Symposium on *Language: A Biological Mode*
by Ruth Millikan

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What (socio-)biology Tells to Psychology about Language

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1 Introduction

One of the most prominent features of the scientific study of language in the past fifty years has been its intimate connection with psychological issues. Starting from Noam Chomsky's attack on behaviorism and from his contribution to the cognitive turn in psychology, linguistics notoriously has played a significant role in the elaboration of the cognitive science paradigm. It may, therefore, seem bizarre to raise the question of how far a non-psychologically-oriented analysis of language can reach. Yet, in her new book Ruth Millikan appears to have somehow explored that issue – this is how we might intend her speaking of "a biological model" of language. And the results are quite challenging. The readers of her previous works will recognize a number of old themes, spread along the different articles that have become the chapters of the book. Still, there is a deep unity running throughout the chapters: all of them shed some light on one facet or another of what we might call Millikan's "non-psychological stance". This does not mean that Millikan rejects any psychological explanation of language – of course, this is not the case. But it is a fact that her analysis stems from a couple of non-psychological notions, that in fact fix the framework in which psychological explanations too must be accommodated. As a result of this original approach, new light is shed on some old issues, though not without possible contradictions with the intuitions underlying more cognitively-oriented accounts.

Here, I will firstly contrast Millikan's biological perspective on language with the more standard Chomskyan approach, in particular with the notion of Universal Grammar. This will suffice to make apparent how a non-psychological notion, such as that of public language, might have important consequences for psycho-linguistic issues, such as that of the boundary between acquired and innate linguistic patterns, or between syntax and lexicon. Then I will touch upon three themes emerging from the commentaries published here, each showing how Millikan's work challenges psychologically-oriented thought. The themes are: the notion of convention (Tomasello's comment), "theory of mind" and pragmatics (Origgi's comment), and the nature of concepts (Lalumera's comment). For each of them, I suggest, this tension with psychological explanations might have different reasons. In part, there are substantial disagreements. In part, as Millikan herself maintains in her replies to the comments, there is some (reciprocal?) misunderstanding. In part, it could be a matter of division of labor. But, to be true, which interpretation is the right one on any given issue is often not so easy to discern.

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[*] Millikan (2005).

2 Millikan and Chomsky

To begin with, Millikan's position about Chomsky testifies to a sort of delicate equilibrium between at least two different possible interpretations: is it a division of labor, or a substantial disagreement? A whole chapter of the book, "In Defense of Public Language", is devoted to the discussion of Chomsky's hostility to the notion of public language. Millikan doesn't reject his arguments for that conclusion; she rather offers what she judges to be a better characterization of public language, in fact a characterization that in her opinion would be coherent with a large part of Chomsky's theoretical apparatus. In particular, the notion of Universal Grammar (UG) is explicitly maintained to be preserved by her reassessment of what a public language is – whilst, on the contrary, the thesis that language is not for communication should be given up. However, we had better keep in mind that the notion of UG has undergone considerable modifications throughout the evolution of the Chomskyan paradigm; and some of these modifications are perhaps more dramatic than it has been admitted so far.

2.1 A Recent Debate

This is apparent, for instance, in the recent debate between Hauser, Chomsky and Fitch on one side, and Pinker and Jackendoff on the other. Hauser et al. (2002) draw a distinction between the faculty of language in the broad sense and in the narrow sense, and propose the hypothesis that the second includes only recursion, which therefore would be "the only uniquely human component of the faculty of language". This looks as a substantial change if compared with Chomsky's earlier position. As Pinker and Jackendoff (2005:204) put it:

> the position that very little is special to language, and that the special bits are minor modifications of other cognitive processes, is one that Chomsky's strongest critics have counterposed to his for years. Not surprisingly, many have viewed the *Science* paper as a major recantation.

In general, Pinker and Jackendoff argue that Hauser *et al.*'s paper relies heavily on the theoretical framework called the "Minimalist Program". This framework has been developed by Chomsky in order to meet a requirement of simplification in the theory of grammar. It should be noted, however, that such a simplification brings with it a rather different picture of the boundary between syntax and lexicon, between innate and acquired features, and between what does fall within the domain of linguistics as a science and what doesn't.

The Minimalist turn has been somehow heralded by a couple of works which have explored the contribution of the lexicon to the construction of phrases and sentences. These works were located at the extreme periphery of the Chomskyan framework: taking for granted the X-bar theory, which specifies very schematically how grammatical categories fit in a phrase, it was suggested that lexical entries could carry all the remaining information required for constructing any particular phrase. This implies that a huge amount of regularities in the distribution of words have to be accounted for by lexical devices rather than strictly syntactic ones.

Moreover, this implies in turn that, for each particular language, a large part of its grammar has to be learned: the lexicon is something every child apprehends from her linguistic community. Then, approaches deriving from the Chomskyan framework such as Pollard and Sag's "Head-driven Phrase Structure Grammar" (Pollard and Sag 1994) end up resembling to explicitly anti-Chomskyan approaches such as the construction-based accounts of Goldberg (1995) and others (Fillmore et al., 1988; Tomasello 2003). In both approaches, lexicon is not conceived as wholly idiosyncratic, in straight opposition to the pure regularity of syntax. On the contrary, it has become usual to talk of a lexicon-syntax continuum, implying (at least) two different things. First, a large amount of syntactic regularities emerge from use, just as lexicon does. Second, there are gradients of regularity varying along a continuum, that is, syntactic norms can be more or less general, and rarely (if ever) they are

completely general: as the study of idioms has revealed, each syntactic norm can have a different degree of generalization, with pure idioms and lexical meaning being just a particular case at one extreme of the continuum. These sorts of considerations force Chomsky to reappraise the notion of Universal Grammar. In fact, Universal Grammar is now often equated with a set of (rather general) filters imposed on language learning, thus acknowledging a large amount of syntactic features which must be apprehended: innate filters constrain, but do not specify, the huge number of lexically-governed syntactic regularities. In the Minimalist Program, just as in Hauser et al. (2005), Universal Grammar seems to become still narrower, being roughly reduced to recursion alone.

There is however a third aspect to be considered. It concerns the issue of what does fall within the domain of linguistics as a science. This is made clear by Jackendoff and Pinker (2005:219), when they observe that the Minimalist Program does not reject a central thesis of classical Generative Grammar: the assumption that language faculty in the strict sense has to be identified with "an abstract core of computational operations". Grammar would then be precisely such a computational core, with lexical entries providing only the materials to which this core applies. Now, in criticizing the rigid demarcation between grammar and lexicon, Jackendoff and Pinker appeal to the existence of a continuum between pure regularity and idiomaticity in order to cast doubts on the underlying distinction between syntactic *procedures* and lexical *data*, as well: how can local, partially idiomatic syntactic regularities be processed, if syntactic procedures apply universally across language? However, in so doing Jackendoff and Pinker also reject the idea that language has a "core" and a "periphery" at all. In their view language does not have a grammatical core, but rather a variety of different levels or tiers, each with its evolutionary history to be told, and each having equal rights to be investigated by linguistics as a science. Then, though these authors' view may appear closer to classical Generative Grammar than the Minimalist Program as to the extent of the contribution of innate factors to language – Jackendoff and Pinker in fact assume it is wider than recursion alone –, they depart from classical Generative Grammar more radically than the Minimalist Program in rejecting the thesis of a grammatical core of language.

2.2 Individual Psychology and Public Language

Do these considerations have any relevance to Millikan's book? They do indeed, as far as I can see. To begin with, this book provides us with a robust and enlightening exploration of one way in which language faculty relies on biological factors, over and above the hypothesized innate filters on language acquisition. As a consequence, even if we admitted with Chomsky that only the biological side of language allows for a scientific explanation, this would in no way entail that linguistics as a science may concern nothing but Universal Grammar. However, in this respect Millikan's approach partly diverges from that of Pinker and Jackendoff. These authors emphasize that other biological factors, besides recursion, constrain the acquisition of linguistic forms. In other words, they are essentially concerned with Universal Grammar, that is, with the faculty of language as an issue of individual psychology – though they maintain that that faculty is neither just recursion as in Hauser et al. (2005), nor just *grammar* in the strict sense of the word. On the other hand, Millikan is primarily concerned with public language conceived – so to say – as a socio-biological fact:

> if you are interested in individual psychology, public language is merely a stimulus to transition from the initial state of the language faculty S_o to a more steady state S_s. If you are interested in public language forms, on the other hand, the language faculty is merely how public language forms reproduce themselves. A public language is interesting in its own right, I will argue, because it has certain functions that are all its own (Millikan 2005: 39).

This makes clear to what extent Millikan's perspective is different from, though not necessarily

in opposition to, Pinker & Jackendoff's.[1] However, she shares with them the conviction – *contra* Chomsky – that there is more to language as a scientific topic than just the alleged grammatical core. The passage quoted above is followed by the conclusion that "the study of the functions of public language is a separate discipline, independent of the study of individual psychology". Here a division of labor between individual psychology and the study of public language is proposed, which is in itself a major divergence from Chomsky. This is confirmed by Millikan's assumption that language "is for" communication. Chomsky's resolute rejection of that thesis makes sense only assuming a notion of language wholly deprived of its public dimension. If, on the contrary, one admits "public language forms" as a legitimate subject matter for the scientific analysis of language, then it is hardly surprising that they are credited with the function of communication.

Besides the rebuttal of the "computational core" thesis, there is another point that Millikan's position shares with Pinker and Jackendoff's: the attention to idioms as evidence that language shows variable degrees of (ir)regularity. Millikan (2005: 38) declares to be "impressed – overwhelmed? – by the huge number of idiosyncrasies and idiomatic elements in any natural language". She goes on to say: "Perhaps whether 'a language' is best idealized as a monolithic structure, or as a relatively loose texture of interlocking crisscrossing conventions, is only a matter of what you are interested in." But, again, this concession suggests a division of labor that could hardly be accepted by Chomsky. The second "idealization" focuses on the existence of linguistic conventions with different degrees of generality. Millikan maintains that not only single words but also grammatical forms are conventional in this sense (e.g., Millikan 2005: 35). This, however, blurs the distinction between lexicon and syntax, just as suggested by Pinker and Jackendoff, and by cognitive linguistics. Then Millikan's perspective conflicts with Chomsky's both because he wouldn't concede that linguistic conventions have any room in a scientific study of language, and because he wouldn't assign any significant theoretical role to the "idiosyncrasies" and idiomaticity exhibited by those conventions.

With regard to the question of nativism, Millikan suggests that innate features in grammar are a very limited amount, a position that is nearer to the Minimalist Program than to classical Generative Grammar. Millikan's view depends on her notion of "linguistic convention":

> To become conventional, an activity or pattern of activity must, first, be reproduced, hence proliferated […]. Further, it must be proliferated due in part to the weight of tradition (Millikan 2005: 30).

In principle, this sort of definition allows to draw a neat line between linguistic conventions and the innate aspects of grammar: "If there are certain aspects of grammar that are never reproduced at all, but always resupplied by the language module *de novo* […they] are not conventional aspects of grammars" (Millikan 2005: 35). Millikan notes that focusing on linguistic conventions as she does leads to a picture that challenges the notion of language as "a system of rules and principles", at least

> if 'system' implies much systematicity. To view the young child's language faculty as a filter through which language conventions are to be transmitted is to view it not as aiming toward some steady state as the child matures, to view it not, for example, as a process of acquiring permanent parameter settings, but as a faculty engaged in the accumulation of a larger and larger repertoire of conventional patterns it can recognize and produce on demand (Millikan 2005: 37).

In other words, given that syntax appears to be as much conventional and variable as lexicon, its acquisition could hardly be conceived as a case of setting a set of innate parameters.

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1 On closer inspection, Pinker and Jackendoff do not significantly disagree with Millikan's position on UG: they maintain that recursion is not all there is to Universal Grammar, but they reject any account in which innate grammar inflates too much, as in parameter setting theory.

This, as far as the comparison with Chomsky, and with Pinker and Jackendoff, is concerned. I would just like to add a little note on the kind of biological perspective Millikan adopts. In Chomsky's account, biology appears to be what guarantees a deep uniformity beneath the surface of languages. In Millikan's position, what seems significant is the way she insists, instead, on biological *variability*, even in comparisons such as the following:

> But the idea that there is at the center of each such language [German, French, etc.] some univocal 'system of rules and principles' seems as unlikely as that members of an animal species should be genotipically identical or that the whole gene pool should contain no incompatible genes (Millikan 2005: 38).

More importantly, according to Millikan linguistic conventions are to be conceived on the model of the biological notion of species: her definition of "convention" depends crucially on the way patterns of activity are transmitted through reproduction. This emerges clearly in the following passage, which should be read as applying to linguistic forms in general (rather than to "word types and their elements" alone):

> word types and their elements, phonological segments and letters, are like species. In biology, what makes a dog a dog is, in the first instance, that it was born of a dog, not that it has some particular shape. Similarly, what makes a shape or sound into a token of a particular word on this way of reckoning is its lineage, what it was reproduced from, on what prior word tokens it was modeled (Millikan 2005: 34).

Now, this perspective reveals to what extent pattern reproduction is permeated by historical contingency:

> a reproduced pattern […] is not a conventional pattern unless it is one that would have no particular reason to emerge again, rather than some alternative pattern, if once forgotten […].Conventional patterns are exemplified rather than other patterns owing only to historical accident, but having occurred, they cause their own recurrence (Millikan 2005: 31-32).

This insistence on the role of historical contingency in the domain of biology is in line with the major current reflections on evolutionary processes. The works of Stephen Jay Gould, in particular, have importantly contributed to advertise the idea that biological history is, after all, genuine history. This idea is crucially involved in Millikan's own attempt to conceive of historical conventions, such as those involved in language use, as biological categories. Therefore, the difference from Chomsky's reference to biology couldn't be more evident.

3 Tomasello: the Notion of Convention

In his comment, Tomasello makes two main criticisms to Millikan's book, both concerning her use of the term "convention": according to him, Millikan's use of the term is too broad. In particular he criticizes her application of the concept to the following two cases: counterpart reproduction, and the hearer's cooperative response to "conventional directive uses of language such as paradigm uses of the imperative" (Millikan 2005: 152). In fact, as Millikan emphasizes in her reply, these two objections can be considered as two sides of the same coin. By "counterpart reproduction" she intends that an individual adopts a behavior not as a consequence of imitation, but rather as an adaptation to the behavior of others. Now, in Millikan's view the role of the hearer in linguistic interactions is itself a case of counterpart reproduction, and the hearer's compliance with the speaker's demands expressed by directives is but an instance of that general role. Then, it is not surprising that Tomasello adopts one and the same line of reasoning with reference both to counterpart reproduction and to cooperative compliance. His argument is that since the behaviors in

point are not passed on by imitation, they cannot be considered conventional.

Millikan agrees with Tomasello that hearers do not learn to play their role by copying, and that there could possibly be other factors "buried in evolutionary history"[2] that explain cooperative compliance. But she doesn't think those evolutionary considerations count as an argument against her view: they would rather complement it, specifying a mechanism through which that sort of counterpart reproduction can be accomplished. But then what about the fact that in counterpart reproduction cooperative responses are not handed down by imitation? Note that Millikan herself considers the transmission by imitation as a constitutive feature of what a convention is: she explicitly claims that learning a certain behavior "by operant conditioning or other forms of trial and error" cannot count as a proper case of reproduction, in the sense required for having a conventional behavior. This notwithstanding, she accepts counterpart reproduction as a source of conventional behavior, no matter which psychological mechanism is involved: the subject "might learn how to fit in by trial and error, or maybe in more reflective ways, it doesn't matter".

