, by contrast, Tyson *never* existed, nothing in language would change. The next day we would find two headlines in the newspaper, *Tyson proved a fiction,* or *Hamlet proved to really have lived*, whose very understanding by us depends on the names occurring in them to mean exactly what they do now. A specification as part of the lexical meaning of the relevant names according to which Tyson is embodied and Hamlet is not and was not, would positively hinder the way language is actually and compositionally understood in such counterfactual cases.

In conclusion, in explaining namehood, or rigidity, the difference that existence makes, makes *no* difference. Our materialist belief that Tyson reduces to an actual individual in flesh and blood should not enter into the lexical meaning of *Tyson*. It should not, because embodiment is no constraint on this item to function in language as it does and to mean what it does. The analysis of the pure conceptual content of Tyson, viewed apart from its mode of reference on an occasion, does not force an externalist conclusion either.

6. Some doubts

This brings me to my final suggestion, that despite one crucial element in it which I think is correct, the N-to-D-movement style analysis of proper names, defended above and found in various ways in Longobardi (1994, 2001), Cheng and Sybesma (1999) and Borer (2004), is in fact not accurate. There are some genuine empirical complications with it. First, as Juan Uriagereka notes (in personal communciation), it seems not safe to assume that argument-NPs must be DPs, a crucial presupposition to the above analysis. Thus,

(28) I hunt partriges every fall,

appears to mean the same as

(29) I go partrige-hunting every fall,

where the NP has incorporated into the verb. But if *partrige* is a DP in (28), this incorporation should be impossible, on the assumption that DPs don't incorporate. This doubt about the arguments-as-DP analysis does not mean that D is not the 'locus of referentiality', or a 'subordinator', as proposed above, but only that some syntactic arguments can be purely conceptual arguments.

The second complication is that the above N-to-D proposal tells us virtually nothing about how to handle cases mentioned initially in this paper, like (30-31) (cf. Uriagereka 2002: Chapter 12):

(30) If I was Maurice Green, the world's fastest man would be a philosopher.

(31) This Tyson is a sad memory of his former self.

The problem is this. It seems clear that reference in such cases is non-rigid: In the latter case, a particular Tyson is referred to, say the one I am just witnessing in a fight, and it does not follow that Tyson *as such*, or on the individual-level, *is a sad memory of his former self*. In (30), as I have pointed out, that such a phantasy would break down the very moment reference *was* rigid here. What such cases demand however is

not the impossible thing that I be someone else, but that relevant *parts* or aspects of me can be swapped for relevant parts of another person: what we are creating here, in the words of Uriagereka (2002), is a *chimera*, which is composed of some aspects of me – in particular, my being a philosopher – and some aspects of Green – in particular, his running power. Note, however, that while these observations speak *against* rigidity in such cases, at the same time they positively *demand* it. The paraphrase

(32) If some relevant parts of me were swapped with some relevant parts of Green

as such suggests this: for this phantasy is only coherent if, no matter all the swapping of parts of me and him that takes place, I remain I and Green remains Green. Else, as I pointed out, an absurdity ensues, which it clearly does not. Put differently, although we are not talking about me (or Green) simpliciter, but *parts* of me, we are still talking about relevant parts-of-*me*, where the embedded *me* is me simpliciter, a rigidly referring me. In short, no matter how we syntactically construct complex modes of presentations of individuals, partition them and quantify over their parts, as in (32), there is a sense in which *rigidity is preserved*, and apparently just because the same lexical concept occurs in our derivation, whatever structure we derive in it. That raises, despite all we have said, the doubt that *rigidity cannot be the effect of a syntactic process* after all; and that, rather, it is what *survives* the syntactic process. Since what survives the syntactic process unscathed is in effect the *lexical items* it contains, namehood should be a matter of the lexicon, after all. A Longobardi-style head movement analysis seems powerless to explain the rigidity effect as manifest in such examples.

7. Rigidity is the lack of classification

My suggestion is to preserve one crucial ingredient of the previous analysis, while breaking with others. The Longobardi-style idea I want to keep is that namehood resides in the *absence* of a DP-internal operator-variable or quantificational structure, and the absence of a *partitioning* of a given nominal space taken from the lexicon for purposes of quantification. This is perfectly sound in that if I refer to myself *non-rigidly*, as in *some relevant parts of me* in (32), or as in the song *All of me*, the structural resources I use are *more* rather than less: in *rigidly* referring to me, I just use *I*. Rigidity should reside in little or no structural complexity, and descriptiveness/quantification should consist in the building up of such complexity, in particular the transformations associated with quantification.