As far as I can see, there is a major reason for this position. Millikan's analysis of conventions aims to preserve an important point of David Lewis's picture: the idea that conventional behavior can emerge spontaneously as an answer to coordination problems, every time people have common purposes that can be reached only through a combined action. Unlike Lewis, though, Millikan insists on the possibly unconscious character of this reciprocal attunement. With the standard example of driving, a person "might unconsciously learn to conform to the convention of driving on a given side of the road solely as a skill" (Millikan 2005: 148). However, the point is that a coordination convention like that cannot emerge unless there is at some point someone who behaves in certain ways in response to others' behavior. With regard to speaker-hearer interaction, Millikan observes that "if hearers never complied with imperatives, speakers would soon cease to issue them" (Millikan 2005: 58). Then, we have a kind of conventions whose existence depend on someone's responses to certain behaviors, where those responses are not copied in general. Precisely, they cannot be copied when the convention gets established – there is nothing to be copied yet – , and possibly they are not even copied on further occasions. One can learn to drive on the proper side of the road by just aiming to avoid others, by explicit instruction, and so on. Or as another example, in compliance with imperatives there might be evolutionary dispositions at work. What does matter is that, in any case, were the response not there, the convention could not have been established, too. So, it seems correct to consider counterpart reproduction as (part of) the way in which certain conventions are established.

Why, then, does Tomasello disagree? We should keep in mind that Millikan's and Tomasello's concerns are quite different. Tomasello is mainly interested in what makes cognitive organisms such as human beings capable of cultural behaviors. According to him, what really makes humans different from other primates is not being smarter in producing innovations, but rather being better at fixing them, thanks to more sophisticated imitation abilities. These are supposed to represent the major turning point in the recent biological history of human cognition. In this light, it should be clear why Tomasello chooses to apply the notion of convention only to behaviors reproduced by imitation: his main concern is to investigate precisely that sort of behaviors, and the cognitive mechanisms they involve. The notion of counterpart reproduction refers instead to behaviors which, not being transmitted by imitation, are irrelevant for the analysis of the cognitive mechanisms that made us specifically designed for culture. Therefore, it makes sense to exclude them from Tomasello's cognitive account of what (cultural) conventions are.

On the other hand, Millikan is mainly concerned with conventions as socio-biological entities. Recall, for instance, her vindication of the notion of public language, maintained to be in itself a legitimate subject matter for scientific enquiry. The fact that Millikan's notion of convention is socially (rather than psychologically, or cognitively) oriented is apparent in the case of coordination conventions. These are constituted by patterns of behavior involving actions and responses,

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2   From now on, when no reference is given quotations refer to Millikan's replies.

independently from the specific cognitive mechanisms which deliver those responses. As social facts, coordination conventions exhibit specific features that Millikan's notion of counterpart reproduction helps us to describe.

4 Origgi: "Theory of Mind" and Pragmatics

It should be noted, however, that this way of framing the divergence between Millikan's and Tomasello's perspective is rather different from the one Millikan herself has proposed. She writes:

> A genuine disagreement here may be that I am interested in everyday purposes that could motivate the hearer in an ordinary way, not a purpose (perhaps opaque to the hearer?) somehow buried in evolutionary history.

I think this statement is somehow misguiding. For one thing, Tomasello's appeal to dispositions "buried in evolutionary history" does not preclude an interest in everyday mechanisms "that could motivate the hearer in an ordinary way". On the contrary, such dispositions are thought to explain precisely why the hearer, on occasion, reacts the way she does. On the other hand, the "purposes" that Millikan is mainly interested in are not, strictly speaking, what "could motivate the hearer in an ordinary way". Precisely because of her socio-biological approach, Millikan is not much interested in the particular purposes of subjects in actual situations. Let us take the example of imperatives: we can imagine a huge variety of idiosyncratic purposes a subject might have in performing imperatives on particular occasions, but this fact does not change the general function of imperatives, which is pretty conventional – it is the function that is responsible for the survival of the convention.

In general, Millikan's analysis of conventional behaviors is characterized by a proclivity to abstract from the details of the psychological mechanisms involved. Firstly, psychological mechanisms are not her main concern. Secondly, a convention has some stability of its own, whilst the cognitive mechanisms involved can vary significantly. (Though, we will come back to the variability issue later.) However, this does not mean that Millikan has nothing to say about the subject. On the contrary, one of her worries in Millikan (2005) (but also in her other publications, from Millikan (1984) onwards) is the refutation of the widespread idea that pragmatic processes occurring in language understanding are instances of high-level cognitive processes. This could be considered as another testimony of her attention to the variability of psychological processes: besides explicit inferences, she emphasizes, there are other, less-demanding, mechanisms that might account for pragmatic phenomena.

Here we have another point of possible disagreement between Millikan and Tomasello, but also between Millikan and Origgi. Both Tomasello and Origgi seem to think that Millikan underestimates the importance of a particular human cognitive feature, that is, "the capacity of reading other people's intentions" (Origgi), or else "shared intentionality" (Tomasello). However, this criticism could be misleading, for two reasons. First, as far as Origgi's comment is concerned, it shouldn't be forgotten that Millikan's occasional lack of interest in specific psychological mechanisms proceeds from the motivation we have already pointed out: the socio-biologically oriented nature of her analysis, and the variability of the psychological processes involved. Second, Millikan does acknowledge the role of recognizing other person's purposes in pragmatic processes: in Millikan (2005), chapter 10, she proposes her view on the issue. To be true, in her account little room is left for the notion of "theory of mind", in particular for the assumption that understanding language requires a propositional theory of how the mind works – how beliefs, desires, intentions and so on explain our behavior. According to her, given that plausibly people form such a theory of mind long after spoken language has been acquired, we had better think of some simpler way of recognizing one another's purposes – maybe one shared with "many animals, perhaps most mammals":

> A simple form that this can take is understanding purposive behavior to be 'goal-directed'; that is, as part of a flexible pattern with a strong tendency to produce a given effect regardless of interfering circumstances. (Millikan 2005: 206)

There is a problem, though, in this perspective: if we assume that mind-reading ability is what mainly explains the emergence of language and culture in the human species, it follows that this ability has to be thought of as a species-specific one. Tomasello and Origgi show a clear interest in the issue of human cognitive species-specificity; consequently they have to be more exigent as to the notion of mind-reading adopted. However, one could plausibly argue that mind-reading is an ability that comes in degrees: the level required by language understanding and culture could be lower than a full-fledged, propositional theory of mind (as Millikan suggests), and nonetheless it could be higher than that exhibited by other mammals (as Origgi and Tomasello presumably would require).

However, I don't think that these considerations would completely remove Origgi's dissatisfaction with Millikan's account. Two issues at least remain to be addressed. The first concerns pragmatic abilities. Even after having made clear that Millikan's acknowledges some mind-reading abilities, there remain to consider her statements about the quasi-perceptual nature of language understanding, and the idea that, at least in many cases, context is enough to identify the "reproductive family a particular linguistic token comes from" (in Origgi's words). This seems in line with some recent approaches – such as Coulson's (2000) – in which a crucial role is played by patterns recognition. Understanding the speaker intentions, at least in some cases, would amount to recognize patterns of linguistic and extra-linguistic behavior, thanks to both linguistic and contextual clues: in other words, it would be a case of cognitive pattern completion. This perspective is attractive in that it would license a unified cognitive account of the different tiers of linguistic capacities: for example, in the construction-based approach to syntax the general hypothesis is that syntactic forms, just as lexical forms, are represented by mental frames induced from experience. Isn't this view at odds with the importance currently attributed to mind-reading in pragmatic processes, and in general with the evidence of creativity in comprehension? This is not necessarily so. Let me make three rather sketchy remarks. First, pattern recognition is a probabilistic, though not exactly a creative, process; that is why Millikan can claim that even "semantically conveyed information is *never* simply 'decoded'" (Millikan 2005: 201). Second, more to the point, a frame-based account of pragmatics can in fact accommodate the evidence of creativity in understanding processes, thanks to the notion of "conceptual blending" (Fauconnier and Turner 2002) – again, for instance, along the lines of Coulson (2000). Patterns recognition is but a first step. Once the cognitive system has activated the supposedly relevant frames, it generally performs upon them conceptual blending procedures so as to construct new frames.[3] Third, a frame-based account is not necessarily in competition with a mind-reading-based account. The importance of grasping the others' intentions for language understanding is something no one wants to deny; however, the nature of the mechanisms involved can be debated. For instance, we should consider the possibility that some mental frames represent purposive behaviors, and that only by finally coming across frames of this sort a genuine pragmatic comprehension can be accomplished.

5 Lalumera: the Nature of Concepts

There is another, more general, point of dispute between Millikan and Origgi to be considered. Origgi charges Millikan's account with a behavioristic flavor; even more than that, she reproaches Millikan's frequent appeal to bee dances in order to understand human language. These criticisms can partly be answered by a previously made observation. Millikan's socio-biological approach

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3   Please note that this is but a *sketchy* remark. In particular, I'm not considering the possible feedback of conceptual blending on the attribution of relevance to stored frames, so as to even modify the results of pattern recognition processes.

drives her to abstract away from a number of psychological details. In so doing, she can pick up some important features that human language and bee dances have in common: their being reproduced because of their function, the "counterpart reproduction" effect, their arbitrariness, and their "intentionality" – that is, their capacity to refer to something else. This does not necessarily prevent her from acknowledging distinctive features of human language, too – such as the role of mind-reading abilities. Moreover, we should not interpret as a concession to behaviorism Millikan's claim that only linguistic forms which benefit both speakers and hearers are reproduced. That is not meant to suggest that language is individually learned by conditioning; rather, the point is that linguistic forms – no matter how they have been learned – do not survive for long unless they are beneficial for language users.

But, again, it is unlikely that these considerations may dissolve any substantial disagreement. What Origgi's observations point to in general is the risk that Millikan's non-psychological notions are driving her analysis of psychological issues in the wrong direction. A crucial point of dispute is suggested by Origgi's charge of "extreme externalism"; this leads us to the issue of the nature of concepts, that is the main topic of Lalumera's comment (the issue has been addressed by Millikan (2005) in chapters 6 and 7). In line with strong externalist approaches, Millikan maintains that our cognitive conceptions of referents do not determine substance boundaries, nor concepts' individuation. According to her, "what is philosophically important about conceptions is that they are in principle fallible, and not definitional" (Lalumera's comment). Conceptions in fact support our ability "of identifying [an object] through different encounters, and of projecting some of its invariant properties from one encounter to the next" (idem), but they can do this only provisionally: conceptions can change dramatically, while remaining the conceptions of the same referent.

The nature of concepts is the object of a very large debate, which couldn't be exhaustively tackled here. I would like to make just two brief observations.

Firstly, Millikan's position threatens to cause a clash between "the philosophical and the psychological notion of concept", as Lalumera observes: the psychologists' standard view of concepts take them to be nothing else but our cognitive conceptions of referents, that is precisely what Millikan denies. In her comment, Lalumera suggests a weaker realist (if not externalist) account of concepts, that she hopes could do the work of Millikan's account without colliding with psychological intuitions. In this light, she asks whether Millikan really intends concepts and conceptions to be two different entities – more precisely, whether "concepts *are* ways of identifying substances, or rather they *involve* ways of identifying them". Lalumera appears to prefer the first answer, which would bring back the notion of concept within a more psychologically satisfactory perspective. I would rather pose a slightly different question: couldn't Millikan here just draw a boundary between her socio-biological approach and psychological concerns, and acknowledge that two different notions of concept are at play?

This point can be made by considering the notion of deference, which has been so crucial in the externalism/internalism debate. One of the arguments for externalism has been that people do not consider their conceptions as the ultimate criteria for determining what a concept does refer to: they rather defer to experts, or even to future discoveries, that could amend their current opinions on the referents at issue. Now, this observation is certainly correct, but it does not change the fact that people do use what they currently know about the referent in order to (re)identify it – simply, what they know can change. Even a person who does defer to experts has to know something in order to be able to refer: hadn't she any knowledge, she couldn't refer at all. In a cognitive explanation of how subjects actually refer, what matters is this relation between the subject's knowledge and acts of reference. On the other hand, as we have made clear, Millikan starts from a social rather than an individual perspective. Just as, in general, conventions are social facts which are not changed by improper uses of them, so Millikan's notion of reference aims to abstract away from the particularities of individual behaviors. In this perspective, what does matter are not specific acts of reference – how individual knowledge determines what subjects refer to actually -; rather, what

does matter is what in the long run is (socially known to be) the right reference.[4] Accordingly, while psychologists are interested in a cognitive notion of concept that is apt to account for individual acts of reference, Millikan tailors her notion of concept on the socio-normative notion of reference. I cannot persuade myself that one of these two notions could be reduced to the other.

The second point concerns the use Millikan does of the "psychological variability argument" in the context of her defense of externalism. In fact, her main argument for externalism is that only reference is public, that is, stable across individuals, whilst conceptions vary wildly. Consequently, "within a linguistic community there need not be shared definitional criteria associated with each substance term or concept" (Lalumera's comment).[5] By the way, the thesis of representations' variability has been at the heart of Frege's anti-psychologism. Millikan, however, uses that thesis for a quite different purpose than Frege did: she abandons any idea of a stable Fregean sense, instead of searching a metaphysical source for it. In this sense, her position is psychologically wholly realistic. In fact, psychologists would certainly agree that there is no publicity of conceptions in a strong, metaphysical, sense: conceptions may vary between subjects, and within the same subject from one moment to another. But this does not preclude, *per se*, that inter- and intra-subjective stability of conceptions – even if partial – is large, and that it may have an important role in explaining our behaviors. In Lalumera's words:

> One thing is to deny that concepts of substances are associated with conceptions or components that all competent users *must* possess (Millikan 2005: 69). Quite another thing is to deny the non-modal claim, that concepts of many substances are associated with conceptions that *do* remain robustly similar across users […]. Still, [Millikan] seems to consider concept publicity as an accident rather than the norm, discarding the non-modal claim as well as the modal one.

It seems as though Millikan thought she needed to reject even the weaker, non-modal, claim of conceptions' stability – and therefore to collide with psychology – if she wants to establish her socio-normative notion of concept. I'm not convinced that this is the case. As I tried to show, there is a possible division of labor between Millikan's and the psychologists' approach to concepts. Therefore, Millikan might concede to psychologists their notion of concepts – and even the large stability they usually suppose them to have – and still have *her* notion of concept.[6]

Moreover, I tend to think that, from Frege up to the present day, philosophical thought has strongly overestimated the variability of our representations/conceptions. It is not just that the world presents us with a large network of regularities. It is not just that our cognitive systems are largely similar. We should also keep in mind that our cognitive systems are designed so as to search and

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4   Obviously, as far as the notion of reference is concerned, socio-normative considerations are tied also to metaphysical ones. I am not going to address this issue here.

5   Millikan's notion of substance covers a number of both natural and cultural objects. See Lalumera's comment for a clarification of this notion.

6   Millikan (personal communication) has directed my attention to the following lines in her reply to Lalumera's comment: "Also, using Lalumera's words, 'concepts of many substances are associated with conceptions that *do* remain robustly similar across users', at least across normal adult users. The fact that we don't generally share *exactly* the same conceptions seldom causes problems". This quotation seems to show that Millikan explicitly agrees with the non-modal claim of conceptions' stability, contrary to what Lalumera and I do argue. Rather, her worry would be that we must keep apart concepts (in her sense) and conceptions – the latter being what the psychologists are generally concerned with when they speak of concepts. However, it is not so obvious that such a distinction would entail "a strong conflict with contemporary psychologists […] on *how we know* how to apply a substance concept" – as Millikan claims further on in her reply to Lalumera's comment. In Millikan's words, psychologists are not concerned with the question of how *concepts* apply; rather, they are concerned with the question of how *conceptions* (*concepts* in *their* sense) apply. But then, Millikan's insistence on a conflict with contemporary psychologists is perhaps misleading: as a matter of fact, that insistence may lead to think that she wants to reject their theory of what a "concept" is (including their confidence in the stability of conceptions), and not simply their use of the term "concept".

detect regularities, so much so that they discriminate between regularities that are (more or less) general and others that are (more or less) local. Consider, in this light, the fact that we have both a semantic and an episodic memory. It is not as though all the information that impinge on us were mixed up in one and the same mental bag. Rather, our minds appear to organize the information in a way that is sensitive to how much general it is. Add to this another factor, that is, the characteristic attention humans place on what conspecifics are attending to, which is part of our (possibly low-level) mind-reading abilities. This factor could promote the capacity to detect – and give a special role to – information that is shared with conspecifics. In a social species as we are, the individual's conceptual systems could be largely oriented by what anyone assume is (probably) known by others.

For all these reasons, it might be the case that the conceptions we have of many kinds of objects are "robustly similar across users" – as Lalumera writes. If this is true, Millikan had better not rely too much on conceptions' variability. But I think she doesn't need to. Her notion of concept does not depend on this argument.

References

Coulson, S.
2000   *Semantic Leaps: Frame-Shifting and Conceptual Blending in Meaning Construction.* Cambridge: Cambridge University Press.

Fauconnier, G. and M. Turner
2002   *The way we think.* New York: Basic Books.

Fillmore, C.J., P. Kay and M.K. O'Connor
1988   "Regularity and idiomaticity in grammatical constructions: the case of *let alone*". *Language* 64: 501-538.

Goldberg, A.E.
1995   *Constructions: A Construction Grammar Approach to Argument Structure*. Chicago: University of Chicago Press.
Hauser, M.D., N. Chomsky and W.T. Fitch
2002   "The Faculty of Language: What Is It, Who Has It and How Did It Evolve?". *Science* 298: 1569-1579.
Jackendoff, R. and S. Pinker
2005   "The Nature of the Language Faculty and its Implications for Evolution of Language". *Cognition* 97: 211-225.

Millikan, R.G.
1984   *Language, Thought, and Other Biological Categories.* Cambridge MA: MIT Press.

2005   *Language: A Biological Model.* Oxford: Claredon Press.

Pinker, S. and R. Jackendoff
2005   "The faculty of language: what's special about it?". *Cognition* 95: 201-236.

Pollard, C. and I.A. Sag
1994   *Head-driven Phrase Structure Grammar.* Stanford: Stanford University Press.

Tomasello, M.
2003   *Constructing a Language: A Usage-Based Theory of Language Acquisition.* Cambridge MA
        and London: Harvard University Press.

Précis of *Language: A Biological Model*

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Because the content of the various chapters of *Language: A Biological Model* is somewhat diverse, I'll briefly summarize the whole first, and then explain the various themes chapter by chapter.[1] (Some of the chapters were new or heavily revised, while some others were merely reprinted.)

Guiding the work of most linguists and philosophers of language today is the assumption that language is governed by rules. I present a different view of the partial regularities that language displays and of the way these compose norms and conventions. This view has implications for a variety of issues in the study of language.

The view is that the central norms of language are like the norms of function and behavior that account for the survival and proliferation of biological species. Specific linguistic forms survive and are reproduced together with cooperative hearer responses because, enough of the time, these patterns of production and response benefit both speakers and hearers. The norm for a linguistic form is for it to perform the function that has been accounting for its survival and to do this in the historically usual way. The norms applying to language are, then, non-evaluative.

I claim that what needs to be reproduced for an information-bearing language to survive is not specific conceptual rules or inference patterns, but mainly patterns in truth and satisfaction conditions. The specific psychological processes, ways of recognizing, evidence relied on for application, that support our uses of proper names, of words for kinds, properties and so forth, need not be uniform from person to person for satisfaction conditions to remain uniform. An implication is that we must reject conceptual analysis, as it is traditionally conceived, as a basic tool in philosophy.

I give a new description of the distinction between propositional content and force. Force has to do with function, with what a language form does. On the one hand, "what it does" is what has caused it to survive in the language; on the other, "what it does" is what it does on a particular occasion in accordance with a particular speaker's perhaps idiosyncratic purpose, or what the speaker purposes (explicit intention is not usually involved) to do with it. Propositional content has to do, instead, with what the form needs to correspond to or end up corresponding to, *if* it needs to correspond to anything, in order to perform its function in the usual way. I also give a new description of illocutionary acts.

I argue that the process involved in understanding language is best modeled as a form of direct perception of the world. Typically, language understanding is parallel, for example, to perception mediated by the natural signs contained in structured light. For the most part, no thoughts of speaker's intentions are required for a thoroughly pragmatic understanding of natural language use. In line with this, I propose a description of how children learn language without having to have a theory of mind.

That is the gist of the whole. Now I'll summarize chapter by chapter.

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1 Millikan (2005).

1 Language Conventions Made Simple

The conventionality of natural language is captured in much simpler terms than David Lewis's (1969), displaying its continuity with more rudimentary conventions involving neither coordinations, regular conformity (either *de facto* or *de jure*) nor rational underpinnings. This "natural conventionality" is composed of two simple characteristics: (1) natural conventions are reproduced patterns (2) they are proliferated due partly to weight of precedent, rather than due, for example, to their intrinsically superior capacity to perform certain functions. I discuss these two characteristics as they characterize simple non-coordinating conventions (decorating for Christmas with red and green), then simple coordination conventions (driving on the right, Lewis's telephone convention) and finally language conventions. Language conventions involve both speaker and hearer, starting with a lead by the speaker, then completed by a hearer who follows through with a belief or an action. Language conventions are not regularities in use. In particular, hearer's often fail to complete the conventional patterns. They don't believe what they are told; they don't do as directed. I argue that the conception of conventions that seems to have been universally adopted in speech act theory (and, incidentally, that accounts for Davidson's problems in "A Nice Derangement of Epitaphs" – Davidson 1986) is mistaken. I point toward a new way to understand the nature of illocutionary acts, one which is followed out later in chapter 8.

2 In Defense of Public Language

Chomsky has claimed that both common sense and technical notions of public language or "externalized language" are confused, ill-defined, or of no scientific interest. And Chomsky claims that there is no sense in which the function of language is communication. This chapter agrees with Chomsky's criticisms of various traditional notions of public language. But given the right understanding of language conventions and the right understanding of the functions of public language, public language is the raison d'etre and the foundation on which Chomsky's "I-language" is built and maintained, and the function of public language is clearly communication. If there is a UG, it fits in here perfectly, as a filter determining how children will understand hence pass on the conventions. "Public language" is described as a mass noun. We need not individuate the "languages" that Chomsky so objects to, or not without imprecision and some arbitrariness. The mass that is language is a sprawling mass of overlapping and crisscrossing lineages of linguistic convention, some known to some people and some to others. Language communities consist of people who know very many of the same conventions. I put emphasis on the enormous number of brute memory idioms and other irregularities found in languages that make it so difficult for adult to join new language communities without sometimes faltering over the local linguistic customs.

3 Meaning, Meaning and Meaning

To understand how language works, one must look, first, to the cooperative functions that various language forms perform, understanding these on a biological model as what these forms accomplish that keeps them in circulation. Linguistic cooperative functions are called "stabilizing functions". Stabilizing functions are functions that have been of interest both to speakers and to hearers on enough occasions that a practice has been sustained in which speakers sometimes use the forms for that purpose and hearers sometimes respond to the forms in the required way. The "sometimes" is very important. Language forms are often used for non-stabilizing purposes and hearers often refuse or fail to follow through.

   Second, we should look at language mechanics, at *how* language forms perform their functions, and especially to the conditions in the world that are necessary to support their specific functions. These are, in part, truth or satisfaction conditions, which are determined by a kind of "meaning" that I call "semantic mapping functions" --"functions", this time, in the mathematical sense. I argue for a

distinction between merely describing "truth conditions" and describing "semantic mapping functions". Representations that have identical truth conditions can have different semantic mapping functions because they show different kinds of contrasts with other representations within their own representational systems. It is also possible for representations with the same truth conditions to have different stabilizing functions, and this casts light on some traditional problems such as the informativeness of identity sentences, sentences asserting existence and sentences of the form "'X' means Y".

Third we need to describe the psychological mechanisms that are involved in implementing the functions of various language forms. We must describe the ways that people manage to recognize the states of affairs that correspond to the satisfaction conditions of sentences in the public language so as to use the language in the conventional way. These mechanisms are learned ways of recognizing objects, properties and so forth. People typically possess very many different ways of recognizing any given familiar object or property, through perception and through description, different ways often being known to different users of the corresponding word, *none of these being definitional of it*. Roughly speaking, reference is all that goes public. (How kind terms work in this connection is explained in chapter 6.) I call the ways an individual has of identifying the reference of a word their "conception" corresponding to it. I use the theory of conceptions in showing how to eliminate a need to introduce anything akin to intensions or Fregean senses in the understanding of linguistic meaning. This part is just a sketch. The main work had been done in prior essays to which I refer.

4 The Son and the Daughter: On Sellars, Brandom and Millikan

I compare my position with Robert Brandom's with respect to common origins in the works of Wilfrid Sellars and Wittgenstein. I take seriously the "picturing" themes from Sellars and early Wittgenstein. Brandom follows Sellars and later Wittgenstein in deriving the normativity of language from social practice, although there are also hints in Sellars of a possible derivation from evolutionary theory. I argue that there may have been an equivocation in Sellars's own writing that allowed two fairly faithful students to end up disagreeing with one another as we two have. An important claim common to Brandom and me is that there is no representation prior to function or "attitude".

5 The Language-thought Partnership

I take language and thought to stand largely parallel to one another. For example, the intentionality of each is defined independently of that of the other: thought is possible without language, and language is possible that does not convey thought. On the other hand, public language is not merely a stimulus to the development of thought. It is constitutive of developed human thought.

The intentionality of public language is derived from its stabilizing functions, which are derived, in turn, not from speaker intentions but from cooperative work that the language has been doing for both partners in communication. But though a given speaker's purpose in use may differ radically from functions of the public forms used, obviously a coincidence between these must occur some critical proportion of the time. Although the function of many kinds of language forms is to transmit thoughts or intentional attitudes, this is not always the case, nor is transmitting intentional attitudes what makes language have intentionality. Even bee dances display intentionality, in the fundamental sense that they may be true or they may be false, but it is unlikely that they transmit any thoughts. I distinguish "indicative", "imperative" and "pushmi-pullyu" representations. I describe forms of purpose other than explicit intentions that are commonly behind language use and briefly present my views on the origin of the intentionality of perception and thought and the development of thought in individual humans.

The challenge that faces a developing human mind is to develop its own inner representational

system with which to map relevant affairs in its world and to learn how to make accurate representations of its environment in this system. The difficulty is that the aspects of the environment that one needs most to map are distal, and correspond to proximal stimuli in highly complex and often unreliable ways. I sketch how the development of an inner representational system is accomplished and explain how and why learning language from others and acquiring information through language plays an essential role in this process. Most of our thought is possible only with the aid of others in the language community. Their speech is used by us as a medium of direct perception, through which concepts are formed and through which the structure of the world is perceived.

6 Why (most) Kinds are not Classes

Most category words do not designate classes but units of another kind entirely. I call these units "real kinds". Among them are "historical kinds" and "eternal kinds". Historical kinds, exemplified well by species, are tied together in a unit, not by having a certain set of properties in common, but by historical and causal ties that tend to make one like the next. Eternal kinds are of the sort Putnam called "natural kinds," having an inner nature in common. I compare historical and eternal kinds to individuals, and argue that just as in the case of individuals, different people may identify the same real kinds in different ways, so that no particular set of properties or paradigms or exemplars will be definitional of any word designating the kind.

7 Cutting Philosophy of Language Down to Size

Two mistaken assumptions lay at the heart of the mid-century philosophy of language. First, unlike the act of *knowing* an empirical fact, the act of *referentially meaning* something (though perhaps not of successfully referring) is completed within the mind itself. This is the "seed assumption". Second, a univocal term in a public language is associated with a psychological state common to all competent users. This is the "one-one assumption". I compare the work of Putnam on the meaning of natural kind terms to that of Russell, who opposed Bradley on how we have thoughts of individuals. Neither Russell nor Putnam succeeded in challenging the seed assumption. Quine's theory of the indeterminacy of translation resulted directly from the one-one assumption. I explain how to deny both assumptions, hence how to achieve a true meaning externalism, by recognizing that the purpose of a thinker intending a certain referent is not a psychological purpose, but a biological purpose.

8 Proper Function and Convention in Speech Acts

In "Intention and Convention in Speech Acts", Strawson applied Grice's theory of communication to speech act theory and with this tool, drew a distinction between two kinds of illocutionary act. Rather prosaically, I call these "K-I (kind I) -" and "K-II (kind II) speech acts". Strawson claimed that, contrary to Austin's views, only K-II acts are "essentially conventional". At the center of this dispute were cases where the speaker explicitly uses a verb describing the speech act he is ostensibly performing. Austin thought this explicitness obviously made the illocutionary act performed conventional; Strawson disagrees. The disagreement turns on the question what a linguistic convention is, and on the question what kinds of acts are "conventional" in the sense that they could not be performed in the absence of conventions. I import my own description of the nature of linguistic convention, and invoke my usual claim that human intentions and the functions of language forms are both "purposes" in the same sense of that term. I claim that what makes individual acts falling under a K-I kind into illocutionary acts of certain kind (an order, a request, an entreaty) is not Gricean intentions behind them, as Strawson supposed, but purposes, and that both people and language forms can have purposes, indeed, the same purposes. But these two sources of

purpose sometimes come apart. Then I argue that there are, in fact, no acts that could not, in principle, be performed in the absence of conventions. Trivially, acts like "marrying someone" are *classified* (across cultures, for instance) in terms of their conventional outcomes. This is a merely verbal matter. Conventional outcomes are how things will come out if the conventions happen to be followed through on. Outcomes of this sort are perfectly ordinary affairs, produced (if at all) perhaps following conventions, but in no way *constituted* by (some mysterious force called) *convention*. The relation of outcomes to purposes is then discussed and K-II acts are analyzed.

9 Pushmi-pullyu Representations

Pushmi-pullyu representations (P-PRs) are at the same time descriptive and directive. The simplest are animal's signals to conspecifics, such as danger signals and mating displays. The rabbit's danger signal, for example, tells when there is danger and also tells nearby rabbits when to seek cover. P-PRs also appear in human language: "No Johnny, we don't eat peas with our fingers"; "The meeting is (hereby) adjourned". Human intentions are P-PRs in thought, directing what is to be done and at the same time telling what the future will be so that one's other plans can be made accordingly. If Gibson is right, basic perceptual representations may be P-PRs representing affordances, what the environment is like and what one might do about or with it. I speculate that Inner representations of the social roles that we play as we play them may be P-PRs. Thus we fall naturally into doing "what's done" or "what one does". These primitive ways of thinking may be an essential glue holding human societies together. I also speculate about whether the "thick concepts" sometimes discussed in ethics may be related to P-PRs.