What we *drop*, on the other hand, is the requirement that NPs in argument positions are quantificational by default, with namehood and rigidity arising from *overriding* this default. Instead, rigidity is an inherent property of the name *itself*, as a lexical item. What the formation of DP-internal quantificational structure does is to *override rigidity* by allowing a richer and more flexible mode of reference. To implement these pieces of a theory, compare (25), repeated here,

(25) Sin-saang mou lei

teacher not-have come

‘ Teacher/*the teacher did not come.’

with (33):

(33) Go sin-saang mou lei.

CL teacher not-have come

‘The teacher (*Teacher) didn’t come.’

(25), *lacking* an overt classifier, yields a proper name interpretation; (33), *having* an overt classifier, crucially forbids it. *In classifier languages like Chinese, a proper*

name interpretation and classifiers are inherently inimical. What follows from this observation, that *names do not take classifiers*, and are interpreted *restrictively*, if they do? Cheng and Sybesma (1999:524) comment on (33) as follows:

'We attribute this to the individualizing function of classifiers. The [Cl+N] phrase with an overt classifier yields an interpretation in which the classifier picks out a particular instance of the kind denoted by the common noun. This yields the definite interpretation and not a proper name interpretation.'

But then it follows analytically that *prior* to the attachment of the classifier to the noun, it is *not yet* individualized! It is precisely, in my initial terms above, a *pure or bare concept*, not classified for individuality or mass, hence not (yet) quantifiable. If we take this together with the observation that proper names do not take classifiers, we arrive at the striking conclusion that nouns that end up acting as names, or as denoting rigidly, as such do *not* denote individuals. Before we get mental access to a person's individuality, its individual presentation rather than mass presentation must be computed in the course of the derivation, through the attachment of an appropriate classifier. As soon as we quantify a name, however, a restrictive reading arises, and namehood is lost. This does again not mean that names are not predicates. It rather entails that predicates as such can be rigid, an assumption that, as noted, philosophers have argued to be right on independent grounds.

This conclusion now makes sense of our intitial methodological considerations: that to mentally represent individuals and sets of them, we *presuppose* what is in my terms a concept, hence do not explain it, something the syntax of these mental representations mirrors by making a word denoting a set of individuals more

complex than a word denoting a concept, and making a construction denoting one individual more complex than a construction denoting a mass (hence, as noted, in English, *empty* determiners give us mass readings (cf. (7-8)), but to get count readings, there must be an *overt* determiner (cf. (9)).[18] If we wish to take seriously that countable individuals require a lexical determiner, but masses not, masses should not depend on individuals. They don't on the account now defended, where nouns taken from the lexicon are simply concepts, and are as such neither masses nor countable. As observed in the previous section, names in particular, as pure concepts, should not be specified for either mass or count, as they can be both.

This then is explanation of rigidity that I want to defend. The explanation is that something that has no parts, stages or instances that one can count and over which one can quantify, cannot refer other than rigidly. It is nothing that could change from world to world. We have rigidity only if we *don't* attach a classifier to the semantic space denoted by a nominal. The classifier, by introducing a partition into a nominal space over which we can then quantify, makes rigidity impossible. Once we *have* parts/stages, these parts can be referred to, such as the stage referred to in *this Tyson-stage is a disappointment*, and they can be swapped for others, creating chimeras. Rigidity, by contrast, is the trivial effect of syntactic atomicity. We have drawn this conclusion by departing from Cheng and Sybesma's quote above, which does not I

[18] That conclusion is directly inconsistent with Chierchia's proposal on Chinese nouns, according to which they are mass nouns, their semantics is given in terms of individuals, and they are of the type <e>. But Cheng and Sybesma (1999:519-520) provide evidence that Chinese makes a count/mass distinction. They do this at the classifier level, which immediately suggests that individuality does not exist yet at the level of the noun. Chinese nouns can also be interpreted as bearing number specifications, despite their lack of plural marking, which makes it problematic to interpret them uniformly as masses. Theoretical considerations in Borer (2004: Chapter 3) also argue against Chierchia's proposal regarding a semantic parameter distinguishing Chinese noun interpretation from the English one. See also Longobardi (2001).