10 Semantics/Pragmatics (Purposes and Cross-purposes)

The description I have given of convention and of linguistic function yields a robust description, in naturalistic terms, of the distinction between semantics and pragmatics, taking semantics as the study of what is conventional in language use and pragmatics as the study of what is conveyed in ways not yet hardened into convention. This distinction between what is handed down by convention and what is understood pragmatically is intrinsically blurry, however, depending on statistics over individual psychological processing. Besides the more obvious fact that new usages may slowly become conventional, understanding exactly how to carry on a convention is often problematic, different people understanding exactly what is conventional in different ways, but without significant resulting failure in communication. This sort of scattering constitutes indeterminacy in the convention itself.

I discuss implications for the theory of language interpretation, drawing the conclusion that there may be many ways of grasping the content that the specific speaker intends to convey without employing a theory of mind. A coordinate claim is that during normal conversation, it is not language that is most directly perceived by the hearer but rather the world that is most directly perceived *through* language. Moreover, if this claim is true, and if it is true, as I argue, that there are ways to grasp another person's purposes without employing a theory of mind, then perhaps we can understand how it is that children learn so much language so quickly without instruction. They learn what the patterns of language mean exactly as they learn what is "meant," for example, by the patterns of ambient light in which their eyes are bathed.

References

Davidson, D.
1986 "A Nice Derangement of Epitaphs", in R. Grandy & R. Warner (eds.), *Philosophical Grounds of Rationality*. Oxford: Oxford University Press, 156-74

Lewis, D.
1969   *Convention. A Philosophical Study*. Cambridge: Harvard University Press.

Strawson, P.
1964   "Intention and Convention in Speech Acts". *Philosophical Review* 73: 439-60

Concepts, Conceptions and Psychological Explanation: Comments on Millikan

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Millikan's new book gives the reader the chance of «a bird's eye view» of her many important claims about language, thought, and reality, supported here by some new arguments, but mainly presented with their interconnecting links. My commentary will be focused on the issue of concepts, touched here in chapter 6 - one of the completely «new» ones - and in parts of chapter 3, but developed in full details in previous works (Millikan 1998; 2000). Actually, I am very sympathetic to this view (as well as with Millikan's all theoretical framework and conclusions). As my job requires masking my agreement, however, I will develop two doubts, and then raise a simple question of clarification. My doubts arise in connection with a broad issue, that is, the relationship between the philosophical and the psychological notion of concept. It might be one of those cases – which Millikan envisages – where we have two separate ideas of one and the same substance. But the interesting question would then be how the equivocation should be eliminated. My first doubt is about publicity. According to Millikan, what is public about concepts (and many word meanings) is just reference. Conceptions generally vary from person to person and for the same person from time to time. But conceptions as Millikan describes them are bound to play a role in our explanations of rational behavior, which may require – this is my worry – a stronger notion of publicity. My second doubt is about empty concepts. In line with content externalism, Millikan holds that empty concepts are genuine concepts insofar as they have explicit conceptual components grounded in experience. They tend to be analytic and definitional, whereas substance concepts are synthetic and recognitional. Still, many empty concepts play exactly the same role of non-empty ones in categorization tasks, in practical inferences, and (again) in psychological explanation. So it might be asked why marking a sharp difference where sameness seems to be equally relevant. Finally, the question of clarification. I am not clear whether Millikan's position on the relationship between concepts and conceptions has stayed the same or changed from *On Clear and Confused Ideas* to *Language: a Biological Model*. The issue of publicity and the problem of empty concepts may depend on that - I hope to make it more clear in what follows. I will proceed by going through the main lines of Millikan's account of concepts once again, then by tackling publicity and empty concepts in this order, and I'll conclude with the clarification question.

1 Concepts, Conceptions, and Publicity

Millikan focuses on concepts of Aristotelian substances, a wide ontological category including individuals, natural kinds, species and subspecies, and artifacts - anything that supports inductive generalizations in virtue of «a good reason» (Millikan 2005: 107) or «a ground of real connection» (Millikan 2000). Concepts of substances involve ways of tracking their appointed substance: of identifying it through different encounters, and of projecting some of its invariant properties from one encounter to the next. These ways – called «conceptions» - may be perceptual, language-

mediated, or theory-shaped; their richness may span from grade zero (just a name, enabling one to recognize when that kind of thing is talked about) to maximum expertise.

Conceptions explain our judgments of categorization (Millikan prefers «identification» and «reidentification»), but they neither suffice for determining substance boundaries, nor for concepts' individuation. A concept is a concept of F not because we sort out Fs reliably (let alone infallibly) by means of it, but because there's F out there, and our cognitive system created and preserved that very concept with the function of keeping track and representing F. The general idea behind this claim is a genuinely realist one. There is an independently constituted world out there - and we try to map its constancies at best with our representational systems - not a fuzzy mass of stimulations we try to make sense of, by imposing our definitional standards. Thus, Millikan's view combines the typically psychological thesis that concepts are mechanisms involved in categorization and in knowledge projection, with philosophical externalism and with the realism of direct reference theories.

What is philosophically important about conceptions is that they are in principle fallible, and not definitional. Thus, we do not have «a set of properties in mind, whether derived from paradigm cases or from exemplars» (Millikan 2005, 112) that we impose onto the world in order to determine its structure. The «we» here is to be intended both distributively and collectively: generally each of us does not employ definitions in order to track substances cognitively, and within a linguistic community there need not be shared definitional criteria associated with each substance term or concept, that anyone must possess in order to be credited with mastery. On this view conceptions turn out to be generally a private matter, and referents alone (substances) are public. There can be no conceptual analysis intended as an individuation of the proper conceptual parts of people's shared conceptions of things, because people's conceptions of things need not have conceptual parts, and generally they are not shared. There is no such thing as «the» conception of, say, sugar, or «the» meaning of «sugar» but rather different people's conceptions (also liable to variation over time) of one substance. Millikan accordingly rejects «any traditional notion of intension» and «any Frege-like notion of sense». She then offers «an alternative explanation of the phenomena that lead the postulation of intensions and Fregean senses», that is, identity statements, existence statements, intensional and belief contexts (Millikan 2005, 72-73).

One may also expect here, however, an alternative explanation of those phenomena that lead the postulation of *shared concepts* – or, to use a common jargon, to impose a *publicity requirement* on theories of concepts. Identity, existence and opaque contexts are not at issue. Rather, the problem arises in connection with a central explanatory role usually attributed to the notion of concept. It is very common to argue that concepts are the entities involved in commonsense psychological explanations of people's behavior. A simple argument for publicity may go as follows. Intentional or commonsense psychology aims at explaining and predicting people's behavior in terms of their beliefs, desires, hopes, and so on. Its method consists in redescribing a particular action or state of an individual (like my seeking water, or my mental state of wanting to have water) as a case of a law-like generalization about the relationship between people's beliefs, and their desires or needs (like «Thirsty people seek water», or «Thirsty people who know that water quenches thirst want water»). In order to have generalizations about propositional attitudes, it ought to be possible that different individuals have the same belief or desire. Therefore, it ought to be possible that concepts as components of attitudes are *public*. In short, no intentional psychology without publicity of concepts.

Which notion of publicity is required here? Publicity as mere functional identity (publicity of reference) does not seem to suffice. People act on their beliefs and concepts, they do not act on things. Thirsty people decide to stand up and go and seek water because their concepts cause their actions, not the nature of water. So concepts, and not just water, should be public in some sense, and not merely water.

Nevertheless, one need not go as far as imposing that concepts ought to be *identical* across

subjects, in order to satisfy the requirement imposed by commonsense psychology. Actually, the argument above is a favourite tool for philosophers like Fodor (1998), who defends a no-structure, purely atomistic view of concepts, and a very strong notion of publicity as concept identity – same unstructured concept of water for all individuals cognitively connected with water. But atomism is merely one way to satisfy the publicity requirement (incidentally, not one I would be prepared to defend). Another way may go through a notion of publicity as strong concept similarity. In order to fall under psychological generalizations, people concepts' ought to be significantly and objectively overlapping, that is, similar along some similarity metrics or other. Concept similarity would require identity of some of their components, but does not require, obviously, that components be conceptual themselves. Exemplar theories or prototype theories of concepts tend to have their own similarity metrics, based on feature identity, where features are various kinds of non-conceptual components. «Mixed» accounts of the structure of concepts, such as Millikan's, could borrow from many different models.

One thing is to deny that concepts of substances are associated with conceptions or components that all competent users *must* possess (Millikan 2005, 69). Quite another thing is to deny the non-modal claim, that concepts of many substances are associated with conceptions that *do* remain robustly similar across users. How can the latter thesis be defended? One may appeal to Quinean «shared similarity standards», for concepts of perceptually recognizable substances, and for non-perceptual cases to the fact that concepts are learned through linguistic communication. For communication to be possible, concepts would surely need to be public, but just in the functional sense – they need to be concepts of the same thing. The use of concepts in interpersonal communication, however, can explain in most cases how coreferential concepts of different individuals come to be strongly cognitive similar, through phenomena such as deference to experts, simple trust and transmission of information. Now, Millikan avails herself of all these answers. Still, she seems to consider concept publicity as an accident rather than the norm, discarding the non-modal claim as well as the modal one. Thereby apparently failing to account for the role of public concepts in psychological explanation. Of course, she may discard commonsense psychology altogether. This, however, would be far from being obvious from a theorist of concepts.

But why am I discussing «concepts» at all? Isn't Millikan just claiming that «conceptions» are generally private? In fact, the term «concept» seldom appears in the new book. I am assuming, here, that the action-causing and the correspondent action-explaining role is taken up, on Millikan's view, by conceptions. Concepts are just identifying abilities typed by their ends (Millikan 2000) - that is, types of conceptions, individuated by reference to what they are concepts of. Questioning this point, however, would bring me too early to my conclusion.

2 Empty Concepts

Concept externalism is the thesis that concepts are dependent for their individuation on the subject's environment. Millikan's externalism about substance concepts and substance terms has it that these concepts and terms are individuated at least in part by the functional-historical relation each of them bears to a certain substance. From this follows that if there's nothing out there for us to keep track of, there are no substance concepts. (This, incidentally, is a risk you should be prepared to face if you are sympathetical to global scepticism of the sort generated by brain-in-vats hypotheses). And from this also follows that, strictly speaking, there can be no empty concepts, because if something is a concept at all, it is the concept of some existing entity. Millikan allows for the possibility of concepts of non existing entities, such as Santa Claus, provided that they do not function as identifying abilities, and they are ultimately analyzable along Russellian lines as bunches of definite descriptions. The function of concepts associated with empty terms is to describe, not to keep track of something. They are – borrowing from Millikan's own couples of opposites - «classifiers», not «identifiers», they are «analytical concepts» and not «synthetical concepts». They are entitled to the realm of the intentional because they are essentially composed out of conceptual parts, and these

conceptual parts are themselves not empty. Thus, these concepts differ from the case of, so to say, a "conceptual sneeze", "a quirkish regular response to certain sensory stimulations, resulted presumably from the faulty operation of a system *designed* to *design* genuine concepts..." (Millikan 2005, 72). But they are also different from genuine substance concepts both in their structure and in their function.

The upshot of this view is to avoid both the unpalatable idea that our cognitive system has "conceptual gaps" whenever we do fiction, make plans and hypotheses about non-existing things, and the Meinongian temptation of creating a brand new property out there in the world in order to assign a referent to our terms. Yet, this view is quite out of line with psychological practice, as typically developmental psychologists do not care whether the concepts they study are empty or not. This is not in itself an objection. But it should be relevant that from the psychological point of view Santa Claus concepts may function exactly like George Bush concepts – they may figure as middle terms in inferences, and be employed in action planning and in judgments of recognition; they may involve different means of identifications, most of which are non-definitional; they may allow knowledge projection; they can be learned and applied in recognition «in the flesh»; they can grow in richness as subjects learn more about Santa Claus and George Bush, or meet them again. I, for one, have recently meliorated my own Santa Claus concept by learning, from Millikan's book, that he acquired red and white attire and beard late in his career – so now I'm able to identify something as Santa Claus even if he is dressed in green. These concepts can qualify to be, in some (I think) plausible sense, substance concepts. My George Bush concept is a substance concept on Millikan's view, given that the actual president of the USA (referentially meant) is an aristotelian substance; I tend to think that my Santa Claus concept is more like a substance concept than a descriptive one.

Pseudo-scientific concepts, like phlogiston in the past, and concepts of races now, can make good examples of this sort. .People identify human races through the color of the skin, through bodily features, but also through the conviction that something "hidden" is transmitted from parents to children. They project knowledge (and prejudices) from one exemplar of a certain race to the others. In all this cognitive work, the biological fact that races do not exist - or at least not as sharply defined sets of men and women sharing relevant properties - plays no role. It plays no role in how categorization and action planning are performed, let alone in the folk psychology of behavior. Why not simply say that concepts like these are all substance concepts – on the grounds that their function and structure is identical to substance concepts? Why marking the difference? The philosopher has a straight answer, but the scientifically minded philosopher may not have an equally straight one. On an alternative view, to be a substance concept would be to keep track of a definite range of phenomena with the conviction (explicit but often implicit) that they have an underlying cause, or *essence*. That the conviction is true, rather than false, does not change the status of concepts, it changes the status of our knowledge of the world. It makes our concepts scientific, but does not make them genuine concepts, because they already are. Likewise, that the conviction is false, does not turn them into representational sneezes.

I am here pressing Millikan's view towards a position generally known as «psychological essentialism» (Gelman 2003, Bloom 2000): I am claiming that substance concepts can be redescribed as *essentialist concepts*, with a gain in psychological plausibility. The class of essentialist concepts would capture what's shared by phlogiston concepts and oxygen concepts, and not shared by drug-induced illusions. The class of essentialist concepts could then comprehend the class of scientifically validated substance concepts as a proper part. I take this move to be compatible with Millikan's externalism (only maybe somehow mitigated). Psychological essentialism need not be accompanied by an internalist semantics, as essential concepts (including phlogiston and race) can well be individuated in terms of salient aspects of the subject's environment, either accessed through perception, or through language. Psychological essentialism need not entail antirealism either, as it is perfectly compatible with the view that things exist

independently of how we conceive of them. Only, how we conceive of them makes a difference for what counts as a substance concept, or a concept at all. The broader class of essentialist concepts seems to me also compatible with Millikan's ontology, as the notion of substance is both world-dependent and cognition-dependent, and somehow vague in boundaries. What counts as a substance on Millikan's view allows for gradation, varying on two different levels: the number of projectible properties, and the «tightness» of the relation by means of which substance instances are homogeneous. It seems to me that, given these criteria, no genuine cut-off point could be found between essentialist concepts - including pseudoscientific and prescientific concepts, as well as many concepts of «fictional kinds», like Santa Claus, Superman or Cinderella – and scientifically validated ones. At least if we care to keep an eye to the psychology of concepts.

3 A Question of Clarification

Finally, let me word a genuine doubt. I am not clear whether, on Millikan's updated view, concepts *are* ways of identifying substances, or rather they *involve* ways of identifying them, as she sometimes writes. This is not merely a terminological issue. If concepts just *involve* or are *associated with* ways of identifying, then we can count two things per person per substance, namely a concept *and* a conception (or many conceptions). In this case, we can think of concepts as labels, or mental words, that come on top of conceptions, so to say. Labels and mental words cannot account for categorization, and don't have much in common with those entities that psychologists call «concepts». On the other hand, they are surely compositional, *qua* words, and they are public, because they do not have components that may vary from individual to individual. Concepts as labels may also – if one cares – be processed by a Turing-like computational module.

The other option is that concepts just are conceptions – as in Millikan (2000), they are abilities of identification typed by their ends. In this sense, people's different conceptions of water count as water concepts, just like people's different handwritings count as ways of reproducing the word «water», and different ways to prepare an apple cake all count as apple cake cooking abilities. If that is Millikan's view – and I have been supposing it is - then traditional explanatory roles assigned to concepts are inherited by conceptions. On this view conceptions would better be public, at least in the sense of strongly similar, as I have suggested. And if concept just are conceptions, then features of conceptions – for example, the feature of including an essentialist conviction – would obviously be relevant for establishing what kind of concept a certain concept is, along with the features of the environment it aims at keeping track of.

In either case, I take Millikan's view on concepts as the most challenging on the philosophical market, uniquely combining serious work in ontology and epistemology with attention to psychological work. I am therefore very glad for the occasion of commenting on her stimulating new book, which I enjoyed reading as the previous ones.

References

Bloom, P.
2000   *How Children Learn the Meanings of Words*. Cambridge (Mass.): MIT Press.

Gelman, S.
2003   *The Essential Child*. Oxford: Oxford Univeristy Press.

Fodor, J.
1998   *Concepts. Where Cognitive Science Went Wrong*. Oxford: Oxford University Press.

Millikan, R.G.

1998   "A common structure for concepts of individuals, stuffs, and real kinds: More Mama, more milk, and more mouse". *Behavioral and Brain Sciences* 9 (1): 55-100.

2000   *On Clear and Confused Ideas. An Essay on Substance Concepts*. Cambridge: Cambridge University Press.

2005   *Language: A Biological Model*. Oxford: Clarendon.

Comments on Ruth Millikan's *Language:*
A Biological Model

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Ruth Millikan's writings have brought a breath of fresh air in the last 20 years of philosophy. One of her central claim is that linguistic conventions should not be modelled on the form of social norms, that is, rules that can be an object of evaluation in a social group. Rather, the normativity of language is of the same kind as the normativity that is found in the biological realm: biological items have each an effect that accounts for their proliferation, and that effect is their function, which they perform more or less effectively. Linguistic items have functions in this biological, nondeontic sense, that is, they have effects that account for their proliferation. Language conventions are stabilized by these repeated effects. Millikan's "natural conventionality", as opposed for example to David Lewis' highly normative model of conventionality, has two simple ingredients: natural conventions are patterns that are (1) *reproduced*, and these patterns are reproduced because of the (2) *weight of the precedent* and not because of their superior capacity to perform a certain function in a given situation, which partly explains the arbitrariness of conventions (Millikan 2005, p.2). Explaining the conventionality of language in such terms is indeed a major step toward a naturalistic account of meaning and understanding.

The evolutionary framework that Millikan has been proposing since 1984 is also a powerful conceptual tool to articulate biological and cultural evolution in an innovative way, as Dan Sperber and I have argued in a previous paper (Origgi & Sperber 2000). Instead of a *memetic* view of replication of cultural units through a process that merely parallels that of genetic replication, Millikan's theory allows for clearer explanation of how a cultural and biological evolution may interact, and how items (linguistic or other) may have simultaneously biological and cultural functions.

Millikan's notion of *reproduction* is independent from specific mechanisms, either genic replication or social imitation and is thus suitably general. In particular, a linguistic pattern such as a word, a verbal mode or an intonation, may be directly copied from an individual to another, inferred on the basis of received instructions (as in education) or reproduced by *counterpart reproduction,* that is, by fitting in with the pattern another person has initiated. Conventional greetings for example are, she argues, reproduced in this third possible way.

Millikan has introduced a fruitful distinction between two kinds of proper functions, that is, functions whose effects account for the reproduction of an item, *direct* and *derived* proper functions. This distinction, we argued, helps to explain how culture and biology intertwine in the case of language. The direct proper function of an item is the effect that is historically responsible for its reproduction. Sometimes a device, in order to perform its direct proper function, produces items that are adapted to particular contextual circumstances. These contextually adapted items have a derived proper function, that is an effect that contributes not to their own reproduction but to the reproduction of the device that produced them. For instance, language is both a biological and a cultural phenomenon whose evolution is to be understood at a double scale: the biological evolution

of our "language faculty" -that is, the *ensemble* of organs and cognitive dispositions that made language possible as an adaptation- and the cultural/historical evolution of languages. Although Millikan doesn't develop this point, her distinction can be used in order to understand how a linguistic item may have a direct proper function of a cultural kind, (for example, the function to stabilize a pattern of information exchange between two people) and, at the same time, a derived proper function of a biological kind to contribute to the proliferation of the biological devices that makes linguistic communication possible (Origgi & Sperber 2000).

Although Millikan's account is a source of inspiration to rethink the articulation of biological and cultural phenomena within a unified framework, she couples her view with an account of linguistic meaning and intentionality that Sperber and I criticized in our paper.

I will call her view here: "extreme externalism": according to Millikan, language is a way of stabilizing "patterns in truth and satisfaction conditions" (cf. *Précis*, p. 1) no matter how these patterns are realized by our cognitive processes. Basically, if a linguistic item proliferates it is because the effects of its production are often enough advantageous to the hearer and the speaker. These advantageous effects consist in coordinating speakers and hearer's behaviour. This coordinating function corresponds to the conventional meaning of these items. It is thus of little use for Millikan to investigate what happens in people's mind when they speak or understand insofar as linguistic patterns succeed in stabilizing conformity to use. In particular, it is of no use to the interlocutor themselves. Speaker and hearer may succeed in performing this coordination function through different cognitive processes, as Millikan says in her *précis*: "The specific psychological processes, ways of recognizing, evidence relied on for application, that support our uses of proper names, of words for kinds, properties and so forth, need not be uniform from person to person for satisfaction conditions to remain uniform". Positing a number of filters that constrain which linguistic forms will be replicated (such as the anatomy of our auditory system and some Chomskyan grammatical constraints on what aspects of language will be perceived by a child as functionally relevant) is all that is needed at the psychological level to explain how language proliferates and serves its communicative, coordinative, and informational purposes.

Since Grice, the fact that the interpretation of utterances is highly context dependent has been taken as evidence that comprehension consists in inferring the speaker's meaning (a mental state) and not just in decoding the linguistic meaning. Millikan agrees that interpretation is never just a matter of decoding: the hearer is able to coordinate with the speaker and respond in the conventional way because the context in which the linguistic pattern has been initiated by the speaker allows, in a critical number of cases, the hearer to respond appropriately. However, the hearer doesn't have to reconstruct what the speaker had in mind, unlike the Gricean pragmatic approach suggests: the environmental conditions in which the communication takes place are usually enough for the hearer to recognise from which reproductive family a particular linguistic token comes from (e.g. if a token of "bank" reproduces the word referring to financial institutions, or that referring to the side of rivers) and thus figure out its meaning. For this, in most circumstances, the hearer need not pay any attention to the intentions of the speaker. That is because, for Millikan, linguistic meaning is not in the head. Interpreting utterances is just another way of perceiving the world: "interpreting the meaning of what you hear through the medium of speech sounds that impinge on your ears is much like interpreting the meaning of what you see through the medium of light patterns that impinges on your eyes". So, when communication proceeds normally, the hearer directly perceives the world through the words, and not the speaker's thoughts and intentions. Roughly, a linguistic stimulus, if it succeeds in performing its function, attunes us directly with a piece of the world, and what happens in the speaker's mind is of little relevance.

This is indeed a very special view of language. As Millikan acknowledges, that is how most animal codes work (p. 96). A bee dance succeeds in attuning other bees to the location of nectar, as if they had experienced the trajectory themselves. But human languages seem very different from

animal codes. They have evolved in social contexts, for a great variety of communicative purposes and not just for information transmission in the narrow sense of the term. Their complex relation with our social cognition is one of the central tenets of the contemporary debate on evolution of language. It is hard to imagine how such a fine-grained social ability would have developed independently of any awareness of the thoughts and intentions of others.

Millikan says that our ability to effortlessly disambiguate most linguistic forms is not due to our capacity of reading other people's intentions, but to the fact that language attunes us to an external context in which it is immediately perceivable what the linguistic forms refer to. Here is one the examples Millikan discusses in the book: if someone says "Hit-me" while playing blackjack, the environmental context of playing cards is rich enough to allow the hearer to interpret the speaker in the appropriate way, and cause her to give another card to the player instead of beating him, without having to attend to his mental states. I find this example quite puzzling: this is typically an example in which the use of the expression "hit-me" depends on the existence of an explicit, normative convention, of the kind Millikan sees as untypical of conventions in general (cf. Millikan 2005, ch.1): the hearer responds to "hit-me" in this circumstance by following a rule of the game that would have not survived without the existence of a social, normative context. But most of the contexts in which we interpret language don't provide uniquely appropriate responses of this kind.

Nevertheless, given her view of language as a form of direct perception of the world, Millikan insists that, as bees, we are immediately attuned to the environmental cues that it is the purpose of that piece of language to direct our attention to. One of her key arguments on this point is some psychological evidence she refers to in the last part of the book according to which children learn language in that way, that is, by directly perceiving what a word is about without inferring what their parents or instructors have in mind (Millikan 2005, 213 and ff). I found this was particularly surprising. My first surprise comes from my own experience with my child Leo, a 5 years old bilingual boy. The only word I can concede he could have learned in a "Millikanian" way, that is, by a sort of behaviouristic training for responding in the appropriate way in the appropriate conditions, is the word "STOP" used to cause him to stop moving forwards in the street . I trained him from the onset to respond to my utterance of the word "STOP" by stopping immediately, and I can use this "command" when I want him to stop while he's biking or rolling. His reactions to other similar but different stimuli, such as "HALT" or "*Arrête-toi*" in French would not be as immediate. With "STOP" he doesn't need to think about what I mean: he reacts to this just by stopping, even in absence of any clear reason to stop (i.e. even if there are no crossroads or traffic-lights). But this is far from being paradigmatic of his way of learning language. It is an interesting curiosity that people may be trained to respond to a few linguistic items as animals might. But it seems to me very odd to define such circumstances as the Normal conditions in which a linguistic item performs its function. But let's put aside this anecdotal evidence and have a closer look to the psychological evidence Millikan refers to in the book. It is surprising to find Paul Bloom's work on language learning (cf. Bloom 2000) mentioned at p. 213 among the references in support to her view of language. Bloom is quite explicit in his book (cf. p. 78 and ff.) about the role of mindreading abilities in language learning (see also Tomasello 1999). Children don't grasp what adults say just because they are equipped "with a neuronal organisation that is easily tuned to interpret the kinds of informational patterns that language presents" (Millikan 2005, p. 214) and they don't see through their parents' words as they would see through binoculars. According to Bloom, children understand words as signs of a communicative intention: without this understanding of the communicative act, language learning would be quite limited.

Also, I am not aware of any evidence that would show that the first experiences of language learning have to do with a sort of perception of the world by proxy of the kind Millikan describes, that is, in which children are told how the world is and experience it through a new medium. Children come to the world within a linguistic community, they listen to language even in their mothers' wombs and seem very competent in distinguishing between a perceptual stimulus and a

linguistic one. They don't "follow the mental focus of another person" as they would "follow the focus of binoculars or of a camera" (Millikan 2005, p. 217). It seems quite plausible, from a psychological and developmental point of view, that they follow the mental focus of that person because it's a person, whose mind and emotions may be more relevant for their little lives than the world around.

References

Bloom, P.
2000   *How Children Learn the Meanings of Words.* Cambridge MA: MIT Press.

Origgi, G.; Sperber, D.
2000   *Evolution, Communication and the Proper Function of Language,* in P. Carruthers & A. Chamberlain (eds.), *Evolution and the Human Mind.* Cambridge: Cambridge University Press, 140-169.

Tomasello, M.
1999   *The Cultural Origins of Human Cognition.* Cambridge MA: Harvard University Press.

Conventions are Shared

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For psychologists, Ruth Millikan's work is among the most interesting in all of contemporary philosophy - mainly because she connects so well with evolution and human psychology (and actually knows, and makes use of, the empirical literature). In the current volume she espouses a "biological" model of language (Millikan, 2005). Central to this model, and comprising the main topic of Chapter 1, are conventions, a mainstay in the philosophical analysis of language since the ancient Greeks. Millikan has some of her own ideas about conventions, however, and these definitely push the discussion forward in interesting and productive ways. Nevertheless, her account of conventions, in my opinion, could profit from being applied more systematically to processes of communication in nonhuman animals as compared with humans (the animal examples she uses in the book seem fairly randomly chosen). Such an application could help to sharpen the notion of convention and also to identify processes involved in the creation of communicative conventions in human evolution.

In this commentary, therefore, I attempt to apply some of Millikan's ideas about conventions to the comparison I know best: gestural communication in great apes and human children. This comparison is arguably the most important one in thinking about the evolution of human language because it is great ape gestures, not vocalizations, that are the most likely precursor of human symbolic communication. I do not have the space here to argue the point at length, but the basic idea (as laid out most systematically by Call & Tomasello, in press) is that great ape vocalizations are mostly genetically fixed (they do not change if an individual is brought up in isolation or by members of another species who use different vocalizations, for example), tied very tightly to hightened emotional states (because they are so often associated with evolutionarily urgent functions like escaping predators, staying close to the group, and finding food). Many great ape gestures, in contrast, are learned and used quite flexibly for different communicative functions (often in relatively relaxed social circumstances) - and such voluntary, flexible use is clearly prerequisite for anything resembling human symbolic communication.

In any case, in the current paper I compare the gestures of great apes, mostly our nearest relatives, chimpanzees, to those of human beings, mostly infants in the period before language acquisition begins in earnest. Staying within the gestural modality, and leaving the complexities of language mostly to the side, facilitates our ability to identify the critical differences of process.

1 Conventions

Human communication and language are clearly based, as Millikan argues, not on prescriptive normative rules, but rather on non-evaluative norms and conventions. Attempting to simplify away from the classic account of Lewis (1969), Millikan proposes that communicative conventions are characterized by two crucial characteristics. First, to be a convention a communicative sign (where 'sign' is the most general term possible) must be reproduced by individuals from other individuals.

This means that the vast majority of animal communication, which is mostly genetically fixed, is not conventional:

> Similar norms govern the primitive communications systems of animals, though in that case the reproduction of cooperative patterns of interaction is transmitted genetically rather than culturally, or rather than conventionally (p. vi).

Second, in addition, to be a convention a communicative sign must be reproduced due to "weight of precedent", not to each individual deciding that it is the most efficient way to get something done. This means that conventions often have a degree of arbitrariness, with little likelihood of a lapsed convention reappearing spontaneously without precedent.