think support what they otherwise defend, namely rigidity as an effect of N-do-Cl movement.[19]

8. English

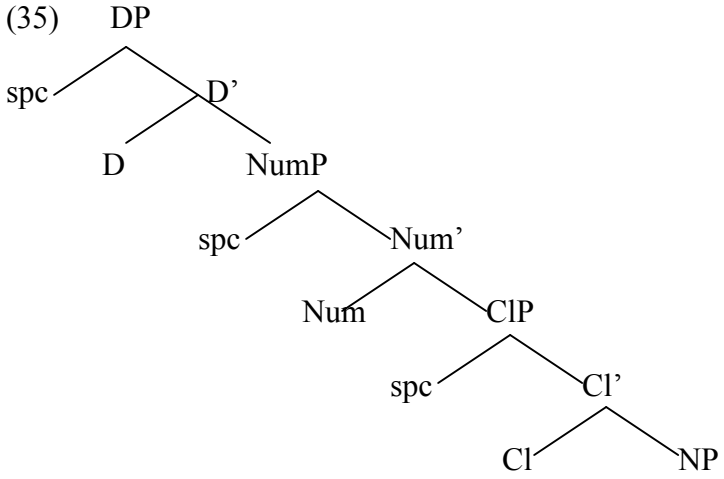
That leaves an obvious question: what we do with English, which does not have classifiers. As we noted, Cheng and Sybesma argue classifiers to be functionally equivalent to determiners, and English of course has determiners. But this would not seem right in the light of Tang's (1990) view on the structure of a DP like (34), namely (35):

(34) Zhe san ben shu

DEM three CL book

'These three units of (the kind) book'

(35)



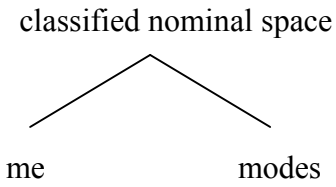
On this analysis, we get a definite description including a quantification over 'units' of the nominal space 'book', only if we have, over and above a classifier that enables quantification in the first place, also a quantifier in the numeration, hosted by the D-position. This raises the possibility that the analogon in English to Chinese 'individualizing' classifiers are not determiners, but *covert* classifiers that exist at the level of the noun, rather than at the level of the syntax, as in Chinese. Though

[19] In fact, on their account it is not quite clear how rigidity would follow at all; cf. their p. 523.

phonetically covert, they do show up here and there however, even in English, in the form of measure phrases, say, or the partitive syntax that we used in our paraphrases above, e.g. *all-parts-of-me*.

I will adopt this suggestion for English, following Uriagereka (2002: Chapter 12). I assume, then, that when we quantify a nominal such as *Tyson* in (1d), what we quantify is a *classified nominal space*, in which a in and of itself rigid Tyson-space is partitioned in a number of temporary and individual Tyson-stages. Thus the underlying structure of a DP like *this Tyson* is like that of 'this *stage-of*-Tyson', where what we quantify over is one particular *individual presentation* of the underlying Tyson-space. Equally, when I quantify me, as in *all of me*, there is a classified nominal space, a space partitioned into me-modes (modes of presentations/parts of me):

(36)



At the level of the lexicon, on the other hand, I am there just as a pure concept, and have no parts or presentations to speak of, parts and presentations that could change from context to context, or from world to world, precluding rigidity.

9. Conclusions

Needless to say, this second explanation of the rigidity effect is strongly internalist and syntactic as well. But it is in one sense diametrically opposed to the previous explanation, on which rigidity is an LF-effect, a semantic consequence of a syntactic operation internal to the nominal domain. On the present account, rigidity is a property of pure concepts, or pure conceptual contents, as we find them in the lexicon.

Rigidity shows up at LF if, and only if, a given nominal space taken from the lexicon, after entering the derivation, is not classified. Thus understood, the association of rigidity with names is not an intrinsic one; rigidity cross-cuts categorial distinctions, and the name/noun distinction is as such spurious as well, as modulo convention, all names can act as nouns and vice versa. If we like, we may say that names are distinct from nouns in being 'tagged' in the lexicon as names; which does not exclude them to act as noun in a suitable syntactic context (thus, *Tyson* may either act as a noun true of people all called 'Tyson', or as a different noun true of different stages of the one-and-only Tyson).