Millikan distinguishes two ways in which communicative conventions may be reproduced. First, conventions may be reproduced "by being copied from one another directly, or one person may tell another how a pattern goes" (p. 4). Thus, individuals do something in the way others are doing it - and because others are doing it in that way - and this is the prototype of a convention. But Millikan proposes that conventions may also be reproduced by "counterpart reproduction", that is, by one individual adapting to the behavior of others (typically directed at herself) - as, for example, the way a child might learn to shake hands not by copying others but by adjusting to the seeming expectations of others as they thrust their hand out toward her upon greeting. This is, to my knowledge, a unique proposal, and I must say that as important as counterpart reproduction surely is in the process of human culture and cultural transmission in general, I do not see how patterns reproduced in this way qualify specifically as conventions.

Let us take the main examples of counterpart reproduction that Millikan offers (the several others she offers are in principle no different): shaking hands, dancing the tango (or some such), and using chopsticks or a fork to eat. Millikan proposes that in all of these cases we learn the conventional behavior not by copying or imitating others but by adjusting our behavior to that of others (or to the cultural environment as it presents itself to us). Let us assume for the moment that this account of acquisition is factually true. Then in what sense is the behavior reproduced "by weight of precedent"? It seems to me that these cases all are most similar to another example Millikan characterizes as not conventional.

> I learned from my mother, and she from hers, to open a stuck jar lid by first immersing it in hot water. Opening jars this way is not thereby 'conventional'. To be thought of as conventional, a reproduced pattern must be perceived as proliferated due, in important part, to weight of precedent, not to its intrinsically superior capacity to produce a desired result, or due, say, to ignorance of any alternatives. (p. 7)

In what way are Millikan's exemplars of counterpart conventions different? If I learned to shake hands by adjusting to some strange behaviors people directed at me (and what those people seemingly wanted me to do in response), then I learned that behavior individually as a way of coping with something in my social world (just as Millikan adjusts to recalcitrant jars in her physical world). If I learned to dance the tango by adjusting to my more skilled female partner, then I learned that behavior individually, not socially based on the precedent of other males performing their role in the tango. If I use a fork simply because that is what I find when I come to the table (whereas a Japanese person uses chopsticks for the same reason) my acquisition is not conventional but simply practical.

In point of fact, I think that all three of these examples are indeed conventions. But they are conventions precisely because they are not acquired by adjusting to the environment in acts of counterpart reproduction, but rather because, at least to some significant extent, they are acquired by imitation or some other form of social learning. Thus, dancing the tango is conventional because in large measure males and females learn their roles by imitating other males and females, and using a fork (instead of the hands or chopsticks) is conventional in large measure because children

copy the adults around them. And so I would simply say that to the extent that these behaviors are not copied they are not conventional, and to the extent they are copied they are conventional.

This point is absolutely critical in highlighting another aspect of conventional activities that is often considered requisite but that Millikan, for principled reasons, ignores. To most people conventions are shared or agreed upon - in the sense that we all know we all know them. But Millikan wants to avoid the well-known difficulties of 'mutual knowledge' and so does not invoke sharedness or agreement as a criterion. But sharedness is critical to conventions (the Wikipedia entry is: "A convention is a rule or a selection from among two or more alternatives, where the rule or alternative is agreed upon among participants."), and it arises as a natural result of imitation or other forms of cultural learning but not as a result of individual learning. This is most readily seen by looking at our nearest primate relatives and the way they acquire their communicative activities.

2 Chimpanzee Gestural Communication

Like all mammals, chimpanzees have a number of more or less involuntary postural and facial displays that express their mood, for example, raising of body hair indicating an aggressive mood, penile erection indicating a sexually receptive mood, and 'play-face' indicating a playful mood. These evolved displays are as inflexible as their vocalizations. But in addition, chimpanzees also use a number of gestures intentionally, that is, in flexible ways tailored for particular communicative circumstances (e.g., Goodall, 1986; Tomasello et al., 1994, 1997; Call & Tomasello, in press). What marks these gestures as different from involuntary displays is, first, that they are clearly learned as not all individuals use them and there are marked individual differences, and, second, that they are used flexibly both in the sense that a single gesture may be used in different contexts and in the sense that different gestures may be used in the same context - often in rapid succession when an initial gesture does not lead to the desired response. Moreover, they clearly are communicative signals in the sense that they do not function mechanically to move or manipulate the recipient's body, but rather they operate informationally to solicit some reaction from the other. This interpretation is substantiated by the fact that in chimpanzee gestural communication the signaler typically waits expectantly for a response from the recipient after the gesture has been produced, and sometimes even alternates gaze between recipient and goal.

From a functional point of view, chimpanzees use two types of gesture: intention-movements and attention-getters. Intention-movements are typically abbreviations of some full-fledged social behavior and are used to initiate recurrent social interactions (see Tinbergen, 1951, on intention movements, cited by Millikan in footnote 7). For example, many youngsters ritualize 'arm-raise', abbreviated from play hitting, to initiate play. Many infants also ritualize signals for asking their mother to lower her back so they can climb on, for example, a brief touch on the top of the rear end, ritualized from occasions on which they pushed her rear end down mechanically. Infants often do something similar, such as a light touch on the arm (ritualized from actually pulling the arm), to ask their mothers to move it so as to allow nursing. Any given intention-movement is used only in contexts in which the real behavior also occurs, and so their 'meaning' is inherent in the activity from the outset.

Chimpanzees almost certainly learn their intention-movement gestures by a process of ontogenetic ritualization, not imitation. For example, it is likely that the 'arm-raise' gesture to initiate play originates as follows:

(1) an initiating chimpanzee youngster begins rough-and-tumble play with another by play hitting;

(2) after repeated instances of this the recipient begins to anticipate the impending hit on the basis of the first part of the sequence (the raising of the arm) and so begins the rough-and-tumble play at that early point in the sequence;

(3) the initiator notices the recipient's anticipation and comes to anticipate this anticipation - so that on some future occasion it produces just the 'arm-raise' in order to elicit play, with no attempt to actually hit, waiting for a response from the recipient.

The 'arm-raise', which was originally a preparation for instrumental action, has become an intentional communicative signal used to elicit play from others. Evidence that ontogenetic ritualization is the major, if not exclusive, process of chimpanzee gesture learning is that: (i) some youngsters use gestures that no other group member used (thus precluding imitation as a means of acquisition), (ii) some youngsters use gestures that they have had little or no opportunity to observe, (iii) youngsters raised only with peers develop many of the same gestures as those raised with adults, and (iv) individual variability in types of gestures used by individuals of the same group is very high (Tomasello et al., 1994, 1997).

The other type of chimpanzee gesture is attention-getters, such things as slapping the ground, throwing things at others, or poking others in the back in order to get their attention. Because they are not ritualizations of pre-existing social behaviors, these signals are more context-free and occur quite widely across contexts. This means that attention-getters do not carry with them their meaning in the same way as intention-movements. Most typically, an attention-getter is used when the signaler is already displaying its mood in some other, often involuntary facial expression or posture; for example, a youngster might have a play face and posture and slap the ground so that a non-attending potential playmate will notice this face and posture and so start to play. Despite this functional difference with intention movements, attention-getters are still very likely learned by ontogenetic ritualization. The scenario would be something like this: an individual slaps the ground or pokes another during play and then notices that this always serves to cause the other to attend to her, and this connection is then basically exploited more generally in the future.

The important point for current purposes is that ontogenetic ritualization is basically an instance of counterpart reproduction. In my view, if chimpanzees acquire their gestures solely by means of ontogenetic ritualization, they are not acquiring them by weight of precedent, and the gestures are therefore not shared conventions. That is to say, an individual understands a ritualized gesture from one perspective only - from either the initiator's or the recipient's perspective depending on which role she played in the ritualizing interactions - not as bi-directional, shared communicative convention. This is in stark contrast to gestures acquired by means of copying or imitative learning (probably none by apes in their natural environments) in which an individual understands the communicative intention of a gesturer and then reproduce the "same" gesture when she has the "same" intention. The communicative sign that we are both using must be the "same" - must in some sense be shared between us - because I copied your use.

3 Human Infant Gestural Communication

At around their first birthdays, human infants begin to use two types of gesture that are somewhat analogous to the two types of chimpanzee gesture. First, analogous to intention movements, infants use characterizing (or symbolic) gestures. These gestures indicate some specific referent non-deictically, often iconically, and thus they have a meaning that does not change with context. During their second year of life infants produce such gestures routinely (e.g., waving for 'bye-bye', headshaking for 'no', raising the palms for 'all gone', opening/closing the mouth for 'fish', panting like a dog, driving motions for 'car', blowing for 'hot things') (Acredolo & Goodwyn, 1988; Iverson, Capirci & Caselli, 1994). Almost all of these characterizing gestures first appear in infant-caregiver imitation games or in social routines repeatedly produced in the context of infant-caregiver interaction, and could only be learned via imitation, as there are few spontaneous child behaviors that could be ritualized appropriately (and it is likely that in cases such as "panting like a dog" the adult first mimics the dog and the child mimics the adult). The imitative process involved in the acquisition of these gestures allows us to assume that the child, as learner, understands the meaning

of the gesture from both the initiator's and the recipient's perspective and therefore that these characterizing gestures are shared communicative conventions from the start.

The second type of gesture is deictic gestures, including such things as: (1) pointing, in which infants use an extended arm-hand-finger to direct the adult's attention to an outside entity; (2) showing, in which they hold up an object manually in the adult's line of sight in order to share attention to it; and (3) offering, in which they hold out an object to an adult intending that she take it (Bates, Benigni, Bretherton, Camaioni, & Volterra, 1979). Like chimpanzee attention-getters, infants' deictic pointing serves merely to direct the attention of the other to something - in this case typically not the self but some outside entity - and the meaning of the gesture must be inferred from context. In terms of learning, there has been very little study of how human infants acquire their deictic gestures. It is likely that they learn some of them by ritualization and some of them by imitation. But in the most critical case, pointing, imitation is the most likely acquisition mechanism - if not at the beginning then soon after - and so once again at least by the middle of the second year human infants understand their pointing gesture as a shared communicative convention.

The central point is this. When we compare the gestures of chimpanzees with those of human infants, what stands out most clearly is precisely the fact that the infants' gestures are not individual procedures for getting others to do things, but rather imitatively learned, and so shared, communicative conventions. Regardless of how one chooses to deal with the mutual knowledge involved, in these cases we both know that we both know the gestural convention.

An important source of evidence, albeit an indirect source of evidence, for this analysis is the fact that many other ape and human social behaviors are distinguished by exactly the same difference between individualistic and shared versions - based on what we have called, following Gilbert (1989), Searle (1995), and Bratman (1992), shared intentionality. For example, Tomasello et al. (2005) argue and present evidence that in many domains of activity chimpanzees possess an individualistic version of the social skill that human children have in shared version. Just very briefly, for instance, chimpanzees engage in (a) gaze following, (b) manipulative communication, (c) group action, and (d) social learning. But human skills and motivations for shared intentionality transform these into their collectively based counterparts of (a') joint attention, (b') cooperative communication, (c') collaborative actions with shared goals and differentiated roles, and (d') instructed learning based on a teacher-pupil interaction. These are all cornerstones of cultural living, and help to account for the very different nature of human social life. The argument here is simply that shared intentionality might very well operate in the current case as well, transforming chimpanzees' one-way, ritualized gestures into human imitated and therefore shared gestural conventions.

4 The Drift to Arbitrariness

Let me be very clear that counterpart reproduction, in my opinion, plays a crucial role in processes of culture and cultural transmission, as children adapt to the particular carpentered world into which they are born. And Millikan has drawn attention to this process in ways that should help us to think about culture and its reproduction in much more productive ways. It is just that counterpart reproduction does not produce conventions.

Additional evidence for this, again indirect, comes from thinking about how the arbitrariness of human gestures and linguistic symbols might have arisen evolutionarily. Millikan makes the very astute observation that behavior patterns produced by counterpart reproduction, in contrast to those produced by imitation, retard drift away from the original:

> Often each individual part needs to fit arbitrary members from a whole collection of counterparts, and vice versa. This easily results in standardization of forms, more easily than by direct copying. Copies of copies easily drift away from the original; the need to fit counterparts retards drift. (pp. 4-5)

But to get to human conventional gestures and linguistic symbols, connected to their conventional referent only arbitrarily, we actually need some drift.

Here is the kind of scenario I think must have occurred when humans stumbled upon the idea of arbitrary gestural and linguistic conventions. First came some kind of intention movement, for example, a female of the early genus *Homo* prepares to go dig for tubers by reaching for her digging stick atop some ledge in the cave, and this becomes ritualized (as others anticipate, and the female anticipates their anticipation) into an upward reaching movement for mobilizing others to come along. The participants in this scenario understand, in some sense, that this reaching comes from an instrumental action for retrieving a necessary tool for digging.

But now let us assume that our early *Homo* individuals have, since their split from chimpanzees, acquired skills for imitating intentional actions in a way more like modern humans than other apes (reproducing the appropriate action for the appropriate function). Let us also assume the following extension of the scenario. Some individuals not familiar with digging, perhaps males or even children, observe this "Let's go digging" gesture, and for them the connection between the ritualized upward reaching and going digging for tubers is opaque. But on one occasion they tag along and see that this gesture which seemingly mobilized others for action is associated with going digging for tubers. They might then imitatively learn the gesture to initiate the same activity themselves on future occasions, and then perhaps generalize it to other digging activities, even those that do not involve sticks - so that the original instrumental basis for the gesture (reaching for a stick) is now completely gone. One can possibly imagine in addition some kind of general insight at some later point that most of the communicative signs we use have only arbitrary connections to their meanings and referents, and so, voila, we can if we want make up new arbitrary ones.

This drift toward arbitrariness, as Millikan observes, can only plausibly happen in cases where the propagation takes place mainly by imitation, not counterpart reproduction which typically possesses more constraints. And so my contention is simply that the arbitrariness of human communicative signs arose as a kind of byproduct of reproduction by copying, and so this form of reproduction was critical to the evolution of conventional symbols in a way that counterpart reproduction was not. Thus, again, as important as it is in its own right, counterpart reproduction does not generate conventions.

Relatedly, there is one other point on which I disagree with Millikan's application of the concept of convention. It concerns the level of speech acts. As part of her otherwise trenchant analysis of how different kinds of speech acts require different kinds of coordination between speaker and listener, Millikan claims:

> The speaker's production of the expression and hearer's cooperative response to it constitute a reproduced pattern whose form is arbitrary relative to its coordinating function ..... In the case of conventional directive uses of language such as paradigm uses of the imperative .... when you ask or tell me to do something in a conventional way, using some appropriate shape from some public language to do so, it is *conventional* for me to comply: this outcome is the completion of the conventional pattern (p. 152-3)

But this seems to me to be exactly the wrong analysis, especially given Millikan's commitment to a biological approach. In an imperative speech act the speaker requests that the listener help her in some way. This request is cast in conventional form, of course, but the tendency of the listener to help (altruistically) must be justified evolutionary by some advantage to her for providing this help (e.g., by increasing her reputation for being a helpful partner). It is unlikely in the extreme that anyone cooperates with an imperative speech act by imitating other recipients' compliance, simply in order to follow precedent. Responding to an imperative by providing the requested help thus does not represent the completion of a conventional pattern, learned by imitation, but rather a 'natural', biologically evolved behavioral response. A different, but analogous, characterization would be necessary for an indicative speech act in which the speaker volunteers information she

believes to be helpful or interesting for the listener, who then takes it up.

5   Conclusion

In her new book, Millikan has provided us with an extremely interesting and important account of language and its biological and social bases. In two important ways, however, I believe she has used the concept of convention too broadly. First, although counterpart reproduction is an extremely important phenomenon - human culture would not be possible without it - it does not create conventions because it does not involve imitation and the resulting understanding of the activity as shared. Counterpart reproduction thus creates learned cultural activities, not conventions. Second, neither speech act motives nor the tendency of listeners to respond to these cooperatively are learned activities at all, and so they also are not conventional. Rather, human communicative motives and corresponding response tendencies are biologically evolved patterns of cooperative activity in which both speaker and hearer benefit in some way, with linguistic and other communicative conventions serving merely to express them publicly.

Overall, then, the notion of convention should be restricted - in my opinion and as Millikan herself argues - to behaviors that are socially learned and used due to weight of precedent. We only differ in how this criterion is applied. In addition, I also believe, perhaps contra Millikan, that this acquisition process created a shared understanding, which, despite the conceptual difficulties it presents for philosophers, is of the essence of conventions.

References

Acredolo, L. P., & Goodwyn, S. W.
1988   "Symbolic gesturing in normal infants". *Child Development* 59: 450-466.

Bates, E., Benigni, L., Bretherton, I., Camaioni, L., & Volterra, V.
1979   *The emergence of symbols: Cognition and communication in infancy*. New York: Academic Press.

Bratman, M. E.
1992   "Shared cooperative activity". *Philosophical Review* 101(2): 327-341

Call, J. & Tomasello, M.
in press   *The gestural communication of apes and monkeys*. Erlbaum.

Gilbert, M.
1989   *On Social Facts*. New York: Routledge.

Goodall, J.
1986   *The chimpanzees of Gombe. Patterns of behavior*. Cambridge, MA: Harvard University Press.

Iverson, J.M., Capirci, O., & Caselli, M.C.
1994   "From communication to language in two modalities". *Cognitive Development* 9: 23-43.

Lewis, D.
1969   *Convention: A Philosophical Study*. Cambridge, MA: Harvard University Press.

Millikan, R.
2005   *Language: A biological model*. New York: Oxford University Press.

Searle, J.
1995   *The construction of social reality*. New York: Free Press.

Smith, W.J.
1977   *The behavior of communicating: An ethological approach*. Cambridge, MA: Harvard University Press.

Tinbergen, N.
1951   *The study of instinct.* New York: Oxford University Press.

Tomasello, M., Call, J., Nagell, K., Olguin, R., & Carpenter, M.
1994   "The learning and use of gestural signals by young chimpanzees: A trans-generational study". *Primates* 37: 137-154.

Tomasello, M., Call, J., Warren, J., Frost, T., Carpenter, M., & Nagell, K.
1997   "The ontogeny of chimpanzee gestural signals: A comparison across groups and generations". *Evolution of Communication* 1: 223-253.

Tomasello, M., Carpenter, M., Call, J., Behne, T., & Moll, H.
2005   "Understanding and sharing intentions: The origins of cultural cognition". *Behavioral and Brain Sciences* 28: 675-735.

Reply to Lalumera

Ruth Millikan

It was well known to psychologists in the American behaviorist tradition that the behaviors easily trained by operant conditioning are not muscle contractions or specific movements of the animal's limbs but the effecting of certain environmental results. For example, Guthrie's cats could easily be trained to push against an upright lever and then pull a string in order to let themselves out of a box, but what parts of their anatomy they would use for this would vary not only from cat to cat but from occasion to occasion for an individual cat. What the cats learned was to push against the lever and pull the string, not to contract thus and such muscles. Behavioral psychology alone could not predict which parts of the anatomy the cat would use to reach these ends. A sophisticated feline neuroscience plus a minute physiological knowledge of the particular cat's perceptual and motor systems would be needed, I suppose, for that. The classical behaviorists also did not attempt to formulate behavioral laws having, in their antecedents, reference to retinal or tactual stimulations of the behaving animal. They realized that they could only describe, say, what the cat would do if it found itself in a certain kind of box, not the variety of stimulations to which it might owe the discovery that it was within the box, how it was oriented there and so forth.

A similar point can be made about the prediction of human behavior from knowledge of relevant beliefs and desires involving substance concepts.[1] Having a substance concept is a bit like knowing how to push against a lever or pull a string or recognize when you are in a box. Having a substance concept involves, in part, a fallible ability to recognize when one is encountering information -- natural information or information through language -- about the object of the concept.[2] But like the ability to pull a string, this ability may be realized in any number of alternative ways. There are many different ways to recognize, say, one's husband, or a deer, or butter. One may use one way on one occasion and other ways on other occasions. The sum of different ways a person is able to recognize a substance make up what I have called their "conception" of the substance and, of course, different people may have somewhat different, sometimes even very different, conceptions of the same substance. That I do not know precisely by what means you will recognize your husband, or a deer, or a stick of butter, on some particular occasion does not imply that I cannot predict your behavior on meeting your husband, or finding a deer in your garden, or being in unobstructed presence to a stick of butter. Knowing, for example that what you want is to kiss your husband, I can predict what you will do when you find him. That I won't know exactly how you will recognize him and exactly how you will capture the kiss does not interfere. Similarly, if I assume you won't want one there, I can predict what you will do on finding a deer in your garden, and if I know you want butter on your toast, I can predict what you will do in the unobstructed presence of a stick of butter. The exact methods of recognizing, of deer shooing away, of butter procurement and so forth will not usually be relevant to my needs for prediction. To predict the general course of a person's behavior, all you usually need is some knowledge of what they want or intend, of what they know or can find out how to recognize, and what kinds of things they are able to do, letting the chips of detail fall where they may.

Taking a more complicated case, suppose that I know that you expect to attend John's paper at

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1 My "substance" concepts are concepts of "substances" as defined in *On Clear and confused ideas* (Millikan 2000, Chapters 1 and 2). They are not "substances" in Aristotle's sense but in a somewhat analogous sense.
2 On the sense of "information" intended, see my Millikan (2004), Chapters 3-4.

the APA meetings in Boston this year. On that basis I predict that you will arrive at the Blue Room of the Boston Hilton at about 10 am on the morning of December 29th. I do this confidently, but without knowing exactly how you will find out where the APA is meeting this year, or find out in which room John will be speaking and at what time. I may also have no idea how you will get to Boston or, for example, exactly how you will recognize when you have arrived at your train station, arrived in Boston, at the Hilton, at the blue room, how you will know what time it is when the proper time comes, and so forth. My prediction is based on the assumption merely that you have the ability to find out when and where John's talk will be, the ability to figure out how to get to Boston some way or another, the ability to recognize or find out how to recognize when you have arrived, what time it is and so forth. Knowledge of your particular conceptions --of John, of talks, of Boston, of busses or trains or planes if that's what you take, and so forth -- is not relevant to my ability to predict your arrival.

But there are times when knowledge of your conceptions *may* help me in predicting your behavior. If I know that you desperately want a chance to talk to your favorite author, Joan Jones, and I know that you are in the very same room with Joan at a party, I may predict that you will eagerly approach and speak to her. -- But only if I assume you can recognize her, say, that you know what she looks like! Knowing what she looks like (from some certain variety of angles, under some certain variety of lighting conditions, at some distances, etc.) would make up part of your conception of her, so what I may need to know concerns part of your conception. If a person is blind, many common ways of recognizing a great number of things will not be available to them. Their conceptions of many things must differ substantially from the norm. Your predictions of their behaviors in certain kinds of situations should differ accordingly. Taking a more familiar kind of case, if the author you wish to meet has the pen name "Miss Muggins" but her real name is "Joan Jones", and I hear someone introducing her to you as "Joan Jones", it will help me in predicting your behavior to know whether you know that Miss Muggins's real name is "Joan Jones". Knowing to recognize information that comes to you by way of the name "Joan Jones" as information about Miss Muggins would be part of your conception of Miss Muggins.

There is no perfectly general rule then whether knowing about a person's conceptions will be relevant to predicting their behavior. It will depend, sometimes, on the fineness of grain of the prediction. Also, using Lalumera's words, "concepts of many substances are associated with conceptions that *do* remain robustly similar across users", at least across normal adult users. The fact that we don't generally share *exactly* the same conceptions seldom causes problems, and even when our conceptions differ radically, as when someone is blind or deaf or knows just a lot more or a lot less about something than we do, hence has lots more or many fewer ways of recognizing incoming information about it, this does not often cause serious problems. Lalumera's worry about psychological prediction and explanation in the absence of "concept publicity" --I would rather call it an absence of "conception publicity"-- is unnecessary.

Lalumera's second worry is about empty substance concepts. Here it seems she may have misunderstood my position, or I have not made it clear. I do not think empty substance concepts are "ultimately analyzable along Russellian lines as bunches of definite descriptions", nor are they "classifiers" rather than "identifiers". Perhaps better: they are "would-be identifiers" that fail.

The status of empty substance concepts is a bit like that of nonfunctional tools, for example, badly designed or broken tools, or in the case of fictional substances (Santa Clause), like toy tools. Consider a can opener that is so poorly designed that it never has worked and never could work. Is it right to call it a "can opener" at all? Such a would-be can opener is like an empty substance concept, for it is a little unclear whether to call such a concept a "concept" at all. It is a mechanism or set of dispositions designed for the (biological) purpose of identifying and reidentifying a substance, but there is no *particular* substance it was designed to identify and it does not *actually* succeed in identifying and reidentifying anything. Consider a can opener designed only for *pretending* to open cans, that is, a toy can opener. The concept of a (purposefully) fictional thing is

like that. It is used for pretending to think and talk about something but, of course, there is nothing in particular it pretends to think about. It is a pretend concept, which is different, of course, from a would-be concept.

What about conceptual "sneezes"? If the would-be conception governing a would-be substance concept contains explicit ways of attempting identification, for example, if certain descriptions are taken as reliable indicators of the (supposed) substance, then, I suggested, a *word* that expresses the concept might sensibly be said to be "meaningful", otherwise not. The point is partly substantive and partly merely terminological. I take there to be two basic aspects of referential meaning, first, having a referent, second, having an associated conception. I take a conception to be made up of ways one knows to identify something. If a word expresses a would-be conception that does not succeed in identifying anything, and no prior part of which identifies anything either, it lacks both dimensions of meaning. It is a meaningless response to arbitrary stimulations; it is not about anything; it is a mental "sneeze".

Does a merely would-be concept have a "function"? Well, does the would-be can opener have a "function"? There may be a fairly definite way that the can opener functions in practice, namely, it draws people to it who wish to open cans and induces them to manipulate it in certain fairly well defined ways, but these manipulations result in failures. Similarly for an empty or would-be concept. It may be expressed in inference dispositions or in dispositions to respond to perceptual input, dispositions that constitute attempts to identify and reidentify a something-or-other. But these attempts result in failures. Nothing gets reidentified. There is also an important *difference* between the functioning of the would-be can opener and the functioning of the would-be concept however. The person manipulating the would-be can opener is likely to know quite immediately that her manipulations are not successfully opening a can. The person operating with the would-be concept may not ever find out that it is not successfully reidentifying anything real, that it is doing so only a small part of the time or, perhaps, that it is (not so much "empty" as) equivocal.[3]

A series of similar would-be conceptions associated by different people with the same word are sometimes passed on from person to person by mouth or print along with various descriptions supposedly applying to it's referent or extension, thus aping the function of a genuine substance term and concept. Lalumera is right that the psychological processing of an empty or would-be concept can sometimes mimic that of a genuinely referential concept for quite a long way, even though it fails in the end to serve its defining function as a referential or extensional thought. The person operating with a *pretend* concept may also have acquired its pretend conception from others, and may follow with it along specified psychological paths, but they will not be *failing* to identify. It is not their purpose to identify anything real in thought, not their purpose really to think of something, but merely to think along pretend or "as if" lines.

Lalumera's final question concerns the relation between substance concepts, conceptions, and ways of identifying substances. My claim has been that having the concept of a substance *involves* knowing ways to (re)identify it rather than *consists in* knowing ways to (re)identify it. This is because having a substance concept *also* involves grasping the or some point in (re)identifying the substance, for example, it involves understanding some of the kinds of stable properties the substance can be expected to display over various encounters with it, hence what kind of things might be learned if one learns to (re)identify it correctly. A good chunk of Millikan (2000) is devoted to unpacking this idea, but Millikan (2000) is not the book under review here.

Having a substance concept does involve having some particular way(s) of identifying something, just as having the ability to swim involves having some particular way(s) that one can swim. Does this mean we can "count two things per person per substance"? Well, I suppose if I am asked to count just "things", I could count the person's ability to swim as one thing and count the ways they know how to swim as another thing, but what would the point of the counting be? I have

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3   I have discussed the "epistemology of concepts", that is, the ways we find out through experience whether our concepts are adequate, in Millikan (1984, Part IV) and in Millikan (2000, chapter 7).

said that a substance concept can be thought of, very preliminarily and very roughly, as a mental word for a substance (Millikan 2000). Then knowing how to reidentify can be thought of as knowing when to apply a concept that is, knowing when to iterate a mental word in judgments. There is no conflict with contemporary psychologists here. There is a strong conflict with contemporary psychologists, however, on *how we know* how to apply a substance concept. We do not do this by recognizing the presence of certain properties whose presence is definitional of the substance, or by comparison with exemplars or paradigms. And there is a strong conflict concerning what it is to understand a public language *word* for a substance. Two people both of whom understand a certain word perfectly do not necessarily use the same methods of applying it. The meanings of extensional public language words correspond, in the first instance, to their referents or extensions; conceptions may differ from user to user.

These various ideas are spelled out in Millikan (2000)*,* not in Millikan (2005). But I am grateful to Lalumera for bringing this earlier book into view. It is probably the more fundamental book, in many ways presupposed by the latter.

References

Millikan, R.G.

1984   *Language, Thought, and Other Biological Categories.* Cambridge MA: MIT Press.

2000   *On Clear and Confused Ideas. An Essay on Substance Concepts*. Cambridge: Cambridge University Press.

2005   *Language: A biological model*. New York: Oxford University Press.

Reply to Origgi

Ruth Millikan

Let me begin by making some adjustments to Origgi's statement of my position that will lessen the apparent contrast between our views. She says --and this is right-- "Basically, if a linguistic item proliferates it is because the effects of its reproduction are often enough advantageous to the hearer and the speaker". But then she says "these advantageous effects consist in coordinating speakers and hearer behavior...It is thus of little use for Millikan to investigate what happens in people's minds when they speak or understand...". But the example of linguistic function that I make reference to most often is that of indicative mood sentences, claiming that their most prominent coordinating or "stabilizing" function is to instill true beliefs in hearers. Acquiring beliefs is certainly something that happens in hearers' minds. Learning language is, in one large part, learning how to form true beliefs on the basis of auditory input from indicative sentences one hears. Similarly, learning how to interpret the world one sees is, in one large part, learning how to form true beliefs on the basis of visual input. No matter how arrived at, I take it that arriving at true beliefs is advantageous to hearers. Imparting true beliefs is advantageous to speakers as well. It leads to the possibility of cooperative action in situations of common concern, or to predicted hearer reactions of interest to the speaker, or to hearer approval, and so forth. Origgi's emphasis on behavior is not mine. Similarly, when I said in my précis, "The specific psychological processes, ways of recognizing...that support our uses of proper names, words for kinds, properties and so forth, need not be uniform from person to person" I was not contrasting an interest in psychological processing with an interest in behavior. I intended a claim about the directly referential nature of much language, about the absence of a public sense or public intension corresponding to most extensional terms.