Dispensing with such a distinction, I have suggested that nouns are lexically underspecified for name, count, or mass properties. Under strict compositionality, the reference of complex expressions must be built from from that of its maximally simple or atomic parts: at the heart of human reference lies that of non-structured human concepts. While conceptual atomism is of course controversial, particularly because it strongly invites nativist conclusions, *any* account will have to assume primitives at some point, as long as we are talking about a combinatorial system building up complex meanings from simple ones (Fodor 1998). Our concepts of persons, I contend, function as primitives in this sense; they are not analyzable in terms of – *are not* – a number of other concepts, in some combination.[20]

In general, at the abstract level of atoms, we in particular do not know already whether something will act as a noun (*wolf*, say) or a name (*Wolf*), which we will

[20] Thus, *I* am certainly not analyzable as my body or any part of it, as far as natural language goes: we individuate my body differently from me (e.g., I may lose a leg, while not changing as a person; and for all I can tell I might be the well-known brain-in-the-vat, hence not even have the body I think I have); nor do we say that I am my brain: rather I *have* a brain. As what of all things should I be analyzable, or be decomposible into? Each person as a person is only circularly identifiable (i.e. by referring to the person it is).

know only from the way the system of language gets used on an occasion. When we are at the pristine level of atoms, we cannot quantify yet, quantification over variables being a necessary ingredient in descriptions whose reference can vary from world to world. Before we can quantify, we must be able to count; and before we can count individual presentations of some nominal space, we need a mass, a sequence of complexity orders that is itself suggested by the way in which the linguistic system builds up mass and individual presentations of what remains *conceptually* the same thing. I have suggested these are presentations a given nominal space may have, and which need to be structurally fabricated, though this is not to say that a given item listed in the lexicon may not be conventionally associated with a noun or a mass-reading.

Rather large philosophical consequences lurk in the background now, one of which is a strong doubt induced with regard to the 'causal theory of reference' (see further AUTHOR 2004). For Kripke, rigidity of course pointed in a radically externalist direction, in fact the causal theory of reference. In this tradition, referring rigidly to things like Hamlet or Pegasus is a non-issue. But this conclusion, I have argued, is based on the confusion, possibly based on a specific epistemological and metaphysical agenda, of the way in which the linguistic system fabricates linguistic meaning in a partial autonomy with the way in which it interacts with other systems in the mind.

On the Kripkean conception, it is a fact about Gödel, this external object, that he might not have been another person, hence would not be someone else if we found that he did not do what, if anything, we think he did, namely discover the Incompleteness Theorems. But now we should ask: how can this be? This external thing, Gödel, clearly does not as such tell us about how we will regard it to change or

not in possible circumstances. For all that its *physical properties*, as an external object, tell us and allow us to conclude, he could as well be a *different* thing, if, say, his hair was combed differently (as Chomsky 1991 points out). That he is *not* relevantly different if this happens, and stays, as we think, *the person* he was (though not the physical object he was), depends on *us* looking at this thing under the *perspective* that our human *concept* of a person affords. If we looked at it as an *entity*, or a *physical object*, he *would* be relevantly different. Hence it is not properties of the external object – or causal relations between it and us – which tell us when it changes and when it is the same.[21] It is our *concepts* that trigger our judgements and acts of reference on particular occasions. As Hume noted, personal identity is not a matter of physical appearance, and, and as I have argued, it is not a matter of physicality at all.[22]

If anything we see here how and why the fascinating interplay between themes from linguistic theory, psychology, and philosophy of mind, which has always characterized the theory of names, should and will no doubt continue. What is novel in this paper from a philosophical point of view is the fact that the interpretation of names as well as our very theoretical conception of namehood – traditionally strongholds of externalist views of language – must be regarded as having surprising internalist ingredients.

10. References

[21] This is a somewhat 'Kopernican' turn in the theory of names: not our names revolve around the world and change together with it, but the world changes according to what our names suggest.

[22] In a philosophical mood, we could attempt to go further here and speculate whether there is a giant 'supervenience' failure as regards human concepts and the physical world, in that physical properties of objects do not determine the properties of our concepts. No doubt, if you had a conversation with a Martian using the word *pling* to talk about some thing, X, and told him everything there was to be known about X in physical terms (particles, vibrating strings in empty space, electromagnetic fields, etc.), he need not grasp that X, denoted by *pling*, was what is known to us as *books*. But this is required by standard models of reductive explanation: once all the physical facts are in, the concept must be determined. As the example suggests, it is not.

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