I tried to answer the question Origgi asks about language learning in Chapter 10 section 4 of Millikan (2005, pp. 202-219). The section is titled "Understanding What Speakers Intend," and it opens with explicit reference to Bloom's work. Origgi and Bloom assume that children must learn language either (1) in accordance with classical principles of association (Bloom) or conditioning (Origgi?) or (2) by having a theory of mind. I rejected this dichotomy. I opened section 4 by reminding the reader of the current deadlock between arguments in the literature supporting the idea that children must wield a theory of mind if they are to learn language in the normal way and the arguments suggesting that neither small children nor, for example, even 15 year old deaf children, in fact possess the kind of theory of mind supposedly required. "The puzzle is to understand how very young children can be aware of the intentions and the focus of attention of those from whom they learn language without yet having this sort of sophisticated theory of mind" (p. 205).

I argued that recognizing purpose in another's activity need not be the same thing as projecting an explicit intention as its cause, indeed, that even animals understand one another's behaviors as purposeful in the sense of being goal directed. Similarly, children lacking a representational theory of mind need have no problem understanding that someone else is behaving purposefully in speaking to them, or in helping them to do something. They understand when someone's purpose is to get them to do something, and when it is to get them to see something, to show them something. No representational theory of mind, no concept of beliefs or desires or intentions, is needed to understand that the speech of another is purposeful, to attend to its purpose, or to attempt to divine its purpose.

The understanding of language, I argued, is a form of perception of the world through a medium purposefully structured by another person for the purpose of showing me something. I can perceive what it is the other's purpose to show me and I can understand that this showing is purposive without having a representational theory of mind.

References

Millikan, R.G.
2005   *Language: A biological model*. New York: Oxford University Press.

Reply to Tomasello

Ruth Millikan

I think that most of the disagreement between Tomasello and myself is a matter of misunderstanding. To begin, agreement comes out explicitly, for example, in this passage from my *Varieties of Meaning* (Millikan 2004):

> The evolution of intentional signs from natural signs also occurs quite rapidly through a similar ratcheting process involving learning. According to Tomasello, "the available evidence suggests that ontogenetic ritualization, not imitative learning, is responsible for chimpanzees' acquisition of communicative gestures" where "in ontogenetic ritualization a communicatory signal is created by two organisms shaping each others' behavior in repeated instances of social interaction. For example, an infant may initiate nursing by going directly to the mother's nipple, perhaps grabbing and moving her arm in the process. In some future encounter the mother might anticipate the infant's impending behavioral effects at the first touch of her arm, and so become receptive at that point --leading the infant on some future occasion still to abbreviate its behavior to a touch on the arm while waiting for a response..." (Tomasello 2000 p. 176). Similarly, the human mother who sees her baby reaching for something may hand it to him, from which he soon learns simply to hold out his hand toward something he wants. Consider, as more sophisticated examples, how members of a sports team, or of a string quartet, or dance partners, may fine tune their coordination through practice, without an explicit understanding or awareness of the subtle signs they are using to accomplish this. It is possible that the emergence of a certain amount of intentional human signing (not necessarily involving explicit intentions) may have originally evolved in this way in connection with mutually beneficial social activities such as cooperative hunting, warfare, the creation of environmental structures like shelters and fortifications that benefit all, and so forth. Intentional signs originating in this way might then be passed on among humans by imitation. (p. 104)

And I agree with Tomasello one hundred percent that signs of this sort would not become *conventional* signs *until* they began to be passed on by imitation. In Chapter 1 of Millikan (2005) I begin the discussion of conventions by saying,

> Being perpetuated by reproduction is the first basic feature of natural conventionality. Natural conventions are handed down. (p. 3)

and in Chapter 3 I begin by saying

> A convention, in the sense that a natural language contains conventions, is merely a pattern of behavior that is (1) handed down from one person, pair, or group of persons to others --the pattern is reproduced-- and ... (p. 56)

It's true that the term "reproduced" could be read to include any behavior learned by operant conditioning or other forms of trial and error, where a behavior is repeated because it has been rewarded, but I did not intend it to cover such cases. (The technical definition of "reproduction," as I use this term, was given way back in Millikan 1984, where it is made explicit that it does not

cover trial and error learning, and I should have made things clearer in Millikan 2005 too.)

Before explaining why I think that some conventions may sometimes be handed down in part by "counterpart reproduction" and why this is important, I should clarify my project in attempting a description of linguistic conventions.

I began the first chapter of Millikan 2005 by remarking that "the big dictionary" (Webster's International, 2nd edition) lists "a dozen different senses" for the word "convention," each of which, I now add, contains two to five alternative clauses.

> Left unfettered, convention wanders freely from conventional wisdom through conventional medicine, conventions of art and "conventions of morality" to conventions of bidding in bridge. (p. 1)

A one liner from *Wikipedia i*s not of much interest here, nor is my project lexicographical or, God forbid, conceptual analysis! I was trying to describe what I took to be an important kind of *phenomenon* of a much more general nature than the conventions of language alone, but of which language conventions are a prime example. I was not claiming to define *language*, or claiming that *conventionality* in my sense is the only important characteristic of human language, or claiming that it is all that is needed to make a human language. However, Tomasello is right that conventions handed down by counterpart reproduction were quite purposefully included in my description of this phenomenon, and this was exactly because I believe that language forms are sometimes handed down, in some part, by this method. So let me try to explain counterpart reproduction more clearly.

Here, from Chapter 3, is the preliminary description of "conventions" as I proposed to use that term:

> A convention, in the sense that a natural language contains conventions, is merely a pattern of behavior that is (1) handed down from one person, pair, or group of persons to others -- the pattern is reproduced-- and (2) is such that, *if* the pattern has a function, then it is not the only pattern that might have served that function about as well. Thus if a different precedent had been set instead, a different pattern of behavior would probably have been handed down instead. (p. 56)

Counterpart reproduction occurs in cases where conventions solve "coordination problems", which I describe more simply than does Lewis. I say:

There is a need for coordination when:

> (1) members of a group ("partners" in a projected coordination) have a purpose in common;
> (2) achieving this purpose requires actions by each of the partners;
> (3) more than one combination of actions will achieve the purpose;
> (4) the set of workable combinations fails fully to determine what any single partner's contribution must be independently of the actions performed by the others.

> Coordination is achieved if the partners' combined actions achieve the common purpose. Coordination conventions are conventional patterns of activity that proliferate, in part, because (causally because) they achieve coordinations. [Here I should have made clear that, as Davidson said, reasons can be causes.]

> Some coordinations require the partners to act the same way, others to act differently. (p. 9)

A coordination convention thus involves at least two (assume just two) different roles, one for each participant, though it can be that these roles are performed by acting similarly. A convention that is reproduced, handed down, because it solves a coordination problem is a convention that requires partners, each reproducing his or her own part of the convention, each playing his or her own role.

Call these "role 1" and "role 2". My claim was that conventions of this sort may sometimes be handed down in part by partners who need to play role 2 encountering partners who are already playing role 1, or vice versa, and simply fitting in with them. Suppose someone learns role 2 by fitting in with someone already playing role 1. He might learn how to fit in by trial and error, or maybe in more reflective ways, it doesn't matter. The assumption (I should have been more explicit) is that the role 1 partner is playing role 1 because this has solved similar problems with previous partners who played role 2, or perhaps because he has observed similar coordinations achieved by others. Role 2 is thus handed down from previous performances of role 2 by way of partner 1's experience, direct or vicarious, with earlier role 2 partners. I called this "counterpart reproduction," in this case, of role 2. Why do I think this sometimes occurs?

In several places I use the example of learning social distance to illustrate unconscious learning of conventions, but the example will do as well to illustrate counterpart reproduction:

> Nor should we suppose that conventions are instantiated only by people knowingly following them. Witness the conventions for correct social distance when conversing. These distances vary from culture to culture, and are unconsciously reproduced by being learned as a skill. If you are at the wrong social distance, the one to whom you are speaking will move, so that to avoid slow circling about as you talk, you learn to stay at the conventional distance. Similarly, a person, even everyone, might unconsciously learn to conform to the convention of driving on a given side of the road solely as a skill --as a means of avoiding oncoming traffic. (p. 148)

I suppose it is also possible that a person could learn Lewis's telephone convention, the convention that when a connection is broken the original caller calls back while the other waits, not by imitating but by fitting in with what they found their partners doing. But I was not interested in claiming that there are any particular conventions that are *always* handed down in this way. Rather, I was making two more general points, one about social conventions generally, the other about language specifically.

About social convention in general, I was interested in the fact that various things done conventionally in a culture may be self perpetuated by a sort of practical necessity because one custom requires others to fit and these others ultimately turn around to support the one. Thus my chopsticks example. If people eat with chopsticks, then chopsticks will be what is manufactured, and what is sold, and chopsticks will be what you can buy to eat with, and what's on the table when you go out to dinner, what's around when you teach your children to eat. Eating with chopsticks, putting chopsticks on the table and, say, manufacturing chopsticks are all conventional, but they are also all practical things to do given one another. Chopsticks are manufactured because they are used and used because they are manufactured. That is counterpart reproduction.

That something like this sort of circle, but even tighter, is relevant to language should be obvious. Though the forms of the various languages may be to a high degree conventional, which implies that they are, in a sense, arbitrary, when in Turkey it is best to speak Turkish, in Poland Polish, and there is nothing arbitrary about that. The reason, of course, is that in Turkey one's coordination partners are most likely to know how to coordinate with the conventions of Turkish, in Poland, with the conventions of Polish -- coordination, in this instance, beginning with understanding. People in Turkey speak Turkish because other people in Turkey understand Turkish and people in Turkey understand Turkish because other people in Turkey speak Turkish. That is counterpart reproduction.

As for learning language, it is well known that children understand a great deal of language long before they reproduce the forms that they understand. Informally, they learn to play --they reproduce-- role 2 for their native language before role 1. And it should be clear that they don't learn to play role 2 --the understanding role-- by imitation. They don't watch how other people understand and then copy. But they wouldn't learn role 2 unless their teachers were playing role 1,

and their teachers wouldn't be playing role 1 unless they had had some reasonably successful experience with previous partners who played role 2. (If nobody ever understood you, you would either stop speaking or change your language.) That is what counterpart reproduction is. It is not, I believe, controversial. I think I just did not make myself clear enough.

There is, perhaps, some disagreement over the case of linguistic imperatives or directives. However, Tomasello rejects my position on directives because he sees it as failing the very requirement that I accept it for fulfilling! The disagreement then is not about the requirement but about the example. Tomasello says:

> In an imperative speech act the speaker requests that the listener help her in some way. This request is cast in conventional form, of course, but the tendency of the listener to help (altruistically) must be justified evolutionary by some advantage to her for providing this help (e.g., by increasing her reputation for being a helpful partner). It is unlikely in the extreme that anyone cooperates with an imperative speech act by imitating other recipients' compliance, simply in order to follow precedent. Responding to an imperative by providing the requested help thus does not represent the completion of a conventional pattern, learned by imitation, but rather a 'natural', biologically evolved behavioral response.

Linguistic conventions are what I called "coordination conventions," defined as conventions that solve coordination problems, defined in turn by the fact that partners to the convention have a common purpose and such that

> Coordination is achieved if the partners' combined actions achieve the common purpose. Coordination conventions are conventional patterns of activity that proliferate, in part, because (causally because) they achieve coordinations. (p.9)

Achieving coordinations is achieving purposes held in common. Clearly, linguistic conventions are not reproduced "simply in order to follow precedent." The question we must answer is what the common purpose is in the case of directive language forms, especially, what purposes complying serves for the hearer. A genuine disagreement here may be that I am interested in everyday purposes that could motivate the hearer in an ordinary way, not a purpose (perhaps opaque to the hearer?) somehow buried in evolutionary history. We are interested here in a role 2 response that the hearer might learn by successful doing followed by reward. We are back to counterpart reproduction.

An important key here is one that I have emphasized over and over in chapter after chapter of book after book. On every level of selection (and there are many --see, for example, Millikan 2005 chapter 10) items that survive because they serve a function may in fact serve that function only a very small proportion of the time. They may fail in their functions much more often than they succeed. Language forms survive because they sometimes serve cooperating functions. But we are not looking for a cooperative function that is always or, perhaps, even usually served by directives, but only one that is served often enough to keep directives in circulation. In chapter 2 I put the matter this way:

> The root function of the imperative mood, for example, is to produce a corresponding action by the hearer, where the speaker is interested in having that action performed, and the hearer is interested in completing the conventional pattern, perhaps because he has a further interest in common with the speaker (hearers often want direction from speakers) or because conforming is sanctioned in one way or another. Performance of this imperative function is accomplished through the hearer's first forming an intention to perform the designated action, an intention formed following the truth-conditional semantic conventions of the language, so that reinforcement of the hearer's compliance tends to reinforce his

observance of these semantic conventions as well. (p. 47)

In Chapter 3 I put the matter this way:

> ...if it were not sometimes in the interest of hearers to comply with imperatives —advice, instructions, directions, friendly requests, sanctioned directives, and so forth— they would soon cease ever to comply. And if hearers never complied with imperatives, speakers would soon cease to issue them. (p. 58)

Recall, among other directives, army orders! Imperative speech acts in which "the speaker requests that the listener help her in some way" and in which that way will not also help the hearer in some way, are indeed sometimes performed, and, conceivably, cooperative compliance in these cases has received a direct push from our evolutionary history. But this is not essential to the analysis. Consider how small children learn to follow directives. Largely by reward. That they are receptive to acceptance and approval as a reward is, of course, a product of our social evolutionary history, and this kind of reward may indeed be the most basic and effective kind. That does not challenge but merely supplements the analysis.

I said that counterpart reproduction retards drift. I had in mind a large population in which the role played by any one individual would have to fit with that played by a random partner or random partners from a large pool. I should have been more accurate, for it is really just the need for coordination between randomly selected individuals that retards drift, and of course only some coordination patterns are conventional, others are passed on genetically. Also, retarding drift is not, of course, stopping drift. All human languages have changed again and again over time. The anticipatory movements that were at the origin of various animals' signals to conspecifics are, in general, no longer recognizable. The various calls characteristic of the various species of birds are about as arbitrary as are the words in human languages. As for humans recognizing the arbitrariness of their own languages, I *believe* it is the case --I would have to search for references, indeed, I probably learned this in my undergraduate days-- that isolated primitive peoples generally are not aware that their languages are arbitrary, just as little children are not.

A final comment on possible differences with Tomasello. In using as his title "Conventions are Shared" and later citing Gilbert, Searle and Bratman on "shared intentionality," I take it he may have in mind that human language rests essentially on sharing something like a representational theory of mind. If so, I am sorry he did not join the issue by discussing my chapter 10, where I try to explain how children learning language can take account of other people's minds in sophisticated ways without yet having any sort of "theory" of mind.

References

Millikan, R. G.
1984   *Language, Thought, and Other Biological Categories.* Cambridge MA: MIT Press.

2004   *Varieties of Meaning.* Cambridge MA: MIT Press.

2005   *Language: A biological model.* New York: Oxford University Press.

Tomasello, M.
2000   "Two hypotheses about primate cognition", in C Heyes & L Huber (eds.), *The Evolution of Cognition.* Cambridge MA: MIT Press, 165-183.